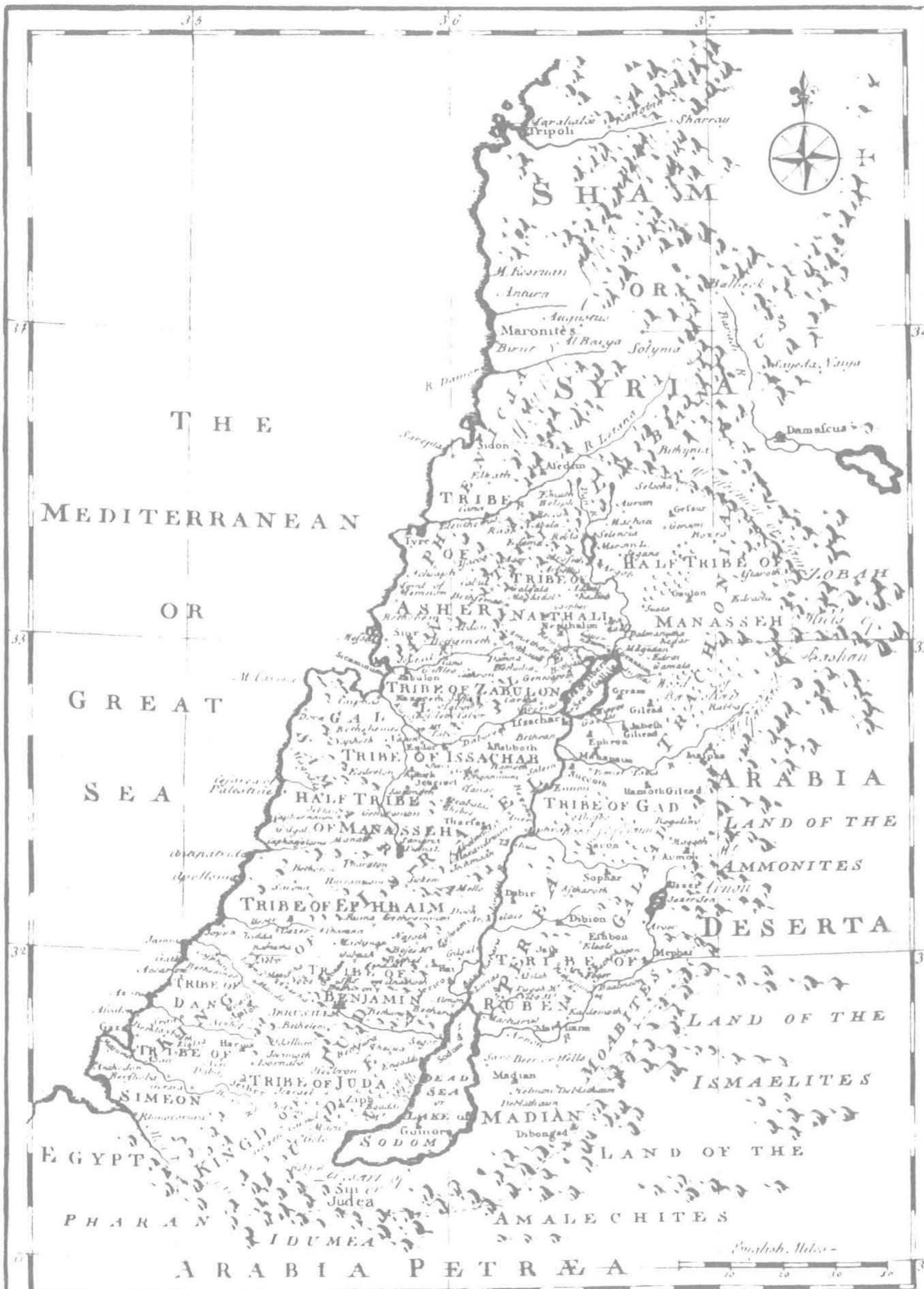
Bulletin of the Anglo-Israel Archaeological Society



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

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Editorial

This issue of the *Bulletin* opens with an article by Avi Sasson on a fascinating village of caves of the Ottoman period that he investigated at Maghar, situated in the southern coastal plain of Israel. Not enough research has been undertaken on cave dwellers in the southern Levant, even though the use of caves goes back to prehistoric times and troglodytes are occasionally mentioned in historical sources (notably the Horites). The use of caves as dwellings is a worldwide phenomenon; the subject was dealt with in David Kempe's excellent book entitled *Living Underground: A History of Cave and Cliff Dwelling* (1988; London). The first to deal with the subject in Palestine was Charles F. Tyrwhitt Drake in the early 1870s, but although he provided invaluable information based on his observations, he was also openly prejudiced against the incumbents of the caves: 'And yet the indolent, able-bodied rascals, dignified by the title of reasonable beings, who own this byre are too lazy to build themselves huts, but prefer using the caves bequeathed them by the Hebrews and heathen of old.' (pp. 35–36 in W. Besant, ed., 1877. *The Literary Remains of the Late Charles F. Tyrwhitt Drake*. London). Tyrwhitt Drake probably did not know that the main reason for living in caves in the nineteenth century instead of building houses, was that the Ottoman Turks who had control of the land did not tax those living in caves or flimsy structures. It had nothing whatsoever to do with people being indolent or lazy. Sasson is a lecturer in the Land of Israel Studies Department at Bar-Ilan University, as well as the co-ordinator of the study programme at the Ashkelon Regional Academic College.

The second article, by Anna de Vincenz, deals with an unusual lamp which was discovered in Hirschfeld's excavations at the Byzantine village of Ein Gedi, which was thought by many to be solely a Jewish place of abode during the fourth to sixth centuries, mainly because of the existence at the site of an impressive synagogue paved with beautiful mosaics. The discovery of a lamp with a representation of the bejeweled cross, probably symbolizing the replica cross on Golgotha (the place of the crucifixion), and of the relics of the True Cross (supposedly found by the Empress Helena in the early fourth century), must indicate that at least some of the inhabitants in the Ein Gedi village were Christian. De Vincenz is an expert on ceramics from the Byzantine, Early Islamic and Medieval periods, and is a Senior Fellow at the Albright Institute of Archaeological Research in Jerusalem.

It sounded like an amazing discovery. A stone box for bones (an ossuary) was found bearing an inscription in Aramaic script typical of the first century AD, reading: 'James son of Joseph the brother of Jesus'. Could this be the real thing? Was this the ossuary of the brother of Jesus of Nazareth? The whole world quickly became aware of the discovery and reports soon appeared on the front pages of

many leading newspapers. And then the tumult and excitement died down and word slowly crept out that the whole thing might be nothing but a deception. Scholars and scientists began looking at the artifact – the 'James Ossuary' - with critical eyes. The third article in this issue of the *Bulletin* is by Emile Puech, and deals with the inscription on the ossuary. Puech is Professor of Semitic epigraphy and Qumranology at the École Biblique et Archéologique Française de Jérusalem, and is Directeur de Recherche of the CNRS in Paris. He is also the Editor of the *Revue de Qumran*. Claudine Dauphin, who is a member of the Editorial Board of the *Bulletin*, kindly translated Puech's article from the French.

A report on the discovery of the ossuary first appeared as a 'world exclusive' in the November-December 2002 issue of *Biblical Archaeology Review*, written by the French epigraphist André Lemaire of the École Pratique des Hautes Etudes in Paris. In this article Lemaire claimed that the inscribed ossuary might represent the earliest evidence for Jesus found in Jerusalem: 'amazing as it may sound, a limestone bone box… has surfaced in Israel that may once have contained the bones of James, the brother of Jesus' (2002: 25). The Israel Antiquities Authority subsequently permitted it to be transported to Canada for exhibition purposes at the Royal Ontario Museum in Toronto. Unfortunately, *en route* from Tel Aviv to Toronto, a hairline fracture already visible in the original photographs, developed into a crack and on arrival it was seen that a fragment of the ossuary had broken off across the critical area of the inscription. In Toronto, following first aid conservation measures, various experts examined the item, some claiming that it was indeed a very important discovery, while others expressed their doubts about its authenticity and decided to bide their time until further information about its provenance became available. The doubts which were being expressed about this artifact were strengthened when an investigative reporter for the *Ha'aretz* newspaper in Israel, Sara Leibovitch-Dar, tracked down the owner of the ossuary, Oded Golan, at his home in Tel Aviv, and declared that the story about how he had acquired the ossuary in the first place sounded rather fishy to say the least (Leibovitch-Dar, S., 2002. A Story Full of Cracks. *Ha'aretz* Magazine [English edition], 8 November: 8–11). Although known as a collector of antiquities with a reputation for a sharp eye for a good bargain, Golan has said he had never realized the significance of the object and feigned ignorance regarding the meaning of the inscription, even though the ossuary had supposedly been in his possession over a period of 35 years. At the press conference organized by the Biblical Archaeology Society in Washington in November 2002, it was claimed that the ossuary originally came from a location south of the Mount of Olives in Jerusalem. To add to the confusion regarding the matter of provenance and when it was found, Golan at a meeting in December 2002 with two archaeology students from the Hebrew University, Orit Peleg and Rafi Lewis, made a surprising statement indicating that the James ossuary had come from a tomb close to the junction of the Kidron and Hinnom Valleys in Jerusalem (Akeldama), suggesting therefore that it might actually have come from a tomb I excavated with Boaz Zissu and James Tabor which had been badly looted by antiquities robbers in 1998 before we even got to the site. The tomb robbers

apparently had a very rich haul of complete ossuaries from our cave; those they did not take away with them they simply smashed up using pickaxes. Apparently a few of the ossuaries taken away had inscriptions, judging by the stories circulating in 2000 amongst the antiquities dealers in Jerusalem. Why would Golan have wanted to volunteer this information? We shall probably never know for certain, but perhaps this was all a slip of the tongue, or a bit of bravado while showing off his collection to the students. I heard another story from Gil Chaya, who sells antiquities from his establishment 'Biblical Antiquities' in Jerusalem and is a nephew of the London-based collector of antiquities Shlomo Moussaieff. Chaya claims that he was aware of the ossuary long before it became the subject of international news and that word was out even then that the inscription on the ossuary had been 'made' in a special workshop that specialized in producing choice fakes made of stone, inscribed items, as well as large numbers of engraved seals. Indeed, Chaya claimed that Moussaieff himself had also been offered the ossuary for sale, but wisely turned it down suspecting that the object might be a forgery.

Back in September 2002, Lemaire and the Biblical Archaeology Society, decided to have the ossuary checked by two scientists from the Geological Survey of Israel in Jerusalem, Amnon Rosenfeld and Shimon Ilani. They examined the ossuary and declared it authentic: '… the patina does not contain modern elements (such as modern pigments) and it adheres firmly to the stone. No signs of the use of a modern tool or instrument was (sic) found. No evidence that might detract from the authenticity of the patina and the inscription was found.' (Lemaire 2002: 29). The patina was composed of calcite (calcium carbonate) that is the product of a natural process in environments such as the hills of Jerusalem. This scientific determination was very encouraging but not conclusive; after all the patina on the stone box might be authentic but the inscription itself could still be a forgery. This was the question I asked myself when first hearing about the ossuary: was the patina in the grooves of the inscription the same as the patina on the walls of the ossuary? The geological report was unclear about this. Because of the doubts expressed regarding the authenticity of the ossuary inscription, the Director of the Israel Antiquities Authority, Shuka Dorfman, appointed two committees to examine the subject: the 'Writing and Content Committee' which was to look at epigraphical / paleographical matters, and the 'Materials and Patina Committee' which was to do a scientific examination of the patina and other aspects under laboratory conditions (Dahari, U., et al., *Final Report of the Examining Committees for the Yehoash Inscription and James Ossuary*, published on the internet at the following site: www.bibleinterp.com/articles/final_reports). Owing to the original determination of authenticity provided by the geologists from the Geological Institute in Jerusalem, its Director, Amos Bein, decided that full scientific assistance was to be given to the new investigation and he appointed Avner Ayalon, a senior research scientist at their Institute, to look specifically into the matter of the patinas.

On 15 June 2003, the two committees met and reached a collective conclusion that while the ossuary itself is clearly authentic, the inscription itself is most likely to be a forgery. Based on his experience in geochemical applications and the

identification of materials through the study of isotopes in rocks, the geologist Ayalon determined that the patina on the body of the ossuary and the patina in the grooves of the inscription differed substantially one from the other. The patina from the letters of the inscription, he claimed, 'could not have [been] formed within the Jerusalem area's climatic conditions.' Yuval Goren, an expert on petrography at Tel Aviv University, also made a careful examination using a stereoscopic microscope and high-resolution photography, and he too observed that the two patinas on the ossuary differed, and that the grooves of the inscription had cut through the original patina ('varnish' as he calls it) evident on the body of the ossuary. Goren concluded that the patina substance in the grooves of the inscription could not be natural and was presumably made 'by grinding and dissolving chalk in hot water (possibly the powder resulting from the newly-carved inscription), and spilling the paste onto the inscription and surrounding area, in order to blur the freshly engraved signs.' Indeed, another Committee member, Jacques Neguer, pointed out that the scratches and incisions seen on the surface of the ossuary, however slight, had a very definite patina, whereas the grooves of the inscription did not. Hershel Shanks, the Editor of *BAR*, claims that this 'may in fact have had something to do with the cleaning of the inscription; it had been partially scrubbed (apparently by the owner's mother)' (Shanks, H., 2003. 'Why I am Not Yet Convinced That the "Brother of Jesus" Inscription is a Forgery'. Document circulated by e-mail). Examination of the inscription suggests that the 'James son of Joseph' part of the inscription does differ in style and execution from the second 'the brother of Jesus' part. Indeed, another Committee member, Tal Ilan, an expert on Hebrew and Aramaic names in the Early Roman period, pointed out that she too had noticed this difference, adding that 'two handwritings do not necessarily prove a forgery, and perhaps point to the opposite.' Similarly, Orna Cohen, a conservator of antiquities, noticed during a microscopic examination of the surface inside the letters of 'the brother of Jesus' part of the inscription, 'the same yellowish patina as on the ossuary surface'. It is interesting to note that during Ayalon's examination of the patina scraped from the last letter of the name Jesus, it yielded an oxygen isotope value similar to the values that were obtained for the patina of the ossuary surface itself. Hence, while the inscription in its entirety might be a fake, the second half of it may originally have been authentic before it was tampered with. It is possible, therefore, that 'the brother of Jesus' part of the inscription was incised as a convenience by a family member of the dead person, perhaps as a means of distinguishing this ossuary from that of another with the same name in the family who was *not* a brother of Jesus. While one might cautiously accept that part of the inscription is authentic, the fact remains that the ossuary itself lacks clear provenance and was at some stage tampered with, whether intentionally with a view to creating a fake or accidentally (as Shanks has suggested) because of the cleaning habits of Oded Golan's mother. Whatever the truth, the bottom line is that we must regard the inscription with a good deal of suspicion.

The fourth research article by Mark Merrony, a committee member of the Society, is based on the analysis of a corpus of mosaic pavements in Late Roman Phoenicia

and northern Palestine for his D.Phil. degree. This issue of the *Bulletin* ends with a review article by Claudine Dauphin on a recently published book dealing with rural landscapes, settlement archaeology and political ideology. A review of Netzer's recent excavation report on his Jericho excavations, and ten lecture summaries are also provided. The Editors and Committee gratefully acknowledge the very kind donations made by the Sydney and Elizabeth Corob Charitable Trust and we much appreciate Joe Dwek's contribution in helping with the Manchester lecture series. Our thanks also go to Dr Marcus for his donation to the Society. My thanks to Ashley Jones, Publishing Editor, and to Diana Davis, Executive Secretary of the Society, for help in producing this issue.

Shimon Gibson

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Maghar: A Village of Caves from the Ottoman Period in the coastal plain

AVI SASSON

> Whatever will be, this is what it is today. A leveled, emptied out hillock, without a sign of anything, neither a house, nor the stones of a house, only small carobs planted in a furrow. (Smilansky 1964: 148)

In geographical-historical research, the regional aspect constitutes a basis for the study of the individual village and the way in which it was incorporated into a given area. This article focuses on one such village in the coastal plain – the village of Maghar – which has a number of unique geographical characteristics that are instructive for research into other similar villages in the coastal plain all of which have hitherto been insufficiently studied. The present research also hopes to be instructive in regard to the overall regional aspects of indigenous Arab rural settlement in the coastal plain. Anthropological research conducted in different parts of the world has concentrated on the many different aspects of the subject of cave dwellings: geographical, historical, archaeological, and ritualistic (Kempe 1988). The geographical-historical study of cave dwellers in Israel has concentrated mainly on the archaeological and anthropological perspectives, within a limited number of regions, and without any apparent comprehensive outlook (Havakook 1985; Hirschfeld 1995). This study will focus on a single settlement, but, at the same time, it will also point out the necessity for research on cave dwellings in the coastal plain of Israel on a much broader level.

The central matter to be addressed in this article is the character of the village of Maghar – the 'cave village' – since passers-by approaching the foot of the village would not nowadays be able to observe any apparent structural features at this location (Fig. 1). This village is unique in the large number of caves that were used as dwellings, a phenomenon as yet unknown at other coastal plain sites. Our research included the gathering together of historical sources, the surveying and documentation of central complexes and their characteristics, and it ended with an attempt to reconstruct the overall structure of the village.

History of the research

Very few historical sources refer to the village of Maghar. This was neither a multi-period archaeological *tell* with any relevant historical sources, nor was it situated

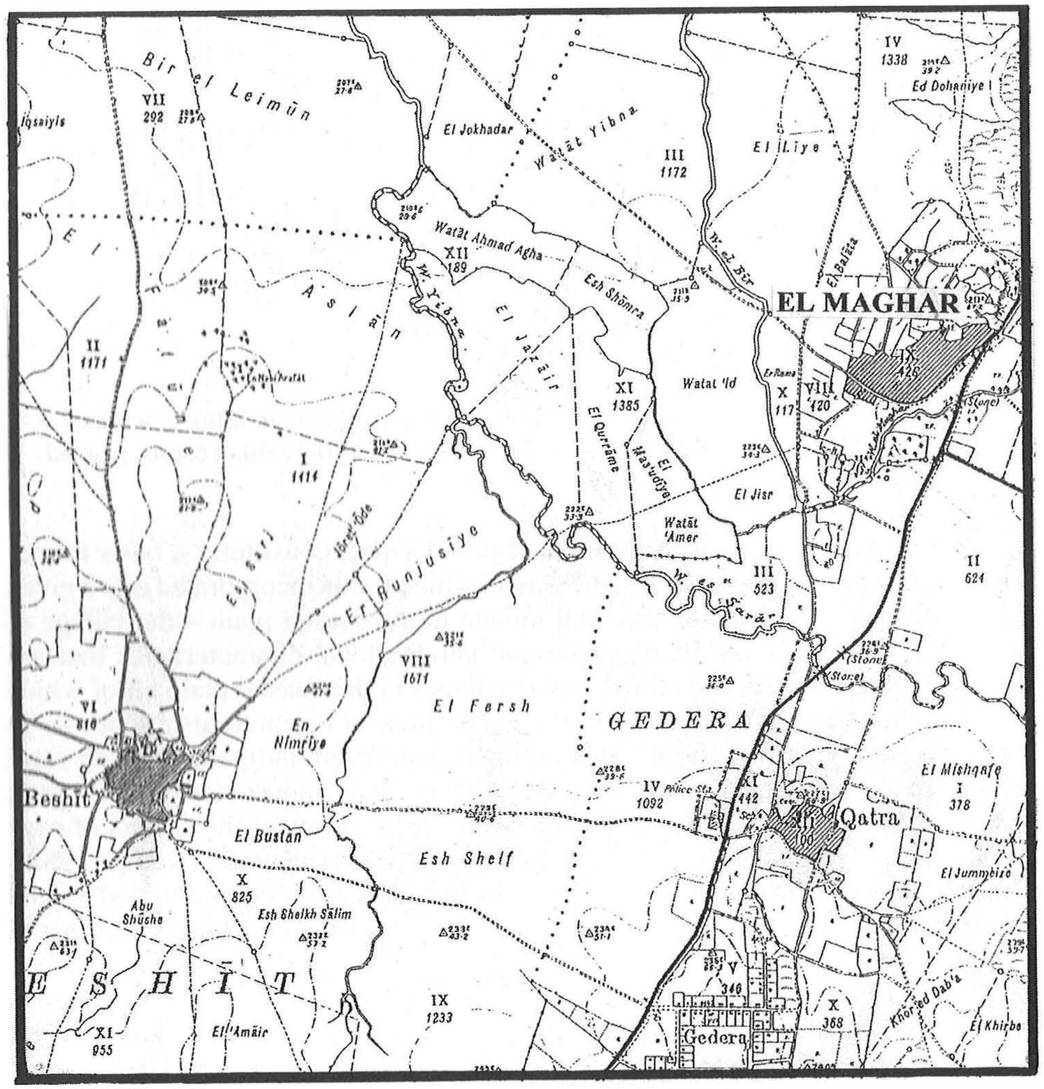

Fig. 1. Topographical map showing the location of the village of Maghar (1945).

at any in a significant location in the coastal plain. Furthermore, this site was not on the normal route traversed by scholars and travellers in Ottoman times. In the nineteenth century many explorers ignored the village and its surroundings, except for members of the British *Survey of Western Palestine* and Thomson, who also left a drawing of the village (Thomson 1881: 144; Conder and Kitchener 1882: 411–413) (Fig. 2). The description provided by Victor Guérin, who passed through the region and even mentioned the village, indicates that he did not actually enter the village, but only described it incidentally (Guérin 1869: 36). By contrast, other

Fig. 2. Engraving depicting the village of Maghar (after Thomson 1881: 144).

researchers, such as Witman, probably observed the village and its vicinity but did not describe it, and possibly did not even know its name (Witman 1803: 254). Beginning in the 1940s, Jacob Kaplan conducted an archaeological survey in the region and the results were published in 1953. Since then only random archaeological discoveries in the region have been published. In the absence of historical sources, we made use of topographical maps dating from different points in time for our research. We also conducted a field survey at the site, during the course of which we documented a number of structures within the village. The initial fieldwork focused primarily on the two upper terraces of the village and on the southern slopes.

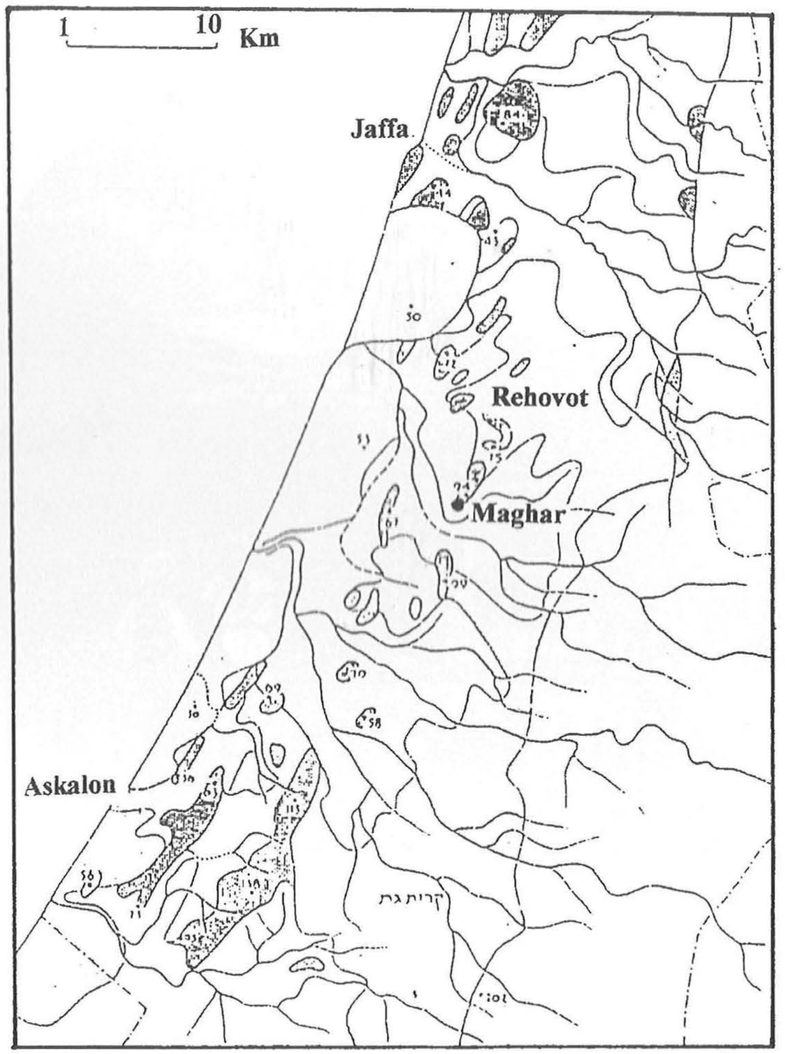

Fig. 3. Map showing the *Kurkar* ridges (dotted) along the southern coastal plain.

Geographical background of the Maghar region

The hills of Maghar are part of the eastern *kurkar* ridge of the coastal plain, a long ridge with a north-south orientation (Nir 1971: 85; Karmon 1983: 243–45) (Fig. 3). This ridge is cut by many streams descending from the foothills of the Shephelah; notably Nahal Sorek and its tributary Nahal Ekron, that flows to the north. The village of Maghar was situated on three hills (Figs. 4–5), the highest of which reaches an elevation of 85 metres above sea level. About 1.2 km to the northeast of the village (within a military zone) is the highest point of the Maghar hills and of the entire *kurkar* ridge: 94 metres above sea level (at map reference 1305–1398 on

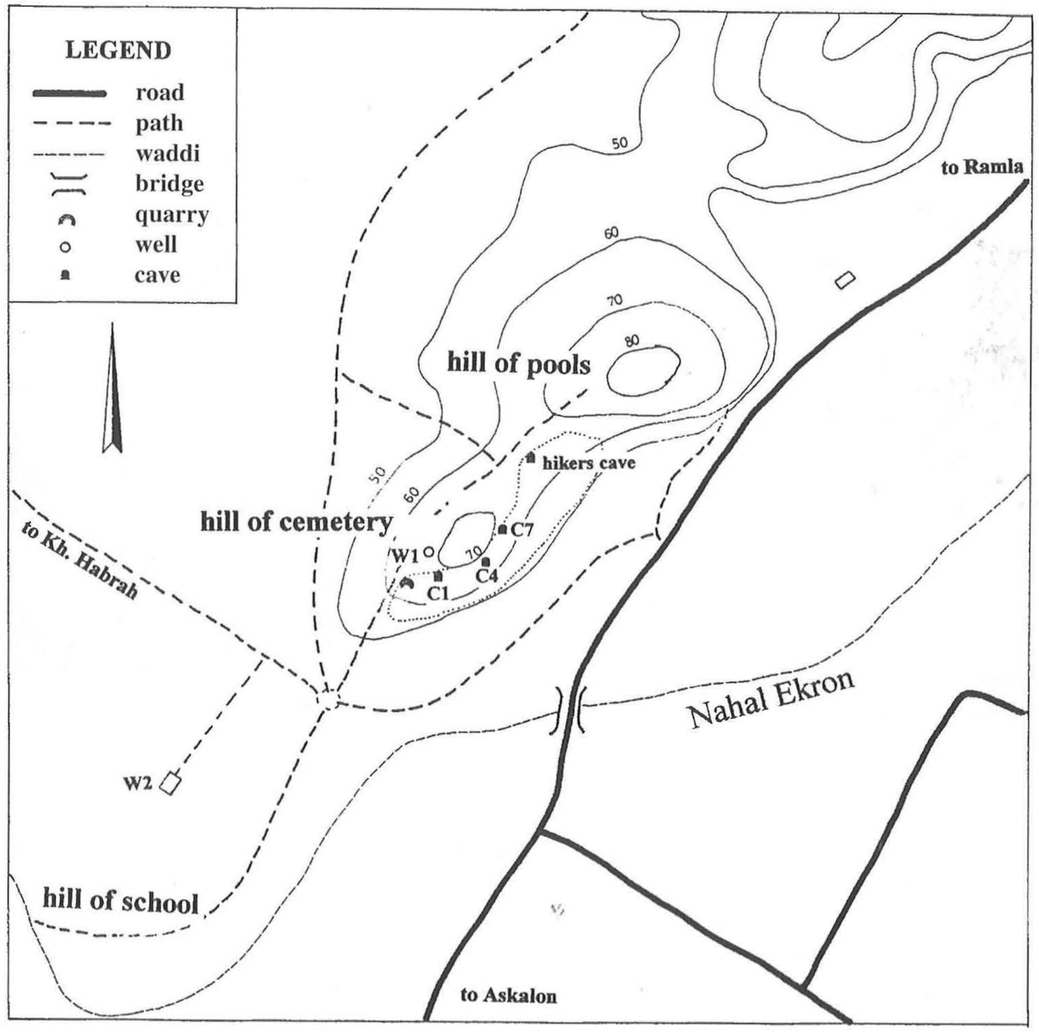

Fig. 4. Map of the area of Maghar and the various sites.

the Israel grid; 6697–5234 on the UTM grid). The rock comprising the ridge is *kurkar* (sandstone) which has a number of fossil *hamra* (red clay) layers, the erosion of which is responsible for the many cavities within the cliff, and even for its partial collapse, especially in the vicinity of Maghar. The inhabitants of the village may possibly have taken advantage of this phenomenon when constructing their homes (see below). The long Yavneh valley extends to the west of the Maghar hills. To the east of these hills lies the broad valley formed by the Nahal Sorek stream and its small tributaries.

The village territory of Maghar consists of three hills, the southernmost of which was not used for dwellings, but only for agriculture and with a school built at its centre ('hill of the school'). A citrus orchard is currently planted there (at map ref. 1293–1382 on the Israel grid; 6685–5238 on the UTM grid). The houses of the

Fig. 5a. Maghar seen from Tell Qatra.

village were spread out over the eastern slopes of the two northern hills. The village cemetery was located on the peak of the central hill ('hill of the cemetery'; at map ref. 1296–1386 on the Israel grid; 6688–5242 on the UTM grid). Houses were not built on the northern hill, and it was used for various purposes (see below). Water pools are currently located at the top of this hill ('hill of the pools' at map ref. 1293–1382 on the Israel grid; 6690–5234 on the UTM grid).

The Maghar hills are among the few places left in this region which still possesses the wild flora that is characteristic of the coastal plain and of the *kurkar* hills (Waisel 1984: 200–201; Waisel *et al.*, 1982: 158–61; Zohary 1980: 131). In 1950 pools for water were built on the peak of the highest of the hills of the village, as part of the overall water-supply system of Jerusalem. The water reaches the pools from drilled wells in the vicinity of Kibbutz Givat Brenner and the city of Yavneh, and is transferred from there to the Shahmeh and Huldah pumping stations. Currently these pools serve the settlements in the region. At the foot of the hill are seepage basins for run-off water in the winter (Ya'akobovich 1981: 45–46, 49, 121–22).

The historical background

The ancient history of the Maghar hills, has become clearer as a a result of the archaeological survey conducted there by Kaplan. Settlement already began there

Fig. 5b. Maghar seen from Tell Qatra.

in Chalcolithic times (fourth millennium BC). Since a Muslim cemetery is situated above the remains of the settlement from this period, its full extent has not yet been determined. It may possibly have been a seasonal encampment of nomads. Remains of a settlement from the Early Bronze Age I (3300–3000 BC) were also located to the south of this hill, on a mound alongside the route leading to the west (in the direction of the fields of Kibbutz Givat Brenner). On the summit peak of the ridge, Kaplan also found potsherds dating from the Middle Bronze II (2000–1500 BC) and Iron II (1000–700 BC) periods (Kaplan 1953: 141–42). The first to establish a settlement in this area was most probably the Canaanites, who dedicated their settlement to the god Baal. In the division of the Land of Israel into tribal holdings, the area was given to the tribe of Judah, as its northern boundary: 'The boundary then proceeded to the northern flank of Ekron; the boundary curved to Shikkeron, passed on to Mount Baalah, and proceeded to Jabneel; and the boundary ran on to the Sea' (Joshua 15:11). This verse apparently describes the area of Nahal Sorek. 'Ekron' is Tell Miqne (Khirbet el-Muqanna, presently located in the fields of Kibbutz Revadim), 'Jabneel' is Yavneh, and Shikkeron and Mount Baalah are to be sought between the two. Kaplan identified 'Mount Baalah' with the ridge of the Maghar hills, but, according to him the settlement of 'Baalath' is to be placed in the Ajalon Valley (Kaplan 1957: 206–7) Aharoni also accepted Kaplan's identification (Aharoni 1979: 256). Mazar and Kallai also concurred with the

identification of 'Mount Baalah' in the area of the Maghar ridge, albeit without indicating a precise location (Mazar 1975: 94–96; Kallai 1967: 105, 311). Belkind offers a different and interesting view. He identifies Maghar with the 'Mearah of the Sidonians' mentioned in connection with the boundary of 'the land that still remains' to be occupied by the Israelites (Joshua 13:1–4) (Belkind 1983: 158). The background for this identification is the description in the passage from Joshua of the boundaries of the land; 'Mearah of the Sidonians' is mentioned in proximity to the description of the districts of the Philistines. This identification has not been generally accepted, since it most likely refers to a site in the north of the country (Aharoni1979: 237; Broshi 1968).

Witman, during his exploration of the Yavneh area, identified in the region of the Maghar hills the 'rock of Etam,' in the context of the narrative of Samson and the Philistines (Judges 15). It should be noted, however, that Witman did not visit Maghar, nor did he make any explicit mention of the village, but merely described the area in general fashion (Witman 1803: 254). Warren, and later Conder and Kitchener, regarded the site, with its many caves, as a suitable location for Makkedah, which is mentioned in the narrative of the war of Joshua against the Five Kings (Conder and Kitchener 1882: 411–413). This view, however has not been supported by Israeli scholars, who maintain that this war must have occurred further to the east (Aharoni 1974: 44; Eph'al 1977: 84; Broshi 1968). Later on, following the settlement of the tribe of Dan in the coastal area of Philistia, the settlement of 'Baalath' is mentioned in this territory: 'Eltekeh, Gibbethon, Baalath' (Joshua 19:44) (Aharoni 1974: 69).

Pottery from the Hellenistic period was also discovered by Kaplan at the top of the ridge on which the village is situated. The overall character and extent of the village in this period has not yet been fully determined. In the Roman period the hill was abandoned and the settlement shifted to Kh. Habrah, a small hill located 800 metres to the west. Although pottery from the Roman and Byzantine periods has been identified, there are no remains of buildings on the Maghar hill, nor on the ridge to the north of the cemetery (Kaplan 1953: 141). The settlement at Kh. Habrah flourished in the Roman and Byzantine periods, apparently at the expense of the settlement at Maghar, with Jewish habitations clustering around a synagogue (Ilan 1991: 267–68). The hill of Maghar was used as a cemetery for the inhabitants at Habra, and possibly also as a stone quarry. On the northeast slope of the 'hill of the pools', Kaplan discovered burial caves with *loculi* and *arcosolia* characteristic of the Early Roman period, along with fragments of soft limestone ossuaries (Kaplan 1953: 141; *ibid.* 1957). The Byzantine period constituted the high point of settlement at Kh. Habrah, as has been indicated by the different archaeological surveys conducted there. Kaplan also identified remains from the Early Umayyad period on the peak of the Maghar hill, at the same place where remains from earlier periods had been uncovered (Kaplan 1953: 141). The history of the site in Medieval times has not been determined due to the absence of archaeological finds dating from this time. Yaqut (early thirteenth century) defined Maghar as 'a village in the district of Palestine' (Le Strange 1890: 498). We may asume that the settlement from this

period was extremely small. In the Ottoman period this village resembled many other villages in the coastal plain in terms of the character of its population and the livelihoods of its inhabitants. This village, however, was unique in at least one respect, namely, the nature of its dwellings.

In the First World War a bloody battle was waged between the Turkish forces and the British army that was advancing from the south on its way to the conquest of Ramle. The Turkish defensive line was based mainly upon the hills of Maghar and extended to the northwest, towards the hills on which were situated the villages of Zarnuka (currently the Kiryat Moshe quarter of Rehovot) and Kubbeba (the present-day Kefar Gevirol). As in other places, the Turks utilized the cactus hedges which encompassed the villages as part of their defenses. The British encamped near Nahal Sorek, which is called Wadi Jammus in this area. The British cavalry, supported by artillery fire, charged straight towards the Turkish rifle barrels. At a certain phase of the battle, they were forced to dismount and charge on foot. Although the British army suffered heavy losses, thousands of Turkish soldiers were actually taken prisoner. In the Second World War, a last-ditch plan was prepared, in case the German army which intended to conquer the region from Egypt, was not repulsed. This plan included the Maghar hills as a key point for the harassment and surveillance of the German forces. Fortunately, the British did not need to implement this plan.

In the 1920s, one inhabitant of the village, 'the bandit Muhammad Abu Ghanem,' came to be renowned as a merciless highway robber. He began his career in the Gederah area, and subsquently came to dominate the entire Jaffa-Jerusalem region. Interestingly, he was apprehended in Transjordan, but succeeded in escaping and was later recaptured in the vicinity of Ramallah (Tidhar 1924: 96–99).

In the 1948 conflict, the village of Maghar controlled the main road, and harassed Jewish convoys and individual travellers. Many Jewish inhabitants of the region relate how they had to duck down to protect themselves, while travelling by car from Gederah to Rehovot, in order to avoid being shot at by snipers from the village (Hayyot 1995; Yair 1994). The village was conquered as part of the overall 'Barak Operation', and the actual mission for the conquest of the village was called the 'Dror [Freedom] Operation'. This action was carried out by Battalion 53 and the inhabitants of the village subsequently fled (except for a group of Syrian defenders). After the war, the village was systematically razed, and not a single house was left standing.

Smilansky described the atmosphere of those times:

> "This hill, a pair of falcons circling around. Thorns and recently-planted dwarf carob trees. Can you think what distinguishes this hill from all others? This one, a bit bent, a bit bare among all the fields, the plantations, and the furrows, and, at first glance, it contains no sign of any riddle. In truth, it is not a riddle, but a habitat of man that is desolate; a village was here. A settled site, perhaps from the days of the Patriarchs, that has experienced many changes, glory and ebb, and it appears that the cup of

> wrath has passed over it many times, with its destruction and re-establishment. Its place on the hills, always to be read, hints from anew of shelter and protection. Until now. And behold, this is all that it is today. [. . .] It is unbelievable to what extent all that has disappeared from here and is no more imposes fear on all. But only a few years ago, with such care people plucked up their courage to pass by here, when it still stood. They called this the 'hornets' nest,' the 'ravens' nest,' or even 'the vultures' den,' whatever one could imagine. It was marked on the maps as a trouble spot. Around and around they circumvented it during the time of the [Arab] 'disturbances,' and when were there none such [disturbances]. All manner of strategems were conceived by the cunning to pass by it without being harmed. And someone always paid for it with his life, one foul day. 'The village of murderers,' they called it then in the newspapers, without troubling themselves greatly to learn what was its appearance and where exactly it was. But it is always good for people to have a place that is portrayed as a sort of den of thieves and a vipers' nest. It is hard to live in a world that has no places like this. [. . .] Whatever will be, this is what it is today. A leveled, emptied out hillock, without a sign of anything, neither a house, nor the stones of a house, only small carobs planted in a furrow'. (Smilansky 1964: 145–48)

The location of the village, dominating a number of important routes, was one of the major factors in its development. The eastern branch of the ancient *Via Maris*, that came from Egypt in the south and continued through the 'long valley' of the coastal plain northwards, apparently passed the southern foot of these hills. The actual line of the main road was more to the west, through the city of Yavneh, but those dwelling in the hills of Maghar were closely connected to this route. In certain periods the transverse road extending from the coastal plain in the Yavneh region, passed to the south of the village towards the Judean Shephelah and the Jerusalem hill country. A section of a road from the Roman period with a milestone was discovered in the fields of the agricultural settlement of Benei Darom (Rubinstein 1994). This section of road was most likely part of the ancient route that extended from Ascalon in the direction of Gezer (Avi-Yonah 1977: 187).

The village of Maghar

On historical and archaeological grounds it would appear that Maghar was inhabited from prehistoric times and through to the Early Umayyad period (seventh to eighth centuries AD). Following this there is a gap in our knowledge of the site until the sixteenth century. One of the difficulties facing us is how to determine the exact date of the establishment of the settlement in the Ottoman period. The relevant testimonies appear to indicate that the settlement was already in existence by the beginning of the sixteenth century. According to data supplied by Hütteroth and Abdulfattah, in that century there were 31 extended families at Maghar, comprising about 150 souls, 22 of which were liable to taxation (Hütteroth and Abdulfattah 1977: 146). Grossman maintains that the village was settled on a permanent basis

throughout the Ottoman period (Grossman 1986: 380; *ibid.* 1994: 157, map 11). Ben-Arieh has raised the possibility that this village, like those of Zarnuka and Kubbeba, situated on the same ridge to the north of Maghar, were settled by immigrant Egyptians in the mid-nineteenth century (Ben-Arieh 1987). It is unclear, however, whether this means that the village was actually founded by Egyptians, or that they accommodated themselves within an already existing settlement. This hypothesis could explain the growth of the village and the doubling of its population in the nineteenth century.

The lack of historical sources and testimonies by travellers or explorers visiting the village, raises difficulties in estimating the number of its population. In the mid-nineteenth century, Victor Guérin noted that the village had two hundred inhabitants (Guérin 1869: 36). Despite Guérin's fundamental and accurate work, it would seem that he was not precise in this instance. Although Guérin passed close to the foot of the village, neither did he enter it nor did he meet with its inhabitants as was his usual practice. Consequently, in our opinion, his description is inaccurate and based on a demographic estimate which cannot be accepted. Hartmann counted 56 houses in the village during a survey he conducted there in 1871 (Hartmann 1883: 133).

Based on our own examination conducted in the village, it may be estimated that there are between 60 and 80 units of quarried structures from the Ottoman period. If each unit served as the dwelling for a single family, numbering on average between five and six individuals, then the village would have had a population of at least 350. Obviously, further research is necessary to confirm this estimate. In any case, it would appear that Maghar was much larger than its neighbouring villages.

The village houses and the agricultural areas

The geographical boundaries of the village relied upon natural landscape features. To the south and west, the area was delimited by Nahal Ekron and Nahal Sorek (Fig. 4). A confirmation of this is perhaps reflected in the development of the agricultural settlement in the early twentieth century, with residents from Gederah '[...] who uprooted some of their vineyards, and with the money they received they purchased a sizable plot of land to the north of Nahal Sorek, close to the village of Mearah. This area comprised about one thousand and six hundred dunams [1 dunam = 0. 247 acres, i. e., approximately 395 acres]' (Belkind 1983: 144). This may possibly be the area marked on the 1941 topographical map of the region as 'el-Currame' (the vineyards – *ha-keramim*) (Sheets 12–13). It is more difficult, at the present time, to clearly define the northern and eastern boundaries of the village.

Based on the finds from the caves, the descriptions by travellers, and on the evidence of the different maps of the region, it would appear that the village mainly extended over the eastern slope, and partly over the western slope, with a total area covering about ten dunams. The southern hill of the village, descending toward Nahal Ekron (Wadi Maghar), was not used for dwellings. Instead, a school was built at its centre and trees were grown along its terraced slopes. The plantations of the village extended along the northern and southern slopes of the hill. An orchard

with fig trees apparently existed on the northern slope. This orchard was unique in the coastal plain that it came to be specifically noted in the description of agriculture in the memoirs of the survey of the Palestine Exploration Fund (*SWP*) (Conder and Kitchener 1882: 418). An ancient olive orchard also grew on the southern slope, of which a few trees still remain. The fertile valleys to the east and to the west, in which the agricultural settlements are at present located, were originally used by the inhabitants for growing non-irrigated grains namely wheat, barley, and maize. Crops of this sort were typical of the coastal plain in the nineteenth century (Robinson 1856: 258; Witman 1803: 149; Volney 1787). Generally speaking, the village of Maghar, like other villages in the coastal plain, relied upon the potential of the very rich lands in its vicinity, and not only on the alluvial soil near the streams (Grossman 1994: 156). Based on the number of threshing floors in the village (two or three according to the different testimonies), it may be assumed that the harvest was large, probably exceeding local needs. An allusion to the nature of the crops grown in the region may possibly be assumed from the name of the neighbouring village to the west, Kh. Habrah, which is about 800 metres away from Maghar. Mazar had previously noted the connection between this name with 'Hebron' and with that of threshing floors in ancient periods (Mazar 1975: 53). Klein already noted that in Second Temple times the city of Yavneh was a commercial centre for the marketing of grain (Klein 1939: 153–54). Although the present study deals specifically with the Ottoman period, these ancient testimonies do cast light on the general geographical-economic character of the region, and this is probably relevant for the later periods as well.

There is no available data regarding irrigated agriculture at Maghar. Similarly there is no evidence on the subject for the southern coastal plain as a whole. Around the southern well, we examined the remains of an irrigation system comprising pipes and ditches for water. The character of these finds attest to their dating from the time of the British Mandate, when orchards were being planted in the region.

The dwelling caves

Based on our field survey we may offer a reconstruction of the typical appearance of a dwelling cave at Maghar. It should be noted that these caves were not of uniform size, as was already noted by members of the *SWP*. Nonetheless, there are many common features, regardless of the size of the cave. Those who built the houses and carved out the caves at the site, utilized the topography of the landscape and the peculiarities of its geology. The majority of the houses have at least two rock-cut walls, one of which was usually the western wall, which was on the side of the slope of the hill. The builders possibly made use of the underlying strata of fossil red loam, which had been washed away or artificially removed, in order to hew out the chambers. The residents appear to have sought to quarry the caves on a horizontal level, so that the northern rock-cut wall of one house would subsequently constitute the southern wall of the following unit and so forth. Rock-cut walls on three sides were found in a number of houses. The height and width of the rock-cut terrace did

Fig. 6. Inner wall of c4 (Avi Sasson)

not in any way attest to the actual height of the house. On the contrary, we discovered rock terraces extending to a height of eight to ten metres, whereas the small square alcoves for the roof beams reached only to a height of three to four metres (Fig. 6).

In some instances, the cave façade was rock-cut, including the entrance way, whereas in other instances it would be of mud–brick construction. The primary construction material at Maghar and elsewhere for houses consisted of bricks made of mud and clay, as was the case in some of the other regions of the country (Volney 1787; Conder and Kitchener 1882: 411; Guérin 1869: 36). This explains, *inter alia*, the lack of building stone within the village. This is reminiscent of the prayer made by the High Priest in Second Temple times, in which he said that the people of Sharon 'should not make their houses their graves' (PT *Yoma* 2:2). (The 'Sharon' was not used in the limited way as Rosenson [1989] would define it).

The façade of the completed house that eventually concealed a cave behind it, is reminiscent to a certain degree of the dwellings within burial caves at the village of Silwan. In the Ottoman period, the Silwan villagers made use of the cave *loculi* as dwellings and for everyday use, by closing off the entrance façades either with bricks or with wooden panels (Ussishkin 1986: 13–15, and Ills. 168, 169). The residents of Maghar utilized various archaeological remains found in the vicinity, such as those taken from the ancient cemetery on the northeast slope of Maghar, or

Fig. 7. Inner hall and niche in the 'hikers' cave' (Avi Sasson)

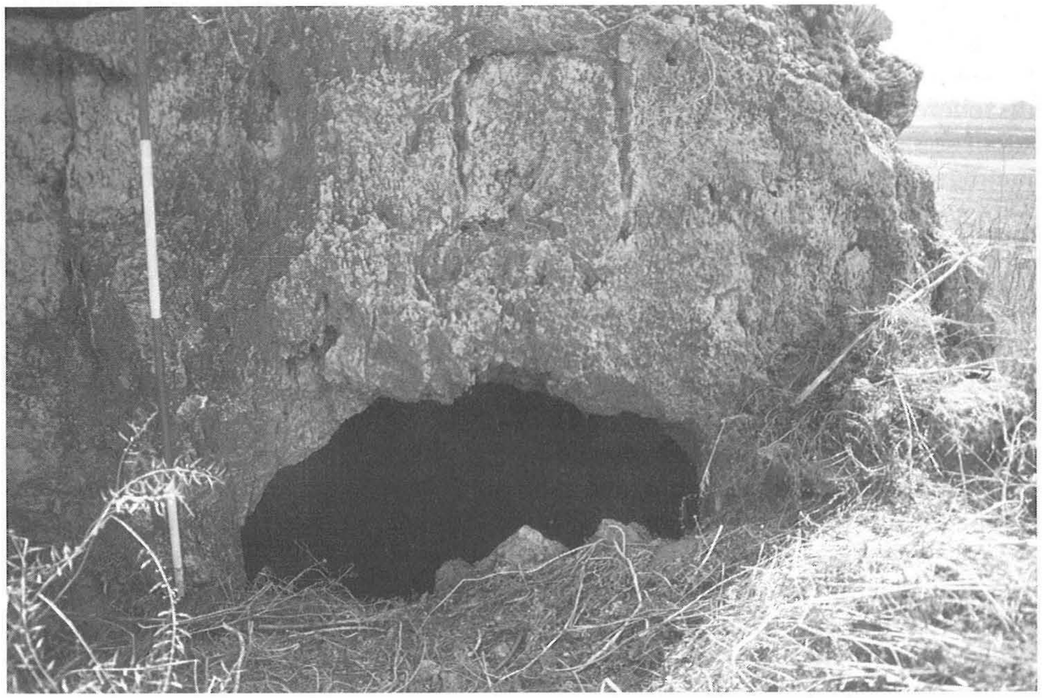

Fig. 8. the niche, rock-cut chamber, in c5 from south (Avi Sasson)

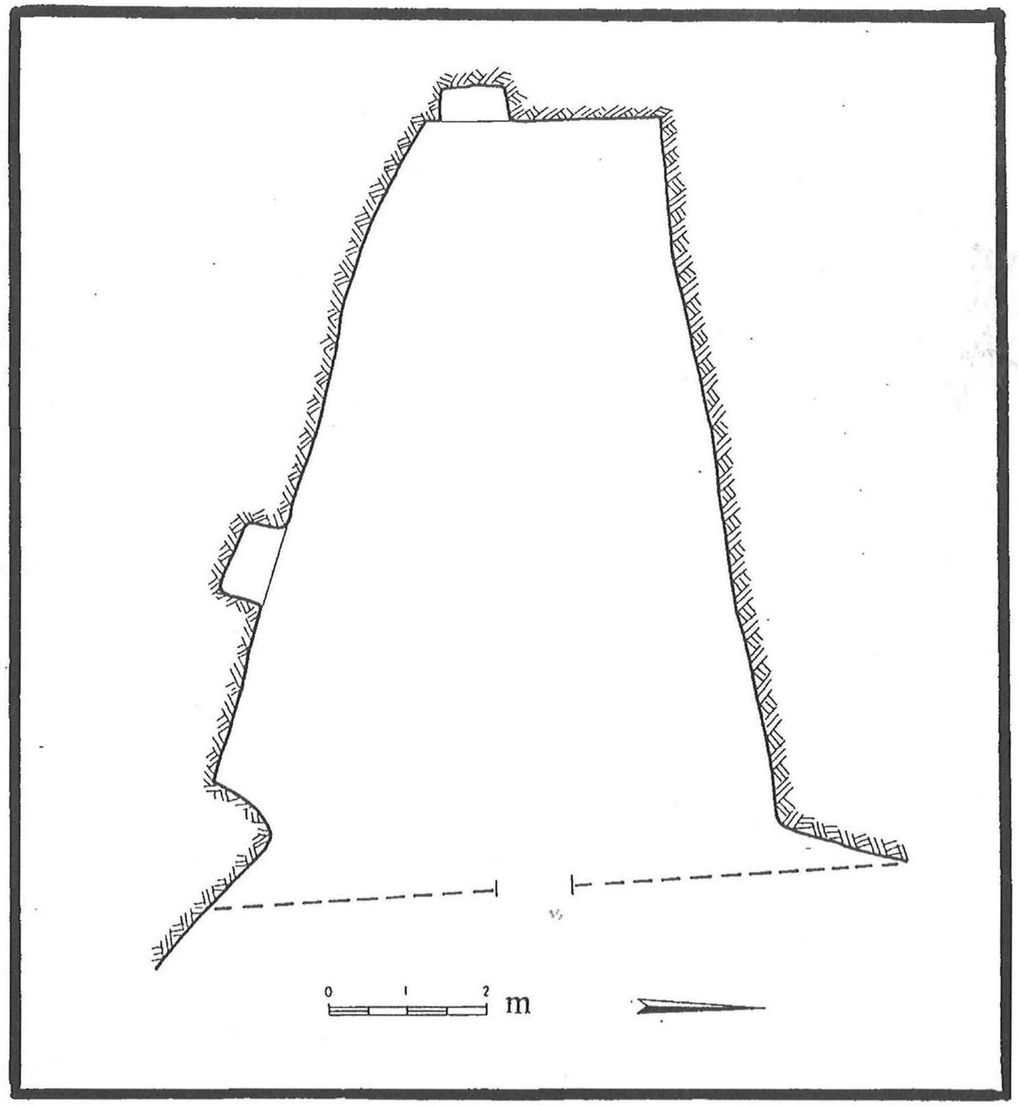

Fig. 9. Plan of the 'hiker's cave'.

from nearby Kh. Habrah, for secondary construction in the cave dwellings. Good quality elements made of marble or limestone were used to reinforce the weaker parts of the houses, such as entrance thresholds, window frames, and so forth. Indeed, one stone with an ancient inscription was discovered by Kaplan incorporated into one of the houses (Kaplan 1945; *ibid.* 1947). We also found a slab of limestone incorporated into the window of a house in the area of orchards on the hill to the south of Nahal Sorek.

Most of the houses comprised only one large room or two smaller ones. This is

also confirmed by the testimonies of soldiers who entered the village immediately following the 1948 war, and this is also the picture emerging from the remains evident at the site (Kehati 1995). In a number of caves, there were niches of various sizes in the walls (Fig. 7). These niches apparently functioned as cupboards, and the larger ones as storerooms. No more than one rock-cut chamber of this type existed within each dwelling unit. It was usually only a small space but in some places people could stand upright, with a small entrance opening (Figs. 8–9). These storerooms were cut in the bedrock close to the façade wall, usually on the eastern side of the structure (Fig. 10).

Eliyahu Ze'ev ha-Levi Lewin-Epstein, one of the founders of the *moshava* (agricultural settlement) of Rehovot, describes in his memoirs the way of life of the peasants in the vicinity of Rehovot. Regarding the house of the *fellah* he wrote:

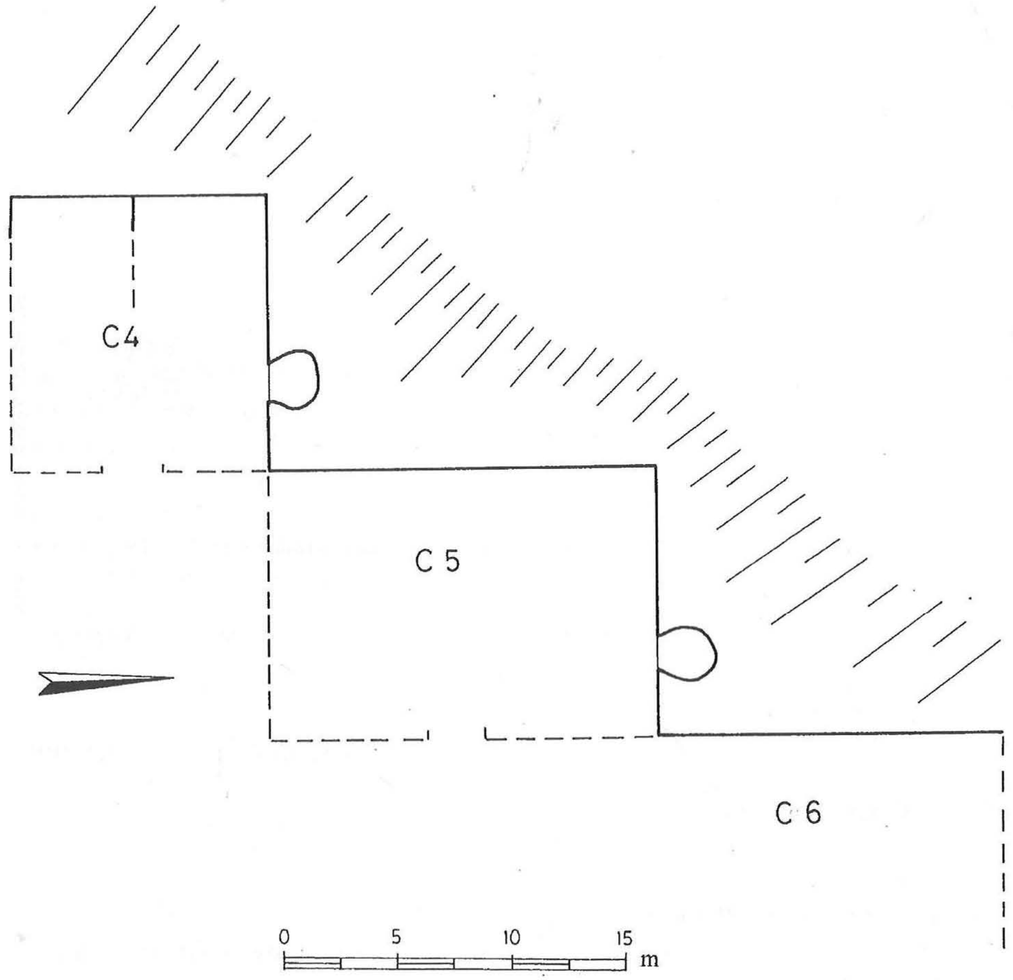

Fig. 10. Plan of Caves Nos. 4–6.

> 'The *fellahin* would live in caves made of clay, with neither windows nor entrance way. A large hole in the middle of the wall served as an entrance for those entering and leaving. The entire cave was no more than a single room – if such a chamber could be called a 'room' – and lacked a floor. It contained no chairs, no table, no bowls, and no forks' (Lewin-Epstein 1932: 239).

It may be assumed that Lewin-Epstein was describing the appearance of the house of a *fellah* in one of the villages close to where he lived. However, his description corresponds remarkably well to the dwelling house which was so characteristic of the cave-village under discussion.

Cave dwelling in the Land of Israel is known mainly in the hill country (Havakook 1985; Hirschfeld 1995: 109–211; Seger 1988). In the coastal plain there were only a limited number of sites with dwelling caves and these were mainly used by fishermen and their families during the fishing seasons (Avitsur 1976: 36). The inhabitants of the cave village at Maghar could not have had any connection to the nomadic bedouin settlement, since cave dwellings are not at all characteristic of the bedouin population in the Palestine region (Havakook 1986: 234–43).

Water sources of the village

A number of water sources were available to the village of Maghar during the course of the year. The most important of these is the perennial Nahal Sorek, that flows to the south of the village. It may be assumed that its water was used primarily for agricultural purposes, and less as a source of drinking water, although such a possibility cannot be discounted. The *SWP* expedition also noted 'two wells: one north, one west' (Conder and Kitchener 1882: 411). In the field survey we located two sources of water: a cistern and a well (for their descriptions, see below). The 'western' cistern is situated on the southern slope of the cemetery hill, and collected the run-off water of the quarries in this area (Figs. 11–12). The cistern may possibly also have been fed by the water of the southern well. In any event, this cistern was most probably used by the villagers for drinking water and for everyday use, and not for their agricultural needs. The additional source of water is the southern well, located to the west of the spur between the cemetery hill and the school hill. This is a relatively large structure that has survived almost in its entirety (Figs. 13–14). Its location attests to the fact that this was the main water source for the agricultural area to the west of the village.

A legend about the subterranean caverns at Maghar was once related by a peasant who was resident in the village:

> It so happened that the Prophet cast a sealed copper vessel into the good waters of the Muristan in Ramle, and eventually the same vessel was discovered in one of the wells of the village' (Braslevsky 1954: 370).

This legend may possibly attest to the good quality of the water.

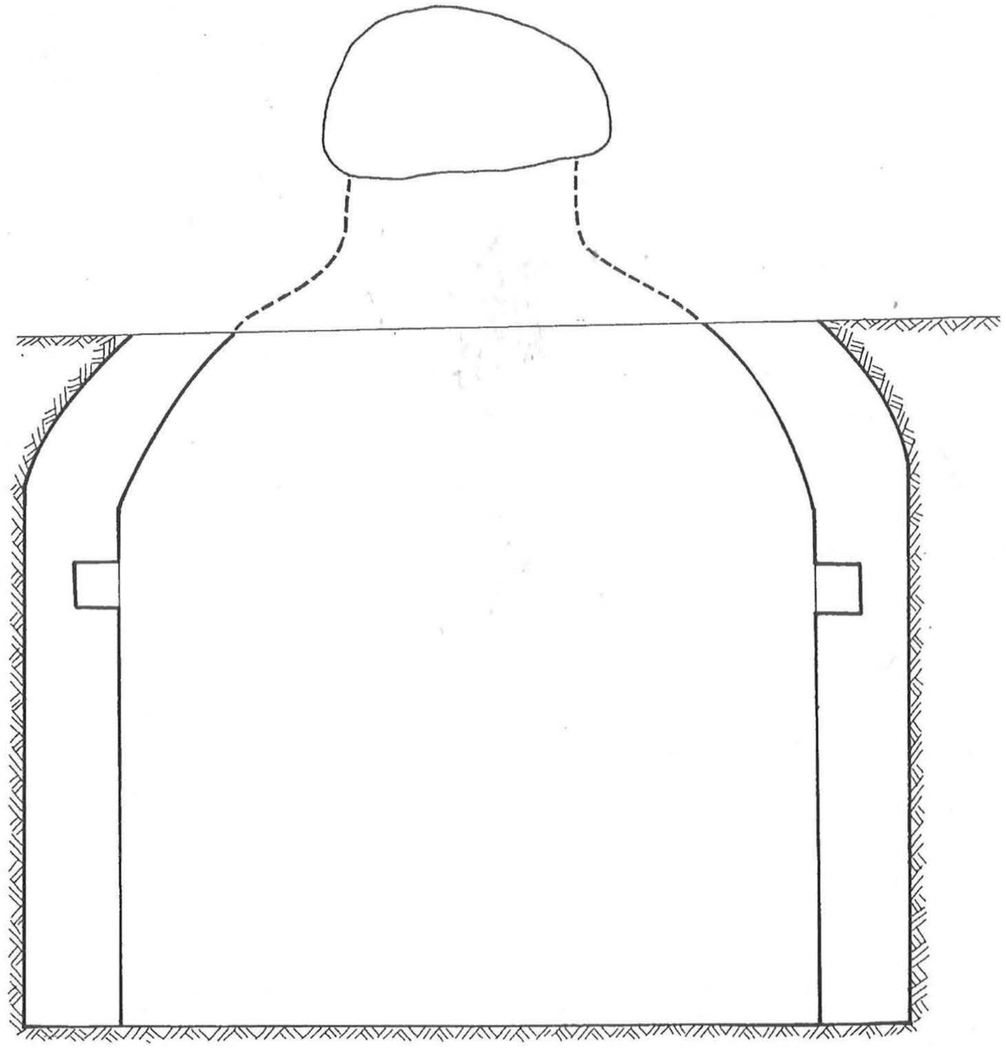

Fig. 11. Cross-section of cistern W1.

The cemetery of the village

This cemetery is located at the peak of the central hill of the village. At present about ten tombs have survived, most of which are within very small caves cut into the *kurkar* rock. Most of the tombs were opened, desecrated, and robbed. In the centre of the cemetery stands a tombstone which is larger and more prominent than the others. This is most likely the tombstone of Nebi Abu-Taka, marked on top of this hill on various topographical maps (Fig. 15). We do not know of traditions or special qualities that were attributed to this holy man. The area of the cemetery is clearly visible at present, not only by the few remaining tombs, but also by the

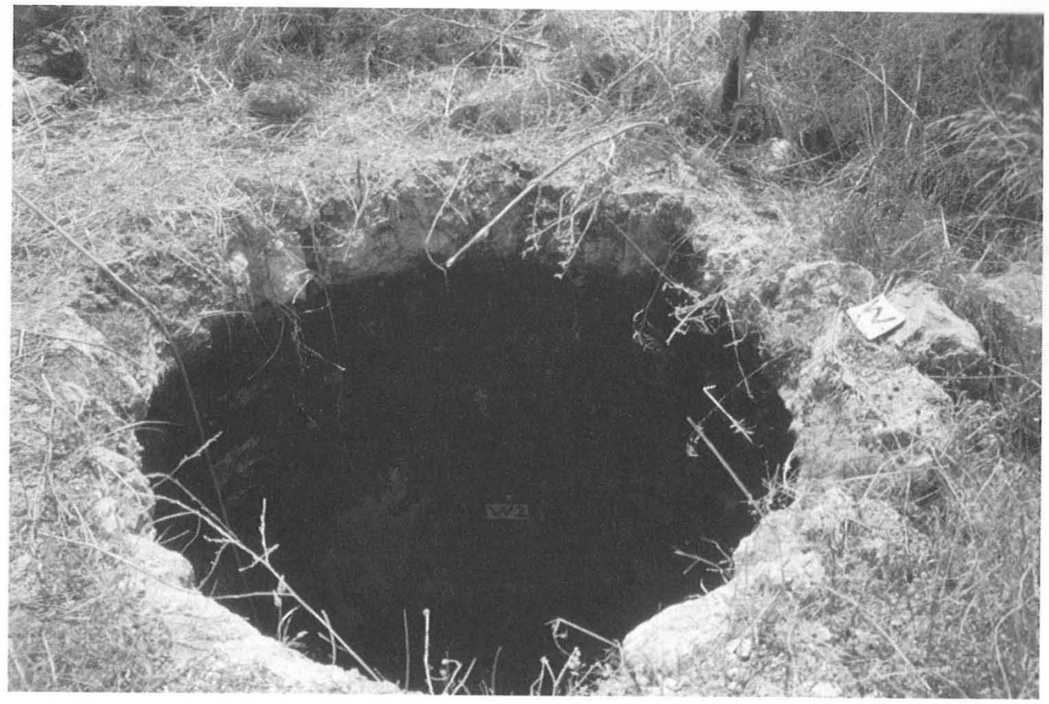

Fig. 12. Cistern (W1) (Avi Sasson)

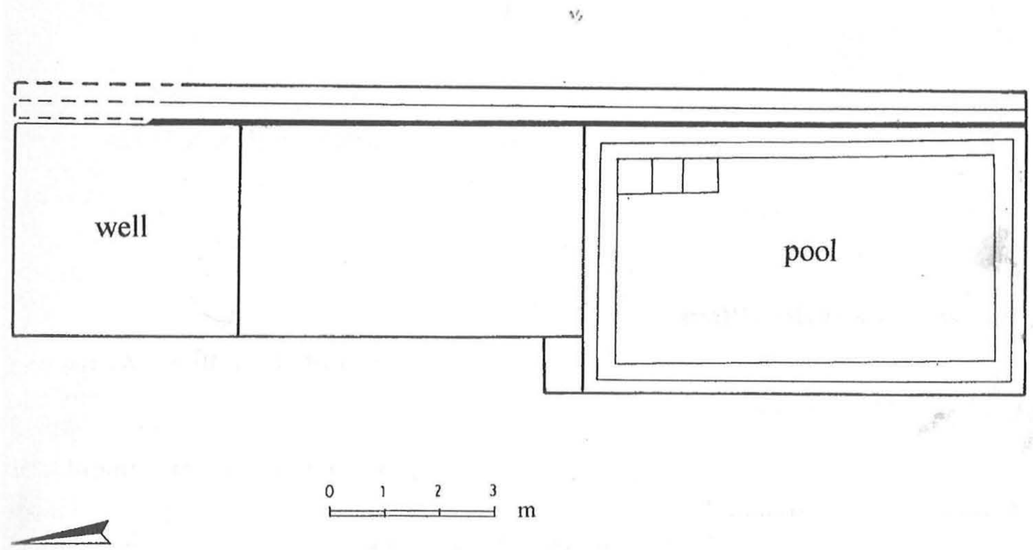

Fig. 13. Plan of the southern well W2.

Fig. 14. The southern well from east (W2) (Avi Sasson)

Fig. 15. Tomb of Nebi Abu-Taka (Avi Sasson)

absence of structural remains in its proximity. The closest houses are situated on the terrace under the cemetery and to its south.

The location of the cemetery on the central hill of the settlement and at such a high place, would at first seem to defy logic. However, various archaeological finds and an analysis of the landscape does shed light on the reason for the position of the cemetery. The inhabitants of the village apparently buried their dead in the area of the much earlier Chalcolithic cemetery, within caves, some of which were still open, while others had to be rock-cut. The houses of the village were probably built on a lower level so that they should not be disturbed by the location of the cemetery. Nor was the agricultural potential of the area harmed, because the top of the ridge is characterized by exposed rock, with hardly any soil suitable for farming.

Roads and paths

As was noted previously, the village is situated in close proximity to a number of regional highways. In addition to this, sources indicate another road passing along the spur between 'the hill of the school' and the 'hill of the cemetery', in the direction of Kh. Habrah and the city of Yavneh. Preserved within the village itself is the central path that passed along the edge of the ridge, as well as a path descending between the 'hill of the cemetery' and the 'hill of the pool'. This path was also used to gain access from one terrace to another. The passage between houses along the same terrace was facilitated on the eastern sides of each one of the terraces, when it was wide enough.

The Threshing Floors

According to the different available testimonies, the village of Maghar possessed a number of threshing floors. One such installation stood on the southern side of the cemetery (Kaplan 1953: 141). This side of the slope has exposed rock formed, *inter alia*, as the result of the quarrying of the *kurkar* rock for construction materials. This slope was also the centre of the village, was easily accessible, and was sufficiently close to houses and storerooms, which was necessary for threshing floors. An additional threshing floor was situated at the foot of the village to the east (where the road turning to Tel Nof is presently located, next to the tamarisk trees). Additional threshing floors may possibly have stood on the 'hill of the school' and on the 'hill of the cemetery'.

Conclusion

The village of Maghar is set apart from other villages by its rock-cut caves, since these were used as dwellings in a fashion that was not wholly characteristic of the region. On the other hand, a study of the available written sources reveals that apart from this phenomenan, the village was not unusual from the others, neither in terms of construction materials or building styles, nor the general way of life, or

agricultural pursuits. The main significance of the village lies in the antiquity of the place, and in the uniqueness of its caves.

The uniform style and similar size of the caves strongly attests to an excellent community-based organization in the village. This uniformity would appear to have been intentional, otherwise the village would have had dense and spontaneous construction, such as is known from other Near Eastern villages, including those in the coastal plain. This social organization must have influenced other spheres of village life, especially in regard to the division of agricultural plots and water rights.

Our research indicates that this is the only planned cave settlement in the coastal plain of Israel. An analysis of the structure and character of this cave-village may facilitate the investigation of other sites with cave dwellings. Different testimonies exist regarding settlement in caves at sites in the southern coastal plain. About one kilometre to the north of Maghar, is a small village named Summeil Maghar or Maghar Summeil (Grossman 1986: 380). On the Palmahim shore, in the southern bank of Nahal Sorek, there is a group of caves known as el-Dakakin ('the shops' in Arabic). We also have knowledge of another cave-village on the *kurkar* ridge to the south of Ashkelon that was inhabited in the Ottoman period (Grossman 1994, 158). In the *kurkar* ridge southeast of Ashkelon (the 'Geveram ridge') we were able to locate a cave akin to those in Maghar; two other caves used as dwellings were recently discovered in the vicinity of the village of Julis.

Settlement in caves appears therefore to have been the reality of the coastal plain in the Ottoman period, on the eve of the renewal of Jewish settlement in this region in the late nineteenth century. A similar phenomenon existed among the early Jewish settlers, such as the first residents of Gederah, who settled in caves, as a continuation of earlier practices. The culture of cave dwelling in this region was apparently not the exclusive practice of individuals or nomads, but rather by those who took this step as a result of pre-planning and out of clear choice and decision making.

Appendix: site descriptions and the findings of the field survey

During the course of the fieldwork, we documented various types of features in the village. Each feature was labelled with a separate letter and number, according to the function of the site (W = waterworks; C = cave; Q = quarry). In the site descriptions, we also referred to trees growing adjacent to the features, as an aid in locating them on the ground (to the best of my knowledge, a large proportion of these trees did not belong to the Arab village, but were planted much later by the Jewish National Fund.) Schematic plans were prepared for some of the features. This report only includes part of the upper side of the village (Fig. 4).

Quarries

Quarry 1 (Fig. 16): Height: *c.* 2 metres; length: *c.* 6 metres; faces east. Four rock-cut terraces are visible.

Quarry 2: It was not documented.

Quarry 3: It extends over a relatively large area, in the southern part of the village. Three long rock-cut terraces extending along the slope are visible, each one has an average height of 1–1. 5 metres Only in the western parts are there distinct quarrying marks and grooves.

Fig. 16. Quarry 1 from south (Avi Sasson)

The quarry begins at a distance of approximately 10 metres to the east of the central path of the hill.

Quarry 4: Length: *c.* 5 metres; width: *c.* 25 metres; depth: *c.* 2 metres. An elongated cave quarried in an east-west direction, at a distance of *c.* 30 metres from the path. Its use has not yet been determined; it may possibly conceal a deeper cave, it may be a quarry, or it may possibly be a trench dug by IDF soldiers. On the northern side is a mound of stones and earth removed from the cave. The impression is that on its eastern side there is a continuation, or a pit filled with alluvium and leaves. Quarrying marks are visible on its northern side.

Waterworks

Cistern on the 'hill of the village' (W1) (Figs. 11–12): About 10 metres to the east of the path. The cross section of the cistern is pear-shaped: a narrow entrance and a wide chamber, characteristic of cisterns in different regions. Diameter of the surviving opening is *c.* 2 metres; in the past the opening was narrower. Base diameter: 3 metres; depth: 3 metres; in the past the cistern was deeper, but it is now blocked with stones, alluvium, and rubbish. Height of cistern dome: 0.80 metres; thickness of cistern wall: 0.40 metres The cistern is built of local *kurkar* stones, with remains of plaster.

In the upper course of stones, at a height of 1.80 metres from the floor of the cistern, before the rising of the dome, one can see 8 niches (height: 0.20 metres; width: 0.15 metres; depth: 0.20 metres) that apparently held some sort of scaffolding during work on the cistern opening. A ceramic jar (opening diameter: 0.10 metres; length: 0.30 metres) is incorporated into the eastern wall of the cistern at a height of 1.20 metres. The function of the jar has not been determined; it may possibly be part of the cistern wall, or was perhaps a repository for some sort of dedicatory hoard.

A partially rock-cut conduit runs down the eastern side of the cistern, and drained the run-off rainwater. The cistern is generally situated in a small topographical depression, around which are mounds of building stones, possibly from a structure that stood nearby.

Alongside the cistern is a rounded stone (0. 60/1. 20 metres) that was used to cover the cistern opening. A peg was inserted into the stone, in order to anchor the rope of the bucket.

The southern well (W2) (Figs. 13–14): The well was built of local *kurkar* stone. Two building styles are noticeable, one with a thin layer of plaster which is typical of the Ottoman period. Visible in the later repairs is a coat of cement mixed with shells, typical of the British Mandate period. To the east of the well complex are iron pipes which were used to irrigate the surrounding orchards. The structures apparently were preserved due to the encompassing thicket of trees, thorns, and raspberry bushes.

The well complex is situated between the central and southern hills of Maghar, with the lower part facing westwards, and in the heart of the agricultural area, which comprises a citrus orchard and a vineyard. A plum tree is growing to the south of the pool, and a fig tree grows to the east of the central structure. An additional fig tree, with many branches and large in size, is located about 15 metres to the east of the well.

The well apparently originally had a water wheel of the 'Saqiya' ('Persian Well', or 'antillia') type characteristic of the coastal plain, with three chambers: a structure to the north – now mostly ruined; in the center – a complete structure that appears to be the well structure itself; and to the south – a reservoir.

The central well: The entrance to the structure is on the eastern side and is supported by an arch. A wall with a window is built on the western side. The structure has a flat roof and on its western side runs a channel that extends both to the reservoir and to the ruined structure. Length of the structure (north-south): 6.20 metres; width (east-west, including the channel): 4.50 metres. Thickness of wall of building: 0.35 metres; width of channel: 0.25 metres; thickness of channel walls: 0.35/0.10 metres.

It is unclear whether or not this is a Saqiya well, since we did not find a series of openings in the roof of the structure for the chain. The well may have been motorized, but no evidence of this was found. The well mainly served the cultivation at the foot of the village, and less for drinking purposes. This apparently is the southern well as described by Conder and Kitchener (1882: 411).

The pool: Built to the south of and adjoining the central structure. It is square in shape, and is covered with cement mixed with shells. The pool has two steps that entirely encircle it on the inside, with an additional three small steps in the northeast corner providing access to the pool. In the eastern wall are two drainage holes for excess water, one on the northern side and the other on the southern side, in which an iron pipe was inserted (so that the drainage opening could be closed and regulated). The channel continues along the eastern wall.

Pool dimensions: length of eastern wall: 8 metres; thickness of wall: 0.75 metres; depth of pool: 2.6 metres; height of upper encircling step: 0.35 metres; width: 0.25 metres; height of lower encircling step: 0.30 metres; width: 0.30 metres; height of each of the small steps: 0.30 metres; length: 0.60 metres; width: 0.65 metres; height of southern drainage opening: 0.50 metres; height of northern drainage opening: 1.80 metres

The southern structure: Of identical shape and dimensions to the central structure. All that has survived of it are the foundations of its walls, in which are visible the beginnings of arches. Nothing of the roof has survived.

Dwelling caves

Cave 1: Height: *c.* 2 metres; length: *c.* 7 metres; width: *c.* 3 metres A cave with an elliptical plan. In the façade there is a small rock surface facing a small depression descending to the

southeast. In the western wall of the cave are three rounded niches that functioned as cupboards.

Cave 2: Cave cut downwards into the rock with two openings in its upper part. The cave has a large chamber, with a flat and low floor. Its shape hints that this may not have been a dwelling cave. Due to technical limitations relating to the nature of the cave, it was only partially surveyed. A unique cave among the finds in Maghar, it is located at the eastern edge of an exposed rocky slope.

Cave 3: Quarried at right angles. Length of northern side: *c.* 10 metres; length of south side: *c.* 6 metres. Further along the rock-cut wall are traces of walls built of small *kurkar* stones. This may have been two dwelling units. An iron rod with two flanking loops, apparently a mount for an oil-lamp, was found *in situ*.

Cave 4 (Figs. 6, 10): On the same terrace, to the north of cave No. 3, and higher than it by *c.* 22 metres. It comprises three elements: (1) On the western side is the preserved western wall of a room, with two corners of additional walls. Length of the western wall: 5.2 metres. Remains extending for a length of about one metre survived from the walls abutting it. Length of the room: 4.5 metres. No additional remains were discerned on its southern side. (2) The central room of the structure: length (east-west): 7.5 metres; width (north-south): 6. metres; estimated height: *c.* 2.5 metres At this height there is a groove within the rock wall which was used to anchor the wooden beams that supported the ceiling of the structure. No built walls are preserved in this room. Above the presumed height of the ceiling, at the northwestern corner of the structure, are the remains of a retaining wall. Maximum height of the terrace above the room: *c.* 8 metres. (3) The cave: in the north-eastern corner of the cave is a semi-circular rock-cut cave, with no traces of plaster or the quarrying of chambers or shelves. The entrance is partially blocked by fallen stones. Quarrying marks are visible in the walls of the cave (thickness of the chisel blade used: 10/30 cm.). Length of the entrance: 1.5 metres; length of the cave: 2.5 metres; width of the cave: 2.2 metres; height: 1.7 metres. This small cave within the dwelling complex, was most probably used as the storeroom of this living unit.

Cave 5 (Fig. 10): Adjoining Cave No. 4, and a similar structure. Length of the longest side: 17 metres. Visible at the southern edge of the complex are the remains of walls with an east-west orientation. Length of the small room: *c.* 2.5 metres The lower part of the wall is rock-cut, and its upper part is built. At the end of the wall extending from Cave 4 is a fill of stones in a crack in the *kurkar*. Above the wall are square holes, used to anchor the wooden beams to support the ceiling. In the western wall that faces east, are two sections of walls, built of stone, running in a southerly direction. The walls, that are about half a metre thick, continue for a length of about one metre. Total length of the north-western room: *c.* 6 metres. Above the cave is a very large terrace (13 metres) that functioned as the courtyard in front of the house.

Cave 6 (Fig. 10): This is a continuation of Cave 5, and it rests on the eastern part of the quarrying of the preceding unit. The eastern wall of the cave is a terrace built of *kurkar* stones, and the western wall is rock-cut to a height of *c.* 1.5–2.0 metres (only a general description could be provided here because cactus plants cover this part.) Visible in the wall are repairs in the bedrock in the form of a fill of small *kurkar* stones in some empty spaces. Along the wall are relatively large chambers that were used for storage. In its southern part is a wall facing eastwards that constitutes the infrastructure for an additional room. It is difficult to define the length of Cave 6 in the absence of a wall that would enclose this unit on the north.

Cave 7 (the 'bat cave'): This cave is highly instructive regarding the character of the village caves, since this is the only one known to us that has been preserved in its entirety. Since this cave is also a natural 'bat colony,' we could not remain in it for long periods of time.

The outer wall of the cave, which is the cliff on which the village was situated, is quarried straight and vertically. The cave entrance is square with drafted edges (in the centre of the

wall), and measures 1.20 × 1.00 metres. A rock-cut wall enclosing the unit extends eastwards from the northern end of the wall. The upper continuation of the western wall is built of small *kurkar* stones. Length of the outer wall (north-south): 10 metres; length of the inner cave walls (north-south): 8 metres; length of the inner walls (east-west): *c.* 7 metres; height of the cave: 2 metres. The ceiling and the walls of the cave are flat. In the centre of the cave is a rock-cut retaining pillar with a diameter of approximately one metre. This find is the only one of its type in Maghar and its environs, and, to the best of our knowledge, is not even paralleled in the entire coastal plain. This method of supporting rock-cut caves is well-known from caves in the Judean Shephelah, in cisterns in the Negev, and in other regions as well.

Cave 8: Situated *c.* 10 metres to the north of Cave 7. U-shaped quarrying, with no building details or chambers. Length: *c.* 12 metres. A small terrace is built on the slope under the cave, which functioned as the foundation for the cave structure.

Caves 9–10: Caves of simple plan. Not documented by us.

The 'hikers' cave' (Figs. 7, 9) One of the caves that is preserved almost in its entirety. It is situated at the foot of the 'hill of the pools', to the north of the central path of the Maghar hill. Visible in the cave are many quarrying marks, both in the cave itself and in the chambers within it. The height of the cave progressively decreases the further one advances into it. Length of the cave: 9 metres; width of the entrance: 6.5 metres. This is not the width of the original entrance; the wall that enclosed the entrance has collapsed. Based on the debris of the stones at the cave entrance, a wall was built across in its façade. Height of the entrance: 3 metres; height of the western wall: 1.5 metres. In the western wall is a rock-cut alcove, that was used as a shelf. Length: 0.90 m. ; height: 0.60 metres. ; depth: 0.40 metres. In the southern wall is a large but shallow rectangular alcove, apparently used as the base for a cupboard. Height: 1.70 metres; width: 0.90 metres. ; depth: 0.50 metres. To the left of the cave are the remains of an additional cave.

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A *Crux Gemmata* Representation on a Lamp From Ein Gedi

ANNA DE VINCENZ

Among the many finds from the excavations conducted by Yizhar Hirschfeld at the Byzantine-period village of Ein Gedi on the western shore of the Dead Sea, is a unique lamp with a representation of a *crux gemmata* on its upper face (discus). The village of Ein Gedi has previously been identified as Jewish primarily because of the presence there of a large synagogue, discovered in the 1970s, with Hebrew and Aramaic inscriptions on its floors (Barag *et al.* 1981: 116–119). During a study undertaken by this author on the ceramics derived from the excavation of the village, a number of artifacts were found bearing cross decorations. These items include 'Candlestick' lamps and Late Roman Fine Ware vessels, all of which come from scattered locations in the village, dating from the fifth and sixth centuries AD. The purpose of the present article is to discuss the significance of the appearance of 'Christian' artifacts, such as the *crux gemmata* lamp, in a village context previously regarded as solely Jewish.

Description of the lamp

The lamp is mould-made, flat and elongated in shape, with a bow-shaped nozzle (Fig. 1). It has a stump-handle without a perforation. The filling hole is central and rather small. The wick-hole is broken. It is made of a particularly fine ware (Munsell Chart reading: 7. 5YR 7/4 pink, with many black and white inclusions). The rim is decorated with an intertwining zigzag line. The area of the discus is surrounded by a band filled with dots. This band continues towards the wick-hole, which it probably originally surrounded. The discus itself is decorated with a bejeweled cross standing on an indeterminate structural feature. The filling hole forms the centre of the cross. Four circles are positioned between the arms of the cross.

Iconography of the cross and symbolism

In a study on the origins of the 'Jerusalem Cross', Conrad Schick (1894: 183–189) proposed that the Byzantine cross with the four circles between the arms of the cross was the actual precursor of the later 'Jerusalem Cross'. The circles subsequently developed into four smaller crosses, as may be seen in the study made by Testa

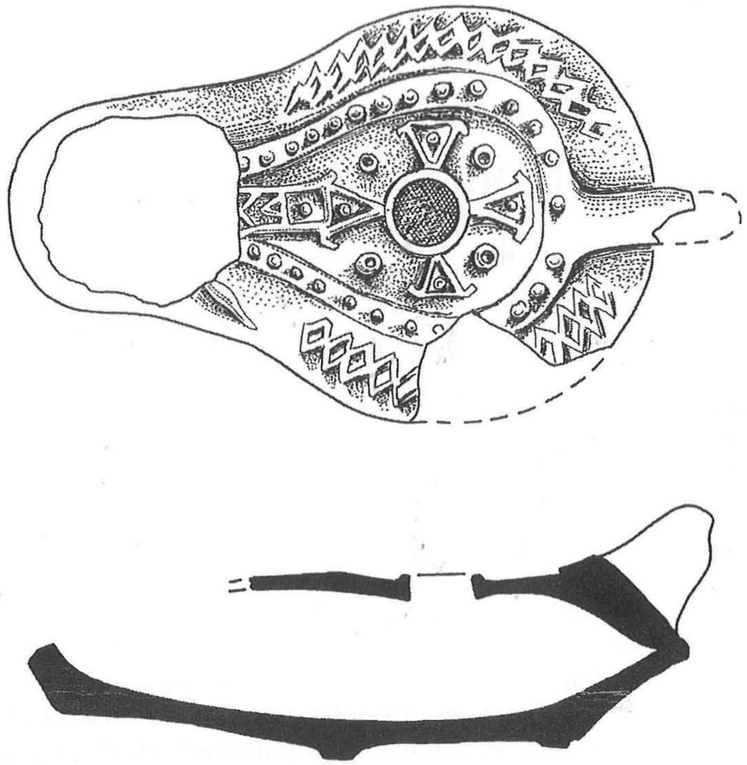

Fig. 1. Ein Gedi: lamp with *crux gemmata* representation.

(1981: Fig. 128: 14 and 20). The use of circles with crosses as a decoration between the arms of a cross is also shown on a sculptured slab from the Armenian Garden excavations in Jerusalem published by Tushingham (1985: 498: Pl. 124). There the bejeweled main cross is depicted standing on a pedestal or a column. This latter feature parallels the Ein Gedi Cross. The Armenian Garden cross was dated according to existing parallels to the sixth century (Tushingham 1985: 100). Other researchers such as Desreumaux and Humbert made a typology of crosses based on their finds from Khirbet es-Samra in Transjordan (1981: 33–84). The Ein Gedi cross does not have an exact parallel in their typology, but it is close to Type L, the so-called *nufus* cross, because of the circles between the arms (1981: 380: L), and, more specifically, to their Type S with representations of crosses on pedestals which are presumably bejewelled crosses (1981: 382: Sa-Sb). Greek type crosses also

appear on ceramic vessels of the Late Roman C type. One type has a cross with splayed tips, while another has flaring arms. Plain or bejeweled crosses were stamped on the bottoms of bowls in the fifth and sixth centuries (Hayes 1972: 364–365: Fig. 78: 69o-q, 70r-s; 366–367: Fig. 79: 70a, 71–75). A bowl depicting three stamped crosses on a pedestal was found at the Athenian Agora and seems to relate directly to the scene of the crucifixion (Hayes 1972: 349; 366: Fig. 79: 76n).

The overall significance of the Ein Gedi lamp is due to the particularly rare representation of a bejeweled cross on it. The cross is shown standing on an indeterminate structural feature, which might represent a pedestal, column or steps. Unlike common representations of crosses on lamps (examples are known from Ein Gedi), it would appear that a very special bejeweled cross was represented here, one which would have had a very specific meaning for Christians. There are numerous examples in Byzantine art of crosses shown on pedestals or positioned on top of rocky mounts, and these refer specifically to the cross on Golgotha in Jerusalem and not just as a general symbol of the crucifixion and of Christianity. Historical sources dating back to the late fourth century mention that a replica of the cross was set up on top of the actual rock of Golgotha in the area of the Church of the Holy Sepulchre of today (Hunt 1984: 12). Indeed, steps leading up to the rock of Golgotha are mentioned by visiting pilgrims in the sixth century, notably by Theodosius and the Piacenza Pilgrim, and archaeological excavations there have revealed remains which may have been part of some plastered structure on which a replica cross would have been placed (Gibson and Taylor 1994: 80–81). According to an account by Theophanes (*Chron.* 86:28), a gold and bejeweled cross was placed on the Rock of Calvary; some scholars (notably Taylor 1993: 123) believe this to be a legendary tradition but this need not necessarily be the case. From the fourth century onwards the adoration of the True Cross became very fashionable and relics were dispersed throughout the Christian world; many fragments of wood were kept in bejeweled reliquaries which took the form of the bejeweled cross surmounting Golgotha (Tsaferis 1974).

Conclusion

The heavy symbolism inherent in the representation of the bejeweled cross on the Ein Gedi lamp, must suggest that its owner was a Christian. It is hardly likely that a Jew or a pagan would have owned such a lamp, especially one with a depiction of the replica cross on Golgotha, representing not just the symbolism of the crucifixion of Jesus but also the adoration of the True Cross. As mentioned previously, additional artifacts (primarily bowls and lamps) were found at various locations in the village ornamented with common crosses (Fig. 2). One could conceivably argue that non-Christians regarded these common crosses as ornamental signs devoid of any symbolical meaning. However, the same cannot be argued for the lamp with the bejeweled cross.

What does this imply in terms of the ethnic make-up of the village of Ein Gedi in the fifth and sixth centuries, and the religious beliefs that were held by its

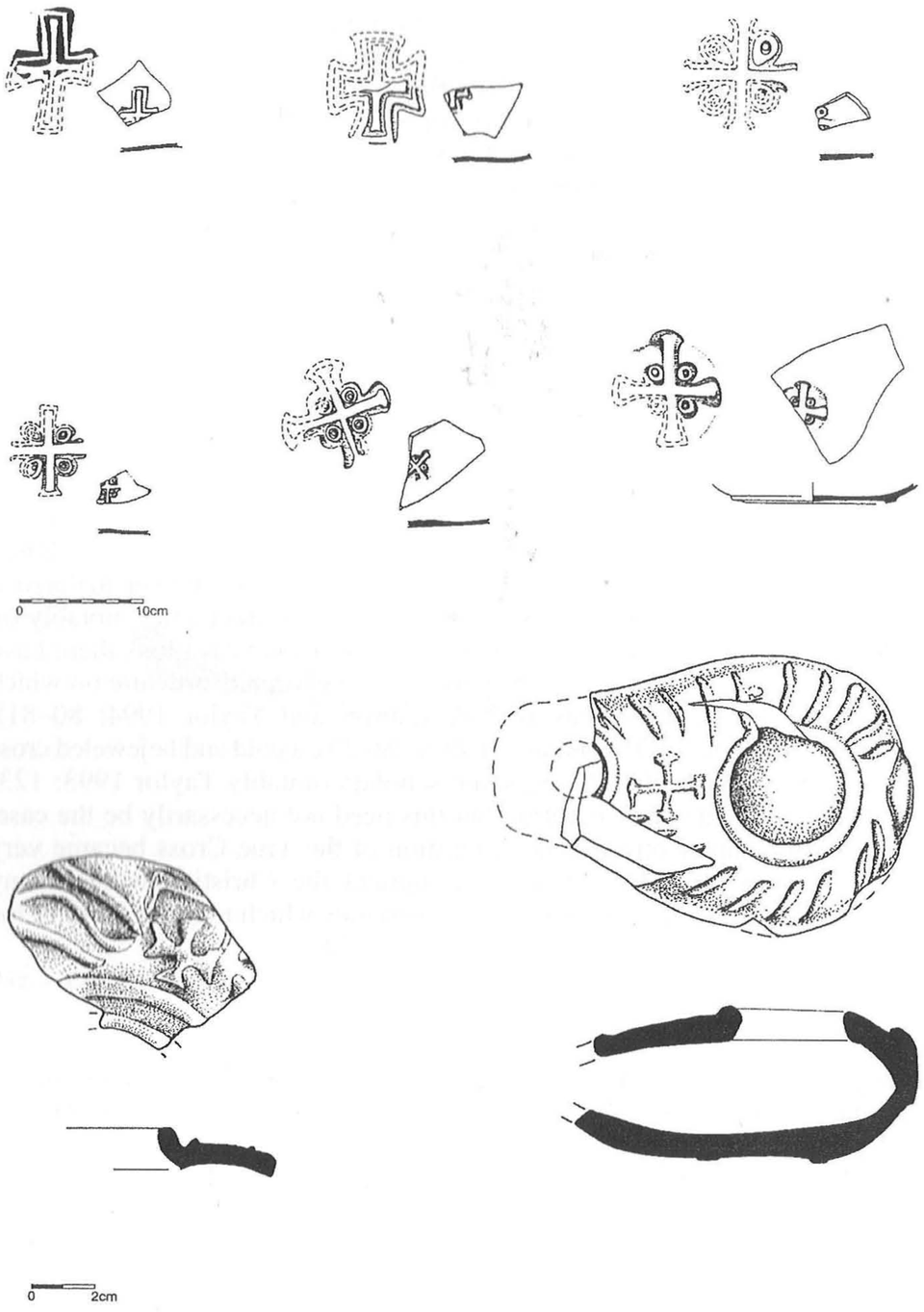

Fig. 2. Ein Gedi: Stamped LRC bowls and two lamps.

inhabitants? The archaeologists working at the site, notably Barag and Hirschfeld, have identified the village as Jewish because of the presence there of a prominent synagogue. Does this mean that only Jews inhabited the village in the Byzantine period? I think the presence of this special lamp with a depiction of a bejeweled cross, with the strong Christian symbolism connecting it to the crucifixion and the Rock of Golgotha, undoubtedly indicates some Christian presence at Ein Gedi. Further support for this comes from the array of other artifacts from the site bearing crosses. Does this, therefore, suggest a mixed Jewish/Christian village at the site and what would have been the size of the Christian component amongst its inhabitants? The fact that the church fathers, Eusebius (early fourth century) and Jerome (late fourth-early fifth centuries), refer to Ein Gedi as 'a very large village of Jews' (*Onom.* 86:16) probably indicates that the proportion of Jewish inhabitants at the site was in fact much larger than that of Christians.

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I wish to thank Professor Yizhar Hirschfeld for allowing me to publish this lamp. A technical report on this lamp will also appear in the chapter on the ceramics in the Final Report of the Ein Gedi excavations. The drawings are the work of Helena Bitan and Julia Rudman, whom I thank.

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James the Just, or just James? The 'James Ossuary' on Trial

EMILE PUECH

A limestone ossuary inscribed in Aramaic 'James son of Joseph, the brother of Jesus' was recently published by the French Old Testament scholar André Lemaire (2002) and gained worldwide notoriety. Its owner, the Tel Aviv collector, Oded Golan, claimed to have acquired it from a Palestinian antiquities dealer after the Six Days' War. While emphatically stating that both the inscription and ossuary date to *circa* AD 63 and that it most probably refers to James, the brother of Jesus, Lemaire further asserted in an interview to the French newspaper *Le Monde*, that this 'was not absolutely certain, but probable, even very probable' (Tincq 2002: 26). Referring to James, first Bishop of Jerusalem, stoned and executed in AD 62, Lemaire's astonishingly precise conclusion went far beyond the basic archaeological and epigraphic data provided by this ossuary – as was immediately pointed out by a number of scholars, notably Israeli. Was this ossuary really connected with James, son of Joseph, husband of Mary, and genetic brother of Jesus of Nazareth?

Ossuaries

Thousands of ossuaries have been recorded and more than one thousand are known in the collections of the State of Israel (Rahmani 1994). Although first century AD Jews not only practiced burial of bones in stone ossuaries, but also in sarcophagi (some inscribed), this Herodian-period (first century BC) custom peaked in frequency in the first century AD, becoming widespread in Roman Palestine by the early second century AD. The use of ossuaries is evidenced for at least three generations before the fall of Jerusalem to the Romans in AD 70, and thus covers about one century. Approximately a year after burial, and once the body had been purified by the decomposition of the flesh (symbolising 'sin') into the soil of the Land of Israel, the bones were gathered together and placed in an indestructible box in a state of maximum purity to await resurrection. Stemming from a novel interpretation and 'modern' adaptation of the vision of the dry bones, as spelled out in Ezekiel 37, this practice appears to have coincided with the concept in the last third of the first century AD of the belief in an afterlife as reflected by the Pharisaic School of Shammai (Puech 1993a).

An ossuary could contain the bones of one person, sometimes of several, and customarily those of husband and wife (and children), or father and son, mother

and daughter, brothers and sisters – in fact, the bones of a family in the strictest sense. Depositing bones in an ossuary was the task of sons (never of strangers) and was considered a duty of filial piety. Secondary burial in an ossuary, like burial in a sarcophagus, though to a lesser degree, implied the ownership of a rock-cut tomb that not everyone could afford. Associated with relatively affluent families, ossuary reburial was the answer to the necessity of making room for future inhumations on funerary benches and in *loculi* (*kokhim*). This practice, therefore, presupposed a tomb belonging to a family in the widest sense. The marked decline of ossuary reburial after AD 70 was due to the reduced size of the Jewish population of Jerusalem. Quite a few ossuaries were not decorated or even inscribed (those decorated and inscribed are but few). [1] This sorely deprives us of precious onomastic data for statistical purposes. Finally, ossuaries were not exclusive to Jerusalemites. In Jericho, some ossuaries even belonged to well-off Jerusalemite families (Hachlili 1978; 1979; 1980; Hachlili and Smith 1979; Hachlili and Killebrew 1983), and ossuaries have occasionally been found in various other locations, even as far as the Galilee.

The inscription

The relatively carefully cut letters of this inscription provide an even wider chronological range than that of the use of ossuaries (Fig. 1). The shape of all the letters in calligraphic and cursive scripts is well attested at least from the mid-first century BC to the second century AD, this including the *aleph*, *daled* and *yod*. [2] Contrary to Lemaire, it is absolutely impossible to pinpoint on palaeographic grounds the date of this ossuary to the first century AD nor to the decade preceding the Fall of Jerusalem, except for reasons other than palaeographic ones. The script is continuous, interrupted only by a space and characterised by a final ending given to the preceding letter; hence a sentence with two components linked by a comma in the translation: 'James son of Joseph, the brother of Jesus'.

Fig. 1. The inscription on the 'James ossuary'. (Drawing by S. Gibson based on photographs published in Lemaire 2002).

The language

The Aramaic formula which is recognizable both by the term used to express filiation (*br*) and by the typically Aramaic *'ḥwy d-yšw'*), literally 'his brother, (the one of) Jesus', is also well-attested, even in its abridged form in literary and epigraphic texts from the first century BC onwards (*Genesis Apocryphon*, legal acts, ossuaries, *targum*s, and mosaics). [3] The *'ḥwy d-* formula, however, is different from *'ḥ d-*. Thus, one should not translate (following Lemaire) 'brother of Jesus' which renders the latter formulation, but 'the brother of Jesus'. In one case, Jesus is one of James' brothers, in the other, he may be his only brother, this being of some consequence for the interpretation of the inscription.

Onomastics

Lastly, the three names mentioned were of common use in the Hasmonean, Herodian, and post-Herodian periods, together with Simon, Judah, Eleazar, Manaem, John and Matthew. If only restricted to the medium of ossuaries, many more examples of names are known than the numbers quoted by Lemaire who limited his 'statistical' search to the Israeli state collection published by L. Y. Rahmani (1994; see now the lists published by Tal 2002). The onomastic list may be greatly lengthened by the habitual use in the Hasmonean, Herodian and post-Herodian periods of papponymia (re-use of the names of grandfathers) and of patronymia as early as in the first century BC, not to mention other epigraphic sources, thus modifying considerably the results obtained by statistics. Moreover, the anonymous character of most ossuaries and the fact that they concern only one part of the population, probably those siding with the School of Shammai, should not be forgotten.

The great frequency of a restricted number of anthroponyms resulted in the widespread use of appellations or nicknames acting as second names. This would have been the case for the famous ossuary published as having belonged to Caiaphas, but which should be attributed to a 'Joseph son of Qoppa' or 'Qépha' (Puech 1993b). [4] In order to distinguish between namesakes, the profession or another detail describing the deceased was added on ossuaries, for instance 'priest', 'scribe', 'mason', 'artisan', 'proselyte', or even his/her place of origin (Tyre, Alexandria, Jerusalem, and so forth). Alternatively, the place of origin would be clearly identifiable from a Palmyran, Caucasian or other script.

The identification

Do all the above considerations compel one to accept the proposed identification as the only possible or even the most plausible? The only legitimate conclusion is that the archaeological data (the ossuary itself), palaeography, linguistics and onomastics provide a great number of possibilities within a century at least.

Lemaire brings to his own rescue well-known historical data from the New

Testament, the works of the Jewish historian Flavius Josephus and of the Church Fathers. It is well established that Joseph did not repudiate his pregnant wife Mary, but took her into his house, where she begat Jesus. Thus, Jesus is most frequently dubbed 'son of Mary', which would be totally unexpected if he had really been Joseph's son. Admittedly, the Gospels also mention the brother and sisters of Jesus. In order to understand these terms in Palestinian Greek which describe family links, we should bear in mind that this Greek expressed in translated form the native language of a bilingual population. Words written or spoken in Greek retained their usual sense in the current local language – Aramaic – where the term 'brother', for instance, meant concurrently blood-brother, half-brother, husband, uncle, nephew, cousin, friend, and companion (likewise for sister).

Only the context allows us in general to qualify with greater precision what was evidently clearer for contemporaries than for us. Thus, the use of these words in the Gospels may not be limited only to the restricted meanings in our European languages. Flavius Josephus himself wrote: 'Hananiah summoned before a sanhedrin James brother of Jesus, dubbed Christ' (*Jewish Antiquities* XX. 200; ed. R. Marcus 1976). On the one hand, he qualified Jesus so as to distinguish him from others by the name of Jesus, and on the other, it is by no means certain that to his mind 'brother' meant 'brother by blood tie'. [5] Like the New Testament, Josephus' works must be viewed against a common Semitic background.

Nowhere in the New Testament is a James, son of Joseph, the husband of Mary, mentioned, except by false deduction from one of the possible meanings of the attribute 'brother of Jesus'. In Mark 6: 3, Jesus is not even said to be 'the carpenter's son', as he is in Matthew 13: 55. It is only by popular hearsay that he was thought to be 'the son of Joseph' (Luke 3: 23 and 4: 22; John 6: 42). In Luke's Gospel (1: 26–38), however, Joseph is manifestedly not Jesus' father. Nothing in the New Testament proves that James, Joseph, Simon and Jude were the sons of Joseph *and* Mary; it is but a mere modern extrapolation from the restricted sense imposed upon the word 'brother' in Mark 6: 3. In this Palestinian context, 'brother' did not suddenly take on the meaning of 'cousin' with the compiling of the *Protoevangelium of James*, as stated by Lemaire in connection with this ossuary. It had this meaning long before the first century AD. Texts should be examined within the context of their time, not bent to conform to our demonstrations from a modern viewpoint. Paul calls James 'the Lord's brother', when describing the Head of the Church of Jerusalem, also known traditionally as 'James the Just', a useful precision in order to distinguish him from the others named James, notably the apostles. [6]

According to John 19: 25–27, Jesus entrusted his mother to John, his beloved disciple, who took her into his own home. If Jesus had had brothers, it would have been difficult to entrust her to someone other than his genetic brothers for whom (since Joseph must have died before Jesus' active public life) it would have been a duty of filial piety. Yet, this was clearly not the case, at least if one takes the above passage in its literal sense, but in any event this much debated point does not weaken the demonstration of the other scriptural evidence taken as a whole. To his mother

Mary's remark, 'Son, why has thou thus dealt with us? behold, thy father and I have sought thee sorrowing', the child Jesus answered: 'How is it that ye sought me? wist ye not that I must be about my Father's business?' (Luke 2: 48–50), a father whom he called *Abba*. Luke as well as Matthew (1: 16 and 20–25) did not consider Joseph to be Jesus' father. Yet it was Joseph who named the child 'Jesus', in the same way that Zachariah named his son 'John' (the Baptist). Thus, Joseph fulfilled his legal role as father by inserting Jesus into the Davidic line. Moreover, long before the *Protoevangelium of James*, despite diverging traditions, the two Gospels were in agreement over Jesus' virginal conception, which accomplished prophetic promises. After participating in his education as a father should, Joseph passed on his carpentry skills to Jesus to such an extent that it was commonly believed that Jesus was the 'son of Joseph, carpenter, son of the carpenter'.

Jewish Christian ossuaries

Although it is quite possible that some Jewish Christians followed the Pharisaic funerary practice of the School of Shammai of reburial in ossuaries devoid of distinctive signs (cross, chrism), does this necessarily mean that this ossuary belonged to James the Just? This ossuary bears no trace whatsoever (including in the formulation) of any cultic practice by the first Christian community of Jerusalem, nor of any special respect or care.

James' tomb

At least until the second century AD, Jewish and Christian Tradition located James' tomb at the place of his lapidation and execution in Aelia Capitolina, below the walls of the Temple where a stele was erected. Having preserved the memory of this location, Christian tradition, however, subsequently moved this location to opposite the tomb known today as Zachariah's (Puech, 2003b). Unfortunately, the exact provenance of the 'James ossuary' is unknown, but East Jerusalem appears probable, judging from the comments of antiquities dealers. It is, however, out of the question that this ossuary comes from excavations, albeit illegal, of a rock-cut tomb in the supposed area of James' tomb since 1967 and even prior to that, since no excavation was ever undertaken there. Moreover, Joseph husband of Mary certainly did not own a rock-cut tomb in Jerusalem, since Jesus was laid in a recently cut tomb belonging to Joseph of Arimathea. If James had had a burial cave cut for himself and his family, he was clearly not laid to rest there according to a unanimous tradition. A tomb was found containing ossuaries incised with the names of Jesus, Joseph, Maria(m), John and Matthew, but the excavator did not jump to the conclusion that this was the family tomb of Jesus and of his apostles (Kloner 1996; Kloner and Zissu 2003: 207–208). Faced with discoveries of this sort, it is necessary to remain within the realm of possibility and likelihood.

Reasons for the Aramaic formula

But why then was the phrase 'the brother of Jesus' added? The formula is rare for an ossuary, but not abnormal, and certainly well known in first century AD Aramaic, notably in the abridged spelling standing for *'ḥwyy dy*. Was the aim to distinguish the first name which was insufficiently known, by the addition of that of a more famous brother, as Lemaire has suggested? This could not be the case for James, first Bishop of Jerusalem, in his time one of the city's most famous characters according to Jewish tradition, a 'pillar' with Peter and John according to Jewish Christian tradition. [7] One would then have expected 'James the Just' or 'the brother of the Lord/Messiah', but not 'the brother of Jesus'. Besides, the comparative example put forward by Lemaire, 'Simay son of Asiyah, the brother of Ḥanin' shows that such an explanation is not easily acceptable, since none of the three names are known. A simpler explanation may be that a brother by the name of Jesus, not a son of James if he was married, had deposited the bones in the ossuary, hence the mention of his name. A much more plausible explanation is that it was important to distinguish between people bearing the same name and the same patronymic in a family tomb in the widest sense, when names repeated themselves from generation to generation, in collateral branches or even in the case of half-brothers, hence the occasional necessary descriptive phrase, 'the brother of X'. This is all the more likely, since in a family tomb (which was not a public square), there was no question of authenticating the bones of an individual by citing the name of a better-known brother: fame is irrelevant in this context. Ossuary inscriptions were of strictly private and family use. That is all that may be said. Thus may be discarded Lemaire's decisive point in favour of his hypothesis.

A more modest and prosaic conclusion

Without any clear and indisputable proof, it is difficult to accept as very probable or even possible the identification of this ossuary as put forward by Lemaire. Too many suppositions are marshaled, and even they are unlikely. Whatever the place of discovery, the ossuary cannot have originated in the as yet untouched area unanimously agreed upon by Jewish and Christian tradition to have been where James was buried. Consequently, this ossuary certainly does not provide the most ancient epigraphic proof of the existence of Jesus of Nazareth. The simple attestation of a James son of Joseph who had a brother named Jesus, is all that we are left with. To say more than that would go far beyond the conclusions that one may scientifically extract from the basic data provided.

It is salutary to recall the wise comment of a great historian, Father F. -M. Abel, regarding the priestly family of the Bene Hezir: 'Amongst the names noted by the Marquis de Vogüé in this text, there is a Shimon, son of Joseph. That inside an undesecrated burial chamber was discovered in the fourth century a set of *nari* [soft limestone] ossuaries bearing names such as Jacob, Simeon, Zachariah, Joseph, etc, incised with a stonemason's point, is a plausible hypothesis, for similar discoveries

still happen in the twentieth century. That the bones of James, the Lord's brother, were secretly brought to this hideout in the troubled reign of Hadrian is another supposition that may be put forward. But, by getting lost in the field of suppositions, one leaves the realm of history' (Abel 1919: 499).

André Lemaire who admits himself that nothing in this inscription confirms his identification, has had the audacity nevertheless to state that this ossuary was most probably that of James, first Bishop of Jerusalem, the Lord's brother. On the sole basis of the data provided by this ossuary, it is quite impossible to accept his conclusion which, moreover, conflicts with tradition, unless the facts are instructed to 'speak' so as to conform to a preconceived idea. But in this case, history and likelihood are overtaken by pure fantasy.

The Last Turn of the Screw

Since I wrote the above in the week following the publication of the inscribed ossuary in the *Biblical Archaeology Review*, the Israel Antiquities Authority concluded their scientific investigations on 15 June 2003 with the following statement: 'The inscription is a forgery because the patina in the grooves on the letters is artificial and not natural, and includes ground-up micro-fossils which would not necessarily appear in normal calcium carbonate solutions resulting from moisture and humidity' (for the scientific determinations, see Dahari *et al.* 2003). Thus, if the ossuary is a real archaeological artefact, its inscription is a fake cut by a modern forger, who on the one hand used well-known names and Aramaic formulae, and on the other skillfully combined a mixture of letters in cursive, semi-cursive and calligraphic scripts dating to between the end of the first century BC and the second century AD. André Lemaire's demonstration to convince us into believing that this was the most ancient archaeological evidence for Jesus' existence, for that of his siblings, and for Mary's non-Virginity (which already could not pass the test of an objective study of the inscription) collapses altogether and forever.

As I had not seen with my own eyes the ossuary in question, my study was undertaken solely on the basis of published illustrations, and I could not but accept the scientific veracity of the conclusion reached in 2002 by the scientists of the Geological Survey of Israel that the inscription was authentic (quoted in Lemaire 2002). Lemaire's demonstration, however, went far beyond the conclusions that one could justifiably reach. This context being taken into consideration, my study, which aimed to refute from a completely different point of view Lemaire's fallacious arguments, has retained its internal consistency.

Following the conclusion of the two committees of the Israel Antiquities Authority that the ossuary inscription is most probably a fake (Dahari *et al.* 2003), in July 2003 the owner of 'James' ossuary' was arrested on suspicion of forgery by the Israeli Police, and the tools of his 'trade' were allegedly seized in the workshop of his Tel Aviv abode. Thus ends happily for the seekers of truth, the saga of an apparently sensational discovery that turns out to be merely hot air blown out of

proportions by a manipulated media. With good reason, the last word should be given to *Ecclesiastes* (1: 2): 'Vanity of vanities. . . ; all is vanity'.

Acknowledgments

My arguments against Lemaire's assertions were first summarized by S. A. Kingsley ('Ossuary of James and Bones of Contention', *Minerva* 14/1, January-February 2003, 3–4), and subsequently aired in greater detail in *Minerva Online*, Jan/Feb 2003. I am grateful to Dr S. Gibson for suggesting the publication of a fuller account in the *BAIAS* and to Dr S. A. Kingsley, Managing Editor of *Minerva*, for granting me permission to do so. Quotations from the New Testament follow the Authorized or King James Version.

Notes

1. Traces of two incised rosettes and red paint are visible on the side of the ossuary opposite the one bearing the 'James' inscription, are mentioned by Lemaire (2003). From his comments, it would appear that these decorations were of lower quality than the incised inscription on the other side of this particular ossuary.
2. See, for example, Yardeni 2000: 147–211.
3. The Aramaic form *d(y)* is never a preposition (Lemaire 2003), but either a genitival or relative particle, or a conjunction.
4. Moreover, since Caiaphas was a Sadducee, the custom of ossuary reburial was probably not followed by his family. See also Puech 1993a: 193–95.
5. According to Heinrich 1973 ed. : 19, the Greek word *adelphos* meant 'brother, son of the same father or same mother (= half-brother), relative (= cousin), member of a same people or belief, friend, companion', thus offering precisely the same range of senses as in Aramaic or Hebrew noted above. This must be emphasized and taken into account as regards common usage in Palestine, as well as for the recipients of written works who were not to be mistaken.
6. In particular, see Hegesippus in his *Memoirs* quoted by Eusebius of Caesarea in his *Ecclesiastical History* II: 4–18 (ed. G. Bardy 1952), or Josephus also cited by Eusebius (*Hist. Eccl.* II: 20). In the *Epistle to the Galatians* 1: 18–19, Paul mentions his trip to Jerusalem to see Peter, with whom he stayed fifteen days. He adds: 'But other of the apostles saw I none, save James the Lord's brother'.
7. Paul's *Epistle to the Galatians* 2: 9. Also Hegesippus in Eusebius of Caesarea's *Ecclesiastical History* II: 23: 19.

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The Late Roman Mosaic Pavements of Phoenicia and Northern Palestine: a Stratigraphic Approach

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The analysis of a corpus of 850 mosaic pavements in Late Roman Phoenicia and Northern Palestine, viewed within their socio-economic context, entailed examining the laying of mosaic pavements (this including technical aspects and bedding), the quality of decoration, the distribution of pavements in time and space, as well as the inscriptions which provide dates and names of donors and artists. The approach adopted represents a novel alternative to typical interpretations of mosaic floor decoration which either overwhelmingly focus on the development and diffusion of style (Budde 1969; Dauphin 1976: 113–149; Smith 1969: 95–113), or provide an exegesis of figurative iconography (Levi 1947; Lavin 1963: 181–286; Dauphin 1978: 10–34; Dunbabin 1978; Maguire 1987; Merrony 1998: 441–482). Key aspects of this study focus on the extent to which chronological patterns of mosaic floor laying may be used to gauge economic conditions, the factors which determined the quality and distribution of technique and decoration in different building types, and the social mechanisms of patronage.

Mosaics as an index of regional economic conditions: the limits of inference

In order to examine the floor mosaics within their broader economic context, the temporal pattern of floor laying was traced. It is largely dependent on the varying reliability of the dates ascribed to the corpus of mosaics studied. Where poor quality excavations in the region have been conducted (Whittow 1990: 15–16), the reliability of the dates assigned to the material by the excavators is questionable. Moreover, owing to a lack of available evidence, many of the examples from the corpus cannot be dated with any degree of accuracy other than in broad Late Roman terms. A great number of floors are dated by association with architectural sculpture, artefacts, coinage and pottery. Such media, however, often provide merely a *terminus ante quem*, or *terminus post quem*[1]. Dates, therefore, are ascribed in terms of decades or centuries, rather than years. Many mosaics in the sample examined have been dated by style – a method which presents its own particular set of problems. For instance, the figurative floors of the 'House of the Nile Festival' at Sepphoris have been dated by its excavators to the fifth century mainly on stylistic

grounds (Netzer and Weiss 1994), although the abstract composition and marked figural linearity in the style of these floors indicate rather the sixth century, in keeping with other pavements in the region. Scholars have also tended to date floors on the basis of style by comparison with other mosaic pavements in the same region dated stylistically in their own right, or with the support of loose archaeological evidence (Balty 1995; Donceel-Voûte 1998).

Despite the caveats referred to above, a general consensus among scholars has led to a widely accepted chronological scheme for much of the material included in the corpus. Nevertheless, caution is advisable, since future analysis may well result in a reappraisal of much of the material.

Mosaics containing inscriptions offer more reliable evidence for dating. Frequently, these state that a floor was laid in a particular indiction (a 15–year cycle particular to the Byzantine period) which may be matched to a precise calendar year. Alternatively, the laying of a particular mosaic is ascribed to an eponym (such as a bishop) whose tenure can be gleaned from historical sources, often with a fair degree of accuracy.

Those floors of the corpus that are too chronologically imprecise to be of any value for even this quantitative approach, were rejected. In cases when a pavement has been ascribed a date straddling the turn of a century and overlapping into the next century whilst also filling much or all of it (the floors of the Church of Mi'lya[2], for instance, that date to AD 400–525), or where a floor is dated to the end of one century, or the early part of the next (the floor covering the Portico area in Beirut[3] is one such example datable to AD 475–525), it was imperative to follow 'statistical' guidelines in order to infer more secure dates[4]. Goldrei devised the following solution ('counting method') for the floors falling within these date ranges. This is a method of attributing or counting examples with a relatively precise dating to a particular century. This method is crude, no more than most of the dating, and designed to match its limitations, but we argue that it is appropriate for the investigation in hand. In the case of Mi'lya, the greater part of the chronological bracket belongs to the fifth century; the probability is, therefore, that it dates to the fifth century. We have thus counted this as an example belonging to this century. However, in the case of the Beirut example, the date is evenly balanced across AD 500 and it is not possible to date it to the end of the fifth, or beginning of the sixth century, on the basis of probability. There is, however, a 50/50 chance that it falls either into last 25 years of the fifth, or the first 25 years of the sixth. Thus we count half of this example towards the fifth century and half towards the sixth. The chronological patterns which emerge are presented in Figure 1.

Pavements were also reviewed against the general economic situation in the Later Roman Empire. Other economic indicators, such as coinage (Harl, 1996: 191–94; Hendy 1985: 640; 1989: 147–50), inscriptions (Di Segni 1999: 149–78), spatial and temporal distribution of public buildings and settlements (Randsborg 1991: 53; Dauphin 1998), and agricultural production and export (Kingsley 1999; Lewit 1991) were also examined and related to the chronological patterns observed in the corpus in the light of historical events in this period; notably the Justinianic plague of AD

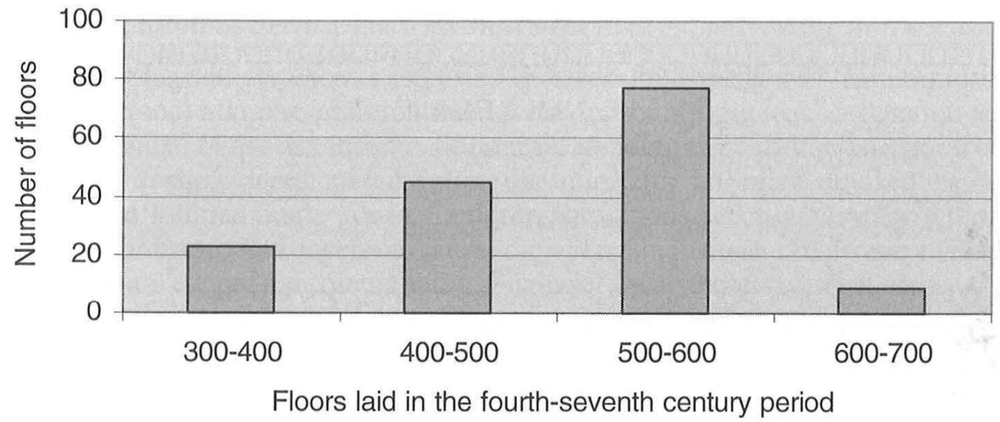

Fig. 1. Chronological pattern of the Late Roman mosaic pavements of Phoenicia and Northern Palestine.

540 (Allen 1979: 5–20; McCormick 1998: 35–118; Sarris 1994), as well as the Persian and Muslim conquests of 614 and 636–40 respectively (Foss 1997: 469–86; Schick 1995).

The observed pattern demonstrates that variations in the quantity of pavements laid during the Late Roman era broadly reflect fluctuating economic conditions in Phoenicia and Northern Palestine in the fourth-seventh century period. This was characterized by a slow development in the fourth century, prelude to a prosperous period peaking in the second half of the sixth century, before an economic decline in the seventh century (Fig. 1)[5].

Constituents of bedding and technique

The bedding of pavements and the techniques (*tesserae* size, cube density, interstice width, and colour range) involved in laying floors were also important economic markers. An assessment was also made of technical quality and how this may have related to room function, financial expenditure, the relative wealth of patrons, whether individual or collective, the hierarchy of patrons (or the liturgical hierarchy of patrons in religious buildings). The constituents of the bedding of mosaic pavements – the quality and thickness of materials – was also examined in relation to the same factors. Finally, the relative cost of laying floor mosaics was calculated.

Previous analyses of bedding have failed to grasp its economic significance in relation to financial expenditure since the 'hidden costs' of the bedding of floor mosaics had never been taken into account, let alone calculated (Caillet 1990: 409–14; Mango 1986: 11). These 'hidden costs' represented a considerable financial outlay in their own right, and are associated with the process of slaking lime – a major constituent of bedding – which required the acquisition and transportation of fuel, the cost of raw materials and the necessary manpower (Fig. 2).

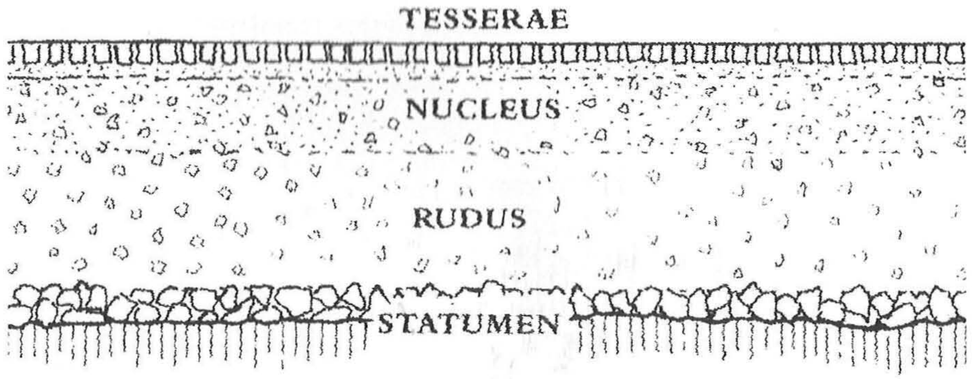

Fig. 2. Section of mosaic floor bedding (courtesy of R. Ling).

The solidity of each layer of bedding was achieved by the presence of lime mortar – a key ingredient. In Phoenicia and Northern Palestine, its ratio in bedding was generally one part of lime to three parts of aggregate. The amount of lime required to satisfy this ratio in bedding was considerable. For example, a bedding thickness of one metre over a floor area of 100 square metres would require over 33 cubic metres of lime. Moreover, the association of textual evidence culled from ancient sources such as *Diocletian's Edict of Prices* (Giacchero 1974) and the observation of technical details of the pavements themselves, demonstrate that mosaicists worked in teams whose wage scale was hierarchically organised: the *lapidaries structor* and the *calcis coctor* prepared the bedding, the *pictor imaginarius* or 'mosaic painter' drew the cartoon, the *tessellarius* was responsible for the execution of the less elaborate mosaic decoration, and the *museiarius* for the more elaborate decoration. Elaborate floors of good technical quality would have required an increase in the basic number of artisans and workers, and a commensurate increase in cost. The oft-repeated assumption that the sums of donations inscribed on floor mosaics related to the cost *per se* of the floors is mistaken, since these reflect levels of donation rather than actual costs. In fact, much of the real expense borne by the patrons who commissioned mosaic pavements lie 'hidden' in the bedding of floors and in the wages of teams of mosaicists, the multi-coloured tessellated surface being but the visible tip of a financial pyramid.

Although function, social status and financial expenditure have long been known to be key determinants of technical quality, the more unexpected conclusions of this research were that function did not always hold sway over technique, and technical quality did not always directly relate to social levels of patronage. Two figurative pavements from the 'reception' rooms of private buildings at Caesarea Maritima in Northern Palestine (Floor 11020 dated to the sixth century[6] and the fifth-or-sixth-century Ibex Mosaic[7]) were of a relatively poor technical quality and thus seemingly incompatible with their function as rooms of prestige and the financial means of the patrons who commissioned them.

The articulation of decoration with function, liturgy, patronage and society

The same range of issues was explored in assessing quality and distribution of representational and non-representational decoration. Figurative decoration was assigned a level of ornamentation commensurate with its quality, while floral and geometric designs were coded following international rules devised by the Association Internationale pour l'Etude de la Mosaïque Antique (AIEMA)[8] and graded according to four levels of complexity (Fig. 3). For instance, Room 1 (Phase B, AD 610) of the ecclesiastical farm at Shelomi[9], has a border pattern (Figs. 4–5) of Complexity Level IV (Balmelle *et al.* 1985: Pl. 82c) and may be contrasted with the field pattern (Fig. 6) in the nave of the sixth century Church at Nahariyya[10] of Complexity Level II (Balmelle *et al.* 1985: Pl. 219c). It may be inferred that the differentiations of quality and distribution of decoration in rooms and parts of rooms in different building types was determined by the same factors as technical quality (that is, its articulation with function, domestic and ritual use, financial resource, and category of patronage). In recent decades, despite the codification of entire mosaic floors, interpretations have concentrated on analysing a small number of motifs (Vitto 1996: 115–46). Alternatively, floors have often been merely coded *per se* purely for descriptive purposes, with little or no attempt to analyse the data (Avi-Yonah 1932: 136–81; 1933, 26–72; 1934, 187–93; Ovadiah and Ovadiah 1987).

This approach is novel in Graeco-Roman mosaic studies in so far that coding on the one hand, and complexity-grading on the other, have in association provided an analytical framework for a comprehensive assessment of quality of decoration and of its distribution according to functional building type, rooms and parts of rooms across the entire region. By emphasising the interaction of socio-economic aspects with decoration and technique, We hope to have succeeded in reintroducing the human element into mosaic studies which were in danger of turning into an abstract and sterile discipline.

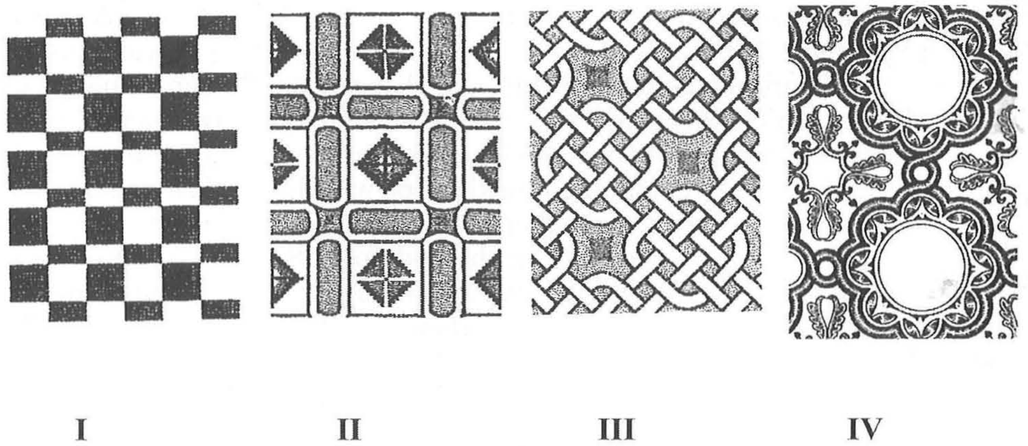

Fig. 3. Selective examples of Complexity Levels in non-representative decoration.

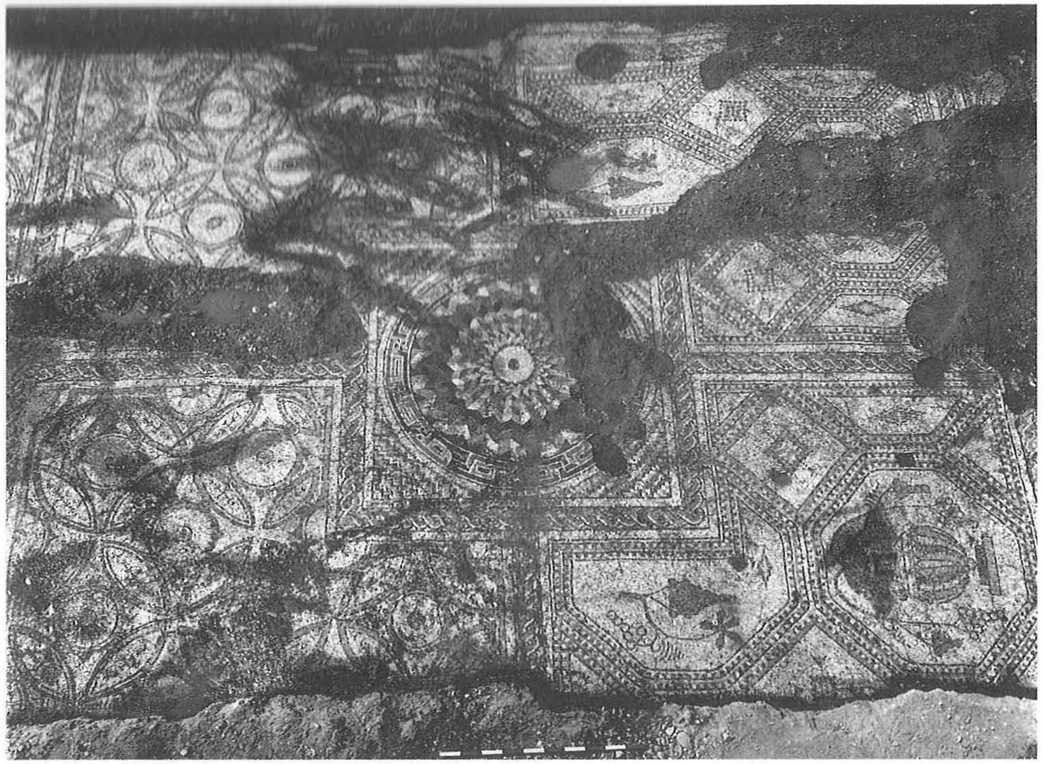

Fig. 4. Shelomi ecclesiastical farm (No. 7/56). Room 1 mosaic pavement (Photo: Zeev Radovan; courtesy C. Dauphin).

Patronage

The human aspect of mosaic studies is also vibrantly present in an appraisal of patronage as revealed by the mosaic inscriptions of Late Roman Phoenicia and Northern Palestine. The trends of benefaction observed (Fig. 7) fit well into the well-chartered historical picture of institutional change from Roman Antiquity to the Later Roman period (Kennedy 1985: 3–27): the scarcity of civic patronage by *curiales*, the greater number of donations by ecclesiastics on behalf of the Church or at their private initiative, and the donations of wealthy laymen and women towards the decoration of churches (Saller and Bagatti 1949; Hunt 1994: 106–26). These adhere to the pattern of decline of the traditional Roman system of civic management, counterbalanced by the increase in power of the Church and of wealthy landowners who soon became an urban force to be reckoned with. By contrast, Jewish and Samaritan patronage was less institutionalised, more family-and-community-oriented and extended to the lower echelons who offered benefaction in kind, material, or produce (Dauphin 1998: 332–36).

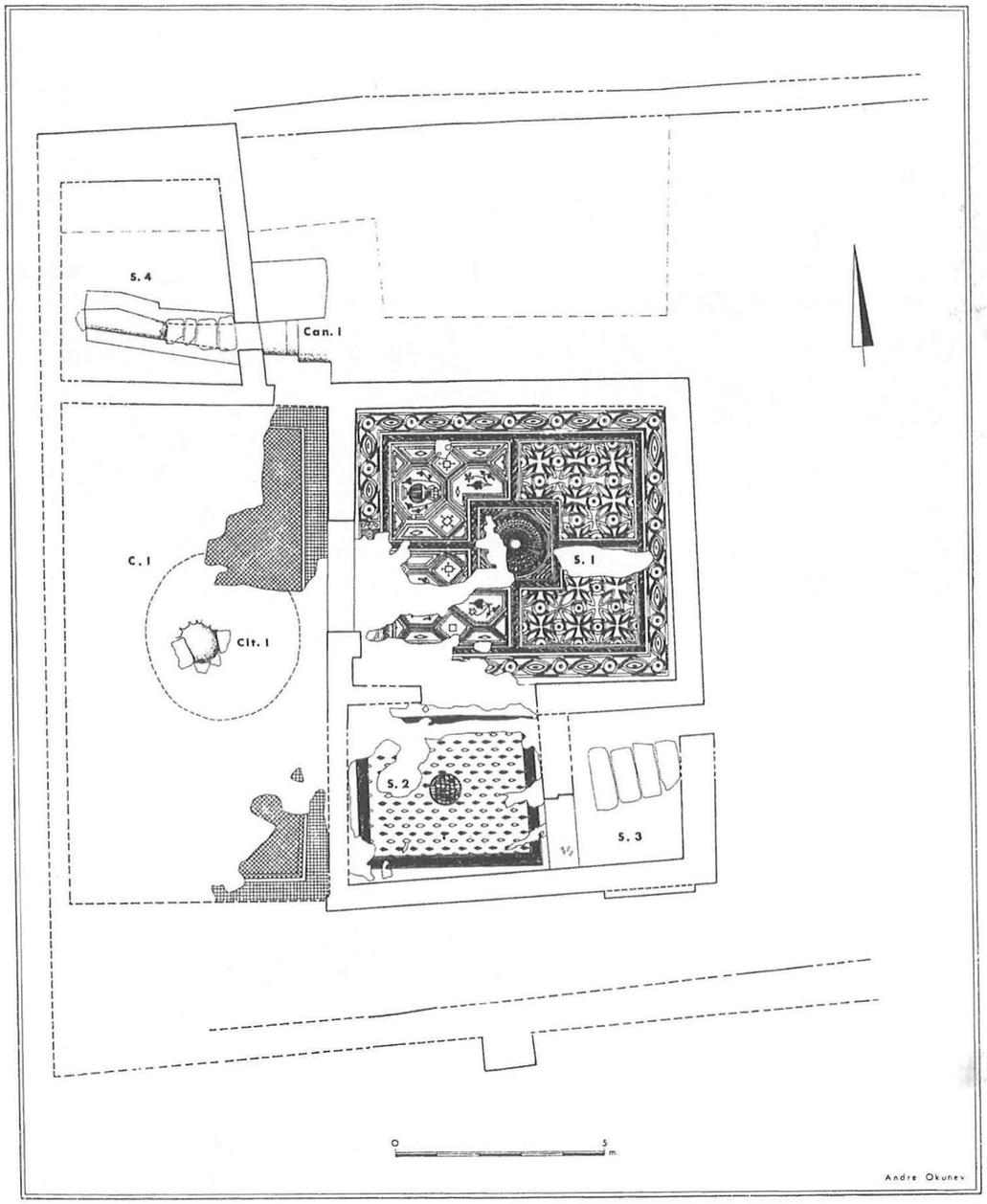

Fig. 5. Plan of the ecclesiastical property of Shelomi: S = room; C = courtyard; Can. = channel; Cit. = cistern (drawing Andrei Okunev; courtesy of C. Dauphin).

A stratigraphic analysis: archaeological, economic and social

Thus holistic and multifaceted, the socio-economic approach presented above represents a divergence from typical art-historical analyses that are concerned with

Fig. 6. Nahariyya (No. 7/19) Church, detail of nave pattern and central motif (courtesy of C. Dauphin).

Fig. 7. The occupation of donors in the mosaic floor inscriptions of Late Roman Phoenicia and Northern Palestine.

the meaning and style of mosaic floor decoration on a two-dimensional surface level. Instead, strong emphasis has been laid on a three-dimensional, archaeological appraisal of floor mosaics from a socio-economic viewpoint. Archaeology in which the stratigraphic analysis of excavated sites plays a major role, offers us an apt

metaphor. Our research has comprised the physical stratigraphic analysis of mosaic pavements from bedding to surface, and a social stratigraphic analysis which has revealed the makeup of the teams who laid the floor mosaics, the people who walked on them and admired them, and the patrons who commissioned and paid for them.

Notes

1 Butcher (1997–1998: 173–80) has demonstrated that coinage dating to the reign of Emperor Anastasius (AD 491–518) remained in circulation in Beirut for several decades.

2 Merrony, 2002, II, 411, No.7/16.

3 Merrony, 2002, II, 360–1, No.3/1b.

4 Special thanks are due to the generous time and precious advice given in November 2000 and August 2001 by Dr Derek Goldrei, Senior Lecturer in Mathematics at the Open University and at Somerville College, University of Oxford. For a full account of the 'Counting Method' devised, Merrony 2002, I, 43–44.

5 Dauphin (1998, II, Chs IX-XI) has argued that seventh-century decline was due to cumulative causes, notably the famines and plagues of the sixth century.

6 Merrony, 2002, 541–2, No.11/8/23.

7 Merrony, 2002, 546–7, No.11/8/33.

8 Non-representational mosaic floor decoration was first codified by Avi-Yonah (1932, 136–81; 1933, 26–72; 1934, 187–93), continued and extended by Ovadiah and Ovadiah (1987), and comprehensively standardised by the AIEMA (1985).

9 Merrony II 2002, 404–6, No.7/5b.

10 Merrony II 2002, 413–4, No.7/19.

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Review Article

Rural Landscape, Settlement Archaeology and Political Ideology

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Maier, A.M., Dar, S. and Safrai, S. (eds.), *The Rural Landscape of Ancient Israel*. BAR International Series 1121, Archaeopress, Oxford, 2003. Pp. 158, 49 ills. (line drawings and photographs). Price: £ 31.00.

As explained by the editors in the 'Foreword' (and unnecessarily repeated in their 'Introduction: The Rural Landscape of Ancient Israel'), this volume is an offshoot of a collection of studies in Hebrew on *The Village in Ancient Israel* (Dar and Safrai 1997 eds). Since none of the contributions are rehashed English versions, this is a new product, with a wider scope and precise goals. The ten contributors were requested to synthetize general phenomena relating to rural (non-urban) life (as opposed to solely village life) between the late prehistoric and Modern periods, and to focus on Physical Makeup (architecture, village planning, inter-village installations), Rural Settlement Patterns and Processes, and Rural Economic Activities (agriculture, production and trade).

'Rural Landscape' is broadly defined by the editors (p. iii) as 'the rural surroundings of human culture..., whether phenomena of natural or manmade (physical and/or ideological) origin'. The equation of 'ancient Israel' with 'the approximate region of the modern-day State of Israel' in the 'pre-modern periods' (blurring the contours, but in fact encompassing all the Territories Occupied since 1967), is not only nonsensical both from geo-historical and ethno-religious ('biblical') perspectives, but clearly consequently uncomfortable both for the editors themselves who predominantly use the phrase 'ancient Palestine' or the term 'Palestine' (pp. ii and iv), and for some of the contributors (R. Greenberg, M. Haiman and W. Dever – the latter, moreover, using 'Palestine' as shorthand for Palestine and Transjordan!). Z. Safrai's use of the phrase 'Land of Israel' is justified insofar that his discussion is based primarily on the Rabbinical sources ('The Agrarian Structure in Palestine in the Time of the Second Temple, Mishnah, and Talmud'), whereas Z. Amar's mantra-like obsessive repetition of 'Land of Israel' (28 times in the course of eight pages) in his 'Transformations in the Agriculture

of al-Sham during the Mamluk period (1250–1517 CE)', coupled with his definition of al-Sham as 'Israel and Syria' (p. 149), are both historically incorrect and absurd. Based on the Themes created by the Byzantine Emperor Heraclius as part of his reorganization of Syria and Palestine (Hitti 1949: 154; Shahid 1986), the four military *junūd* of Bilād al-*Sh*a'm included the *jund* of al-Filasṭīn, with whose agriculture under Mamluk rule Amar is concerned. 'Israel' is ridiculously anachronistic here and its use out of context testifies to continued pointless linguistic revisionism.

The volume opens on a definition of Landscape Archaeology, a discussion of its methods and their application in projects conducted by S. Gibson at Sataf in the Jerusalem Hills, the Golan Heights, Dor and Modi'in ('From Wildscape to Landscape: Landscape Archaeology in the Southern Levant – Methods and Practice'). Since Gibson's article embodies the only attempt (besides 'Street Villages and Rural Estate Centers: the Organization of Rural Settlement in the Latin Kingdom of Jerusalem' by A.J. Boas) to retrieve and recreate for the reader rural landscapes in keeping with the title and aim of this book, the accusation of blowing his own trumpet which is levelled at him by the Editors in their 'Introduction' (p. iii: 'There is though one aspect of Gibson's paper that we would like to qualify. From reading his contribution, one can get the impression that prior to recent times, and in fact, save for his own studies, little, if any "Landscape Archaeology" was conducted in the Land of Israel. This is far from the truth.') is offensive, gratuitous, wrong and misplaced. Basic publishing courtesy required the Editors to warn Gibson of acute disagreement before putting the volume to press, thus allowing for discussion, explanations and consequent possible amendments to his original text. To keep quiet and spring in print harsh criticism on an invited author is tantamount to hitting him under the belt – a particularly despicable form of ensuring an audience before a kill. The examples cited by the Editors as evidence that 'from very early stages of the archaeological research of the land, and continuing to the present-day research, many scholars have chosen field methodologies and interpretative approaches that, even if not dubbed as such, are in fact "Landscape Archaeology", or at the very least, methodologically comparable' do not suffice (curiously, except for Dar 1986, all the scholars brandished as proof have all been omitted from the bibliography appended to the 'Introduction'). For all their interest in the relationship between man and nature, neither the soil expert Reifenberg, nor the geographers Kedar and Evenari who studied ancient agriculture in desert environments, pursued projects of 'Landscape Archaeology'. No-one, least of all Gibson (or I who witnessed in 1988 the first applications of that British-born-and-bred discipline in the Southern Levant at Sataf and soon after within the framework of my Golan Byzantine Expedition), would dispute that, following his field surveys in Samaria and in the Hermon in the early 1980s and the discovery of rural and cultic features, S. Dar (1986; 1993) was the first to piece together regional patterns of settlement against their historical background. At Sumaqa, on Mount Carmel, he inaugurated for the Roman and Byzantine periods an approach which combined with the excavation of the settlement, survey of the agricultural and industrial features

associated with this village in the surrounding countryside, and even an analysis of the diet of the villagers (Dar 1988–9; 1999). It is thus all the more disappointing that he has not provided (inexplicably) any offering to the volume under review of which he is one of the three editors. Nor should it be forgotten that long before the 1987–89 excavations at Manahat in the Rephaim Valley (Edelstein, Milevski and Aurant 1998), G. Edelstein was linking together settlements, terraces and stone mounds in a holistic approach to the rural landscape of Jerusalem (Edelstein, Gat and Gibson 1983). But neither Dar nor Edelstein were engaged in 'real' Landscape Archaeology which, for all its flexibility is not an umbrella-term for a hotch-potch of approaches to ancient environments, but – as demonstrated by Gibson in this volume – is a specific 'method for studying the development through time and space of a continuous distribution of large and small man-made features across a given landscape', 'a form of "total archaeology" – settlement patterns, field systems, territories and communications' which 'differs from the conventional concept of "off-site" features spread between occupation sites' (pp. 1–2). One of the major successes of Landscape Archaeology in Israel was Gibson's discovery at Sataf of a dispersed EBI settlement (early 3rd millenium BC) buried under later agricultural terraces. Such 'disappearance' of scattered remains (as opposed to traces of nucleated settlements which are more easily detectible) may explain why so few EBI sites have been found during surveys in the terraced highland zone, this signalling caution in the interpretation of settlement patterns in the central highlands in the protohistoric and early historic periods.

Gibson's article being the only diachronic contribution, the nine other studies are arranged chronologically, the Bronze and Iron Ages seemingly being periods of predilection for model-making and refuting which constitute the hard core of this book. R. Greenberg ('Discontinuities in Rural Settlement in Early Bronze Age-Middle Bronze Age I Palestine') fully implements his avowed aim: to 'put to a more rigorous test' the models for the EB-MB transition which oppose the theoreticians of the 'Arizona School' (Dever 1989 and 1995; Joffe 1991 and 1993; Palumbo 1991; Falconer 1994), and those of the 'Tel Aviv School' (Portugali and Gophna 1993; Portugali 1994; Finkelstein 1995; Finkelstein and Ussishkin 2000). Aware of the flaws on both sides (since the models put forward are based on fragmentary evidence from various parts of Palestine blended together to create uniformity), Greenberg has opted more modestly for regional analysis and selected the 200 km^2 Hula Valley, fertile, well watered, comprising two major *tels* – Hazor and Laish/Dan -, with important routes connecting Syria and Palestine either crossing it or in its vicinity. Greenberg's argument, however, that 'while the environment ... conducive to settlement...could support a dense population, its degradation by human agents could rapidly transform it into a marginal area' makes it 'a sensitive barometer of settlement trends in Palestine as a whole' (p. 29), is untenable – which he admits himself at the end of his article (p. 33: '...if not a paradigm for the settlement history of Palestine as a whole, the Hula Valley evidence should at least be seen as an expression of an alternative mode of development, and an antidote to facile generalizations'). Population distribution through time depends on climate and

natural environment, in particular hydrography, but a direct correspondence also links settlement density to fertility of soil. Thus, every region of Palestine in antiquity offered its own particular agricultural and demographic landscape, as we have amply demonstrated using conjointly several 'ciphering grids' (physical geography, pedology, hydrography and climate) to analyze in detail the regional distribution of 2,930 Byzantine sites in Palestine (Dauphin 1998, II: Ch. IV and Figs 38–39; 1999a: 79–82). Only such an approach (a variant of Landscape Archaeology conceived and applied by us to Byzantine Historical Geography) enables one to follow the rhythm of growth particular to each region and progress from a microanalysis to a global view. Greenberg's conclusions that 'each region studied will show its own distinctive settlement history within the larger trend' and that 'rural life is dynamic, not static' (p. 33) – hence our sets of settlement distribution maps per region and per century and those indicating religious demography (Dauphin 1998, II: Figs 40–107; 1999a: 84 and Figs 4–7) – show that he is reaching out unwittingly towards the new discipline which we have dubbed 'Archaeological Demography'. Tracing demographic dynamics requires 'borrowing the most appropriate analytical tools from the range offered by Landscape Archaeology, Physical geography, Anthropology, Religious Sociology, and Social and Economic History, and...testing itself painstakingly at every stage' (Dauphin 1999a: 89). Perhaps applying (with modifications and refinements) such an approach to EB-MB Palestine would ultimately cut a 'third way' out of the rut of speculation: Greenberg is already firmly set in that beneficial direction.

Taking a firm stand against the view that few, even none, of the EB walled settlements qualified as towns (Falconer 1994: 312) and that all MBII sites in the Jordan Valley were villages (Falconer 1997a; 1997b), A.M. Maier ('Does Size Count? Urban and Cultic Perspectives on the Rural Landscape during the Middle Bronze II') turns the implications of this volume's title on their head and deliberately focuses on the interaction of rural sites with the urban centres. Grounded in the results of an extensive survey and in-depth study of the Central Jordan Valley, particularly in the MBII (Maier 1997), his demonstration is all the more persuasive for being short and snappy. Against size as a sole criterion for the definition of cities (that the average size of Mesopotamian urban units was much larger than their Southern Levant counterparts does not imply that only sites in the Southern Levant of similar extent to the Mesopotamian cities should be considered urban), Maier puts forward functionality in relation to the surroundings, as well as social stratification and specialization, whilst stressing that urban and rural modes of existence should be viewed as complementary parts of a matrix. A distribution map of Central Jordan Valley MBII settlements hierarchically graded according to function (not size) would have given a visual, 'human' dimension to an academic scoring of points. The 'reality' of the archaeological settlement pattern viewed within its pedological and hydrographic environment would have also probably dispelled the unease created by an apparent lip-service paid to central-place theory and its offshoots. While it is true, as mentioned by Maier (p. 62), that 'any classic settlement pattern' (Haggett 1965; Clarke 1968: 505–11; Hodder and Orton 1976)

combines upper-echelon sites of various sizes that play 'central', regional role(s) alongside smaller sites fulfilling a variety of subsidiary roles, ancient landscapes being organic and dynamic defy rigid systems imposed upon them wholesale. Regular, recurrent patterns as proposed by Christaller (1966) rarely tally with the archaeological data, so that after an excited flurry of attempts, British archaeologists had abandoned central-place theory and other fixed models by the early 1980s in order to observe, record and understand the interrelations actually etched in the landscapes. Maier's tracing of the cultic and cultural dynamics of the Central Jordan Valley in the MBII by observing the regional distribution of hierarchically-graded cultic remains leads him to draw a 'sacred landscape' as proposed by Alcock (1993), whose components mirror the urban/rural functional division and which was dominated by 'élite' urban entities. However, unlike Ancient Greece whose 'sacred landscape' can be recreated by collating the archaeological and literary sources, as illustrated remarkably by Dillon's work on pilgrimage (1997), the MBII Southern Levant is devoid of written documentation acting as 'safety nets' for constructs of heaped-up hypotheses drawing on 'disparate cultures' (as Maier admits), which however splendid their internal logic, may crash if one of the 'links' is severed by conflicting future discoveries.

W.G. Dever ('The Rural Landscape of Palestine in the Early Bronze IV Period') pursues an even trickier project: to recreate the 'mental map' of the population of Palestine in the EBIV (*ca* 2300–2000 BC), a reportedly disjunctive episode during which the urban occupation of the EBI-III era (*ca* 3400–2300 BC) came to a complete halt and was replaced by hundreds of small, seasonal pastoral-nomadic encampments, many in the marginal, semi-arid hinterlands of the Central Negev, the Jordan Valley and Southern Transjordan. A few permanent agricultural and agro-pastoral villages remained from the EB, and others were new foundations in the Palestinian heartland. After modifying his original 'pastoral-nomadic' model (Dever 1971; 1973) to embrace a socio-economic system of mixed agro-pastoralism and 'ruralism', Dever (1995) has now espoused 'post-processual archaeology'. Claiming that not only behaviour may be 'read' from artefacts acting as 'encoded messages' from the past – hence its other name 'contextual archaeology' -, but also the thought that produced this behaviour – hence 'cognitive archaeology' (Hodder 1986), it is the Anglo-Saxon adaptation to archaeology of the 'Histoire des Mentalités' which developed in France in the early 1970s out of the lessons of the Ecole des Annales (Le Goff and Nora 1974). Aware of the 'pitfalls of embridled, subjective speculation' and of the limitations of 'ethnographic observations' and 'arguments drawn from analogy', when attempting ideological rather than simple descriptive explanations, Dever nevertheless has the courage to address two fundamental questions: 'Why did the EB folk by and large deliberately avoid the prime locations that had attracted intensive occupation throughout Early Bronze I-III (a period of at least a thousand years), i.e., those sites with optimum soil and water resources, as well as defensible positions?', and, '...is there at work in the EBIV shift in settlement patterns a cultural concept of the "ideal landscape"; and if so, what factors determined it?'. The answers, he suggests, lie in 'cultural

constraints that would tend to influence the concept of the ideal environment or "landscape"': the pastoral nomads' preference for a peripatetic existence, the high value they attribute to freedom of movement, their pride in economic independence and the tribal system enshrined in the 'desert code' (pp. 46–47). Long before Rowton (1977), Gertrude Bell the Arabist, traveller, Honorary Director of Antiquities for Iraq and founder of the National Museum of Iraq (Winstone 1980: 243), who, rivalled only by T.E. Lawrence, remains unsurpassed in her profound understanding of the Arab mentality, had viewed perceptively the 'Desert and the Sown' – a phrase wrongly attributed by Dever to Reifenberg (1947), but which Bell had borrowed from 'Omar Khayyām ('The strip of herbage strown that just divides the desert from the sown') and chosen for the subtitle of her 1907 book of travels in Syria, Palestine and Jordan (Kamm 1956: 113) -, engaged not in an 'unremitting struggle'[1], but twinned as the two aspects of the same society. Arguing for the 'oscillation between the two poles over long time-periods' in Levantine societies, 'first one then the other prevailing', Dever conclusively interprets the EBIV as 'a cycle in which the rural-pastoral morpheme predominated' as the result of deliberate cultural choice, until the MBA urban renaissance (*ca* 2000 BC). What caused these dramatic 'ideological' shifts remains unexplained, but as Dever rightly points out, ethnographic analogy has its limitations (p. 49).

On the Iron Age landscape of the Negev Highlands, M. Haiman ('The 10th century B.C. Settlement of the Negev Highlands and Iron Age Rural Palestine') has – against all expectations (and the sedentarisation of local nomads as posited notably by Finkelstein 1984) – read a fascinating phenomenon: the transfer in the late eleventh century BC to the Negev Highlands of a settlement system by a population migrating from Northern Palestine at the instigation of the Israelite United Monarchy. The raw data was culled by Haiman between 1979 and 1990 within the framework of the Negev Emergency Survey: 350 sites of which 80% on mountain ridges and slopes affording a wide panoramic view. The majority (220) were dispersed settlements comprising casemate fortresses and square towers (many free standing), permanent dwellings consisting of 1–4 elongated rectangular rooms and columns near water cisterns (Group I), courtyard houses (Group II), and, rectangular, round, oval and horseshoe huts used for seasonal agriculture or animal husbandry (Group III). Although only some fortresses and associated neighbouring settlements formed units, as at Atar ha-Roeh and Ḥorvat Ramat Boqer (Cohen 1981; 1985), almost all the sites were scattered within the general distribution area of the fortresses and within their viewing range, but far from natural water sources. Haiman emphasizes that the distribution of sites in the inhospitable Central Negev Highlands, more than 10 km from a water source is unique to the IA in contrast to the EBII and MBI whose permanent sites were built solely in proximity of water sources (Haiman 1989: 179–83). Moreover, even with rock-cut cisterns and open reservoirs (with an evaporation rate of 2.50 m per year) and despite considerable evidence for agriculture (sickle blades, silos and theshing floors) and pastoralism (360 pens for sheep and goat), this area which had enough arable land (large-scale terracing of *wādīs* postdates the IA), but an average annual rainfall of 80 mm and

frequent droughts, could not sustain for long a permanent population of 1,500 for the settlements, an unspecified transient number in the fortresses, and 6,500 animals in pens (calculated by Haiman on the basis of living space for humans and surface area of pens for animals). Thus, 'other considerations, not connected with the exploitation of environmental conditions for subsistence, caused this population of farmers and shepherds to migrate into the desert' (p. 74). The Negev Highlands settlement is dated by its rich pottery assemblage (Cohen 1986: 363–84, 410–35) and references to it in Pharaoh Shishak's list of 80 site names (Mazar 1957) to between the late eleventh century BC and 925 BC. Haiman's demonstration that the new type of '*Ḥaṣerim*' rural settlement type (Gophna and Singer-Avitz 1984) which emerged at the end of the eleventh century BC and comprised a small group of buildings linked to a fortress with relatively thin walls, cisterns and silos (sometimes above sites from earlier stages of the IA), reflecting the socio-cultural changes accompanying the transition from a tribal society to a monarchy, was transferred to the Negev Highlands, and subsequently served as a model in the peripheral regions of Northern Palestine, is unfortunately jammed by confusing statements, such as 'on the basis of the similarity between the settlement in the Negev Highlands and that in the north, it is suggested that the Negev settlements are an expansion from the north. Although most of the examples of rural settlements presented below are later than the Negev settlements, they may be viewed as providing a relevant typological perspective' (p. 75). Haiman's hypothesis would have gained in clarity (even if it smacked of simplification) if he had tentatively traced the migration in the late eleventh century BC from Lower Galilee, Tell el-Ful, Khirbet ed-Dawwara and 'Izbet Sarta, through Tel Masos, to the Negev Highlands, followed by the 'reflux' of the site-type to eighth–sixth century BC sites in the Northern Negev, the Judaean Desert and Hills, and Samaria. His suggestion that 'the settlement in the heart of a desert, virtually lacking in sources of livelihood, was initiated and supported by the Israelite United Monarchy to protect the interests of the state in response to geopolitical conditions that threatened its southern border' is breathtaking. Since the fortified frontier settlements 'could not have supported themselves under the prevailing environmental conditions, they could exist only as long as they furthered the interests of the state. When the geopolitical conditions changed following the division of the United Monarchy of Israel and Shishak's campaign, settlement in the Negev Highlands ceased and was not renewed. Once the region lost its geopolitical significance, there was no justification to support settlement in the desert, and the border of Judah was pushed north, to the Beersheba Valley' (p. 77). Not only is this archaeo-historical reconstruction ingenious, but also the amazing forerunner of modern events (the Jewish colonization south of the Gaza Strip and the 1980 pull-out from Yamit) – another proof perhaps of repetitive historical patterns.

A. Faust has chosen to focus on a little-studied but essential rural unit: 'The Farmstead in the Highlands of Iron Age II Israel'. Basing his analysis on the results of recent surveys and the excavations of several farms in the Hebron Mountains, the Jerusalem and Benjamin areas, in Samaria and on the western slopes of the

Samaria Hills, Faust differentiates between farmsteads defined as 'isolated structures located in the midst of agricultural areas', their inhabitants being 'the people who cultivate and use the land', and estates which 'extend over large areas and belong to the wealthy', residents besides the owner including servants, hired labour and slaves (p. 91). In a linguistic excursus in the middle of his article, he argues that the Biblical '*ir* should be translated as 'settlement' and not 'city (or town)', and that besides 'courtyard' (hence *haṣerim* as 'courtyard-houses' in Haiman's contribution), *haṣer* meant 'farmstead' and not 'village'. Grouped according to the above-mentioned regions, 19 sites are described in a Gazetteer. Domestic and communal winepresses in vineyards attest to wine production as a major activity of farmsteads, oil presses indicating oil production, but on a lesser scale. After remarking that 'grazing does not leave any clear traces' and that threshing floors which would indicate cereals are not mentioned by excavators or surveyors, Faust affirms that 'it is highly likely that these activities took place on the farms' (p. 95). The distribution of IAII farmsteads on the soil maps of Israel (Dan and Raz 1970; Dan, Yaalon, Koyumdjisky and Raz 1976) would have immediately instructed him as to the soil potential of each farmstead. For instance, Terrae Rossae (Type A soils) are particularly suited to the Mediterranean trilogy of cereals, olive and vine as well as fruit trees (apricot, apple and plum). Requiring little or no water, olive and vine also prospered on Brown and Pale Rendzinae (Type B soils) which are not rich but easily cultivated soils. The difficulties inherent in identifying the plot(s) of land belonging to a farmstead and in assessing the extent of its agricultural land, have caused conflicting estimates, partly due to the widely differing standards of recording and thus inevitable subjectivity of the surveyors conducting projects for the Archaeological Survey of Israel (Dauphin 1998, I: 50; Gibson in the present volume under review, p. 11, n. 6). Conducting a project of Landscape Archaeology on the western slopes of Samaria with total pedestrian coverage, 19th century topographical and British Mandate period cadastral maps, as well as aerial photographs, would test the range of probabilities suggested by Faust ('no-man's land' and inhabited land suitable for grazing), and confirm or knock down his hypotheses construed from an accumulation of variables (p. 97). Although Faust repeatedly hides behind scarcity of data so that final conclusions should await further information (pp. 98 and 100), he asserts that 'it is quite clear that most of the farmsteads participated in a market economy', surpluses (notably of wine, evidenced by the large quantities of storage jars found at Kh. er-Ras 2, Nahal Zimra and Pisgat Ze'ev) being sold or bartered. He even qualifies the economy in which participated the farmsteads of the Jerusalem area as '*relatively* open' (my italics C.D.). Applying modern economic concepts to antiquity is not only facile, but wrong, as authoritatively demonstrated by Finley (1973), Godelier (1985), Polani and Arensberg (1957; 1985). The masterly presentation by Grégoire and Renger (1988–1989; also, Renger 2003) of the concept of Oikos, as detected notably in Mesopotamia (Grégoire 1981: 69–75) and at Ebla (Grégoire and Renger 1988–1989: 218–24), and 'Talmudic' economy viewed by Ben-David (1974) serve as approaches to be emulated and as exemplary socio-economic thought rooted in

and growing from the data itself. The twists and turns of Faust's discussion of the connections between farmhouse size and number of inhabitants, and of the number of rooms as well as of fluctuating internal divisions, the exceptions that must be taken into account, the stringing together of possibilities, likelihoods and probabilities, all indicate the discomfort unconsciously felt by Faust in chaining himself to the unquestioned adoption by Safrai (1998) of the concepts of 'nuclear' and 'extended' families into which moulds Faust forcibly pushes his archaeological evidence[2]. All of us archaeologists of the Near-East would gain much in finally abandoning a slavish adherence to these oft-repeated concepts developed from anthropology, and in rethinking systems of kinship on analogy with societies nearer to that under scrutiny in terms of time and space (Grégoire 1981: 75–82), this forcing us to redefine and refine basic concepts, or create new ones. From a socio-economic overview, Faust proceeds to a regional examination of the distribution of IAII farmsteads. Unlike farmsteads in Israel, Judah and the western slopes of Samaria which formed enclosed complexes, those around Jerusalem lay, unwalled, in the agricultural landscape. Faust suggests that the high density of settlement (farmsteads, villages and towns) in the vicinity of Jerusalem which had expanded significantly in size and population in the eighth–seventh centuries BC, reduced the threat of thieves and robbers (or reflected a centralized controlled situation).The important number of villages in Samaria as opposed to their rarity in Judah, combined with the predominance of farmsteads in Judah as opposed to one only in Israel, is interpreted by Faust according to two criteria: demography and security problems – two out of several possibilities aired by Safrai (1998) to explain the decrease in the number of isolated structures in the course of the Roman period. The Kingdom of Israel being much more developed and densely populated than the Kingdom of Judah, 'all available land in Israel was used, the population grew and farmsteads became villages' (p. 98) is the more sober option healthily preferred by Faust. The alternative explanation (deemed 'not reasonable' by Faust) disturbingly projects into the past a major obsession of the present, its wording smacking of modern political discourse relayed by a news bulletin: 'Lack of security might have caused the inhabitants to prefer a settlement form of nucleated villages. However, we have no indication of security problems in the Samaria region. On the contrary, the Hebron Mountains are expected to be more problematic in this respect' (p. 98). On the western slopes of Samaria – ecologically the worst area in the entire region – farmsteads were substantially smaller and devoid of four-room houses considered by many archaeologists to be characteristic of the Israelites. Refuting the suggestions that this area was settled by refugees from the heart of Samaria after its destruction (Finkelstein 1981), that it was connected to the population expansion in the eighth century BC (Eitam 1992: 177), or to the early settlement of the Israelite tribes (Dar 1982: 59), Faust suggests three possibilties. The farmsteads on the western slopes of the Samaria Hills were part of and supplied some of the agricultural needs of the hinterland of the Coastal Plain which flourished after the Assyrian conquest, and had given it great importance as a route to Egypt. Alternatively, whilst the western slopes of Samaria became part of the Coastal Plain

hinterland, some of its population originated from elsewhere in Samaria. Finally, the unique features of the farmsteads of the Western slopes may have been due to a population that had been exiled from Mesopotamia by the Assyrians, although a significant portion was local. The latter hypothesis concords with the results of recent exegetic research on the demography of the Exile (Gonçalves 1999).

Whether Pagan, Jewish, Samaritan or Christian, Roman and Byzantine society was legally-minded and heavily codified. Z. Safrai ('The Agrarian Structure in Palestine in the Time of the Second Temple, Mishnah and Talmud') explores the legislation pertaining to the ownership of land and water (although, once mentioned, the issue of water is never addressed) and the way it shaped the rural landscape of Palestine, with its variety of landholdings (private plots, family property, Temple Lands, Royal Estates, Public lands). Although his interest lies with the agricultural dimension of Jewish society as determined by Rabbinical law, the awareness of the rich religious demographic diversity of Palestine in the Hellenistic, Roman and Byzantine periods has compelled him to include the land which was part of city (*polis*) territories, land owned by the Legion[3] and that of the *Limes*. From the onset Safrai admits that the Rabbinical rulings pertaining to land may have been utopian concepts, that were not uniformly applied in actual fact, and are not to be assumed' (p. 105). His discourse therefore confusingly oscillates between an ideological Land of Israel where daily economic life would have functioned autarkically, regulated by rigorous observance of the Torah, 'a creation of God – and of rabbinic fantasy' (Barth 1982: 514), and the reality of an Ereṣ Israel which following the destruction of the Temple in Jerusalem in AD 70 was reduced to the Galilee, the Golan, the *Darom* and increasingly smaller pockets of Jewish population[4]. In order to trace survival of biblical injunctions at the time of the compilation of the Mishna, Tosephta and Talmuds, Safrai has judiciously chosen as markers the Jubilee Year (which following Lev. 25: 8–12 cancels land sales and demands that every fifty years the land return to its original owners) and the Sabbatical Year (whereby, according to Ex. 23: 11, fields had to lie fallow). Despite being alluded to in one of the two Hebrew inscriptions on the fifth-century AD mosaic pavement in the western hall of the synagogue at Ein-Gedi as part of a computing system according to Gutman, Yeivin and Netzer (1981: 124), and thus being vividly present in collective memory, the Jubilee Year was most probably not practised beyond the early Second Temple period[5]. The Sabbatical Year (*shebi'it*) was adhered to, despite severe hardship owing to the burden of taxation which intensified under Byzantine rule. So was the *ḥallah* – the portion of dough which had to be set aside for the priests according to Num. 15: 19–20, or, since Titus' destruction of the Temple, thrown into the fire – which, twinned with the *shebi'it*, we have used as a marker in our appraisal of the application of regulations pertaining to 'Forbidden Foods' (Lev. 11 and Deut. 14: 3–21) in the Byzantine 'Promised Land' and exploration of their links with Land as a territorial concept (Dauphin 2002).

Safrai illustrates well the passage from family ownership of land, including threshing floors – noting that beyond siblings, the 'family' included relatives, the term 'brother' in Aramaic meaning concurrently blood-brother, half-brother,

husband, uncle, nephew, cousin, friend, and companion (Puech 2003: 48) – to private ownership, except for tombs which could be neither moved from place to place nor transferred from one family to another (p. 109). Predominance of private landed property (plots and estates) in Roman Judaea is well attested by a vast number of deeds of sale and rent contracts, notably in the Babatha Archive (Cotton and Yardeni 1997). A glaring omission is A. Ben-David's impressively documented and clearly-argued *Talmudische Öikonomie* (1974), thankfully devoid of the socio-economic anachronisms such as 'capitalistic' and 'socialist' (intimately bound up with the Industrial Revolution) favoured by Safrai for purposes of comparison, which would have provided a remarkable backcloth to Safrai's study which unfortunately lacks in archaeological examples. The 'urban' evolution of Farj in the Golan which we traced in our analysis of architectural stratigraphy based on masonry typology (Dauphin and Schonfield 1983; Dauphin 1984: 239–41), revealed the breakup of the *latifundium* into small properties – an evolution detected in the limestone Belus region of Northern Syria, in particular at Beḥyo, by Tchalenko (1953,I: 343–73; II: Pl. LIX) -, which illustrates the transformation of a settlement belonging to a single owner into one with many owners mentioned by the Mishnah (Erubin 5: 6; Danby 1933: 128).

Safrai paints, however, a bizarre rural picture of Christian Byzantine Palestine exclusively dominated by the Church (including the monasteries). His data is at best selective, curiously ignoring the results of recent excavations of estates, notably at Ramat ha-Nadiv (Hirschfeld 2000), and is patchy. Generalizations abound: p. 117: 'Many monasteries in the land, as well as other monasteries in the Byzantine world, were economic entities. These monasteries generally contained oil presses and wine presses, which attest to the large-scale production of oil and wine... In all of them the monks themselves engaged in agriculture, and the monastery functioned as an autonomous agricultural settlement'. Some comments are baffling and unreferenced: 'The books of responsa by the monks in the south contain various queries pertaining to church and monastery property, commerce and the transport of goods, as a regular commercial body': is Safrai alluding to the 840 letters of Barsanaphius and John the 'Prophet' who lived in the sixth century in total seclusion in the Monastery of Seridos in the hinterland of Gaza (Regnault 1966)? At worst, he is completely wrong. As oft-repeated by us in print in negation of a mistaken title in Hebrew ('Shelomi: a Byzantine Monastery Farm in Western Galilee') given by the Editor of *Qadmoniot* XII, No. 1 (45) (1979) instead of the original English ('ecclesiastical property'), we did not excavate a monastery at Shelomi, but a farm (correctly redubbed 'ecclesiastical' in the English version which was published later) belonging to an unidentified monastery in the region of Tyre (Dauphin 1993). In fact, the Shelomi farm illustrates to perfection the leasing of ecclesiastical property to lay tenants mentioned by Safrai (p. 114), but without providing any example. For all that the Byzantine emperor was God's representative on earth, the Byzantine State was not a theocracy. Acquaintance with the meticulous unravelling by Kaplan (1976) of the complex Byzantine legislation which regulated Church properties and those of the Crown, would have prevented Safrai from decontextualised assertions,

such as 'the Church did not *customarily* sell lands either' (p. 114 – my italics C.D.). Firstly, both 'Church' and 'property' should be qualified within the framework of Justinianic legislation. The preamble of *Novella* 7 of 15th April 535 listed the sole three types of ecclesistical establishments permitted to possess property: churches and oratories, charitable institutions, and monasteries (Schoell and Kroll 1895 III: 48–51; Kaplan 1976: 45). Ecclesiastical properties comprised real estate (buildings, fields and orchards), agricultural slaves and civil *annonae* (*Novella* 7, c. 1; Schoell and Kroll 1895 III: 51–52; Kaplan 1976: 46). Secondly, no alienation, sale, gift, exchange, or *emphyteosis* (a lease renewable every 25 or 29 years) in perpetuity (which would amount to quasi-alienation) was authorized, except between Emperor and Church (*Novella* 7, c. 1 and 2; Schoell and Kroll 1895 eds, III: 51–54; Kaplan 1976: 46–47). *Emphyteosis* of ecclesiastical property was permitted only when limited to three generations through son, daughter, nephew, niece, wife or husband. Land which had been returned to its ecclesiastical owner could be granted anew, but the owner was not compelled to do so. (*Novella* 7, c. 3; Schoell and Kroll 1895 eds, III: 54–56; Kaplan 1976: 47). At Shelomi, we have been very fortunate in tracing stratigraphically the emphyteotic life of the farm from its foundation in the late fifth century or early sixth century through three generations to its surrender to its monastic owner at the end of the sixth century, its emphyteotic leasing anew being associated with important structural changes (including the laying of polychrome mosaic pavements dated epigraphically to 610), its destruction by fire soon after, its abandonment, and its reoccupation in the late seventh or eighth century (Dauphin 1986: 46–50). Contrary to Safrai's categorical statement, Church lands were not exempt from taxation, as evidenced by *Novella* 17, c. 7,1 of 16th April 535 (Schoell and Kroll 1895 eds, III: 121–22; Kaplan 1976: 49). That ecclesiastical landed property was taxed was vividly illustrated at Shelomi by the discovery on the mosaic pavement of Room 1 of a unique iron rod, 2.95 m or 5 Roman cubits in length, used to measure land for the purpose of taxation (Dauphin 1982).

Since the mid-1950s, much research has been conducted on Byzantine rural history, both ecclesiastical and lay (notably Svoronos 1956; Lemerle 1958; Lipchits 1974: 57–58), culminating in M. Kaplan's remarkable study, *Les Hommes et la Terre à Byzance* (1992), which seems to have escaped Safrai's notice. By combining all available (excepting the Rabbinical) textual (historical, legal, hagiographic) and archaeological sources replaced in their environmental contexts, Kaplan has recreated across the Byzantine Empire (including Palestine) the rich patterns of rural landholdings, as well as traced the historical and socio-economic developments that modified them. Caused by the increasingly heavy burden of taxation shouldered by the multitude of free peasant families in the latter part of Justinian's reign, rural exodus resulted initially in the abandonment of privately-owned plots of land.[6] Subsequently their regrouping was cut short in Palestine by the Persian and Arab Conquests, but led in the Middle Byzantine period in the heart of the Empire – Asia Minor – to the resurgence of the 'great estate' and the 'big landowner'.

After the dryness of the desert and rocky areas which complete Safrai's review of types of land, 'Dogs in Ancient Rural Jewish Society' by J. Schwartz 'rehumanize' the landscape, although, described in biblical literature as 'settlement predators' that barked, scrounged for food and attacked, they were definitely not viewed as 'man's best friends'. This distaste was offset by reverence to dogs in Persian Zoroastrism (Wapnish and Hesse 1993: 71–72) and in the Greek healing cults of Apollo and his son Asclepius (Parker 1996: 182–83)[7], as well as by their role as pets in the Graeco-Roman world (Toynbee 1973: 71–72). Schwartz's review of the various types of dogs (sheep or herding dog, hunting dog, guard dog and *kufri*) mentioned in the ancient Jewish sources from the Bible through the Mishnah, Tosephta and Talmuds to the Midrashim, is thorough and enhanced by the use of Roman poetry and agricultural treatises, and by the Zoroastrian *Vandidat* for comparison. The discussion on the diet of dogs (p. 130) contains unexpected details: besides carrion, a bone or even a slaughtered animal, sheep dogs were fed a dough (barley flour with whey or bread made with bread-wheat mixed with the lukewarm liquid of boiled beans) which, if eaten also by the shepherd, had to conform to the rules and regulations attached to dough prepared for human consumption, such as the law of *ḥallah* (Mishnah Hallah 1: 8)!

The paucity of references to hunting dogs in the Jewish sources of the Second Temple, Mishnaic and Talmudic periods (Josephus, *Antiquitates Judaicae* 4.206, H.St. J. Thackeray 1930 ed.; the *Testament of Judah* 2: 2–6, J.H. Charlesworth 1983 ed.; and none in Talmudic literature), as well as of depictions of dogs in Jewish material culture (on a terracotta lamp and on the mosaic pavements of the synagogues at Beth She'an and Gaza) remains unexplained and baffling. Schwartz notes the popularity of the motif in non-Jewish art, citing a few examples of hunting dogs on mosaic pavements between the mid-third century and the sixth century (p. 131). The high incidence of depictions of dogs of various types – 32 instances, or 8.6% of 370 examples out of 1,340 scroll-fillers on 116 'Inhabited Scroll' mosaic pavements of the fourth–seventh centuries in Asia Minor and the Eastern Provinces of the Byzantine Empire; and six instances, or 10.7% of 56 animal depictions out of 206 scroll-fillers on 35 pieces of architectural sculpture (Dauphin 1978)[8] – vindicates a comprehensive iconographic study which would provide a visual dimension to Schwartz's textual gleanings and perhaps some new insights into the role of dogs. The Midrashic tradition which describes a dog guarding a vineyard (p. 132) has a Christian iconographic counterpart: a dog dances before a piper sitting on a basket as part of a vintaging scene on the mosaic pavement of Room L in the Monastery of Lady Mary at Beth She'an dated by a Greek inscription to 553–554 or 568–569 (Fitzgerald 1939). The enigmatic *kufri* which rid houses of vermin and which it was forbidden by the Mishnah (*Kilayim* 1: 6) to mate with a fox, is described in detail towards its identification (pp. 133–34). Was it a small dog, a black dog, a type of jackal, the progeny of a dog and a vixen, or – as suggested by Schwartz – a hedgehog? Indigenous to Asia, with pointed ears similar to those of dogs, small, and with elongated, narrow muzzles and short legs like foxes (which

belong to the canine family), hedgehogs establish dens in burrows. When they roll themselves into a ball to protect themselves from predators, they resemble dogs asleep with their tails curled around them, like the dog lulled to sleep by a naked piper on the 'Inhabited acanthus scroll' mosaic pavement of the nave of the sixth century Church in Nahariya (Dauphin 1984 ed.: 75, Pl. XVa-b).

By skilfully weaving together the collection of taxes, the administration of justice and the cultivation of land as derived from Frankish historical sources, with the surveyed and excavated villages, castles, manor houses and farmhouses, A. J. Boas ('Street Villages and Rural Estate Centers: The Reorganization of Rural Settlement in the Latin Kingdom of Jerusalem') has succeeded in conjuring up the rural landscape of the Crusaders' Palestine (AD 1099–1291) and in peopling it. Boas dispels two popular tenets, that 'the countryside was almost solely the result of military activity, in the form of monumental fortresses built to defend the borders and roads', and that 'the Frankish population was located almost exclusively in the cities and that agricultural activity remained primarily in the hands of the local non-Frankish population' (p. 137). By putting an end to Muslim incursions into Frankish territory (the castles built in the mid-twelfth century in Gaza, Betgibelin-Beth Guvrin, Blanche Garde-Telle es-Safi and Ibelin-Yavneh aimed at surrounding and smothering Fatimid Ascalon, which finally fell to the Franks in 1153), castles erected in the agricultural zones enabled the establishment of villages in the shadow of their walls for whose inhabitants they provided refuge in times of danger, although they were but fortified towers, such as the Red Tower (Burj al-Ahmar) near Tulkarm excavated by D. Pringle (1986). Boas discloses their function as administrative centres for the collection of taxes (the annual land tax or *carragium*, the *portagium* or tax on the transport of grain to granaries and on the use of threshing floors, and the payment for pasture rights) and tithes in the form of produce (the dime paid by Muslim peasants to the Church and other tithes levied by the military orders). In the twelfth and thirteenth centuries, this role was fulfilled by manor houses which were of two types. Courtyard-buildings (pp. 140–41), of which only two have been excavated, at Har Hozevim (May 2000) and at Khirbet el-Lawza (Ellenblum, Rubin and Solar 1996), comprised the hall which served as the living quarters of the landlord or his representative (*locator* or *raicius*), farmyards, barrel-vaulted structures for storage and stabling (the Arabic term el-Babariyya by which these vaults are known, as at Burj Bardawil, 32 km north of Jerusalem on the way to Nablūs, derives from the Frankish term for cow-shed, *bovaria)*, and various installations for the *bannum* (a monopoly of the lord to construct mills and ovens which the peasants were compelled to use and for the use of which they had to pay). The second stage of the tower at ar-Ram, a building at Jifna (21 km north of Jerusalem), and the well-preserved building of Aqua Bella represent the other type of manorhouse: a rectangular, quadrangular structure enclosing a central courtyard (p. 142).

The segregation of the rural population into separate villages (*casalia*) of Eastern Christians, Muslims and Franks is illustrated by village types. Local villages consisted of the house of the *rais* (the village head), a church or mosque, simple

peasant dwellings, and communal cisterns, threshing floors, dovecotes, mills and ovens. In the forty years between the building in the 1130s and 1140s of the fortresses isolating Fatimid Ascalon and the loss of the hinterland following the Battle of Hattin in 1187, planned villages were established around Jerusalem, Acre and Tyre along the lines of the Western European *villesneuves*. These street-or string-villages (strikingly illustrated by Figs 9.1 and 9.2) consisted of a row of houses built on long, narrow plots on either side of a central, axial road. The difficulty of defending such settlements was amply compensated by the facility in parcelling out even plots. These villages also bespeak of a countryside whose internal security had been achieved by the mid-twelfth century. Only four are archaeologically attested: *Parva Mahumeria* (el-Qubeibeh), el-Kurum, *Magna Mahumeria* (el-Bira) and a village in *Vallis de Cursu* (Wadi el-Haramiyeh), all north of Jerusalem (p. 139). Isolated farms (*curtiles*) are mentioned as one of the three categories of rural settlements (p. 138), but dismissed as 'not a typical feature of the local countryside of the Near East in the Middle Ages' (p. 140). Does this imply that none have been found or even suspected?

Boas' clear description of the tightly-organized Crusader landscape farmed by Frankish *villani* (who unlike their Western counterparts were free settlers who could alienate their land and were not forced to serve in the lords' *demesnes*), and by local Christians and Muslims, lacks a map indicating the distribution of the various types of sites excavated or surveyed, as well as those mentioned by or surmised from the abundant textual sources. If also mapped, additional information on ownership (crown, barons, churches and monasteries, military orders and small landowners) derived from the historical sources would have produced a fascinating picture. Even more telling than a single map freezing the landscape of the Latin Kingdom of Jerusalem on the eve of the battle of Hattin, several maps showing the development of the Frankish hold over cities and countryside, would have been an invaluable visual asset enhancing Boas' illuminating study.

The collapse of the Latin Kingdom of Jerusalem in 1291 was a decisive turning point for agriculture in Palestine. According to Z. Amar ('Transformations in the Agriculture of al-Sham during the Mamluk period (1250–1517 CE'), the change for the worst under the Mamluks was due in the first instance to the growth of the *Iqta'* system of collecting revenues for paying the troops and for other state expenditures, which from being sporadic became systematic and predatory. By granting army officers the uncontrolled right to collect taxes on specific pieces of land, central government lost its grip both on the land and on the revenue. To compensate, the Mamluk regime created government monopolies over almost all basic crops, fixing the prices of products and giving the population no choice but to buy at the prescribed and often inflated prices. Vagueness plagues Amar's mention of fundamental socio-economic processes. The 'governing apparatus' which was a Byzantine legacy and by which the central regime controlled the countryside, crops, techniques and processing methods, maintenance, and water rights (p. 149) is not described. Nor is the fundamental 'process of supplanting the small land-holder class through increasing the involvement of the regime' on its

own landholdings (for instance the Sultan's) as well as 'in all the areas under its jurisdiction' (p. 150). Details of the process itself and by what methods 'dispossessing the peasants from their lands and gradually turning them into serfs' were achieved, are not provided. Such lack of explanations is particularly frustrating considering that Amar emphasizes the importance of the exploitation of the peasantry (together with lack of technological initiative) in the economic stagnation which rapidly set in under Mamluk rule. Amar admits however that new crops, such as the citrus fruits and eggplants were cultivated for the first time then, but only in the Jordan Valley (p. 153). Further economic decline is attributed by Amar to the increasing strenghtening of the *Waḳf* from the reign of Saladin until the Ottoman Empire. The *Waḳf* is given a rough and negative treatment, from which it emerges as staffed with a 'non-productive economic administration' which was 'open to corruption and moral pollution', Amar glossing over its beneficial role in ameliorating poverty and misery, in furthering learning and managing architectural gems – mosques, *madrasas* and *türbas* (Gibb and Kramers 1953 eds: 624–28, art. *Waḳf*). Moreover, the relation of such turpitude to the question of the decay of agriculture is not explained. Although Amar admits the importance of the impact of the Black Death in the mid-fourteenth century and of the sharp demographic decline in all countries of the Near East, as well as the Mongol invasion and the rise of the Ottoman Empire (which did not affect Bilād al-Sha'm directly), he reiterates that Mamluk policy was a decisive factor in the setback suffered by agriculture.

Amar's review of the various agricultural crops cultivated in Mamluk Palestine makes for fascinating reading with some surprising insights into familiar aspects of the vegetation. Amar plots the downward trend of the date industry from the thirteenth century, with the slow disappearance of the date palm from the general landscape until the sixteenth century when Ottoman archives recorded the import of dates to Ramla. Conversely, the sugar industry developed: unlike the date palm, sugar cane grows fast and its cultivation does not require trained manpower, so that gains are rapid and obtained without much effort. By the beginning of the sixteenth century, however, Palestine was unable to supply its own needs for sugar (let alone the Venetian and European sugar trades which had been based on the Levant) and was compelled to import it. A pattern similar to that of the date palm is observed for wine grapes (owing to the enforced Islamic prohibition of wine) and wheat. Olive cultivation in Samaria replaced that of grapes, but whereas olive oil and olive-oil soap had been exported from Nablūs to Egypt, Hijāz and Yemen in the first half of the Mamluk period, cheaper olive oil and soap were imported from Europe by Italian merchants in the fifteenth century. Cotton replaced grain as a cash crop and became the leading export product owing to increasing demand from Europe and the development of a well-organized marketing system in the hands of Venetian merchants. These purchased the cotton directly from the local peasants, prices fluctuating according to supply and demand. Cultivation of the white mulberry extended and intensified, particularly in the north bordering Phoenicia where the silkworm industry was operated by the Italians.

Amar's enlightening economic survey ends unexpectedly on an attempt to link

some of the changes in the crops cultivated to 'changes in the gastronomic tastes of consumers'. To our knowledge, this is the first time that changing patterns of diet are discussed in a volume predominantly devoted to archaeology as causes or effects of modifications in the choice of crops for cultivation. As a member of the British Diet Group (in antiquity), we cannot but applaud. The increased consumption of sugar and carobs resulted from the scarcity of date honey, rice became an alternative to bread and wheat-based gruel, and sesame (whose seeds were crushed for oil) was a crop better suited to 'a time when agriculture was in decline' (p. 156). Further research is necessary to support these hypotheses, but it is already evident that by linking together agriculture, trade and diet Amar offers a novel approach to solving some economic riddles.

It is thus particularly unfortunate that Amar's contribution suffers from inadequate editing or cursory proof-reading which did not 'catch' words probably left over from previous versions of the text. On p. 149: 'The beginning of the Ayyubid period represented was...'; p. 150: 'The apathy of the Mamluk regime...is reflected... in the its failure...'; p. 151: 'the development of agriculture in the al-Sham', 'the decline of agriculture in the al-Sham'; finally, on p. 152, the text suddenly breaks into bold print in the middle of a sentence. The computer is also to blame for the transformation of a Semitic diacritic sign into a small square (p. iv: ha□er). Otherwise, scattered across the volume there are spelling mistakes (p. 74: 'draught' for 'drought'; p. 108: the historian A. Vööbus has become Voeoebus) and the occasional contorted, barely intelligible sentence verging on gibberish, for instance in Faust's contribution, p. 101, n. 12: 'In any event, even if one claims, for whatever reason, that these structures (or some of them) were estates, he is freed from the above-mentioned constant (due to the possibility of space being consumed for 'display'), but he still refer to these houses as inhabiting a large number of individuals as an estate, by its nature, required a minimal number of workers in addition to the owner family (which was the one who could used the above-mentioned spaces)'. The rarity of maps has already been noted. Much too heavy inking has unfortunately spoilt the display of black-and-white photographs, especially those illustrating Gibson's surveys in the Golan and Modi'in, and some illustrations would have provided Maier's, Faust's, Safrai's, Schwartz's and Amar's articles with a visual dimension which they sorely lack. The importance of this volume is amply demonstrated by our detailed and lengthy critique (which should not be construed as solely 'critical'). Wide temporal coverage, thematic variety and wealth in models and ideas make it into a landmark taking stock of the state of research in Palestinian rural studies. This book is very much a 'child' of its time, 'child' because the archaeological/historical landscape has percolated through academic consciousness only too slowly and not widely enough in the last twenty-five years, maturity being still ahead, and 'of its time' because the academic 'language' already shows signs (there are several other instances than those cited above) of contamination by the modern Israeli political discourse. Or perhaps we archaeologists are to be blamed for unwittingly providing ideologues with patterns (notably settlement encircling) which we have read on the geo-historical palimpsest

and which strategists horrifyingly attempt to apply, forgetting that historical mirror-images are inevitably distorted by the imprint of accumulated experiences on the 'mental maps' detected by cognitive archaeology, so that similar methods of landscape manipulation for ideological purposes will not inevitably produce the same results. Caution should therefore be exercised to prevent archaeology from being appropriated by ideology, for danger lurks.

The Rural Landscape of Ancient Israel must be hailed as the first step towards a collaborative Rural History of Palestine from prehistory to the end of the Ottoman period (unfortunately lacking in this volume, despite the abundance of its historical, cadastral and census records and its largely untapped archaeological data) modelled perhaps on the inspirational three volumes of the *Histoire de la France rurale* (Duby and Wallon 1976).

Notes

1. Reifenberg (1947) decontextualised Bell's phrase and transformed its deep meaning in order to emphasize the inexorable encroachment of desert sands on agricultural land, a reversible process according to Zionist dogma which had already been put into action by the end of the British Mandate over Palestine.
2. Safrai, however, also integrates into the system of land ownership the 'large and undefined clan' (p. 109 of his contribution to the volume under review).
3. Contrary to Safrai's belief that 'the surveyors of Hauran and eastern Syria did not find proof of an orderly Roman land parceling, but rather of less orderly, earlier divisions of land', centuriation was recorded by Villeneuve (1985).
4. Dauphin (1998, I: Chapters VI, VII and VIII) graphically traces this decline illustrated by regional-chronological distribution maps.
5. The Lord's injunction, 'Ye shall not therefore oppress one another' (Lev. 25: 17) as both a reason for and the aim of the Jubilee Year, leads Safrai to attribute Judaism the monopoly of 'concern for the poor', 'unquestionably one of the components of this religion's uniqueness' (p. 106), forgetting that *caritas* is one of the Virtues of Christianity and *zakāt*, the alms-tax, fulfills one of the fundamental obligations of every Muslim.
6. A similar phenomenon is described by Safrai under the heading 'ownerless lands' (pp. 119–22) on the evidence of the Tosephta at the time of the two Jewish Revolts against Rome.
7. The faunal remains collected in the fill from the Graeco-Roman sanctuary of Apollo and Asclepius, upon whose ruins the episcopal basilica of Dor was erected in the late fifth century AD, were identified by Dr L. Kolska Horowitz (Zoology Department, The Hebrew University of Jerusalem) as bones of dogs, as well as of pigs and cattle – the latter two presumably the left-overs from sacrifice (Dauphin 1999: 420).
8. Several 'Inhabited Scroll' pavements discovered since our 1973 collecting of data should be added, as well as all other Hellenistic, Roman and Byzantine non-'Inhabited Scroll' pavements exhibiting dogs. Likewise for architectural sculpture.

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Book Review

Ehud Netzer, *Hasmonean and Herodian Palaces at Jericho: Final Reports of the 1973–1987 Excavations. Volume 1: Stratigraphy and Architecture*, Israel Exploration Society / Institute of Archaeology, The Hebrew University of Jerusalem, Jerusalem, 2001.

Ehud Netzer first struck the ground with his spade at Tulul Abu al-'Alayiq on the outskirts of Jericho on the last day of 1972. It probably did not dawn on him that mid-winter's day, thirty years ago, that his remarkable series of finds would compel him to devote 13 seasons of excavation there, spread over 15 years. The site of Tulul Abu al-'Alayiq (literally 'the mounds where barley grows') covers an area of 30 acres and contains the extensive remains of palatial buildings, water installations and gardens and its principal landmarks are the two artifical mounds straddling the Wadi Qelt. It was initially surveyed by Robinson and Smith in 1838 and the first exploratary excavations were made by Warren in 1868 (Warren 1876: 192–97). At the beginning of the twentieth century, Tulul Abu al-'Alayiq attracted the attention of Sellin and Watzinger (1909: 30–36), whose records were unfortunately lost during World War I. After a gap of 40 years, two American expeditions, one under Kelso and Baramki and the other led by Detweiler, uncovered some of the palatial buildings and an impressive sunken garden created by Herod (Kelso and Baramki 1955; Prichard 1958). Although their results provided tantalising glimpses of the monumental buildings there from the Second Temple period, it took a further 20 years before Netzer began his comprehensive investigation of the site. His efforts were richly rewarded by the unearthing of the most important Hasmonaean and Herodian palace complexes found hitherto.

While it has taken no less than 14 years for this initial volume to appear in print, Netzer has published several popular accounts of his excavations, so there are few new surprises, although it is satisfying seeing this level of detail in a single report. This volume is divided into three main parts. The first is a description of the remains of the Hasmonaean Winter Palace Complex and Herod's Second Palace, which substantially overlaps the former, the second deals with the structures associated with Herod's Third Palace. The final part, which is entitled 'synthesis', discusses the planning and suggested reconstructions of the various palaces, and includes a short chapter on the stratigraphy. These sections are preceded by a brief introduction outlining the history and suggested chronology of the site. The text is generously accompanied by 51 plans, including both remains on the ground and reconstructions, and there are also 480 in-place illustrations and 16 pages of colour plates at the back. This volume is the first in a series of Final Reports on the palaces at Jericho. The second will cover related sites in the Jericho area, including Tel as-Samarat to the north of the royal estate, which was the location of the Herodian hippodrome,

the palace-fortress of Kypros to the southwest, and the water systems serving Tulul Abu al-'Alayiq. The third volume will deal with the ceramic finds; additional volumes in this series are also planned.

Netzer's excavations have revealed that the site served as the principal winter retreat of the Hasmonaean kings and their Herodian successors. Sumptuous palatial buildings grew up amid a vast royal estate covering some 110 acres, which contained plantations of date palm and *opobalsamum* (Pliny, *Natural History*, xii 111; Strabo, xvi 2, 41; Stern 1974, 485–90). The *opobalsamum* bush yields an aromatic resin that was much prized in classical antiquity as a medicament and fragrance. From the material remains, we learn that the Hasmonaean complex developed in stages (pp. 1–7; 334–38). The original core was a rectangular building with a central courtyard, apparently built by John Hyrcanus I (134–104 BC). A pleasure complex followed, which primarily consisted of two large swimming pools overlooked by a pavilion. Column drums and blocks of Ionic entablature were found close by, which Netzer judges to have belonged to a peripteral colonnade enclosing the pavilion. At some stage, perhaps during a period of civil strife in the reign of Alexander Jannaeus (103–76 BC), the core building was built over with an elevated palace, surrounded by a moat. This built-up mass constitutes the 'Northern Tell'. Further additions included twin residences, each built around an internal courtyard in the East-Greek manner, assigned by Netzer to the reign of Queen Salome Alexandra (76–67 BC), which he conveniently connects with the rival Hasmonaean princes, Hyrcanus II and Aristobulus II (p. 5). The Hasmonaean palace complex also included several bathing facilities, some identified as ritual baths (*miqva'ot*). Notable decorative features found in the Hasmonaean complex include wall paintings simulating veined marble and stucco imitating isodomic drafted masonry from the core building (18, Ills. 12–13 and Pl. IV) and a crudely executed polychrome mosaic bearing simple geometric designs (101, Ill. 134 and Pl. VII) in a bathhouse connected with the pools complex.

The Hasmonaean palaces were largely destroyed a few decades later, perhaps by an earthquake in 31 BC, as Netzer has suggested. In any event, they were superseded by three sequential palaces, built by Herod the Great (37–4 BC). Pritchard (1958) reported the first of these, which was uncovered by Detweiler's team. Netzer focuses his attention on the other two Herodian palaces. Of these, the Third Palace, built on the edge of the Wadi, is distinctive from an architectural point of view. Unlike its predecessor, it takes as its source not the East Greek quadrangular arrangement, but a more open scheme, bearing the hallmarks of the Italian terraced villa. It possessed a Roman-style bathing suite built of cast concrete, faced with regular shaped stones cut to resemble brick, laid in a lozenge pattern, known as *opus reticulatum*. Associated with this palatial residence is an equally Italianate sunken garden on the other side of the wadi, enclosed by walls of concrete with an *opus reticulatum* facing. Closeby, there is a large pool, which was suitable for swimming and boating, and also an enigmatic circular building raised on an artificial mound, the 'Southern Tell,' which was possibly used as an *oecus* (banqueting chamber). The Romanising features of Herod's Third Palace, including fine *opus sectile* (coloured stone tiles

arranged in a pattern) floors, make it very likely that Italian master-craftsmen played a leading role in its design and construction. Netzer provides a detailed description of the remains of these palace buildings and offers credible reconstructions.

Netzer duly informs us in the concise preface that his team's appreciation of the vast assortment of buildings, which they were bringing to light, was a gradual process. 'During the first seasons we regarded the Hasmonean palace complex as consisting essentially of a main building and a large pool to its east. Over the years, it became evident that this complex was more intricate, both with regard to its structures and stages. We were able to distinguish seven stages extending from the time of John Hyrcanus I (or even his father Simeon) until the days of Mattathias Antigonus, the last of the Hasmonean kings. The architectural complex on both sides of Wadi Qelt was initially constructed as an annex to Herod's First Winter Palace (excavated in 1951 and regarded by Pritchard as a gymnasium) … Over the years remains dating to the reign of Herod … increased in number until ultimately it became clear to us that the site contained another Herodian palace complex (the Second Palace). Altogether there were three Herodian palaces, which eventually operated as a single entity' (p. XII). The sequence of seven building stages attributed to the Hasmonaeans must be regarded as tentative, because the stratigraphic evidence presented by Netzer is completely dependent on a decipherment of the structural remains which, on the face of it, relies on a degree of conjecture. It is strange, in view of the current state of archaeological methodology, that hardly any other finds – including pottery – are brought into consideration of the stratigraphy. It might be expected that the later volumes dealing with artefact finds will clarify the picture, but until then, Netzer's proposed building sequence should be treated with caution.

The generous provision of illustrations greatly assists the reader to comprehend the tangle of walls and other structural features that spread over the site. However, the user-friendliness of this volume is spoilt somewhat by the quirky locus reference system that has been adopted, which makes searches for their position on the plans an arduous and perplexing experience. For example, there are loci AB37 and A(B)37, which are totally distinct and even discussed in separate chapters of the report. The origin of this confusing system is that, early on, a single letter and number (e. g. A37) were used to define a locus, the alphabetic letter corresponding to a specific zone of the site. Later in the excavations, it was realised that this system of referencing was too coarse and that each zone needed to be broken down into smaller areas, each defined by a pair of alphabetic letters. At the same time, a locus that had been specified earlier using a single letter, was now relabelled with a second letter in brackets, so that its revised classification could be matched to its earlier one. For example, A37 was redesignated as A(B)37.

For this reviewer, a remarkable revelation in this volume is the elaborate series of water installations, which has important implications for the history of this branch of technology. A striking element is the large number of swimming pools and baths. Around 35 BC, Herod had the Hasmonaean crown prince and High Priest, Aristobulus, drowned in one of the larger pools (Josephus, *Jewish Antiquities*, xv

50–61; *Jewish War,* i 435–37). But we find a plethora of sophisticated features and techniques, even associated with the earlier construction phases. There are distribution and settling tanks (to separate out silt). There are open conduits and also enclosed pipes fashioned from rubble in a mortared casing, as well as lead pipes, able to withstand internal pressure and capable of siphoning water to elevated positions, including the fortified Hasmonaean palace (p. 33). Pools were lined with waterproof ash-lime plaster, similar in its properties to the Roman *pozzolana* concrete which sets hard even when immersed in water and used to spectacular effect in Herod's artificial harbour at Caesarea. Yet, here we find these engineering practices in a Hasmonaean context, in palace walls as well as in water installations (see p. 27) prior to Judaea falling under Roman domination, if Netzer's dating of them is correct.

Unfortunately, this volume contains its fair share of errors and gaps. The aerial view of the site shown in Figure III. 1 is from the northwest and not the southwest as stated in the caption. Illustration 98 shows a view of Structure AC14 from the west, not the east, as printed, while Ills. 131 and 132 have their captions the wrong way round. There are also a number of discrepancies between the text and the illustrations, but there is only space to mention a few representative instances here. Septic Tank A(B)215 is mentioned as shown on Plan 18 (p. 95), but it is to be found instead on Plan 14. Also, the range of elevations of the Western Garden given on page 92 does not tally with those indicated in Plan 17. There are omissions in the illustrations as, for example, sections and elevations are shown for the Garden Triclinium (AL94) but these are not labelled, which makes it difficult to work out their corresponding positions on the plan. Important information is lacking in places, which can be exasperating. Thus, it is not explicitly stated that the *opus reticulatum* blocks are cut stones rather than bricks, as is often the case in the Roman buildings of Italy, nor is the composition of the finely rendered bathtub in Room AA24 and shown in Ill. 47 and Plate V described. Such imperfections are symptomatic of careless editing.

The deficiencies should not detract from the importance of this first volume. The companion volumes on the 1973–1987 excavations around Jericho are also eagerly awaited and they might be expected to help authenticate the chronology of the ancient buildings detailed in this publication.

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Summaries of Lectures

'Dushara and All the Gods': the Representation of the Divine Among the Nabataeans

John F. Healey

The Nabataeans, neighbours of the Judaeans during the Greek and Roman periods, had a religious tradition, which can be partially understood on the basis of inscriptions (Aramaic and Greek) and archaeology. There are typical Nabataean religious installations: temples (two main types). processional ways, 'high-places', rock-sanctuaries and votive niches. There are examples of all of these at the Nabataean capital, Petra.

In inscriptions we find reference to 'Dushara and all the gods', but the Nabataean pantheon was small. Dushara (Greek Dusares) was regarded as the god *par excellence*. His name is a descriptive title of the god probably meaning 'the one of the Shara mountain (in southern Jordan)'. There is a hint that Dushara is astral, since he is called 'the one who separates night from day'. Suggesting a link with the sun-god, Strabo says: 'They worship the sun, building an altar on the top of the house, and pouring libations on it daily and burning frankincense. ' And Epiphanius mentions the cult of Dushara at Petra and his birth of a virgin sun-goddess: '. . . they praise the virgin with hymns in the Arab language and the child who is born of her they call Dusares. ' Dushara was also venerated in pre-Islamic north Arabia. So far as the cult of Dushara is concerned, a later source refers to his cult centred on a black stone: 'The image is a black stone, square, unshaped. . . It is placed on a gold-plated base. To this they sacrifice and pour out the blood of sacrificial victims.' Certainly the

Nabataeans were fond of aniconic cults, i.e. without figurative statues of gods. Instead the gods were often represented as plain stone pillars, though statuettes were used when the Nabataeans came under Roman influence. A marble hand from a statue was found in the presumed Dushara temple at Petra. Finally, one should note Dushara's role as the dynastic god of the Nabataean royal family. He is identified as the god of whom several of the Nabataean kings were worshippers.

There are two names of goddesses which are found more than any others: Allat and al-'Uzza. Both were well known in pre-Islamic Mecca, where they were distinct goddesses, but there is reason to regard al-'Uzza ('the Mighty One') among the Nabataeans as a manifestation of the underlying goddess Allat. In Petra the goddess was worshipped under the title al-'Uzza, whereas at Ramm, for example, she was called Allat. A bilingual Greek and Nabataean dedication from Cos identifies al-'Uzza with Aphrodite and there was a temple of Aphrodite in Petra in the second century AD. Important is the Arabian background of Nabataean religion and its aniconism, a feature that it shares with Arabian religion and is reflected both in the Jewish and the Islamic reluctance to represent the divine in images.

The worship of Dushara and Allat/al-'Uzza, if not to the exclusion, then to the diminution of other deities, is a distinct feature showing a 'trend toward practical monotheism' (Teixidor).

C. L. Woolley, T. E. Lawrence and 'The Wilderness of Zin'

T. Sam N. Moorhead

This lecture emanates from research carried out whilst preparing for the republication of *The Wilderness of Zin* by C. Leonard Woolley and T. E. Lawrence by The Palestine Exploration Fund and Stacey International (2003). The lecturer has written a new introduction for this revised edition which provides a backdrop to the survey. The new publication also includes much unpublished material relating to the survey which is housed in the archives of the PEF.

Kitchener, the British Agent at Cairo in 1913, was concerned that Britain should have good quality maps of Sinai and southern Palestine in case hostilities began with Turkey. He was aware of the deficiences as he had been part of the *Survey of Western Palestine* in the 1870s and 1880s which had mapped the region only as far south as Beersheba. In 1913, Captain Newcombe and his engineers mapped British-held Sinai, but there was a difficulty in obtaining Turkish consent for a British military survey of the Negeb Desert of southern Palestine. It was suggested that permission be sought through the offices of Palestine Exploration Fund (PEF), a tactic that succeeded.

Woolley and Lawrence were working on the British Museum excavations at Carchemish on the Euphrates in Northern Syria. The Director of the British Museum, Sir Frederic Kenyon, allowed them to join the survey in January and February 1914 to undertake archaeological research for the PEF. It is clear, however, that this was a military survey, that Newcombe was in overall command, and that the two archaeologists were effectively providing cover for the sappers.

The two men were, by their own admission, not experts in the region and were to acknowledge that they did repeat work carried out before by other scholars. However, in the space of about seven weeks they visited and planned several major Byzantine sites (including Khalasa, Esbeita, Abda and Kurnub), made important observations about the management of water in earlier periods, investigated several *tell* sites (including Ain el-Guderat), studied and even excavated a number of burials, and collected pottery and flints. They also took many photographs and recorded a number of inscriptions, both Greek and Nabataean. Lawrence was even to paddle over to the Crusader Castle on Pharaoh's Island in the Gulf of Akaba and visited Petra on his way

home to Carchemish. However, their most important contribution to research in the region was probably their conclusions about Kadesh-Barnea where they questioned the veracity of the biblical account of the Exodus, showing Trumbull's earlier work to be a 'farrago of lies'. Their conclusion, which has been accepted until very recent times, was that if the Israelites were ever here they occupied a wider region around Kadesh-Barnea.

Both men prepared their findings for publication in the months before and after the outbreak of war in August 1914. When they left for Cairo in December 1914, David Hogarth, at the Ashmolean Museum, saw the work through press as the *Annual of the Palestine Exploration Fund for 1914–15*. Any impact that the publication might have had was lost in the din of war and it was only with the republication of the survey by Jonathan Cape in 1936, after Lawrence's death, that the world paid tribute to this remarkable episode in archaeological research.

Despite the fact that their work was effectively a smokescreen for a military mapping exercise, the final publication is full of important information and is also beautifully written. It is with this in mind that the PEF decided to republish a work which in many ways encapsulates the spirit of its age. Copies may be obtained direct from PEF (2 Hinde Mews, Marylebone Lane, London, W1U 2AA; www.pef.org.uk). £25.

Classical Phoenician Scarabs. A Miniaturist Art of the Persian Period

John Boardman

Scarab seals of green stone, mainly jasper, are characteristic products of the Phoenician world through the Persian period, from the later sixth century through the mid-fourth century BC. Some 1500 are known, The majority have been found in the rich cemeteries of the west Mediterranean, at Carthage, and on Ibiza, Sardinia and in Spain, and it is commonly thought that they were made in the west, in Sardinia or Carthage. However, there are many reasons, both from finds and on grounds of iconography and comparisons with other arts, east and west, including coinage, which suggest that the main and possibly only centre of production was in Phoenicia itself. The evidence for this is surveyed, together with a conspectus of the devices and styles employed. The range is considerable. Many are wholly Egyptianizing in the familiar Phoenician manner. Many are more Levantine with connections to other arts of the homeland and of Syria. Many have purely Greek devices, and in all categories there are mixtures of motifs and styles. (A catalogue, with illustrations of many, may be found on the website: www.beazley.ox.ac.uk).

The 'Golden Age of Solomon': Fact or Fiction? The Archaeological Evidence

William G. Dever

In the past decade or so, 'minimalists' in both biblical and archaeological circles have called into question much of the historicity of the Hebrew Bible and of ancient Israel. The case of Solomon and his reputed 'Golden Age' has become the focus of much of the controversy. For instance, some 'post-Zionist' archaeologists in Israel are fuelling the fire by dating the well-known Solomonic defences in I Kings 9:15–17 down into the 9th century BC, thus removing them as evidence of the sort of centralization and monumental architecture that usually define 'statehood'. This illustrated lecture presents ample evidence for the conventional tenth century BC date for Solomon and the biblical 'United Monarchy' at Hazor, Megiddo, and Gezer (the latter the lecturer's own site) as well as rich comparative data for Solomon's temple in Jerusalem.

The United Monarchy of Early Israel: Myth or Reality?

Israel Finkelstein

The story of the United Monarchy of David and Solomon is one of the greatest epics of Western civilization. A young shepherd kills the giant Goliath with a single sling shot. At once, David saves Israel from the Philistines, becoming a metaphor for the weak overcoming the mighty. He then flees from the rage of Saul, the first tragic king of Israel, who commits suicide on the battlefield. Conquering Jerusalem, David embarks on an unprecedented campaign of conquest and establishes a great empire stretching from the Nile to the Euphrates. And he receives an unconditional promise from God: his dynasty will rule in Jerusalem forever.

David's son and successor Solomon has likewise captivated Western literary and religious imagination. His wisdom is the standard by which all rulers are rated. His wealth and opulence – displayed in his magnificent palace and far-reaching trading expeditions – was reportedly so great, that it became the ideal countless later kings attempted to achieve.

No wonder that David and Solomon have always been revered in Western tradition. From Constantine to Charlemagne, from the 'David Throne' of the kings of England to the 'Crown of Solomon' of the Ottonian kings of Germany, David and Solomon supplied the greatest monarchs of the world with a model of kingship: pious, courageous, human, but capable of moral weakness.

But is this great epic historically reliable? What if David and Solomon were, as some scholars now contend, entirely legendary characters with no more historical substance than King Arthur or Helen of Troy?

The quest for the United Monarchy – the glamorous empire of David and Solomon – has been the most spectacular venture of Biblical Archaeology. The obvious place to start the search was, of course, Jerusalem. Yet, Jerusalem proved to be a hard nut to crack: The nature of the site made it difficult to peel away the layers of later centuries. And the place where the Temple and perhaps also the palace stood – the Temple Mount – has always been out of bounds for archaeologists.

So, the search was diverted to other sites; first and foremost among them, Megiddo in the Jezreel Valley. Megiddo is specifically mentioned in I Kings 9:15 as having been built by Solomon. Starting almost a century ago, on 1 April 1903, when the German explorer Gottlieb Schumacher began excavations at the site, Megiddo has become the focus of the endeavour to add flesh and bones to the Solomonic myth. Strategically located on the international highway connecting Egypt and Mesopotamia, Megiddo has been excavated by no less than four expeditions, including the Oriental Institute dig in the 1920s and 1930s, and in recent years by the Tel Aviv University, led by David Ussishkin, Baruch Halpern and myself. And it has yielded more 'biblical' monuments than at any other site in the Levant.

The Oriental Institute's excavation at Megiddo, between 1925 and 1939, was the most comprehensive dig in the history of biblical archaeology. The OI team worked at Megiddo for fourteen years, all year round, with hundreds of workers, and uncovered the remains of about thirty cities built one on top of the other. Close to the surface, they unearthed two sets of large public buildings, each divided into three aisles, separated by two sets of stone pillars and troughs. P. L. O. Guy – one of the directors of the expedition – identified the buildings as stables. He attributed them to King Solomon. Guy wrote: 'And if we ask ourselves who, at Megiddo, shortly after the defeat of the Philistines by King David, built with the help of skilled foreign masons a city with many stables? I believe that we shall find our answer in the Bible . . . if one reads the history of Solomon … one is struck by the frequency with which chariots and horses crop up'. It looked as if the great United Monarchy had been traced. And the 'stables' paradigm became

the ultimate word for almost 30 years to come.

The change came with the excavation of the Israeli archaeologist Yigael Yadin at Hazor in the north, on behalf of the Hebrew University, in the 1950s. Yadin noticed the similarity between the six-chambered gate that he uncovered at Hazor and the one that the OI had unearthed at Megiddo. Turning to I Kings 9:15, which says: 'And this is the account of the forced labour which King Solomon levied to build the house of the Lord and his own house and the *Millo* and the wall of Jerusalem and Hazor and Megiddo and Gezer', Yadin decided to dig Gezer – not in the field, but rather in the old reports dating from the beginning of the twentieth century. And, indeed, he discovered a similar gate there, hiding in the plan of what had been described as a Maccabean castle. Ostensibly, a perfect match between text and archaeology.

Yadin proceeded to carry out soundings at Megiddo and revised the stratigraphy and historical interpretation of the OI team. According to him, Solomonic Megiddo is represented not only by the gate, but also by two beautiful ashlar palaces – one discovered in the 1920s and the other traced by him in the 1960s. Both were found *under* the city of the 'stables'. Yadin had no doubts when he wrote: 'Indeed, it seems that there is no example in the history of archaeology where a passage (i.e., I Kings 9:15) helped so much in identifying and dating structures in several of the most important tells in the Holy Land….'

Yadin's interpretation seemed to fit the biblical testimony perfectly: Canaanite Megiddo was destroyed by David; the palaces were attributed to the Golden Age of Solomon; and the stables were down-dated to the early nineth century BC, to the days of King Ahab, who is reported to have faced the great Assyrian king Shalmaneser III in Syria with a huge force of 2000 chariots. No wonder that Yadin's interpretation has become the standard theory on the United Monarchy.

But it was haunted by severe problems from the outset. First, the gate at Megiddo seems to have been built later than the gates at Hazor and Gezer. Second, similar gates have been discovered at other places in the country, among them sites that date to late monarchic times, and at sites built outside the borders of the great United Monarchy even according to the maximalist view.

No less important, Yadin argued that the identification of the Solomonic cities was done according to stratigraphy, chronology and on the basis of the biblical passage. But unfortunately neither ancient walls nor old pots carry labels saying, 'made by Solomon'. Stratigraphy and pottery provide relative chronology: what comes first and what comes later, but not absolute dating. In order to reach a date we need a find that would anchor the archaeology of Israel to the well-dated monarchs of Egypt and Assyria. The problem is, there is no such anchor for the tenth century BC. The fragment of the Shoshenq I stele found by the OI team at Megiddo would have given us an anchor, but unfortunately, it was uncovered out of context. This means that the connection between the remains in the ground and the historical sequence has to be established based on biblical material. Hence, Yadin's theory rested solely on the biblical passage. This we should honestly acknowledge: that until recently, the *entire* interpretation of the archaeology of Israel in early monarchic times rested on a *single* biblical verse.

That is not all. Biblical scholars have recently argued that the descriptions of the United Monarchy draw a picture of an idyllic golden age, and that they are wrapped in a theological and ideological construct of later times. Hence, one may ask, whether the famous verse does not in fact reflect the reality of the time of the compilation of the text, i.e. in the seventh century BC. Last but not least, the conventional theory raises severe historical and archaeological problems. Here are a few examples. First, the rise of territorial states in the Levant was an outcome of the westward expansion of the Assyrian empire in the early ninth century BC. Indeed, all extra-biblically documented

states in the region – Aram Damascus, Moab, Ammon and northern Israel – developed in the ninth century BC. It is extremely difficult to envision a great empire ruled from a marginal region, a century before this process. Second, the Megiddo palaces, dated to the time of Solomon in the tenth century BC, were built in a typical north Syrian style. Yet, most if not all of the prototypes in Syria date a century *after* the Megiddo buildings.

Third, and most annoyingly, over a century of archaeological explorations in Jerusalem – the capital of the glorious United Monarchy – has failed to reveal evidence of any meaningful tenth-century building activity there. The famous stepped stone structure – usually presented as dating from the time of the United Monarchy – was built *earlier* and renovated *later*, in the eighth century BC. The common pretext, that the tenth-century remains were eradicated by later building activities, should be brushed aside. Monumental fortifications, for example,from the Middle Bronze and from the late-Iron II have survived the later occupations. To make a long story short, tenth-century Jerusalem – of the time of David and Solomon – was no more than a poor, small, remote highlands village, and not the exquisitely decorated capital of a great empire.

So much for the negative evidence. More straight-forward clues come from two sites related to the Omride dynasty which ruled over the Northern Kingdom in the ninth century – Samaria in the highlands, its capital, and Jezreel in the valley, where the wicked Jezebel was killed and thrown to the dogs from the window of her palace.

Ashlar blocks uncovered in the foundations of the palace at Megiddo carry specific masons' marks, found in one other building in Israel: the palace of the Omride dynasty at Samaria. Norma Franklin has recently shown that these masons' marks are so unique and limited in distribution that they must have been executed by the same group of masons. But one palace was dated to the tenth century and the other to the ninth century BC. There are only two alternatives: either to push the Megiddo building ahead to the ninth century, or to pull the Samaria palace back to the tenth century. The biblical source on the building of Samaria by the Omrides must be reliable, since it is supported by Assyrian texts that relate to the Northern Kingdom as *bit omri*, that is, 'the House of Omri' – the typical genre of calling a state after the founder of its capital. Therefore, there is hardly a doubt that down-dating Megiddo would be the only option.

Recent excavations at Jezreel (located less than ten miles from Megiddo) by David Ussishkin and John Woodhead revealed equally surprising results. The destruction layer of the Omride compound, dated to the mid-ninth century BC, yielded a rich collection of vessels identical to a Megiddo assemblage which has conventionally been dated to the late tenth century BC. In this case, too, there is only one option: down-dating the Megiddo palaces to the ninth century BC.

Finally, the technique of radiocarbon dating has improved over the last decade. The results of a series of samples of grain and beams from several northern sites, including Megiddo, are *lower* by almost a century than expected according to the conventional chronology. In other words, strata, which have been conventionally dated to the 11th century, provided dates in the tenth century BC and so on. These surprising results fit what I have said before: The date of the 'Solomonic' monuments should be lowered by 75–100 years!

What is the meaning of all this for biblical and historical studies? Well, the great biblical story of the United Monarchy is left with no material evidence. The beautiful Megiddo palaces – until recently the symbol of Solomonic splendor – date to the time of the Omride Dynasty of the Northern Kingdom, almost a century later. This should come as no surprise; extra-biblical texts attest to the great power of ninth century Israel: Shalmaneser III tells us that Ahab was the most powerful participant in the anti-Assyrian coalition

that faced him in western Syria. Mesha, king of Moab, mentions the Omride conquests in Transjordan, and Hazael, king of Aram Damascus, relates to their conquests to the north of the Sea of Galilee. The great, powerful and glamorous Israelite state was the Northern Kingdom – the wicked kingdom in the eyes of the biblical historian – not the small and poor territory dominated by tenth century Jerusalem.

A group of European scholars have recently doubted the very existence of David and Solomon. But the ninth century BC Tel Dan Aramaic inscription, which refers to Judah as the 'House of David', strikes a death blow to this view. David and Solomon did exist, but they were local chiefs, who ruled from a small village over the still sparsely-inhabited Judaean hills.

If these are the facts on the ground, what is the origin of the biblical tale of an illustrious United Monarchy? In order to answer this question we need to acknowledge that the biblical narrative of the ancient History of Israel was put in writing in the late seventh century BC, in the days of King Josiah, who is described in the text as the most righteous monarch of the lineage of David. The text intended to serve Josiah's political and religious agenda, of territorial expansion into the lands of vanquished Israel and centralization of the cult in Jerusalem. It should come as no surprise then, that it is easy to identify the landscapes and costumes of the seventh-century – the time of the compilation of the text – as the stage setting behind the biblical tale. The lavish visit of Solomon's trading partner, the Queen of Sheba, to Jerusalem no doubt reflects the participation of seventh-century Judah in the lucrative Arabian trade. The same holds true for the description of the trade expeditions to lands afar that set off from Ezion-geber on the Gulf of Aqaba – a site which was not inhabited before late-monarchic times. And the armour of Goliath, which resembles that of a Greek hoplite of the seventh or sixth century BC (and not of an early Aegean warrior), should probably be understood against the background of the service of Greek mercenaries in the armies of the seventh century – certainly Egypt and possibly also Judah.

This does not mean that the text is devoid of any early material. For example, the narrative of David and his gang, wandering in the southern fringe of Judah, is a typical depiction of a group of outlaws in the second millennium tradition. The demographic background behind these stories does not fit the situation in Judah in late-monarchic times, when the southern fringe was already densely settled. It must reflect earlier, orally-transmitted memories, which were incorporated into the seventh-century history.

Finally, and most important is the ideological layer in the stories: the tale of a glamorous United Monarchy had an obvious meaning for the people of Judah in the days of the compilation of the text. In a time when the Northern Kingdom was no more than a memory and the mighty Assyrian army had faded away, a new David – the pious Josiah – came to the throne in Jerusalem, intent on 'restoring' the glory of his distant ancestors. He was about to 'replay' the history of Israel. By cleaning Judah of the abominations of the nations and undoing the sins of the past, he could stop the cycle of idolatry and calamity, which characterized the history of ancient Israel. He could 'recreate' the United Monarchy the way *it should have been*, before it went astray. So Josiah embarked on re-establishing a United Monarchy. He was about to 'regain' the territories of the now destroyed Northern Kingdom, and rule from Jerusalem over all Israelite territories and all Israelite people. These dreams came to a tragic end at Megiddo in 609, when Josiah was killed by Pharaoh Necho.

All this may seem belittling to the statures of the historical David and Solomon. But, in the same breath, we gain a glimpse into the grandeur of the Northern Kingdom – the first true Israelite state. And no less important, we are given a glimpse into the fascinating world of late-monarchic Judah, at the time and place

when the founding document of our civilization was created.

Medicine in Egyptian Canaan

Walter Y. Loebl

While Canaan was under Egyptian control during parts of the Bronze Age, Egyptian doctors may well have accompanied the resident Egyptian military and civil administrators. The lecture surveyed and illustrated the available medical information. A wealth of data on medicine in ancient Egypt has been discovered during the investigation of human remains, using modern imaging techniques and immunological methods. Other data have been gleaned from the surviving medical papyri and from the study of ancient Egyptian art. A wide spectrum of genetic, congenital and acquired conditions have been documented. Skeletal evidence of inflammation, trauma and infection is well represented. The ancient Egyptian physicians were experienced and compassionate. More than thirty are known from the Old Kingdom Period alone, including one woman. Their knowledge of anatomy was based on the ritual custom of embalming the dead. These doctors employed a wide variety of medications, as well as surgery such as trepanation of the skull and circumcision. They also used magic and incantations. A number of aspects of ancient Egyptian medicine have remarkable parallels in the modern National Health Service in England.

Archaeological Light on the Prophet Isaiah

H. G. M. Williamson

In earlier days, Biblical Archaeology was considered rather crudely to be a means of 'proving' the Bible, and this concern both set the agenda for primary research and drove much of the interpretation of the data. This has been recognized for some time now as wholly unsatisfactory, and for a while it looked as though archaeology and Biblical studies might go their own way in complete isolation from each other. Most recently, several new models have emerged as a means for these two disciplines to inform one another while still maintaining their own individual integrity. With regard to the prophets, where the issues are in any case rather different from the historical books, the American scholar Philip J. King has advocated and pioneered a new style of 'Archaeological Commentary', where the findings of archaeology are used for purely illustrative and explanatory purposes, relating the text to its cultural context without any necessary regard for its historical accuracy or the like.

This lecture sought to take up and develop this model with regard to the prophet Isaiah (a book on which King has not yet published). In order to keep the material within bounds, the general scholarly view was adopted that only a relatively small part of this large book could be ascribed to the eighth century prophet himself or to the times in which he lived. From among these, the following passages and topics were illustrated from a wide variety of archaeological sources.

(1) Isaiah 6, the call (or perhaps better recommissioning) of the prophet, provides several topics which have been illuminated in recent years. First, it is often overlooked that God is described as being of enormous size, and this common ancient Near Eastern concept is well illustrated by the large footprints carved into the steps leading into the temple at En Dara. Second, the throne of God may be illustrated by the common motif of the sphinx throne from Sidon and elsewhere, where the sphinx seem to play the role of the cherubim in other biblical descriptions of the ark as God's throne. Moreover, the slope on the seat of the Sidon throne is so steep that it could not have supported a cult statue; the view that there must have been a cult statue of God in the Jerusalem temple is not, therefore, a necessary conclusion to be drawn from the texts. And finally, the seraphim, which elsewhere in the Bible

appear to be snake-like creatures with wings, are now widely associated with the so-called *uraei*, of which many examples are found depicted on seals and the like both in the Levant and in Egypt.

(2) Isaiah 20 is a prose account which refers among other things to Sargon's campaign against Ashdod. That this is likely to rest on sound historical memory is suggested by Sargon's own account, including the fragment of a victory stele from Ashdod itself. It is of interest to note that this is the only mention of Sargon anywhere in the Bible, so that the name has been correctly remembered in this passage and not drawn from elsewhere. The Hebrew word for the commander-in-chief is likewise merely the Hebrew form of the Assyrian word *turtanu*.

(3) Isaiah 22:10 refers to the defensive preparations which Hezekiah undertook during the years leading up to his anti-Assyrian rebellion and which led directly to Sennacherib's invasion in 701 BC. The description fits exactly with the nature of the so-called 'broad wall' which was found in the modern Jewish Quarter in Jerusalem as part of the eighth-century BC enclosure of the Western Hill of Jerusalem.

(4) Isaiah 1:5–8 and 29:3. Excavations at Lachish have done a great deal to illustrate the nature of Sennacherib's campaign, including the use of siege warfare and the destruction of captured cities. The relevant material was shown and discussed in some detail.

(5) Finally, Isaiah 8:21 was discussed to indicate how sometimes even a small find (in this case of a brief inscription from Ekron) can shed welcome light on an obscure and poorly-understood passage. This part of the lecture was based upon my own short contribution to a previous number of this *Bulletin*: *BAIAS* 18 (2000): 51–55.

Qumran During the Jewish Revolts (Period III)

Joan E. Taylor

Khirbet Qumran is one of the most famous sites in the areas of Israel and Jordan, since it was close to this site that the Dead Sea Scrolls were discovered, from 1947 through to the 1950s, in various caves between 1.2 and 2.1 km north of the ruins (Caves 1, 2, 3 and 11) and in some artificial caves cut into the marl terrace on which the ruins stand (Caves 4–10). Interest in Qumran has focused on the occupation periods associated with an Essene community, considered to be responsible for the Scrolls, from the second century BC to AD 68.

However, there is more to Qumran than an Essene settlement. Prior to its apparent sectarian use there was an occupation period in the Iron Age. Pottery from this stratum dates to the eighth-sixth centuries BC. It is thought that it was built as a fortress to protect the lucrative trade of balsam oil controlled by the Judaean monarchy, since a jar-handle with the inscription in palaeo-Hebrew characters, 'to the king' (*lm/lk*) was found. The site of Qumran is located just beside one of the few passes from the north-western Dead Sea to the higher region of the Buqei'a, and balsam oil could have been transported overland across this area to Jerusalem, and to other sites in Judaea. As such this links Qumran with the other Israelite fortresses on the plain of Buqei'a above, at Khirbet Abu Tabaq, Khirbet es-Samra and Khirbet el-Maqari, which Frank Moore Cross and J. T. Milik explored in the 1950s. There was also another Israelite fortress a little less than one km south, on the plain between Khirbet Qumran and Ein Feshkha, with pottery dating from the ninth century, which seems to have been superseded by the building on the plateau. A long wall was built from the south-east corner of the ruins to the Wadi Qumran, and then a thicker wall down on the plain ran over a distance of 500 m south to Ein Feshkha. This fortress was destroyed in the sixth century BC, probably when the Babylonians invaded Judaea. A layer of ash is associated consistently with Israelite sherds.

There is also the neglected post-Essene occupation of the site during what de Vaux called 'Period III'. De Vaux argued that

Period III lasted only a few years, between AD 68 and 73, during which time the Roman military occupied Qumran. However, a closer examination of the evidence suggests that this period was of longer duration than de Vaux supposed, and that its occupation may also have had something to do with the balsam trade as well as other economic resources. Pliny stresses the importance of both palm trees and balsam as local cash crops to the Romans.

The destruction that took place at the site would be consistent with a historical scenario in which the attackers shot burning arrows on to the roofs. All the rooms on the western side of the site, where some kind of agricultural processes took place, were filled with debris from the collapse of roofs and damage extended elsewhere too where the fire would have spread, but it was not a case of all-encompassing destruction, or levelling. When new settlers occupied the buildings they cleaned up and used the existing structures. While the construction was not such good quality as during the Essene occupation periods, there was nevertheless an attempt to make durable structures suitable for long-term use. The overall lay-out indicates a military mindset, with armouries built alongside sleeping-quarters in a one-storey structure, with room enough for about 30 people. There was no attempt to rebuild the agricultural processing areas.

Ein Feshkha, a kilometre further south than Qumran but joined to it by a long wall along the eastern side, has a different character in Period III. A structure had been established at Ein Feshkha from the end of the reign of Herod the Great (Period I). The Period II structures – contemporaneous with the Period II structures of Qumran – here were likewise partly destroyed in 68. In Period III, part of the remaining structure was re-utilised (Loci 21 and 22) until the 90s. A coin of Domitian from Antioch (81–96 AD; Locus 16) and a coin hoard of 17 coins of Agrippa II, dating from 78–95 AD, indicate the period of occupation. There was also a legionary brick. This brick – unpublished and undescribed but presumably of the usual type which bore the stamped name of the Legion X Fretensis – would have been manufactured in Jerusalem after AD 70, and indicates that, like Qumran, Ein Feshkha was associated with the Roman military control.

De Vaux concentrated on the military usefulness of holding a site like Qumran, which provides a good view over the Dead Sea and holds a pass, but he could not understand who then occupied Ein Feshkha in Period III, and why. The Romans act in a strictly military fashion in his scenario. However, Pliny tells us that 5 years after the Roman conquest of Judaea, the sale of balsam brought in 800,000 sesterces to the Roman treasury (*Historia Naturalis* 13. 6–9,118–119). If the oil was bled from the trees not only in Jericho, but also in Ein Gedi and other sites along the north-western side of the Dead Sea, such as En Feshkha, then the pass *en route* to Jerusalem would have been as important in the Roman period, post 68 AD, as it was in the Iron Age (and throughout intervening periods). Roman-sponsored auxiliaries living at Qumran may have supervised a small band of locals living at Ein Feshkha (and elsewhere?), who continued to use their skills to harness economic resources, now for the Roman treasury. This would fit with Pliny's testimony.

How long were the buildings occupied in Period III at Qumran? De Vaux thought the lack of remains testified to a small occupation, perhaps of very limited duration. De Vaux, again thinking only militarily, believed it lasted only up until the fall of Masada, that is for merely 5 years maximum. De Vaux dismissed the coin of Agrippa II found outside the buildings dated to AD 87 as not belonging to Period III, and therefore confined the coin record to narrow parameters. However, the coins of Ein Feshkha indicate later occupation. Further down the western coast, Ein Gedi appears to have been imperial property with a garrison of Roman troops until the time of the Bar Kokhba – or Second – Revolt,

when they were temporarily evicted by rebel forces. Masada was occupied by Roman troops for at least 40 years, even after Arabia was added to the Roman Empire in AD 106. If these sites tell us anything, it is that the Romans were quite slow to move out of the region, and continued a presence there for more than military reasons.

The fewer finds from Period III may not be quite as telling as one might first think because, unlike previous periods at Qumran, there is no destruction level from one particular time to seal evidence underneath. The Period III form of Qumran – a central built-up area with ruined structures to the west – was the form in which Qumran was left to weather the centuries. Subsequent damage to the site was done by the elements and by time, and any pottery or artefacts left on the surface would have gradually been smashed and distributed over the surface. Photographs and reports of the site prior to excavation indicate that it was covered with a very large amount of broken pottery and rubble which was cleared away prior to excavation. Given that de Vaux was working with only 15 workers, for only 3 weeks, in his initial season, and that he managed to make amazing progress in uncovering lower levels, this pottery and rubble was cleared away very swiftly. The lack of finds from Period III may, in this case, come down partly to the procedures of very rapid excavation.

Other evidence tends to have been overlooked. One of the inkwells, for example, turned up in a Period III context in Locus 36. A lamp found in Locus 10 (KhQ 3149) has recently been identified by Robert Donceel as a moulded 'Jerash' type, dated to the first decades of the second century. Lack of detailed stratigraphy in fact means that some supposedly Period II pottery might well belong to Period III, since many types continued in use until the Bar Kokhba Revolt of 132–135, as can be found by a comparison between the discoveries of Nahal Hever and Muraba'at, Qumran and other Dead Sea and Judaean sites. The small occupation in the Bar Kochba period identified by de Vaux may rather be the final evidence of a tiny but continuing occupation through to 132, for over 70 years, at which point the rebels took the area for a short time.

Solomon Schechter and his Oxbridge Academic Friends

Stefen C. Reif

However scientific research appears to be, and whatever degree of detachment scholars purport to achieve, there are.often personal elements involved. This was also true in the late nineteenth century and applied to manuscript discovery and study as well as archaeology. Personal, national and religious backgrounds had an impact on scholarly work, as in the cases of Solomon Schechter, Agnes Smith Lewis and Margaret Dunlop Gibson, and Adolf Neubauer.

Factors other than Schechter's erudition undoubtedly played a part in his Genizah discovery and studies. One personal element was the role of David Samuel Margoliouth, a Jewish convert to Anglicanism, at Oxford. In the course of his inaugural lecture as Laudian Professor of Arabic in 1890, Margoliouth argued that the evidence from the 'whole rabbinic farrago' for the original Hebrew text of Ben Sira could safely be discounted. He regarded the Greek and Syriac texts preserved by the Church as superior witnesses. Schechter defended the notion of an authentic Jewish transmission and hoped for manuscript discoveries to support his theory. Hence his excitement when Lewis and Gibson brought him just such a text in 1896.

The Damascus Document (=CD), called originally the 'Zadokite Fragment' by Schechter, who discovered it in the Genizah in 1897, was subsequently identified among the Qumran scrolls. Schechter long remained unsure of its provenance and held back its publication. Until at least 1903 he associated it with a Samaritan sect. Between then and

the publication of his edition in 1910, he revised his theory. While he, perhaps under the influence of Israel Friedlander, opted for a Sadducean context, another of his colleagues in New York, Louis Ginzberg, used his great talmudic learning to argue for a Pharisaic origin. What is more, Schechter's reluctance to make some of his remarkable Genizah finds available to certain scholars had a negative impact on scientific progress.

With their strong Scottish Presbyterian background, their intensive education and their huge fortune, Lewis and Gibson were able to devote themselves to Semitic scholarship. During trips to the Near East, they acquired or photographed some valuable manuscripts, which they subsequently published. Their Cambridge friends were mainly among the non-establishment. They developed a close relationship with Mathilde and Solomon Schechter and, having been asked by the latter to find manuscripts during their 1896 visit, returned with 2000 (Genizah) fragments acquired in Jerusalem, the plain of Sharon and Cairo. Two of these, from the Palestinian Talmud and Ben Sira, inspired Schechter's Cairo expedition.

Before he was appointed at the Bodleian Library in 1868, Adolf Neubauer had studied a wide range of Semitic and European literature in his native Slovakia as well as in Paris, St Petersburg and Jerusalem and had published widely. In Oxford, he compiled a catalogue of the Bodley's Hebrew manuscripts, greatly expanded its holdings, and was appointed to a Readership in Rabbinic Hebrew. In 1876, Neubauer wrote about the importance of oriental Genizah collections and he brought his nephew Adolf Buchler to work on Oxford Genizah texts in 1891 and 1893. Immediately after the appearance of Mrs Lewis's Ben Sira fragment, and while Schechter was still in Cairo, he arranged the publication of a volume of such items found in Bodley. The tensions over the Genizah and the Ben Sira texts damaged his relationship with the Schechters but Neubauer still figures prominently in Mathilde's reminiscences.

From Forest to Desert in the Bethlehem Area

F. Nigel Hepper

If one draws a line south-eastwards from the summit above Beit Jala to Ein Gedi on the Dead Sea, it drops from about +900m to about -400m. This transect is very interesting ecologically, going from forest to desert. Rainfall decreases abruptly down the gradiant. At the upper levels there is a narrow zone of Mediterranean type trees and shrubs with associated herbaceous plants. Of course, human activity has greatly modified the natural forest which at best is remnant woodland usually known as maquis today. Typical woodland contains the trees: Oak Quercus calliprinos, Palestine Pistacio Pistacia Palaestina, Judas Tree Cercis siliquast-cum and Strawberry tree Arbutus andrachne; the shrubs Soft-hairy Rockrose Cistus incanus. Spiny Broom Calicotome villosa; the herbs: numerous orchids (such as Orchis galilea, Ophrys umbilicata, Lodorum aborium), legumes (such as Lotus, Medicagoi, Trifolium, Trigonella); Compusitae (Crupina, Chrysanthemum); Cyclamen perslcum. Ranunculus asiaticus and many grasses.

Already at Bethlehem on the town terraces there are desert elements apparent, but after good winter rainfall the Bethlehem fields are colourful with typical Mediterranean weeds. Eastwards there is marked decline in the annual average rainfall. Around Herodion the desert flora is distinct. Further east in the Judean Desert where the ground has been undisturbed, interesting zonation and differences in the flora are evident. For example, a south-facing slope has a reduced flora and vegetation cover compared with a north-facing one where there is better moisture retention. There

are few woody plants in this arid environment most of the herbaceous ones are dwarf perennials, subterranean bulbs or annuals persisting as seeds awaiting irregular rainfall.

Down by the Dead Sea other factors come in such as the almost total lack of precipitation and the presence of salt in the soil. Halophytes, as salt-tolerant plants are called, manage to colonize such areas. Ein Gedi itself is different again owing to the presence of spring fresh water coupled with high atmospheric temperatures. Here are splendid relic examples of wild tropical African plants and animals - but tha was beyond my remit for this lecture. (It was illustrated by colour slides taken during field work during several visits to the Bethlehem region.)

Ancient Pots, Modern Science: Investigating a Late Bronze Age Potter's Workshop at Lachish

Pamela Magrill

Large quantities of pottery regularly come to light from excavations in the Near East but the remains of workshops where ancient pottery was actually produced are rare. One such workshop was discovered at Lachish (Tell ed-Duweir), a large *tell* about 25 miles southwest of Jerusalem, during British excavations held there led by J. L. Starkey in the 1930s. The workshop, dating to the end of the Late Bronze Age (*ca.* 1200-1150 BC), was located in a large cave (locus 4034) situated on the northeast slopes of the mound. Finds included lumps of prepared clay, large quantities of unfired sherds, tools, pigments, stone pivots from potters' wheels, a number of figurines and a fragmentary press mould as well as about 40 complete tired pottery vessels.

A brief account of Cave 4034 and its contents was published in *Lachish IV* (Tufnell 1958: 291-93) but no in-depth study of the material was carried out at the time. In 1980 a large collection of artefacts and archives from the 1930s excavations at Lachish came to the Department of Western Asiatic Antiquities (now the Department of the Ancient Near East) in the British Museum from the Institute of Archaeology, University of London. This provided an opportunity during the 1990s for the writer (then Curator of the Lachish Collection) and Dr. Andrew Middleton of the Department of Scientific Research at the British Museum, to carry out a wide-ranging research project with the aim of reconstructing the activities of the ancient Lachish potters from the procurement and preparation of the clay to the production and firing of finished vessels. As the project progressed, a professional potter, Trevor Thomas, also became involved.

During the course of the project various aspects were investigated using appropriate scientific techniques. Questions concerning raw materials such as the type(s) and source(s) of clay used by the potters and whether they prepared specific clay pastes for particular types of vessels were examined by petrographic analysis. Questions relating to how vessels were formed and the use of the potter's wheel were initially investigated macroscopically by observation, then in the lab using xero-radiography and finally, in the case of two particular types of vessels, cooking pots and pilgrim flasks, by experimental replication studies in the potter's studio by Trevor Thomas. Firing temperatures and conditions were also investigated using scientific techniques including experimental firing of samples of unfired clay and sherds in the lab.

The results of the project enabled us to better understand the activities of the Late Bronze Age potters at Lachish and so, in a sense, through the use of archaeology, modern scientific techniques and the practical knowledge and skills of a professional potter, to bring this ancient workshop back to life.

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Medicine in Talmudic Palestine

Walter Y. Loebl

Talmudic Medicine has its origins in earlier events. The defeat of Samaria and of Jerusalem intimated a new belief in the after-life, and people's faith in God's Biblical monopoly over health and disease diminished. Contact with Mesopotamian medicine during the exile, and the introduction of Hellenism by Alexander the Great from 332 BC, stimulated the people's pursuit of medical care, and the employment of physicians. Public health, in particular, was a Talmudic concern. Thus odours, dust, smoke, and burials had to be sited down-wind from habitation.

Data on Mesopotamian medicine are scant. Compared with the wealth of Egyptian material, there is little paleopathological, archaeological, epigraphic, or artistic evidence in existence. The work of the Mesopotamian practitioner the *ASU*, was regulated by the laws of Hammurabi, as formulated on his eighteenth century BC stele. The rank of the patient determined the remuneration of the *ASU* – or the severity of his penalty in case of failure: when a seignior died from the operatlon, the *ASU* lost one hand. The treatment that was given by the *ASU* could be supplemented by the *ASHIPU*. the magician, and a number of other experts and deities.

Numerous cuneiform tablets reveal the details of the work of these practitioners and the remedies that they used – that are mostly bizarre. A collection of cuneiform texts from the fifth century BC probably represents student texts from an ancient Mesopotamian medical school. A large number of incantation bowls has also been discovered. They are from sixth-seventh centuries AD and about half of them are inscribed in Hebrew script, often the translations of Biblical texts into Aramaic. Greek medicine combined a strong belief in several medical deities, with a philosophical system of medical idealogy. The cult of Asklepios and the healing methods at his sacred precinct, the Asklepieion, played no role in Talmudic medicine, as they were obviously pagan. But the theories of the four qualities (hot, cold, wet and dry), that combined in pairs to form the elements (air, water, earth and fire), and humours (blood, phlegm, yellow bile and black bile), were widely accepted, as was the principle of treating like with like.

Devoid of observation and experimentation, the Greek concepts of piolgy and of the cause of illness were mostly erroneous. For example, it was was believed that the blood stayed in the veins – with *pneuma* in the arteries. Air reached the brain first, and then it distributed in the arteries. The gender of a baby was determined by the strength of the ejaculated sperm. But in Alexandria, vivisection on condemned prisoners was practised. Thereby it was discovered, that male and female embryos in the womb develop at an equal rate.

A fascinating picture of Talmudic medicine is provided by the illnesses of Rabbi Judah the Patriarch, the redactor of the Mishnah at the beginning of the third century AD. His chronic painful bowel problems, combined with intermittent arthritis, and diverse complaints of the mouth, eyes and bladder, can be ascribed to a single disease now – thanks to recent immunological and genetic research.

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