

# Bulletin of the Anglo-Israel Archaeological Society



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On the cover: an 18th-century map of the Holy Land, by Eman. Bowen.

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## Editorial

Mosaic floors, especially decorated ones, are much more than just artistic features; they tell a story not only about the craftsmen who made them, their workshops, choice of designs and influences, but also about the people who commissioned them, their social background, status and their attention to fashions in other parts of the ancient world. The first article in this issue deals with only one fragment of a decorated mosaic floor, but the author, Alexandra Ariotti, an archaeologist who has dug at various sites in Israel and Jordan, undertook a fascinating investigation that led her from the north wall of the Church of St. James in Sydney, Australia, all the way to the site of a fifth- or sixth-century synagogue at Na'aran near Jericho which is now undergoing conservation by a joint team of Italian and Palestinian architects/restorers. Ariotti found that not only had the mosaic fragment been haplessly yanked out of the floor of the ancient synagogue by Anzac troops at the time of the First World War and subsequently shipped to Australia, but it also had the fate of being culturally misappropriated and labelled as an item of Christian derivation. For the first time Ariotti puts the record straight.

There are four other research articles in this issue. The first by Dr Achim Lichtenberger, of the Archäologisches Seminar and Museum in Münster, deals with city foundation legends in the Decapolis – a group of cities situated in the Roman provinces of Syria, Syria-Palaestina and Arabia – and shows what the study of the local coinage of that period can contribute to this fascinating subject. The second by Dr André Lemaire, of the Ecole Pratique des Hautes Etudes at the Sorbonne in Paris, is a rejoinder to an article on the now infamous 'James Ossuary' by Emile Puech published in a previous issue of *BAIAS* (21: 45–53). As the person who originally published the ossuary, Lemaire believes he has since suffered an injustice at the hands of the Israel Antiquities Authority, who investigated the authenticity of this artefact, and the scholarly world. Lemaire expresses surprise that in my Editorial in that issue of *BAIAS* I dealt with what he calls 'unfounded rumours'. I can reassure readers that this is definitely not the case and I refer them to my recent article 'A Lost Cause: A Response from Shimon Gibson on the James Ossuary Inscription' (*Biblical Archaeology Review* Nov/Dec 2004, 30, 6: 55–58) that contains all the details that have a bearing on this issue. In that article I concluded: 'what we are left with is an artefact with hardly any apparent archaeological or historical significance. Hence any attempt to pursue the truth about the James Ossuary is definitely a lost cause.' My stance on the matter has not changed since then. In any case, the subject is now *sub judice* and the alleged forger, Oded Golan, is being tried in the Israeli law courts. The truth will eventually come out.

Controversy is the subject of two other articles in this issue, with the scholars, Professor Itzhak Beit-Arieh of Tel Aviv University and Dr Benjamin Sidel of the

Albright Institute of Archaeological Research in Jerusalem, locking swords on the subject of the phasing and function of Unit A at the Early Bronze II site of Nabi Salah in southern Sinai. Readers will find it fascinating how one may reach differing interpretations about the same archaeological materials. A knockout has not yet been declared!

A review article is provided on Ephraim Stern's very important book on the Assyrian, Babylonian and Persian periods in Palestine. Peter James surveys the strengths and weaknesses of the book, takes Stern to task on the supposed 'gap' in the Babylonian period, and provides interesting insights concerning the chronological problems of that time. Two book reviews deal with a study of the Temple Mount in Jerusalem by Asher Kaufman, reviewed by the Honorary Secretary of our Society, Stephen Rosenberg, and on Qumran and the Dead Sea Scrolls by Jodi Magness, reviewed by Emile Puech, the Editor of *Revue de Qumran*. I have a copy of Kaufman's book in front of me, sent to me courtesy of the author, and it is truly a beautiful book, even though I do not subscribe to every one of Kaufman's theories and especially in regard to his theory about the physical location of the Jewish temple at the time of Herod the Great. Jodi Magness, a professor at the University of North Carolina at Chapel Hill, is a very prolific writer, always with a book or two in the works and a flurry of scholarly papers being published, and her great interest in the Qumran site is well represented in her book. Puech's review was translated from the French courtesy of Marion Rosenberg and edited by Claudine Dauphin: my thanks to them both.

This *Bulletin* concludes with five lecture summaries, a grant report and obituaries. Unfortunately, scholarship and the archaeological world have lost many colleagues in 2003 and 2004 – J.B. Segal, Dan Urman, Avner Raban and Roger Moorey – two of whom had a notable connection with the Anglo-Israel Archaeological Society: Professor Ben Segal, who served for many years as Chairman and later as Honorary Vice-President of the Society, and Professor Dan Urman, who served behind-the-scenes as a member of the Editorial Advisory Board of *BAIAS*. These gentlemen were total opposites: Ben Segal was softly-spoken and reflective, while Urman was gregarious and had a fiery spirit. I will always recall Ben Segal's calming but strong and sensible presence on the Committee of the Society, when as a youngster in the early 1980s I joined the Committee at the bidding of Richard Barnett who was Chairman at the time. When I first met Professor Urman, however, at Wolfson College in Oxford, he nearly bit my head off about a somewhat unfavourable review I had written on a book he published about his research in the Golan; we subsequently became good friends. Professor Avner Raban was a man of moods, rebellious, erudite, with strong views and a love for the pleasures of life. I never knew where I was with him: he could growl or show affection but his head always seemed to be elsewhere. He loved the Mediterranean Sea and spent a large chunk of his life underwater. I shall never forget Raban striding into a lecture hall at an archaeological conference held in Jerusalem in the early 1980s, interrupting a lecture given by Haya Ritter Kaplan on sea levels off Jaffa, and with a husky voice announcing to the lecturer 'You have got it all wrong,' before then striding out

again. The late Dr Roger Moorey, a very private gentleman of the highest scholarly calibre, not long ago gave to the Society a fascinating lecture on ancient idols and figurines in Canaan and Israel, but never got to see the publication of his lecture summary (provided in this issue of the *Bulletin*). Professor Claudine Dauphin, a close friend of Moorey, has provided a heartfelt and extremely moving obituary. I myself shall miss these four individuals and will remember them fondly each time I come across their written work.

The Editors and Committee gratefully acknowledge the very kind donations made by the Polonsky Foundation, the Sidney and Elizabeth Corob Charitable Trust, Mr and Mrs R Grutz, the Kessler Foundation and others, towards the publication of the *Bulletin* and the leaflet of the Society. My sincere thanks to Ashley Jones, Publishing Editor, Claudine Dauphin, Editorial Advisory Board member, and to Diana Davis, Executive Secretary of the Society, for help in producing this issue. Members may wish to know that past issues of the *Bulletin* are now available online to paying subscribers via the EBSCO website.

*Shimon Gibson*

## A Missing Piece Found: Tracing the History of a Mosaic Fragment at the Church of St. James From Jericho to Sydney and Back Again

ALEXANDRA ARIOTTI

Since 1999, a team of Italian and Palestinian restorers subsidized by the Italian Ministry of Foreign Affairs has been working to conserve a badly damaged mosaic floor of a Byzantine period synagogue at Na'aran, located about seven kilometres northeast of Jericho. According to the restorers, Carla Benelli and Osama Hamdan, much of the damage caused to the mosaic pavement occurred in the past century with portions of its highly stylized polychrome designs having gone missing.<sup>1</sup> Although it remains an almost impossible task to trace the whereabouts of such pieces in order to 'repatriate' them to their original architectural setting, it has now been established with certainty that at least one fragment of this mosaic floor is presently housed in an Anglican church in Australia. Ironically the significance of this fragment of Jewish faith and tradition is totally lost on those visiting the church. Instead, it is regarded as a Christian relic and as a military souvenir taken from the Holy Land by the Australian and New Zealand Army Corps (Anzac) during the First World War and it was donated to this church specifically because of its presumed Christian associations. Given what is now known, however, it appears that this small mosaic fragment has not only been fundamentally misinterpreted, but it has also been culturally misappropriated, on the basis of inadequate scholarship.

### **The mosaic fragment: a description**

Encased in a black semi-circular marble medallion and exhibited on the north wall of the Church of St. James in Sydney, the mosaic fragment measures 0.73 m in width and 0.42 m in height (Fig. 1). Its decoration is dominated by a reddish-brown horizontal grapevine (30 × 3 cm) with thin, tendril-like branches flanked by two sets of black, olive and brown bunches of grapes and leaves (17 × 10 cm). This motif is set against a cream background and bordered by at least two semi-circular and vertical bands that measure 2 cm in width and consist of a single row of black *tesserae*. Made of limestone, basalt, marble and terracotta, the *tesserae* are small and irregular in shape, each individual *tessera* measuring 1 × 1 cm to

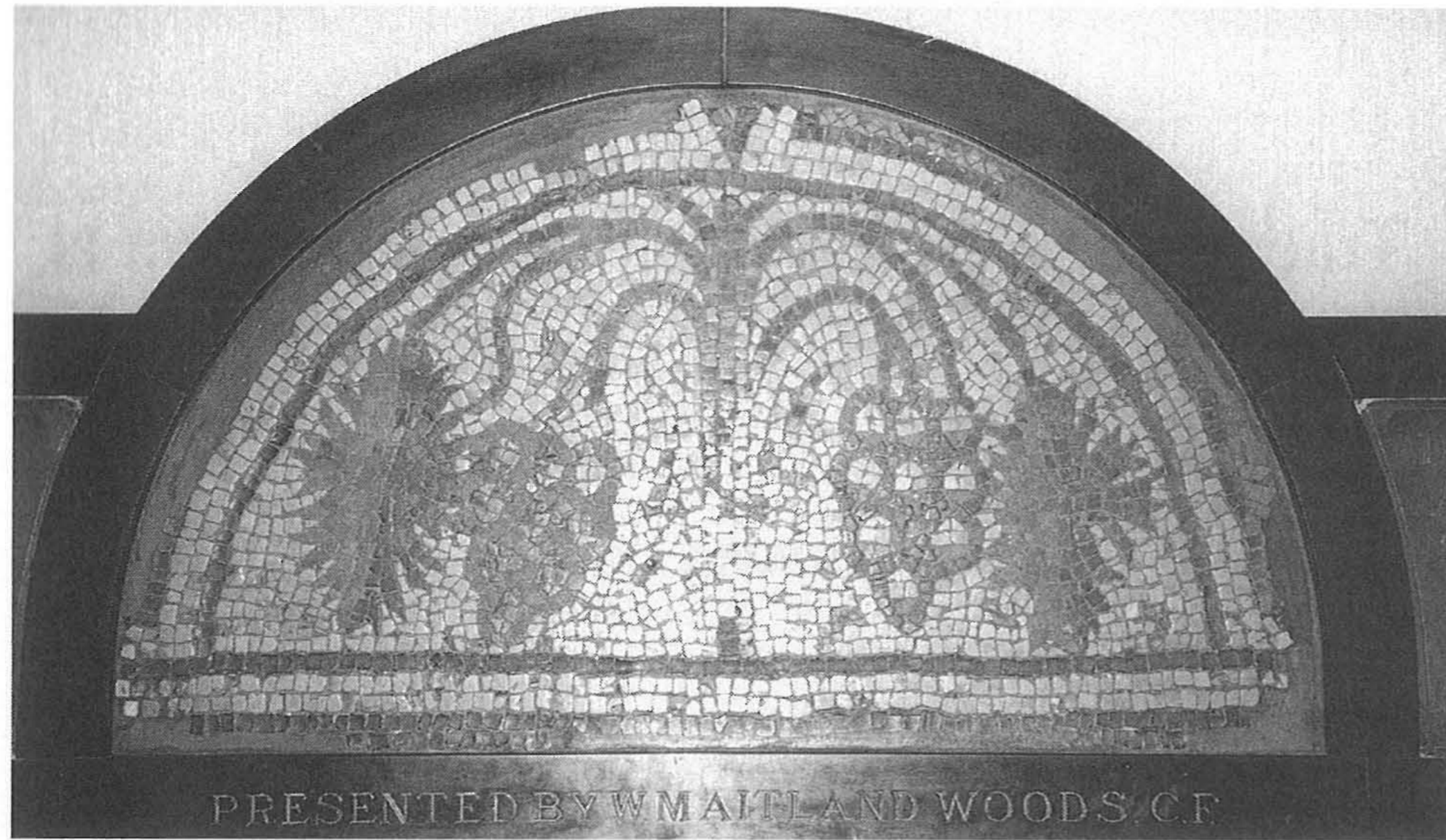


Fig. 1. Grapevine design, St. James' Church mosaic fragment.

2 × 2 cm (cf. Roussin 1991: 35). The technique used to produce the mosaic is *opus vermiculatum* or 'worm-shaped work' where the *tesserae* are cut in irregular or asymmetrical shapes and laid in patterns dictated by the design. The size and number of the *tesserae* largely determines the quality of the pavement; for example, the nuances of the modelling could be rendered more subtle with smaller sized *tesserae*. The St. James' Church fragment is composed of 80 to 100 *tesserae* per square decimetre, which, according to Avi-Yonah's classification of Palestinian mosaic pavements (1934: 374), places this mosaic in a category of work of 'standard' quality.

### The discovery of the mosaic

To some extent, the parish archives of the Church of St. James provide documentation regarding the discovery and acquisition of the mosaic fragment by the Reverend William Maitland Woods (1864–1927), Senior Chaplain to the Australian Imperial Force (AIF) in Egypt, Sinai, Palestine and Syria during the First World War, as well as of the donation of this piece to mark the 100th anniversary of the laying of the foundation stone of the church in 1919. The church's bulletin, the *Monthly Church Messenger* (1919: 4), states that the mosaic fragment originally formed part of the floor of 'an early Christian church ... [situated] under the Mount of Temptation close to Old Jericho in Palestine'. In his account of the discovery of the mosaic, Maitland Woods (1919: 8) proposed that the locality of the 'church' would have corresponded to a twelfth-century Russian pilgrim's description of a



Fig. 2. Portion of mosaic, St. James' Church.

church erected to consecrate the place where Joshua and the Archangel Michael conferred prior to the attack on Ai, as described in Joshua 5:7. The mosaic floor was exposed by chance following the detonation of a high explosive shell and some fragments were recovered by the Second Division of the Anzac Light Horse Brigade in August 1918 (Maitland Woods 1918: 1).

In an article published in the *Brisbane Cathedral Notes* (1918: 1), Maitland Woods reported that the AIF headquarters was based near a spring called Ain ed-Duq located in the valley foothills, two and a half miles (seven kilometres) north of Jericho, in a region known as biblical Gilgal. Gilgal is the place near the traditional spot where Joshua and the children of Israel crossed the Jordan River with the Ark of the Covenant, eventually establishing there a camp in which they positioned twelve stones from the river to signify the twelve tribes of Israel (Josh. 4:19–20). The troops were stationed in the Jordan Valley area at the time of the military campaign by the British and Australian Imperial Forces against the Turkish Army during the summer of 1918. Maitland Woods describes the circumstances leading to the discovery of the mosaic as follows:

Close to where [some] ruins existed was an ancient aqueduct. The enemy guns were searching for this aqueduct and shelling heavily. One of the shells exploded, exposing a broken floor of a mosaic. The Senior Chaplain of the Desert Corps [i.e. the author, Maitland Woods] was communicated with by telephone, and invited to make investigations. Following an examination of the floor, investigations of historical records revealed that in 1147 AD, a

pilgrim had recorded that a church had been built during the time of Constantine the Great (325 AD) and had been seen by him on the date above-mentioned. After that, the locality of this church has been unknown. It is natural to suppose that during the Turkish incursion in 1517 AD, this church with many others was utterly destroyed (Maitland Woods 1918: 1).

In a letter addressed to the Académie des Inscriptions et Belles Lettres (Paris) of March, 1919 (Picard 1919: 91), an English officer, Major Furber, described how the troops 'uncovered a portion of [a mosaic] including [an] inscription, but unfortunately ... [they] had not the time to uncover the whole of the pavement'. Woods and some of the soldiers drew the visible sections of the mosaic and removed parts of it. With the permission of the War Trophy Committee in London, a fragment was presented for 'safe-keeping' to the Church of St. James, since Maitland Woods was, at that time, well acquainted with the church rector, Dr P. Micklem. Another piece representing, in Maitland Woods' words 'a cloven tongue, this being a symbol of the Holy Ghost' (1918: 1), was apparently entrusted to St. John's Cathedral in Brisbane as a 'symbolic message from one of the oldest churches in the world ... to the youngest cathedral in Australia'.<sup>2</sup>

These records suggest that Maitland Woods' knowledge of the Old Testament, as well as his reliance on the narrative of a twelfth-century Russian pilgrim, were strong influences in regard to his understanding of the mosaic and the character of the site where it was found. Maitland Woods was convinced that the mosaic came from the floor of a church mentioned in the medieval account of a Russian pilgrim. On the basis of his research on the 'historical records of the localities from ancient Jericho to the Mount of Temptation', it would appear that Maitland Woods (1918: 1) was referring to the narrative of the Russian pilgrim Daniel the Abbot, from 1106–1107 (cf. Wilkinson *et al.* 1988).<sup>3</sup> As a member of the Russian Orthodox Church and as an abbot of a monastery, Daniel travelled extensively throughout Palestine. His first excursion was to the Jordan Valley where he travelled to a place near Jericho where, according to tradition, the Archangel Michael appeared before Joshua in the presence of the Israelite Army (*PPTS* 1895: 31). Following this meeting, Joshua led the Israelite Army to Jericho where 'they utterly destroyed all that was in the city' (Josh. 7:59). Abbot Daniel claimed that a monastery and church had been built at this place and were dedicated to St. Michael (*PPTS* 1895: 32). Most notably, he mentioned a 'large and lofty' church containing the twelve stones that marked the site of Gilgal and are depicted on the Madaba Map (Avi-Yonah 1954). Abbot Daniel's information about these holy places was most certainly provided by his guide, an unnamed Greek monk from the Monastery of Laura at St. Saba (Wilkinson *et al.* 1988: 10). It is reasonable to assume, therefore, that his identification of such holy sites was, at best, questionable. Some scholars have proposed that the site of the Monastery and Church of St. Michael was not situated at Gilgal, but at Jiljulieh, further up the Jordan Valley where there are traces of an old monastery (Wilson 1895: 32). Yet the current consensus places the traditional site of biblical Gilgal at Tell Matlab, approximately two kilometres to the northeast of Jericho, where there are the remains of a church (Wilkinson *et al.* 1988: 52).

The identification of the mosaic fragment at St. James' Church seems to have been based entirely on Maitland Woods' interpretation of the site where the mosaic pavement was discovered. Maitland Woods was described in the *Australian Dictionary of Biography* (Haldane-Stevenson 1943: 567) as a 'strict Anglo-Catholic' and a 'keen amateur archaeologist' who, as Senior Chaplain of the Anzac Mounted Division, lectured to the troops on the history and archaeology of the Holy Land during the intermittent long periods of inaction when the AIF was advancing through the region. He clearly regarded the two mosaic fragments from Jericho as valuable Christian relics, and the motivation behind the donation of these pieces to the Australian churches appears to be founded in his desire to inspire the church-going public. However, as we shall see, the ruined site from which these pieces were taken was not of a church, but of a synagogue.

### The synagogue and its mosaic pavement

Enclosed with Major Furber's letter to the Académie was a photograph of the mosaic pavement *in situ* showing a Hebrew inscription and an image of a bison. M. Clermont-Ganneau, a member of the Académie, inspected the photograph and declared the site to be that of an ancient synagogue. At the same time, British Army officers (and archaeologists) R. Engelbach and E. J. H. MacKay made a physical examination of the site from which the St. James' Church mosaic had been removed (Benoit 1961: 162; Avi-Yonah 1977: 891). Informed of its existence by Engelbach and MacKay, the scholars Fathers L.-H. Vincent and M. J. Lagrange recorded some of the surviving structural remains of the building, including part of its mosaic pavement. Father Vincent subsequently excavated the site between 1919 and 1921 on behalf of the Pontifical Biblical School (Avi-Yonah 1977: 891). Although parts of the mosaic and the plan of the building were reproduced in Eleazar Sukenik's *The Ancient Synagogue of Beth Alpha* (1932), it was not until 1961, when Pierre Benoit unearthed Father Vincent's excavation report in the Dominican archives, that the findings were published in their entirety. Father Vincent confirmed Clermont-Ganneau's view that the ruins were not of a church, but of a synagogue, specifically, the Na'aran Synagogue which he dated to the late third to early fourth century AD (Benoit 1961: 165).

Na'aran is described in a number of ancient literary sources as a Jewish settlement that was occupied until the seventh century AD (Tsafrir *et al.* 1994: 197). It was named in Joshua (16:7) as Naarath on the border of Ephraim, and as Naaran in the region of Ephraim in I Chronicles 7:28. In *Antiquities of the Jews* (XVII; 1951 ed.: 340), the first-century Jewish historian Flavius Josephus mentioned the village of Neara as situated in the valley of Jericho. The Christian historian Eusebius of Caesarea (1960: 203) mentions the town of Noorath located five miles from Jericho in the territory of Ephraim in his early fourth-century topographical work (and source for the sixth-century Madaba Map mosaic), the *Onomasticon*. The town was also referred to in the *Life of St. Chariton*, the biography of the first Christian monk to settle in the Judean desert in the fourth century AD (Negev and Gibson 2001: 354).

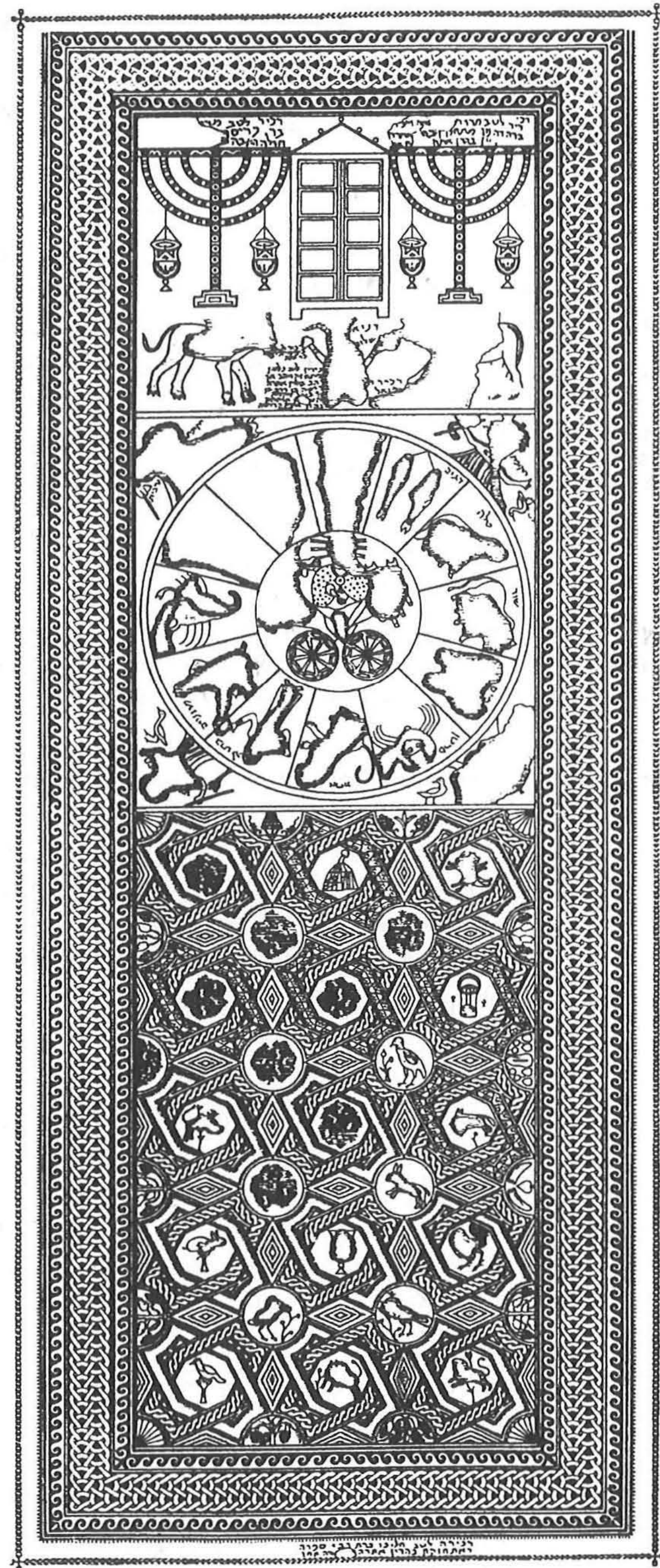


Fig. 3. Drawing by L. H. Vincent of the mosaic pavement *in situ* at the Na'aran synagogue (reproduced from Avi-Yonah 1977: 893).

Father Vincent's excavation of the site revealed a synagogue complex comprising, among other architectural components, a main prayer hall divided by six columns into a nave and two aisles and paved with a multicoloured rectangular mosaic carpet featuring numerous geometric, vegetal and figural designs, as well as Hebrew dedicatory inscriptions (Negev and Gibson 2001: 354) (Fig. 3). Measuring 14.94 m in width, Father Vincent estimated the length of the pavement to be 21.94 m, although its precise length could not be fully determined because the southern end of the building was not preserved (Benoit 1961: 162). His drawing of the synagogue's mosaic pavement *in situ* clearly shows a series of semi-circular roundels in which images of grapevines flanked by bunches of grapes and leaves are represented, that greatly resemble in appearance and dimension those in the St. James' Church mosaic fragment (Fig. 4). Father Vincent's drawing also reveals that one of the semi-circular segments was missing, and it appears to be of the same shape and size as the St. James' Church fragment (0.73 m × 0.42 m). Hence, it is almost certain that the St. James' Church mosaic fragment was removed from the floor of the Na'aran synagogue. It is interesting to note that even having viewed parts of the mosaic pavement *in situ* with its Hebrew inscription and depictions of such Jewish symbols as the *menorah*, Maitland Woods remained unmoved in his belief that this site was most likely the Monastery and Church of St. Michael and not that of a synagogue.

A number of publications that document the Na'aran synagogue, including Father Vincent's drawings, indicate that its floor was decorated with a range of motifs of Jewish symbolism (Avi-Yonah 1977: 891; Benoit 1961: 185; Ilan 1991: 149–150; Negev and Gibson 2001: 354). Near the entrance to the synagogue hall are two gazelle, as well as a Hebrew inscription that states: 'Let be remembered for good



Fig. 4. Grapevine image, Na'aran synagogue pavement (after Benoit 1961: 186).

Halifou, daughter of Rabbi Safra, who donated to this Holy Place. Amen!’ (Klein 1977: 109–110; Negev and Gibson 2001: 355). In Byzantine rabbinical literature, the gazelle was used as an analogy to God: ‘As a gazelle leaps from place to place ... so God jumps and leaps from synagogue to synagogue so that he may bless Israel’ (in *Numbers Rabbah* 11: 2; Gutmann 1975: 1). Inside the synagogue itself, the northern half of the pavement is decorated with octagons bounded by bands of guilloche and lotus flowers, within which are images of animals and birds. Representations of animals and birds, either alone or grouped with fruits and plants, were popular in the floor decoration of both secular and religious buildings in Byzantine Palestine. While most of the animals shown in the mosaics probably existed at one time in this region, these images were not drawn from observation, but copied from a standard and conventional repertoire of animal representations on late Roman mosaics (Piccirillo 1993: 3). Father Vincent noticed that many of the birds and animals were deliberately destroyed, but their cages and baskets, as well as the Hebrew inscriptions had not been touched (Benoit 1961: 162). Despite a traditionally tolerant approach to figurative representation in art by the *halakha* or Jewish law from the fourth century onwards, it seems that local Jewish iconoclasts carried out this destruction because they evidently saw these images as idolatrous and profane (Ovadia 1987: 160; Negev and Gibson 2001: 355).

The central part of the mosaic pavement depicts the twelve zodiac signs in radial arrangement with Helios, the Sun God, at the centre and surrounded by the personifications of the four seasons. The zodiac composition was a regular component in synagogue mosaic pavements from the early fourth to sixth centuries AD. Various explanations have been advanced to account for the use of this motif here and in other synagogues (Hammath Tiberias, Ma’on, Beth Alpha Hosefa and Khirbet Susiyah): for instance, as a calendar to assist in the planning of the liturgical year, as a symbol of cosmic forces, or as the divinely governed order of nature (Schapiro 1980: 28). Indeed, the fourth-century *Midrash Devarim Rabbah* states:

The Holy One Blessed-Be-He showed Abraham all of the zodiac [*mazalot*] surrounding his *shekhinah* [Divine Presence] ... and said just as the zodiac surrounds Me, with My glory in the centre, so shall your descendants multiply and camp under many flags, with My *shekhinah* in the centre. (Gutmann 1989: 1337).

This citation is one of the few rabbinical quotes that may be associated with the imagery on mosaic floors of synagogues: Helios could symbolise the *shekhinah*, or God’s divine presence. However, at the same time, Helios, the Sun-God, is derived from representations of *Sol Invictus*, the unconquered Sun, whose cult was favoured by emperors of the third and early fourth century AD (Dunbabin 1999: 189). Although the personifications of the four seasons appear on the floors of the church of St. George at Khirbet al-Makhayyat (Mount Nebo), the Petra church and the church of St. John the Baptist at Jerash, there is to date no evidence of the use of the zodiac in the decoration of Christian buildings in Palestine.

The southern part of the pavement is decorated with a Torah shrine flanked by two *menoroth* above which are featured some Hebrew inscriptions indicating that

the synagogue was built with donations from many members of the community (Negev and Gibson 2001: 355). The *menorah* is described as a seven-branched golden lampstand placed in the Tabernacle (Exodus 25:31–40). As a symbol of God, or of the astral path to God, the *menorah* is a prominent feature of synagogue mosaic decoration, together with other Jewish ritual objects like the *mahatta* (incense shovel), the *shofar* (ram’s horn trumpet), the *lulav* (palm branch) and the *ethrog* (citrus fruit), all of which appear in the synagogues of Hammath-Tiberias, Bet-She’an and Beth Alpha (Dauphin 1978: 29; Roussin 1991: 50). Not only were these objects used in the synagogue service, but they had also been given mystic or eschatological interpretation in Jewish literary sources. For example, the Jewish philosopher Philo of Alexandria (c. 20 BC–AD 50) in his first-century work *De Vita Mosis*, referred to the *menorah*:

Moses placed the *Menorah* at the south, figuring thereby the movement of the luminaries above; for the sun and the moon and the others run their courses in the south ... the seven lamps and candlebearers [are] symbols of what the men of science call the planets (Philo of Alexandria 1961: 102).

Other Jewish literary documents suggest that the *lulav* and *ethrog* were associated with Tabernacles as a festival of rain and light, but they also embodied mystical overtones to become, for example, a eucharist of escape from evil and of the passing into a realm of justice (Goodenough 1988: 113). Similarly, the *shofar* signifies the covenant relationship between God and Israel: it is the horn of a ram or trumpet that ‘the Lord God shall blow’ to announce Israel’s future deliverance when the Jews will be redeemed from their sorrows (Zechariah 9:14).

A figurative scene is also featured in the southern part of the Na’aran synagogue pavement. Located beneath the *Torah* shrine a man with raised hands stands between two lions. Although badly defaced, scholars have identified this image as that of ‘Daniel in the Lion’s Den’ on the basis of an accompanying inscription that reads: ‘Daniel shalom’ (Ovadia 1987: 155). One of the few biblical episodes to be depicted on synagogue floors, some scholars have contended that the inclusion of the Daniel story in the Na’aran pavement served as a reference to the oppressive rule of Emperors Justin I (AD 518–27), Justinian (AD 527–65) and Justin II (AD 565–78) (Ovadia 1987: 156). During this period, stringent imperial edicts not only permitted the construction of a very limited number of synagogues in Palestine, but the Imperial authorities also intervened in the internal life of the Jewish community. In his *Secret History*, the early sixth-century historian Procopius of Caesarea (1966 ed.: 132) stated that the Emperor Justinian ‘took pains to abolish the [religious ritual] laws which the Hebrews honour’. He further claimed that Justinian had decreed that in those years in which the date of the Jewish Passover fell before the Christian Easter, Jews were forbidden to celebrate Passover on its appointed date (1966 ed.: 133). They were also prohibited from reading the Torah in Hebrew (only in Greek) nor could they recite the Mishnah in the synagogue (Wilken 1992: 204). Given the extent of this oppression of Jewish cultural and religious life under Justinian’s reign, the refusal of the Jews to submit to these Imperial laws could be

seen to mirror Daniel's resistance to King Darius' will as recounted in Daniel (6:6–22) in which Daniel is 'cast unto the den of lions' as a test of his faith. A strong degree of Jewish symbolism may, therefore, be distinguished in the choice of 'Daniel in the Lion's Den' for the Na'aran synagogue pavement. At the same time, the presence of this biblical episode clearly casts doubt on Father Vincent's earlier claim that the Na'aran synagogue should be dated to the late third to fourth century AD.

The inclusion of the representation of the Daniel scene suggests that the Na'aran synagogue may have been constructed during the mid-sixth century AD. Indeed, this synagogue has been attributed to the late 'apsidal' type, with a basilical plan including a long nave and an apse facing south towards Jerusalem (Avi-Yonah 1977: 894). Its layout, as well as its interior decoration, is similar to other synagogues that have been assigned to the sixth century, including Beth Alpha and Ma'on, both of which have images of the *Torah* Ark, the zodiac, ritual objects and lions on their floors. Furthermore, the depiction of human figures, particularly in such biblical scenes as 'Daniel in the Lion's Den', became common only after the fifth century AD (Avi-Yonah 1975: 109). Father Vincent, however, had originally dated the synagogue to the late third to early fourth century AD because its basilical plan was comparable to those of the early Upper Galilee synagogues (Benoit 1961: 165). The dating of these early synagogues has since been revised as recent archaeological investigations provide a much better understanding of their architectural origins and development. Given that Father Vincent did not record any archaeological materials such as coins or pottery, which might confirm the date of the Na'aran synagogue and coupled with the fact that the site has never been excavated using modern archaeological methods, the date of the Na'aran synagogue and its mosaic floor remains a matter of conjecture. Although some decorative elements indicate that it may have been built in the sixth century, at the same time, these images could have been laid during ensuing episodes of remodelling or restoration following the completion of the synagogue's construction.

If the Na'aran synagogue mosaic pavement clearly expresses inherently Jewish iconographic themes in its decoration, how could the St. James' Church mosaic fragment be regarded as Christian? In considering this question, it becomes clear that Maitland Woods cannot be held entirely culpable for the misinterpretation of its provenance. His misreading of this mosaic was not solely influenced by his historical and religious preconceptions. Indeed, it appears that some of the Jewish ritual objects represented on the floors of synagogues, such as at Na'aran, were appropriated from pagan and Christian sources and invested with a specific Jewish significance. These include the palm tree, a Christian motif appropriated by Jews as a symbol of fertility, and the eagle, a symbol of the Phoenician God, Baal Shamin (Roussin 1991: 365). Not only did Jews incorporate and adapt pagan and Christian symbols for the decoration of their synagogues, but Christians also used pagan and Jewish emblems in their churches and manuscripts. Seen as a candleholder or source of light, the *menorah* was also a meaningful symbol for Christians who might wish to indicate God. At the same time, the grapevine motif can be seen to embody certain

Christian iconographic themes while holding particular symbolic value for Jews. Like so many other motifs, the grapevine image was part of the plastic iconographic language used throughout the Roman world, and its appearance was ubiquitous in the Byzantine iconographic canon. From this perspective, therefore, we can appreciate that Maitland Woods' understanding of the St. James' Church mosaic fragment as bearing Christian symbols was partly correct.

### **The grapevine image: a shared Judaeo-Christian icon**

The grapevine and its fruit is a common motif in church and synagogue, as well as in domestic mosaic pavements from the third to eighth centuries in Palestine, a design inherited from the standard still-life motifs of Hellenistic and Roman art. As a pagan symbol denoting the Dionysiac theme of the hope of immortality for the soul, it also appears in a Judaeo-Christian context with its association of salvation, resurrection and life after death. Its imagery is used repeatedly throughout the Old Testament as both a plant of creation and as a symbol of God's people or Israel. Psalm 80:8–9 states that: 'Thou hast brought a vine out of Egypt: thou hast cast out the heathen and planted it', while in Hosea 10:1, 'Israel is an empty vine, he bringeth forth fruit unto himself'. The grapevine also appears in the Book of Isaiah 5:1–7: 'My well beloved hath a vineyard and planted it with the choicest vine ... For the vineyard of the Lord of Hosts is the house of Israel and the men of Judah his pleasant plant'. If the vineyard represents the people of Israel in Isaiah, the grapevine is at first an image of the Church, of the figure of Christ himself and of his people in the Gospels of St. John the Evangelist and St. Matthew. Here, the church portrays itself as the new Israel. In Matthew 21:33–41, the parable of the vineyard is recounted: 'There was a certain householder, which planted a vineyard...' and in John 15:7 the vine becomes Christ as Jesus says, 'I am the true vine...'. The grapevine, therefore, can be interpreted allegorically as either God's people or Israel to the Jews or as Christ or the Church to Christians. This motif can also be read in a literal sense. It is of interest to note that grapevines were a common plant in ancient Palestine (as they are today) and were not trained on trellises but were grown along the ground. Given its religious significance, the appearance of the grapevine on the mosaic floors of ecclesiastical structures would have been considered an appropriate image to represent the Earthly Paradise.

The grapevine and its fruit appear on a number of mosaic pavements in Palestine including the Byzantine church at Shellal, the Armenian chapel in north Jerusalem, the Churches of St. Lot and Procopius at Khirbet al-Makhayyat, the Church of St. John and the funerary chapel at Jerash, as well as the synagogues of Beth Alpha, Ma'on, Khirbet Susiya and Rehov among others (Roussin 1991: 364; Henderson 1985: 53; Maguire 1987: 10). As both churches and synagogues feature the image of the grapevine in their decoration, clearly such symbols took on a particular significance when incorporated into the context proper to each religion. A Christian or a Jew would have interpreted this motif according to their own religious context and cultural upbringing. In this sense, the grapevine image, when divorced from its

architectural context and viewed in isolation, can have both Jewish and Christian interpretations.

Given the polyvalent symbolism of the grapevine, it seems that Maitland Woods was not entirely mistaken in his understanding of a potential Christian value of the St. James' Church mosaic fragment. Nor can he be wholly blamed for the cultural and religious misappropriation of this piece. As has been established, Maitland Woods was not an archaeologist but a chaplain whose interests lay in the biblical Christian past of the Holy Land. There is no evidence to suggest that he thoroughly excavated and recorded the site's archaeological remains. Nor would he have had the opportunity to conduct such an investigation in a modern archaeological sense, because it was a time of war and the area around Jericho was, essentially, a front line. Furthermore, Maitland Woods did not view the mosaic pavement in its entirety because, as Major Furber confirmed in his letter to the Académie in Paris, 'it was four to five feet under the earth and there was [*sic*] no traces of walls or any other parts of the edifice' (Picard 1919: 91). This indicates that the synagogue's architecture and its mosaic floor were not fully exposed at the time the fragments were removed by Maitland Woods and the Anzac troops. Maitland Woods did not have access to the excavation report produced by Father Vincent that confirmed the site as the Na'aran synagogue, because it was not published in his lifetime. On the basis of his own historical inquiries, Maitland Woods mistook the site for the Monastery and Church of St. Michael. That the synagogal architecture of Byzantine Palestine shared the same basilical plan as contemporaneous Palestinian churches may have reinforced his understanding of the site as a church, depending on what he was able to see of its plan. Similarly, the numerous iconographic elements that Maitland Woods may or may not have seen in the Na'aran pavement, were shared by both Jews and Christians alike for the decoration of their cultic buildings. In this way, Maitland Woods' specific reading of the mosaic fragment with its grapevine motif is neither unusual nor surprising.

The St. James' Church mosaic fragment is not a Christian icon, but the material embodiment of Jewish (and Byzantine) history, which is, in itself, an essential component of Jewish cultural and religious identity. However, this fragment continues to be misunderstood because its significance as a component of the Na'aran synagogue floor remains largely unknown to the public. Although its recent history is undoubtedly of some interest to the Italian and Palestinian restorers, who have been attempting to trace the whereabouts of the missing pieces of its mosaic pavement, it remains unlikely that the St. James' Church mosaic will be repatriated in a physical sense. Despite its provenance, this mosaic fragment is now part of the church's history, not to mention it being also a part of Australia's heritage as an Anzac 'trophy of war'. Only with further public documentation that clearly states the proper history of the mosaic and its acquisition, may we be able to truly appreciate the mosaic for what it is and for what it represents. Only then may the Australian public come to understand that it is unusually privileged to possess this small, but noteworthy relic from the Holy Land.

## Notes

- 1 Benelli and Hamdan 2004: personal communication.
- 2 This fragment has not been examined by the author and, as such, its character and provenance cannot be commented upon.
- 3 Maitland Woods refers to the observations made by a Russian pilgrim dated to AD 1147 as his main historical source, but there is no literary source material of that date and therefore he must have been referring to the writings of Daniel the Abbot of 1106–1107 (cf. Wilkinson *et al.* 1988).

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## City Foundation Legends in the Decapolis

ACHIM LICHTENBERGER

Louis Robert deserves the distinction of having collected in tireless and knowledgeable fashion the most disparate and remote evidence for the purpose of reconstructing local traditions of cities in the Greek East during the Imperial period.<sup>1</sup> In addition to epigraphic and archaeological sources, Robert above all took into account literary texts and local civic coinage to reach a better understanding of local traditions. During the Roman Imperial period, civic coins ('Greek Imperials') in particular served to cultivate local traditions – traditions that have left virtually no signs or hardly any imprint on the literary record as it has been handed down to us. Thus, coins represent a next to inexhaustible wealth of information regarding the history of individual *poleis*. Indeed, these historical city traditions can only be reconstructed by combining the most diverse groups of evidence.

The historical traditions of cities are frequently linked to the founding of cities. Research on such traditions assumes an especially exciting dimension, when the function of the traditions is put into question. Most of the local traditions are depicted extensively on coins from the Antonine period. However, this does not necessarily mean that the traditions originated in that period. Instead, it is significant that from a functional perspective at this time they were considered so important ('identity fostering') that they absolutely had to be represented on coins. This can be correlated chronologically with the speeches in praise of the city featured during the so-called Second Sophistics, a phenomenon which was promoted by orators during the second and third centuries AD and became widespread in the East of the *imperium Romanum*. On the one hand, these speeches emphasized and praised the Greek origins (*eugeneia*) – sometimes only assumed – of the city, and on the other, certain cultural or topographical aspects such as an advantageous environmental setting.<sup>2</sup>

While the cities were drained of political power by Rome in the Imperial period, at the same time they experienced the *pax Romana*, an era of economic prosperity, which afforded local elites an opportunity to compete with each other. A symptom and at the same time a promoter of this development was the Panhellenion founded by Hadrian and based in Athens. To be accepted into the prestigious Panhellenion, the cities of the East had to prove their *eugeneia*. Although to our knowledge no city in Syria ever belonged to the Panhellenion, this institution contributed to a general climate in which *eugeneia* was regarded as important for cities in the East.<sup>3</sup>

Until now, the examination of the Greek Imperials was largely confined to the cities of Asia Minor, and research on the topic of foundation legends has hardly

been touched upon in the Near East.<sup>4</sup> This can be attributed, among other things, to the fact that there are clearly better sources available for Asia Minor. Despite these gaps it is possible to find some examples of local foundation legends from cities of the Syrian Decapolis. Although in ancient thought it is not possible to distinguish between mythological and historical traditions, we shall concentrate on examples with a myth as a background. Thus traditions that refer to foundations made by Alexander the Great, Hellenistic kings or Romans, which also existed in the Decapolis,<sup>5</sup> will not be discussed in the following.

### Scythopolis, Gadara and the 'Caesarea Cup'

The Decapolis was a group of cities, situated in present-day northern Jordan, Israel and southern Syria. Their administrative organization attached to the Roman province of Syria goes back to the time when Pompey conquered the Near East in 64/63 BC.<sup>6</sup> From the second century AD onwards they were incorporated into the Roman provinces of Syria, Syria-Palaestina and Arabia. The cities were planned on the model of the Greek *polis*.

Most of the information concerning the foundation legends of the cities of the Decapolis come from Nysa-Scythopolis. The inconclusive etymology of the city's name, Scythopolis ('city of the Scythes'), already led in antiquity to varied speculations concerning its origin. The notion that the city was founded by Iphigenia after she fled from Scythia, as Malalas reports in the sixth century AD, may represent a Byzantine aetiology for the name which only tenuously reflected the real circumstances of the cultural setting of the city in Byzantine times.<sup>7</sup> More importantly and far more widespread, is another tradition already transmitted by Pliny. This tradition states that Dionysus buried his wet nurse Nysa at that location and that the town subsequently received its name from her and from Scythians who had settled there: *Scythopolim, antea Nysam, a Libero Patre sepulta nutrice ibi Scythis deductis* ('Scythopolis, formerly Nysa, after Father Liber's nurse, whom he buried there, where a colony of Scythians are settled.')<sup>8</sup> In this way, Pliny intended to explain the names of both Nysa and Scythopolis, whereas the explanation of the latter is largely unintelligible for us in the brief passage handed down by Pliny. The derivation of the name Nysa, however, is given additional support from other sources, such as in an inscription from Scythopolis dated to the time of Marcus Aurelius, which mentions that Dionysus was honoured there as *ktistes*.<sup>9</sup>

However, K. J. Rigsby has convincingly demonstrated that the name Nysa (as well as the region's well-attested names of Antiochia and Seleukia<sup>10</sup>) was a Seleucid dynastic name which at the time of the founding of the city had nothing to do with a local cult of Dionysus.<sup>11</sup> Thus the Dionysus-Nysa tradition (based on the city name) could only have emerged after a Seleucid woman gave her name to the city. She was later replaced in Pliny's own time by the wet nurse of Dionysus. A similar case is known from Nikaia in Bithynia, which was named in the Hellenistic period after the wife of Lysimachus, whose name during the Imperial period, at any rate, was traced back to one of the nymphs.<sup>12</sup>

During the Roman Imperial period, the Nysa tradition associated with the wet nurse was a theme depicted on the coinage of Scythopolis. Thus, the birth of Dionysus from the thigh of Zeus and his being handed over to the wet nurse Nysa is a subject portrayed on coins since the time of Septimius Severus (Fig. 1).<sup>13</sup> The enthroned Kouroutrophos Nysa is then represented along with the boy Dionysus from the time of Caracalla (Fig. 2).<sup>14</sup> Apparently, the Dionysus-Nysa tradition was so dominant in this period that it not only edged out the Seleucid princess Nysa from memory (at least in official memory), but also another *ktistes* of Scythopolis, Gabinius, who was promoted on Late Hellenistic coins as the one who gave his name to the city.<sup>15</sup> This neglect of Gabinius occurred despite the fact that other Decapolis cities precisely at this time began to highlight their Roman benefactors or (re-) founders (Pompey, Philippus, Gabinius) in the images that they cultivated for themselves.<sup>16</sup> In Scythopolis, however, the Dionysus-*ktistes* tradition dominated the coinage in the second and third centuries AD and it may also be found on another coin type of the city.



Fig. 1. Coin of Scythopolis. Obv.: Bust of Gordian III. Rev.: Nysa on left standing opposite Zeus, from whose thigh emerges the infant Dionysus (Staatliche Museen zu Berlin – Preußischer Kulturbesitz. Münzkabinett) [12.89gr; 23/24mm].

This coin type appears at the time of Commodus and shows Dionysus fighting two opponents (Fig. 3).<sup>17</sup> The interpretation of this coin type is subject to discussion. Eckhel saw the small figure to the right of Dionysus as Priapus.<sup>18</sup> Hill explains the scene as Dionysus threatening a primitive idol,<sup>19</sup> and Seyrig even saw the small figure as the boy Dionysus in front of whom perhaps a Scythian Corybant was performing a dance.<sup>20</sup> Jonas recognized the raw meat-eating Dionysus,<sup>21</sup> and Gitler interpreted the scene as a representation of the Anthesteria.<sup>22</sup> But all of these explanations seem improbable. The most convincing interpretation of the coin type is that it is a combat scene in which Dionysus pulls back the head of



Fig. 2. Coin of Scythopolis. Obv.: Bust of Gordian III. Rev.: Seated Nysa suckling the infant Dionysus (Meshorer 1985, 42 No. 110a) [14.60gr; 24/26mm].



Fig. 3. Coin of Scythopolis. Obv.: Bust of Commodus. Rev.: Dionysus fighting two figures (Meshorer 1985, 41 No. 107) [32.83gr; 35/36mm].

one of his charging opponents and is about to attack him with his thyrsus. At the same time, Dionysus' panther is rushing toward a second opponent, a motif which is well known from the Greek Dionysus iconography.<sup>23</sup> Meshorer and Barkay have also interpreted this scene as a combat scene; Barkay thought of a gigantomachy.<sup>24</sup>

The question then arises whether the limits of interpretation have been reached, or whether it is still not impossible to formulate an interpretation with specific reference to the city. To come right to the point, we believe it probably is. Indeed, it is conceivable that the type represents the city's historical tradition which had as its subject a battle scene, preceding the foundation of the city. Moreover, it is highly plausible that Lycurgus was the opponent of Dionysus.

Both Pliny and the inscription already mentioned substantiate that Dionysus was revered as the *ktistes* of Scythopolis. Legends concerning the founding of a city quite often have as their theme the fight of the eventual founder against a monster-like opponent, who inhabited the place prior to the foundation. Underlying this topos, is probably the idea of the civilizing force which accompanies the act itself of founding a city.<sup>25</sup> Both Dionysus and Heracles were founders and fighters of monsters *par excellence*.<sup>26</sup> When considering the pattern of myth concerning the founding of Thebes by Cadmus, a figure who first had to battle a dragon, it is possible to find many examples of battles against monsters prior to the foundation of a city or before, for example, the founding of a sanctuary.<sup>27</sup>

Unfortunately, we are hardly well informed regarding which battle involving Dionysus held special significance for the inhabitants of Scythopolis. The fifth-century AD author Nonnos, who possessed a good knowledge of eastern local traditions,<sup>28</sup> localizes the battle between Dionysus and Lycurgus in Nysa in Arabia. According to him, Lycurgus is king of the Arabs. P. Chuvin made the probable connection, given the reference to Mount Carmel in the text (*Nonn. Dion.* 20,298), that with Nysa, our Nysa-Scythopolis was actually intended.<sup>29</sup> Thus, either the myth transmitted by Nonnos, or another comparable one, might have been associated with Scythopolis. This idea becomes all the more plausible considering that the myth of the battle between Dionysus and Lycurgus reached Syria early on and Arabia was a region where Lycurgus was identified with a local god.<sup>30</sup>

Moreover, with the founding of Damascus, the fight between Dionysus and Lycurgus was brought in connection with a variation of this myth in which the giant Askos battled Dionysus.<sup>31</sup> After all, Lycurgus distinguished himself as a pursuer of the wet nurses of Dionysus,<sup>32</sup> so that also here there is a possible connection to Scythopolis, which was the site of the worship of Nysa, the wet nurse of Dionysus. The Lycurgus myth could have been linked to Scythopolis, since Lycurgus – as the pursuer of the wet nurse – could have murdered Nysa. This act could have been the precursor to that which Pliny reports, namely, that Nysa was buried by Dionysus.<sup>33</sup> On this point, however, we are beginning to cross the safe limits of interpretation surrounding this coin type, since it ultimately cannot be brought into line with the Lycurgus myth. Indeed, it is not possible to identify Lycurgus – who is usually shown armed with a double axe – with certainty as one of the figures on the coin type.<sup>34</sup> Furthermore, no situation is attested within the Lycurgus myth where Dionysus dominates in direct confrontation with Lycurgus. On the contrary, Dionysus is the weaker of the two and, thoroughly frightened, flees to Thetis. Therefore, it appears likely that we are dealing with an unknown local variation of the myth.

A variation of this sort is not unlikely. The myth of the founding of Damascus, for example, in which the giant Askos also fights against Dionysus, establishes that Dionysus did meet opponents other than Lycurgus in such local traditions. This could explain the second figure on the coin type from Scythopolis. At the same time, the local myth from Damascus demonstrates that it was possible to insert non-canonical local episodes into the well-known Greek myth. Thus, the representation of the Lycurgus myth on the coin type from Scythopolis is conceivable. A myth such as this could supply an aetiology for at least one of the two city names (Nysa).



Fig. 4. 'Cup of Caesarea' in the Louvre, Paris. The foundation myth of Straton's Tower (later, Caesarea).

Thus this coin type with Dionysus fighting opponents probably portrays Dionysus before the founding of the city. It fits into an Antonine-Severan visual programme of the city of Scythopolis, which has as its theme the mythologized history of the city of Scythopolis. The coin type has to be seen alongside the representation of the birth of Dionysus and the coin type showing the boy being raised by the wet nurse.

Perhaps the most well-known piece of evidence representing the foundation of a city in the Near East is the 'Caesarea Cup' in Paris, which has been dated to the fourth century AD (Figs. 4–5).<sup>35</sup> It obviously depicts an otherwise unknown foundation myth. While the cup unfortunately is not completely understood in every detail,<sup>36</sup> it does also display the motif of a battle against a savage lion, a monster-like creature taking place before Straton could proceed to establish Straton's Tower, later known as Caesarea (Fig. 5).

Another representation of a battle between a god and a monster-like creature is found on coins of Gadara from the time of Elagabalus (Fig. 6).<sup>37</sup> On these coins, the city god Heracles is depicted fighting a serpent. The representation of the serpent draws upon Hydra iconography, but because the monster has only three heads and Heracles is depicted in cuirass and with a thunderbolt and a dog, the Hydra myth is clearly not intended. In this case, where Heracles is probably the local Heracles-Melqart, it is not possible to elaborate further on the coin type as the textual traditions for Gadara are not as good as those for Scythopolis.<sup>38</sup> Nevertheless, it is possible to assume that the coin type which hitherto has not been convincingly interpreted, represents a battle scene which preceded Heracles founding a city (probably Gadara).

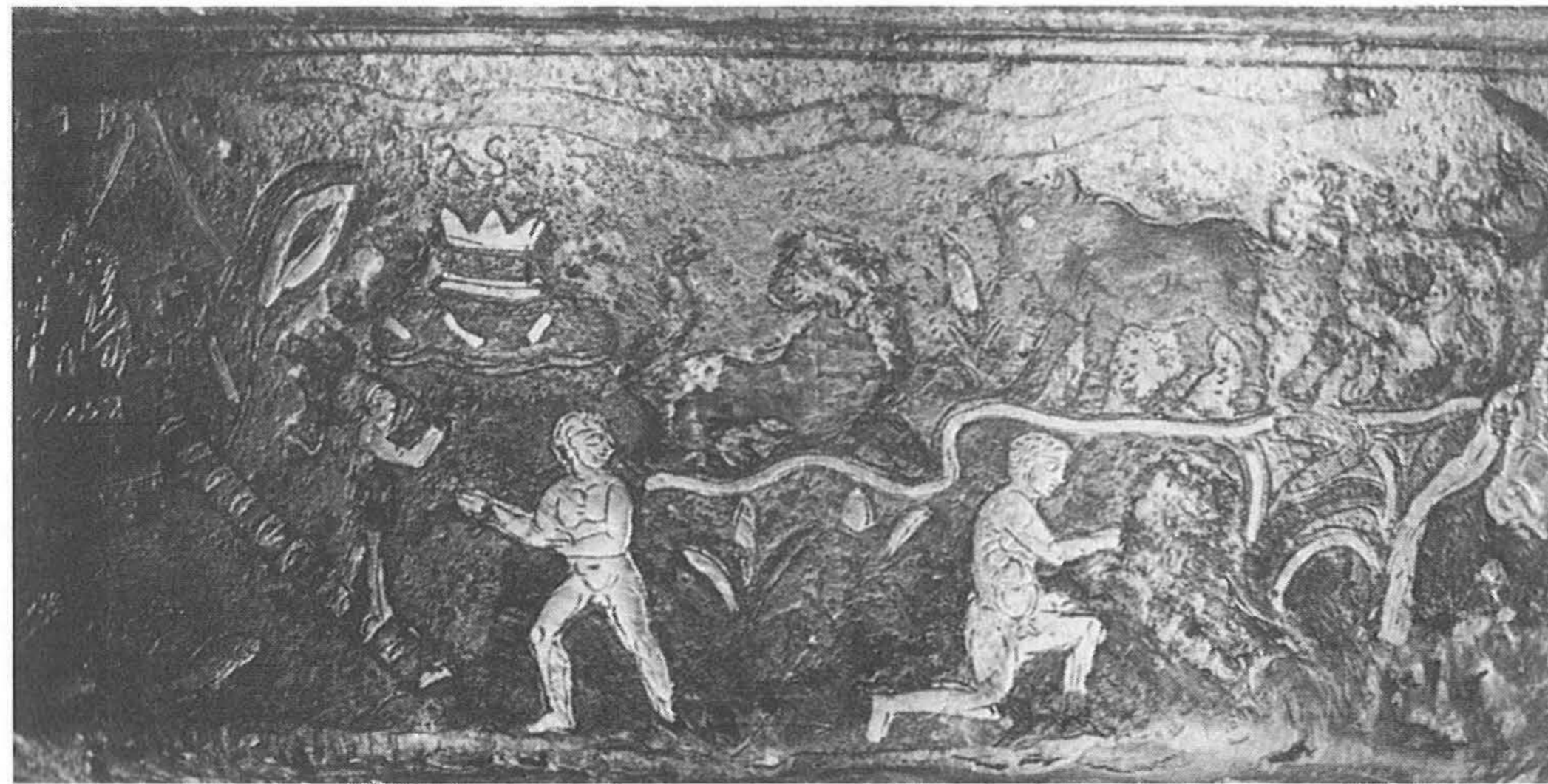


Fig. 5. Detail from the 'Cup of Caesarea' in the Louvre, Paris. Straton leaving ship and fighting a lion.



Fig. 6. Coin of Gadara. Obv.: Bust of Elagabalus. Rev.: Heracles-Melqart fighting a snake-monster (Spijkerman 1978, 150–151, No. 80) [10.24gr; 24/26mm].

**Further evidence for foundation legends of cities of the Decapolis: Hippos and Pella**

At this point it is necessary to touch upon some additional evidence for comparable city foundation legends among the cities of the Decapolis. Since there are only fragmentary sources at our disposal, only a piecemeal reconstruction of these legends is possible.



Fig. 7. Coin of Hippos. Obv.: Bust of Tyche. Rev.: Horse (Meshorer 1985, 74 No. 197) [6.46gr; 17/18mm].

With regard to Hippos: since the first century BC a horse was represented on coins as the city symbol (Fig. 7).<sup>39</sup> From the time of Marcus Aurelius, the horse is superseded by the mythical winged horse Pegasus, which then comes to almost entirely dominate the coinage from the time of Elagabalus (Fig. 8).<sup>40</sup> This development might have occurred parallel to a mythologizing of the city history, and an aetiology of the city name might have brought Pegasus somehow in connection with the naming and, thus, the founding of the city. A similar case is known from Cilician Tarsus.<sup>41</sup> Such a 'do-it-yourself mythology' could become a 'shameless hijacking of well-known myths'<sup>42</sup> by cities which, similar to the Syrian cities, had no Greek past of its own.



Fig. 8. Coin of Hippos. Obv.: Bust of Elagabalus. Rev.: Pegasus (Staatliche Museen zu Berlin – Preußischer Kulturbesitz. Münzkabinett) [16.82gr; 26/28mm].

In the case of Pella: according to Stephanus of Byzantium (*s.v.* Pella), Pella was originally called Boutis. For this strange tradition, there are two possible explanations which are not in contradiction to each other:

- a) Since Stephanus mentions at the same point in the text that Macedonian Pella was called Bounomos, the name Boutis could allude to an exclusive connection with the Macedonian city. This seems probable, because Stephanus is the only author with regard to the Macedonian city who attests to a former toponym within the word field of *bous* ('ox'). Remarkably, Stephanus also relates a structurally similar tradition (*s.v.* Gadara), that in Macedonia there existed an otherwise completely unknown village named Gadara, a toponym, that on the other hand is well attested in the Semitic world.<sup>43</sup> In that case, there could be a similar allusion linking the Syrian city with an invented Macedonian site. Stephanus possibly hands down fragments of foundation myths.
- b) Chuvin supposes that Boutis could refer to an animal that could have accompanied and brought a future founder to the place where he would establish the city.<sup>44</sup> This too is a favourite motif in Greek foundation legends<sup>45</sup> and is also found on the 'Caesarea Cup' (the bull between two lions) (Fig. 5) and possibly on the coins of Gadara (on the right side of Heracles) (Fig. 6). The name 'Boutis' thus might refer to an animal prominent in an otherwise lost foundation myth of Pella.

### The Second Sophistics in the Decapolis

It is obvious, that during the Antonine-Severan period, foundation legends were a well-discussed theme in the images cultivated by the Decapolis cities.

The legends, especially those featuring Greek gods, constitute the building blocks of a Greek past. Indeed, the cities present themselves as being influenced by Greek culture also in the area of the urban landscape and occasionally even on inscriptions where they describe themselves as '*polis Hellenis*'.<sup>46</sup> Josephus also perceived the cities in this way,<sup>47</sup> and the patronage received from Hellenistic, and later from Roman, rulers and officials during the second and first centuries BC may signal this Hellenizing legacy. In a narrower historical context, this orientation toward Greek culture may be attributed to the climate surrounding the Second Sophistics mentioned at the beginning.

Sophists, who were responsible for praising the city as part of their *encomia*, frequently travelled among the cities of the East and gave speeches which addressed topics desired by their local audiences. Occasionally, Sophists would also extol their native cities or rather begin their careers there. With Kerykos of Gerasa and Apsines of Gadara, Sophists are documented for the Decapolis.<sup>48</sup> We know little about the Sophists other than their names. The legends presented here, however, allow us to focus and understand the topics that would have engaged their interest.

### Acknowledgements

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### Notes

- 1 Robert 1940–65; 1975; 1977.
- 2 Bowersock 1969; Weiss 1984: 179–195; Strubbe 1984–86: 253–304; Anderson 1993; Scheer 1993; Lindner 1994; Schmitz 1997. See also Geiger 1990: 141–150; 1994.
- 3 On the Panhellenion: Oliver 1970: 92–138; Romeo 2002: 675–684.
- 4 Both Chuvin (1988; 1991) and Leschhorn (1991) have dealt with city foundation legends in the Near East.
- 5 On this Lichtenberger 2003: 315–320.
- 6 On the Decapolis: Bietenhard 1977: 220–261; Isaac 1981: 67–74; Lichtenberger 2003: 6–20.
- 7 Malalas 139 f.
- 8 Pliny, *NH*, 5.16.74.
- 9 Di Segni 1997: 139–161; Di Segni, Foerster and Tsafirir 1999: 59–75.
- 10 Kindler 1978: 51–55.
- 11 Rigsby 1980: 238–242.
- 12 Robert 1977: 12–16; Merkelbach 1987: 10, 16–25, 34–41.
- 13 Hill 1965: 77, No. 11; Rosenberger 1977: 32, No. 27, 34; 39–40, No. 38, 63; Spijkerman 1978: 194–195, No. 23; 200–205, Nos. 40–41, 57; Meshorer 1985: 42, No. 112; Barkay 2003: 210, No. 39; 216–217, No. 59; 218, No. 65; 229, No. 93.
- 14 Hill 1965: 76–77, Nos. 6–10; Rosenberger 1977: 34–37, Nos. 38, 41–42, 46, 54–58; Spijkerman 1978: 198–207, Nos. 32, 46–48, 58; Meshorer 1985: 42, No. 110; Barkay 2003: 214, No. 50; 217, No. 60; 219, No. 66; 222, No. 74; 224, Nos. 79–84.
- 15 Cf. Barkay 1994/99: 54–62.
- 16 On this aspect, see: Lichtenberger 2003: 346–348.
- 17 Seyrig 1962: 210; Hill 1965: 77, Nos. 12–13; Rosenberger 1977: 34–37, Nos. 43–45, 59–61; Spijkerman 1978: 194–207, Nos. 21, 42–45, 59; Meshorer 1985: 41, No. 107; Barkay 2003: 203–204, No. 19, 207, No. 29, 211, No. 41, 218 No. 62, 221 No. 72, 222–223 No. 75, 226–228 Nos. 85–91.
- 18 J. Eckhel, 1828. *Doctrina numorum veterum*, III. Vienna. 439.
- 19 Hill 1965: XXXVI.
- 20 Seyrig 1962: 210.
- 21 Jonas 1960: 1–8.
- 22 Gitler 1991: 23–33.
- 23 Cf. Gasparri, 1986. *LIMC* III: 474–476 Nos. 613, 617, 623, 628, 632, *s.v.* Dionysos.
- 24 Meshorer 1985: 41, No. 107: 'Dionysos waving thyrsos at fleeing figures'; Barkay 1993: 372; 2003: 118–121.
- 25 Fontenrose 1959: 465.
- 26 For Heracles as the fighter of monsters and the bringer of culture: Fontenrose 1959: 321–364; Lacroix 1974; Burkert 1992. On the civilizing aspect of Dionysus: Bruhl 1953: 134, 136; and on his travels which predestined him for local myths: Bowersock 1990: 41–42.
- 27 For battles against monsters which preceded the foundation of a city: Vian 1963: 101–102. See also Burkert 1992: 115–127, and Fontenrose 1959.
- 28 Robert 1975; Bowersock 1990: 41–53; Chuvin 1991; 1994: 167–176.

- 29 Chuvin 1991: 258–264. On the countless other localizations of the mythical Nysa: Stein *et al.* (1937) *RE* 17 (1937) 162–165 s.v. Nysa.
- 30 Sourdel 1952: 81–84; Knauf 1990: 176–180.
- 31 Steph. Byz. s.v. Damaskos; Etym. m. s.v. Damaskos. In addition K. Tümpel, *RE* 4 (1896) 1701, s.v. Askos.
- 32 T. Heinze, *Der Neue Pauly* 7 (1999) 578, s.v. Lykurgos.
- 33 In contrast: Chuvin 1991: 263.
- 34 On the Lycurgus iconography: Farnoux, *LIMC* VI (1992) 309–319, s.v. Lykurgos I.
- 35 Will 1983: 15, 19–20.
- 36 Also on the ‘Caesarea Cup’: Finkielsztejn 1986: 419–428.
- 37 Meshorer 1976: 64, No. 89; 1985: 83, No. 226; Spijkerman 1978, 150–151 No. 80.
- 38 On this: Lichtenberger 2003: 89–95.
- 39 Spijkerman 1978: 170–171 No. 2; Meshorer 1985: 74 Nos. 197–198.
- 40 The first representations of Pegasus on coins from Hippos are known from the time of Titus (Spijkerman 1978: 170–171, No. 3) and alongside the horse under Domitian (Spijkerman 1978: 170 f., No. 5). From the time of Marcus Aurelius onwards, only Pegasus is depicted on coinage (Spijkerman 1978: 172–179, Nos. 12, 20, 26, 28, 35–39).
- 41 Scheer 1993: 284–286.
- 42 Hollis 1994: 153.
- 43 Mershen and Knauf 1988: 128–145.
- 44 Chuvin 1988: 106.
- 45 For such animals: Strosetzki 1954: 47 ff.; Vian 1963: 76–93; Gierth 1971: 87 ff.; Leschhorn 1991: 445–446.
- 46 Foerster and Tsafirir 1986/87: 53–58.
- 47 *AntJ* 17,320.
- 48 On Apsines, Bowie, *Brill’s New Pauly* 1 (2002) 914, s.v. Apsines. On Kerykos, Steph. Byz. s.v. Gerasa. See also Geiger 1994.

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#### Abbreviations

LIMC	Lexicon iconographicum mythologiae classicae
RE	Paulys Realencyclopädie der classischen Altertumswissenschaft
INJ	Israel Numismatic Journal
ANRW	Aufstieg und Niedergang der römischen Welt
BEFAR	Bibliothèque des Ecoles françaises d'Athènes et de Rome
SCI	Scripta Classica Israelica
RB	Revue biblique
SchwNumRu	Schweizerische numismatische Rundschau
ZPE	Zeitschrift für Papyrologie und Epigraphik
BASOR	Bulletin of the American Schools of Oriental Research
ADPV	Abhandlungen des Deutschen Palästina-Vereins
ZDPV	Zeitschrift des Deutschen Palästina-Vereins
TAPhA	Transactions and Proceedings of the American Philological Association
JSav	Journal des savants
HSCP	Harvard Studies in Classical Philology
BAH	Bibliothèque archéologique et historique
AncSoc	Ancient Society
WürzbJb	Würzburger Jahrbücher für die Altertumswissenschaft

## The 'James Ossuary on Trial': a Short Rejoinder

ANDRÉ LEMAIRE

I was surprised to read Emile Puech's contribution entitled 'James the Just, or Just James? The "James Ossuary" on Trial' in a recent issue of the *Bulletin* (*BAIAS* 21 [2003]: 45–53), which is an updated version of articles he had previously published in the magazine *Minerva* (14/1 [2003]: 3–4) and *Minerva-Online* (Jan/Feb. 2003), in *The Polish Journal of Biblical Research* (*PJBR* 2/1, Dec. [2002]: 7–21), as well as in *Verbum Vitae* (3 [2003]: 269–276). I was also surprised by the comments made by Shimon Gibson in his 'Editorial' which reported on unfounded rumours made by Orit Peleg and Rafi Lewis, which have, following an investigation, turned out to be nothing more than a misunderstanding of what Oded Golan, the owner of the James Ossuary, told them. I would also like to add that Gil Chaya, who is also mentioned in the Editorial, has written to me saying that he never saw or heard about the James Ossuary 'before it became the subject of international news'.

Unfortunately, the *Final Report of the Examining Committee for the Yehoash Inscription and James Ossuary* prepared by Uzi Dahari and others for the Israel Antiquities Authority, which was published on the internet at [www.bibleinterp.com/articles/final\\_reports](http://www.bibleinterp.com/articles/final_reports), also contains a confusion between two very different inscriptions, as well as unfounded rumours, contradictions, prejudices and premature generalizations, as I have shown in my detailed 'Critical Evaluation of the IAA Committee Reports Regarding the Ossuary Inscription' that appeared in *PJBR* (2/2, Oct. [2003]: 29–60) and in a more popular fashion in *Biblical Archaeology Review* (*BAR*) (29/6 [2003]: 50–59, 67, 70). I do not intend to repeat here my detailed arguments and invite those interested to read my scientific assessment of the Committee's report mentioned above.

As for Puech's paper in *BAIAS* 2003, I have already answered his earlier French version in *PJBR* 2002 (see above) in the same journal (*PJBR* 2/2 [2003]: 81–87). I expect that readers have understood that Puech's main argument is that the Aramaic 'ah and the Greek *adelphos* should be interpreted not as meaning 'brother' but 'cousin', following the interpretation by St. Jerome which has often been repeated in Catholic tradition. However, even Catholic scholars would qualify this interpretation as 'venerable' and one that should not be followed nowadays, especially since it lacks a serious philological basis, as has been demonstrated, for example, by François Refoulé, former Director of the Jerusalem École Biblique, in

his book *Les frères et soeurs de Jésus* (Paris, 1995). Although Puech did not note in his articles any epigraphic problem, subsequently, at the Atlanta SBL meeting (November 2003), he attempted to spread anonymously the rumour that, a few years ago, he had seen the ossuary, but without the second part of the inscription 'brother of Jesus'. This contradicted his former numerous articles on the ossuary as was shown by the Editor of *Biblical Archaeology Review* Hershel Shanks ('Lying Scholars?', *BAR* 30/3, May/June [2004]: 48–52, 61). That Emile Puech was the source of this rumour was confirmed by Professor Eric Meyers in a lecture given at Cornerstone University (Michigan) in May 2004.

Unfortunately, all this shows that concerted attempts are being made to manipulate the mass media and it has nothing whatsoever to do with scientific debate. I therefore urge readers of *BAIAS* to acquaint themselves with the scientific and popular debate as it appears on the website of Cornerstone University: [www.studyantiquity.org/guests/lecture-series/2004](http://www.studyantiquity.org/guests/lecture-series/2004).

In regard to the scientific discussion, it is clear that the inscription on the ossuary raises no palaeographic, linguistic, or orthographic problems. The only serious debate concerns the material aspect of the inscribed surface, and here, two interpretations are possible, as even Yuval Goren and Avner Ayalon have recognized in their report that was recently published in the *Journal of Archaeological Science* (31 [2004]: 1185–1189, spec. 1185): 'the entire inscription postdates it [= the varnish] OR it has been cleaned by a sharp tool after its deposition'. Similarly, cleaning with warm water may explain the differences in the percentages of oxygen isotopes 16 and 18. This problem of interpretation was discussed recently by J.A. Harrell in *BAR* (30/1, Jan./Feb. [2004]: 38–41), and by H. Shanks in *BAR* (30/2, March/April [2004]: 44–47). In fact, two earlier independent examinations, one by Avner Ayalon and Shimon Ilani (Geological Survey of Israel) and the other by Edward J. Keall (Royal Ontario Museum), had already concluded in favour of an 'over-cleaning of part of the inscription', as well as the fact that 'it is clear that the inscription is not a modern forgery', (see 'New Tests Bolster Case for Authenticity', *BAR* 29/4, July/August [2003]: 52–57, 70, spec. 70).

## Unit A at the Nabi Salah Site in Southern Sinai: Facts Contra Text

ITZHAK BEIT-ARIEH

Benjamin A. Saidel in his article 'More than Meets the Eye: a Reappraisal of the Occupational History of Unit A at Nabi Salah in Southern Sinai' that was published in a recent issue of *BAIAS* (19–20, 2001–2002), deals with three aspects based on my preliminary report on the excavations at Nabi Salah (Beit-Arieh 1974), which are, in his words, 'alternative interpretations of the installations, stratigraphy and chronology of Unit A.'

Before I relate to each one of these three aspects, I should like to point out that in his article Saidel only refers to my preliminary publication of the work covering the results of the first two excavation seasons made at the site. The author could also have read about the results of an additional third excavation season conducted at Nabi Salah (November to early December 1972), with a detailed description there of Unit A, that was published in Hebrew in my doctorate (Beit-Arieh 1977). My recent book, *Archaeology of Sinai: The Ophir Expedition* (2003), published by the Institute of Archaeology of Tel Aviv University, includes the results of all my researches on Early Bronze II sites in southern Sinai, as well as on additional topics, though, admittedly, it appeared only subsequent to the publication of Saidel's article.

It should also be noted that the site of Nabi Salah consists of additional units that were excavated and published by our expedition; these units are an integral part of the entire site and cannot be discussed separately. To analyse Unit A all by itself without referring to the other units, is in my opinion a serious methodological error when one is conducting archaeological research.

I shall now discuss each one of the three aspects mentioned by Saidel.

### Installations

On the eastern edge of Unit A my expedition uncovered a series of small, stone-paved subsidiary rooms (i.e. compartments). Most were further subdivided into even smaller spaces. These compartments bordered the central courtyard of Unit A to the east (Fig. 1) and in my previous publications I defined them as work and storage spaces based on the finds made in the field (see below) and on the very low walls. Saidel, however, suggests that these compartments were used as 'sheep pens' and especially

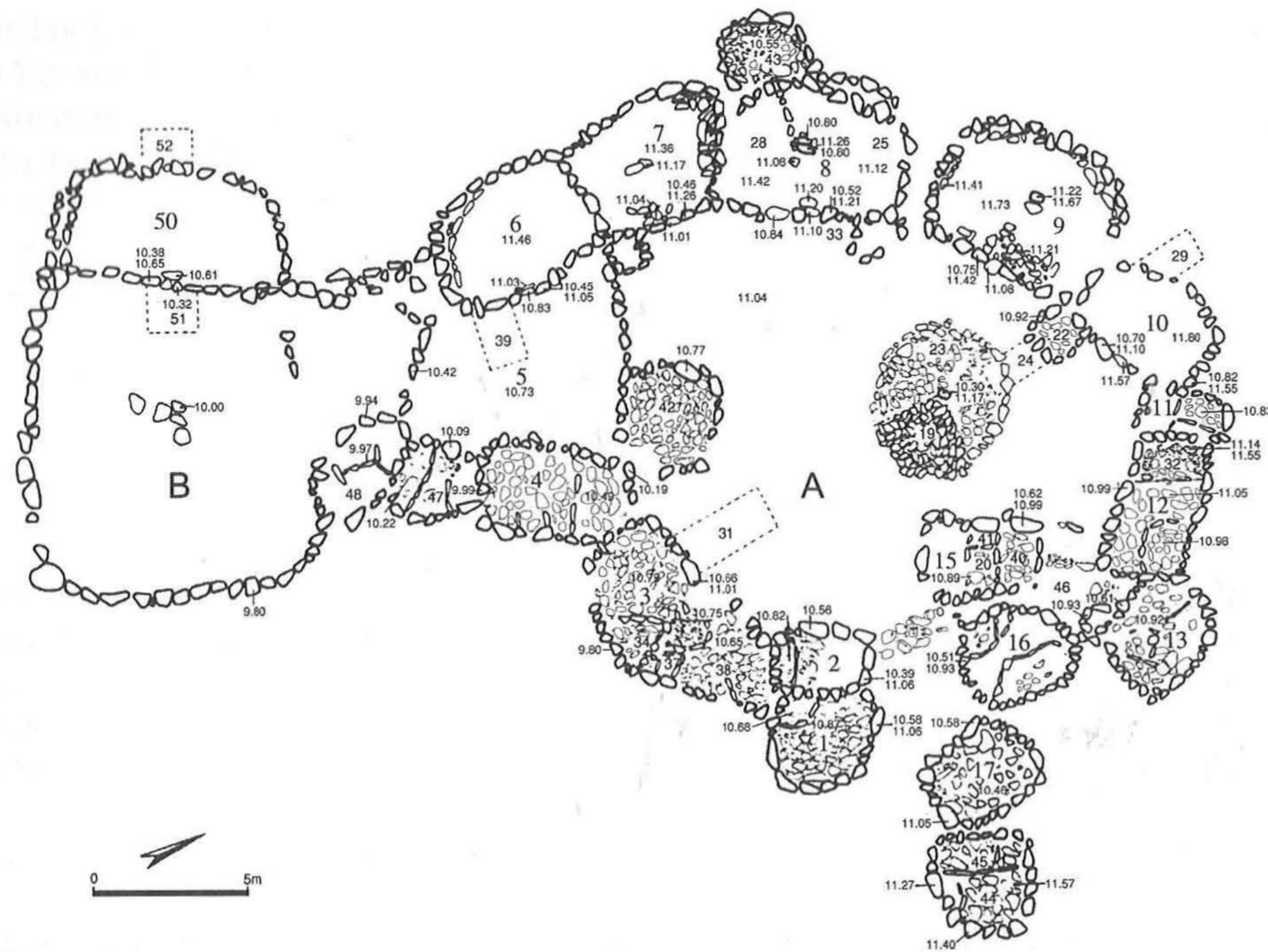


Fig. 1. The Nabi Salah site.

for young sheep that were separated, he claims, from the main herd. I find this suggestion unacceptable and one that is not at all supported by archaeological finds.

First, it is not customary to separate lambs from their mothers or generally even from the herd, as one may see if one observes the sheep herds tended by present-day Bedouin. Second, the floors of sheep pens must be made of earth, as I have myself observed in many Bedouin sheep pens during my visits to their encampments; they were certainly not paved with a hard substance such as stone. Third, it is clear that had the author properly consulted the final report of my excavations in Sinai, he would not have made this suggestion since most of the compartments were found to contain pottery of various types, while in others we found objects that would never, I suspect, have been kept in sheep pens. For example, in Compartment 3, shell 'buttons' were discovered of a type known from other parts of the site (see Beit-Arieh 1977: 16–19; 2003: 27), while in Compartment 32 pure splattered copper was discovered reflecting metallurgical activities that took place at the site (1977: 18; 2003: 196–208). In Compartment 45 a fragment of a copper vessel was found, shells were recovered in Compartments 1 and 17, and, finally, in Compartment 42 a grinding stone was discovered (1977: 19–20). A similar picture has also emerged from the excavations of Unit A at Sheikh Mukhsen (Beit-Arieh 2003: 40–41).

It should be noted once again that most of the compartments were found partitioned into even smaller spaces, as one can see from the plan of the site (Fig. 1). In each space there was sufficient room to store only a few ceramic vessels or some other small objects. Furthermore, the general organization of the small, low-walled compartments on either side of the entrance to the central courtyard and along its eastern side, is not at all compatible with the separation of lambs, since their isolation at the far end of Unit A would have left them exposed to the attack of predatory animals.

Moreover, it should be added that at Canaanite Arad similar stone 'platforms' were found and these were subsequently defined by the excavators as the lower parts of silos (Amiran 1978:17). No suggestion whatsoever was proposed that these could have in any way functioned as sheep pens, even though sheep were evidently being raised by the local population at that time, and the same holds true I believe for many of the EB II and MB I sites investigated in the Negev Highlands (Cohen 1999).

I have no doubt that the small subsidiary rooms (= compartments) in Unit A at Nabi Salah functioned as work and storage areas. On the other hand, Unit C at Nabi Salah perhaps functioned as a sheep pen, as I have already suggested, since its plan would have been appropriate for such a function (Beit-Arieh 1977: 24; 2003: 26).

### Stratigraphy and chronology

In two of the rooms of Unit A we found archaeological evidences for a change in function and for the addition of an architectural element. In Room 12, in which, we surmise, there was originally a sunken dwelling room, standing stones and a fill of earth were added to raise the floor level, and then it was paved with stones, thus changing its function. In Dwelling Room 7 a wide stone bench was added to the left of the entrance and it was built upon the original thin ashy floor of the room.

These changes took place during the continuous occupation of the site and it must be stressed that all the EB II sites in southern Sinai (including Nabi Salah) were single-period and one-stratum sites according to the archaeological criteria as set forth in the final report (Beit-Arieh 2003).

It should also be noted that within all the dwelling rooms a thin layer of ashes had accumulated on the floors and contained finds. It should be emphasized that the accumulation of ashes in the dwelling rooms and courtyards does not represent a destruction layer such as one finds within the settlement strata of tells.

Saidel's stratigraphic theory has been built upon this accumulation of ashes with his suggestion that it represents the refuse of an earlier occupation layer. This suggestion is unrealistic from the stratigraphic point of view, as not one single artefact was recovered from an earlier period. An additional argument against this theory is that in this area, as has been revealed in the survey, the only sites that are earlier than the EB II date from the fifth to fourth millennia BC (Beit-Arieh 2003). There is no possibility whatsoever that the ashes could have remained on the floors for over 1000 to 1500 years! Undoubtedly, the ashes were an integral part of the floors of the rooms, as anyone can testify who has excavated in the highlands of

southern Sinai where the temperatures can fall so low almost every day throughout the year, and especially during the winter, spring and autumn months, that fireplaces have to be lit for essential heating (and cooking) purposes. Such hearths were found in almost every dwelling room, interestingly always left of the entrance, as at Arad. The large quantity of ashes that were left by these fires can now be fully understood following experiments we conducted in one of the rooms: a hearth with a diameter of about 40 cm that was left burning for three hours and fuelled by tinder collected from the proximity of the site, produced one full bucket of ashes (Beit-Arieh 1977: 83, 204, note 16).

The occupants of the rooms apparently did not remove the ashes regularly as they could also be used – as our experiments were able to show – to warm up the cold surfaces of the natural *arkose* stone floors, as well as to provide a soft flooring to lie upon. There is no archaeological evidence to support Saidel's imaginary suggestion (2001–2002: 25) that 'when one architectural unit became unsuitable for human occupation, it was abandoned and a new residential unit was constructed not far away', and looking at the general plan of Nabi Salah and comparing the sizes of Unit A with Units D and F, one has to rule out this idea.

Saidel quotes from my article (1986: 27) saying that one of the reasons for the difficulty in dating desert sites is the meagre number of finds. This is certainly so, but he does not refer to the larger sites that we have excavated (and published) from which we have recovered greater numbers of finds. Prominent among these finds are items of pottery, flint and other small artefacts which do not present any difficulty in dating them to the EB II. Thousands of such artefacts, including those from Unit A at Nabi Salah, are presented in Chapters 4 and 5 of my book (Beit-Arieh 2003).

Difficulties in dating do indeed arise in regard to the hundreds of ephemeral sites of a temporary nature, such as shallow campsites, which have become badly eroded over the years and in which almost no finds have survived (see Beit-Arieh 2003: 274–275 and Chap. 14). Unfortunately, the reference made on this matter by Saidel is irrelevant to the discussion at hand.

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## A Response to Beit-Arieh's 'Unit A at the Nabi Salah Site in Southern Sinai: Facts Contra Text'

BENJAMIN ADAM SAIDEL

I welcome Beit-Arieh's critique (this volume of *BAIAS*) of an article I published in a previous issue of *BAIAS* (19–20, 2001–2002). I am responding, however, to issues in his reply with which I disagree and to others where he either misunderstood my position or may even have misrepresented it. Perhaps my ideas may not have been as clearly stated as they should have been. Therefore, I will use this opportunity to rectify this situation.

### Methodology

Before I address these issues, it is necessary to underscore the methodological gulf that exists between Beit-Arieh and me. Our approach to the occupational history of sites, such as Unit A at Nabi Salah, is a completely different one. Beit-Arieh's interpretation of the occupational history of Unit A at Nabi Salah is clearly represented in his discussion of the modifications made to the architecture in Room 7 and Locus 12 (Beit-Arieh 2004), as follows:<sup>1</sup>

These changes took place during the continuous occupation of the site and it must be stressed that all the EB II sites in southern Sinai (including Nabi Salah) were single-period and one-stratum sites according to the archaeological criteria as set forth in the final report.

This statement underlies his belief that these sites were continuously occupied during the EB II, that is to say from 3100 to 2650 BC, or for approximately 450 years.<sup>2</sup> In contrast, my working hypothesis is that most EB II sites in the Negev and Sinai were used on a seasonal and episodic basis (Saidel 2001–2002: 25–26; 2002: 58, 60). My methodological approach, for both fieldwork and publications (Saidel 2002: 40), has been influenced by the projects and publications of Rosen (1992; 1993) and Bar-Yosef and associates (e.g., Goren-Inbar 1993), who have conducted research in the Negev and Sinai, respectively. These studies emphasize the seasonal nature of arid zone settlements in the Negev and Sinai.

Because of Beit-Arieh's methodological approach he criticizes me for writing solely about Unit A at Nabi Salah:

It should also be noted that the site of Nabi Salah consists of additional units that were excavated and published by our expedition; these units are an integral part of the entire site and cannot be discussed separately. To analyse Unit A all by itself without referring to the other units, is in my opinion a serious methodological error when one is conducting archaeological research.

There are a number of reasons why I regard Beit-Arieh's criticism as irrelevant to my study. First, at Nabi Salah the majority of architectural units are independent constructions situated at various locations in the Wadi el-Sheikh (Beit-Arieh 2003: 11–12, 13 Fig 2.3). For example, Units A and B are not physically or stratigraphically connected with Units C, D, E, and F. Second, Beit-Arieh did not employ a systematic sampling strategy at Nabi Salah, which therefore impedes comparisons of occupational history and artefact distribution within the various independent architectural units. Third, Beit-Arieh never established chronological contemporaneity for these architectural units, other than broadly assigning them to the EB II period. That is to say, he never demonstrated if all the architectural units at Nabi Salah were constructed, inhabited, and abandoned at the same point in time. Fourth, he did not collect enough radiocarbon dates to ascertain when these architectural units were constructed, occupied, and abandoned within the EB II period. The usefulness of radiocarbon dates for dating arid zone settlements and for establishing diachronic changes in settlement patterns within the Negev and Sinai has been demonstrated by a number of archaeologists (Sebbane *et al.* 1993; Avner, Carmi, and Segal 1994). Fifth, the subject of my article was an investigation of the stratigraphy and occupational history of Unit A based in large part on the pattern of ash unearthed at this site. The purpose of my study was not to establish the occupational and chronological contemporaneity of all the architectural units at Nabi Salah.

### Installations

Beit-Arieh spends a considerable portion of his response to my article by focusing on the functions of the installations in Unit A. This issue actually comprises only a minor point in my publication (Saidel 2001–2003: 23). In particular, he disagrees with my suggestion that the granite paved installations in Unit A 'functioned as stables for young sheep and/or goats' (Saidel 2001–2002: 23). Beit-Arieh's rejection of my interpretation is based on two lines of evidence: archaeological data and personal observations of contemporary Bedouin in southern Sinai. I do not doubt that in some cases the installations were used for the storage of material culture, as mentioned by Beit-Arieh (2004). Beit-Arieh, however, forgets to take into consideration that the artefacts found in these *loci* represent the last activities or functions associated with these spaces, and that they may have been used for multiple purposes.

Aside from archaeological data, to counter my suggestion that the installations in Unit A could have been used as animal pens, Beit-Arieh refers to his personal observations of the Bedouin in southern Sinai:

Second, the floors of sheep pens must be made of earth, as I have myself observed in many Bedouin sheep pens during my visits to their encampments; they were certainly not paved with a hard substance such as stone.

Beit-Arieh's methodological approach towards his collection and use of ethnographic data is flawed on two levels. First, he assumes, without explaining why, that the behaviour of the Bedouin in southern Sinai from 1971–1982 can be used as a direct analogue for the behaviour of populations who lived in this region during the Early Bronze II (3100–2650 BC). Specifically, Beit-Arieh implies that the practice of animal husbandry, including stabling, has not changed from the Early Bronze Age to the 1970s/1980s, when he conducted his archaeological investigations there. Second, his 'visits' to Bedouin tents do not constitute in my opinion reliable ethno-archaeological fieldwork. Beit-Arieh does not mention how many visits were made to the Bedouin, over what period of time these observations were made, and during what season(s) he conducted this research. Instead, his comments concerning the animal pens of the Bedouin seem to have been made in the course of casual (perhaps social) visits to Bedouin encampments.<sup>3</sup>

### Ashes

Another line of argument used by Beit-Arieh to undermine my theory is based on the 'experiments' he conducted within an unspecified broadroom:

The large quantity of ashes that were left by these fires can now be fully understood following experiments we conducted in one of the rooms: a hearth with a diameter of about 40 cm that was left burning for three hours and fuelled by tinder collected from the proximity of the site, produced one full bucket of ashes.

Unfortunately, the use of the term 'experiments' in this context is extremely ill-defined. The footnote provided by Beit-Arieh (1977: 204, note 16) refers the reader to his PhD dissertation which provides absolutely no information about the context of these 'experiments'. Specifically, Beit-Arieh does not state the time of year, nor the number of experiments which were carried out (Beit-Arieh 2003: 106, Fig. 3.6). He does not mention if the broadroom where these tests were conducted was roofed or open to the sky. Presumably, if the broadroom structure was roofed for the experiment it would have influenced how fast the fuel was consumed by the fire. Moreover, the size of the fires inside these dwellings would have been of limited size in order to avoid setting the roof on fire, as the roofs of the broadrooms in Unit A and at similar broadroom sites were made of 'layers of branches and twigs mixed with clay' or of goat skins (Beit-Arieh 2003: 104).

Based on data contained in Beit-Arieh's final publication, I see no similarity between the results of his experiments and the archaeological evidence from the excavated broadrooms in southern Sinai. One of the most important points is that in his experiments he created circular hearths, whereas in his excavations he unearthed hearths which 'were basically trench-like pits dug into the floor' (Beit-Arieh 2003: 103). These two types of hearths may have functioned very differently in terms of how fuel was consumed, something not considered by Beit-Arieh.

Beit-Arieh dismisses as an 'imaginary suggestion' my interpretation that Unit A was abandoned by its occupants when the broadrooms became 'unsuitable for human occupation' (Saidel 2001–2002: 25). He believes that most of the ashes generated by these hearths were intentionally left inside the broadrooms by their occupants to function as a form of insulation. Objections to Beit-Arieh's interpretation that the ash functioned as insulation, have already been discussed in my previous publication and therefore I shall not reiterate them here (Saidel 2001–2002: 23, 25).

#### Preliminary or final publications

In his response, Beit-Arieh criticizes me for using data contained in his preliminary publication (1974). Unfortunately, it is a fact that Beit-Arieh's preliminary report (Beit-Arieh 1974: 150–151) provides more detailed information on the excavations in Locus 12 than in his final publication (Beit-Arieh 2003). This is demonstrated by the sherd from a holemouth vessel with a potter's mark, which was found in the fill beneath the floor of Locus 12 (Beit-Arieh 1974: 150–151). This sherd is important as it has been used as evidence for establishing cultural ties and chronological contemporaneity between Unit A and Tel Arad during the EB II period (Amiran, Beit-Arieh and Glass 1973: 194, Plate 50: B–C; Beit-Arieh 1983: 43, Fig. 3: 3, 44; Stager 1992: 23, Fig. 7: 32). Beit-Arieh's preliminary report (1974: 150–151) mentions that this artefact was found in the fill under the floor of Locus 12. In contrast, there is no reference to this artefact in the portion of the final report that describes the excavations in Locus 12 of Unit A (Beit-Arieh 2003: 20). Furthermore, in the pottery chapter (Beit-Arieh 2003: 110–128) of his final publication (2003: 112, Fig. 4.2: 5) Beit-Arieh writes that this sherd was found at 'Nabi Salah', but nowhere does he mention where this sherd was unearthed. That is to say, was this artifact found in Units A, B, C, D, E, F, G, H, J or K at Nabi Salah (Beit-Arieh 2003: 110, 112, Fig. 4.2: 5)? It is also a fact that the pottery chapter in his final report is bereft of *loci* lists for the illustrated ceramics, so it is almost impossible to ascertain the location of these artefacts within the settlements excavated by Beit-Arieh.<sup>4</sup> In summary, Beit-Arieh's preliminary publication (Beit-Arieh 1974), which I cited, actually provides more information on the stratigraphic context of this pottery sherd, than what may be found in his final publication (2003).

#### Conclusion

Beit-Arieh appears to have missed the point of my article and in his response he has misrepresented it. Concerning my interpretation of the ashy deposits in Unit A he writes:

Saidel's stratigraphic theory has been built upon this accumulation of ashes with his suggestion that it represents the refuse of an earlier occupation layer. This suggestion is unrealistic from the stratigraphic point of view, as not one single artefact was recovered from an earlier period. An additional argument against this theory is that in this area, as has been revealed in the survey, the only sites that are earlier than the EB II date from the fifth to fourth millennia BC (Beit-Arieh 2003). There is no possibility whatsoever that the ashes could have remained on the floors for over 1000 to 1500 years!

The problem with Beit-Arieh's statement is that I never suggested or implied in my publication that the ashy deposits in Unit A are 'from the fifth to fourth millennia BC'. Instead, I argued that the finds of ash in the broadrooms and in the courtyard of Unit A, are evidence that this architectural unit was constructed on top of a pre-existing midden which is most likely to be attributed to the EB II (Saidel 2001–2002: 23). Additional evidence for this reinterpretation was his excavations in Locus 12, where a holemouth vessel with a potter's mark was found in a fill underneath the floor of this locus, as I pointed out:

Beit-Arieh, however, did not take into consideration the context of sherd in his dating of Unit A. The artefact was found in the gravel and ash matrix underneath the granite floor in Installation 12, thus providing an EBII *terminus ante quem* for this matrix. This could be interpreted as evidence either of at least two phases of occupation in the EBII or that the architecture in Unit A actually dates to the EBIII. The former is more likely than the latter. (Saidel 2001–2000: 23). Therefore, I would reconstruct this site as evidence for multiple short-term occupations within the course of the EB II period.

The weakest part of Beit-Arieh's response to my article, and my main criticism of his fieldwork methodology, is his inability to demonstrate the stratigraphic relationships between the ashy layers present in the broadrooms and in the courtyard of Unit A. Resolving these stratigraphic relationships is problematic as the descriptions of his excavations in this architectural unit, as set forth in his preliminary report (1974), PhD thesis (1977), and in the final publication (2003), are devoid of section drawings. As a result of the information presented above, I maintain the position that I first articulated in my initial publication.

#### Notes

- 1 In his preliminary report Locus 12 is identified as Room 12 (Beit-Arieh 1974: 150–151).
- 2 Following the dates given by Stager for the EBII period (1992: 40–41).
- 3 For a summary of ethno-archaeological fieldwork carried out by Bar-Yosef and his associates in the same region, and at roughly the same time as Beit-Arieh's archaeological fieldwork, see: Goren-Inbar 1993.
- 4 In his chapter discussing the excavation of sites, such as Sheikh Mukhsen, occasional reference is made to the existence of large vessels, such as *pithoi*, which were found sunken into the floors of broadrooms (Beit-Arieh 2003: 35, Fig. 2.31, 2.32).

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## Review Article

## The Assyrian, Babylonian and Persian Periods in Palestine

PETER JAMES

**Ephraim Stern**, *Archaeology of the Land of the Bible. Vol. II: The Assyrian, Babylonian and Persian Periods 732–332 B.C.E.* Doubleday: New York/London, 2001.

This is the second volume in Doubleday's series on the 'Archaeology of the Land of the Bible'. As one would expect from the Anchor Bible Reference Library, the design, illustrations and paper, are all of high quality.<sup>1</sup> The selection of authors has also been exemplary. Amihai Mazar coped brilliantly with the unenviable task of summarizing in Volume I the archaeological record from 10,000 to 586 BC!. Here Ephraim Stern brings to bear his long experience of the field to summarise a more comfortable 400 years (732–332 BC). A more equal division of labour in a larger series of volumes would clearly have been preferable. There is also an awkward overlap (732–586 BC) between the two published volumes. Unfortunately this constrained Stern to omit discussion of 'the structure and history of Jerusalem in the 8th to 6th centuries BC (particularly the surrounding tombs) to avoid repeating what has already been published' (p. xv). Nevertheless through his treatment of the wider picture of Judahite archaeology, Stern manages to include a brief treatment of Jerusalem – absolutely essential for understanding these periods. And any deficiencies at the editorial stage are outweighed by the pleasure of sharing Stern's synthesis, conveyed in writing of admirable clarity.

Stern, amongst many other achievements, is of course well known for his seminal study of *The Material Culture of the Land of the Bible in the Persian Period 538–332 B.C.E.* (1982). This is not replaced or superseded by the present semi-popular volume. Nevertheless, both in text and bibliography, it provides a vital and most welcome update. Shepherding a vast quantity of data into an easily digestible format (with subsections on all aspects of material culture for the three periods covered), Stern's survey is an extraordinary achievement – especially as most of the timespan is, in terms of dating the archaeology, a rather grey area in the southern Levant. Surprisingly, the relative dating of artefacts is often far easier during the

Late Bronze Age – when, whatever the absolute dates, pharaonic finds from the eighteenth–twentieth dynasties provide us with a firm internal chronology for a period of some four centuries – than it is during the Iron Age, for any period up to late Persian times. Witness for example the continuing controversy over the tenth versus ninth century BC dating of Iron IIA strata. Yet, a fact not yet appreciated by Israel Finkelstein and the ‘Tel Aviv school’, who have now begun to address the Iron IIA problem,<sup>2</sup> is that similar dating problems run into the succeeding periods of the Iron IIB, IIC and Persian – those covered by Stern’s survey. To Stern’s credit, his survey often highlights rather than glosses over these problems.

The uncertainties begin with the Assyrian period. The Assyrian Eponym canon – universally agreed to be sound back to c. 910 ( $\pm 1$ ) BC – has provided the touchstone for all attempts to resolve the ‘mysterious numbers of the Hebrew kings’ (as Thiele called them) into a workable dating scheme for the divided monarchy of Judah and Israel. It is Assyrian chronology that allows us, for example, to date the presence of Ahab at the battle of Karkar to 853 BC. By the same token, we would expect firmly datable Assyrian artefacts and inscriptions to provide the acid test of any archaeological chronology in Palestine. Stern (pp. 14–18) provides a handy (and the most up-to-date) list of the Assyrian texts from Palestine.<sup>3</sup> He describes and illustrates the stela fragments of Sargon II from Ashdod and Samaria, but unfortunately omits the important fact that neither was found in its original context. Stern mentions the tablets from Gezer, Hadid and Keisan but, again, does not discuss their chronological significance. The Gezer tablets were found during R. A. S. Macalister’s excavations in the first decade of the twentieth century, too early for their contexts to be judged in modern terms. In the case of Tell Keisan, the context of its Assyrian tablets led to an acrimonious dispute between the director of the site and the site-supervisor of the area in which they were found.<sup>4</sup> Most potentially useful, chronologically, are those from Hadid, where two tablets precisely dated (by their eponyms) to 698/97 and 664/63 were discovered. Only preliminary reports of the contexts have been published. Both are described as ‘Iron II’, with a little more detail for the first tablet, which was found with pottery ‘dated by the excavator to the second half of the 8th century BCE’ (Na’aman and Zadok 2000: 159). The awaited full publication of the contexts may (hopefully) tell us whether the seventh-century Assyrian tablets confirm the local dating of the associated pottery to the second half of the eighth century BC, or whether they would be better accommodated by nudging the local dates down by a few decades.

Such a possibility has been raised by the discovery of the famous temple inscription at Ekron. This provides the best available fix for Iron IIC chronology (at least with respect to Philistia), since the builder of the temple, Ikausu, is known from Assyrian records. Here unfortunately Stern (p. 120) uncritically repeats the excavators in concluding that ‘Temple complex 650 was erected in the first quarter of the 7th century BC.’ The Assyrian references to Ikausu (673 and 667 BC) fall in the *second* rather than the first quarter of the 7th century, indicating a later construction date for the temple and the beginning of Stratum IC (see James, in press). Again following the excavators, Stern refers to the architecture of the

aforementioned temple as influenced by ‘Neo-Assyrian royal palaces and temples’. Yet it is hard to see what, precisely, is Neo-Assyrian about the building<sup>5</sup> and the excavators have actually been more forthcoming about aspects suggesting Phoenician influence (Gitin 2003: 59\*, n. 3).

The difficulties extend to other Assyrian material (seals, Mesopotamian pottery imports and local copies), where the contexts are often uncertain or the identification equivocal. With respect to pottery Holladay (1976) long ago argued that ‘Assyrian Palace Ware’, conventionally dated to the eighth–early seventh centuries, did not reflect the period of Assyrian domination. Using Mesopotamian contexts, Holladay argued that the *floruit* of the ware should be placed in and following the last days of the Assyrian empire (late seventh–early sixth centuries BC). This now finds strong support from Ammonite archaeology: ‘In the recent excavations at Tell el-‘Umeiri, this pottery was found stratigraphically together with Attic pottery and cylinder seals attributed by experts to the late 6th and early 5th centuries BCE.’ (Stern, p. 257). With respect to Palestine generally, Stern states that Assyrian Palace Ware remained ‘a constant feature’ during the Persian period, though by then an ‘inferior’ product with ‘cruder shapes’ (p. 516).

In the second ‘book’ of the volume, the acute difficulty of identifying the material culture of the Neo-Babylonian domination looms large. Stern’s section here on ‘Pottery Vessels’ begins with the frank statement: ‘There are almost no pottery vessels from Palestine that may be exclusively attributed to the Babylonian period.’ (p. 342) Yet the problem does not seem to be due just to its relatively short duration (c. 604–539 BC). The difficulty extends well into the Persian period, down to c. 450 BC when we begin to reach a firmer horizon of material, identifiable largely through imported Greek pottery and Aramaic *ostraca*. Stern (p. 344) makes these salutary remarks:

... distinguishing among tombs dated to the late Ammonite kingdom, the Babylonian period, and those of the early Persian period is difficult on the basis of local pottery alone, and it is almost impossible without the help of the Greek material. For example, a Persian-period tomb from Shechem, recently published, included all the typical vessels of the Babylonian period – clay alabaster, sack-shaped decanters, a rich assemblage of metal artifacts, one of those shaped like the closed Babylonian lamp – but the date was securely fixed by the presence of three Attic *lekythoi* (bottles), dated to the beginning of the 5th century BCE...

Were it not for the presence of the Greek imports these tombs may well have been dated to the Babylonian (or even Assyrian) period. Surprisingly such discoveries have not forced a rethink of the dating for Iron IIC, but merely an extension. While it was once thought that Iron IIC came to a dramatic close in 587/6 BC, in recent years its terminal date has been lowered by degrees to 530/520 BC (Barkay 1993; cf. Lipschits 2003: 349). In Transjordan it now seems the consensus that the local Iron IIC pottery continued well into the Persian period (MacDonald 1994: 71–72). As one can see from Stern’s own remarks, it is now increasingly clear that Iron IIC forms continued into the fifth century, and not just in Transjordan.<sup>6</sup>

It is not only the pottery that creates dating problems. At Ashkelon a *favissa* was uncovered containing a large number of Egyptian-style bronzes, originally dated to

the Persian period (fourth century BC). Yet, as Stern reports, the current excavators have found 'at least one' such bronze in the stratum sealed by a destruction level they date to 604 BC, and have redated the *favissa* accordingly. Nevertheless, the Persian comparanda remain: 'At Ashkelon, some seven imported Egyptian bronze *situlae* were recovered in the same late 7th century BCE stratum... Two almost identical bronze *situlae* bear Phoenician inscriptions and are dated to the Persian period.' (pp. 498–500) With this confused picture, Stern had little choice than to present the same finds twice – once for the Assyrian period (p. 124), and then again in the Persian chapter (pp. 498–501).

Stern provides many such illustrations and fully admits the fluidity of the pottery chronology between the seventh and fifth centuries. Nevertheless, he tends to use as a dating fulcrum his firm belief in a 'Babylonian gap'. In a recent article he stressed the traditional view that Palestine was very poorly settled during the time of the Neo-Babylonian Empire: 'While the Assyrians left a clear imprint of their presence in Palestine, there is a strange gap after the Babylonian destruction. Call it an archaeological gap, if you wish.' (Stern 2000: 46). This gap, according to Stern, can be discerned not only in the kingdom of Judah, but at sites in Philistia, the Arabah, the Jordan Valley, the area of the erstwhile northern kingdom and even southern Phoenicia. In a nutshell, Stern argues that outside of northern Judah (Benjamin) and Ammon, 'the rest of Palestine was largely barren' (Stern 2000: 51). To many such views are extreme (see e.g. Barstad 2003: esp. 3, n. 1). Biblical scholar Joseph Blenkinsopp took issue with Stern's position and a heated exchange ensued. Stern made a tactical error in resorting to the argument that Blenkinsopp is not an archaeologist, a point which the latter easily deflected: 'I systematically compared his [Stern's] results with those of authors in the two most recent encyclopedias of archaeology in the region, one of which Stern himself edited.... In most of the cases cited, their conclusions disagreed with those of Professor Stern.' (Blenkinsopp 2002: 13).

In the present volume Stern expounds more fully his model for the 'Babylonian gap':

A clear distinction must be made between the influence of Babylonian occupation on the country and its material culture and that of their Assyrian predecessors and the Persians after them. Although the Babylonians created a new administrative organisation, different from that of their predecessors, this did not leave any clear traces in the country's archaeological record. (p. 308)

Stern argues that this 'unique situation' can be explained by two factors. The 'more important' factor concerns the character of the Babylonian domination:

Unlike the previous Assyrian imperial system, which strived to create a network of semi-independent provinces, the Babylonian concept was quite different: their entire focus was on the welfare of the city of Babylon and its immediate surroundings, while the periphery was largely neglected, with negative consequences for those living in those territories. (p. 303)

This is a rather sweeping statement. As to the 'entire focus' being on Babylon, Stern himself naturally mentions the fact that Nabonidus spent some ten years in Teima in the Arabian desert (p. 307). His sojourn there is usually thought to have been connected to Babylonian attempts to control Arabian trade (Kuhrt 1990: 131–132). And even though we have few relevant economic texts, they show a far-flung trading network reaching to Egypt and Ionia. With the latter, relations 'seem more intimate and direct than in the preceding Neo-Assyrian period' (Kuhrt 2002: 21). How was such long-distance trade conducted if 'the periphery was largely neglected'? While the depredations of the Babylonians were in some cases more severe than those of the Assyrians, one wonders whether they would have truly left their western empire a virtual economic desert, a situation which would have hardly benefited the capital.

Stern misunderstands the important difference in the amount and character of evidence that survives from the two Mesopotamian empires. The Babylonian kings did not leave the kind of detailed 'annals' familiar from their predecessors Sennacherib, Esarhaddon and Assurbanipal, whose military records, spattered on sculpted reliefs to clay cylinders, often survive in numerous editions. For Neo-Babylonian campaigns we are largely thrown back on the laconic entries in the Babylonian chronicle, often no more than a sentence each. Here we have chronicle entries for some 20 years, *less than thirty percent* of the empire's duration. Viewed this way one must question Stern's statement that: 'The Babylonians waged far fewer military campaigns for the domination of Palestine than the Assyrians, and the number of the written sources at our disposal describing these is likewise much smaller.' (p. 303) Here the second part of the sentence effectively (and ironically) demolishes the impact of the first. Unfortunately Stern's *idée fixe* of Babylonian emperors neglectful of the 'periphery' leads him into exaggeration: 'The Babylonian kings who came to power after Nebuchadnezzar, Amel Marduk (562–560 BCE) and Nergal Shar Usur (560–556 BCE), reigned for too short a time to influence the remote regions of the empire.' How can this be said? We have no chronicle entries for Amel Marduk,<sup>7</sup> while a fragment describes a campaign of the latter (a.k.a. Neriglissar) reaching as far as western Cilicia and the borders of 'Ludu' (Lydia). If such a campaign does not reflect a king with time or interest enough to 'influence the remote regions of the empire' it is hard to imagine what might.

Of course Stern is not the only archaeologist to make the tacit assumption that the absence of Babylonian records equals Babylonian inactivity in the west. Assyriologists, however, are acutely aware that this assumption is mistaken and assume that there were many more Neo-Babylonian campaigns, for which the records are simply lacking (see e.g. Sack 2003: 223). At the same time, there is an increasing understanding that the Neo-Babylonian empire was truly the heir to the Assyrian, both in terms of its geographical scope and administration.<sup>8</sup> In short, Stern's attempt to draw *such* a sharp distinction between the character of the Assyrian and Babylonian administrations is overstated.

Stern's other factor explaining the putative lack of Babylonian evidence in Palestine is that 'it is almost impossible to determine if a certain artefact with

Babylonian parallels should be dated to the late Assyrian period, to the Babylonian period, or even to the early Persian period.' (p. 308) But there's the rub. When Stern confidently contrasts the number of 'Assyrian' structures and fortresses with a Babylonian lack of them (p. 349), how sure can we be that we are not looking at a false picture? Rather than being archaeological, could the 'gap' perceived by Stern be the artefact of an incorrect and attenuated chronology? In which material from the sixth century has been incorrectly assigned to the seventh?

In lieu of the smoking gun which a well-stratified Assyrian or Babylonian inscription might provide, some light on the chronology might be expected from local epigraphic material. Stern devotes generous space to this and it is good to see as much weight given to Ammon, Moab and Edom as the Hebrew kingdoms. With the latter, as any reader of popular magazines on biblical archaeology will now be aware, almost every king, prince and even minor official of Judah now has his own seal, miraculously supplied by antiquities dealers. (Any gaps in the lists can be expected to be filled in the coming years!) The extent to which the field has been poisoned by such material (either illegally excavated or suspect) is surely one of the most tragic developments in modern Near Eastern archaeology. The seething remarks of Joffe (2003) on this matter are required reading. Here, unfortunately, Stern fails to warn the reader of the awful dangers of using unprovenanced material. For example he cites an intriguing bulla which bears a regnal year (arguably that of Josiah) with the tag *lmlk*. Stern presents it as an 'ending' to the debate on whether the *lmlk* system of taxation continued into the seventh century (p. 175) – yet a stronger caveat than 'said to originate at Eltolad' is needed for a piece that only surfaced through the antiquities market (Avigad 1990: 262). Again, with respect to the fifty sealings discovered by Shiloh in the 'House of Bullae' of the City of David excavations, Stern states: 'It was possible to date the bullae accurately, which included the names of Gemariah son of Shaphan and Berachiah son of Neriah the scribe, contemporaries of Jeremiah...' (p. 323). While the first was from the excavations, the second came from a separate, unprovenanced collection. While this does not mean that it is a forgery, the names on it (compared with those of a character in *Jeremiah*) must necessarily be suspended from any discussion of palaeographic dating. Of the welter of biblical names found on epigraphic material that might be diagnostic enough to help date Iron IIC, the case essentially boils down to two 'matches' from the House of Bullae (pp. 182–184) and one with a bulla from Lachish II (p. 180).<sup>9</sup> These presently provide some of the strongest evidence for dating the end of the Lachish II horizon of pottery to 587/6 BC, but whether they are enough to sustain it is a matter for debate.

Of the non-Hebrew epigraphic material reviewed by Stern, intriguing evidence for the existence of *lmlk*-stamp iconography much later than c. 700 BC (and close to the original Albrightian dating) comes from the (excavated) seal of Milkomur, servant of the Ammonite king Baalis who reigned about 585 BC (p. 241); it bears the same four-winged scarab beetle as many of the Judahite royal stamps. Further discussion of *lmlk* chronology is beyond the scope of this review, but it is interesting that Stern accepts continuity in tradition of royal stamps from the time of Hezekiah

down to Persian times (p. 175). This is difficult to envisage given the large gaps in time (on the present chronology) between *lmlk* and Judahite rosette production (approaching a century), and between the latter and the rosettes of the Persian period (another half century). The problem is aggravated by Stern's 'Babylonian gap', during which the only Hebrew stamps he allows are those bearing the name Mozah (NB: no rosettes), produced in the territory of Benjamin (pp. 335–338). In the Persian period we encounter familiar difficulties, with the Hebrew Yehud (*yhwd*) stamps: '... the various chronologies proposed extend over a very long period: from the 6th to the 2nd centuries BCE.' (p. 549) This difference of opinion may have been resolved in favour of a Hellenistic date, but with respect to their Aramaic counterparts things remain less certain. Here Stern mentions the arguments for dating them to either the late sixth–early fifth centuries (as favoured by Avigad) or the late fifth–fourth centuries (as favoured by most other scholars). In the case of some of the governors mentioned on these Aramaic sealings, he cautiously notes that the question 'remains open'.

In short the fundamental problem of chronology haunts almost every page of Stern's otherwise magisterial survey. Amazingly, we are still unclear about the dating of much of the material under review, from the Assyrian period down to the Hellenistic. Blame for these problems is of course not laid at Stern's door – aside perhaps for his overeagerness for the 'Babylonian gap'. The problems come from a general reluctance by archaeologists in Israel to reappraise, and where necessary abandon certain cherished 'fixed points'. In lieu of those we might have desired from Mesopotamian inscriptions, various historical dates have been accepted, on the assumption that certain destruction levels can be attributed to particular campaigns of Tiglath-pileser III, Sennacherib, Nebuchadrezzar etc. But it should be remembered that these are often assumptions, not in any way 'proven' by diagnostic evidence. While the redating of the destruction of Lachish III from 597 BC to 701 BC (in the late 1970s) now appears to be set in concrete, it has been pointed out that the case does not depend on any new evidence as such but the lack of evidence from earlier strata for a presumed Assyrian destruction (James *et al.* 1991a: 176–178).

The 'Lachish revolution' in dating, as well as being a process still in completion, is producing some surprising consequences. A conspicuous case concerns the substantial amount of Edomite pottery, *ostraca* and other material remains, now dated to the seventh century BC, found in the Negev of southern Judah. Stern sees this as evidence, surely correct, for a westwards expansion of Edomite settlement (pp. 161, 276–279; for further details, see Vaughn 1999: 50–58). Yet from numerous Old Testament sources we would expect such Edomite settlement to belong to the period after the fall of Jerusalem in 587/6 BC, rather than the seventh century when the area was still under Judahite control (Kokkinos 1998: 40–41, esp. n. 27).

Given all these chronological tensions it is for good reason that Stern repeatedly highlights the finds of imported Greek pottery. From the Persian War period (c. 490 BC onwards) this can provide an almost decadal ceramic guide, to cross-date with the less glamorous production in Palestine. Yet, a few generations before this period

we are entangled in another chronological mare's nest. The fragility of Archaic Greek pottery chronology has often been recognized, most recently due to the critical efforts of Michael Vickers (now Professor of Archaeology at Oxford) and the late Persianist David Francis.<sup>10</sup> While it is fair to say that their suggested reductions (at points as much as 80 years) have been generally received as too extreme, the matters raised have certainly not been settled. One problem has always been circularity in argument between Greek and Israeli archaeologists (see Fantalkin 2001: 128, n. 57). The conundrum is highlighted by Stern:

... there has previously been little independent historical evidence to confirm the accuracy of this dating, for the chronology of early Greek pottery is based on its presence at Near Eastern sites with problematic stratigraphy. Some scholars, questioning the traditional chronology for Greek pottery, have tried to lower its dates. The new, securely dated material from Ashkelon, Ekron and Tel Batash, supported by evidence from Mezaad Hashavyahu, Kabri, Tell Keisan, Malhata, and other sites, assures the earlier ones [dates]. (p. 221)

However, Stern's belief that there is 'new, securely dated material' from the listed sites is optimistic. For all those listed there is not a *shred* of historical evidence for dating their destruction to one Babylonian campaign or another, with the single exception of Ashkelon. Indeed, it is only on the basis of a debated epigraphical restoration of the name 'Ashkelon' in the Babylonian Chronicle for the year 604 BC,<sup>11</sup> that the whole concept of a wave of destructions by Nebuchadrezzar in Philistia (and by extension, through the Greek pottery to the other sites) *c.* 605–603 BC rests. Even allowing that Ashkelon was destroyed in 604 BC, there is still no firm link to its archaeology. The current excavators have associated the event with a burnt level (incorporating '7th-century' Greek sherds – Stager 1996) but, as there is no published stratigraphy with details of lower levels that might bear signs of destruction, the link remains speculative. Otherwise, the dating of the local pottery at such sites during this period remains *sub judice*. As Fantalkin (2001: 128) remarks, 'the present state of research does not permit the unequivocal identification of the typological differences between the end of the 7th century and the beginning of the 6th century BCE.'

Further, there is a mounting case for reducing the dates of the Greek imports in question. At three key sites (Naukratis, Old Smyrna and Tokra near Cyrene), Herodotus gives an account consistent with the archaeology – except that the pottery associated with the events he discusses is presently dated some three to four decades too early (Bowden 1991; 1996; James 2003). Either Herodotus is wrong or the conventional Archaic pottery chronology is wrong. The stakes are high. Herodotus' narrative involves a fairly tight pattern of synchronisms between Greece and neighbouring countries such as Lydia, Egypt and Babylonia. If he is wrong about major factors such as the origin of the Greek settlements at Naukratis and Cyrene we are in danger of jettisoning a large part of Archaic Greek history. If on the other hand we do not reject Herodotus, then the horizon of Greek pottery presently dated *c.* 600 BC will need to be lowered. There will be dramatic effects on the horizon of destructions in Philistia (including Ekron IB) presently dated to the Babylonian invasions *c.* 604 BC. The dating here is very tight, as some of the Archaic pottery

found in the destructions can be (conventionally) dated close to *c.* 600 BC; thus even a relatively small adjustment on the Greek side (25–35 years) would tip the Philistia destructions into the first part of the sixth century BC. The 'Babylonian Gap' perceived by Stern may then start to disappear, at least with respect to Philistia, and many of the problems reviewed above will have to be considered in a fresh light. In the meantime, Israeli archaeologists should keep a weather eye on the developments in the continuing debate over Greek Archaic chronology – as much as *vice versa*.

To Stern's credit, he at least alerts his readers to the fact that there may be a problem *viz a viz* cross-dating with Archaic Greek pottery, though he may be premature in pronouncing that 'firmly dated' destructions have now solved it. On this point and on many others (such as the Yehud stamps and the Ashkelon *favissa*) the candour of Stern's writing is most welcome – particularly in a semi-popular book where readers are all too often dished up the 'varnished truth'.

It is a platitude to apologize for nitpicking when reviewing an otherwise excellent book. But I will repeat it, as my comments (largely chronological) certainly fail to do justice to the wider aspects of a work of monumental labour. In completing it, Stern has created a vital platform for all future discussion. Without synthesizing works like this it would be impossible to control all the data and make progress, in whatever direction that might be. Stern has made a significant and lasting contribution to our understanding of these strangely elusive periods (Assyrian, Babylonian and Persian) in the archaeology of Bible lands.

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#### Notes

1 But not, alas, the editing. There are numerous slips, unfortunate in a textbook like this, which could have been avoided by using more expert readers. For example: p. 120, Achish of Ekron is not 'known from Assyrian sources from 676 to 667 BCE', but only in the years 673 and 667 BC; p. 240, the reign of Pada'el did not end in 635 but no later than 667 BC; on p. 268 the names Qosgabar and Qosga (which in any case should be 'Qosg[...]') are interchanged; on p. 363 we are told that the Darius whose accession year is mentioned in a cuneiform tablet from Tawilan in Edom was 'probably the third' Persian ruler of that name (giving 335 BC), but on p. 457 'Darius I (521 BC) is the most likely candidate'. Other errors should have been caught by the copy editor, e.g., p. 328, 'Nebonidus' instead of 'Nabonidus' – worse still, the phantom 'Nebonidus' gets his own entry in the index (p. 656).

2 The problem was highlighted in James *et al.* 1991a: 183–188, James *et al.* 1991b (esp. Table 2, p. 232 – partly corrected from that in James *et al.* 1991a: 195), and James *et al.* 1998, 30–31, where it is argued that Iron IIA, conventionally dated to *c.* 1000–900 BC, began in the mid-ninth century BC. For example at Samaria we followed Kenyon in associating it with the building activities of the Omrides, *c.* 870–850 BC. Finkelstein's advocacy of a tenth- to ninth-century shift has now become the focus of a long-running debate. For the initial

salvoes, see Finkelstein 1995; 1996; Mazar 1997; Finkelstein 1998; for a recent contribution and bibliography, see Gilboa and Sharon 2003. For some brief remarks on the wider implications of the tenth to ninth century controversy, see James 2002a; 2002b.

3 The brief survey in van der Toorn (2000: 99–100) has useful references but was written too early to include the Tel Hadid finds.

4 For further details and references (Gezer and Keisan) see conveniently James *et al.* 1991a: 182, 373 (nn. 66–68).

5 Personal communications from Bob Porter and Lawrence Stager, who have independently examined the alleged Assyrian architectural parallels.

6 In James *et al.* 1991a: 175, we recommended lowering the end of Iron IIC to c. 440 BC.

7 According to 2 Kgs. 25:27, Amel Marduk considerably raised the status of the captive Judahite king, Jehoiachin, at his court. If this is true, it would suggest a measure of conciliation to the Jews, both to those in Palestine and Babylonia.

8 See Sack 2003 and Dalley 2003 – in particular her discussion of the recently discovered tablets from Šēkh Hamad, where Neo-Babylonian administrative texts from the early reign of Nebuchadrezzar continue the scribal tradition of the Assyrian empire.

9 The sealings of ‘Gemaryahu son of Shaphan’ and ‘Azaryahu son of Hilkiyahu’ from the City of David excavations are believed to reflect the biblical characters Gemariah son of Shaphan (Jer. 36:10) and Azariah son of Hilkiyah the priest (1 Chr. 6:13–14) from the last decades of the Judahite monarchy. The sealing of ‘Gedaliah who is over the house’ from Lachish II is believed to come from an early stage in the career of Gedaliah son of Ahikam, appointed governor of Jerusalem after the Babylonian sack of Jerusalem (2 Kgs. 25:22–24) – an identification described by Avigad (1978, 52) as ‘not absolutely certain’. A detailed examination of these and other possible historical links is included in the forthcoming PhD thesis of Peter van der Veen: ‘The Final Phase of Iron Age IIC and the Babylonian Conquest of Judah – a Reassessment of Provenanced Seals and Bullae from Israel and Jordan’. (Trinity College/University of Bristol).

10 The bibliography here is extensive – see conveniently Biers 1992: 99–101; James 2003: 241, nn. 31–32.

11 Wiseman 1991: 23, n. 158; for further references see conveniently Waldbaum and Magness 1997: 37, n. 105.

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## Book Reviews

**Asher S. Kaufman**, *The Temple Mount. Where is the Holy of Holies?* Frontpages: xxx; 184 pp.; 98 ill. (a few in colour). Rubin Mass: Jerusalem, 2004. Price \$40. ISBN 965-09-0170-1

This is the very personal story of one man's search for the location of Herod's Temple in the area of the Temple Mount in the Old City of Jerusalem. It began in 1967 when Asher Kaufman, a Scottish expert on nuclear fusion and later Professor of Physics at the Hebrew University, caught the 'temple bug' that has attacked so many innocent residents of Jerusalem and abroad. But, unlike them, Kaufman accepted the challenge and worked long and hard in libraries across three continents to search out this particular *Shangri-la*. And for the present it remains a *Shangri-la*, unobtainable because of the impossibility of any scientific archaeological dig ever taking place at the site holy to the three monotheistic religions.

To further his researches the author turned to the written sources, particularly the works of Josephus and the Mishnaic tractate of *Middoth*, which gives a general description of the Temple, with basic dimensions. But Kaufman has also made a detailed examination of nineteenth-century photographs, early coin and artistic representations, and scattered structural remains and rock cuttings on the site itself, many of which are now covered over by the recent 'improvement' works of the *Waqf*, the Moslem authority that administers the site.

The author has delved into the situation of underground cisterns and into the possible trail of blood from the altar of sacrifices, in his search for the location of the Holy of Holies. With the precision expected of a nuclear physicist, Kaufman has also checked numerous related subjects, such as the measure of the biblical cubit, the dispersal of annual rainwater, the significance of numerical ratios, and the Jewish and Islamic traditions relating to Herod's Temple.

His work on the manuscript of the Hebrew tractate *Middoth* has been extensive and original and has given rise to a number of scholarly articles, well known to workers in the field. Kaufman has been able to resolve apparent contradictions in the text (which was committed to writing some 130 years after the destruction of Jerusalem in AD 70) with the earlier accounts in Josephus, by a close examination of the original manuscripts. And his conclusion in all this, is that the Holy of Holies was situated on a section of bedrock still partly exposed today on the floor of the Dome of the Spirits, a small open-air *aedicula* about 100 m to the north-north-west of the bedrock centred on the Dome of the Rock (*al-Sakhra*), which normally has been believed to be the original location.

In this publication Kaufman goes still further and sets out a plan of the complete Herodian Temple with its courtyards and side chambers. This is all original material and provides two startling innovations; that the outer western wall of the Temple

is well inside the wall today designated as the Western Wall (where Jews come to pray in their thousands); and that the area around the Temple, designated as ritually 'holy', was not square in plan. It had always been considered from the texts to be a square of 500 by 500 cubits, but Kaufman shows this to be a misreading. It should be, he claims, an area *equivalent* to 500 by 500 cubits, in other words an area of 250,000 square cubits, and therefore not necessarily square or even rectangular. From research he conducted on some scattered remains, above and below ground, Kaufman suggests, unexpectedly, that the Jewish temple was of trapezoidal shape.

These are original deductions and it is fairly clear how he arrived at them, but are they conclusive? One has to say, in spite of the meticulous research, the matter is far from resolved. Kaufman's reliance on literary texts has led to conclusions that, to this reviewer's mind, are impractical. Where the texts, both in Josephus and *Middoth*, speak of dimensions in full numbers of cubits, they may well be approximations. More than that, any plans based purely on *Middoth* are impractical because they do not give enough space between the external altar and the Temple steps for the many priests and animals that must have been milling around on festival days. Nor is there enough space in the 'Court of Israel' for the many worshippers to gather to see their sacrifices being offered. An even more serious criticism is that the literary descriptions record changes of level between the Outer Court (of 'Women') and the Inner Court of the Altar of nearly four metres in height; and between the Altar and the Temple of some three metres or so. Most researchers of *Middoth*, including Kaufman, have concentrated purely on the top plan view and neglected to think about these variations in level, which has made their predictions rather tentative. For instance, how did the hordes of sacrificial animals reach the upper levels around the altar? Was it via long staircases (unlikely) or by an extensive system of ramps for which there is no room on the plans? Neither of these alternatives are considered on the plans prepared by Kaufman, or indeed on those made by other scholars.

Nevertheless, this is a remarkable book and the culmination of 35 years of investigations. It contains a valuable record of early photographs, it sets out the criteria relating to key measurements, gives details of previously visible rock remains on the Mount, and above all offers an acute analysis of the ancient texts applicable to the Herodian Temple. As a pictorial record alone, of the search made for the authentic Jewish temple, it is a delight to read and to look at, but, it must be admitted, the journey to this particular *Shangri-la* is not yet complete.

Stephen G. Rosenberg

**Jodi Magness**, *The Archaeology of Qumran and the Dead Sea Scrolls*. Grand Rapids, Eerdmans, 2002. Pp. xlvi with 66 figs + 238, h/b, Price: \$26. ISBN 0-8028-4589-4.

In this book, the first of a new series of 'Studies in the Dead Sea Scrolls and Related Literature', which is intended for students and the informed reader, as well as for scholars, Jodi Magness proposes to summarize what is known about the archaeology

of Qumrân, presenting her point of view, by using the published archaeological material as well as information supplied by the scrolls and the ancient literary sources. In this she follows the arguments of R. de Vaux in his lectures to the British Academy in the late 1950s, a point of view with which I agree entirely, but which will certainly irritate those archaeologists who resolutely refute these important data on the site. She also agrees with him in identifying the site as having been inhabited by the Essenes, but she declares that she differs on the matter of the dating of its phases of occupation (p. 16). She mentions several times that, as long as a final report of the dig is not available, her suggestions must remain tentative, although she very much hopes that these would not be substantially altered by the full publication of all preserved material. Admittedly, de Vaux published excellent reports and the bulk of the material.

The book, which makes pleasant reading, has no footnotes, but only the main elements of an annotated bibliography at the end of each chapter, and plans as well as figures of inferior quality at the beginning of the book. In the introductory first chapter, the author explains how an archaeologist dates his/her finds and why the archaeology of Qumrân has spawned so many controversial theories. This is due above all to the fact that it is linked to the scrolls, for which de Vaux is often criticized by many opponents, and which may have motivated the production of this monograph.

In the second chapter, Magness rapidly deals with the position of the site, the history of the excavation, the identification with Sokokah (but in contrast to her, I prefer to keep the ancient vocalization preserved by the Septuagint, rather than the later one of the Massoretic Text, Sekakah), and the discovery of the scrolls. One should add that de Vaux kept to the appellation 'the city of salt', since the ruins of 'Ain el-Ghuweir and 'Ain el-Turabeh were not known then, and he did not change his opinion when revising his lectures for publication a little before his death in 1971 (de Vaux 1973: 93).

Chapter 3 provides a brief introduction to the scrolls, raises the question of the identification of the occupants of Qumrân with the Essenes as known from the literary sources, and the connection of the site with the caves containing the scrolls. As regards their name, it seems to me that the question is badly formulated and lacks a clear answer from Magness. It is obvious that the occupants were not under an obligation to define themselves precisely by a particular name, because they were the heirs of true Judaism and legitimate High Priests. They were the real pious *ḥasîdîm* (in Hebrew) or *ḥ'asîn* (in Aramaic), the etymology of the word 'Essene', which one cannot recognise in the farfetched hypothesis which should be rejected out of hand, of the '*osei ha-torah* 'those who keep the Law', just as the word 'Pharisee' cannot be explained by the Hebrew *perushîn*, but by the Aramaic *parîshîn* (see p. 42). As to the relationship between the occupants of the site and the scrolls, Magness rightly notes that one cannot raise as an objection, the absence of any scroll on the site itself, taking into consideration the fires which accompanied successive destructions, and the fact that the contents of the library were probably hidden before the departure of the occupants and the arrival of the Romans. Magness finds the

best proof of this connection in the pottery, identical in both cases, which includes some rare shapes such as the cylindrical jars called 'scroll jars', the presence of several ink-pots, and the proximity of the neighbouring caves (I would add that the only access to Caves Nos. 7 to 9 was by crossing the site itself) and finally the absence of any structures of household type on the site.

Chapter 4 examines the buildings and the phases of occupation. The author disagrees with de Vaux on his division of Period I into Ia and Ib, as well as on his interpretation. She considers that Period Ia simply did not exist (unless one assigns it to the beginning of the first century, pp. 64–66) and that Period Ib must have started in the first half of the first century (between 100 and 50 BC). It did not end with the earthquake of 31 BC followed by abandonment until about 4 BC, but only with a fire after 9 or 8 BC, followed by reoccupation by the same community a little after 4 BC. Discarding Period Ia is of no great help, for it was not simply a case of repairing the walls in a single period but also of a considerable expansion of the area of occupation, as the excavator put forward, constituting a new plan or project, these explaining the subdivision of Period I. If it seems likely that the earthquake of 31 BC did not put an end to the Ib occupation, by dissociating the earthquake from the fire, one may question the main argument put forward by Magness – the sole hoard to have been buried in Locus 120, which she dates to her Period Ib (but after the earthquake), and whose most recent coins date to 9/8 BC. This would be possible if Period II followed Period Ib by only a few months (a winter?), as otherwise one cannot explain the absence of coins until the time of their burial after 4 BC, since it is now known that the minting of Tyrian silver coins had not ceased by that time. Nevertheless, it is difficult to accept this attribution, as in that case the hoard should have been buried underneath the soil of Ib, whereas de Vaux found it in the sediment that accumulated between Ib and II. So where would these sediments have come from? From the earthquake? But then an intermediate soil level 'Ic' would have been detected in L. 120, which the excavator does not mention. It is difficult to believe Magness, who supposes that the occupants had in this case removed the debris and sediments. But then, why would they have left the layer of sediment and ashes of that last destruction only in L. 120, except for the requirements of Magness' new hypothesis (p. 68)? As for the first Essene occupation in the first half of the first century, pottery alone cannot provide the precise elements of an answer, especially since a continuous occupation of *loci* with restorations and enlargements can hardly leave traces of pottery or coins to await the arrival of the present-day archaeologist. All things considered, Magness' argument for a new dating or stratigraphy is far from being totally convincing, even if, notably, one should not adhere to the long period of abandonment during Herod's reign, and even if it is likely, following de Vaux, that most of the occupants lived in the vicinity of the main building.

Chapter 5 discusses the pottery and the architecture. For reasons of ritual purity, the occupants apparently made their own vases, thus avoiding having to import them: cylindrical jars with a large opening for the scrolls or for storing food, or ovoid jars, with the texts found in the caves providing explanations on matters of ritual purity. Once manufactured, however, scroll jars may have been used in a

variety of ways, notably for non-religious purposes. It is thus difficult to be as categorical as Magness. She is more incisive and convincing in refuting the various suggestions making Qumrân a commercial depot or port (Crown and Cansdale), a fortress (Golb), a *villa rustica* (Donceel and Humbert) and later a cultic centre (Humbert), a manor (Hirschfeld), whereas the particular character of a community centre for pious Jews with its *miqwa'ot* (ritual baths) which Magness calls a 'sectarian settlement' – is immediately obvious. One could point out that this is not an architectural definition and that Magness is far too obsessed with refuting any argument against an Essene settlement. She rightly notes the absence of baths, swimming-pools and gardens which are characteristic of villas, as well as the absence of frescoes, stucco (traces of which have since been identified) and mosaics, whereas some of these elements have been found at Rujm el-Bahr and Kh. Mazin in the vicinity of Qumrân. Finally, most judiciously she does not accept the identification of a *triclinium* (by P. Donceel-Voûte) in L. 30.

Chapter 6 deals with sacred space and purity required by communal meals, and, consequently, with the dining room and the presence of toilets. Magness discusses this identification of the archaeological remains in the ritually less pure eastern part of the site, thanks to Essene-Qumrânian habits described in the texts. The installation in L. 51, however, can hardly be considered as more than an extra toilet for the Sabbath or for invalids, if one takes into account the number of occupants; moreover, it was out of use in Period II. Magness proceeds in like manner to discuss community meals according to the texts and the archaeology, considering like others before her, the animal bone deposits as the remnants of meals which were part of the renewing of the covenant (the Passover meal does not come into this, given the remains of bovines and others), but without cultic sacrifices (the absence of an altar on the one hand, and the fact that sacrifices were not permitted outside the Temple of Jerusalem on the other). She accepts the identification of L. 77 as a refectory in Period Ib but suggests that in Period II it was moved to the upper floor (pp. 122–24). This last hypothesis seems untenable in view of the very structure of the *locus* (fragile pillars on only its eastern side, and the too great distance from a *miqweh*). Magness' hypothesis, moreover, has been made redundant by the discovery of two plastered floors in L. 77, corresponding to the two phases of L. 86, the two floors at a slightly higher level on either side. On the other hand, it is possible that other rooms on the upper floor could have served as a refectory (L. 111, and L. 120–123), as suggested by staircase L. 113, the pottery of L. 114, and basins nearby. I should add that this refectory would have suited a relatively small group identifiable with the 'novices' before their final admission to the community, rather than it being in use at the end of Period Ib (after the earthquake, pp. 124–126.). It is clear, however, that the basins for washing were placed at key positions in the structure of this sacred space, either near each of the entrances to the site or near the various points of access to rooms where purity was compulsory, or near the work areas and the toilets. It is quite in order to emphasize the peculiar character of the occupants, who were obsessed by questions of purity, and who thus correspond to the Essenes as described in the textual sources.

This theme introduces the following chapter, which is devoted to *miqwa'ot* associated with the idea of purity in the Jewish world in general and at Qumrân in particular, where Magness recorded ten *miqwa'ot* (likewise R. Reich) out of 16 water points. Against de Vaux, she believes no doubt rightly, that the cisterns were covered (p. 152). The fact that at least some of these basins were in use before the earthquake emphasizes the continued presence of this same Jewish group since Period Ib, which is not unimportant for the new theories which seek to distinguish several successive occupations unconnected with each other.

Chapter 8 deals with the cemetery and, by extension, with the presence of women at Qumrân, a question at the centre of discussions on the identification of the site by those who criticize de Vaux for his male-oriented approach. Amongst the excavated tombs, two questions at present dominate the debate centring on: (1) the identification of the skeletons, and (2) the dating of the tombs: are they all from the Qumrân period? It seems that the south–north-oriented tombs of the main cemetery on the plateau to the east of the site should be attributed to the Essene occupation (p. 175). Magness agrees that the tombs of the southern extension and the southern cemetery, to which she adds T.4, which was oriented east–west, on the plateau (pp. 171–175)<sup>1</sup> are Bedouin tombs (as already suggested by Zias) – a problem which de Vaux did not consider, even if he did take into account the existence of two secondary cemeteries (north and south). Magness leaves open the question of T.A in the northern cemetery, which she confuses throughout with the northern extension of the main cemetery (p. 170), by following the mistake of the recent publication of de Vaux's *Notes* by Humbert and Chambon. I clearly demonstrated in my note in *BASOR*, that the dates of the respective excavations and the description of the localization of T.9–10 and T.A–B contradict both Humbert and Chambon, and now also Magness. With good reason she does not include in her analysis material from Steckoll's excavations for which essential elements are missing. Thus, the bones in only 39 tombs constitute the osteological sample for examination. At present, in my opinion, only one woman has been clearly identified, in T.A in the northern cemetery to the north of the site and of the small *wadi* (to be distinguished from the northern extension). It seems to me that the recent identification by Rohrer-Ertl of the skeletons of T.22 and 24b as being women is very doubtful (Vallois and Zias are of the adverse opinion). It thus appears that the number of women at Qumrân was very small to say the least, in fact limited to only one, as opposed to Magness' three (T.7 which is out of alignment, is now considered to have contained the remains of a man). The few tombs in the northern cemetery could, in my opinion, be connected to 'Ain Feshkha, which lacks a cemetery, especially since access to it from the southeast is very easy. Finally, the total absence of children is to be noted. Even the spindle whorl found in L.7, an object which Magness considers to relate exclusively to female work, might not have been connected at all with the Essene occupation (pp. 177–179). Thus, archaeology confirms the texts on the subject of the presence of a group of Essenes at Qumrân, and the absence of families with women and children goes against the hypothesis of a *villa* or manor (p.185).

In chapter 9 Magness describes the hoard in L. 120, which she interprets as tax paid to the Temple by each Essene upon reaching adulthood, when he was inscribed on the registers, although she is not able to exclude the hypothesis of it being a hoard unrelated to the Temple (p. 193). The white linen habit characteristic of the Essenes, is rightly connected with the vestments of the priests in the Temple. Interpreting anti-Hellenic reactions on the basis of limited archaeological remains, however, is much more questionable.

The last chapter focuses on the sites of 'Ain Feshkha and 'Ain el-Ghuweir and on their relationship to Qumrân. Magness dates Period I to the Herodian era, and Period II parallels that of Qumrân, but she hesitates to plump for an Essene presence at these two sites. If hesitation is possible in the case of 'Ain el-Ghuweir, being at a great distance to the south, it is not as regards 'Ain Feshkha, since a wall links it to Qumrân not far away, and there must have been economic relations, whether for crops, water supply or crafts such as the production of indigo (recently identified on the basis of the shape of the basins), a vegetable dye used to colour linen in strict conformity with Mosaic rules. The discovery of an inkwell at 'Ain Feshkha supports this close relationship, which is not negated by the absence of a *miqweh*, largely compensated for by basins of running water.

On the whole, Magness has made a balanced use of the sources, archaeology and texts, even if she did not take them all into account. In reviving and defending de Vaux's interpretation, she presents a reasonable and convincing point of view concerning the identification of the occupants with the Essenes and the relationship of the caves to the site, relegating other opinions to the realm of largely untenable hypotheses. Her more personal contribution in denying Period Ia remains questionable, whilst her hypothesis of an occupation postdating the earthquake seems likely. One must await, however, the publication of all the material from de Vaux's excavations and of others since, to check the strength of her arguments.

This book, which has the pretension to be more than just an introduction to the archaeology of Qumrân does not lack in errors, probably owing to a large extent to the speed at which it was written. Here are but a few of many mistakes, firstly in the captions to the illustrations: Fig. 3: beginning of the War scroll, not that of Isaiah; Fig. 4: 'looking east', not 'west'; Fig. 5: it is impossible that all the loci are indicated; Fig.16: it is unlikely that the carbonized wood should have originated in the Period II destruction; Fig. 26: Period II and III should be interchanged; Fig. 46: the 'northern cemetery' is in fact the northern extension.

On p. 2 it is not accurate to write that J.-B. Humbert invited the Belgian archaeologist Robert Donceel, who was in fact prevailed upon by the Council of the Ecole Biblique. On the other hand, it was J.-B. Humbert who discharged him from the project; and on p. 3 one is surprised to learn that Chambon is a Belgian archaeologist. It is excessive to state (on pp. 15–16) that de Vaux, as an archaeologist, was influenced by his Dominican status. He consistently refused to identify Qumrân as a 'monastery' or to turn the Qumranites into 'monks', these anachronisms appearing not in his work but in that of his detractors, as he himself

explained in reviewing Driver's theories (see *RB* 73, 1966, 229), which were, alas, followed by a large number of authors who quickly jumped into making amalgamations. Why should the words 'refectory' and 'scriptorium' be undue literary inferences? Most French words come from the Latin and are used more widely than Magness thinks, thus avoiding the use of paraphrases such as 'dining-room', or 'scribes' room'. Conversely, the use of 'triclinium' is not justifiable and de Vaux took great care not to use it. There is nothing incoherent or offensive in that. Besides, Magness herself (among others) deserves to be rebuked for using, page after page, the words 'sect' and 'sectarian', although these words bear no longer the same meaning at all as in Flavius Josephus' description of the three Jewish religious 'schools' or 'tendencies' (schools, pp. 39–40), the Pharisees, Sadducees and Essenes. Logically, these words should also be used for the other two religious parties. The Essenes, moreover, were certainly the most orthodox of the Jews of that time, and neither 'deviants' or 'sectarians'!

On p. 28, it is not accurate to state that the 'archaeologists from the American School of Oriental Research in Jerusalem conducted a systematic survey of the caves and cavities...' This survey was conducted jointly by the archaeologists of the Ecole Biblique et Archéologique Française de Jérusalem, the American School of Oriental Research, and the Palestine Archaeological Museum, but it was a French team that discovered Cave 3 and the copper scrolls, as de Vaux reported (*DJD* III, 2 and 201).

On p. 29 it is unlikely that the scrolls of Cave 4 had been placed on wooden shelves, since most of them were discovered pell-mell on reed mats or palm beds at the bottom of the stairs, mixed with potsherds (see for example de Vaux, *DJD* VI, 21). On p. 30 one would have expected something better than a second-hand reference to Avi-Yonah on the subject of *Mesad Hasidin* (Milik, *Murabba'at* 45), especially considering that the identification has been disputed since by us. In the third paragraph on p. 51, 'southeast corner' should be 'southwest'. On p. 59 Loci 105 and 107 are either to the east of the secondary building, or to the west of the main building (see Fig. 8), but not to the east of the main building. On p. 98 it is wrong to affirm that no Greek inscription was found at Qumrân. See *KhQ*. 192, 387, 439, 635(?), 979 and 2124, even if their dates are sometimes uncertain, but their presence should be interpreted in parallel to that of the Greek of several scrolls of Caves 4 and 7 for example, to the Greek letters of the Copper Scroll, and to the use of the Greek alphabet in various cryptic writings of Cave 4.

On pp. 163–164 it is difficult to understand how the fact of being a man, and a Roman Catholic at that, may affect the study of archaeological or textual data. What counts is the objectivity of the scholar. Is feminist literature in its turn exempt from any subjective slant? On p. 168, in the west–east-oriented tombs of the southern cemetery and of the southern extension, the dead man's head was found at the western end, exactly as in Islamic tombs, and not to the east as in T.4. Only two tombs contained two corpses, T.16 and 24, but not T.37 and T.3 in the southern cemetery. Only two were reburials (T.11 and 24), T.37 being a very doubtful case: (see Excavation Notes).<sup>2</sup> The two cemeteries mentioned on p. 174 are on two small

hills separated by a ravine situated some 800 m to the north of 'Ain el-Ghuweir, not one to the north and the other to the south of the site.

As is obvious, the last word has not yet been written on this subject, even though this book has its uses, especially for the English-speaking reader.

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[Translated from the original French by Marion Rosenberg  
and edited by Claudine Dauphin, CNRS]

#### Notes

1 This is not possible, given the position of the head to the east and the presence of fragments of a cylindrical jar of the Hasmonean period 'with a narrow neck' (see de Vaux, *RB* 60, 1953, 103). The reference (on p.169) to *RB* 61 (1954), 215, Fig. 1.4 is not relevant, a jar of Trench A being similar to that of T.4, except that it is rather more pot-bellied and has a different neck.

2 The excavation records read as follows: '10–3–1956. To the east and a little to the north of 36, tomb oriented here north-south. The tomb is marked by an oval of small stones and not by large boulders as for all others. At a depth of approximately 1 m, the pit is irregular. The skeleton lies on its back, the upper part of the body to the south. The thorax, pelvis and femurs appear to be articulated, but the head was found crushed under the femurs with the bones of the legs and feet. Was this a reburial?'

My own thoughts about this burial are as follows. I must correct somewhat the account published by Humbert and Chambon whose rewriting of the original allows one to believe that the body was oriented north–south, which is not what de Vaux recorded, but definitely south–north; only the head was displaced and appears to have rolled and been smashed.

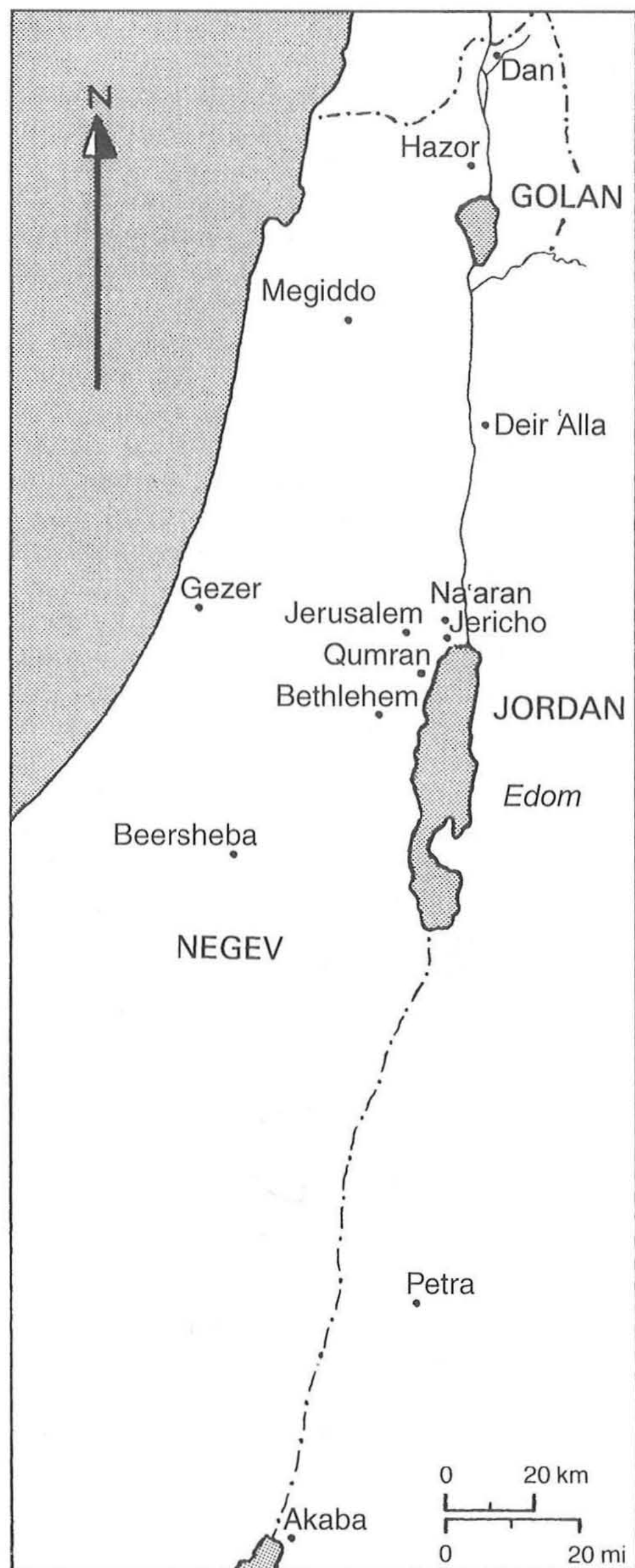
#### Abbreviations

*DJD* = *Discoveries in the Judaean Desert*. (Oxford).

*Murabba'at* 45 = the inventory number of manuscripts found at Murabba'at.

*Kh.Q.* = the inventory numbers of the inscriptions of Khirbet Qumrân, published in *Khirbet Qumrân et 'Ain Feshka*, II, edited by J.-B. Humbert *et al.* 2003. (Fribourg).

## Summaries of Lectures



### Tel Dan in the Early Iron Age: A Cultural Crucible

David Ilan

This lecture dealt with the Iron Age I (c. 1200–1000 BC) strata of Tel Dan – Strata VI, V and IVA. The relatively large aerial extent of the exposures and the large quantity of *in situ* material, including more than 400 complete pottery vessels, comprises a good database for an attempted reconstruction of economic, social and political structures and processes. The early levels indicate more emphasis on domestic production, storage and control, while later levels show more centralized patterns. Population growth can be observed along with fluctuating kin group organization and the development of lineage ‘neighbourhoods’. Several social identities can be inferred and a process of ideological consolidation identified. One conclusion is that the Iron I population of the region consisted of a mixture of social groups, some with local origins, others from more distant places (Cyprus, Phoenicia and southern Israel and Jordan) that existed in a system of fluid social boundaries and expedient alliances, all within the framework of a highly variegated environmental mosaic. A long series of radiocarbon dates sampled by H. Bruins and J. van der Plicht (Gronigen University) from Stratum V was discussed critically as was the Biblical account of the Danites’ migration in Judges 18.

### Tell Deir ‘Alla – From Urban Centre to Graveyard

Gerrit van der Kooij

The mound of Deir ‘Alla lies in the Jordan Valley, to the east of the river Jordan, close

to the River Zerqa, the ancient Jabbok. Archaeological work was started there by Henk Franken in 1960, and in 2000 the fourteenth season took place as a project of cooperation between Leiden University and Yarmouk University at Irbid, running since 1980, not only for research purposes but also for training archaeology students.

By now quite a lot has been studied, although it is still only a very small percentage of the site as a whole, with work at first concentrating on the northern slope and then subsequently on the eastern summit, with the last four campaigns focusing on the southern slope and eastern foot. Excavations also took place – within the programme – at the nearby site of Tell el-Hammeh, with remarkable results concerning iron production during the Iron II period. A detailed landscape study is starting next year and will partly focus on ancient irrigation systems.

The goals of the excavations and the research are concerned mainly with the study of the large variety of settlements through time at this spot. This settlement history is especially interesting for geographical and historical reasons. Geographically this middle part of the Jordan Valley is a steppe zone, which makes it difficult to practice agriculture relying on rainfall only. This dry-farming belt is fine for a small population, but a large group requires irrigation works. On the other hand the valley is suitable for sheep- and cattle-herding during the winter months, because there is no frost and often there are good pastures. In both cases closer economic and social contacts with the milder hilly regions to the west and east are necessary, with its Mediterranean climate (i.e. more rain) and good grazing areas in summer. Historically we are dealing with a sequence of time extending between c. 1700–300 BC, as well as for a couple of centuries AD. In archaeological terms the earliest sequence of time comprises the Middle Bronze, Late Bronze and Iron Ages – a very dynamic period in the southern Levant, with a strong variety of societies, some complex urban ones and

others much more segmented pastoral and peasant societies. This variety seems also to be exemplarily reflected in the history of the use of this part of the valley (the Ghor Abu Obeidah – or the biblical ‘Valley of Succoth’) and in the history of the site of Deir ‘Alla itself. Furthermore such an archaeological history has to be connected with historically known people, such as the local Canaanites, Israelites and Ammonites, and the foreign Egyptians, Aramaeans, as well as the imperialistic Assyrians, Babylonians and Persians.

Let us now look at the vicissitudes of life at Deir ‘Alla through time, starting with the Middle Bronze Age town and ending with the Mamluk period graveyard.

The Middle Bronze Age town had its beginnings around 1650 BC (the beginning of MB III), when people, coming from some unknown place, established a settlement on a low natural hill. At this time Levantine society had developed into a complex social and economic system with city-states. Each city was an economic and political centre for its surrounding region, and often a centre for international trade. This applied also to the small town of Deir ‘Alla, covering an area of 2–3 *ha* (judging by the excavated locations) and probably surrounded by a rampart, with substantial buildings, and clearly with inhabitants who took part in the international distribution of rare implements, such as a trident and a shaft hole axe. After about a century the Middle Bronze Age buildings collapsed, accompanied by a fire, but it appears that much of the town had been deserted beforehand, possibly at the time of the appearance of the eighteenth dynasty of kings of Egypt.

The Late Bronze Age is represented by a new and large settlement. The buildings were generally of a lighter structure than those of the previous town, and had a different orientation. At the northern edge of the site, the earthworks were levelled on top and used as a platform for a religious centre. The town continued to suffer local or more extended collapses and gradually decreased in size. Signs of connections with the Egyptian administration, known from

the region, were also found at Deir ‘Alla, such as a seal of Thotmes III and a seal of Rameses II.

The final phase (E) of this Late Bronze Age town is fairly well understood because it was excavated over a larger area (c. 1100 m<sup>2</sup>) and because it suffered destruction by a sudden earthquake and a severe fire. During this final phase the town had extended again to a size of about 3 *ha*. The excavated parts show some functional division of the town: the northern edge showed features connected with religious affairs (excavated in the 1960s), including a large long-room temple, as well as some rooms with precious or special objects, including faience and Mycenaean pottery. On the southern slope a probable domestic quarter, with storage facilities and large rooms, was partially excavated. What may have been an industrial quarter was established outside the town proper, to the southwest, with clear indications of metal and possibly faience craftsmanship, and for the storage of goods. The final destruction took place during or just after the reign of Pharaoh Tawesret (c. 1185) whose name was found on a faience vase in the temple.

The economy depended on irrigation agriculture, pastoralism and craftsmanship, as well as on trade and exchange, judging from the Egyptian, Syrian and Mycenaean objects found, as well as a jar stopper with an Egyptian seal impression. An indication of the identity of the population may be found in the texts on clay tablets, found in the northern and recently also in the southern quarters, but they have yet to be deciphered.

The Iron Age at the site began around 1150 BC with a new open settlement and it lasted until about 350 BC. This period is conveniently divided at the site into two main periods: the first period from c. 1150 to 950/900 BC (covering the Iron Age I and IIA periods), when the whole surface of the ruined Late Bronze Age site was used, with evidence of continuous settlement. Much of this was excavated already in the 1960s. The second period extends from c. 950/900 to 300 BC (covering the Iron Age IIB, IIC and III periods) and was established on the

eastern part of the site only, with frequently interrupted settlements of various types.

Mention should be made of Phase X, with its remarkable group of fourteen sealed storage vessels originally containing liquids, probably olive oil. One of them included thousands of calcified ants and some centipedes and woodlice. Their purpose is unclear, as well as the find of parts of skeletons of two young children in a rubbish heap.

An especially interesting phase of this second period is Phase IX which was destroyed by an earthquake and fire, c. 800 BC. It was a densely built village, with irrigation agriculture in the surrounding lands, and some herds being grazed at a distance. The village was partly built over a fill with a very large pit, 15 m in diameter, with the oblique sides carefully clad with mudbricks. The new settlement was intensively used for domestic purposes, including remains of food preparation (a large selection of kitchen herbs were found), as well signs of much weaving and other activities. These people had a few connections with the Phoenician world, but much more with the Aramaeans as was made clear by the discovery of a number of Aramaic inscriptions. Interestingly, a special room had on its lime-plastered western wall a long column of text, beautifully written in Aramaic script with black and some red ink, and with some drawings. The text of ‘Balaam, the son of Be’or’, describes the story of this prophet who predicted a negative outcome for the population in the form of ‘curses’.

The following Phase VII new village was quickly established, densely built up, and quickly destroyed by fire and probably again by earthquake. This village dates from the early period of Assyrian domination, from around 700 BC, and seems to have had a population transplanted from elsewhere, as is evident from the many cultural differences to those of the preceding phases, notably with remarkable stone tools, bone tools, plaster objects, and so forth. Some pottery traditions did continue, but many new types appeared made on a fast wheel.

The succeeding villages were generally open in plan with large courtyard spaces and features indicating that they were part of the Neo-Babylonian world and had Ammonite connections (some *ostraca* were found). Following this is a settlement from the time of the Persian Empire, again with some *ostraca* finds, written in Aramaic, the official *lingua franca* of that time. One *ostrakon* requests that families deliver stones for the rebuilding of a gate.

Later occupation at the site was irregular, with abandonment during Hellenistic times. The site was deserted for about one and a half millennia until it was resettled again. The new village was established near the northeast foot of the tell, with the summit of the tell being used as a graveyard with the bodies positioned to the west, and with the heads facing south towards Mecca. This graveyard probably began to be used in the Mamluk period (thirteenth to fifteenth centuries AD) when the Jordan Valley was a centre for the sugar production needed for Egypt, and several villages were established in the neighbourhood at that time, sustained by irrigated agriculture. There are now clear indications that these agricultural activities lasted all the way through to the early years of the Ottoman Empire. It is even possible that the irrigation system that was still being used up to the 1960s – before the new East Ghor canal system came into use – was actually based on the original Mamluk system. Hence, I believe we can study this irrigation system in order to develop a model for understanding the irrigation works that existed here during the Bronze and Iron Ages.

**New Light on the Rise of the Biblical Kingdom of Edom: Recent Archaeological Excavations in Southern Jordan**

Thomas E. Levy

Following the collapse of many Late Bronze Age civilizations in the eastern Mediterranean around 1200 BC, there was a

period of upheaval followed by the emergence of a number of small secondary states in what is today Israel and Jordan. These Iron Age (c. 1200–586 BC) kingdoms or 'statelets' are known to us from the Hebrew Bible and extra-biblical texts from the traditional centres of ancient Near Eastern civilization in Egypt and Mesopotamia. By the Iron Age II period (c. 1000–700 BC), some of these Levantine statelets included Israel, Philistia, Phoenicia, Ammon, Moab and Edom. Until recently, archaeologists assumed that the emergence of Edom, the biblical kingdom that extended from the Wadi Hasa south of the Dead Sea to the Wadi Hisma in the red sandstone region of southern Jordan, happened around the seventh century BC as a result of the expansion of the Assyrian empire. This assumption was based on Crystal Bennett's discovery of a single clay seal impression at Umm al Biyara in Petra that read 'Qos-Gabr – King of Edom' – a name that could be traced in Assyrian annals dated to c. 670 BC. Based on this single discovery, Iron Age 'Edomite' ceramic assemblages, architecture, jewellery and other realms of material culture from Iron Age sites in Edom were all dated to the seventh and, hesitatingly, to the eighth centuries BC. The underlying impact of these assumptions was to discredit the historicity of references to Edom in the Hebrew Bible and to discredit the interpretations of the pioneer American archaeologist, Nelson Glueck, who made the first systematic regional surveys of Edom in the mid-1930s. Recent archaeological excavations and surveys in the Jabal Hamrat Fidan region in the copper ore-rich Faynan by a joint venture between the University of California, San Diego and the Department of Antiquities of Jordan have thrown new light on the rise of Iron Age Edomite kingdom that challenge the existing interpretive framework.

Since 1997 I have been the senior Principal Investigator of the Jabal Hamrat Fidan project with my co-Principal Investigator Russell Adams and co-Field Director Mohammad Najjar. The Jabal Hamrat Fidan (JHF) is a small granite

mountain range that borders the eastern side of the Wadi Arabah and represents the 'gateway' to the copper ore-rich Faynan district. The aim of the JHF project has been a 'deep time' study of the role of copper ore procurement and metallurgy on the evolution of societies from the beginnings of sedentary village life and animal domestication in the Pre-Pottery Neolithic B Period (c. 8500–6000 BC) to the rise of the first historic state level societies in the Iron Age. From the beginning, we felt that Iron Age research in Edom had been biased to the highland region where the most famous sites are located such as Busayra, now identified with the Edomite capital of Bozrah in the Hebrew Bible (Genesis 36:33, Chronicles I:44, Amos I:12), Tawilan, Umm el-Biyara, Sela, and other locales. Geographically, Edom has a very important lowland component that focuses on the Wadi Arabah and especially the Faynan district which contains the largest deposits of copper ore in the southern Levant. Our explorations of Iron Age Edom began with the excavation of an Iron Age cemetery at Wadi Fidan 40 – the only Iron Age cemetery in Edom to have been excavated to date. This was followed by intensive, full-scale archaeological surveys of the Wadi Fidan, Wadi Guwayb, and Wadi el-Jariyeh, all of which had been visited by Nelson Glueck and other early researchers such as the Czech Orientalist, Alois Musil. The centrepiece of our Iron Age research is the excavation in 2002 of the largest copper factory in the southern Levant – Khirbat en Nahas. A synthetic article concerning this research appeared in the December 2004 issue of the British journal, *Antiquity*. The lecture presented for the Anglo-Israel Archaeological Society at the British Museum (September 9th, 2004) was a summary of this research paper and an overview of our new understanding of Iron Age Edom in the light of the JHF project. Although sketch maps were made of Khirbat en Nahas (KEN) by both Musil and Glueck, no detailed mapping or large-scale excavations were made at the site. As part

of the German Mining Museum archaeo-metallurgical research carried out in the Faynan district during the 1980s and early 1990s, an archaeological sondage was made at KEN by Volkmar Fritz along with some sampling of the numerous copper slag mounds that surround the site.

The new UCSD-DOA excavations at KEN show two distinct phases of metal production at the site in the twelfth–eleventh centuries BC and in the tenth–ninth centuries BC. This is based on our stratified excavations at KEN in the gatehouse of a huge (73 × 73m) Iron Age fortress, a specialist 4-room building devoted to slag processing, and a large slag mound associated with a well-preserved stone building. KEN represents the first stratified archaeological site in Edom that has been dated using high precision radiocarbon dating methods. This work was carried out during my sabbatical in Oxford in 2003 at the Oxford Radiocarbon Accelerator Unit at the University of Oxford under Thomas Higham and the Centre for Isotope Research, Groningen University, the Netherlands under Johannes van der Plicht. Thus, for the first time in Iron Age archaeological research in Edom it is possible to date archaeological assemblages with absolute radiocarbon dating methods. If the control of time and space are the archaeologist's most precious commodities, the application of high precision radiocarbon methods at KEN represents a fundamental advance in the control of time for testing social, evolutionary and historical problems in this part of the southern Levant.

The control of space and the context in which artefacts are found is the other essential factor for archaeological investigations. The development of Geographical Information Systems, or GIS, enables all researchers interested in the spatial organization of natural phenomena to store, analyse and produce maps using high-speed computer technology. To bring the control of space in archaeological research to its finest degree of precision, the JHF project has helped to pioneer 'digital archaeology' methods linked to

GIS. For the excavations at KEN, every collected artefact or group of artefacts was recorded with Global Positioning System (GPS) technology so that the data could be downloaded into our GIS. Thus, in the specialist slag processing building all artefacts including a range of imported Iron Age scarabs, metal arrowheads, Egyptian glass bracelet fragments, and metallurgical finds were digitally recorded for x, y and z (elevation) coordinates that could be carefully linked to the radiocarbon samples dated in Oxford and Groningen. In this way typologically characteristic artefacts such as scarabs and arrowheads can give relative historic dates and can be coupled with absolute calibrated radiocarbon dates. The end result demonstrates that the 'highland bias' of archaeological investigations in Edom that linked the emergence of the Edomite kingdom with seventh century BC Assyrian domination is incorrect. We now have to look for local processes that led to the rise of the kingdom of Edom. The key to the emergence of the Edomite kingdom must lie in the copper ore-rich lowlands of Edom and their control of metal production and trade. As most Biblical scholars assume that the Hebrew Bible was finally edited during the seventh century BC, some Biblical passages concerning ancient Edom may shed light on the relationship between Biblical history, archaeology and anthropological models of social evolution. For example, in Genesis 36:31 it is stated that, 'These are the kings who reigned in the land of Edom, before any king reigned over the Israelites.' Leaving aside the precise dating of this text, it implies that social complexity emerged in Edom prior to Israel. The discovery of a thick production layer of copper slag under the specialist building excavated at KEN is dated to the early Iron Age and points to a sophisticated, highly organized period of metal production that, based on radiocarbon determinations, scarabs, arrowheads and early 'Midianite' pottery forms, predates the rise of the United Monarchy (tenth century BC) in Israel. A second possible link with the Hebrew Bible

comes from II Kings 8:20 that refers to the period of the Divided Monarchy and the Kingdom of Judah under King Jehoram traditionally dated to c. 848–841 BC which states: 'In his days Edom revolted against the rule of Judah, and set up a king of their own.' The JHF project has produced a total of 37 high precision radiocarbon dates and the majority cluster around the mid-ninth century BC – precisely around the time that the Edomite revolt supposedly took place and when their own kingdom was established. The main building phase at KEN associated with the specialist building and the building excavated by the German Mining Museum are both dated to the mid-ninth century BC. Thus, there is a possibility that the expansion in metal production at KEN during the ninth century BC is linked to the emergence or re-emergence of the Edomite kingdom as reflected in text above.

Although the excavations at KEN represent a small window on the processes of Edomite social evolution, they do establish a need for further investigating the link between the Hebrew Bible, science-based archaeology and the social evolutionary models rooted in history and anthropology. It was Nelson Glueck, back in the 1930s that suggested that the eleventh–ninth centuries BC were probably the most intensive periods of Iron Age metal production at Khirbat en Nahas and the Iron Age sites of the lowlands of Edom. Our research indicates that rather than 'de-construct' Glueck, as has been the trend over the past 25 years, it is time to reexamine many of his assertions concerning the Bible and archaeology. Glueck is accused of having carried out his surveys and excavations in Jordan with a trowel in one hand and a Bible in the other. It is likely that he exaggerated many of the links between archaeology and Biblical texts. On the other hand, when examined critically, the Hebrew Bible is the oldest ethnohistorical document linked with the Iron Age of the southern Levant. It is impossible to do Iron Age archaeology in the southern Levant without carefully examining the relationship between the

Biblical text and the archeological record. The challenge is to find new tools that will enable the researcher to be as objective as possible. The melding of GIS with high precision radiocarbon dating methods in archaeological research, as demonstrated in the JHF project excavations in southern Jordan represent one step in this direction.

### **Idols of the People: Miniature Images in Canaan, Israel and Judah**

P. R. S. Moorey

A substantial amount of what is loosely called cult or ritual equipment revealed by excavations in the area of modern Israel cannot be connected with any reference in the Hebrew Bible. Much of it is made of clay, the material most available to the greatest number of people, and baked like pottery. Such objects come not from places exclusively used for cult purposes, but from domestic contexts and workplaces, or sometimes from graves, where they are usually unbroken. Otherwise they were discarded, battered and broken like shards of pottery. They provide a primary source for understanding the religious culture of Canaan, Israel and Judah within households, particularly as it involved women and families. The common female images present a basic question: do they represent women as human suppliants or the goddess to whom prayers were addressed? Handheld miniature images are usually interpreted as a means for achieving authority over the animate or inanimate subjects, modelled as in the performance of magic. Many of the terracotta images of the time of the Monarchy are broken, some, if not all, in ways that suggest this was systematic and intended to destroy their potency when discarded.

In late Bronze Age Canaan (c. 1650–1150 BC) the commonest terracottas are mould-made, handheld plaques with low relief designs of a standing or reclining nude female. At this time miniature male votive images were made in metal. One group of females shows predominantly

Egyptian influence and may represent goddesses as similar images also served as necklace pendants. The other group is more Syro-Babylonian in style and is, perhaps, more likely to represent a semi-divine intermediary between humanity and deities rather than a divine figure or its best-known statue.

The free-standing, hand-modelled terracottas of Israel and Judah under the Divided Monarchy include standing females ('Judean Pillar Figurines'), male horse-riders, a variety of animals and models of furniture, buildings and other objects. They illustrate the household, family-related cults of the period in which women played the key role. Here again it is not self evident whether the female images are representative of a goddess, perhaps Asherah, or of the human female suppliant. But if the females are deities, what then of the male horse-riders of the period? Amongst the many challenges presented by these various, but still enigmatic images, this remains the primary open question.

### **Ceramics, Coastlines and Chronology: Unlocking the Mysteries of Trade in the Iron Age**

Nicola Stacey

Ceramics, considered the bottom line of archaeological studies, can offer remarkable insights into a whole range of aspects of the ancient world. The study of Iron Age Black-on-Red pottery throws light on chronology, trade patterns, and the process of commerce throughout the East Mediterranean. Showing many similarities to Cypriot pottery, but dated in the Near East almost a hundred years earlier than in Cyprus, the origin of Black-on-Red had long been a subject of debate. The uncertainty caused by such a distinctive ware offering different dates according to whether the Cypriot or Near Eastern chronology was applied, has had serious repercussions on the chronology of the

Near East, as well as on our perceptions of intercultural relations in the East Mediterranean world in the Iron Age. By investigating Black-on-Red pottery from the Cypriot angle, it has been possible to establish that Einar Gjerstad's traditional dates for the ware are too late: it occurs within Cyprus, and was most probably exported to the Near East, by the end of the tenth century, during the Cypro-Geometric II period. The 'deconstruction' of Gjerstad's Cypriot chronology has important repercussions for the dating of sites within Cyprus, but also the dating of sites in Phoenicia which, tending to rely on the Cypriot chronology, have been out of

line with developments further down the coast. Raising the dates for the introduction of Black-on-Red in Cyprus, such that the chronological discrepancies are resolved, allows us to see a far more interconnected East Mediterranean world. In addition, tracing the process of dissemination of the pottery further west than Cyprus indicates that during the ninth–eighth centuries BC Cyprus had a key role in trans-Mediterranean trade, and was more likely to have been directly involved in the trade of perfumed oil to the Dodecanese islands and Crete than the Phoenicians. Ultimately this Cypriot trade was supplanted by that originating from Corinth.

## Obituaries

J. B. SEGAL (1912–2003)



From time to time fate or providence (or whatever designator is preferred) produces an individual of such talent and intellect which when combined with physical courage, modesty and generosity lifts the spirits of all who have known him. Such a person was Judah Benzion Segal always known affectionately as 'Ben'.

Ben Segal was born in Newcastle on 21<sup>st</sup> June 1912, the son of M. H. Segal a distinguished biblical and Mishnaic scholar and the younger brother of Samuel, a medical doctor, who became the M.P. for Preston and who was later given a life peerage for services to the newly formed N.H.S. After attending Magdalen College School in Oxford, Ben went up to St Catherine's Cambridge to read Semitic languages where he obtained a first. He also managed to find time to earn his colours in 1935 and 1936 with the Cambridge University Boxing Club. A DPhil followed at St. John's College, Oxford.

He then sought work with the Civil Service where his linguistic skills could be utilized and in 1939 he was posted to Khartoum. At the outbreak of war he found himself in a reserved occupation but preferred to leave this and applied to British Army Headquarters in Cairo for active service. With his expertise in modern Semitic

languages (as well as ancient) he was accepted by British Intelligence and given the rank of lieutenant. Most of the war he spent in the Middle East: in Cairo, Jerusalem and later in Cyrenaica (now Libya). In 1943 he led a small group in the desert behind enemy lines, in the harshest of conditions, providing crucial information about enemy deployment and movements. He took the surrender of the Italians at a place called Derna and released some allied prisoners who were in hospital there. A small Union flag that he made from a handkerchief and a blue pen with strips of red material from a Sanussi woman's petticoat was flown from the town hall and this can now be seen on display at the Imperial War Museum. As always, Ben was reticent about this period of his life although he did produce a small volume for relatives in 1996 entitled 'Time of War'. The Military Cross that he was awarded for bravery he declined to mention in his 'Who's Who' entry.

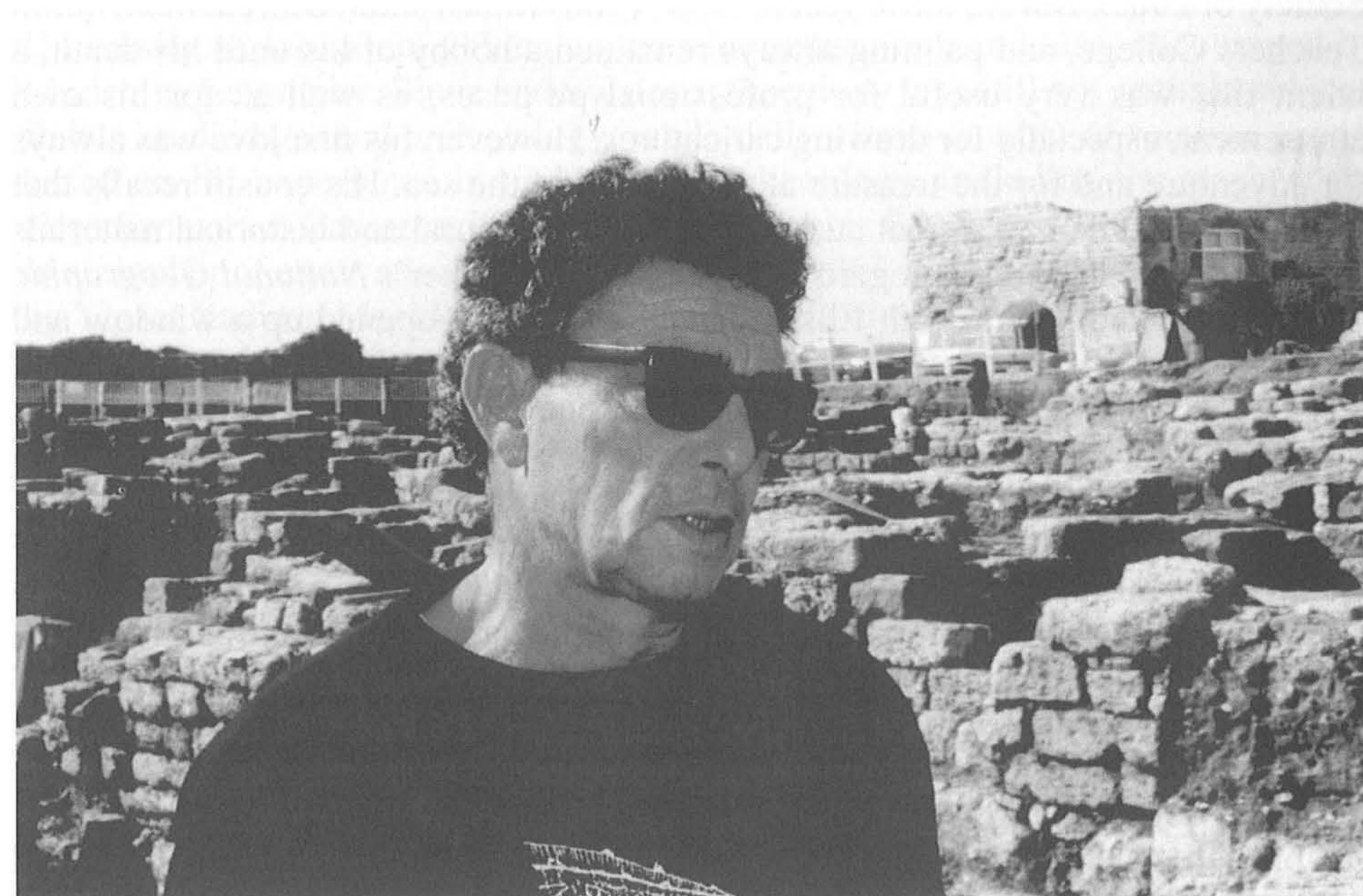
At the end of the war he resumed his academic work at London University and was made Professor of Semitic Languages at SOAS in 1961. During this period numerous books and articles in learned journals appeared, his major work being *Edessa: the Blessed City*. The study of Syriac, whose roots are to be found in Edessa, was the language of the early Christian churches and was one of the main areas of his research. In 1973 he was greatly touched and honoured by being granted the freedom of the city of Urfa (the present-day name of Edessa) in eastern Turkey. Apart from linguistics, but certainly allied, was his enduring interest in the Jews of Cochin. In 1993 appeared another authoritative work, *A History of the Jews of Cochin*. His last major publication was in 1999: *A Catalogue of the Aramaic and Mandaic Incantation Bowls in the British Museum*.

During his post-war academic years and well into his retirement, Ben was always closely involved with his local Jewish community and with matters educational. He was for a time Principal of Leo Baeck College at a crucial time in its development and President of the North Western Reform Synagogue for almost thirty years. He was also Chairman of the Anglo-Israel Archaeological Society, which he took over after the death of his friend and colleague Richard Barnett. This chairmanship was particularly felicitous as his brother, Lord Segal, had been President of the Society between 1974 and 1985.

Throughout his long productive life the qualities fondly remembered by all of us who knew him were modesty, kindness, generosity of spirit and an immense learning unencumbered by any arrogance. His two daughters, his wife, Leah, and all who worked under him and with him will miss a good friend.

Ashley Jones

## AVNER RABAN (1937–2004)



On 11th February 2004 Professor Avner Raban of the Department of Maritime Civilizations and the Centre of Maritime Studies at Haifa University, died quite unexpectedly while visiting London during a sabbatical at Oxford. Raban was born 67 years ago in Kibbutz Ramat David in the Jezreel Valley and eventually over the years became one of the pioneers in the field of underwater archaeology in Israel, achieving world-wide fame with awards in 1994 from UNESCO, and from CMAS (the International Confederation of Underwater Activity) a special award for his contribution to Underwater Research presented by the Crown Prince of Japan.

To follow his career fully, with all his achievements and publications, in a short obituary such as this is virtually impossible, and so I shall focus more specifically on the history of his work relating to underwater archaeology, which, as we shall see, more or less mirrors the development of this unique discipline in Israel. I shall also try to convey my own personal recollections of him as a professor and former 'boss', and what he was like later as a colleague when I was involved in developing my own independent career as a medieval maritime historian and underwater archaeologist.

Raban's interest in underwater activities first began in the 1950s shortly after completing his military service, while fishing with nets along with two of his closest friends from the army, Shuka Shapira and Haim Stave. Not only did they succeed in catching fish, but they also accidentally managed to pick up ancient amphorae.

In 1958 Dr Elisha Linder first introduced the three of them to his collection of underwater artefacts that he kept in his house in Kibbutz Maagan Michael. This meeting is what first instigated the later development of the Undersea Exploration Society of Israel. During those years (1958–1960) Raban studied Art at the Oranim Teachers College, and painting always remained a hobby of his until his death, a talent that was very useful for professional purposes, as well as for his own amusement, especially for drawing caricatures. However, his first love was always for adventure and for the treasure at the bottom of the sea. His cousin recalls that even as a child he had a great curiosity regarding cultural and historical materials and was stimulated in this regard while reading his father's *National Geographic* magazines. His meeting with Elisha Linder eventually opened up a window and provided an opportunity to materialize his childhood fantasies.

The year 1960 was a very significant milestone in his career with the arrival of Edwin A. Link at Caesarea. Link brought with him a ship equipped with a state-of-the-art special apparatus – the airlift – for excavating underwater. The locals regarded him as a possible antiquities robber, but Linder decided to send Raban and another on a mission of 'espionage' to find out what the airlift was actually like and how it worked. A year later, Linder, along with Raban and Shapira, and with a number of leading professors from the Hebrew University of Jerusalem, founded the Undersea Exploration Society of Israel. Linder served as its first general secretary and Raban was the Society's diving instructor, a skill that he had already mastered during his military service as a frogman. The Society, which was first established under the umbrella of the Israel Exploration Society, eventually joined the International Confederation of Underwater Activity (CMAS), headed by the famous Jacques Cousteau, and some of the Society members, including Raban, were invited to participate in a number of underwater expeditions working in the Mediterranean, such as at the famous Yassi Ada shipwreck off the coast of Turkey (1963). Following the ways of the Kibbutz, one person within the group had to be chosen to join the scientific work, and this included future studies in a university. Raban was chosen for the job and many of his friends wondered at the time whether this man, who had already gained a reputation for being wild and rebellious, would survive within academia.

As an archaeologist under the umbrella of the Undersea Exploration Society of Israel, Raban participated as co-director in various underwater archaeological expeditions working along the Israeli coastline, despite the fact that he had still not undertaken formal studies in archaeology: notably at Akhziv (1961); in underwater surveys along the northern Israeli coast (1964), in an expedition led by Throckmorton, one of the world pioneers in underwater archaeology; at Akko (1965), where he also served as administrator; and at Athlit (1966). Reading the field report of the Akko expedition of 1965, one can tell that Raban was an autodidact. Urged on by Linder, Raban founded the maritime centre at Caesarea with diving lessons at the core of its activities; its aim was to increase the number of civilian divers that could man the Undersea Exploration Society. Hence, the navy's diving rules were accordingly adjusted to suit civilian purposes. Although

this centre did not survive for very long, it did lay the foundation for the future Israel Diving Federation, some members of which still take part as volunteers in local underwater excavations.

It was only in 1966 that Raban began his academic studies at the Hebrew University of Jerusalem, taking courses in archaeology and also, encouraged by Linder, in geography. Since underwater archaeology was still not a taught subject, Raban studied general archaeology, while delivering a number of seminar papers based on his experience in underwater archaeology, including the work at the Herodian port of Caesarea. In order to qualify for his BA degree, Raban participated in a number of excavations on land, notably in 1967 in the excavations conducted by Yigael Yadin at Megiddo, and in 1969 as a field director of the Hazor excavations which were also conducted by Yadin. In June 1967 the Six Day War broke out, and Raban, the military frogman, was sent with his friend Shuka to check the Straits of Tiran and Snapir in the Red Sea for mines. During these dives he discovered several wrecks of the seventeenth and nineteenth centuries. This discovery eventually led to underwater excavations at Sharm-el-Sheikh (1968) in which Raban was the scientific director, even though he was still only a BA student at the Hebrew University. Following his graduation in 1969, Raban conducted an interdisciplinary survey of the Gulf of Eilat and of the eastern coast of Sinai in 1969–70; the underwater excavation of the 'Mercury Wreck' in the Red Sea in 1972; and the underwater excavation of a wreck in Na'ama Bay in the Red Sea in 1973.

It was during this period that Raban acquired worldwide fame. In September 1970 he participated in the second world congress on underwater sciences of the CMAS at Havana in Cuba, talking about 'The Sharm-el-Sheikh wreck'. On that occasion he was elected for three years as the chairman of the archaeological commission of the CMAS. Of all the accolades that he received before his death, Raban considered the most important the words written about him by Yigael Yadin in the Hebrew archaeological journal *Qadmoniot* (2 [14], 1971: 37): '...the qualities inherent in A. Raban, with a combination of the typical "sea wolf" and the professional archaeologist, makes him the right person to introduce in this issue [of *Qadmoniot*] the main methods used in underwater archaeology that differ considerably from those used in land archaeology'. This volume was published to mark a decade of underwater archaeology activities in Israel. At that time, Raban began teaching a side course on the subject at university and then two years later Elisha Linder formally established the Department of Maritime Civilizations and the Centre of Maritime Studies at Haifa University with a degree course for the MA. Until Raban achieved his PhD degree in archaeology at the Hebrew University of Jerusalem in 1981, he served as the senior marine scientist at the Centre of Maritime Studies and was a teacher in the department. Having obtained his PhD, Raban began advancing his academic career until in 1999 he was conferred full professorship, chairing at different times also the department, which has, since 2000, provided PhD degrees with Raban tutoring a considerable number of MA and PhD dissertations personally. His combination of geographical studies with underwater archaeology, enabled him to expand his research to examine the subject of land and

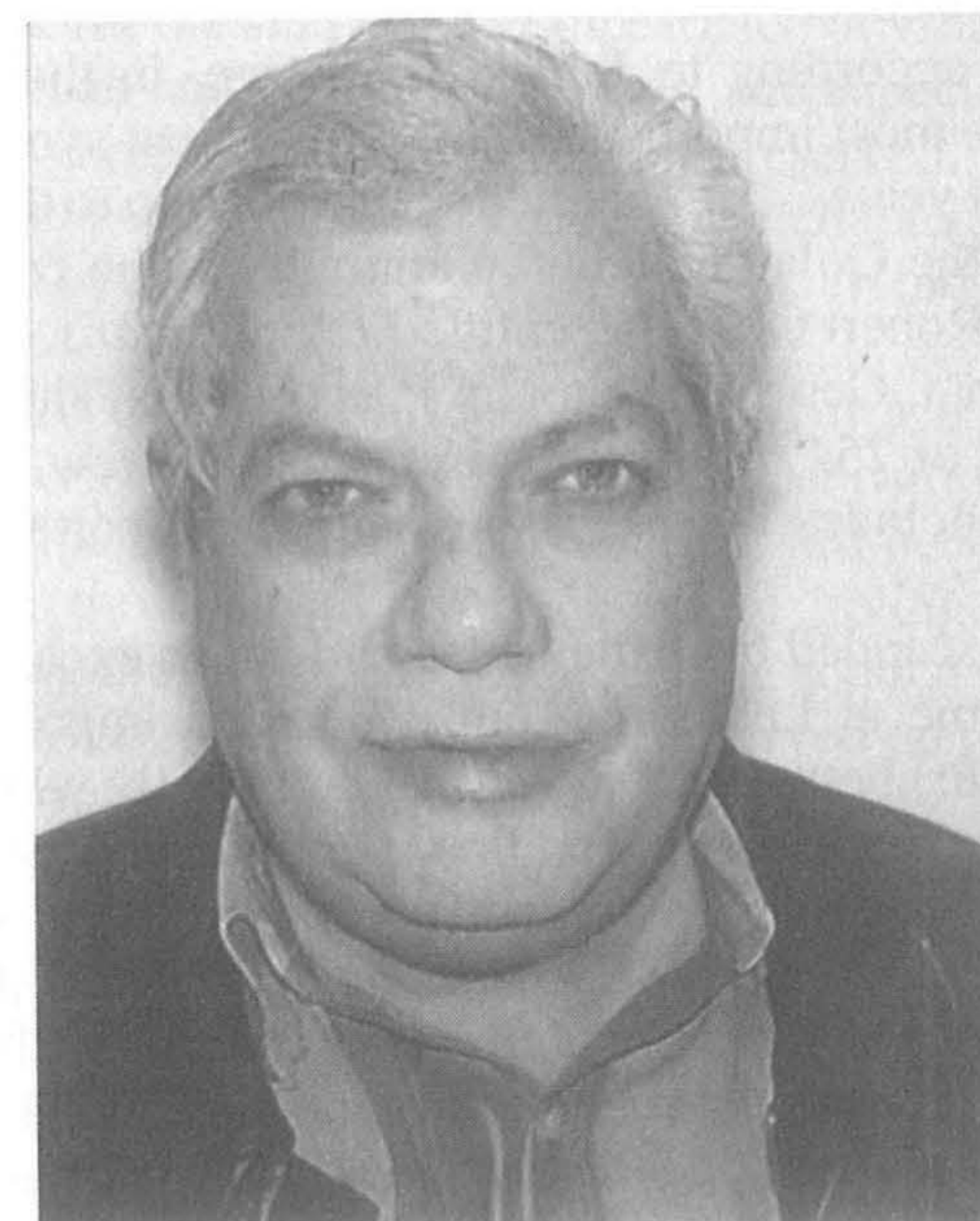
sea relations, and marine engineering, both of which were essential topics for the study of ports and port towns. His specialization of this field gained him membership between 1982–87 in the International Committee for Sea Level Changes in the INQUA project IGCP 200.

It was during my studies at the Department of Maritime Civilizations in 1975 that I first got to know Raban as a professor and I spent much time with him working on underwater expeditions along the coast of Israel, searching for wrecks and excavating ancient harbours at Acre, Dor, Atlit and Caesarea, as well as abroad in Crete and Sicily. Although my major field was medieval history, Raban gave me the enthusiasm to adopt underwater archaeology as my 'second' major field, which I still practice. Working with Raban was challenging. I rapidly came to realize what Yadin meant when he labelled Raban a 'sea wolf'. He was in many respects an individualist, even when dancing – producing his own rhythm and pace even when it contradicted the accepted one. With regard to his scientific work, Raban shared very little of his thoughts in conversation, but when he did produce views and solutions they tended to sparkle with genius. He was reluctant to accept other people's opinions and was always determined to keep his own even if that meant his views would at times have to be modified. Like a wolf he was very territorial and regarded the Israeli coastline much as his own; one had to be very brave to face Raban with the decision to do one's own work. However, if one succeeded, then he gave that person respect. His credo, he once told me, is that one does not wait for leadership and management but one should take it instead. His challenging behaviour led determined students like myself to strive for excellence and independence in their field. On the other hand, as a 'wolf' he surrounded himself with his own 'pack' which consisted of many old friends from the military, such as the above-mentioned Shuka and Haim, who had accompanied the development of the Department of Maritime Civilizations and had taken part in many of Raban's underwater expeditions. Until his death they would meet regularly, with talking, eating and a lot of drinking, and some of his favoured students were able to come along and hear stories and experiences from the 'good old days'.

As a true archaeologist Raban never abandoned his excavations and surveys on land, and between 1972 and 1992 he headed an archaeology survey of the western Jezreel Valley (incorporating the Maps of Mishmar Ha 'Emeq, Nazareth and Afula) which was published by the Israel Antiquities Authority in 1999. Among the sites he excavated were also ancient port towns which, as he correctly realized, served as the integral link between the land and sea. At Caesarea Maritima he dedicated many years of his life, to major research activities from the 1980s on. This work involved an international collaborative effort with universities from the United States, Canada and Europe, with hundreds of volunteers working at the site every year, on land and underwater as well, with Raban in charge of this massive project. Unfortunately Raban died before being able to complete the writing and editing of two volumes of his report entitled *The Harbours of Caesarea Maritima*.

*Ruthi Gertwagen*

## DAN URMAN (1945–2004)



Dan Urman, a professor at the Ben-Gurion University in Beer Sheva, passed away at the Soroka Hospital on Sunday morning, 14<sup>th</sup> March 2004. Dan's health had deteriorated seriously in 1999, and he died not long after the discovery of leukemia during a recent hospitalization. He had taught at Ben-Gurion University since 1979, attaining the rank of associate professor and serving periodically as chairman of the Department of Jewish History and the Bible.

Dan was born in Haifa, Palestine, on 1<sup>st</sup> April 1945. Following his service in the Israel Defense Forces (1963–1965) he began studying history and archaeology at the Hebrew University of Jerusalem. He received his BA in 1974 and his MA in 1976, after which he travelled to the United States for his doctoral studies at New York University. His dissertation, entitled 'The Golan during the Roman and Byzantine Periods: Topography, Settlements, Economy', supervised by Baruch A. Levine, F. E. Peters, and Lawrence H. Schiffman, was approved with distinction in October 1979. An expanded, revised version of his thesis appeared in 1985 as *The Golan: A Profile of a Region During the Roman and Byzantine Periods* (BAR International Series 269, Oxford). Dan's work on the Golan benefited from his fieldwork as team leader for the Archaeological Survey of Israel's survey of Arab villages in Israel and as an IDF Staff Officer for Archaeological Affairs in the Golan. In these capacities he led excavations at a dozen different sites in the Golan and published numerous articles.

The pinnacle of Dan's work on the Golan was undoubtedly his essay entitled 'Public structures and Jewish Communities in the Golan Heights', which appeared in D. Urman and P. V. M. Flesher, eds. *Ancient Synagogues: Historical Analysis and Archaeological Discovery* (Leiden: Brill, 1995, 317–617). This volume, which Dan co-edited, was, according to James F. Strange, in the *Review of Biblical Literature*, 'one of the most important volumes in ancient synagogue research that has appeared in recent years.' Another major contribution to the study of the socio-religious makeup of the Golan Heights in antiquity is the corpus of inscriptions jointly authored with Robert C. Gregg, entitled *Jews, Pagans, and Christians in the Golan Heights* (Atlanta, Georgia: Scholars Press, 1996). This handsome volume presents full editions of 254 inscriptions in Greek, Hebrew, Aramaic, and Latin from 46 sites and concludes by discussing settlement patterns in the Golan by religious group.

Eventually, Dan descended from the Golan and began excavating closer to Beer Sheva and to his home at Lehavim. Dan conducted important excavations at Nessana ('Auja el-Hafir) between 1987 and 1995, initially with the late Dr. Yosef Shereshevsky, and then by himself. The first of a three-volume excavation report, completed before Dan's death, has recently been published by the Ben-Gurion University Press, entitled *Nessana: Excavations and Studies*. Dan left behind the other two volumes in advanced stages of preparation, with the hope that his colleagues would see them through to publication. Dan was a dynamic, engaging and highly acclaimed speaker, appearing frequently at conferences and universities throughout the world. He was also a popular teacher and lecturer in non-professional forums, and was a scholar with a talent for reaching out to the public at large.

*Victor Avigdor Hurowitz*

Dan Urman was a friend and colleague of many years and we shared many common archaeological and historical interests, particularly in regard to the study of the Golan Heights, but also in terms of the Byzantine period archaeology of the Land of Israel.

Dan's love for archaeology began already during his youth while at high school at Kfar Ata, close to Haifa. His academic studies were made at the Hebrew University in the field of History and Archaeology, with the writing of his PhD on the Golan during his sojourn in New York University in 1979. On completing his army service, Dan headed between 1965 and 1968 a team investigating archaeological remains found within abandoned Arab villages, and this work was made on behalf of the Archaeological Survey of Israel, an association founded in 1964 to map the ancient surface remains scattered across the country.

Following the 1967 war, Dan was appointed as IAA Staff Officer in charge of the antiquities in the Golan, and in the following years he conducted a series of salvage excavations at Baniyas, Zurman, Juhader, Kursi, Katsrin, Horvat Zemel, Khispin and Horvat Tibeh. At Kursi, Urman was joined by Vassilios Tzaferis of

the Israel Department of Antiquities, in the uncovering of a Byzantine church and monastery complex. Urman also undertook a major architectural survey of the village of Rafid, which was later utterly destroyed in 1973. This unpublished survey is of great importance for the study of the appearance of village settlements in the Golan during the Roman and Byzantine periods, and especially because some of the ancient buildings there were extremely well preserved. Dan had managed to write some of the archaeological and architectural chapters of a monograph on Rafid, but was unable to complete the writing, and his friends are now planning to do this on his behalf.

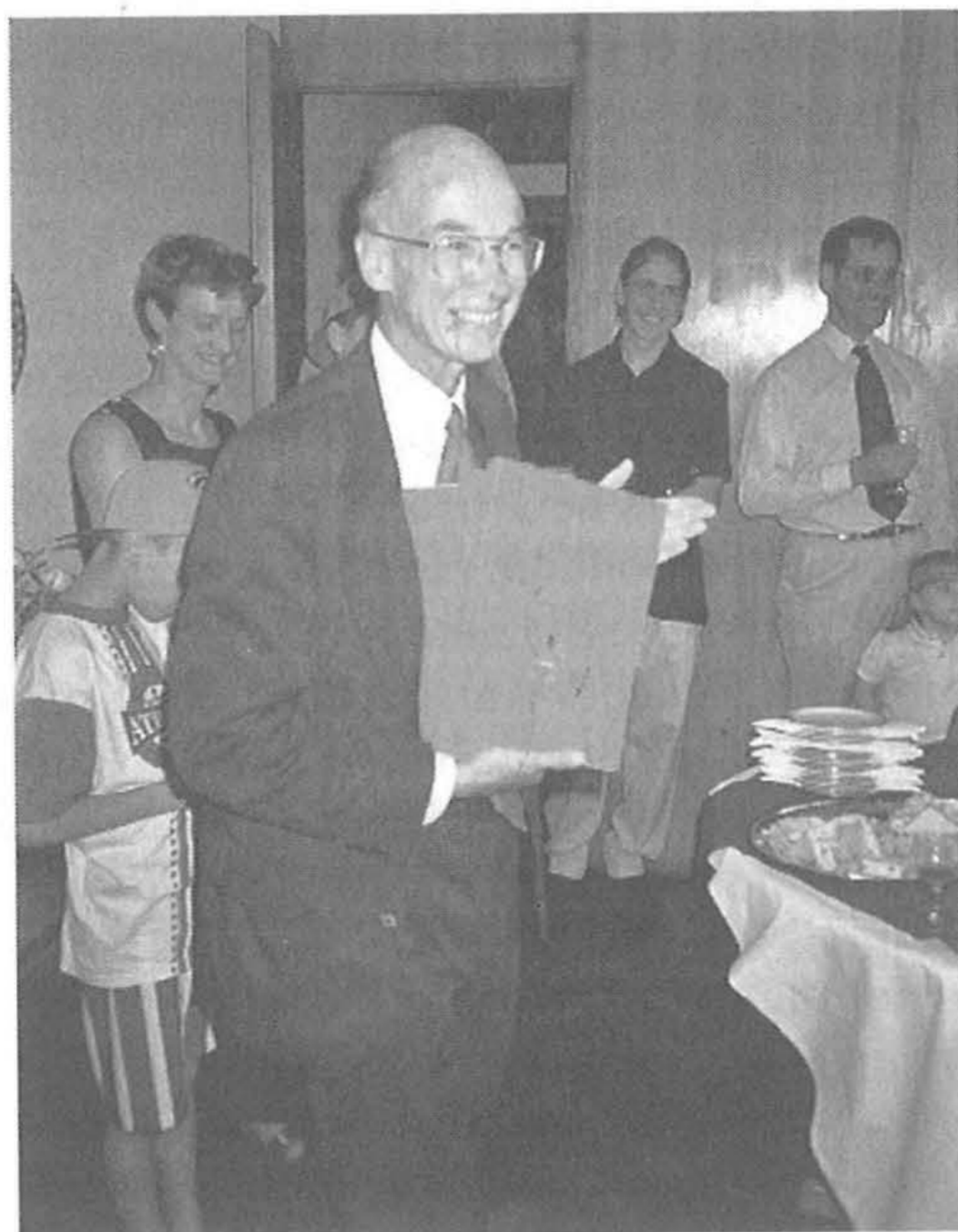
In 1979 Dan joined the Ben Gurion University in a teaching capacity and eventually began a series of extremely important excavations at Nessana in the Negev. The first volume of his work at Nessana has just been published. Dan was a good speaker and travelled all over the world giving papers at scientific conferences and during those years he was a visiting scholar in the Jewish Studies Department at the University of London. He also served on the Editorial Board of the *Bulletin* of the Anglo-Israel Archaeological Society. Dan's students will remember him as a demanding teacher who expected high scientific standards from them, but the door to his office was always open to anyone for advice and consultation. His death is a massive blow to his family and close friends, as well as to the community of archaeologists and historians worldwide.

May he always be remembered!

*Shimon Dar*

## PETER ROGER STUART MOOREY (1937–2004)

ARCHAEOLOGIST, MUSEOLOGIST, MENTOR, COLLEAGUE, AND FRIEND  
– A TRIBUTE



Roger Moorey presenting the three volumes of Claudine Dauphin's *La Palestine Byzantine* at the book launch, Somerville College, Oxford, 10<sup>th</sup> July 1999 (Photo R. Middleton)

I am everything because I am simply the current of life, with its detail eliminated; I am immortal because in me every death meets, from that of the fish just now to that of Zeus, and conjoined in me they turn again into a life that is no longer individual and determined but Pan's and so free ... You are young and handsome; follow me now into the sea and you will avoid sorrow and old age; come to my dwelling beneath the high mountains of dark motionless waters where all is silence and quiet, so infused that who possesses it does not even notice it.... and remember that when you are tired, when you can drag on no longer, you have only to lean over the sea and call me; I will always be there because I am everywhere, and your thirst for sleep will be assuaged.... She told me about her existence beneath the sea, about bearded Tritons and translucent caverns, but she said that those too were unreal visions and that the truth lay much deeper, in the blind mute palace of formless waters, eternal, without a gleam, without a whisper.

Giuseppe Tomaso di Lampedusa, *The Professor and the Siren*, transl. by A. Colquhoun (in Colquhoun, Gilmour and Waldman 1995)

In the silence of the night of 23<sup>rd</sup> December 2004, Roger Moorey – ‘our’ professor – slipped away unobtrusively to be reunited with Lighea the mermaid, daughter of Calliope, the Muse of Poetry. He was particularly enchanted by the vibrantly lyrical prose, sometimes tinged with irony, beautiful, rich and bewitching of the author of *The Leopard*. To sustain my delight in Sicily which I ecstatically described to him in Oxford in October 1997, Roger had urged me to read *The Professor and the Siren*, which disclosed striking analogies in upbringing, academic career, international stature, distinctions, and serene solitude between himself and the bachelor professor of Lampedusa's short story.

Born on 30<sup>th</sup> May 1937 in Bush Hill Park, Middlesex, Peter Roger Stuart Moorey developed very early a singular maturity, fierce privacy and wistfulness: the death of his mother when he was but a small boy, followed by that of his father (who in the meantime had remarried), left him to play the role of both brother and father to his ‘real’ sister and his half-sister. National Service (1956–58) in the Intelligence Corps in Cyprus at the time of Suez and the Eoka troubles threw open for him the Eastern Mediterranean Pandora's box of archaeological riches and complex, troubled, contemporary historical events of which he had got his first glimpses from reading while a boarder at Mill Hill School. In December 1955, he had been interviewed at Corpus Christi College, Oxford, by Michael Brock (who was Isaiah Berlin's second-in-command in creating Wolfson College, Oxford), and in Oxford parlance secured an Exhibition to read Modern History. He was thus closely following in the footsteps of his father, a graduate of Selwyn College, Cambridge, who had taught History in Middlesex County Council schools. At Oxford from the autumn of 1958, he moved in a circle of aspiring archaeologists. In their postgraduate years, Roger, the prehistorian Denis Britton and the archaeo-scientist Michael Tite shared a house on the Cowley Road. Later, in Crick Road, they lived on the floor below that inhabited by the then University Registrar, an experience which first acquainted Moorey with the workings of the university. Tite's interest in the sciences played no mean a part in Moorey's lifelong belief in interdisciplinarity, and their enduring friendship in parallel careers at Oxford (after a spell at the British Museum, Tite returned to Oxford to head the Research Laboratory for Archaeology and the History of Art) provided Moorey with an incentive to keep abreast of new developments in the archaeological sciences, especially in the analyses of metal artefacts. The bronze-working centres of Western Asia, early metallurgy in Mesopotamia, and the history of related technologies, such as gold-figure decoration on Achaemenid silver vessels, occupied him to the end. His last study on ‘The Hammered Bronzework of Iron Age Luristan (Iran): Problems of Chronology and Iconography’, was his contribution to *The Iranian World: Essays on Iranian Art and Archaeology presented to Ezat O. Negahban* (Alizadeh 1999 [ed.]: 146–57), and at the time of his death, he was writing an introduction to a British Museum exhibition catalogue of Achaemenid jewellery.

In the summer of 1959, his first trip abroad, overland to Yugoslavia for six weeks with a gang of students which included Michael Tite and Fiona Gregg who was to marry the prehistorian Derek Roe, gave Roger his first insight into the mosaic of

peoples and creeds of the Balkans, with striking parallels with the Near East which would dominate his academic career. In an age where the choice of a candidate for an academic post was determined less by his/her specialization, than by the applicant's motivation, interest, common sense and commitment – all of which characterized Roger to an unusually high degree – Robert W. Hamilton, Keeper of Antiquities and Keeper of the Ashmolean Museum, gave Roger the chance of his life (as he gratefully always emphasized) immediately after he had gained a First Class BA degree in 1961, by appointing him Assistant Keeper of Antiquities. For the next four decades, the Ashmolean Museum would be his academic base, the geographical range of the collections for which he was responsible, expanding from the entire Near East to include Egypt, and from 1994, Cyprus, as he rose in 1973 to the Senior Assistant Keepership, and in 1983 to the Keepership which he held until he retired in June 2002. After Corpus Christi College, Wolfson became his academic 'home' upon his election to a Fellowship in 1976. He remained, however, faithful to Corpus, so much so that the Director and Staff of the Ashmolean Museum held Roger's retirement party on 21<sup>st</sup> June 2002 in its gardens. In September 2002, he took on the triple role of Emeritus Fellow, Senior Research Fellow and Vice-Gerent of Wolfson, the latter appointment coming to an end on 30<sup>th</sup> September 2004, the very day that Roger was hospitalized for the first time in his life.

A man of duty and a staunch believer in public service, he displayed amazing patience in fulfilling countless requests from scholars pertaining to the archaeological artefacts held by the Ashmolean Museum, in guiding them through the Museum's collections, arranging for them to study objects or tablets, and facilitating their work to the extent of obtaining for those with short study-periods special passes and even keys for evening work. To members of the general public and collectors who called upon him for the identification of an heirloom or acquisition, Roger invariably showed interest, and diplomacy if breaking disappointing news – as when he had to announce to a Swiss Ambassador to Iraq who had assembled a vast collection of antiquities in his Baghdad years (1956–59), that his 'Luristan bronzes' were merely 'cast-bronze cheap figurines sold on Indian markets to every gullible British clergyman's wife during the Raj'! He diligently published the bronzes, seals, glass, jewellery and terracottas in the Ashmolean and in private hands (from his 1971 *Catalogue of the Ancient Persian Bronzes in the Ashmolean Museum*, which had gained him in 1967 an Oxford DPhil, to his 2003 *Ancient Near Eastern Terracottas*). Concurrently, by penning Ashmolean Museum and British Museum booklets, he succeeded beyond all expectations in rendering attractive and intelligible to the lay reader and museum visitor the collections of Near Eastern and Egyptian antiquities (*Archaeology, Artefacts and the Bible*, 1969; *Ancient Bronzes from Luristan*, 1974) as well as their civilizations (*Ancient Egypt*, 1971, which went through four revised editions, 1983, 1988, 1999 and 2000; *Ancient Iran*, 1975; *Ancient Iraq*, 1976; and, *The Ancient Near East*, 1987, with a revised edition in 1994).

The beginning of Moorey's museological career coincided with the Persian Government's offer (which ultimately came to nothing) of the Directorship of the

Antiquities Service of Iran to R. W. Hamilton (1992: 203–7), who had been Director of the Palestine Department of Antiquities at the end of the British Mandate and subsequently Fellow and Secretary-Librarian of the British School of Archaeology in Iraq (1948–49), before taking up a Lectureship in Near Eastern Archaeology at the Oriental Institute, Oxford, and joining annually Max Mallowan's excavations at Nimrud in Iraqi Kurdistan. The three strands of Hamilton's legacy are traceable throughout Moorey's career. Elected Principal of St. Hugh's College in 1962, Kathleen Kenyon brought to Oxford her aura of 'excavator of Jericho' and formidable will. Moorey's recognition under her influence of the paramount 'biblical' dimension of Near Eastern archaeology led to his participation in Kenyon's Jerusalem excavations in 1963, and – more to his taste than 'dirt archaeology' – prolific writing, editing and revising on the biblical Levant. *The Making of the Past: Biblical Lands* (1975, new ed. 1991) was translated notably into Dutch, Spanish, Hungarian and Japanese; he edited with Peter Parr *Archaeology in the Levant: Essays for Kathleen Kenyon* (1978); published *Excavation in Palestine* in 1981, a revised edition of K.M. Kenyon, *The Bible and Recent Archaeology* in 1987, and in 1991 *A Century of Biblical Archaeology* which won the *Biblical Archaeology Review's* 'Best Popular Book' Annual Prize. Moreover, in 1968, he had been persuaded by Kathleen Kenyon to launch a new peer-reviewed journal, *Levant*, which he edited until 1986, setting very high standards of scholarship and writing whilst following a sound policy of non-intervention as regards content, and correction of grammar and spelling but not of style, provided the form passed muster.

In 1985, the Percia Schimmel Prize of the Israel Museum for Distinguished Contributions to Archaeology in Eretz Israel and the Lands of the Bible was awarded to him as much for the invaluable support and assistance which he extended to Israeli archaeologists on sabbatical leave in Oxford (to the extent that in the early 1980s, from the late Spring to the Autumn, Wolfson College almost turned into a kibbutz!), as for his 'biblical' output. The admiration he accorded female archaeological pioneers (Moorey 1992), first and foremost Gertrude Bell (1868–1926), founder of the National Museum of Iraq and Honorary Director of Antiquities for Iraq, and Kathleen Kenyon, extended to their Israeli 'daughters', Ruth Amiran and Trude Dothan. Pirhiya Beck with her modesty and unequalled knowledge of ceramics, and Roger Moorey with his diffident wisdom, understood each other well, and he genuinely grieved her sudden and premature death. In Aharon Kempinski he recognized early an uncommonly incisive intellectual power, and his death came as a shock poignantly expressed in a letter of sympathy to Aharon's mother. Roger Moorey's trips to Israel for conferences or lecturing were inevitably 'a mixture of hectic programming, near-misses and exhilaration', causing him to comment with his dry sense of humour: 'I do more in a week there than in a month here [Oxford]'. The presentation of the Schimmel Prize had been shown on Israeli television, and, upon visiting the British School of Archaeology in Jerusalem's excavations at Belmont Castle, Moorey was delighted to be recognized in Suba, the local kibbutz, as 'the man from Oxford'.

His real inclinations, 'everything from Ur to Samarra', however, were not stifled. The publication in 1978 of the finds from the *Kish Excavations 1923–1933* which he had examined in Baghdad Museum in 1969 in the course of a six-month sabbatical leave in Iraq and Iran, was followed by *The Origins of Civilization: The Wolfson College Lectures 1978* (1979) which he edited; the revised edition of C. L. Woolley, *Ur of the Chaldees* (1982) with translations into Spanish and Japanese; *Materials and Manufacture in Ancient Mesopotamia: the evidence of Art and Archaeology: metals and metalwork; glazed materials and glass* (1985); and *Ancient Mesopotamian Material Industries* (1994), winner of the J. R. Wiseman Award of the Archaeological Institute of America of which Moorey became an Honorary Member in 2000. It was also in Iraq that, after a twelve-year gap, he returned to fieldwork, joining the British Archaeological Expedition to Iraq excavations at Abu Salabikh in 1975, 1977 and 1981. Its Director, Nicholas Postgate, gratefully admits that in the first season he 'could not have coped without his [Roger's] knowledge of Mesopotamian artefacts in general and, because of his work on the Kish material, of Late Early Dynastic in particular'. Besides cataloguing the finds (which 'entailed sitting out in the sun under his straw hat and sorting buckets and buckets of dark grey clay lumps for the occasional one with an excellent seal impression'), Roger helped with 'the best will in the world' to move the photographic bipod from square to square across the surface of the West Mound, so that vertical shots could be taken from about 10 m up. This probably not being 'his idea of a good way to spend an afternoon', Roger commented wittily once that now he knew what it was like being the 'backlegs of a pantomime horse' (N. Postgate, personal letter 23<sup>rd</sup> February 2005). For all their gruelling aspects, field trips to Iraq held some rewards. On a visit by the Abu Salabikh team to Adab, an Early Dynastic site of the first rank deep in the desert, with ED III pots and hearths lying *in situ* on the surface, unable to resist the temptation to clear the sand away from some of them and falling on his knees, Roger Moorey brushed away at the surface without a permit – an amusing snapshot which reveals 'Roger the man' driven by the excitement of archaeological discovery behind 'Moorey the Assistant Keeper of Antiquities of the Ashmolean Museum'.

Rarely at rest, Moorey's brilliant and inquisitive mind was stimulated by archaeological puzzles to be solved. An American Assyriologist, Norman Yoffee, who, during his first sabbatical at Oxford (1979–80), confused the non-initiates (to Roger's great amusement) by claiming that he was writing an anthropological study of cricket. Yoffee describes Roger Moorey 'in best British style' as seeing 'academic work as a game', and academic exchange as supplying each other clues for one's enquiries, adding, 'of course, he was our best puzzle-solver' (personal letter 28<sup>th</sup> February 2005). Yet, none of Roger's work was ephemeral. Dan Potts whom Roger considered a kindred spirit and whose breadth of learning he respected, points out that although 'aware of intellectual trends', Roger was 'not caught up in them', but integrated the best in all archaeological and historical approaches in order to pursue scholarship of exemplary rigour and the highest quality, his published production besides books and booklets numbering eighty-five contributions to

journals, *Festschriften* and conference *Actae* and over seventy reviews. His 'contributions are truly building blocks on which everything Near Eastern scholars do in a remarkable number of sub-fields will continue to rest for years and years to come' (personal letter 7<sup>th</sup> March 2005). Despite a predilection for neat, longhand draughts which were subsequently typed by a succession of devoted secretaries, and a distaste for the evanescence of telephone conversations and the unspecified geographical provenance of electronic mail, Moorey was 'truly a millennium man' (Kingsley 2004). The microfiche appended to his 1978 publication of the Kish excavations, his insatiable curiosity for recent developments in information technology and his prescience in pushing for the creation in 1994 of the first IT post at the Ashmolean, as well as the online publishing of *Ancient Near Eastern Terracottas* (2003), all reflect his modernity.

Tutor in Old Testament Archaeology for the Faculty of Theology and supervisor of graduate students in Near Eastern Archaeology for the Faculty of Oriental Studies (1972–95), Roger Moorey chaired Oxford University's Committee for Archaeology in 1988–90, becoming its Vice-Chairman in 1993 when a new degree in Archaeology and Anthropology was introduced, for which he lectured and tutored in Near Eastern Archaeology. Putting into practice Albert Einstein's recommendation that 'the essential role of the teacher is to arouse the joy of working and of knowing', Moorey was an exceptional teacher, always available, generous with his time, encouraging, and willing to share his extensive knowledge. A good listener who was genuinely interested in discussing his students' findings, he never imposed his views, and gently manoeuvred his charges away from pitfalls. Constantly on the lookout for references that might relate to his graduate students' topics of research, following the sale of Max Mallowan's library and in the absence abroad of one of his postgraduates, Diana Stein, he went so far as to purchase the reports of the Nuzi excavations in Northern Iraq (key volumes which he knew she was seeking) from an antiquities dealer on her behalf. Ezra Marcus describes a typical DPhil tutorial session with Moorey as 'usually a half-an-hour in his office followed by an hour at a local pub for lunch, the Eagle and Child with its memories of Tolkien, or the Lamb and Flag, across from each other in St. Giles a stone's throw from the Ashmolean. At core a true Englishman, he invariably ordered 'a Ploughman's lunch' for himself, and insisted on paying, arguing that the 'princely' sum which he was paid per term by the university to supervise Ezra was best used in this way (personal letter 24 January 2005). The respect and admiration which Moorey engendered in virtually everyone who came into contact with him, especially his students, in response to his very rare personal concern to invest tremendous energy in bringing out the best in them, are embodied in *Culture Through Objects. Ancient Near Eastern Studies in Honour of P.R.S. Moorey* (Potts, Roaf and Stein 2003 eds.), the unauthorized, clandestine *Festschrift* undertaken without the knowledge of the recipient who had dissuaded his Ashmolean colleagues from organizing a similar homage. When the news was broken to him not long before the book appeared in print, he gracefully succumbed to the inevitable. Furthermore, upon being presented with the volume at the Rencontre



Sean Kingsley and his two mentors, Claudine Dauphin and Roger Moorey, at his DPhil Graduation Lunch, Oxford, 7<sup>th</sup> May 2000 (Photo Madeleine Kingsley)

Assyriologique Internationale in London on 7<sup>th</sup> July 2003, with characteristic modesty he downplayed his role as mentor and emphasized all that he had learnt from his colleagues and research students.

A leading light in his undergraduate days of the Oxford University Archaeological Society, he was its Senior Treasurer (1971–77) and subsequently Vice-President from 1980, after being Treasurer of the Oxford Architectural and Historical Society (1964–67). Elected a Fellow of the Society of Antiquaries (London) in 1967, he was increasingly in demand as a committee member from the early 1970s. His unruffled wisdom and calm consensual approach were greatly respected by his peers of the British School of Archaeology in Iraq (1970–96) and of the British Institute of Persian Studies (1972–94). In 1977, at the young age of forty, he was elected a Fellow of the British Academy. From 1987, he chaired the Publications Committee of the British School of Archaeology in Jerusalem, and was its President from 1991 until its merger into the Council for British Research in the Levant in 1999, having vice-chaired the Council of the British Institute for the Archaeology and History of Jordan in Amman (1988–92) and concurrently being a Member of Council (1992–96). As Member of the Standing Committee on Overseas Schools and Institutes of the British Academy (1987–96) and Chairman of the Academy's Section 10 (Archaeology), he had the unenviable task of handling adroitly the 'tempers and tensions' of committee members in assessing the problems of the British Schools of Archaeology in the Near East and discussing their future.

*Ex officio* Secretary of the University of Oxford's Griffith Institute for Egyptology and the Ancient Near East since 1982, and a Trustee of the Oriental Museum of the University of Durham (1989–92), he was a founding member of the Council of the British Association for Near Eastern Archaeology (BANE) from 1982 and organized its annual conference in Oxford in 1988 and 1996.

Besides teaching and actively participating in innumerable committees, Moorey not only curated the Ashmolean Near Eastern, Egyptian and Cypriot collections, but also selflessly devoted an inordinate amount of energy to administering the Museum. As Senior Keeper, he deputized for the Director during periods of absence, and was Acting Director in Trinity Terms 1990 and 1996 when the Director took sabbatical leave, from 1 October 1997 for a calendar year whilst the Museum awaited the appointment of a new Director, and for the final six months before retirement in order to cover the Director's sabbatical and extra leave. The early 1990s were stressful years for the Museum staff, with a major building programme stretching 'as far as the eye can see' and producing internal chaos as well as, within the University, conflicts surrounding the absorption by the new Sackler Library of the previously independent Griffith Institute Library which had been housed in the Ashmolean Library. Moorey claimed to have worked out a kind of *modus vivendi* combining his various 'posts' in the Museum, University and College, but for all that he excelled in fund raising and entertaining donors, the intense pressure which he sustained uncomplainingly took its toll on his health and usually positive outlook on life. During his August and September 1996 sabbatical leave, he delivered at the University of Sydney a captivating and witty lecture ('From Backpacker to Camel: The Prehistory of Caravan Trade in the Near East') in memory of Tony McNicoll, who like him had been a 'Kenyon pup', but his 'exploration' of Australia, which thrilled him, allowed him to take stock and breathe.

Moorey rarely ever completely relaxed. In April 1993, for instance, he took two weeks' leave to write the lectures which he delivered at Ben-Gurion University of the Negev in May, and Christmas vacation 1993–94 was spent briefly entertaining his two sisters and their families at his home before being 'buried' for ten days in the proofs of *Ancient Mesopotamian Materials and Industries*, hating every minute of indexing 360 pages in double columns on dark December days which he compared to 'a 1000 lines of a schoolboy's imposition day after day'. Nicholas Postgate recalls Moorey telling him that he had written one of his general books in hotel rooms in the evenings while he was on a lecture tour in the United States of America (personal letter 23<sup>rd</sup> February 2005). Blessed with an extraordinary power of concentration and ability to compartmentalize his work, Moorey succeeded in maintaining a high rate of productivity at the cost of deliberately 'pulling up the drawbridge', isolating himself completely and not allowing his time to be gnawed at by trivia and uninvited inquisitiveness. Shutting out the wider world with its timetables, appointments and other demands, allowed his inner world to develop, nurtured by literature and history. A voracious reader with staggeringly wide interests, he discussed de Tocqueville (his knowledge of French was impeccable despite his claim that he could not easily converse in it, and his understanding of

the French mentality incisive), quoted from Lermontov and Dostoievsky (he had learnt Russian during his National Service in the Intelligence Corps), yearned to emulate in his own writing the stylistic achievements of Yeats and Henry James, praised John Huston's film of James Joyce's short story *The Dead* as 'the best cinema version of a literary masterpiece known to me' (adding, 'Don't miss it!'), relished Pablo Neruda's poetry, and welcomed every opportunity to make good any deficiencies in his knowledge of the modern history of Middle Eastern states. Fascinated by informative correspondence, such as Queen Victoria's epistolary exchanges with Disraeli, he crafted letters (from his office on Ashmolean-headed white sheets of paper, and from home on cream-coloured Basildon Bond small letter-pads with associated envelopes, the mark of a certain generational gentility) in the most beautiful, precise prose. At times of intense work pressure, he yearned for 'lowtide beach strolls' in an evocation of two exiles, Victor Hugo on Guernsey and Pablo Neruda on the Lipari island of Salina. Yet, Roger was no hermit. Endlessly curious about everything, he particularly enjoyed private parties where the 'real' world impinged on the academic. I cherish the memory of Roger at my birthday party in Oxford in February 1980, unexpectedly sitting cross-legged on the floor at the feet of a Ballet Rambert prima ballerina friend of mine, riveted by her narrative replete with the elations of Royal performances and tours abroad which compensated for the daily grind of practice and rehearsals – a world diametrically opposed to his.

Roger's liberality and generosity of spirit shone through his personal interests, his ever non-judgemental and kind approach to human exchange, and his solicitude for the well-being of others (his always judicious advice was sought after as much by Ashmolean Museum attendants as academicians). His prime interest in the people behind the pots was reflected in his topics of research and titles of lectures. 'What Do We Know About the People Buried in the Royal Cemetery at Ur?', and 'Idols of the People', the ancient Near Eastern clay figurines on which he had focused for his 2001 Schweich Lectures at the British Academy, published in 2003, are but two of many examples. Although he exercised diplomacy to the extent of frequently appearing infuriatingly non-committed politically, he shared with T. E. Lawrence, whose *Seven Pillars of Wisdom* he held in great admiration as a 'work of high literary aspiration which stands ... alongside the writings of Yeats, Eliot and Joyce' (Calder, 1997, xxv), besides a similar crossing from Modern History at Oxford into 'intelligence' and archaeology, a profound empathy with all the peoples of the Near East without any favouritism. The 1990–91 Gulf War distressed him deeply. But whilst Lawrence abhorred the casting of young English soldiers 'by thousands into the fire to the worst of deaths... that the corn and rice and oil of Mesopotamia might be ours' (Lawrence, 1935; 1997 ed., 7), Roger characteristically felt 'for all those ordinary Iraqis (and Kuwaitis) suffering for the whims and fancies of a tyrant so much the creation of those now trying to unseat him'. Roger's prescience in judging that the 'legacy of hate that will remain' would be 'disastrous for all aspects of British relations with all but a small minority of Arabs' (in the same way that in April 1994 he judged with great severity Western

policy towards the Muslims in Bosnia, understandably resulting in the targeting of Americans, British and French) stemmed from the channelled meditations of the remarkable analytical mind of a brilliant historian endowed with a rare humanity.

In recent years, Roger often evoked with me the passing of the world of our parents and of ours, and our anxiety (perfectly expressed by James Joyce in *The Dead*) that 'in our sceptical' and 'thought-tormented age', 'the new generation ... growing up in our midst, a generation actuated by new ideas and new principles ... educated or hypereducated as it is, will lack those qualities of humanity, of hospitality, of kindly humour which belonged to an older day'. This 'gentleman amongst scholars' (Kingsley 2004) had no reason to fear, for his formidable legacy is vibrant. A model to emulate, in his slightly withdrawn manner, he has instigated a powerful chain of scholarship and friendship which will endure and flourish – in his memory.

If the ship never returns to port with its fingers uncalloused,  
 If the watersong followed its course in the thundering sea,  
 ... here let us submit to the seas' return, our destiny.  
 ... night came down, galloping over the spread of the world.  
 ... night erases all trace of the sea, the ship heels, is at rest.  
 ... night lit up its starry institution.  
 It is time ... to ...

... go on in the dream, reaching the other shore of the sea which has no other shore.

Pablo Neruda, *La barcarola termina – The Watersong Ends* transl. by A. Reid (in Neruda 1975a: 234–7)

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*Claudine Dauphin*

## Grants Given by the Society

### JENNIFER A. THOMPSON

In May and June 2004 I undertook research in Jerusalem, pertaining to my PhD dissertation at Cardiff University, Wales. This research was in part funded by a travel grant from the Anglo-Israel Archaeological Society. My dissertation is being written on the Crusader cemetery at 'Atlit, Israel. The Crusader cemetery was originally excavated in 1934 by British archaeologist C. N. Johns. The work on the cemetery was part of the larger excavation of the Crusader castle and fortified town at 'Atlit. The excavation of the castle and fortified town was published by Johns in the 1940s but the Crusader cemetery excavation has not yet been published.

The Crusader cemetery is located outside the walls of the fortified Crusader town, which is currently part of the IDF base at 'Atlit. A modern-day road borders the cemetery on one side and a beach on the other. The public have free access to the cemetery and the beach is a popular area for people to swim, surf and fish.

The main purpose of my trip to Israel was to conduct a survey of the Crusader cemetery as part of my dissertation research. This survey will be combined with my publication of Johns' excavation of the Crusader cemetery to form the basis of my PhD dissertation. While in Israel I also spent time at the Rockefeller Museum in Jerusalem where I was able to work with the artefacts from the 1934 excavation of the cemetery.

For the survey of the Crusader cemetery, I chose four areas which were divided into 4m<sup>2</sup> areas. The tombs within these squares were drawn into a grid and numbered. Measurements of the tombs were taken and a list of characteristics was drawn up

for the tombs. A GPS survey was also conducted, resulting in a map of most of the tombs in the cemetery. A fully accurate mapping could not be completed as most of the eastern half of the cemetery is covered in vegetation, making it impossible to locate all of the tombs.

The survey of the Crusader cemetery showed that there were four tomb types, with a possible fifth type. A tomb typology is being drawn up based on the characteristics recorded during the survey. As well, the tombs seemed to have been grouped within the cemetery based on their type. This would seem to indicate that the tomb types represented distinctions in class or status. There were also a number of unmarked burials that were uncovered in the 1934 excavations, some of them multiple burials. These could not be studied in this survey.

The cemetery is in a bad state of repair. C. N. Johns rebuilt the cemetery walls in 1934 but these are now beginning to fall apart again. There are large gaps in the northern wall and the southwestern corner of the wall is missing. The southwestern corner is being eroded by the Mediterranean Sea, resulting in the exposure of a burial. The tombstones themselves show significant erosion since they were uncovered in 1934.

The results of the survey work at the Crusader cemetery at 'Atlit will be published in my PhD dissertation, which should be completed by November 2005. I wish to acknowledge the help and support of the Anglo-Israel Archaeological Society, the Israel Antiquities Authority, and the Council for British Research in the Levant during my time in Israel in May and June 2004.

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