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

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Editorial

The past year has been momentous in terms of the archaeology of Israel in the media, and the theme has been one of tombs, from the sensation of the so-called 'Jesus Family Tomb' in Talpiot to the discovery of what might be Herod's Tomb at the palace-fortress of Herodium. To everyone's great pleasure, the excavator of the latter site, Professor Ehud Netzer, provided the Anglo-Israel Archaeological Society and British Friends of the Hebrew University of Jerusalem with a stimulating lecture about the history of excavating this 'unique site', as he put it. The audience of the packed lecture theatre at the Clore Education Centre in the British Museum was treated to the very interesting story of Netzer's 30-year quest for the lost tomb. The find consists of a monumental platform close to the 'steep ascent of steps' mentioned by Josephus Flavius, which led up from the lower residences and pools to the upper fortress of Herodium. With the help of two young archaeologists, Netzer uncovered the podium of a structure that might be interpreted as a funerary chamber or '*nefesh*'. Many fragments of a large stone sarcophagus, decorated with rosettes in high relief, were found in the fills around the ashlar foundations of the podium, perhaps the sarcophagus of Herod the Great. Various decorated architectural fragments were also found and they presumably came from the destroyed superstructure of the funerary structure, which seems to have been built in classical style. There are a number of questions still remaining: could this be the tomb not just of Herod the Great but perhaps also of his son, Herod Archelaus? Who smashed the sarcophagus (Netzer says this was done intentionally)? Was the funerary monument built to contain the sarcophagus, or was Herod buried in an adjoining cave? Netzer says he plans to continue digging, so hopefully he will come up with more answers.

The *Bulletin* continues to be at the cutting edge of archaeological discovery and debate in Israel and its environs, with articles in this issue ranging from an excavation report to a re-examination of the controversial caves near Qumran that have yielded the Dead Sea Scrolls. It opens with an article in which Boaz Zissu reviews a burial cave found over 70 years ago just north of the village of El-Maghar and publishes in full for the first time material which was previously available only in the IAA archives. The evidence here helps to provide data about Jews in the Coastal Plain during the Second Temple Period. Readers will remember that a previous issue (*BAIAS* 21) carried an article on the village of Maghar by Avi Sasson.

The surveys, excavations and restorations at the Nazareth Village Farm in Galilee are presented here by Ross Voss, Stephen Pfann and Yehudah

Rapuano. Their excavations have provided new evidence for terrace farming over a vast period of time, from the Early Bronze Age to the Ayyubid/Crusader Period, with much of this material deriving from the Roman Period. The terraces and associated watchtowers show how farming took place at the time of the Second Temple, providing a background for the life and parables of Jesus of Nazareth. Pottery discovered here will undoubtedly play its part in a better understanding of Galilean typologies.

A re-examination of the 'synagogue' of Delos by Lidia Matassa is perhaps the most controversial article in this issue. Matassa carefully dismantles the arguments used hitherto, casting serious doubt on whether this was indeed a Jewish structure. In addition, the significance of two Samaritan inscriptions on Delos is explored.

Likewise, Leah Di Segni and Shimon Gibson re-examine the identification of an inscription from Khirbet el-Jiljil published by Émile Puech. While Puech identified this partly illegible inscription as indicating that the burial of St. Stephen was located in a circular mausoleum there, Di Segni and Gibson question Puech's reading and identify the circular structure as a building for wine-pressing, part of a *villa rustica* complex.

Finally, Stephen Pfann considers the scroll caves around Qumran, as well as the kinds of libraries from which the scrolls may have come. Radically, Pfann argues that the caves may not be dated only to one time – appropriate to hiding places ahead of the Roman quelling of the Jewish uprising in AD 68 – but rather he points out that different caves have different characters, evident from their documentary contents. Caves 1Q and 6Q are clearly '*Yahad*' caves and are older palaeographically, while 11Q and 3Q – both of which are further away from the site of Qumran – have a priestly or Temple orientation, and palaeographical dates are largely from the first century AD.

The latter caves, with 2Q, Pfann associates with rebels fleeing from Jerusalem. If Pfann is right, then this would alter how scholars interpret many of the Dead Sea Scrolls.

The *Bulletin* concludes with three book reviews, ten lecture summaries, and an obituary of Prof. Yizhar Hirschfeld, who contributed a very important article on his work at Horvat Sa'adon in the last issue. His graciousness and knowledge will be much missed in the archaeological community, and we personally regret losing a scholar who was willing to think expansively and ask important questions. The review article included here, written by one of the editors, is intended as part of a thought-provoking discussion (one which he himself enjoyed) on innovative perspectives regarding Qumran.

From this issue Dr. Joan E. Taylor, Adjunct Senior Lecturer at the University of Waikato, New Zealand, and Honorary Research Fellow at UCL's Department of History, joins as co-editor of the *Bulletin*. Certain changes to the structure and appearance of the *Bulletin* are currently being considered. The editors have worked together on various projects over the years, and even

co-authored a monograph on the Church of the Holy Sepulchre in Jerusalem, and look forward to a rewarding collaboration. We would like to offer our congratulations to one of our previous grant recipients, Elaine Myers, who has now completed her doctorate. There is good news also regarding the AIAS website (www.aias.org.uk) which has been updated and now has a dig section with links to relevant sites.

The Editors and Committee gratefully acknowledge the very kind donations made by: the Avenue Charitable Trust; Mr R. Beecroft; Mr P. Brett; the Sidney and Elizabeth Corob Charitable Trust; Mr and Mrs R. Grutz; the Lauffer Family Charitable Trust; the Morris Charitable Trust; the Polonsky Foundation; Dr Stephen Rosenberg; the Sacerdoti Charitable Trust and others. Also thanks are due to Joe and Linda Dwek for their support of the Manchester lecture series and student membership. We would also like to thank Ashley Jones, the Reviews Editor, and Diana Davis, the Executive Secretary of the Society, for their help in producing this issue.

Shimon Gibson and Joan E. Taylor

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A Burial Cave from the Second Temple Period at El-Maghar on the Southern Coastal Plain

BOAZ ZISSU

At the beginning of 1946 a rock-cut burial cave was discovered in the course of quarrying for *kurkar* (hard, coarse sandstone), north of the village of El-Maghar, at the base of the northern slope of its hill (Israel grid, estimated map references 1298/1390). The cave was excavated on 2–3 May 1946 by Jacob Ory, then Antiquities Inspector of the British Mandate Department of Antiquities (henceforth MDA). A plan and sections of the cave were prepared and a brief report was submitted to the Department of Antiquities at that time. A full report on the excavation was to the best of our knowledge never published. This article, based on information preserved in the IAA archives, is intended to present the basic data on this cave, in the light of what is known of the Jewish presence in this area during the Roman and Byzantine periods.

The cave architecture and burial practices

The entrance to the burial complex, located in the northern wall, was found open. It was partly damaged by the quarrying operations, which chipped off the upper part of the façade, above the opening (Figs. 1, 2, 3). A slanted (possibly previously stepped) rock-cut, corridor led towards the opening. The typical rectangular blocking stone was not found, but a notch cut in the western wall of the corridor was probably intended to facilitate the operation of the stone.

Three steps descend from the opening into the rectangular burial room (2.7 × 3.2 m), in the centre of which a standing pit was cut, partly surrounded by ledges. Nine *kokhim* (loculi, elongated niches) were hewn in four of the walls, eight of which are of the common type (nos. 1–8 are rectangular, elongated niches, ca. 1.8–2 m in length, ca. 0.5–0.7 m in width and ca. 0.6–1 m in height). Niche no. 9 has a trapezoid horizontal section (0.6 × 1.1 × 1.4 × 0.9 m) and the bottom of this niche is 0.35 m deeper than the floor of the cave. Niche 3a is rectangular, ca. 0.45 m long, 0.2 m wide and 0.2 m high. The ledges are located in front of *kokhim* 1–3 and 5–7.

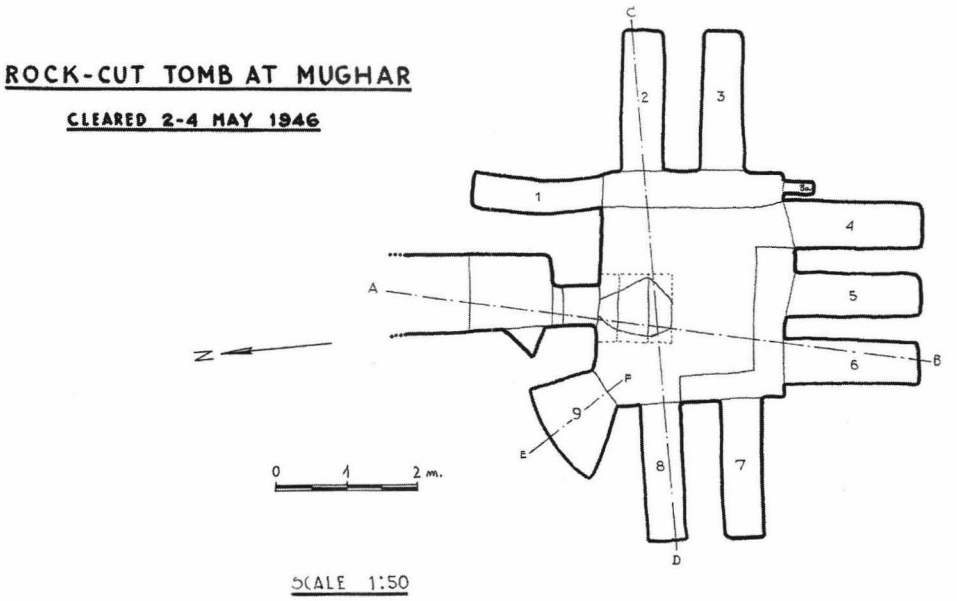

Fig. 1. Plan of burial cave. (J. Ory, courtesy of IAA Archives)

Unfortunately, only meagre data was given regarding the contents of the tomb, which was already disturbed at the time of its discovery. The cave was apparently opened for the first time in 1929 but was not investigated and was subsequently covered (Ory's report S4320, IAA Archives). From the same report we can learn that the cave was rediscovered before March 6th, 1946. Ory arrived at the spot, found that 'two fragments of decorated

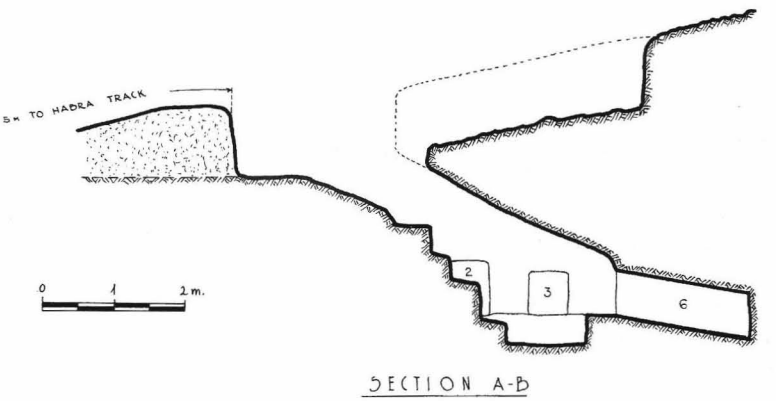

Fig. 2. Section of burial cave. (J. Ory, courtesy of IAA Archives)

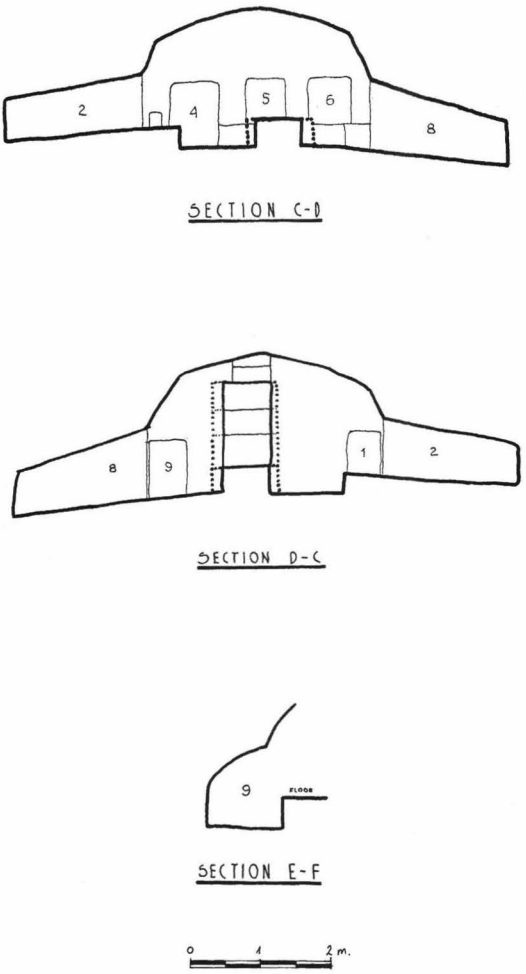

Fig. 3. Section of burial cave. (J. Ory, courtesy of IAA Archives)

ossuaries were lying close by', and initiated the clearance of the tomb, which was 'filled with drift to about one metre from roof' (Ory's report S4320). The project was approved only in April 1946 (Ory's reports S4329, S4343). In his brief excavation report, Ory wrote that the tomb chamber contained: 'fragmentary stone ossuaries and fragments of other ossuaries' (Ory's report S 4345). It remains unknown what the meaning of this description might be, since 'other' may imply, for example, clay ossuaries, of the type Ory already uncovered in 1932 at Umm Kalkha, east of El-Maghar (see Fig. 4), so this issue remains unclear. The ossuaries were 'provisionally stored at the R.E. (Royal Engineers) hut in the camp north of Maghar' (the British army

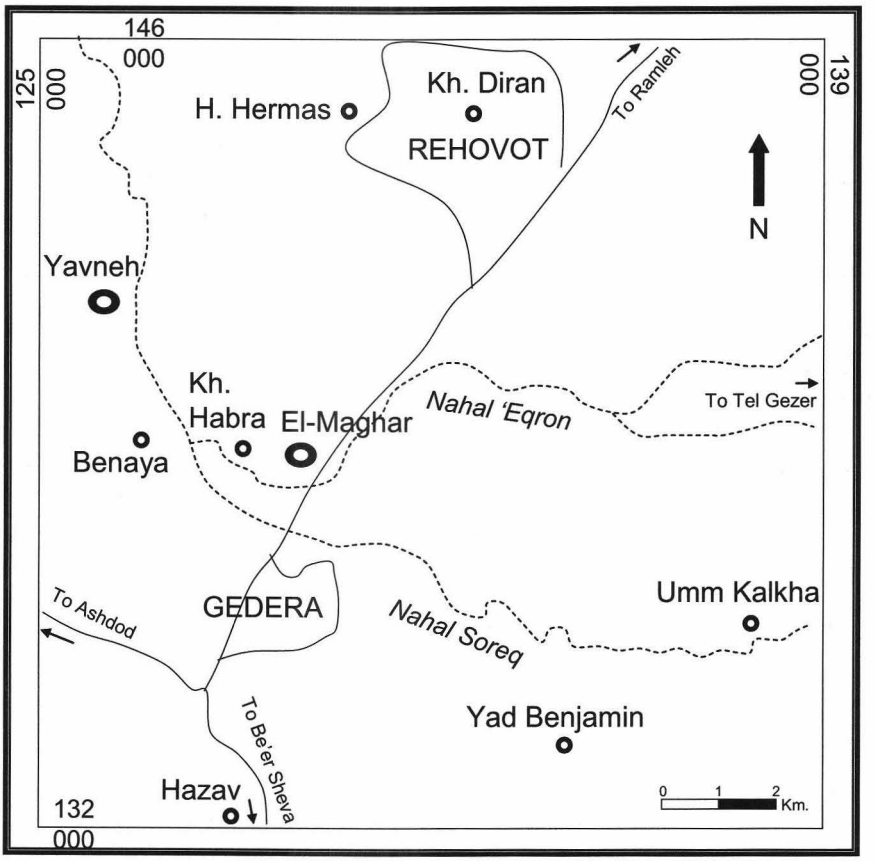

Fig. 4. Location map, showing Second Temple period sites mentioned in the article

base) and were subsequently lost. They are not mentioned in L.Y. Rahmani's list (1994: 304–307).

The architecture of the cave, with the common *kokhim*, is typical of the late Second Temple period, and is found mostly in Jerusalem and its Judaean surroundings (Kloner and Zissu 2003; Hachlili 2005). These types of tombs were used by extended Jewish families which practised double burial: primary burial of bodies and a secondary collection of bones. It is commonly accepted that the *kokhim* were initially intended for primary burials. The secondary burial – or mass collection of bones into ossuaries placed in niches or small chambers (as niche no. 9) located within the family tomb – was a long-established custom, performed by Jewish families in Jerusalem and Judaea from the last third of the first century BC until the end of the Second Temple period, and even afterwards. It is hard to determine whether the already existing niches or small chambers remained in use, while individual bone collection became common within the family tomb.

We presume that niche 3a was used as a makeshift ossuary. These small niches, intended to contain collected bones of a single individual, are found in Jerusalem tombs (Kloner and Zissu 2003: 1–16, 1–17, 2–25, etc.) and in Judaean tombs as well (e.g. at Beth Govrin, Oren and Rappaport 1984: 121–128, Tombs III, IV, V). It is difficult to determine whether they were hewn in walls of caves as a result of a scarcity of ossuaries (e.g. following the destruction of Jerusalem and the disappearance of its stone artisans) or simply as a makeshift solution to the individual collection.

In summary, from the plan of the tomb and the short report, we can learn that the cave belonged to a Jewish family which practised double burial according to the ancestral custom. The owners of the tomb lived in close vicinity, at El-Maghar or at Kh. Habra (see discussion below). In the absence of finds we should rely on the architecture in order to date the tomb: *kokhim* tombs became widespread in Judaea by the late second century BC. The existence of chamber 9 hints that the cave was used in a 'pre-ossuaries' period – predating the last third of the first century BC. The fact that ossuaries were used, and the appearance of niche 3a, indicates a first century AD or perhaps an early-second century AD date. Ossuaries were used in certain parts of Judaea ('Darom' area) also from the late-second to the mid-third centuries AD (Rahmani 1994: 24–25) but there is no evidence to support such a late date for this tomb.

The site – El-Maghar

El-Maghar is situated on the Judaean coastal plain (map ref. Israel grid. 1296/1386), at the top of two hills on the eastern *kurkar* ridge (Fig. 5). This long ridge is orientated north-south, and stretches along the entire Coastal Plain, parallel to the Mediterranean coast (Orni and Efrat 1971: 41). The strategic location, at ca. 88 m above sea level and ca. 50 m above the surrounding area offers a view over, and control of, the whole plain. Nahal Soreq and its tributary, Nahal 'Eqron, pass just to the south of the site. The Soreq Valley has fertile agricultural lands and water sources (wells), and was used as a natural east-west passage from the coastal plain to the Shephelah and the Hills of Judaea. The site also controls the north-south route from Lydda to Ascalon (Sasson 2003: 14–16; 20).

Few nineteenth-century explorers and travellers visited the area, and fewer still left any significant information (see sources and discussion in Sasson 2003: 11–13). The PEF-SWP mentioned the site and marked it (and the neighbouring Kh. Habra) on the SWP Map (Conder and Kitchener 1882: 411–413).

Since the 1940s, Jacob Kaplan surveyed the region and published short reports. At El-Maghar he collected pottery from the Chalcolithic, Bronze and Iron Ages as well as some Hellenistic, Roman and Byzantine material (Kaplan 1944/5; Kaplan 1948/9; Kaplan 1953: 141–142). Kaplan suggested identifying 'Mount Ba'alah' here (Joshua 15:11; Kaplan 1957: 206–207), an

Fig. 5. El-Maghar hill and village, 1940s (courtesy of the IAA, British Mandate Archive)

identification accepted by Y. Aharoni and others (Aharoni 1979: 256; but see discussion of other suggestions in Sasson 2003: 17–18).

In 1994 Avi Sasson conducted a detailed survey of the site, aimed at documentation and reconstruction of the remains of the Ottoman period 'cave village', which existed until 1948 (Sasson 2003).

The Roman-Byzantine settlement at El-Maghar and Kh. Habra

In the following section, we will try to assess the extent of the 'Roman-Byzantine period' settlement at both sites. An examination of the results of Kaplan's survey and other random archaeological discoveries will perhaps be of assistance.

Kaplan mentioned the discovery of Hellenistic period pottery at the top of the hill. The few remains make determination of the exact character of the Hellenistic period settlement difficult. He reported Early-Roman period burial caves, containing *kokhim* and *arcosolia* as well as fragments of soft limestone ossuaries, on the north-eastern slope of El-Maghar hill (the above-discussed tomb should be sought-for in the same area). In 1944 Kaplan purchased, from a peasant, a Jewish burial inscription in Greek from the Byzantine period. The inscription was found at the bottom of the eastern slope of the hill (not *in situ*); Kaplan believed that it originated from one of the tombs on the western slopes of the hill, which was still in use by Kh. Habra's inhabitants (Kaplan 1944/5; Schwabe 1944/5a).

Pottery of the Roman and Byzantine periods has been identified on the top of the hill, but since no architectural remains were clearly identified, Kaplan believed that the settlement of these periods shifted to nearby Kh. Habra, and the hill of El-Maghar served as the necropolis and stone quarry of Kh. Habra (Kaplan 1953: 141; Kaplan 1957).

Kh. Habra indeed produced important finds, among them a segment of a column of a synagogue from the Byzantine period, with an Aramaic inscription (Schwabe 1944/5b; Reich 1980; Naveh 1989: 307–308). Zvi Ilan surveyed the site in the 1980s and reported two marble capitals and fragmentary marble architectural decorations, typical of a public building (Ilan 1991: 267–268). A bilingual (Hebrew-Greek) tomb inscription kept in the von Ustinow collection, Oslo, originates from Kh. Habra (Vincent 1902). Kaplan suggested the identification of Kh. Habra with Kefar Hebron, a place mentioned in the Cairo Genizah (Kaplan 1948/9; but see Ilan 1991: 267–268).

It appears that without conducting a full archaeological excavation at El-Maghar, it is problematic to conclude with certainty that there was no Roman-Byzantine settlement. Furthermore, since El-Maghar has been inhabited until recent times, and its later inhabitants used to dismantle and reuse building material from earlier buildings (Sasson 2003: 23–25), this may have contributed to the obliteration of evidence.

Therefore, at this stage of the research, it is difficult to clearly relate the tomb excavated by Ory to the settlement at El-Maghar or to the one at Kh. Habra. In his reports, Ory also raised the possibility that the rock-cut tomb may have belonged to the residents of Kh. Habra. Additional support to Ory and Kaplan's view may be arrived at from a lithological perspective. Kh. Habra is located on the plain, on a low, alluvial hill slightly elevated from its surroundings. The nearest bedrock formations are exposed some 800 m east, on the slopes of El-Maghar hill. It would have been easier to cut in the rocky slope of the hill a family burial cave following the ancient custom, than to build it in the plain (with quarried material originating from the same *kurkar* hill).

Archaeological evidence for Jewish presence in the Judaean Coastal Plain during the Second Temple Period

As we have seen above, the epigraphic finds from El-Maghar – Kh. Habra area attest to Jewish presence during the Byzantine period. Kaplan and Ory's reports on discoveries of ossuaries and *kokhim* tombs are the only clear evidence for Jews living at these sites around the first century AD.

From historical sources we can learn that the area was inhabited by Jews during the Second Temple period. During the 65 years that followed the destruction of Jerusalem and its Temple, Yavneh started the most glorious chapter in its history, as the place of the Jewish leadership, headed first by Rabban Yohanan ben Zakkai and then by Rabban Gamaliel. We can assume that the surrounding Jewish region flourished during this period (see recent studies by Taxel 2005: 145–153; Shahar 2005).

The archaeology also provides some data that enable us to reconstruct a partial list of places which were apparently inhabited by Jews in this region (see Fig. 4). Limestone ossuaries or their fragments were discovered at

Umm Kalkha (Ory, MDA unpublished reports from 1932; Rahmani 1994, nos. 39, 40), at Yavneh (Shapira 1962: 12; this assemblage included 'burial chests made of clay, limestone ossuaries and second century AD oil lamps; decorated ossuary on surface of site: Taxel 2005:146), at Yavneh-Yam (Kaplan excavations 1968 – Rahmani 1994: nos. 874–876; Fischer 2005: 191, 195), at Hazav (Meiron 1969: 16) and Benaya (Meiron 1969: 16; the assemblage included also bones collected in storage jars). Excavations conducted at Yad Benjamin, east of modern Gedera revealed a typical hiding complex, common beneath Jewish sites in the Judaean Shephelah. It contained a limestone vessel, unsusceptible to impurity, among other finds from the Bar Kokhba Revolt (Weksler-Bdolah 2001). The site excavated at H. Hermas, west of modern Rehovot, produced among other finds a fragment of a stone vessel (Sion and Parnus 2006: fig. 10:1). A stepped and plastered installation, of the kind usually identified as a ritual bath (*miqveh*) was uncovered at Kh. Deiran, within Rehovot (Bushnino 2000: 94*–95*).

Although most of the data originates from sporadic discoveries, it provides us with reliable information in order to draw a map of places inhabited by Jews by the late Second Temple period, until the days of the Bar Kokhba revolt, in the Judaean Coastal plain. Similar research in the central parts of Judaean Hills and Foothills enabled us to identify some 330 landmarks (Zissu 2001).

Acknowledgements

The reports are kept in the Israel Antiquities Authority Archives, Rockefeller Museum, Jerusalem (British Mandate Scientific Files, file 127) and are published here with the kind permission of the IAA. Our sincere thanks are due to Arieh Rochman-Halperin and Yael Barschak of the IAA archives, for his professional assistance and to Professors Hanan Eshel, Ze'ev Safrai, and D. Benjamin Cahn for their advice. This study was supported by the Dr. Irving and Cerna Moskowitz Chair at the Martin (Szusz) Department of Land of Israel Studies and Archaeology, Bar Ilan University.

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Surveys and Excavations at the Nazareth Village Farm (1997–2002): Final Report

STEPHEN PFANN, ROSS VOSS AND YEHUDAH RAPUANO

Dedicated to the late Mark Goodman

For nearly two decades, the University of the Holy Land (UHL) and its subsidiary, the Center for the Study of Early Christianity (CSEC), has laboured to lay the academic foundation for the construction of a first-century Galilean village or town based upon archaeology and early Jewish and Christian sources. It was hoped that such a 'model village' would provide a 'time capsule' into which the contemporary visitor might step to encounter more effectively the rural setting of Galilean Judaism and the birth-place of early Christianity. At Nazareth Village this educational vision is currently being realized (for a popular publication on the Nazareth Village Farm project, see Kauffmann 2005).

The Nazareth Farm site discovery and survey

On a visit to Nazareth Hospital in November 1996, one of the authors of this paper (Stephen Pfann) identified an ancient winepress associated with agricultural terraces in a small valley about 500 m from the site of ancient Nazareth. This was located to the southwest, in an undeveloped sector of the hospital grounds and on adjacent land. Potsherds found on the surface of the terraces dated, in particular, from what appeared to be the Early-to-Late Roman Period. It was concluded that these terraces and the wine press were connected with the nearby original town of ancient Nazareth, located just to the east on the property of the Basilica of the Annunciation, in the heart of the modern city. The importance of this discovery was confirmed through an official archaeological survey that further advanced our understanding of the nature of the rural area directly associated with the ancient town.

A survey of the area, which covered approximately 15 acres, was subsequently commissioned by UHL/CSEC and was conducted in February 1997 by the institution's archaeological staff, under the direction of Ross Voss. The survey revealed, along with the aforementioned winepress and

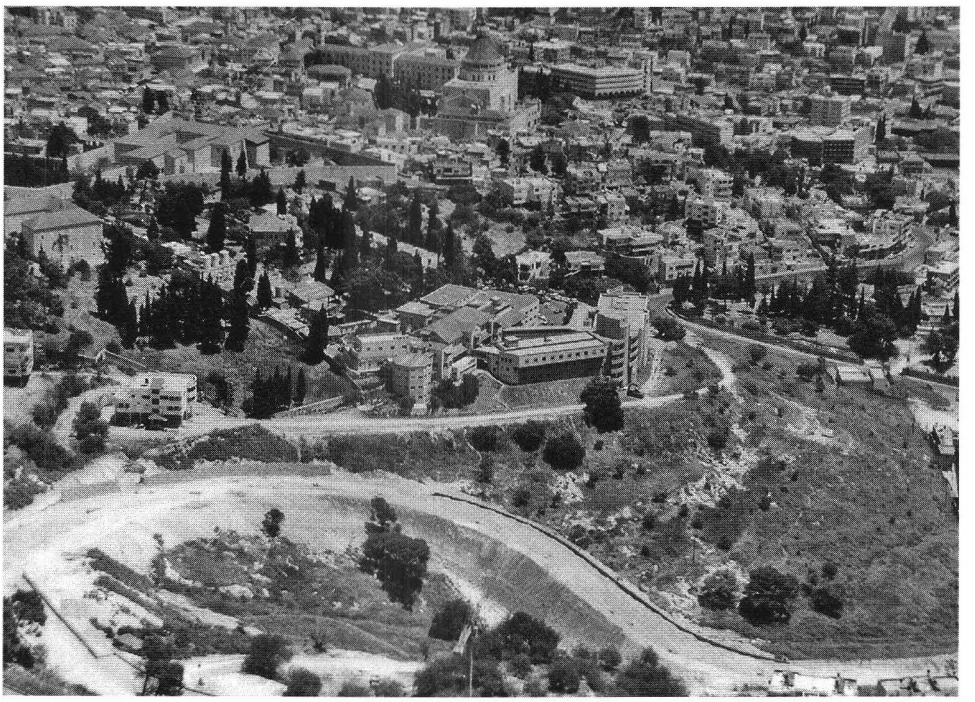

Fig. 1. Above: Basilica of the Annunciation, Nazareth Hospital; Below: Areas A, B and C of survey

terraces, remnants of three watchtowers, two olive-crushing stones, components of an irrigation system, and evidence for stone quarrying. Surface pottery spanned a period from as early as the Early Bronze Age (third millennium BC) to the Crusader/Ayyubid period (thirteenth century AD), with the predominant forms deriving from the Early to Late Roman period. This survey confirmed the present form of the landscape to be the remnants of a complete Roman period terrace farm, various parts of which continued to be utilized for farming until modern times. Two distinct components of the farm were tentatively identified which were defined by the type of terracing found there: a 'wet' farm (Area B), which depended upon access to springs or reservoirs for irrigation, and a non-irrigated 'dry' farm (Areas A and C).

It was concluded that excavation would be necessary in order to further define the nature of the ancient farm with the hope that the excavations would illuminate previously unknown aspects of terrace farming in the Galilee. Hitherto, little research had been undertaken on terracing and ancient methods of cultivation practised in the Galilee (Golomb and Kedar 1971). The remains of the farm were considered to be the most important, since they could potentially provide a key witness to the life and livelihood of the ancient villagers. The site also appears to be the last vestiges of intact

farmland that is certain to have been farmed by inhabitants of the town of Nazareth, which lay only 500 m to the east. The initial evidence concerning the character of the site indicates that the small valley and its slopes likely comprised the property of a single extended family, which produced a variety of crops. Much of the extent of the original farm has been preserved, although most terraces on the slopes facing the hospital to the west have been displaced by modern buildings.

The Nazareth Village Farm: initial survey

The archaeological survey of the surface of the land adjacent to Nazareth Hospital was conducted in February 1997, between coordinates 1778-2338 and 1788-2350 Nazareth Map 3089/0). The summary of the results of the survey of the three distinct areas A, B and C are as follows.

Area A: a dry farm

This area is located on the western and southern slope of the hill, below the hospital. The terracing was built upon a rock slope; this was cut previously by surface quarrying that seemed intentionally undertaken in order to

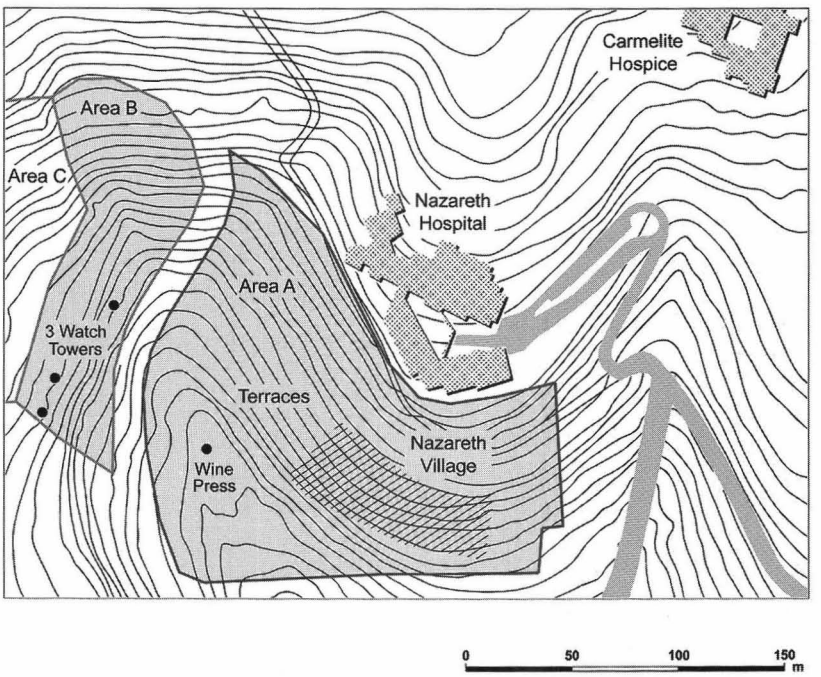

Fig. 2. Areas of Excavation at the Nazareth Village Farm

Fig. 3. Area A dry farm at time of survey

provide level foundations for the terraces as well as to provide quarry-stone for farm buildings or terrace walls.

The present terraces in Area A support olive trees that were cultivated in the first half of the twentieth century. Photographs taken by the Germans in World War I show olive trees growing on the hill with a thick covering of pine trees spread in the narrow valley below. Only a few pine trees remain; the rest have been cut down or burnt. Many of the olive trees have survived the periodic summer grass fires, but others were burnt, leaving charred stumps.

The revetments of the terraces are in a poor state of preservation, due in part to the type of terrace wall that was constructed. However, this does not mean that the terraces were poorly or carelessly constructed, as the following factors indicate. Firstly, a certain effort appears to have been exerted to level the stone surface during quarrying for the building of a number of terraces. Secondly, care was taken to produce a uniform oval shape and size of the stones. This ensured a certain uniformity to the construction of the terrace wall as well as uniform spacing between the stones in order to allow proper aeration and drainage of the soil of the terrace itself. Also, the soil layers of many of the terraces seemed to be fairly well preserved, providing a virtual stepped appearance to the eroded sloped hillside (although, for the most part, lacking or hiding the remains of the revetment of each terrace).

Concerning the terrace walls which were at least partially preserved: they were built of oval limestone fragments 0.30×0.20 m and smaller. The rough surface of each stone would suggest that these were not brought from surrounding wadis (where the rocks have smoothed surfaces). This suggests

that the stones were quarried and shaped from the local limestone with impact devices (perhaps hammers and hatchets), or else were field stones. Terrace walls which are made of rough stones tend to need more maintenance than those made of cut and fitted stones (as in a 'wet' farm). This would indicate that the type of farm that existed here was a non-irrigated 'dry' farm. This type of farm relies upon watering by rain and dewfalls that are supplemented by hand watering from run-off rainwater. The rainwater was channelled into the small pools (and perhaps cisterns?) that have been identified among the terraces.

The terraces, which are in a fragmentary state of preservation, seem, at this preliminary stage of our study, to have been layered with soils of at least two consistencies (as has been noted in other terraces found in the farms around Jerusalem: Gibson and Edelstein 1985; Gibson 1995: 134–136). The overall depth of the soil over much of this area is relatively shallow. This would suggest that vines were the primary crop intended for cultivation on such terraces. However certain terraces were deep enough to raise olive trees, many of which still survive on these terraced slopes today. Crops of the dry farms in the area would typically have been olives, grapes, figs, almonds, wheat and barley.

Observable structures on the site included a winepress, a base of a watchtower, pools with channels, agricultural terraces and stone quarrying, as well as a single cylindrical crushing stone. Evidence of quarried rock surfaces appeared in various places where the surmounting layers of terrace soils had eroded away.

Area B: an irrigated 'wet' farm

Situated to the southwest contiguous to the first area, but divided by a small water-worn valley and continuing across the full length of the slope facing the first area (interrupted in part by recently constructed homes). It is a homogeneous area built with terraces of sturdy construction. The revetment walls are built from semi-dressed stones carefully fitted together and strengthened with chink stones, leaning slightly backwards into the soil of the terrace. At two places water channels could be discerned in connection with a platform with well-built stones, which was conjectured to be a 'spring house'. One channel was connected with a cistern and the other descended to the better-built terraces below.

This type of construction normally supports what would typically be a 'wet farm', i.e. one that is irrigated directly from springs or pools. This allows the terrace to bear the heavier burden of water-laden soil for crops that require irrigation. Typical crops would include legumes and leafy vegetables. Most of these beautifully preserved terraces are also deep enough to allow the cultivation of larger trees. At the time of this survey, a grove of fruit-producing carob trees were cultivated on the lower terraces. This grove once continued along the southern slopes of the valleys as one can see carob trees growing

Fig. 4. Areas B and C at time of survey

in the 'green' areas between the modern building complexes. In the non-terraced valley below, there is a stand of Aleppo pine trees, remnants of a small forest of trees that once filled the uncultivated bottom of the valley.

The ruins of three watchtowers surmount the walls of three separate terraces (for the study of rural watchtowers, see Ron 1977; Dar 1986; Gibson and Edelstein 1985: 144–145). One terrace was identified as being conspicuously built of well-crafted stones with monumental characteristics. Observable structures were three watchtowers and agricultural terraces, possibly the foundation of a farmhouse, channels, a threshing floor and a tomb (all of which still need to be investigated). There was also a single cylindrical crushing stone.

Area C: an additional part of the dry farm

Above and to the west of Area B lay a series of dry farm terraces that originally ascended to the crest of the hill. Earlier construction of private homes, the recent construction of a road and the current construction of apartments has either covered or obliterated most of the terraces associated with this area. Three of the remaining terraces were investigated, providing information on their history. Pottery from the first to the third centuries and from the eleventh to the twelfth centuries AD was found. Local residents remember beans, lentils and carobs being harvested as recently as only a few decades ago.

The area immediately above and to the west of Areas B and C is still called in Arabic *al Kurum,* 'the vineyards'.

The GPS mapping survey

The survey of the above features was undertaken by GPS mapping procedures over the course of two days in April 1997 by Mordechai Haiman (Israel Antiquities Authority) with the participation of Voss. Many of the terraces existing in Areas A and B were plotted, though not all were examined, due to the heavy vegetation. The survey registered for the first time this part of Nazareth's ancient historical landscape of terraces and agricultural installations. Their discovery and recognition heightened the urgency for excavation in light of accelerated housing and road development currently removing or covering many of the surveyed and unsurveyed features.

In all, four successive seasons of excavation were carried out at the site between 1997 and 2000. The discoveries from this excavation and from the cleaning of the more eroded terrace areas in preparation for the construction of the Nazareth Village are the subject of the present report.

Geological features of the Nazareth Village Farm and the Nazareth Ridge

The geological formation that underlies the soils of the Nazareth Ridge is comprised of a relatively thin layer of semi-hard Eocene limestone forming a crust over a thick layer of Senonian chalk. In addition to this, wherever the chalk has been exposed over an extended period to temperate or wet weather conditions, the surfaces harden to a form of rock known as *nari.* Both Eocene limestone and *nari* are useful as building stones when quarried and comprise the primary building stones found in the ruins of the archaeological sites in the vicinity, namely Nazareth, Sepphoris, and Jafia.

The natural soils are calcareous in nature since they derive from the local limestone, Eocene limestone, producing a soil known as 'Mediterranean brown forest soil' or 'brown rendzina'. This soil is rich and does not have the same problems of clumping common to the terra rossa soil to the south and the north, or the stony consistency of the basaltic soils to the east. This is the primary type of soil, which predominates on the rocky slopes of bedrock in valleys and potholes as a fine deep chocolate brown that whitens as it is diluted by the eroding limestone or by the rock debris from the local quarrying. The Senonian chalk produces 'white rendzina', a highly calcareous and relatively infertile soil that is exposed only in patches and so is hardly noticeable.

Summary of the excavated areas

The following is a summary of the stratigraphy and architecture of the separate excavation areas (F = feature; L = layer):

Area A-1

Agricultural terrace and stone quarry (F16) (Figs. 5 and 6; for the pottery finds, see Fig. 37:2–7). A section of the agricultural surfaces and wall surfaces of this extended terrace was cleaned. A metre-wide trench was cut from the back of the terrace to the terrace wall. Four of five of the original layers of the soil remained preserved. There is evidence that the terrace walls underwent partial collapse and rebuilding more than once during the period of use.

Layers (counting from bottom up from the bedrock/quarry surface): Layer 1: crushed Senonian chalk (Munsell Chart reading: very pale brown: 10YR 8/3); Layer 2: Mediterranean brown forest soil (dark brown: 7.5 YR 3/2; with 10% small to medium limestone grains); Layer 3: crushed Eocene limestone debris (white: 10YR 9/1 [off chart]); Layer 4: Mediterranean brown forest soil (brown: 7.5 YR 4/3; with 10% small to coarse grains); Layer 4a: mixed recent soil and debris.

What appeared to be a limestone tile was found in the course of excavating the trench and was left projecting from the western baulk in the lower dark layer (L2), which contained potsherds typical of the Early Roman Period (cf. Fig. 5). The character of the stone matched that of the local *nari* or Eocene limestone which is also typical of the upper exposed surfaces of the Nazareth Ridge rock formation. The tile was approximately 3.2 cm thick. After extraction it became clear that the tile had two side edges that were smooth and well cut at a right angle, while the remaining sides were unworked and rough. The upper and lower surfaces were levelled but unsmoothed. The tile was evidently a rejected remnant cut from an actual tile intended for use. Another tile remnant of similar form and dimensions was found during the excavations in Area A-3.

Below the soil layers of the terrace lay a rock surface that was evidently quarried not only to provide stone for the stone masons but also in order to provide an elongated horizontal, semi-level surface as a base for agricultural

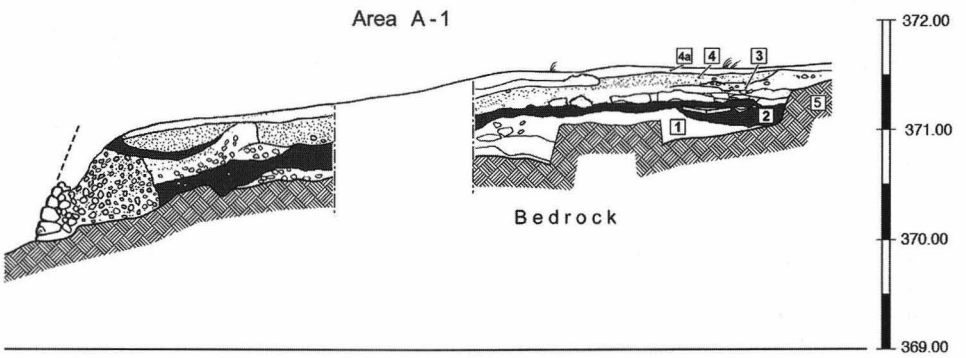

Fig. 5. The multi-layered terrace of Area A-1

Fig. 6. Area A-1 terrace and quarry; quarry and Area A-3 above

terraces. This practice vastly extends the potential surface area fit for agricultural terraces on any rock slope. At the bottom of the trench towards its northern end was found the rim of a Kfar Hananiah Type 3B casserole dating to the Early Roman Period (circa first century AD; Fig. 37:3). The date matches the date and character of the other potsherds in the lower layers of this terrace.

Evidence for the extended quarrying of this general area can be found in the surrounding rock surfaces. There one finds the typical squared blocks with cut separation channels for ashlar removal. The quarrying was deliberately carried out in such a way as to leave a horizontal floor for the base of the agricultural terrace. (Surface quarrying such as this would normally be done parallel to the diagonal surface of the rock, not horizontally.) Rock debris and rejected chips were also found as evidence of the quarrying process.

At the same level, at the northern extreme of the terrace and below the escarpment which ascends to the next level above, was a low rectangular raised stone platform ($0.1 \times 1.6 \times 3.5$ m) with a carved post-hole at its corner (inside of which hatch marks were visible). This likely provided a shaded work area for the stone masons and subsequently for farm workers. A 1 m squared rock depression immediately to the west was excavated.

While cleaning this uneven rock surface, a horizontal handle of a crater (?) dating to the Late Hellenistic or Roman Period (Fig. 37:2) was found.

The stone terrace wall was preserved in places nearly up to its full height. However in certain places, including the portion that was cut by our trench, there was evidence of rebuilding of the terrace wall and part of the terrace behind it. It is here that the pottery, including one piece of early Ottoman porcelain, was found.

Adjacent to the excavated area, to the east, was a large raised stone platform that projects 2 m from the terrace above into the terrace below. This likely formed the base of a watchtower whose stone and soil superstructure is now missing. The tower would have provided an overview and protection for the crops on the terraces of the immediate area.

Area A-2

A wine press was uncovered in this area (F27) (Fig. 7, 8 and 9; pottery Fig. 38). The main pressing floor (floor 1) was well-cut, square (2.5 × 2.5 m), and levelled into the surface of the Eocene limestone. The floor is accompanied, to the south, by a collecting vat with a sump. The smooth, somewhat bleached surface is not at all preserved in the middle of the floor. This may be due to a secondary use of the floor as a channel for rainwater or as a crushing surface for olives (note the crushing stone below), as well as the natural erosion of the rock surface.

A second adjacent floor (floor 2) was located immediately to the east (2.5 × 2.2 m). The surface is badly eroded, making the original dimensions and use of the floor difficult to establish. Although there is a rough depression below and to the southeast of this floor (which could have been used as a collecting vat), the rock surfaces are natural and unworked. Floor 2 may have been used as a gathering point for grapes that were about to be pressed in floor 1. Alternatively, floor 2 may also have been used as a second pressing floor when floor 1 (or its collecting/fermentation vat) was occupied. Rough-hewn presses (as well as those using natural depressions) are not uncommon in ancient terrace farms (Frankel 1984; Gibson 1995: 90).

The presses are difficult to date since the accumulation of soil and datable material (e.g., pottery) which was excavated from them represents material which was deposited there after the presses went out of use (likely terrace erosion from terrace F26 above, see *infra*). No mosaic floor or even tessarae (mosaic tiles) were found which would commonly be associated with Late Roman to Byzantine wine press floors. Modern debris was found on the surface. Late Hellenistic to Islamic pottery was collected with the usual predominance of Early and Late Roman pottery (Fig. 38:1–6). In the lower part of the accumulation near the rock surfaces, many potsherds with the typical ribbing of the Early to Late Roman Period were found (which are typical of the fill found within the terraces on the hill above the presses).

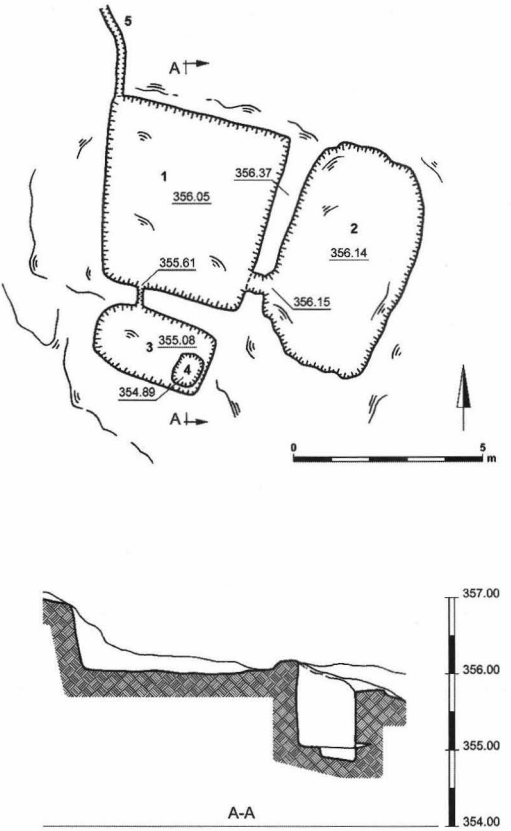

Fig. 7. Wine press plan and section: 1: pressing floor; 2: holding area; 3: collecting vat; 4: sump

Fig. 8. Pressing drum from below Area A-2

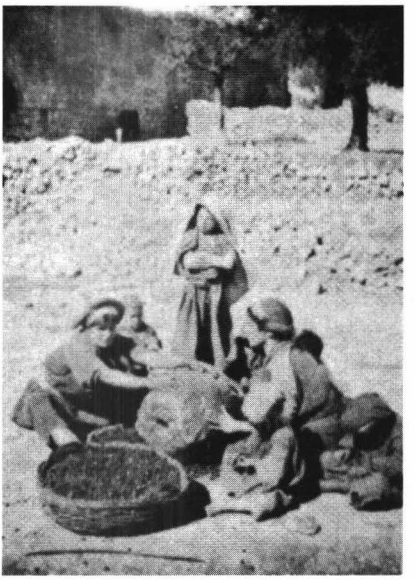

Fig. 9. Local residents using drum press at beginning of 20th century (from Dalman 1928–42: IV, Pl. 47)

During a later period (post-Roman) the press was no longer used for wine production. It may have served as the floor of an olive press during other periods, taking into account the damage in the middle of the pressing floor. One cannot rule out the possibility that the floor was utilized in the off season for this purpose while the wine press was still in use. Still later, the pressing floor, along with the vat and other rock depressions, were utilized to gather rainwater to supplement rain and dew fall on the farm or to water thirsty animals. Small channels were cut into the rock surfaces to gather runoff into the pools.

A number of shallow horizontal recesses are in the immediate vicinity of the press to the north and to the west. Twelve metres to the west a natural cave was found (2.2 × 2.5 × 40 m) containing the bones of a small carnivore.

About 30 m to the south and below the press, on the surface of the ground, one of two crushing stones was found. It comprised a cylindrical drum with the ends rounded off (cf. Fig. 8). The practice of using rolling stones for crushing grapes and olives extends back into antiquity, and was still widespread in the early twentieth century (cf. Fig. 9) (Dalman 1928–42: IV, 183, Pl. 47; Frankel *et al.* 1994: 97–98).

Area A-3

An elliptical terrace (F10a) (Figs. 10 and 11; pottery Fig. 39). The vestiges of this U-shaped terrace can be traced about 25 m along the hillside. The eroded

Fig. 10. Area A-3: A. remnants of revetment. B. eroded embankment, C. later retaining stones

face of its terrace wall (F10a) was cleaned. The 0.20 × 0.20 to 0.30 × 0.30 m sized, *nari* lower stones of the stone retaining wall were preserved in some places. A more recent row of similar sized stones was found parallel to and above and slightly inside what would have been the original top of the stone terrace wall. It was decided that this line of stones was not part of the original wall since there were no stones below it but only the earthen embankment of the original wall (made of earth and 0.05 × 0.05 to 0.20 × 0.30 m

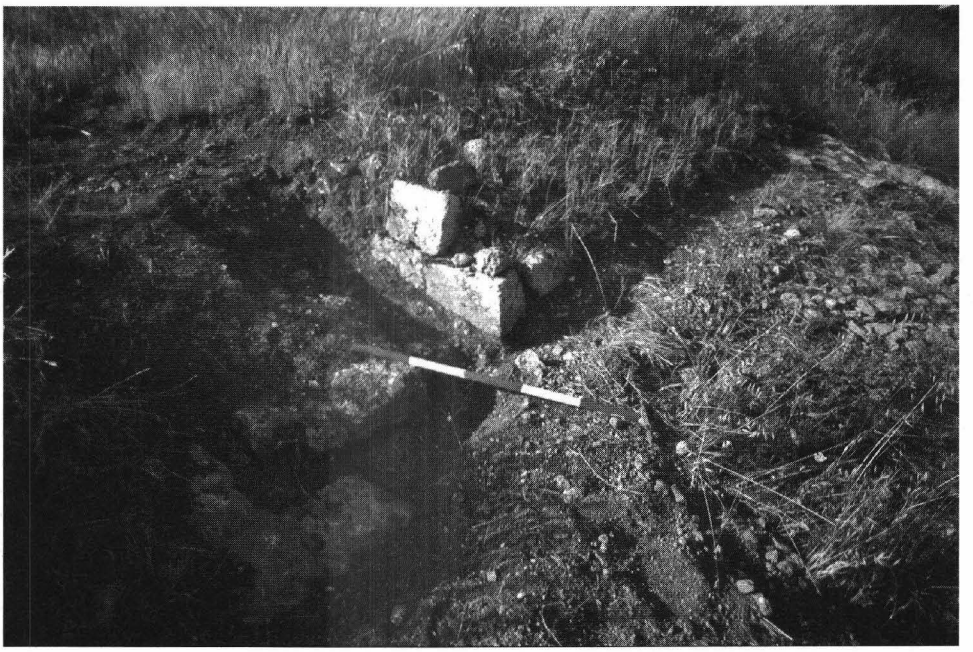

Fig. 11. Staircase in Area A-3

stones). Evidently, at some point in history the collapsed terrace face was not rebuilt. Rather, only a line of stones from the collapse was set into the top of the embankment in order to protect the layers of agricultural soil from washing away during the yearly rains. (This practice was applied to other terraces at this farm but none so well defined as here (cf. Gibson 1995). No trench was cut to investigate the stratigraphic layers of this terrace. Potsherds were collected during the cleaning of the terrace face, confirming the predominance of Early and Late Roman pottery in this area (Fig. 39:1–3).

At about mid-point, the remains of steps (F10b) leading from the terrace below were cleared; only a portion (one squared stone) of the lowest step remained. However, the remains of the eastern sidewall of the staircase were preserved by several well cut and fitted stones (cf. Fig. 11).

Area A

Terraces and stone quarries were investigated in this area (Figs. 12–18; pottery Fig. 37:1).

During the 1997 and 1998 excavations the overall form of the local 'wet' and 'dry' farm and its history was determined. A typology of terrace form and construction was also established, relative to both the internal and external structure of terraces.

Fig. 12. Terrace wall clarification in Western Area A

Excavations in 1999 and 2000 were conducted exclusively in Area A, the hill on which the Nazareth Hospital is situated. Since the primary focus in 1999 and 2000 was the restoration of the agricultural terrace system revealed in 1997 and 1998, all efforts were directed toward excavation of at least part of each remaining agricultural terrace that was undergoing restoration or consolidation. The first step in this process was to excavate the stone and earth which had spilled out of partially collapsed terrace walls. Removal of this collapse enabled us to trace the actual extent of a particular terrace and assist in its eventual restoration. In most places the upper courses of the terrace-facing wall had collapsed, leaving a deposit of cobble-sized stones and eroded agricultural soil banked against the remaining face of the lower courses of intact terrace wall. This embankment served to hold the rest of the terrace wall in place and helped to prevent further erosion as long as the bedrock beneath was not too steep.

In certain terraces, the earthen embankment is difficult to discern since the contents of the structure, lacking the additional crushed lime and pebble/cobble mix of other terraces, did not differ significantly from the agricultural layers in consistency. In such cases, at some point in the history of the terrace, both the revetment and embankment collapsed, leaving the terrace builder to rebuild the revetment wall afresh, but this time directly

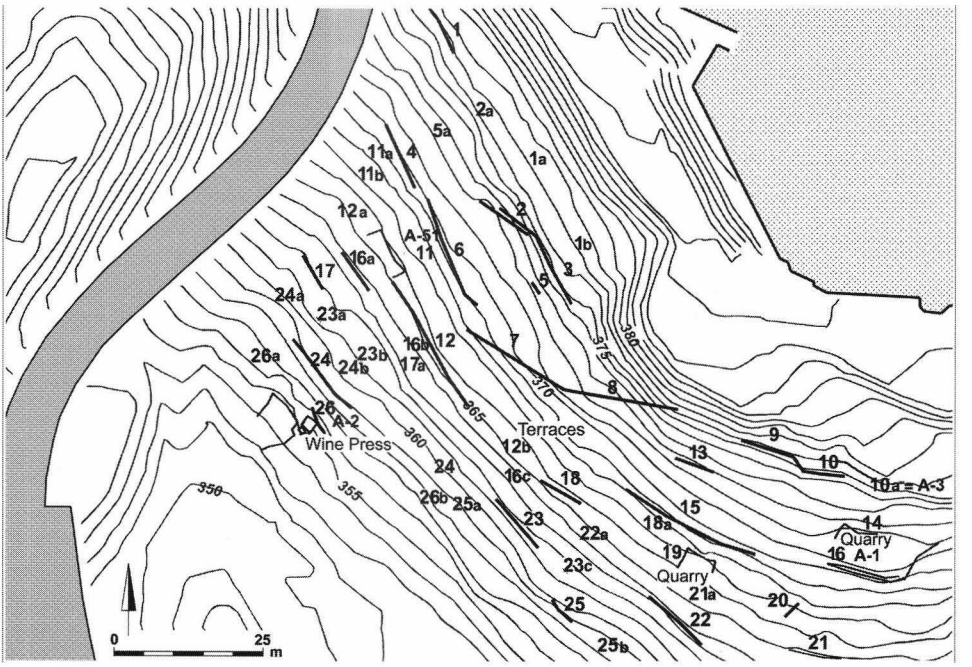

Fig. 13. Initial top plan of the features of Area A

against the exposed remains of the agricultural layers, causing the line of the terrace wall to recede.

At certain farms of the Early Roman Period, and especially later during the Byzantine Period, the stones that were used for the terrace retaining walls were hard and heavy enough to be crafted with flatter upper and lower surfaces so as to allow for the creation of a free-standing stone retaining wall. The strength of these walls allowed for the importation of soils during a terrace's construction without risking the collapse of the wall. On the other hand, the stones of the terraces at Nazareth are derived from the local *nari* and chalk, which is more friable, and are crafted with rounded surfaces. This does not allow for the creation of a sufficiently stable, free-standing wall that could withstand on its own the task of importing and layering the agricultural soil layers and then continue to fulfil its function to retain the heavy soil layers. Even the significant terrace wall of the wet farm F7 in Area B-2, with its larger, relatively well-crafted stones, was likewise built as an ensemble, with its revetment leaning against an embankment for support (see below).

If the area was subject to significant water infiltration, large stretches of the face of the terrace often collapsed. Despite this problem, the soils behind the collapsed terrace wall usually remained intact because the triangular shape of the cobbles and pebbles which backed the terrace served as an effective

Fig. 14. Retainer rows of stones atop an eroded revetment and rampart

embankment to hold the farm soil in place (see the section drawing of terraces in Gibson 2001: 114, Fig. 4.1). Even though these spills excavated from the front of the terraces were of an eroded nature, the ceramics recovered from them provide a relative *terminus post quem* date picture of the periods in which the land was cultivated (on the methods of dating terraces, see Gibson 1995: 160–164).

To facilitate the description of the terrace system in Area A, each major horizontal terrace step or line of terraces was assigned a number. For example, step number 1 begins at the top at the northwest end of the hill and extends east at more or less the same elevation. Each descending step follows the next consecutive number, with numbered sub-divisions which cover individual terrace segments (see Appendix 1). These and the quarry features along them are approximately 30 m in length, extending in an easterly direction. Each terrace wall, quarry, or other feature was given its

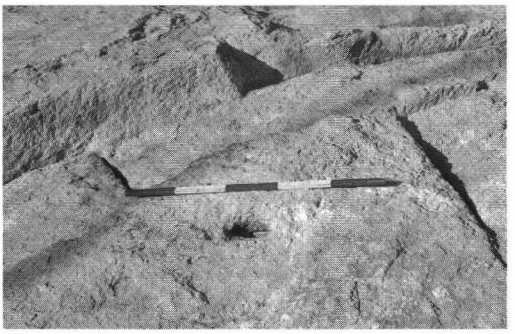

Fig. 15. Unused chisel socket in Area 3A-1

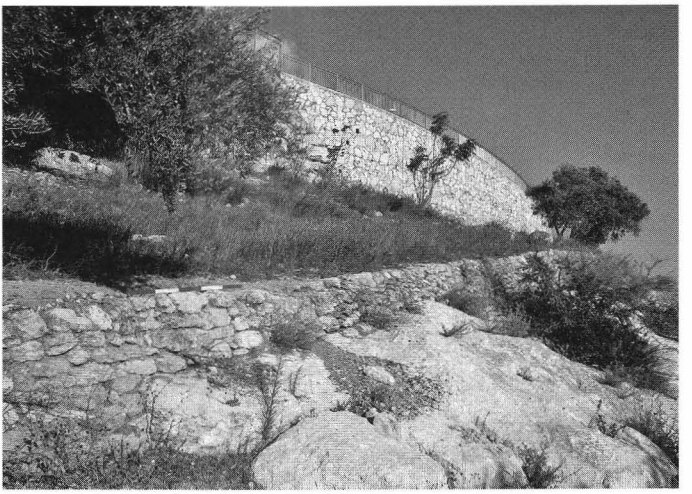

Fig. 16. Step 3-3: Road toward Nazareth (revetment wall restored)

own feature number and is referenced therefore to the step and sub-division of its particular area.

The farm, the valley and its terraces

A close examination of the hill where the present terraces are located indicated that less than half the number of terraces that once covered the hill in antiquity are still in place. Approximately 35 major lines of terrace walls or formations are currently extant. These terraces are spaced 4 to 5 m apart from one another in serried fashion down the slope of the hill. The excavation revealed foundation lines in the bedrock where other terrace retaining walls had once existed but are now completely eroded away. These missing terraces indicate that in antiquity there were perhaps double the number of terraces that exist at present.

The ancient terrace system consisted of many more steps of narrower plots of land separated from each other by approximately 2 m intervals. The long, relatively narrow, and multilayered upper terraces would have been best suited for viticulture, as the rock-hewn wine press at the bottom of the hill suggests. Lower elliptical terraces could be utilized for growing trees such as the olive, which would have been planted in locations where the roots could grow deeper (cf. Fig. 33).

Most of the terraces reveal several stages of repair or complete rebuilding easily discernible by the variations of stone patching and the incorporation of modern building materials in some of them. Many older terraces are still in place but buried under earth eroded down the slope. These ancient terraces still retain their original farm soils. They have generally survived because the

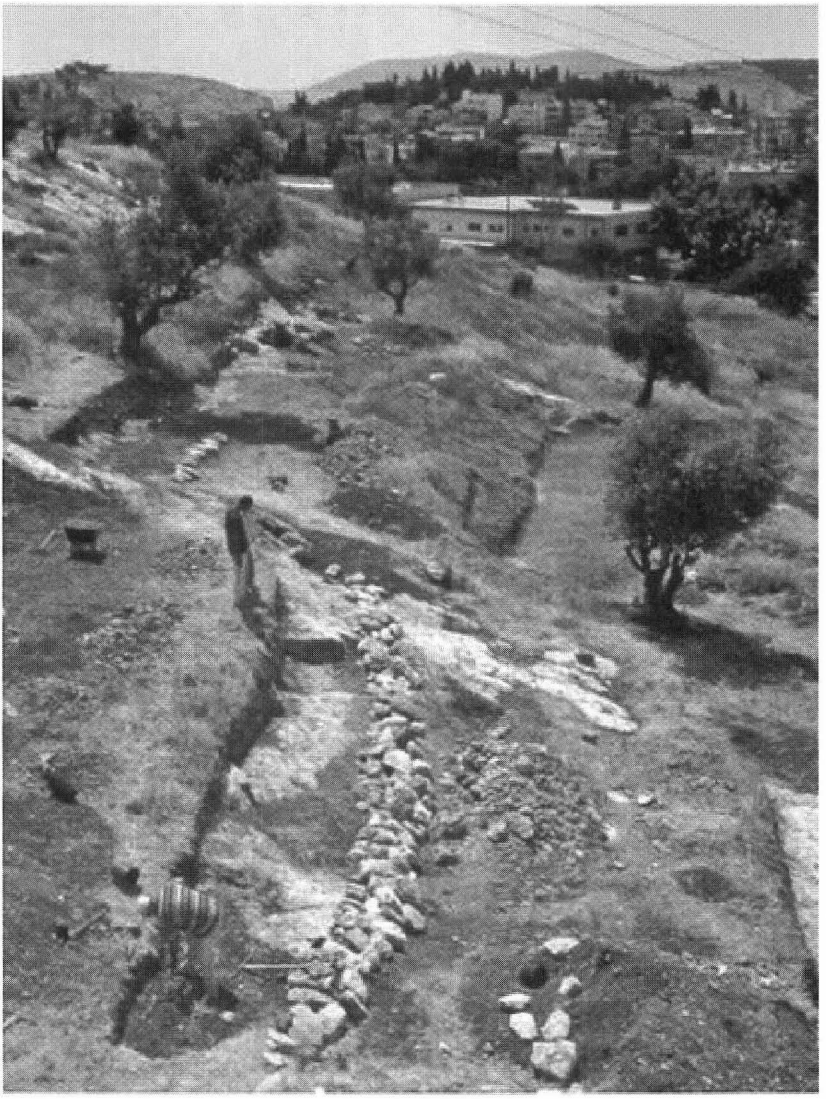

Fig. 17. Elliptical terrace 16a (Step 5-1) during excavation

walls were securely anchored on levelled bedrock, especially in areas of ancient quarry cuttings. In a number of instances there is evidence that stop-gap measures were taken to impede the erosion of the inner agricultural soils of the terrace. When the revetment and embankment had deteriorated it was common practice to place a line of 0.20×0.20 to 0.30×0.30 m sized stones at the forward edge of the terrace to keep back the soils (see Gibson 2001: 114, Fig. 4.1).

Fig. 18. Step 7-3: Remains of ancient revetment wall and agricultural soils

The stone quarries

Both Eocene limestone and *nari* are useful as building stones when quarried and comprise the primary building stones that are found in the ruins of archaeological sites in the vicinity, including Sepphoris and Jafia. Since the Senonian chalk lies only 1 to 2 m below the surface of the bedrock in most places, the semi-hard limestone must be cut from the rock slopes riding just above the chalk layer by a method that might be best expressed as 'surface quarrying' or as an ancient form of 'strip quarrying', forming pits across the rounded slopes like dimples in a golf ball. The cuts are normally squared and fairly uniform to produce stones approximately $0.60 \times 0.40 \times 0.30$ m (cf. especially the quarries, blocks and tiles of Areas A-1, A-3, C-1 *supra* and Steps 3A-1).

Numerous quarries were cut into the rock face. More than half of the hill exhibits quarry activity, which demonstrates quite well that the hill was bare of soil prior to its conversion to stepped terrace farmland. The quarry activity was abruptly interrupted when the decision was made to convert the land to terrace agriculture. This is seen by the number of partially hewn ashlar blocks left *in situ* but now covered by soil or terraces. The pristine condition of the stone blocks and ashlar negatives at the base of the quarries is another indication that the quarries were soon covered over and not left exposed to the weather. After the terrace retaining walls were constructed,

the soil had to be brought up the steep slope in baskets on the backs of donkeys (on the local transportation of fills for terraces, see Gibson 2001: 114, and references there). There are indications that the quarrymen anticipated the conversion of the hill to terrace agriculture by the way in which extraction of some of the ashlar blocks left narrow strips of levelled bedrock onto which the terrace walls were constructed. Also in the larger quarries inset deeper into the hill, limestone chips and chalk debris left over from the stone extraction were reserved and used as bedding fill under the agricultural soils.

The quarry stones were cut using chisels and adzes. Chisels were placed in a hole with wooden wedges to break blocks of stone away. A water-logged wood technique would not have been feasible in such a climate.

Area A: finds made during the construction of the Nazareth Village

Various finds were made during the construction of the Nazareth Village Project in 2000–2002 and were recorded by Mark Goodman. These conprise a number of unstratified finds including a coin and pottery vessel fragments from Area A (Figs. 19 and 20).

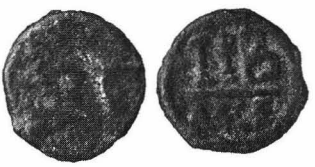

Fig. 19. Coin of Tiberius II (578–82 CE)

The following coin was a surface find in Area A:

12 nummi of Tiberius II (AD 578–582), minted in Alexandria.
Wt.: 1.96 gm; diam.: 14 mm.
Reference: Bellinger (1966, 1: 286, No. 56).
Obv: Bust of Tiberias II draped r. with cross on diadem
Rev.: I B with *CHI RHO* (staurogram) between, on two steps:
Below in ex.: ALEX; which reads: ALEXANDRIA

This represents the latest Byzantine coin that has been found in the Nazareth area.

From Bagatti's excavations in Nazareth 4 coins were found, all Byzantine (mid-fourth to early fifth century) and 2 coins from the vicinity: one Late Roman (the earliest coin, mid-third century) and one Byzantine (late fifth to early sixth century). These were recorded as follows: Grotto No. 25: 3 unidentifiable Byzantine (one with head of Emperor; two very small,

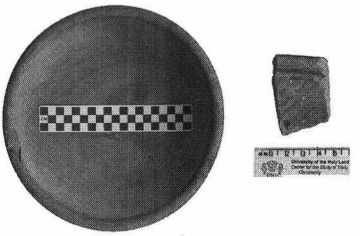

Fig. 20. Gaza ware bowl and fragment of Early Bronze III platter

typical of late fourth to early fifth century AD) (Bagatti 1969: I: 46). Grotto No. 29 (embedded in the plaster): one with head of Emperor, apparently Constans (AD 337–350) (Bagatti 1969: I, 210, Fig. 172). In addition there were finds from the village: one coin of Anastasius (AD 491–518) (Bagatti 1969: I, 234). Surface find from ploughing the land around the village: one coin of Gordian III (AD 238–244) (Bagatti 1969: I, 251). More than 60 other coins from the Islamic to Mamluk Period were unearthed in the 1955 excavations (Bagatti 1969: II, 194–201). In addition, 165 coins were uncovered by Yardenna Alexandre in the 1997–1998 excavations at Mary's Well, Nazareth. The coins were overwhelmingly Mamluk, but also included a few Hellenistic, Hasmonean, Early Roman, Byzantine, Umayyad and Crusader coins (Alexandre, forthcoming).

The unstratified pottery vessels included a complete Gaza Ware bowl (Fig. 20), which was found during the clearance operations which preceded the construction of the Nazareth Village. Data: Diameter: 26 cm. Height: 7 cm. An Early Bronze III platter fragment (Figs. 20, 39) was found along the path. There have been a number of non-diagnostic potsherds that were suspected to have been from the Early Bronze Age, based upon clay consistency and manufacture. This, however, is the first truly identifiable form to come from that period. According to the late Douglas Esse (1991: 45, 76–83), writing about the Southern Galilee area, this form of platter with net burnishing is typical of the Early Bronze III and is distinct from those of the Early Bronze II (as is also the case with the Khirbet Kerak Ware with string cut bases on bowls). The indentation on the underside of the vessel, below the rim, is typical of platters relatively late in the Early Bronze III (see also Braun 1996). In addition, the upper stone of a push grinder (partial) was found in the fill. To date this is the only push grinder to be published from the Nazareth area.

Area B-1 and B-2

The lower half of a large hill bordered by a spring-fed wadi on its north and east sides, Area B-1 and B-2 is now covered by a modern road

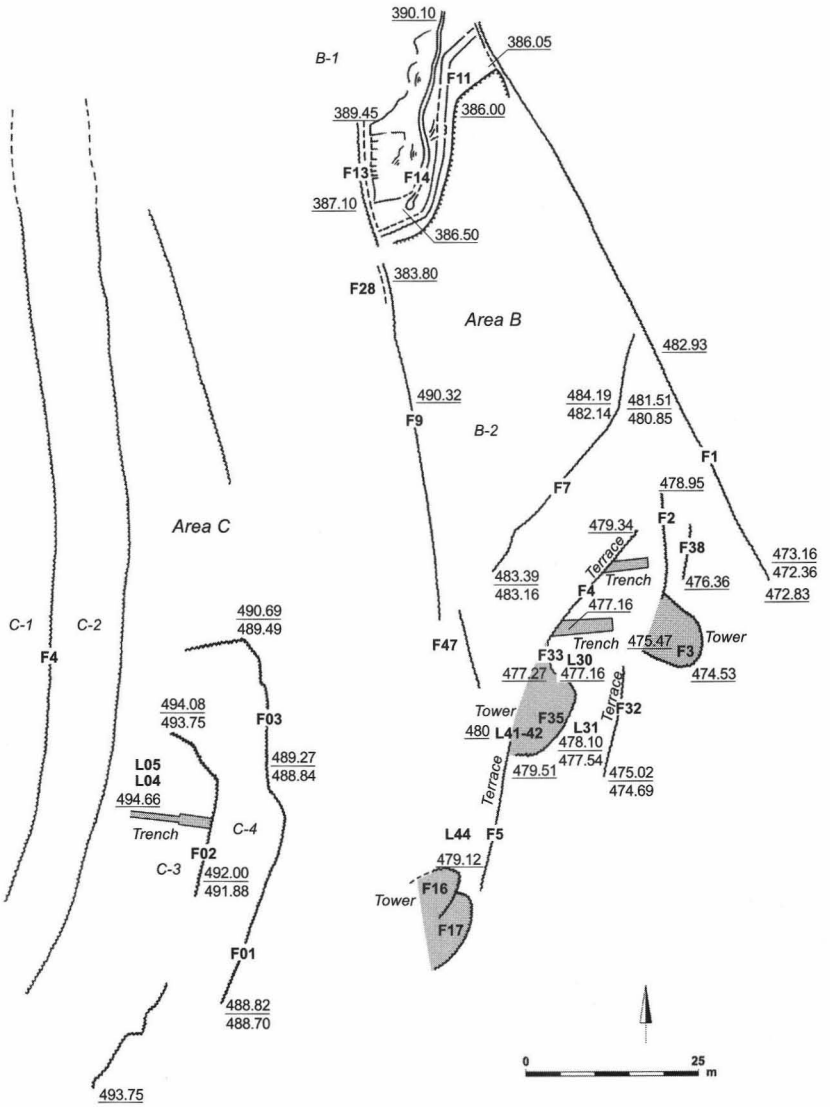

Fig. 21. Map of Areas B and C

construction which arcs around and separates the higher reaches of the west side of the hill. For the purposes of the survey the lower half of this hill has been designated Area B. The southern border of Area B is defined by a housing development and a saddle in the topography to the southwest of the same hill which rises to another set of terraced slopes designated Area C.

The natural topography of the hill consists of a series of broad arcing limestone plates. These plates widen at the lower elevations as one approaches the wadi. In Area B, the edges of four broad plates are crowned by large, strongly built terraces which are their most conspicuous feature. Smaller subsidiary terraces are spaced at intervals of approximately 5 m. The lowest major terrace, F1, has lost most of its farm soil but the line of the terrace is still intact. The next terrace rise to the west, F2, has an attached watchtower (F3). The next line of terraces, F4 and F5, begins approximately 5 m west of F2. These terraces bond to watchtower F6. Both terraces retain a substantial amount of farm soil, as yet unexcavated. The biggest terrace, F7, supports the largest field. This terrace extends from the wadi channel running in a southwesterly direction for approximately 36 m before it abuts terrace F8. The field supported by this terrace is 25 m wide and 45 m long. F8 extends 12 m from the south end of F7 and is situated approximately 5 m west of tower F6 at an elevation 1.5 m above tower F6 and its associated terraces.

Terrace F5 continues south for 20 m where it abuts the semi-circular buttress F17 supporting the east face of watchtower F16. Five metres west of F5 terrace wall, F25 extends south approximately 25 m before abutting the north face of watchtower F16. Each terrace currently retains farm soil layers. The last great terrace associated with watchtower F16 is terrace F26. This terrace bonds to the northwest end of the watchtower and then extends north for more than 35 m. Part of this terrace has been disturbed by a subsidiary access road associated with a new housing development which has carved away the higher slope of the hill. The farm soil behind the terrace is now covered by this hopefully temporary road.

The northern and western limit of Area B (B-1) has been delimited by the scree of limestone boulders and cobbles spilling from the new road winding down through the wadi. This road has severed the connection of the ancient spring and the wadi channel, and has covered the north end of the terrace system which fronts the wadi. Four terrace walls (F10, 11, 12, and 13) enclose an area approximately 30 m long by 9 m wide. A cistern (F19) and channel (F14) were visible under the heavy plant cover fronting the east side of this exposed limestone plate. The sharp 3 m drop in elevation

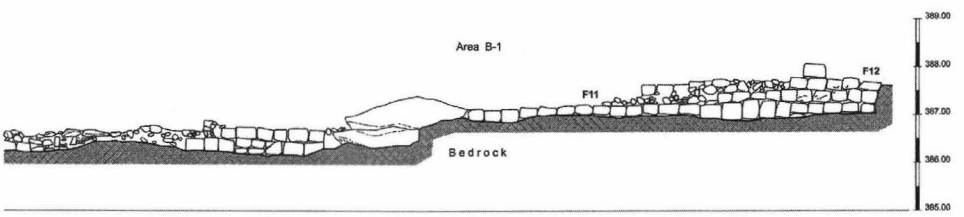

Fig. 22. Main terrace wall F11 of Area B-1 likely contemporary with the terrace rebuilds.

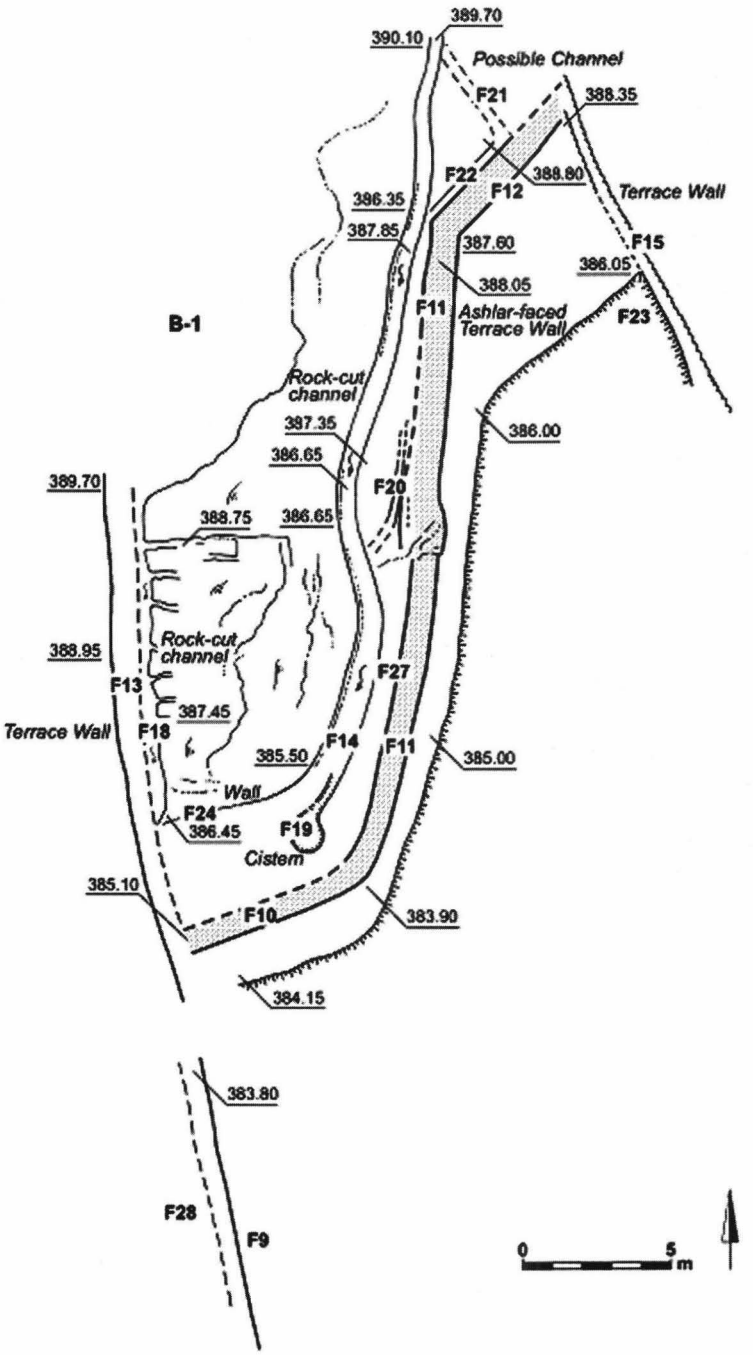

Fig. 23. Plan of Area B-1

from the edge of this plate to a rock-hewn enclosure (F23) adjacent to the wadi but separated by a 1 m thick barrier of bedrock is suggestive of a spring house.

Area B-1

Excavation

Excavation in Area B-1 involved the removal of a heavy layer of plant overgrowth covering a variety of features associated with 'wet' farm terrace agriculture (Figs. 21, 22 and 23; pottery Fig. 40). The plate on which these features were preserved was 35 m long and 13 m wide. The northern end of this plate – now covered by a scree of limestone boulders and cobbles cut out of the higher slopes for a road – was originally bordered by a spring-fed wadi channel. The eastern face of this plate ends in a sharp vertical drop, apparently the result of major quarrying. The northeastern exposure drops more than 3 m. This area is partially enclosed. A line of bedrock separates this deep exposure from the wadi bed 1.5 m away. A terrace wall (F15) rests on top of this bedrock border and fronts the wadi channel. This semi-enclosed 'basin' (= F23) is approximately 6 m wide. Its overall dimensions are unknown due to the soil which has washed in and filled it. Water had evidently flowed from a rock-cut channel (F21) following a defile in the rock which spilled into the catch basin (F23). Whether this installation is a spring house or just an abandoned quarry remains to be determined. A 2×2 m probe along its northwest face (L3) exposed a layer of softer chalk underlying the harder limestone. More than a metre of chalk had been quarried out, but where the cutting stopped the surface was left rough, showing no sign of preparation for holding water. A much larger exposure will be necessary to determine if there was an opening hewn into the rock through the chalk, and to see if the bottom of the quarry was plastered in any way to collect or store water. L3 consisted of colluvium with numerous ashlars. These were sitting in brown soil, all of which had washed down off from terrace walls F11 and F12 at the end of the terrace plate. Modern pottery (Bas No. 3), including bathroom tile fragments, roof tile fragments, and a Gaza water jar rim fragment, were predominant. Several sherds of Roman Period pottery were also recovered including a handle and two body sherds. A large bone, probably the leg of a cow or ox, is notable.

The eastern terrace walls

Three distinct terrace walls crown the south and eastern edge of the bedrock plate. From south to north these are F10, F11, and F12. All three were built of a combination of ashlar stones lining the inner and outer faces

of the walls with a rubble core of chalk stone cobbles and pebbles. These walls, which average 1 m in thickness, are secondary terrace constructions, not original to the modification of the bedrock plate and channel system on which they currently sit. All of the ashlar stones making up these terraces are in secondary use. Many of the stones represent voussoirs, ashlars which were part of vaulted arches now robbed from some earlier architectural unit, the location of which has not been discovered. The bulk of the ashlars making up the outside face of these three terraces were taken from walls. The average size of these ashlars is $0.30 \times 0.30 \times 0.25$ m, while the average size of the voussoirs is $0.20 \times 0.30 \times 0.25$ m. Another clue as to the secondary nature of this terrace construction is the fact that each wall either sits in a rock-cut channel or blocks the outlets from these channels. For example, three separate outlets from channel F14 are blocked by ashlars (= F27) along the east side of the channel. F11 actually has its foundation course along its southern end resting in a rock-hewn channel. This provided a secure footing for the terrace but ended the channel's use as a conductor of water. Another channel (F14) was modified so that all water was conducted into cistern F19. At least three original openings through the bedrock leading east off this channel were plugged with ashlars (F27). These three plug walls reused voussoirs to shut off the flow of water, presumably to the next lower terrace plate. The northernmost plug of F27 which is adjacent to terrace wall F12 also cut off the flow of water in channel F22 from reaching channel F14. Terrace wall F12 also blocked the flow of water running through capstoned channel F21, which may have been heading towards a spillway down to F23. Finally, terrace F10 surrounds the south and east side of cistern F19 and shuts off any outlet to the lower terrace field from this end.

Erosion of these terrace walls has exposed much of the rubble fill which had spilled out and covered the face of these walls, preserving the remaining lowest courses. A portion of the core of terrace wall F11 was excavated in an effort to expose its inner face and its relationship to channel F14 and plug walls F27. Several jar rim fragments and a yellow glazed bowl rim fragment are apparently of Crusader-Ayyubid date. If this dating of the terraces holds up, their construction would have occurred in the twelfth or thirteenth centuries. Since all of the original farm soil supported by these terraces has eroded off, there is no other independent way of dating their use except by excavating the make-up of the walls themselves. Dating the original hewing of the channels is even more problematic.

The western terrace walls

The western side of the exposed bedrock plate was bordered by a heavily eroded terrace wall (F13). This terrace retained a higher step of farm soil on the wall's better preserved western side. This terrace also bordered and

kept this soil from choking a rock-hewn channel (F18). The few sherds of pottery recovered from L2 (Bas No. 6) all date to the Roman Period, except for one fragment of a modern roof tile. The southern end of terrace F13 is abutted on its east face by terrace wall F10. F13 stops at the point where the bedrock plate drops off and intersects another exposed plate of chalkstone bedrock. Another terrace wall (F9) begins just at the junction of the bedrock plates; it barely touches the western side of F13 at its south end. The bedrock on which F9 was built dives below the soil approximately 20 m south of where the wall begins. Where the bedrock is visible, a channel (F28) can be seen into which terrace F9 was set. As with the other channels that had carried water from a nearby spring, channel F28's use for 'wet' farm irrigation was cancelled by the construction of a later terrace. A temporary access road for a housing complex higher up the hill has damaged some of terrace wall F9, causing its partial collapse. This road has also covered the farm soil retained by the terrace with a scree of chalk boulders and cobbles. Neither the terrace nor the farm soil behind it has been excavated. These two western terraces differ from the eastern terraces in Area B-1 in that they are constructed out of smaller and softer chalkstones. The stones are semi-dressed, but no true ashlar appears to have been used in the courses that are currently visible. Whether this has any chronological significance remains to be seen.

The irrigation system

The channels alluded to above in relation to the terrace wall rebuilds were each hewn out of hard limestone and chalk layers. The largest and longest channel F14 (*c.* 25 + metres) received, and/or diverted, water from three other channels (F20, F21 and F22). In its last stage of use F14 carried water to a rock-hewn cistern (F19). This cistern – which has not been excavated – had been re-plastered and mortared with modern cement. It does not seem likely that all of the effort put into the construction of these channels was done just to supply this one modest cistern. The plug walls F27 served to keep the water flowing south to the point where it ends at the mouth of this cistern. Excavation of the cistern will be necessary to see if it was contemporary with the terrace rebuilds, as appears likely. The most important evidence for 'wet' farm irrigation is seen along channel F18. This channel, which is hewn out of chalk, contains a series of eight outlets or valves for releasing water. These were cut through the eastern border of the channel with each valve spaced approximately 0.80 m apart. Each valve is 12 cm wide and 0.5 m long. The upper four valves were hewn out of hard limestone and the lower four in chalk. The bedrock, which had been covered by soil, would have supported the water and the soil itself, which would have conserved the water by shielding the moisture from evaporation.

Area B-2

Excavation

Terraces, a channel and watchtowers were investigated descending eastward down the slope, as well as extensive slope terracing immediately below and to the east of B-1 (Fig. 21, 24; pottery Fig. 41). Several places in the high revetment of this terrace, the highest and most impressive of the entire farm, were repaired at various points in antiquity. Cleaning in the partially collapsed southeast corner of the terrace exposed some potsherds from the Byzantine era. This indicates that this particular terrace of the 'wet' farm had been utilized as late as that period since it had been repaired then. Also it indicates (taking into account the line of the Roman Period channel which descends from Area B-1 along the terrace's southern edge) that this 'wet' farm terrace was likely preserved to its present dimensions throughout its history.

South-east terrace and tower

At the end of the channel descending southeast from Area B-1 was the remains of a terrace that has recently been used as a modern access road. This terrace is likely the lowest element of the Area-C dry farm since the water channel is actually cut into the escarpment that the terrace wall surmounted.

Fig. 24. Section of Wet Farm terrace wall F-7

Additional work in Area B-2

In 1997 excavation was concentrated in Area B-1 (Fig. 21; pottery Fig. 41). Here a complex system of rock-hewn channels for 'wet' farm irrigation was exposed. The principal goal of the first season was to obtain a comprehensive plan of the terrace system and the irrigation channels detected in Area B-1. Unfortunately, most of the farm soils in this area had eroded off the bedrock plate, so it was not possible to date the creation of the system or investigate the type of farm soils once supported by the terrace walls except by probes into portions of the rubble fill making up the wall. To overcome these deficiencies in our data, excavations in 1998 were focused further east on the terrace steps below Area B-1. The broader limestone plates of Area B-2 contain large field plots and deep soil deposits. This area also has the best-preserved terrace walls, several of which stand to their original height. Wet farm agricultural features undoubtedly exist in Area B-2 but no irrigation channels have been exposed because the soils behind the terrace walls still hide the channels. The intact soil deposits in this area contained sufficient amounts of pottery from which to date the stages of cultivation of the land. The well-stratified soil deposits in Area B-2 have enabled us to follow the method of soil preparation and a sequence of utilization of the land. Four trenches were opened in this area. The largest probe was opened above watchtower F6, where we wanted to trace the superstructure of the tower in anticipation of its restoration. Additional stretches of terrace walls not fully seen in 1997 were exposed and several new terrace walls were discovered (F33, F36 and F37) in 1998. We now have plans that provide a full picture of the extent of the agricultural plots farmed in the area. In addition, walls representing the base of two watchtowers (F3 and F6) were exposed in the course of the excavation.

Area (B-2) terraces

The longest terrace (F1) in Area B was exposed over its entire length of 80 m. This terrace fronts the spring-fed wadi channel that runs parallel with the wall and the bedrock. Limestone cobbles make up the face and body of the terrace (0.15×0.15 m) but these stones exhibit very little tooling. A new terrace wall (F37) was discovered running at a right angle from the base of terrace F1, which projects 4 m towards the channel of the spring. Both terraces rest directly on the bedrock plate, with terrace F1 skirting the edge of the plate itself.

Terrace F7 sits recessed 0.5 m back from the exposed and quarried face of the limestone plate. This plate represents the northeastern limit of Area B-2. It was completely exposed and is 45 m long. The northern half of the terrace sits directly on bedrock, while the southern half was founded partially on Eocene limestone derived Mediterranean brown forest (MBF) soil ('brown

rendzina' soil) but also on the remains of a collapsed terrace wall (F/L40). This earlier collapsed wall now serves as an embankment that continues to retain the original soil it supported, as well as serving as the foundation for this portion of F7. Several stages of patchwork and repairs to this terrace are visible. The final episode of repair consisted of a double course of flat limestone slabs that caps earlier patchworks of cobbles and boulders. The last 5 m at the north end of this terrace was built of fine ashlars (*c.* 0.40 × 0.45 m). The last ashlar of just one course abuts the inside line of terrace wall F1. Erosion has carried away the higher courses at the end of the wall but three other courses of ashlars remain to the south. None of the soil layers retained by this terrace have yet been excavated; however, pottery recovered from one small cobble patchwork collapse (L39) contained Black Gaza Ware. Three major breaches of terrace F7 were repaired by Mark Goodman and his team of restorers in October 1998. The gaps left by these breaches provided the opportunity to photograph the soil profiles retained by this terrace and to see the method by which the terrace was constructed. Behind those portions of the terrace whose face had been patched with small cobbles, much larger stones of semi-dressed ashlars were employed as plugs to hold back the soil layers and relieve pressure from the face of the terrace itself. The revetment wall (F7) leans back into the terrace and the bedrock that the wall sits on is recessed 50 cm from the edge of the plate since the bedrock slopes sharply; however, the foundation of the terrace appears rather vulnerable to slippage. Under these conditions the cobble fill of chalkstone that in profile is triangular in form is the crucial factor in supporting both the terrace face and the soils behind it. The cobbles filter and allow water to pass through the terrace. In addition, they bear the weight of the protective revetment and receive the pressure exerted by the soil layers behind it.

The southern end of terrace F7 abuts the northern end and face of terrace wall F47. Terrace F47 is approximately 15 m long and one to three courses high. The northern half of the terrace was set directly on MBF soil and the southern half of the wall with just one surviving course sits directly on the exposed bedrock. At this juncture the rest of the wall has eroded away; it likely had continued south to meet the north face of watchtower F16. The stones of this terrace include semi-dressed boulder-size stones and cobbles. No soils supported by this terrace have yet been excavated.

A 1 × 4.5 m trench was excavated immediately behind terrace wall F2. This 15 m long terrace supports a triangular plot of land (L34) that extends behind and north of watchtower F3. A homogeneous fill of poured agricultural soil was revealed. The MBF soil consists of compact granular earth with numerous inclusions of limestone pebbles and chips. These inclusions are at their heaviest concentration just above the surface of the limestone plate (F48). Many of these chips are the result of sheering or peeling of the bedrock from water retention and temperature variations. The soil is at

its deepest (0.60 m) immediately behind terrace wall F2. The soil thins as it extends west on the rising bedrock (F48) just at the point where it reaches the next terrace (F4). The soil is retained by a terrace wall (F2) that consists of four courses of boulder and cobble-size limestone cemented in place with mud mortar and chink stones. Behind this one-row wide fronting of stones there is a triangular-shaped pile of loose limestone pebbles, cobbles and brown granular earth. The quantity of pottery present in this soil layer is light. Of the 206 pottery sherds collected, most are tiny ribbed, thin-walled body fragments. These, along with the more diagnostic rim and handle fragments, are Early Roman in date with one fragment of a Gaza Ware cooking pot (cf. drawing) and one late medieval sherd.

The largest probe opened is a 2×8 m trench west of terrace wall F32, east of terrace F33, and north of watchtower F6. This trench enabled us to probe below the stone revetment (F35) that skirts the rectangular platform of watchtower F6. This revetment also supports the front face and lower courses of terrace wall F33. A layer of terrace collapse (L30) partially covered this revetment and the farm soil spreading east from the terrace (F33). In this collapse, pottery was predominantly Early Roman, but a few sherds of Black Gaza Ware were present. This layer of collapse (L30) and the revetment (F35) sealed a deposit of agricultural soil (L31). This layer consisted of compact MBF soil which is almost devoid of cobbles, but does contain some very small pebbles. The pottery includes 397 recorded pieces, consisting of some Early Roman, but predominantly Late Roman to early Byzantine sherds (including the Byzantine cooking pot in Fig. 41:2 and a few pieces of Beth Shean jars). The soil was retained by terrace wall F32. This rather thick deposit (1.12 m) of soil covered an area of quarried stone (F49).

The bedrock beneath (L35) was quarried and the remains of partially hewn blocks were still attached to the rock bed after considerable effort had been made in preparation for their removal. This, along with numerous other instances of abruptly abandoned quarry cuttings with almost finished blocks left *in situ,* has yet to be explained. For the most part the quarry cuttings are oriented differently than the terrace walls which cover them. This widespread quarrying followed a logic dictated by the nature of the rock being worked and the purposes to which the stone was put. With the creation of the terrace system, the dictates of the topography and the type of cultivation practised were ordered by different contingencies. The levelling of the rock scarp through major quarrying undoubtedly aided the conversion of the land to stepped agricultural terraces. Quarrying helped define the space of some of the farm plots, but this does not seem to have necessarily been done in conjunction with the conversion to agricultural use. Perhaps the work in the quarries was superseded by the need for agricultural production to supply the growing village of Nazareth. Future investigations must focus

on the nature of the quarrying activities and the date in which they were in operation.

Over 14 m of terrace F32 have been traced. The north end of the terrace meets the southwestern end of watchtower F3. A gully has eroded away most of the stones at this juncture and the wall exhibits several episodes of repairs with small cobble patches. A narrow strip (ca. 2×0.50 m) of this collapse and erosion debris (L45) was excavated along the face of the wall. This layer consisted of decayed organic matter with small cobbles and pebbles that extend to the bedrock on which the terrace was founded. The few sherds of pottery recovered included one Gaza Ware jar fragment and four Roman body sherds.

North of watchtower F3 and 2 m east of terrace F2, the poorly preserved remains of terrace F38 were traced for 7 m. This terrace rests on a bedrock ledge which drops 0.5 m to the next lower step fronted by terrace F1.

A 1×2 m probe behind terrace F5 was opened in an effort to define the relationship between this terrace and watchtower F6. The southern extension of this terrace was founded on MBF soil. The north end of the terrace rests directly on the bedrock and it also extends over the perimeter of the tower and rests partially over the remains of an earlier terrace (F46). The layer of MBF soil (L44) behind F5 was cut by a foundation trench (F50). This trench was associated with the construction of terrace wall F5. No pottery was found in the backfill of the trench. The pottery from L44 was almost exclusively Early Roman with potential dates ranging from the first century BC to the third century AD.

Area B-2: Watchtowers

Northern watchtower (F3) (Fig. 21)

The perimeter walls of the northern watchtower (F3) were partially cleared of the collapse of its higher courses. In particular, the north face and corners of the structure have suffered the greatest amount of erosion. These gaps along the sides and corners have made it difficult to obtain a precise plan. As presently preserved the structure measures 8×7 m. It is roughly square and built of partially tooled limestone boulders and cobbles. Like the better preserved tower F16, explored in 1997, F3 also appears to have had a stone revetment or apron of stone protecting the sides of the tower and holding it in place against the steeply sloping bedrock. Most of this apron has eroded away but when in place it would have measured approximately 12 m in diameter at the base of the structure. The walls of the tower are sloped to counter the effects of the slope on which the tower rests. Limestone chips serve as chinks holding the courses in place. As seen through the gaps of the missing masonry a fill of chalkstone pebbles and cobbles makes up the core of the structure.

Fig. 25. Area B-2 central tower F6

Central watchtower (F6 and F35) (Figs. 21, 25; pottery Fig. 41)

Watchtower F6 was cleared of collapse and some debris that had eroded from the core of the structure. An apron or circular stone revetment (F35) protected the sides of the roughly 9 × 6 m tower but most of this revetment has also eroded away except along the northern side of the structure where it also supports the lower courses of terrace wall (F33). It is composed of limestone boulders and cobble cemented in place with heavy grey mud mortar. In order to trace the perimeter of the structure at its apex, a 5 × 5 m trench was opened with the added purpose of reaching the core of the tower to see how it was built. A sterile layer of topsoil (L41), 0.10 m thick and devoid of pottery, was excavated. It consisted of decomposed chalk that had decayed from the stones of the tower. It sealed a layer of agricultural soil (L42) made up of extremely compact brown granular earth full of limestone pebbles and occasional cobbles. One hundred and six shards were collected from this layer, of which 33 were Gaza Ware jar fragments and the rest were late Hellenistic to Early Roman. This layer sealed another layer of agricultural soil (L43) composed of MBF soil containing inclusions of very small limestone pebbles. Fourteen shards were found in this layer and these were Late Hellenistic to Early Roman in date (typology predominantly first to third century AD in form). L43 covered a fill of chalkstone cobbles and pebbles (F6) that forms the core supporting the body of the tower. The core has not yet been probed; however the outline of the stone making up the core may mark the limits of the presumed super-structure supported by the stone tower. No wall lines or foundation trench were seen which would demonstrate

that a second-floor room existed on top of the stone construction, but further excavation will be necessary to decide this issue.

Southern watchtower F16 and buttress F17 (Figs. 21, 26 and 27; pottery Fig. 41)

Watchtower F16 was cleared of collapse and some debris that had eroded from the upper part of the tower. Buttress F17 supports the east face of the watchtower F16. A probe was cut at the northeastern juncture of F16 and F17. The tower was founded upon the sloping bedrock but the buttress was founded on a layer of eroded agricultural soil (L5).

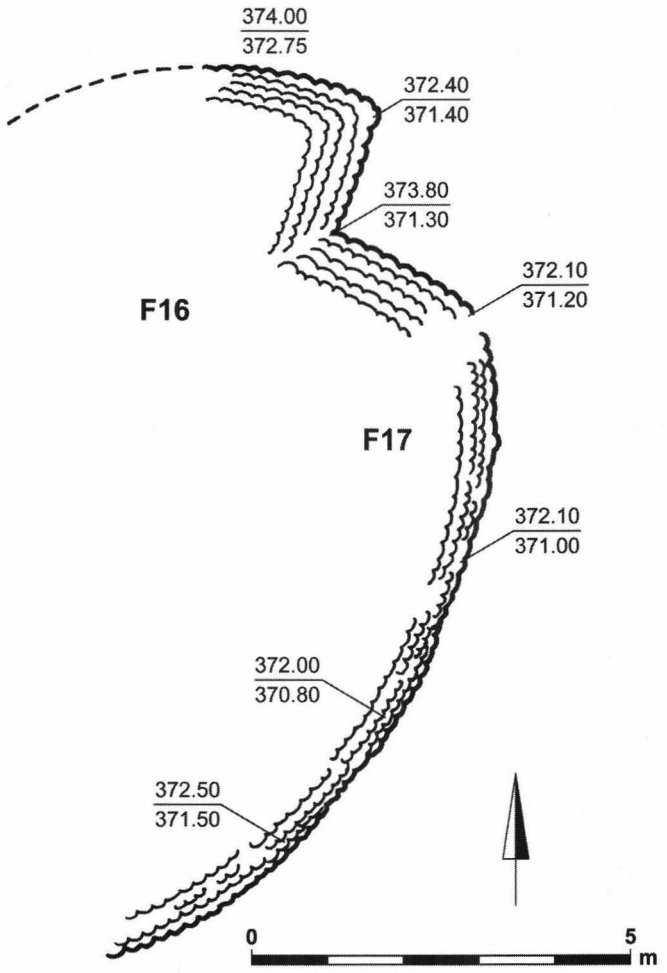

Fig. 26. Southern tower F16/17 plan

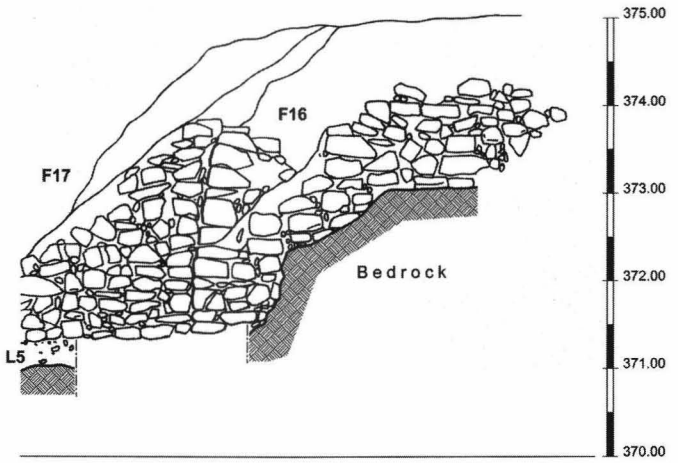

Fig. 27. Southern Tower F16/17

Area C: 'dry' farm above Area B

Above and to the west of Area B lay a series of 'dry' farm terraces which originally ascended to the crest of the hill. Earlier construction of private homes, the recent construction of a road and the current construction of apartments have either covered or obliterated most of the terraces associated with this area. Four of the remaining terraces were investigated.

The terraces that exist on the bedrock plate delineating Area C were cleared of vegetation and traced over their entire length. These terraces have now been numbered and appear on the plan encompassing Areas B and C. For the most part, the terraces follow the rock scarp, breaking in a series of natural steps spaced approximately 7 m apart. Olive trees and carob trees are currently growing on these terraces.

Area C-1 extended upper terrace (Figs. 21, 28; pottery Fig. 42)

Two trenches (each 1 m wide) were cut through the agricultural layers perpendicular to the terrace's retaining wall. Both the northern trench and the southern trench confirm that the terrace was built upon the natural (unquarried) surface of the face of the slope. The soil is consistent from the rock floor until the present surface, which was covered with a layer of crushed Eocene limestone. The retaining wall was built with a foundation core of Eocene stones (0.05×0.05 to 0.20×0.20 m) and a revetment of larger stones (up to 0.30×0.30 m). A row of stones (one to two courses deep) follows the line of the terrace separating the cobbled core from the terrace soil. (Mediterranean Brown Forest soil: brown: 7.5YR 4/4; 2% very fine to fine limestone grains.)

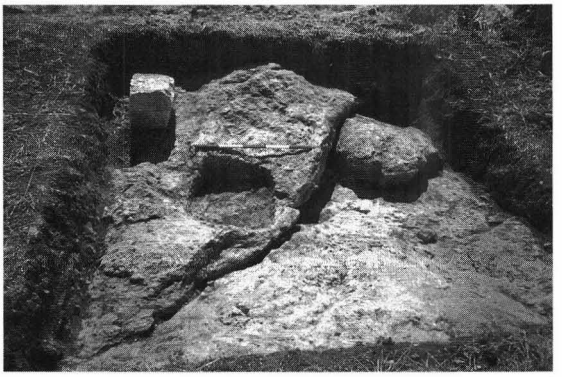

Fig. 28. Stone installation in Area C-1

The southern trench has a roughly square-cut indentation in the rock surface (a press?). The earliest diagnostic pottery (first century AD) was found in this terrace. This is our first indication that the elliptical terraces on the upper parts of the hill predate the more complex terraces of Area B and the rock-cut terraces of area A. A well-cut stone measuring approximately $0.50 \times 0.40 \times 0.35$ m was found in the terrace soil, which may tentatively be dated to or predate the time of the terrace's construction.

Area C-2 'wet' farm terrace (Fig. 21)

Massive retaining wall (similar to Area B) with larger than usual stones used for both the cobbled core and the revetment. The stones are better fitted together. The exposed surface of each stone of the revetment was chiselled flat. Due to these characteristics it seems quite possible that this section of terrace was merely an extension of the Area B 'wet' farm. No test trenches were cut, although a section of the terrace wall was cleaned near the dirt road dividing Area B from Area C.

Area C-3 elliptical terrace F02 (Figs. 21, 29; pottery Fig. 43)

The excavated trench seems to indicate that there was no intentional layering of the terrace soil. (Mediterranean Brown Forest soil: dark reddish brown: 5YR 3/3; 3% very fine to medium limestone grains).

The Area C-3 terrace retaining wall (F02) consists of the usual earthen embankment of soil, cobbles and pebbles that was preserved to about 1.1 m in certain sections with one to five courses of a revetment preserved. Where the terrace retaining wall was founded upon bedrock, a shallow groove or trough was cut in the rock surface to create a consistent foundation for the revetment of the retaining wall. Where the retaining wall rested on natural soil, a foundation trench was cut in the soil and larger stones lined the

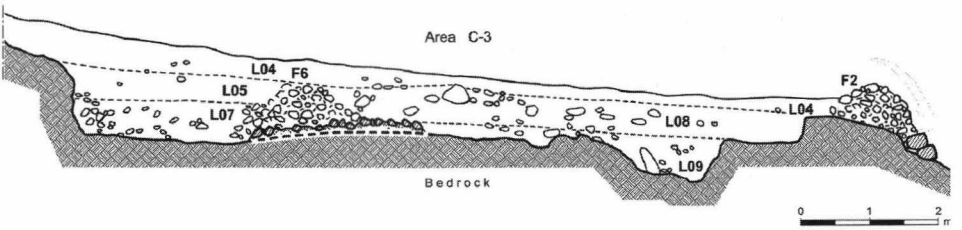

Fig. 29. Area C-3 terrace section

bottom of the trench to provide a firmer foundation for the revetment wall. The terrace ends were typically 'capped off' by incurving the ends of the retaining walls up-slope.

Added to this, at least in the section exposed by the trench, at least a metre of bedrock appears to have been cut horizontally along the western side of the retaining wall to a depth of about 45 cm, effectively retaining an additional amount of imported soil. This would have allowed for deeper rooted trees, including olives, to be planted on the terrace. The modified rock surface seemed at first to indicate the existence of an earlier quarry or a pool. However the rough, natural form of the bedrock in the rest of the trench stood against the first idea. The shallowness of the depression makes the second theory unlikely.

The surface layer of this terrace (L04) contained little pottery but included some Gaza Ware jar fragments and sherds of a brown glazed bowl, medieval or later in date. The next layer (L05) consisted of compact MBF soil. The majority of the pottery was first to third centuries AD. These two layers were separated by what appears to be the remains of a partially eroded earthen embankment of an earlier retaining wall (F06) whose upper limit was submerged just below layer L04. Below layers L05 and L08 were two relatively dark and more clayey layers that were devoid of pottery (L07 and L09 respectively) and likely consisted of original soils produced on the natural slopes.

The terrace of area C-3 was actually two superimposed areas of terracing. However, what was originally two terraces with two retaining walls during the Roman Period was later converted to a single deeper and sloping terrace with a single retaining wall serving during the Islamic period. It seems that during the Roman Period the narrow and relatively shallow terraces were utilized for a vineyard while the terraces were modified during the Islamic period to plant trees (likely olive).

Area C-4 U-shaped lower terrace F02 (Fig. 21)

Terrace F01 is 25 m long. It was built of limestone cobbles, most of which have eroded off the edge of the limestone plate, leaving just one or two courses of

the face of the terrace. The north end of the terrace breaks at a point where the bedrock is stepped back and rises sharply. At this point another terrace wall (F03) extends for approximately 15 m. This wall also is poorly preserved. In both terraces the body or core made up of cobbles and pebbles is better preserved. The retaining wall core acts as an embankment, holding back the farm soils and keeping them from washing away down the slope.

Agricultural 'dry' terraces

The Nazareth archaeological project is important in that it provides essential data on the historical development of a terraced agricultural landscape in the Galilee; data which hitherto has been sorely lacking (cf. Golomb and Kedar 1971). While terracing has been researched fairly comprehensively in the Judean Hills (Ron 1966; Gibson 1995; *idem* 2001: 113–116) the same cannot be said for the Galilean Hills. The word 'terrace' is used to denote an entire structure which includes bedrock (levelled by hand or left natural), the retaining wall, and, perhaps most importantly, the agricultural soils which have been prepared for growing crops. The retaining wall is intended to protect the soils from erosion. Most of the terrace structures we have studied have a bipartite structure: (1) cobble, pebble and soil, earthen embankment which limits and protects the agricultural soils from erosion

Fig. 30. First-century fresco from Pompeii depicting terraced vineyards on Mt. Vesuvius and nearby low-land trellised vineyards, both with watchtowers

Fig. 31. Area C-3 foundation groove, trench and eroded embankment

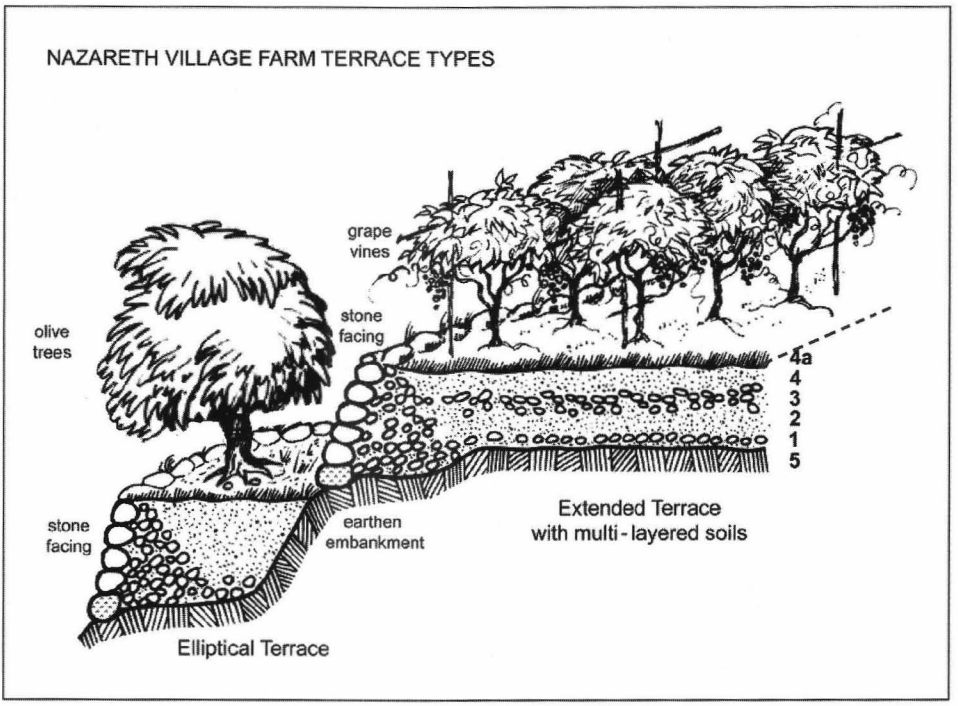

Fig. 32. Primary dry farm terrace types: elliptical and extended

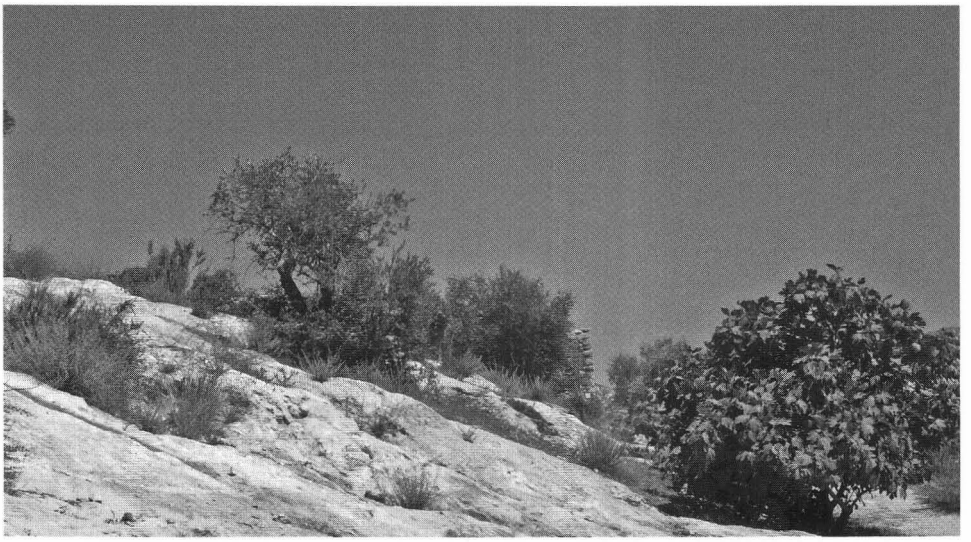

Fig. 33. 'Pothole' agriculture in bedrock including olive tree, fig tree and various herbs

(cf. Fig. 31); and (2) a leaning revetment of uniform and evenly spaced stones which served to protect the earthen embankment from erosion and collapse. Ideally, the stones of the revetment should be roughly conical or pyramidal in shape, with the flat ends facing outward to form a consistently smooth surface. The pointed ends face inward, fusing with the earthen rampart or the agricultural soils. As a means of protection against collapse, foundation trenches for the stone revetments were cut into the bedrock or the natural soil, serving to create firm, non-shifting foundations for the walls (Fig. 31). Capping of the ends of each terrace was also necessary in

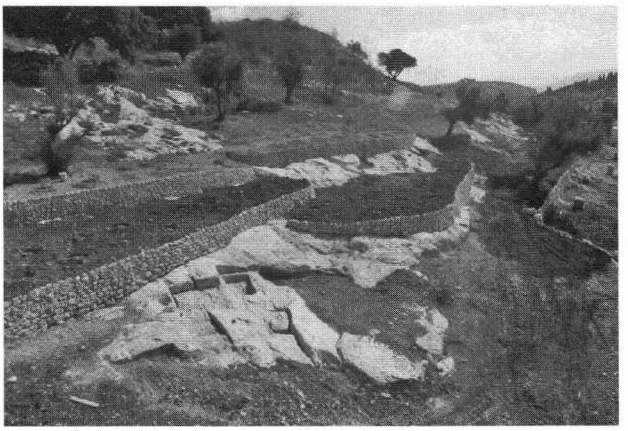

Fig. 34. Reconstructed extended terraces and elliptical terrace with quarry

order to impede erosion of the soil layers. This was done by creating an upslope curve in the line of the retaining wall at either end of the terrace (Fig. 31).

We have distinguished a number of types of terrace soil layers: *Simple*: (e.g., Area C-1) comprising a single layer of imported soil. The soils tend to be local soils (e.g. brown rendzina, i.e. Mediterranean brown forest soil, for the Nazareth Ridge) from nearby ravines or alluvial soils which have been transported by donkey from more distant plains and valleys. *Complex*: At least two intentionally layered terraces have been identified and excavated, one from terrace F7 of the wet farm Area B-2 which is still preserved to a height of 2 m (Fig. 35), and one from the dry farm (Area A-1; *supra* Fig. 5). Multilayered terraces typically have up to 5 distinctive layers, with imported clean soil (Layers 4 and 2) and limestone pebble/chalk layers which produce aeration for plant roots (Layer 3) and a protective structure and barrier to keep soil from flushing through the interstices in the terrace wall (Layer 1). A layer of fertilizer and decaying vegetation often covers the soil surface when the terrace is still being farmed, providing a fifth layer.

Typical layers as illustrated from Area A-1 (counting from bottom up): Layer 4a: mixed recent soil and debris (replacing missing layer 5); Layer 4: Mediterranean brown forest soil (brown: 7.5 YR 4/3; with 10% small to coarse grains); Layer 3: crushed Eocene limestone debris (white: 10YR 9/1 [off chart]); Layer 2: Mediterranean brown forest soil (dark brown: 7.5 YR 3/2; with 10% small to medium limestone grains); Layer 1: crushed Senonian chalk (very pale brown: 10YR 8/3). At times the modifications of the terrace

Fig. 35. Dry farm terrace drainage system at work; note water stains on rock surface

contains multiple and diverse features added at various periods (e.g., C-3; Fig. 29), normally dictated by water availability and changes in the choice of crops.

It is assumed that the earliest stage in the morphology of this landscape was 'pot-hole' agriculture, with the utilization of natural pits in the rock surface and crevices. It is assumed that this type of agriculture was employed by people who found they could utilize the natural deposits of soil in the hill country in order to plant their crops and fruit-bearing trees (elsewhere this is assumed to have taken place in the Chalcolithic or early stages of the Early Bronze Age: Finkelstein and Gophna 1993: 12–13; Gibson and Rowan 2006: 104–105).

The depth and area of these plots were at times extended by adding a small wall on the downhill side of the hole, thus maintaining and protecting the holes from soil erosion and providing room for even more soil to be added. The depth and breadth of the plot would in part have determined what crop could grow there. The volume of soil could be increased by cutting the holes deeper into the bedrock or by adding a single row of stones on the down-slope side of the plot. This latter practice likely led to the invention of the elliptical terrace.

The elliptical (U-shaped) terrace

Normally formed by a U-shaped wall which exploited the contours and recesses of the natural rock surface which had been carved by natural forces (e.g., water and wind erosion). The depressions and valleys formed by water erosion in the otherwise rounded rock aid in the channelling of rain to the plot during the seasonal rains. Improving the bedrock: in various instances the bedrock may be hewn into in order to provide more breadth and depth of soil for deeper rooted plants and especially trees. This is usually done by digging a bowl-shaped pit in the bedrock to contain the roots of a single tree, as was evident in a certain places in area A. However, as in Area C-3, a broader area of bedrock might be excavated to provide room for a number of trees. One olive tree, or at the most two, is the norm for a single standard elliptical terrace.

Although the more primitive elliptical terrace came into use earlier than the more sophisticated extended rock-hewn terrace, the elliptical terrace continued to be used contemporaneously with its preferred counterpart until modern times (cf. the elliptical and extended terraces juxtaposed in Fig. 34).

The extended (I-shaped) terrace

The most effective terrace for farming is one which is level or slightly down-sloped and extends over many tens of metres with a uniform depth and breadth. This type may include any terrace whose platform has been levelled

or extended for the purpose of improving or extending the arable land available to the farmer. In the most favourable case the natural rock striations form uniform horizontal stepped platforms upon which the terraces can be built. In most cases, however, the platforms must be created artificially by levelling inconsistencies with loose rock and soil, or by cutting the bedrock to form a level platform, or both. A shallow groove is often cut into the natural rock surfaces to hold the foundation stones of the terrace's revetment wall. The planting areas are often multi-layered with alternating layers of soil and crushed limestone/rock.

The extended terrace is the best-suited terrace for raising vines. The long, narrow and straight field within its retaining walls fulfills the form and function of a plot that is intended to be used for planting vines in rows, and then training each vine up onto long trellises with the herbage and fruit hanging down from above.

The morphology of 'dry' terraces in this peripheral landscape of Nazareth, may be summed up as follows:

(1) The dry farm terrace depth and width should be uniform.
(2) The length of the terrace should be level and extend as far along the hillside as possible in order to cover the maximum available area with soil for growing crops.

Fig. 36. Wet farm terrace F-7 constructed with layered soil

(3) The terrace floor and the surface of the land should be nearly level but gently sloping in order that the available water should cover the entire surface while allowing for proper drainage, thus preventing pooling of water, water wastage, and the consequent weakening of the integral structure of the terrace.
(4) The terrace, particularly on higher and steep hillsides, should be multi-layered with alternating layers of chalk/chipped rock and soil, providing aeration – especially for the roots of vines which are susceptible to rot. This layering also creates a firm structure to the soil of the terrace, discouraging erosion and the collapse of the terrace.
(5) Terrace walls should be of uniform firmness and aeration.

Agricultural 'wet' terraces

These are terraces which have been constructed in association with conduits and inlets for irrigating crops by water flowing from secondary sources, including springs and reservoirs.

Their morphology may be summed up as follows:

(1) This type of terrace needs to be well built with a strong retaining wall that can contain heavy water-laden and shifting soils.
(2) There must be a water dispersement system which will control available water and disperse it evenly over the entire area of the terrace.
(3) The soil must have an internal structure and consistency in its layering (preferably multilayered with alternating layers of chalk/rock and agricultural soils) in order that it is not flushed from the terrace during irrigation.
(4) The irrigated farm normally will be reserved for crops which cannot grow on a dry farm.

Appendix 1: the terrace steps in Area A

Layers and Features: Step 1

Step 1-1: Extended Terrace F1 (*c.* 14 m) unexcavated.
Step 1-2 + 3: Extended Terrace F1a + 1b (*c.* 43 m) unexcavated. This may well have once formed one continuous terrace with Step 1-1 if the connection between the two has eroded away.
Step 1-4: Terrace F9-10 (*c.* 25 m) unexcavated. Area was built over by development of Nazareth Village.
Step 1-5: Elliptical Terrace F10a (*c.* 25 m = Area A-3, cf. *supra*). Stairway F10b, Terrace erosion L10*c*. Area was displaced by development of Nazareth Village.

Layers and Features: Step 2

Step 2-1: Terrace F2a (*c.* 33 m; at least 3 m deep) unexcavated.
Step 2-2: Terrace F2–F3 (*c.* 23 m; 2.8 m of revetment wall preserved 0.85 m high, at least 3 m deep) unexcavated.

Layers and Features: Step 2A

Step 2A-1: Terrace F5a (*c.* 29 m; 4.65m of revetment wall preserved 1.14 m high, at least 3.5 m deep) unexcavated.
Step 2A-2: Terrace F5 (*c.* 16 m; 2.8 m of revetment wall preserved 1.1 m high, at least 2.5 m deep) unexcavated.

Layers and Features: Step 3

Step 3-1: Extended Terrace F4 (*c.* 23 m; 9 m high, *c.* 6.5 m deep) erosion along wall was excavated and terrace was restored. Soils of terrace were evidently layered. Terrace wall was apparently founded on bedrock. This terrace should be considered a single unit with the Step 3-2 since the rear soil layers continue from one to the other at the same level and on the same plate of bedrock. The southern end of this terrace wall does not turn into the slope at this point, as would be the case if it was intentionally being 'capped off' in the normal fashion. This separation between the ends of each terrace apparently provided space for an earthen ramp that once allowed ease of access for workers and farm animals to and from the terraces below. The terrace was eventually restored.

Step 3-2: Extended Terrace F6 (*c.* 22.1 m, 1.5 m high, 7.6 m deep) erosion along wall was excavated. Soils of terrace were evidently layered similar in form to that of Step 3-1. The wall at its southern end curves up-slope to avoid intersection with the descending terraced road Step 3-3. Terrace wall was founded on bedrock. Terrace has been restored.

Step 3-3: Terrace F7–F8 (*c.* 45 m; 32 m of revetment preserved; *c.* 0.99 m high, at least 8 m deep). This is the longest terrace that can be traced at the site. It stands apart from the other terraces at the site in that it diagonally ascends the slope more than 10 m in elevation for its entire length of at least 45 m (the northern end disappears under the backfill from the building of the hospital). For much of its length the terrace was relatively shallow and bent in form, making it less useful agriculturally. Although a short line of stones helps to define the structure formally as a terrace, the soil was partly mixed with chalk and leached over most of the area. Since as a rule, the other terraces were built to form horizontal platforms (like planter boxes) of ample depth, its use as an agricultural terrace is unlikely. It seems more likely that the terrace formed an access road providing workers, farm animals and carts

safe and easy access to the fields without walking over the otherwise more treacherous smooth bedrock to and from Nazareth. Erosion along the wall was excavated and terrace was restored.

Step 3-4: Terrace F13 (*c.* 3.7 m long, 1.3 m high, 3.1 m deep) unexcavated. This relatively short terrace is built into a depression in the natural bedrock. Its wall, including the revetment, is preserved to a relatively impressive height of 1.4 m. Seepage of water through the body of the terrace can be detected by whitened lines of water residue which emerge from below the foundation stones of the revetment wall and descend toward the terraces below.

Step 3-5: Terrace F14. Quarry F5 = upper Area A-1 (cf. *supra*). In the south-west corner: a quarried basin roughly resembling a wine pressing floor was cleared in 1997. Although there is an apparently cut groove which would allow the fluids to drain to the level below, there is no evidence of a contemporary collecting vat.

Layers and Features: Step 3A

Step 3A-1: Terrace F11a, Quarry F20 (*c.* 23.5 m, 1.23 m high, 4.5 m deep). Terrace erosion in quarry excavated and terrace was not restored to leave quarry exposed extending below Step 3A-3 below.

Step 3A-2: Elliptical Terrace F11 (*c.* 23.4 m long, 1 m high, 7.14 m deep) All features of this terrace had been entirely eroded away, with the exception of the foundation trench that was cut for the placement of the revetment wall. This terrace was reconstructed for the modern visitor's centre based upon information derived from other terraces at the site.

Step 3A-3: Elliptical Terrace F11b (*c.* 14.5 m long, 0.8 m high, 4.1 m deep). Where a terrace once stood whose original layered structure has been eroded away, a beautiful quarry (F20) was revealed under a layer of soil erosion (L10). The quarry is outstanding for the clear definition of its ashlar negatives which enabled us to obtain precise measurements on the size of individual blocks and the number of blocks extracted from a given space. One hole which was to serve as a chisel socket to break a layer of stone away from the bedrock (10 cm wide, *c.* 5 cm deep) still remains as though ready to receive the chisel and chocks that would serve to strip away the first layer of stone. Terrace was not restored, in order to leave quarry exposed.

Layers and Features: Step 4

Step 4-1: Terraced Road F12a (at least 35 m, would have been *c.* 1.2 m high, at least 7.5 m deep). This terrace appears to represent the extension of the road/terrace 3-3 beginning at the western end of terrace 4-2. The terrace was not restored. A modern visitors' footpath has replaced it.

Step 4-2: Extended Terrace F12 (*c.* 29.4 m long, 1.1 m high, 7.2 m deep). Terrace erosion was excavated. Soils of this terrace appear to have been intentionally layered with alternating MBF soil and crushed lime/rock layers. The line of the retaining wall could be followed as the revetment, one course high, was preserved only in small sections. A groove was cut in the bedrock to hold the line of revetment stones. The terrace was restored.

Step 4-2A: Terrace F12b (*c.* 16.5 m long, 1.5 m high, 7.9 m deep). The curve in the terrace wall might be the first in a series of five elliptical terraces descending the slope at this point. However, it seems to be attached to extended terraces at either end and may have been curved to conform to a bend in the hillside. Terrace erosion was excavated and terrace was restored.

Step 4-3: Terrace F18 (*c.* 25.5 m long, 7.2 m of the original revetment could be traced, 1 m high, 3 m deep). Terrace erosion was excavated and terrace was restored to align on the east with Step 6-3 and to connect with elliptical terrace 4-2A immediately to the west. Terrace restored but utilized for path.

Step 4-3a: Terrace F18a (*c.* 15.3 m long, 0.85 m high, 5.1 m deep). Terrace erosion L56 excavated with several pieces of Kfar Hananiah ware; carstic cavity F59 containing fill L57 with Kfar Hananiah ware. Terrace restored. Partially cut by the Nazareth Village oil press building.

Step 4-4: Extended Terrace F15 (*c.* 31 m; existing revetment wall 11.2 m long, 0.9 m high, 2.6 m deep). Terrace erosion L62 (which covers *in situ* farm soil L63) excavated and found to contain several Kfar Hananiah ware sherds. Terrace restored to its full length. Partially cut by the Nazareth Village oil press building.

Step 4-5: Elliptical Terrace F16 (*c.* 37 m) = Area A-1 (cf. *supra*). Agricultural soils L1–L4a and terrace erosion excavated. Quarry F5 exposed. Area was displaced by Nazareth Village.

Layers and Features: Step 5.

Step 5-1: Elliptical Terrace 16a (*c.* 17.5 m long, 0.8 m high, 4.1 m deep). Terrace was not restored, in order to leave quarry exposed.

Step 5-2: Terrace 16b (*c.* 19.3 m, 1.4 m high, 8 m deep). Five metres to the east on the same plate of bedrock as Step 5-1, another layer of eroded debris (L11) was excavated. It covered a slightly modified area of bedrock (F21) that had once supported an agricultural terrace now completely eroded away.

Step 5-3: Terrace 16c (*c.* 26.9 m, 1.5 m high, 7.73 m deep). Another probe, again 5 m to the east, was excavated in a place where the bedrock (F22) forming a natural ridge 5 m long was adapted to serve as a terrace, the superstructure of which has eroded away. Because this bedrock ledge rises

40 cm up from the parent rock, it never lost its soil deposit (L12) of rich brown loam. The pottery in this layer was exclusively Roman.

Step 5-4: Terrace F19 + F20 (*c.* 37.7 m long, 2 m high, 4.4 m deep). Terrace erosion L47 excavated. This erosion covers *in situ* MBF agricultural soil L63. With the aid of four students and volunteers, three trenches were cut in the terrace, revealing that although there was evidence of upkeep and rebuild in the sloped layers, the soil was consistent except for a layer of crushed chalk spread over the bedrock. This would indicate that the terrace was constructed originally as a single layer of imported soil, but it appears to have been deposited on a layer of crushed chalk to impede erosion along the bedrock due to the yearly rains. This may have originally connected to Step 4-3 to form an extended terrace. Quarry F58; cut shaft F67. Terrace restored.

Layers and Features: Step 6

Step 6-1: Elliptical Terrace (*c.* 24.5m long, 1.1 m high, 7.45 m deep). Terrace erosion L50 excavated. Not restored.
Step 6-1A: Extended Terrace (*c.* 31.5m long, 1 m high, 5.2 m deep). Terrace erosion excavated. Restored.
Step 6-2: Terrace F22a (*c.* 17m +). Terrace erosion L41 excavated. Terrace wall F2; Quarry F20. Restored.
Step 6-3: Terrace F21a (*c.* 30.7 m long, 2 m high, 5.4 m deep). Two 2-m sections of revetment preserved. Terrace erosion L47 unexcavated.
Step 6-4: Terrace F21 (*c.* 48.7 m long, 2 m high, *c.* 7.4 m deep). About 10 m of revetment partially preserved. MBF agricultural soil L69 unexcavated.

Layers and Features: Step 7

Step 7-1: Elliptical Terrace F23a (*c.* 21.5 m long, 1 m high, 9.9 m deep). Terrace erosion L51 was excavated and terrace restored.
Step 7-2: Extended Terrace F23b (*c.* 27.7 m long, 1.1 m high, 3.8 m deep). Terrace erosion L52 excavated.
Step 7-3: Extended Terrace F23 (*c.* 17.8 m long, 1.55 m high, 7.6 m deep). Terrace erosion L42 excavated. About 6 m of the revetment to retaining wall was preserved up to 6 courses high – 60 cm founded primarily on soil layer with crushed chalk. The agricultural soils comprise one somewhat inconsistent layer of MBF soil pitted with clusters of stones, softball sized and smaller.
Step 7-4: Elliptical Terrace F23c (*c.* 17.7 m long, 2 m high, 10 m deep). Terrace erosion L61 excavated. Terrace restored.
Step 7-5: Extended Terrace F22 (*c.* 17 m long, 2 m high, 7.4 m deep). Terrace erosion L61a excavated. Terrace restored.

Layers and Features: Step 7A

Step 7A-1: Terrace F24a (*c.* 28.8 m long, 1.2 m high, 3.3 m deep). Terrace restored.
Step 7A-2: Terrace F24b (*c.* 24.4 m long, 1.2 m high, 6.6 m deep). Terrace restored.
Step 7A-3: Terrace F24c (*c.* 24.4 m long, 1 m high, 3.6 m deep). Poorly preserved. Unexcavated. Terrace not restored.

Layers and Features: Step 8

Step 8-1: Terrace F24 (*c.* 23.4 m long, 1.2 m high, 3.4 m deep). Terrace erosion L53 excavated. Terrace restored.
Step 8-2: Terrace F25a (*c.* 31 m long, *c.* 1.2 m high, 3.5 m deep). Terrace erosion L49 excavated. Terrace restored.
Step 8-3: Terrace F25 (*c.* 17 m long, 1 m high, 2.9 m deep). Poorly preserved. Unexcavated. Terrace not restored.

Layers and Features: Step 9

Step 9-1: Terrace 26a.
Step 9-2: Terrace 26, Terrace erosion L54a (5 diagnostic sherds: 2 Galilean bowls 1st–3rd century, Byzantine 4th–5th century AD, 2 Abbasid, more than 35 body sherds, above and inside wine press and store. F27, cf. *supra*).
Step 9-3: Terrace F28. Terrace erosion L54 excavated.
Step 9-3: Terrace F29. Terrace erosion L48 excavated.

Layers and Features: Step 10

Step 10-1: Terrace F27a.
Step 10-2: Depression and Cave F27b, bones of small carnivore. Terrace erosion L54b (3 diagnostic: 1 early Roman, 1 Islamic, 1 Ottoman early porcelain; 17 body sherds).
Step 10-3: Wine Press F27 = Area A-2 (see *supra*).

Appendix 2: Pottery (Yehudah Rapuano)

The ceramic finds from the Nazareth Village Farm excavations were for the most part quite fragmentary, as might be expected of pottery recovered from agricultural installations and terraces. Several periods are represented, illustrating the extensive duration of time from the earliest to the latest settlement and use of the farm (Figs. 37–44; Table 1). It is apparent that the farm territory was not occupied continuously. It seems that each area may have been in use during some of the periods represented, and was abandoned or

at least left dormant in other periods. In no single area of the site was pottery of all the periods represented found.

The surface finds include examples at either extreme of the chronological range of our site. A single potsherd of an Early Bronze Age III platter (Fig. 37:1), with a thickened, incurved rim, represents the earliest find at the Nazareth Farm. It is finished with a typical burnished net pattern on its interior surface. To date, no Early Bronze occupation has been recognized and this is the only artifact recovered from this period at the site. At the other end of the chronological spectrum, an entirely intact bowl made of Black Gaza Ware (BGW) with an externally thickened incurved rim dates to the Ottoman period.

The earliest occupation seems to have occurred in the late Hellenistic period of the first and second centuries BC. Examples dating to this period were primarily the jar and jug sherds discovered in Area B-1. A single jug base of this period was also found in Area A-2 (Fig. 38:5). The horizontal handle of the krater (Fig. 38:6) may derive from this period as well. A small amount of material dated to the Early Roman period of the first century BC to first century AD was found in Areas A-1, A-2, and C-1. The best represented pottery at the site was dated from the Late Roman to the early Byzantine period of the third to fourth or fifth centuries AD. The only area in which pottery from this period was not found was Area B-1.

Two sherds, apparently remnants from a single bowl, of a fine red ware and decorated on their interior surfaces with a golden-brown, speckled, glossy glaze for which no parallels were found, probably date to the Ottoman period. Not illustrated, also of Ottoman date, were several fragments of jars and spouted jugs of dark gray BGW from Areas A-3, B-1, and B-2.

The pottery generally exhibits characteristics typical of the Galilee region. This is especially observed in examples of the Early and Late Roman periods: in the Galilean bowls (e.g. Figs. 38:1; 38:2; 39:1; 41:4; 42:1; 42:2; 43:2; and 43:01, as well as in jars (e.g. Figs. 37:5; 38:4; 39:2; 41:18; 41:16; 41:21; 42:6; 43:9; and 43:13). Also the Byzantine period lid, decorated with bands of combing on its exterior, recovered from Area B-1 (Fig. 40:1) is typically Galilean. Nothing in the way of fine or imported ware was found in the excavations (with the possible exception of a Byzantine period rouletted bowl (Fig. 41:1), and glazed bowls (Figs. 43:5; 43:6; 43:7; and 43:8). All the forms were of a utilitarian nature, emphasizing the rural character of the site.

The sparse, fragmentary nature of the pottery did not permit us to determine the ethnic identity of the occupants of the farm territory in any of the periods. Nevertheless, it may be observed that in the Early and Late Roman periods, the ceramic forms are largely familiar from the Kfar Hananiah pottery repertoire, noteworthy for its having been manufactured primarily for the consumption of those observing Jewish *halacha* (Adan-Bayewitz 1993).

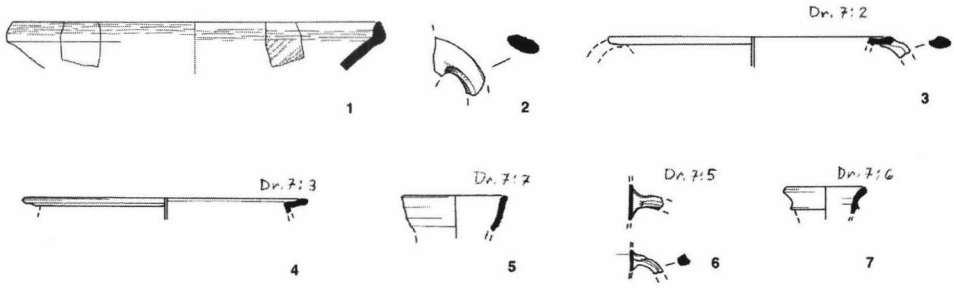

Fig. 37. Pottery from Nazareth village farm. Area A-1.

Description of the pottery by area

Area A-1

A-1 – Locus/Layer 1

No pottery was drawn from Locus/Layer 1. Among the potsherds recovered was a single body sherd which was possibly modern.

A-1 – Locus/Layer 2

Fig. 37:2 is a vertical loop handle, evidently of a krater. This form of vessel is common in the Hellenistic and Roman periods.

A-1 – L10

The pottery recovered from L10 included two everted-rim bowls (Fig. 37:3 and Fig. 37:4) evidently both of the earlier type, without the distinguishing characteristic of a carinated upper body, dating from the mid-first century BC to mid-second century AD (Adan-Bayewitz 1994: 111–119). These bowls were actually small casseroles. A-1–10:1 has a vertical strap handle springing from its rim.

Fig. 37:5 is the slightly everted rim and cup-shaped neck of a storage jar dating to the mid-first century BC to mid-first century AD.

Fig. 37:6 is the vertical strap handle of a cooking pot that evidently dates to the Roman period.

Fig. 37:7 is a juglet with a thickened, everted rim and carinated neck, dating to the first to second centuries AD.

The pottery forms from this Locus/Layer range in date from the mid-first century BC to the second century AD. As a group they all fit comfortably within the first century AD.

Area A-2

A-2 – L1

This locus/layer featured two Galilean bowls, Fig. 38:1 (Adan-Bayewitz 1993: Type 1A) with a single groove on the rim (dated later first century to third century AD), and Fig. 38:2 (Adan-Bayewitz 1993: Form 1B) with a double groove on the rim (dated late first century or second century to mid-fourth century AD).

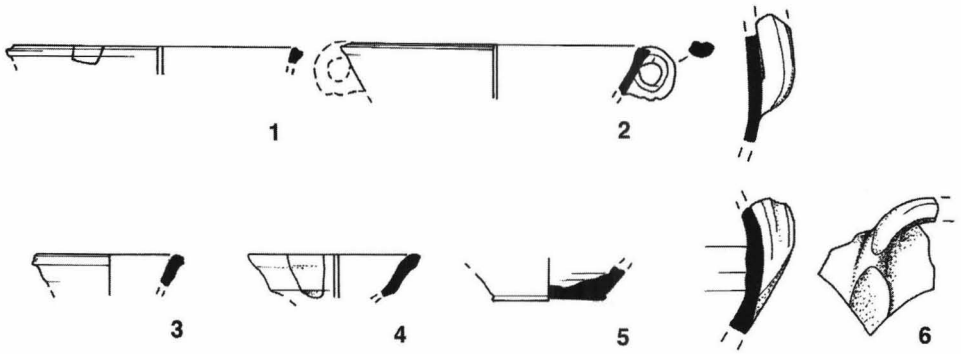

Fig. 38. Pottery from Nazareth village farm. Area A-2.

Fig. 38:3 is the folded, everted rim and short, cylindrical neck of a storage jar that may date to the Herodian period, and Fig. 38:4 is the rim of a storage jar of the Late Hellenistic period. The base of a jug, Fig. 38:5, could date either to the late Hellenistic or Early Roman period.

The drawn pottery gives the impression that this locus/layer dates mainly to the Hellenistic to Early Roman period. Among the pottery sherds that were not drawn, however, there was a bowl evidently dating to the fourth to fifth centuries; an Islamic period bowl with a green glaze; and an example of what appears to be early porcelain, probably dating to the Ottoman period.

A-2 – L2

The single drawn example from this locus/layer, is a vessel, Fig. 38:6, probably a krater or a casserole, with a horizontal handle, possibly dating to the Late Hellenistic or Early Roman period.

Area A-3

A-3 – L1

Fig. 39:1 is a Galilean bowl (Adan-Bayewitz: Form 1E) with a simple rim, that dates from the mid-third century to the early fifth century AD.

Fig. 39:2 is a storage jar with an inverted – everted rim (Meyers, Meyers and Strange 1976: 220–222) dating to the third century to fifth century AD.

Fig. 39:3 is evidently the base of a juglet of the second century to third century AD.

The pottery that was not drawn included a Galilean bowl body sherd; another body sherd which may date to the Abbasid period; and Gaza Ware fragments dating to the Ottoman period.

Fig. 39. Pottery from Nazareth village farm. Area A-3.

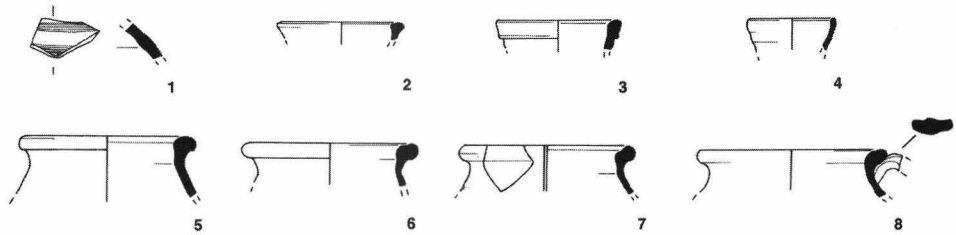

Fig. 40. Pottery from Nazareth village farm. Area B-1.

Area *B-1*

B-1 – F8

Fig. 40:1 is a cooking pot lid decorated with bands of straight combing. It likely dates from the mid to late Byzantine period

Fig. 40:5, Fig. 40:6, Fig. 40:7, and Fig. 40:2 are jars or jugs with thickened, rounded rims dating to the Hellenistic period, probably the second century BC. Fig. 40:6 and Fig. 40:7 have rims that are concave on top. Fig. 40:8 also belongs to this group. It is clear that it is a jug because of the loop handle springing from its rim.

Fig. 40:3 is a storage jar with a relatively short square folded rim probably also dating to the second century BC.

Fig. 40:4 is the incurved rim and cup-shaped neck a storage jar of the Hellenistic period or early Roman period

The undrawn pottery consisted of what appeared to be Roman period body sherds; some possibly Islamic period body sherds; and a Gaza Ware water jar. This locus/layer, predominantly dates to the mid to late Hellenistic period with a few, possibly intrusive, sherds from the Islamic and Ottoman period.

Area *B-2*

B-2 – L5

There was no pottery drawn from this locus. It consisted predominantly of Roman body sherds

B-2 – L7

No pottery was chosen to be drawn from this locus. It included a rim, handle and body sherds evidently dating to the Roman period.

B-2 – L30

Three potsherds, all of storage jars, were drawn from this locus/layer.

Fig. 41:19 with a very short cup-shaped rim, dates from the second century to the fifth century AD, and

Fig. 41: 18 with a short fold on the inside of its rim belongs to the Byzantine to early Islamic period.

The loop handle **B-2 – L30:3** (Dr. 04:01 1998) evidently belongs to an early or middle Roman period storage jar of the first to third century. The undrawn pottery dated to the early Roman; Byzantine; and early Islamic periods.

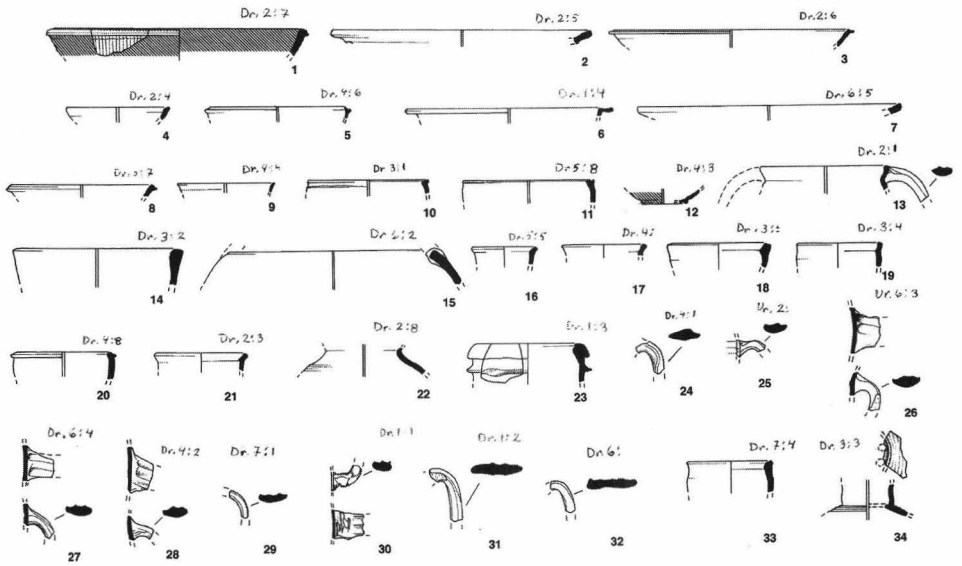

Fig. 41. Pottery from Nazareth village farm. Area B-2.

B-2 – L31

Fig. 41:1 is a deep bowl decorated with a double groove on top of its rim and a ridge on the exterior edge of the rim. It is decorated with rouletting on its exterior wall and covered with a dark reddish-brown wash. It dates to the Byzantine period, probably the fourth century to the fifth century, but may date as late as the sixth to early seventh century AD.

Two Galilean bowls were drawn: Fig. 41:4 has a plain rim (Adan-Bayewitz 1E) and dates to the mid-third to early fifth century AD. Fig. 41:5 is a very small fragment that is evidently an Adan-Bayewitz Type 1B Galilean bowl, with two grooves on top of its rim, dating to the late first or early second century to mid-fourth century AD.

Fig. 41:13 is a globular cooking pot with long loop handles springing from its rim, dated from the fourth century to the sixth century AD.

Fig. 41:6 is a casserole or casserole lid with a bevelled rim. Such vessels appear from the middle Roman throughout the Byzantine period and into the early Islamic period.

Fig. 41:21 is a storage jar dating from the third century to the early fifth century AD.

The storage jar handle (Fig. 41:25) appears to belong to a vessel of the early to late Roman period of the first to third centuries AD.

Fig. 41:22 is probably the neck and shoulder of a storage jar dating to the first to third centuries AD.

Fig. 41:34 is a fragment of a cooking jug with a perforated strainer suspended between its neck and its shoulder. It likely belongs to the middle to late Roman period (late first to third centuries AD).

The undrawn pottery seemed to belong to more or less the same periods as that of the drawn pottery. Some of the sherds may date to the late Hellenistic period but the rest evidently belong to the late Roman to Byzantine periods. It included what may have been Beth Shean jar fragments and a fourth to sixth century cooking pot.

B-2 – L34

Fig. 41:8 Tiny fragment of a rim, probably of a small bowl of the Roman period.

Fig. 41:16 is a storage jar rim (or possibly a jug) evidently dating to the third century to early fifth century.

Fig. 41:17 is the incurving rim of a jug (or possibly jar or pot) of the Roman period.

Fig. 41:20 is the rim of a Gaza Ware jar belonging to the Ottoman period.

Fig. 41:28 is a storage jar handle from the early Roman period to the early Byzantine period.

The undrawn pottery was similar in date to that of the drawn pottery. There was an early Byzantine (third to fifth century) jug and a fragment of an Ottoman cooking pot whose ware contained many shell inclusions.

B-2 – L36

The single example drawn from this locus/layer, Fig. 41:2, is evidently the rim of a bowl or casserole. It is likely an everted-rim bowl (Adan Bayewitz Type 3). The key to determining the precise dating of this type is whether it had a rounded or carinated shoulder. Since its shoulder did not survive it must be dated generally from the first century to the later fourth century AD.

B-2 – L42

Two examples were drawn from this locus/layer:

Fig. 41:15 is a cooking pot with many shell inclusions within its fabric.

Fig. 41:32 is the strap handle of a jar or jug made of Gaza Ware. Both examples date to the Ottoman period.

B-2 – L43

Three very fragmentary sherds were drawn from this locus/layer:

Fig. 41:8 is the edge of the rim of what was evidently a Galilean bowl with a plain rim (Adan-Bayewitz Type 1E), dating from the mid-third century to earlier fifth century AD.

B-2 – L44

The single example represented from this locus/layer Fig. 41:29 is a storage jar handle evidently dating from the first century to the third century AD.

B-2 – L45

None of the pottery of this locus/layer was chosen for drawing. It included Roman body sherds and a single Gaza Ware sherd (personal communication from R. Voss).

Fig. 41:26 and Fig. 41:27 are both handles of storage jars tentatively dated to the first to third centuries.
The undrawn pottery appeared to be of the same date as the drawn pottery: the first to third centuries AD.

B-2 – F2

The single example drawn from this feature (Fig. 41:30) is a storage jar handle evidently dating from the first century to the third century AD.
The undrawn pottery included evidently early Roman period sherds as well as fragments of a Beth Shean jar dating to the Byzantine period and Gaza Ware dating to the Ottoman period.

B-2 – F3

None of the pottery of this feature was drawn. It consisted of Late Roman period to Byzantine period sherds and Gaza Ware fragments of the Ottoman period.

B-2 – F5

A single example was illustrated from this feature:
Fig. 41:32 is a storage jar (or possibly a jug), possibly dating to the third century to early fifth century AD.

B-2 – F6

A single example was drawn from this feature Fig. 41:7 is a Galilean bowl (Adan-Bayewitz Type 1B) with 2 grooves on its rim, late first century or early second century to mid-fourth century AD.

B-2 – F7

Fig. 41:4 is evidently the rim of an everted-rim bowl, possibly Adan-Bayewitz Form 3B, dated from early second century to the later fourth century AD.
Fig. 41:14 is the rim of an Ottoman period cooking pot. There are many shell inclusions within the fabric of the vessel.
Fig. 41:23 is the rim and neck and **B-2 – F7:4**, the handle, of Gaza Ware jars or jugs of the Ottoman period.
In addition to the drawn pottery there were many more Gaza Ware sherds. This feature clearly dates to the Ottoman period.

Area C-1

C-1 – L1

Fig. 42:1 is a Galilean bowl (Adan-Bayewitz Form 1D) bearing two grooves on top of its rim. It dates to the late first or early second to the mid-fourth century AD.
Fig. 42:6 is a jar dating to ca. 50 BC–AD 70.
The deep grooves on handle Fig. 42:7 suggest that it may date from the third century to fifth century AD.
Fig. 42:8 is evidently the body and base of a piriform unguentarium. This form first appeared toward the end of the first century BC and continued to be produced throughout the first century AD. The undrawn pottery from this locus/layer is predominantly of the Roman period.

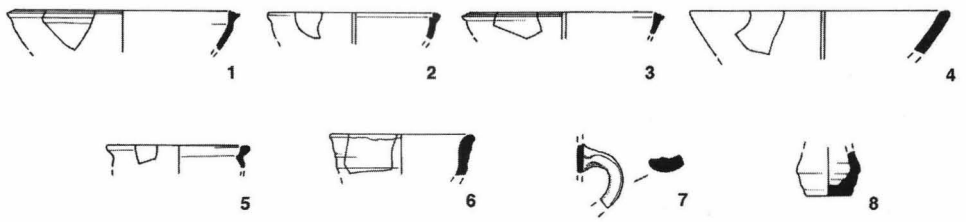

Fig. 42. Pottery from Nazareth village farm. Area C-1.

C-1 – L1(+2)

Fig. 42:2 Galilean bowl (Adan-Bayewitz Type 1B).

The undrawn pottery included what appears to be a third to fourth century jar handle. All the pottery from this locus/layer evidently fits well in the third to fourth century.

Fig. 42:4 is a deep bowl.

Fig. 42:5 is a closed-form bowl with an incurved rim. Considering its context and likely parallels, it probably dates from the first to the fourth centuries.

Area C-3

C-3 – L2

Fig. 43:2 is a Galilean bowl (Adan-Bayewitz Form 1D) with two grooves on top of its rim. It dates to the late first or early second to the mid-fourth century AD.

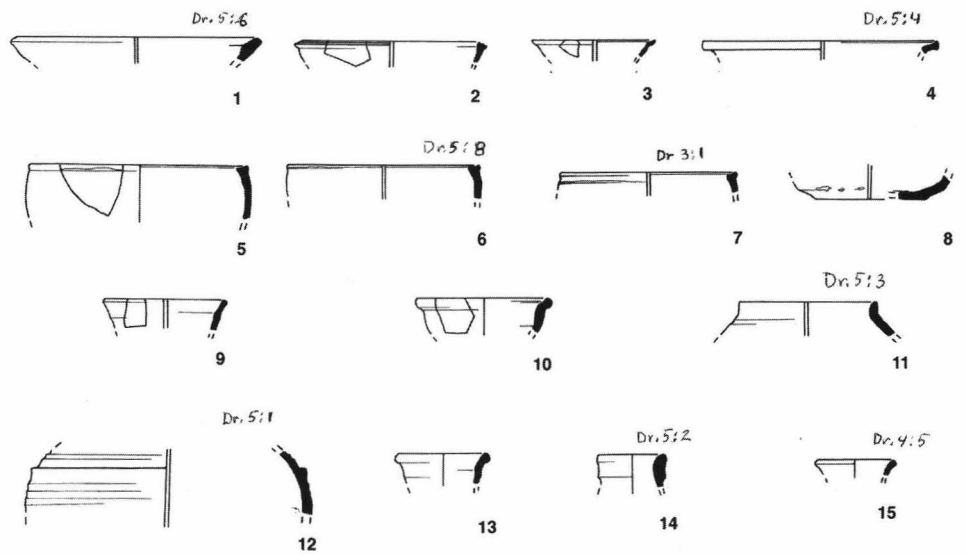

Fig. 43. Pottery from Nazareth village farm. Area C-3.

Fig. 43:3 is a small bowl with a cupped rim.
Fig. 43:5 is the upper part of a hemispherical bowl with a shallow channel rim and dusky red glaze on its interior. No parallel was found for this bowl. It may date to the Ottoman period. Probably the same vessel as B-1/3 = (Dr. 2:5).
Fig. 43:8, also glazed on its interior, is possibly the base of this same bowl.
Parallels for storage jar, Fig. 43:10, date from the second century to the fifth century AD.
Fig. 43:9 is the rim and neck of a typical Northern Israel storage jar possibly dating from the third to early fifth century AD.
Fig. 43:13 Juglet which probably dates from about the middle of the first century to the beginning of the third century AD.
The pottery that was not drawn from this locus/layer included what was evidently a second to third century AD storage jar shoulder with handle and a Hellenistic to early Roman body sherd.

C-3 – L4

Two bowls with glaze on their interiors and over the rim: C-3 – L4: 1 (Dr. 03:01 1998) and Fig. 43:6 join with Fig. 43:5 of L2.
Fig. 43:11 is a storage jar with a short upright rim and a collar-ridge at the base of its neck. It may date to the first to third centuries AD.
The undrawn pottery evidently dated to the early Roman and possibly late Roman period but also included more remnants of a glazed bowl.

C-3 – L5

Fig. 43:1 is a Galilean bowl (Adan-Bayewitz Type 1E) dated to the mid-third century to the earlier fifth century AD.
Fig. 43:3 is a krater dated possibly from the end of the first century to the mid-third century AD.

C-3 – F1

Fig. 43:12 is the shoulder of a storage jar that probably dates to some time in the Byzantine period.
Fig. 43:14 is the rim and neck of a jug or juglet probably dating from the late first through third century AD.
Fig. 43:15 is the rim and neck of a juglet possibly first to the beginning of the second century AD.
C-3 – L8.

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supplied by Elliot Braun. Four seasons of excavations were conducted at the site: in 1997 and 1998 by R. Voss with S. Pfann, in 1999 and 2000 jointly by R. Voss and S. Pfann, with licenses received from the Israel Antiquities Authority (Nos. G-74/1997; G-61/1998; G-138/1999; and G-37/2000). Students from UHL and volunteers participated in the work. Restoration of the terrace farm was conducted under the direction of Mark Goodman. Original plans and sections were drawn by Jay Rosenberg, R. Voss and S. Pfann. Figures and illustrations in this publication were executed by Dov Porotsky. The pottery was drawn by Luba Polyakov, except for the EB platter which is the work of Irena Lidsky-Reznikov. Our thanks to Robert Kool of the Coin Department at the Israel Antiquities Authority for making the identification of the coin and for providing its catalogue information, and to Donald Ariel who provided information on the catalogue of coins from the Nazareth Area, and to Yardenna Alexandre for furnishing the coin information from Mary's Well identified by Ariel Berman.

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Unravelling the Myth of the Synagogue on Delos

LIDIA MATASSA

Introduction

The identification of a synagogue on Delos has been problematic ever since it was first made in 1913 because while there is some evidence relating to Jews and/or Samaritans on Delos not one single piece of it refers to a synagogue or association house.

When we come to look at the material relating to how a building on the island came to be identified as a synagogue, we find a surprisingly large gap between what was originally proposed – and widely accepted – and what has been found. To this day, scholarship continues to build upon the original and quite erroneous identification and, apparently, to be unaware of the nature of the material contradicting it.

Delos is a small island in the Cyclades, measuring just 5 km north to south and 1.30 km east to west (Fig. 1). The mythological birthplace of the gods Apollo and Artemis, it was a major cultic centre by the seventh century BC. It is mentioned in Homer's *Odyssey* (6.160–169) and in Homeric Hymn 3 to Apollo (Crudden 2001).

Delos arrived at its prominent political and economic status by default. According to Thucydides (*Peloponnesian Wars*, 1.96.2; 6.76.3), Xerxes had

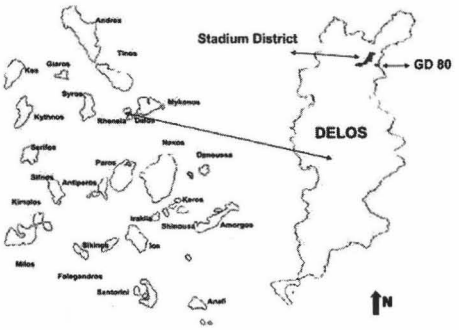

Fig. 1. Map of the Cycladic Islands and Delos showing location of *GD* 80

razed the Athenian sanctuaries during raids into mainland Greece. The Greek city states responded by forming a defensive alliance (478 BC) funded by its member states. To avoid the danger of any one of the city states becoming too powerful, the Athenian-controlled island of Delos was chosen to hold the treasury of what came to be known as the Delian League.

Delos became a hub of commercial, military, maritime trading and slaving activity (the main slave markets were at Rhodes, Delos and Crete; de Souza 1999: 61) whilst continuing to be a major cultic centre. Delos became independent of Athens in 314 BC and, when the Delian League was finally dissolved in the mid-third century BC, its independence continued, along with its economic boom.

Under Roman rule, Athens lobbied for the return of some of her erstwhile territories as Greek power waned. In 166 BC, the Roman Senate returned Delos to Athenian control and it was made a cleruchy of Athens. The Delians were exiled and their land turned over to the colonists. Even so, people were still flocking to Delos from all over the Aegean, many of them establishing businesses, cults and associations on the island (McLean 1996: 189).

The downside of being a thriving and strategically placed cultic, trade and slaving centre was that Delos was often caught between warring factions vying for control of the Aegean. During the first Mithridatic war (88–84 BC), for siding with Rome, Delos was raided by Menophaneses, one of Mithridates Eupator's generals. According to Pausanias (*Description of Greece*, 3.23.2) and Appian (*Mithridateios* 28), some 20,000 of the island's inhabitants were slaughtered during that incursion. There was a further major destruction during the second Mithridatic war (83–81 BC), and another (led by the pirate Athenodoros) during the third Mithridatic war (74–63 BC) (de Souza 1999: 162–163; McLean 1996: 188). The problem of piracy in the Aegean was so widespread that Cicero complained to the Roman Senate in 66 BC, saying that the friends, allies and subjects of Rome had been at the mercy of pirates until Pompey finally drove them away (Cicero, *Leg. Man.* 31–35 and 54–57). In 69 BC, Gaius Triarius, Legate to the Roman Consul Lucullus, repaired some of the damage and built a defensive wall round the town centre of Delos (Phlegon of Tralles, *FGrHist* 257, fr. 12.13; de Souza 1999: 162–163).

By the mid-first century BC, the rise of other trading centres (such as Puteoli and Ostia), as well as the raids and destructions, had taken their toll, and trade routes had altered to accommodate these changes, pushing Delos further and further outside the commercial loop until eventually Athens did not even bother sending its *epimelētēs* to the island, and the priest of Apollo left to live in Athens, only returning for the annual ceremonial sacrifice of twelve animals.

In the second century AD, the philhellenic Emperor Hadrian's attempt to revive the old Delian festivals was unsuccessful (McLean 1996: 189) and by then, according to Pausanias (8.33.2), the island was very sparsely inhabited, although it continued to be cultivated until the last person left, probably during the fifth century AD.

History of the excavations

The École française d'Athènes commenced excavations on Delos in 1873. Between 1904 and 1914, much of the island was excavated. There were further extensive excavations between 1958 and 1975. The École française d'Athènes continues to run excavations on the island in conjunction with the Cycladic Ephoreia, and it maintains a permanent presence there.[1] I will refer to all structures on the island according to their designations in Bruneau and Ducat's seminal guide to the excavations on Delos, the *Guide de Délos* (*GD*; 1983), and to all inscriptions found on the island according to their designations in the collections of inscriptions from Delos, the *Inscriptions de Délos* (the *ID*; Durrbach 1926, 1929; Durrbach and Roussel 1935; Roussel and Launey 1937a,1937b). Using this system, the building known as the 'synagogue' is *GD* 80.

The original identification of the 'synagogue'

It was André Plassart, of the École française d'Athènes who, during the excavations of 1912 and 1913, identified *GD* 80 as a synagogue. His identification relied on six inscriptions. Rather astonishingly, the principal inscription was found not in *GD* 80, but rather some 90 m north of it, in a complex of residential buildings on the east side of the stadium district, and was not associated with *GD* 80 until some time later. This inscription, *ID* 2329, contained the donor names Agathokles and Lysimachos and the word *proseuchē* which, Plassart said, referred to a Jewish 'house of prayer' or 'synagogue'.

Plassart's other five inscriptions were found scattered around *GD* 80 (Plassart 1913: 528), and among these was one which contained one of the donor names found in *ID* 2329 above. Three of the inscriptions contained the epithet *Theo Hypsisto*, and one contained the epithet *Hypsisto*. Plassart's final inscription retained only two legible words, *genomenos eleutheros* '... became free' (Plassart 1913: 528).

In an article written in 1913, André Plassart laid out his argument that the use of the epithets Hypsisto or Theo Hypsisto indicated a tendency towards monotheism and therefore referred to the Jewish deity. However, in the same article, he noted that an inscription had recently been found in Lydia, bearing the epithet Thea Hypsista, probably referring to the Great Mother Goddess of Asia Minor, and that other similar inscriptions had been found in relation to the Thracian-Phrygian deity Dionysos-*S*abazios and to the Syrian Zeus of Heliopolis (Plassart 1913: 529).

So, despite being aware of the non-Jewish uses of the term Theos Hypsistos, and its application to different divinities, male and female, and despite the fact that the inscription on which he was basing his argument was not found in *GD* 80, he proceeded to use it as proof for the existence of a synagogue

(Plassart 1913: 529). According to his argument, since the word *proseuchē* signified a Jewish use and context, he associated the *proseuchē* and *Lysimachos* inscriptions with one another. Considering the use of Theos Hypsistos and Hypsistos in the other inscriptions, and looking at the configuration of the furnishings of the building (arguing that it was similar to later synagogues) Plassart declared *GD* 80 to be a synagogue (Plassart 1913: 528).

I am going to show that the word *proseuchē* in the context in which André Plassart found it refers to the fulfilment of a prayer or votive offering, not to a building and, indeed, probably not to a Jewish context. I will demonstrate that the occurrences of the names *Lysimachos* and *Agathokles* are entirely coincidental and that the old, original and modern arguments relating to the form, style, furnishings and artifacts found in *GD* 80 are irrelevant to its identification as a synagogue. In short, I will demonstrate that there are no compelling reasons to consider *GD* 80 a synagogue.

I will deal first with the limited literary evidence relating to the presence of Jews on the island. I will then turn to the epigraphic evidence and finally to the physical evidence.

The literary evidence

There is very little literary evidence relating to Jews on Delos and while what does exist is useful in establishing the presence of Jews in the region, it does not allude to the existence of a synagogue or indeed to any specifically Jewish structure on Delos.

The earliest reference to Jews on Delos is found in the first book of Maccabees and incorporates a letter from Lucius, a Roman consul:

> Then Numenius and his companions arrived from Rome, with letters to the kings and countries, in which the following was written: 'Lucius, consul of the Romans, to King Ptolemy, greetings. The envoys of the Jews have come to us as our friends and allies to renew our ancient friendship and alliance. They had been sent by the high priest Simon and by the Jewish people and have brought a gold shield weighing one thousand minas. We therefore have decided to write to the kings and countries that they should not seek their harm or make war against them and their cities and their country, or make alliance with those who war against them. And it has seemed good to us to accept the shield from them. Therefore if any scoundrels have fled to you from their country, hand them over to the high priest Simon, so that he may punish them according to their law.' The consul wrote the same thing to King Demetrius and to Attalus and Ariarathes and Arsaces, and to all the countries, and to Sampsames, and to the Spartans, and to Delos, and to Myndos, and to Sicyon, and to Caria, and to Samos, and to Pamphylia, and to Lycia, and to Halicarnassus, and to Rhodes, and to Phaselis, and to Cos, and to Side, and to Aradus and Gortyna and Cnidus and Cyprus and Cyrene. They also sent a copy of these things to the high priest Simon.
>
> (1 Maccabees 15.15–23)

In this passage, the Jews, through the High Priest Simon, have made an offering to the Romans of a valuable shield in return for which the Romans have renewed an old alliance and offered their protection. There is an ongoing debate concerning the chronology of this text, but it is not relevant here.[2]

While this text is useful in that it suggests that the Delians may have had some interaction with Jews, it may be that because we have already assumed that there are Jews on the island, we see the text as confirming their presence there. This has the potential of becoming an entirely circular argument.

What the text *actually* says is only that the Romans have renewed their friendship with the Jews, via a delegation sent to Rome by the high priest Simon, as a consequence of which Rome asked its allies to hand over to the Jewish authorities those who harassed the Jews and 'scoundrels' who, having made war against the Jews, fled to the locations listed in the letter. Notably, there is no mention of *Jews on Delos*, or of any Jewish buildings, houses or associations.

The second text is Josephus's account of the same event. There are variables in this version in that Josephus identifies the Lucius mentioned in the 1 Maccabees passage as the praetor Lucius Valerius, and the island of Delos is not mentioned at all. The chronology of this passage is also disputed (Bartlett 1998: 93–94):

> Lucius Valerius, son of Lucius the praetor, consulted with the senate on the Ides of December in the Temple of Concord. And at the writing of the decree there were present Lucius Coponius, son of Lucius, of the Colline tribe, and Papirius of the Quirine tribe. Whereas Alexander, son of Jason, Numenius, son of Antiochus, and Alexander, son of Dorotheus, envoys of the Jews and worthy men and allies, have discussed the matter of renewing the relation of goodwill and friendship which they formerly maintained with the Romans, and have brought as a token of the alliance a golden shield worth fifty thousand gold pieces, and have asked that letters be given them to the autonomous cities and kings in order that their country and ports may be secure and suffer no harm, it has been decreed to form a relation of goodwill and friendship with them and to provide them with all the things which they have requested, and to accept the shield which they have brought.
>
> (Josephus, *AJ*, XIV.145–148)[3]

While the text is very similar to the text of the Maccabees passage, there is no reference whatsoever to Delos or, again, to the presence of Jews on Delos. Again, past and modern scholarship has assumed that this text refers to Jews on Delos because we assume that, because of its similarity to the passage in Maccabees (above), it must be so. Again, the text actually only notes the renewal of Roman-Jewish friendship and the request made by the Jewish delegation that Jews not be harassed in the autonomous ports and cities of the Mediterranean.

The third text is the most interesting and most substantial. It also comes to us via Josephus, in the form of a letter dealing specifically with the Jews of Delos. This text is thought to date to about the middle of the first century BC:

> Julius Gaius, Praetor, Consul of the Romans, to the magistrates, council and people of Parium, greeting. The Jews in Delos and some of the neighbouring Jews, some of your envoys also being present, have appealed to me and declared that you are preventing them by statute from observing their national customs and sacred rites. Now it displeases me that such statutes should be made against our friends and allies and that they should be forbidden to live in accordance with their customs and to contribute money to common meals and sacred rites, for this they are not forbidden to do even in Rome. For example, Gaius Caesar, our consular praetor, by edict forbade religious societies to assemble in the city, but these people alone he did not forbid to do so or to collect contributions or to hold common meals. Similarly do I forbid other religious societies but permit these people alone to assemble and feast in accordance with their native customs and ordinances. And if you have made any statutes against our friends and allies, you will do well to revoke them because of their worthy deeds on our behalf and their goodwill towards us.
>
> (Josephus, *AJ* 14.213–216)[4]

This text is clear. At some point in the middle of the first century BC, the Jews of Delos (and other Jews) were being prevented by the magistrates, council and people of Parium 'from observing their national customs and sacred rites'. They were not being allowed to meet for religious purposes, to collect religious tithes or to pay for common meals, and assembly by religious societies in Rome had been forbidden, *except for the Jews* who were not forbidden '...to do so or to collect contributions or to hold common meals'. The letter asked that the religious prohibitions against the Jews of Delos (and other neighbouring Jews) be revoked.

We can hypothesize, based on this letter, that the Jews on Delos (and some of the neighbouring Jews) were for some time not permitted the same privileges as Jews in Rome. Thus, at the time of this letter, the Jews at Rome could assemble, collect contributions and hold common meals, but the Jews on Delos (and some of the neighbouring Jews) could not. This does not suggest to me that the Jews on Delos were in a position to have had an identifiable synagogue to use for their traditional practices, given that their religious practices were forbidden by the magistrates, council and people of Parium.

So, it is evident that for at least some unknown time there was a statute of some sort in place forbidding Jews to live in accordance with their native customs, to assemble and to contribute money to communal meals and sacred rites, and it is apposite to note that the prohibition against Jewish practices mentioned in it relates to precisely the period when *GD* 80 is said to have functioned as a synagogue, that is from the middle of the first century BC.

Despite the lack of corroborating evidence, Plassart used the foregoing passage as support for his identification of *GD* 80 as a synagogue. He said

that the text 'undertook to repeal the decree' by which the Jews had been forbidden from observing their ancient customs and, in particular, from organizing communal meals that would have taken place 'in the vast premises of the synagogue' (Plassart 1913: 529). This was the first in a long line of imaginative interpretations of the available evidence, since there is not one shred of evidence connecting *GD* 80 with a reading of the letter about the Delian Jews in Josephus other than Plassart's original assumption (based on his association of the inscriptions mentioned above and in more detail below) that it was a synagogue.

The passage in Josephus does not allude to a synagogue or house being used as a synagogue, and then being prevented from being used as a synagogue. Indeed, it only says that Jews on Delos (and other neighbouring Jews) were being prevented from following their traditional practices and that the Romans thought it desirable that this should change, in line with Roman administrative leniency relative to Jews.

At best, therefore, we have one direct reference to Jews on Delos (and other neighbouring Jews, either on the island or elsewhere in the region either in the Cyclades or the Dodecanese, or even Aegina, Crete, Rhodes or Cyprus; and not necessarily on Delos at all), in the first century BC, suggesting that they were, for some unknown period of time, prevented from following their traditional practices.

As this text provides the only clear reference we have to the presence of Jews on the island of Delos, it must be examined in that context. So, what we do have is what appears to be a reliable and plausible reference to the presence of Jews on the island of Delos, albeit one that is wholly dependent on Josephus. What we do not have is a reference to a synagogue or association house or community building of the Jews on Delos.

The inscriptions

As stated above, Plassart's evidence for the identification of *GD* 80 as a synagogue consisted of six inscriptions. The principal inscription was found in house IIA of *GD* 79, some 90 m north of *GD* 80 at the southeastern side of the stadium district in the densely packed residential area. This inscription contained the names *Agathokles* and *Lysimachos* and the word *proseuchē* which, Plassart said, referred to a Jewish 'house of prayer' or 'synagogue' (and following Plassart most scholars have agreed with this interpretation).

Inscription 1 (*ID* 2329)[5]

Ἀγαθοκλῆς καὶ Λυσίμαχος ἐπὶ προσευχῆι

'Agathokles and Lysimachos for an offering/prayer'[6]

This inscription was found in house IIA of *GD* 79 beside the stadium, 90 m northwest of *GD* 80. It has been dated to around the first century BC and is carved on a plain rectangular marble stele with a cut on the top side containing the remnants of a lead fixing, indicating it held a statue or votive offering, which is not part of any known Jewish custom. The presence of the lead fixing is strong support for the argument that this inscription cannot be a Jewish one. Moreover, as there is no definite article used in the wording of the inscription, the words ἐπί προσευχῇι in this context cannot refer to a building and must be translated as reading 'for an offering' or simply as a 'prayer' (in the sense that a prayer to a deity is always an offering) and not 'for the synagogue' (as Plassart translated it in his 1913 article and as others have continued to do). This basic point is often ignored in the scholarship on the subject or dismissed as irrelevant. Quite clearly, it is not.

On the basis of his presumption that *ID* 2329 indicated the existence of a synagogue, Plassart identified the two names listed on it as Jewish and, as a direct consequence, the names *Agathokles* and *Lysimachos* on Delos have been listed in the *Lexicon of Greek Personal Names* (the *LGPN*) as Jewish. This has created an entirely circular argument for anyone looking for external corroborating evidence concerning these names.

In addition, there are other contemporary instances of the name *Agathokles* from Delos that are not identified as Jewish, including one from the Agora of the Competalists (*ID* 1760);[7] one from the Portico of Antigone (*ID* 1965);[8] one from a list of donors and subscribers found in and belonging to Sarapeion C (*ID* 2618);[9] one from an *Ephebium* list (*ID* 2598);[10] one on a decree of the Athenian cleruchy (*ID* 1497)[11] in honour of the musician *Amphikles*; and one on a white marble stele found in the Sanctuary of the Syrians (*ID* 2263).[12] Despite these other instances of the name *Agathokles* on Delos being roughly contemporary with *ID* 2329, they are not listed as Jewish in the *ID* or the *LGPN*.

Inscription 2 (*ID* 2328)[13]

Λυσίμαχος ὑπὲρ ἑαυτοῦ Θεῷ Ὑψίστῳ χαριστήριον

'Lysimachos for himself [to] God Most High [for a] votive/thank-offering'[14]

This inscription is carved on a small piece of white marble. It was found lying at the foot of a wall in *GD* 80. This inscription is also dated to the first century BC. It was the use of the name *Lysimachos* in this inscription that caused Plassart to associate *ID*s 2329 and 2328 together, resulting in the identification of *GD* 80 as a synagogue.

Again, the identification of the name *Lysimachos* as Jewish in the *LGPN* was made solely on the basis of Plassart's original identification and, again, there are other contemporary inscriptions from Delos containing the name

Lysimachos that are not identified as Jewish. The name appears on *ID* 1764,[15] relating to the Association of Competalists and again on *ID* 2616:[16] a list of donors and subscribers to Sarapeion C.

The fact that the names *Lysimachos* and *Agathokles* both appear in lists of donors and subscribers to Sarapeion C is interesting, and it is well worth mentioning here that the internal configuration of *GD* 80 (our supposed synagogue), *GD* 91 (Sarapeion A) and *GD* 100 (Sarapeion C) is very similar indeed – with benches placed around the internal walls. What these commonalities and similarities mean is, of course, open to interpretation, but it is clear at least that the names *Lysimachos* and *Agathokles* themselves are no indicator of Jewishness on Delos and that the existence of benches around walls does not, in and of itself, imply synagogue use.

***Inscription 3* (*ID* 2330)**[17]

Λαωδίκη Θεῶι Ὑψίστωι σωθεῖσα ταῖς ὑφ' αὐτοῦ θαραπήαις εὐχήν

'Laodike to God Most High for healing him of his infirmities, an offering'[18]

This inscription is carved on a rectangular base of white marble. It was found in *GD* 80, and has been dated to around 108/107 BC. It is a healing inscription in the style of a Greek votive rather than a Jewish dedication. The name *Laodike* is identified in the *LPGN* as possibly being Jewish, but this is again only on the basis of Plassart's identification. There is one other instance of the name *Laodike* from Delos, *ID* 2628,[19] among a list of donor and subscriber names on a marble plaque, which was discovered in the Theatre of the Syrian Sanctuary. However, only Plassart's *Laodike* inscription is identified as Jewish.

***Inscription 4* (*ID* 2331)**[20]

Ζωσᾶς Παρίος Θεῷ Ὑψίστῳ εὐχήν

'Zozas of Paros to the God Most High, an offering'[21]

This inscription was found on a bench in the west of room A in *GD* 80. It is carved on a small base of white marble, in the shape of a horned altar, which Plassart described as 'slightly pyramid-shaped' (it is not). It is dated to the first century BC, and the name *Zozas* is identified in the *LGPN* as possibly belonging to a manumitted slave, but not specifically identified as a Jewish name. The style of this base and that of *ID* 2328 is very similar, and there are many of examples of this type of inscribed base all over Delos itself (and indeed all over the ancient Near East). There was no other instance of the name *Zozas* in the *ID*.

***Inscription 5* (*ID* 2332)**[22]

Ὑψίστῳ εὐχὴν Μαρκία

'[The] Most High [from] Markia'

This inscription was found on a bench in the west of room A in *GD* 80. It is carved on a small, white marble base and dates to the first century BC. The name *Markia* is again identified as Jewish in the *LGPN* on the basis of Plassart's identification. It is the only instance of this name on an inscription from the Delos that I was able to find.

***Inscription 6* (*ID* 2333)**[23]

γενόμενος ἐλεύθερος

'... became free'[24]

This inscription is carved on a small rectangular base of white marble and was found in *GD* 80. The marble is very badly damaged and only those two words can be made out. Given the position of Delos as one of the main Aegean centres of the slave trade, it is hardly surprising to find that there are inscriptions relating to the freeing of slaves found there. Furthermore, there were other inscription bases found in *GD* 80 which neither Plassart nor subsequent scholars have chosen to mention, and whose texts are illegible.[25] It is evident, thus, that other than its proximity to the other four inscription bases found in *GD* 80 (and the one found some 90 m away in the stadium district) and discussed by Plassart, there is nothing Jewish about this inscription and it is merely Plassart's association of the bases that has linked it with the others.

It becomes clear, when looked at in the light of all of the foregoing, that the inscriptions used by Plassart to identify *GD* 80 as a synagogue are, in fact, unrelated. They, like many of the other pieces of marble on the island have ended up together in building *GD* 80 where there is a lime kiln for melting down marble to make lime, and I will return to this point below.

The Samaritan inscriptions

In 1979, two inscriptions were found by Philippe Fraisse of the École française d'Athènes. They were both found in an unexcavated area just beneath current ground level, on a street where they had fallen from the exterior wall onto which they had been fixed, near the shoreline about 100 m north of *GD* 80. Both are written in Greek, and both are dedicated by the 'Israelites who offer to Holy Argarizein' (Mount Gerizim in Samaria).

These two inscriptions do seem to provide evidence of Samaritans on the island, but it is also possible that the dedications were made by Samaritan visitors and traders to the island on behalf of their religious communities at home. It is likely that if there were a Samaritan (or Jewish) community on

Delos that it came there in the same way as the other multinational migrants, to benefit from the free trade status of Delos and to deal in merchandise and slaves from around the Mediterranean region. Unfortunately, other than these two inscriptions, there is no literary, archaeological or epigraphic evidence to tell us anything about Samaritans on Delos. Of course, it is possible to theorize, based on the inscriptions and on the passage in Josephus (*AJ* 14.213–216) above, that the references to the Jews on Delos could relate to Samaritans and that the building from which the two inscriptions came could have been a Samaritan synagogue.

Samaritan inscription 1

Οἱ ἐν Δήλῳ Ἰσραελεῖται οἱ
ἀπαρχόμενοι εἰς ἱερὸν
Ἀργαριζεὶν στεφανοῦσιν
χρυσῷ στεφάνῳ Σαραπίωνα
Ἰασονος Κνώσιον εὐεργεσίας
ἕνεκεν τῆς εἰς ἑαυτούς[26]

'The Israelites on Delos who make first-fruit offerings to Holy Argarizein crown with a golden crown Sarapion son of Jason of Knossos for his benefactions on their behalf'[27]

This inscription has been dated to somewhere between 150 and 50 BC (Bruneau 1982: 469–474). There is substantial damage to the upper area of the stele, but it does not affect the text (Bruneau 1982: 474). The inscription honours *Sarapion (*son of *Jason of Knossos*) for his benefactions on behalf of the 'Israelites on Delos' but does not offer any details as to the presence of a permanent community of Samaritans on the island, and it is not clear whether the Sarapion honoured in the text is a Samaritan, Jew or pagan himself. It does, however, identify the dedicators as 'the Israelites on Delos', which indicates a community of Israelites on the island, be it a temporary, seasonal or permanent one.

Samaritan inscription 2

Ἰσραηλῖται οἱ ἀπαρχόμενοι εἰς ἱερὸν ἅγιον Ἀργαριζεὶν
ἐτίμησαν υαχ Μένιππον Ἀρτεμιδώρου
Ἡρακλειον αὐτὸν καὶ τοὺς εγγόνους αὐτοῦ
κατασκευάσαντα καὶ ἀναθέντα ἐκ τῶν ἰδίον ἐπὶ
προσευχῇ τοῦ θε[οῦ] TON [- - - - - - - - - - - - -]
ΟΛΟΝ ΚΑΙ ΤΟ [- - - - -] καὶ ἐστεφάνωσαν] χρυσῷ
στε[φά–]νῳ καὶ [- - - - - - - - - - - - -][28]
ΚΑ . . .
Τ . .

[The] 'Israelites who make first-fruit offerings to holy Argarizein honour Menippos, son of Artemidoros of Heraclea, himself as well as his descendants to have established and dedicated its expenses, for an offering/prayer [to God], [- - - - - -] and [- - - -] and crowned it with a golden crown and [- - -]'[29]

The second inscription is tentatively dated to between 250–175 BC and is carved onto a white marble stele (Bruneau 1982: 469–474). There is a great deal of damage to the bottom portion of the text, with the second half of the text entirely missing.

The second Samaritan inscription refers to a donation of some unknown thing or act. It is unfortunate that this second inscription, whose damaged portion probably contained the details of the donation, has not survived intact, and thus the two Samaritan inscriptions do not really clear up any of the mystery for us. It is to be hoped that the bottom fragment of the second inscription might at some point be found and the text fully reconstructed so that we might at least know what was offered.

The second inscription is similar to the first and honours Menippos (son of *Artemidoros of Heraclea*) for his benefactions in establishing something somewhere on Delos (perhaps where the stele fell to the ground where it was ultimately found), and again offers no clues as to the presence of a permanent community of Samaritans on the island. Again, it is not clear whether the Menippos of the text is a Samaritan or pagan himself. It is the 'Israelites' who honour Menippos, but the text itself does not say the 'Israelites of Delos', despite reconstructions that include it.

The text of the second inscription has been interpreted on the basis that it must be worded like the first. However, it is inscribed on a reused stele with an earlier text blocked out, and whoever inscribed the new text over the old did not include the words *on Delos*. Nevertheless, Philippe Bruneau of the École française d'Athènes reconstructed it thus (Bruneau 1982: 474).

It is possible that this dedication, like the first, might relate to a non-resident donor or group of Samaritans, or to a group who did not have the same legal status on Delos as those who dedicated the first stele, and its wording and styling is very like that of the first Samaritan inscription.

To add further confusion to the translation and interpretation of the two Samaritan inscriptions, Plassart's initial translation of the phrase ἐπὶ προσευχῆι (from *ID* 2329) as 'for the synagogue', has led to a number of scholars translating the same phrase in the second Samaritan inscription in that way, leading them to think that the building from which the inscription came was a synagogue. Also, as White points out, Bruneau translated ἐπὶ προσευχῆι in the second Samaritan inscription as 'in ex-voto', 'for a vow', whereas in relation to *ID* 2329 he accepted Plassart's reading of it as 'for the synagogue' (White 1987: 142).

In any event, the two Samaritan inscriptions provide at least some indication that the texts referring to the Jews on Delos in Josephus and Maccabees

might relate to Samaritans. The dating of the inscriptions is broad (*c.* 250–50 BC) and it could be that offerings were sent to Mount Gerizim while the temple still stood there; or that offerings continued to be made and sent to Samaria after the destruction of the temple. Or, indeed, it could be that the offerings, in whatever form they took, were made on Delos only, perhaps in the form of votives and dedications by either Samaritan visitors to the island or by Samaritans [*Israelites*] who lived on the island.

In the light of the discovery of two Samaritan inscriptions, it has been suggested that there were communities of both Jews and Samaritans on Delos, and that the letter recorded in Josephus refers to both (White 1987: 153), and I agree it is possible that this is so. However, while the reference in Josephus (*AJ* 14.213–216) to the 'Jews in Delos and some of the neighbouring Jews' does indicate that there was more than one 'Jewish' community in the area, as I have already said, it is possible that these 'neighbouring Jews' may have been on other islands, either in the Cyclades or the Dodecanese or indeed other larger islands in the region, such as Crete, Rhodes or Cyprus. Since we know of the Jewish population on Delos only from Josephus, and of the Samaritans only from the two Samaritan inscriptions, it is difficult to see how this conundrum can be resolved without substantial excavations of the area immediately east of the stadium.

At any rate, the names associated with the Samaritan inscriptions – Jason of Knossos and Menippos, son of Artemidoros of Heraclea – are not specifically identifiable as Jewish or Samaritan names.

Theos Hypsisto/Hypsistos

Writing in 1913, Plassart (1913: 529) outlined his belief that the use of the epithets *Hypsisto* or *Theo Hypsisto* indicated 'a tendency towards monotheism', and Jewish monotheism in particular. However, the inscriptions that refer to *Hypsistos* may also refer to the Greek deity *Zeus Hypsistos*, whose cult (a healing cult, and a more likely association given the physical form of the inscription bases) also used these epithets to describe their chief deity. The sanctuary of the cult of *Zeus Hypsistos* was located on Mt. Cynthus, less than 500 m from *GD* 80.

Plassart only identified the names from the group of inscriptions he considered to be related (see above) as being Jewish without looking at other occurrences of those names on Delos. Additionally, as I have already stated, he noted an occurrence of the term *Thea Hypsista*, which he acknowledged as referring to a Near Eastern female deity, possibly the Great Goddess of Asia Minor (Plassart 1913: 529). Taking this together with the recurrences of the names contained in the inscriptions (as outlined above) Plassart's argument is considerably and correctly diminished. Furthermore, the names on the two Samaritan inscriptions may or may not be Jewish and could be the names of non-Jewish Cretan donors. If it were

possible to relate the two names (Menippos and Jason) from Crete to a Jewish family there, it would be a significant advance in the scholarship on the subject.

Belle Mazur (1935: 21–22) noted that the style of the inscribed bases was inconsistent with Jewish practice, in particular the *proseuchē* and the *Lysimachos* inscriptions which had lead fixings in place for votive offerings or statues. She made the first connection with the Greek cult of *Zeus Hypsistos,* in whose sanctuary on the Athenian Pnyx were found similar inscribed bases, and to the cult of *Theos Hypsistos* from Asia Minor. Mazur was the first to note that Plassart's translation of the phrase ἐπί προσευχῆι as meaning 'for the synagogue' was incorrect because the definite article is absent from the inscription. She correctly translated it, as I also do, as 'for a prayer/votive' (Mazur 1935: 21–22). There is no other way to translate the phrase, and to attempt to do so is to manipulate the evidence to fit a preconceived idea of what it is 'supposed' to mean.

Interestingly, there is another cult that used the epithets *Hypsistos* and *Theos Hypsistos*: the *Hypsistarians* who, while they recognized other gods, considered theirs as being above all. Part of their ritual is described in an inscription carved on one of the blocks of the Hellenistic inner face of the city wall of Oenoanda in northern Lycia:

> Born of itself, untaught, without a mother, unshakeable, not contained in a name, known by many names, dwelling in fire, this is god. We, his angels, are a small part of god. To you who ask this question about god, what his essential nature is, he has pronounced that Aether is god who sees all, on whom you should gaze and pray at dawn, looking towards the sunrise.
>
> (Mitchell 1999: 193–4)

According to descriptions of their practices, the Hypsistarians stood in the open air facing east, looking up to heaven and offering their prayers. Lamps and fire were an essential part of their cult, which was associated with heaven and the sun (Mitchell 1999: 91), and, by the dedication of light, it was thought possible to establish a link with the deity (Mitchell 1999: 92).

The final phase of building *GD* 80 is oriented eastwards, is unroofed, and 40 lamps were found in it by Plassart's excavation team. While it is impossible (and, indeed, would be foolish) to attribute the use of the final phase of *GD* 80 to the Hypsistarians, there is nothing to suggest that the lamps could not have been used in a ritual such as that described in the Oenoanda Oracle. There is certainly no known Jewish ritual with which to compare this and, to add further to this idea, even as late as the fourth century AD, Hypsistarians were sometimes mistaken for Jews (Mitchell 1999: 93–94). In any event, I have offered the Hypsistarians up for consideration *only* to illustrate how tenuous and tendentious the identification of *GD* 80 as a Jewish and/or Samaritan synagogue is.

GD 80 (the building identified as 'the synagogue')

It is important to note that there is nothing in the structure of *GD* 80 itself that is in any way Jewish in nature, although I am mindful of Levine's always useful suggestion that Jews and Jewish architecture are influenced by local material culture.[30] However, as I will show, the internal arrangement of benches in *GD* 80 (which reminded André Plassart of the configuration of later synagogues) is not in any way unique on Delos.

The physical evidence

GD 80 lies on the northeastern shoreline of Delos in the Bay of Gournia, outside the defensive town walls built by Triarius in 69 BC. It stands in the area just east of the stadium and northeast of the gymnasium and *Ephebium* (Figs. 1 and 2).

When Plassart excavated the site in 1912, he found a large rectangular room measuring 16.90 m (north to south) by 14.40 m (west to east). The floor of this room had a coarse flaked marble/gravel-like covering, and there was some plaster left on the base of some of the walls, and rooftiles scattered around the floor. Dividing this originally rectangular space into two almost equal parts, with room A in the north and room B in the south, is an east-west wall with three doorways. This wall was erected some unknown time after the north, west and south walls (it is not bonded into them), and is made up of local gneiss, rubble, and worked marble from abandoned or destroyed buildings including pieces of capitals, marble inscription bases, triglyphs and thresholds.

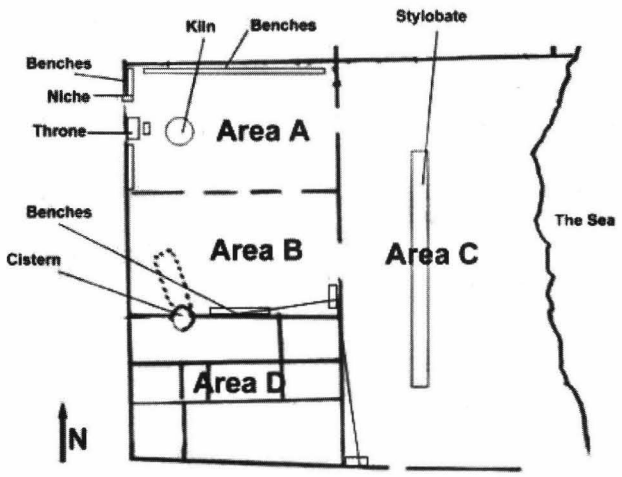

Fig. 2. Plan of *GD* 80 (after Mazur 1935)

There was also a further space, room D, along the south of the building, parallel with rooms A and B, which was divided into smaller chambers and which may have contained a stairwell. Running beneath part of rooms B and D is the cistern around which the building was constructed.

According to Plassart, rooms A and B served as the assembly halls of a synagogue, based on his assumptions about Inscriptions *ID* 2329 and *ID* 2328 and on the letter preserved in Josephus. There are white marble benches in place in this area dating to the period he argued that *GD* 80 was in use as a synagogue (from around the middle of the first century BC). There are also benches running along the south and west inner walls of room B, and some more benches running along the south, west and north walls of area C (the corridor between the main rectangular space and the peristyle courtyard to the east).

In the centre of the west wall of room A is a white marble throne (Fig. 3). This was found *in situ* with the marble benches on either side, along the inside west wall of the area A (Plassart 1913: 526). This throne is obviously similar to the first century BC throne for the priest of Dionysos in the theatre in Athens, or the stone thrones in the Ampherion at Oropos, and to others all over the Graeco-Roman world. Reuse of valuable objects is clearly a sensible way to

Fig. 3. The marble throne in *GD* 80

reduce the cost of furnishing any given space, and the throne (and its footrest) may well come from the theatre on Delos, on the west side of the island.

The benches in *GD* 80 appear to be identical with those still left in the *Ephebium* of the nearby gymnasium, from whence they may have been removed after the destruction and abandonment of the gymnasium some time around 74–63 BC.[31] Of course, this does not prove that *GD* 80 was not used as a synagogue, but it is also striking that two of the Sarapeia on the island have a similar layout, including similarly reused benches. Thus, the internal configuration of *GD* 80 is not in itself evidence that it was used as a synagogue. There are other buildings on the island with this sort of benching still apparent, such as in the Heraion (Deonna 1938: Pl. VII, photo 60); the Italian Agora (Deonna 1938: Pl. VIII, photos 64, 69); in the semi-circular *exedra* of the Sanctuary of Apollo (Deonna 1938: Pl. VII, photo 60); the Ephebium and in the orchestra of the theatre (Deonna 1938: Pl. VIII, photos 67, 68), as well as others dotted around the island.

It is possible to date – approximately – the second phase of the building by reference to the material used in the rebuilt areas of the internal walls, and especially to the marble taken from the nearby gymnasium. A second century BC inscribed base (*ID* 1928) of the Gymnasiarch *Poses* was used in rebuilding one of the walls of *GD* 80, after the destruction or removal of the statue which it carried. Another gymnasium inscription base (*ID* 1923b) relating to *ephebes* under the rule of the *Gymnasiarch Diotimos Theodosion* (126/125 BC) was also found in another rebuilt wall. Other inscriptions from the gymnasium ended up being reused in the Palaestra of the Lake on the western side of the island. As the gymnasium was plundered during the pirate raids of the Mithridatic wars, it is only from this time (74–63 BC) that *GD* 80 could have been adapted for the sort of use that required the seating arrangement found there (Plassart 1913: 532).

On the eastern side of the building is area C, the remains of the corridor and step or stylobate leading out into what was originally a peristyle courtyard. The peristyle would have measured approximately 18 m × 18 m, but has now been destroyed by the sea almost up to the line of the stylobate (Fig. 4). In October 2003 I saw that the northern and southern walls of the existing structure extend to almost the same point of collapse into the sea, some 1.50 m beyond the stylobate, and rooftiles were found along the inside of these perimeter walls indicating that they were at least partially covered.

The seaward side of area C retains a section of a stylobate running parallel just over 6 m from the easternmost wall. The visible section is made of blocks of white marble resting on a gneiss foundation. This line stops approximately 5 m from the north and south walls of area C.

Plassart and other scholars (most notably, Mazur 1935; Bruneau 1970; White 1987, 1990; Binder 1999 and Trümper 2004) interpreted the physical layout of the first phase and second phases of *GD* 80 in several ways, none of which really has much bearing on its identification as a synagogue, other

Fig. 4. View from stylobate of *GD* 80 down to sea

than the fact that in the final phase of the structure it had benches arranged around the walls of the two main areas and that the final phase is oriented towards the east. However, as I mentioned above, this seating arrangement is something of a red herring given the configuration of Sarapeion A (Fig. 5) and Sarapeion C (*GD* 100) in the area between the theatre and Mount Cynthus. There is also, of course, the further connection between the names from the inscriptions found in and near *GD* 80 and the donor names on the Sarapeion C list of subscribers (and the associations of Hermaists and the Poseidonists). There is no dispute about the identification of the list of subscribers to Sarapeion C. It was found in, and specifically refers to, that structure. In fact, more than 170 dedicatory and votive offerings and inscriptions relating Isis, Sarapis and Anubis were found in Sarapeion C alone (Bruneau and Ducat 1983: 227).

Like most large buildings on the island, *GD* 80 had its own cistern (Figs. 2 and 6). *GD* 57 and *GD* 80 both have large courtyards in addition to a smaller peristyle court. This was quite normal for some of the larger Hellenistic houses on Delos where one courtyard was often deeper and sometimes taller than the other, in order to enhance the entrance to a reception room.[32] This certainly seems to apply to *GD* 80 and has implications for its orientation, which is not

Fig. 5. View of *GD* 91 – Sarapeion A

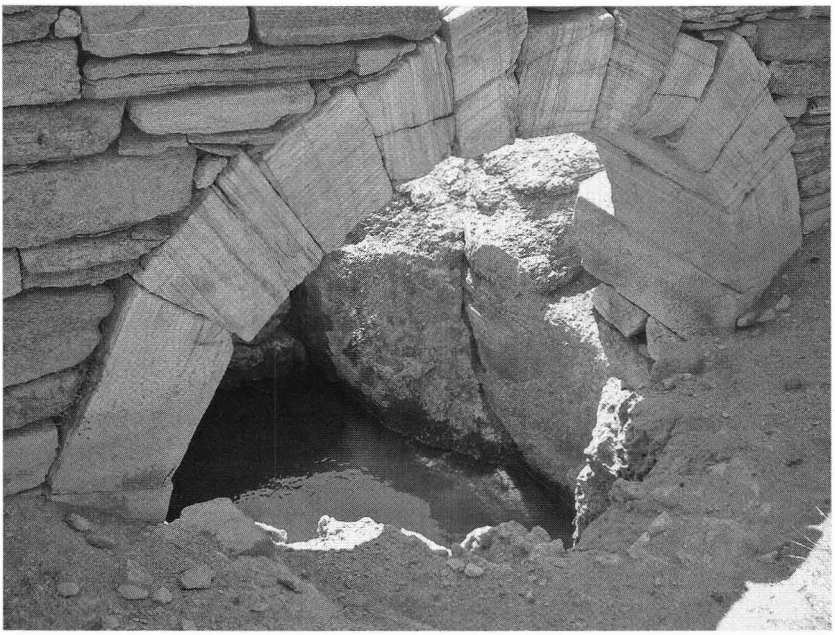

Fig. 6. The cistern in *GD* 80

eastwards since the throne in the benched area of A sits in what was originally one of the two courtyards of the house.

Other than lamps, antefixes, rooftiles and inscription blocks, there was nothing found in the building that would enable it to be absolutely identified as belonging to a particular group, religious or otherwise, although the number of lamps found in the structure is quite curious in itself, and I will come back to this point a little later. In particular, there was no artifact, structure or inscription found within *GD* 80 which is of a Jewish nature. As I have already discussed above, and as described by Mazur in 1935, a number of the inscription or statue bases found in *GD* 80 are in the form of Greek and Near Eastern 'horned' altars, including two of the bases cited as Jewish (Inscription 2 (*ID* 2328) and Inscription 4 (*ID* 2331) by Plassart in 1912/1913.

The rectangular structure of *GD* 80 still retains the remains of a coarse chipped marble floor, which has sometimes been described as 'mosaic', but is more like rough tessellation. Areas A and B are bisected by an east-west wall with three doorways. This wall was erected some unknown time after the north, west and south walls, as it is not bonded to them. When it was excavated in 1912/13 its three doorways were found walled up. This east-west wall is made up of local gneiss, rubble, and reused material from other buildings, including pieces of capitals, marble inscription bases and thresholds. There are also three doorways on the east side of the structure, providing access to areas A and B from the peristyle courtyard along the shoreline.

The cistern

Uniquely on the island, *GD* 80 appears to have been constructed over a rock-fault which was extended into a cistern by means of vaulting. For those who built the house this fault must have represented a convenient location, since it meant the degree of excavation necessary to provide the house with its water supply was considerably lessened. Philippe Bruneau, of the École française d'Athènes, is the only person, following Plassart, who has excavated on the site of *GD* 80 and in 1962 he excavated and cleaned out the well/cistern which André Plassart had left untouched. Bruneau also made extensive and detailed plans of the cistern structure as well as the site in general. Unfortunately, the list of finds from the cistern is not complete but included a piece of bluish marble; a fragment of a bluish marble bowl; three antefixes of beige/pink clay decorated with palmettes; some fragments of a vase with a ringed wall;and three fragments of blown glass (Plassart had also found numerous fragments of small glass vases in *GD* 80, but not in the cistern). From the cistern, Bruneau recovered the only one of the 41 lamps not found during the original excavations of *GD* 80. This lamp dates to the first century BC and depicts a man and woman copulating (Fig. 7).

Only the area immediately underneath the arch of the cistern was accessible when it was in use and although the floor is now quite opened out, this is only

Fig. 7. Lamp 4591 – Copulating Couple (recovered from cistern in *GD* 80)

because of Bruneau's 1962 excavations (Bruneau 1970: 481). Even with the excavated opening, access from room D is both difficult and precarious as the opening lies under and extends only a metre from the arch (Fig. 8). Access is even more restricted from room B as there is a sharp and sheer drop from the floor level to the bottom of the cistern (Fig. 9). There are no steps built into the cistern, and there is insufficient space in the opening in rooms B or D for access via a ladder for the purposes of bathing.

Fig. 8. Cistern from Room D

Fig. 9. Side view of cistern from Room B to show height of arch relative to benches (author's photograph)

It has been suggested that the cistern in *GD* 80 could have been used as a *mikveh* (Binder 1999: 306), but this is physically impossible as access to it would have been even more difficult when the floor was intact. The arch above the cistern provides limited access to the cistern from both B and D and the *highest* point of the arch is just 32 cm off the original floor level (Fig. 9)! The cistern is deep (the bottom of the fault lies at 4 m in places) and is by no means a level surface, running some 6.08 m in length, under a vaulted roof, and was probably constructed before the rest of the building was finished (Bruneau 1970: 481). The arches over the opening to the

cistern serve not only as access for the drawing of water, but also bear weight for the wall that divides areas B and D, so that the floor does not collapse into the cistern.

Binder also says that Bruneau suggests that a wooden ladder or stairs may have been used to enter the cistern for ritual ablutions.[33] What Bruneau actually said was, 'À cette interprétation on peut toutefois objecter que [the cistern] fait défaut tout dispositif d'évacuation d'eaux usées', ('In this interpretation one could, however, object that the cistern is lacking any mechanism to deal with the disposal of waste water') (Bruneau 1970: 481).

Furthermore, while there may be water in the well/cistern from the water table, there is no running water, and it would undoubtedly have presented a most unsatisfactory manner in which to bathe, ritually or otherwise. Emptying this cistern would have been almost impossible, especially as it is partly fed from the aquifer. Most importantly, on an island devoid of a surface supply of water, bathing would have rendered the cistern useless for the collection of water for domestic purposes. This, in turn, would suggest that the building ought to have had a separate domestic water supply if it had a *mikveh*. It does not.

Donald Binder (1999: 306) cites Bruneau as having said that the cistern in *GD* 80 was unusual in that it allowed for human access, but he is incorrect on two counts. The first is that many of the cisterns on Delos are constructed to incorporate stone stairways specifically designed for human access.[34] The second is that Binder did not understand what Bruneau said, which was that *according to Plassart*, it is possible to take water from room B via an opening in the wall framed by a marble arch, leaving just enough space to draw water from Room B. However, Bruneau also went on to say that if this is possible now, it is only because part of the floor is missing, and that he was *not able to accomplish the task himself* [my emphasis] (Bruneau 1970: 482). In any case, access is somewhat better from room D, and it is likely that it was properly accessed from there when the cistern was in use.

The lime kiln

In room A of *GD* 80 there is a substantial lime kiln measuring some 2 m in diameter (Figs. 2 and 10). Produced by melting down marble, lime was a valuable commodity in the ancient world. In agriculture, it was used as a fertilizer and to improve drainage. Lime was also used in construction. Mortar for laying masonry was made by mixing lime with sand. Concrete was made by mixing the lime with crushed or natural stone. Plaster was covered with a similar mix to mortar, with a coat of lime on top. Lime putty was used to set fine brickwork and masonry. Lime white is a mixture of the lime and water and was used for whitening walls, the traditional 'whitewash', and lime plaster was used to waterproof cisterns.

Fig. 10. Lime Kiln in *GD* 80

The town centre of Delos, as it became further and further removed from the commercial and strategic centres of the Mediterranean, lay abandoned and in ruins. The marble lying around the island remained one of its final assets. The lime kiln in *GD* 80 was most likely put in place in the post-abandonment phase of the site as the burning or melting down of marble (an expensive and imported commodity) for lime generally only occurred when the Mediterranean marble trade was tapering off, that is, from about the third century AD, and possibly as late as the fourth century AD, and there was agriculture and viticulture on the southern part of the island up until the beginning of the fifth century AD when the island was finally abandoned, so some of that obsolete marble would have been burned down to make lime to use for this purpose (Brunet 1990).

When Plassart found the marble inscription bases in rooms A and B of *GD* 80, he stated (without explaining his reasoning) that they were not associated with the kiln (Plassart 1913: 526). Given that a number of large marble column barrels and inscription bases were also found in *GD* 80 probably waiting to be sawn into smaller pieces before being burned down, and given also the variety of the inscription bases found in *GD* 80, including two small marble inscription bases with no visible text or with wholly eroded text (Deonna 1938: Pl. CXII, photos 969–970), which were found by Plassart

in the same area as *IDs 2330, 2331* and *2332* – discussed earlier – it is logical to expect that the marble found in this building was destined for the flames of the kiln.

The various interpretations of GD 80

Plassart (1913) identified *GD* 80 as a Hellenistic house with a formal portico entranceway on its eastern extremity. Belle Mazur (1935) interpreted the main structure as a Hellenistic house with a peristyle courtyard, rather than a portico. Both options are equally possible. Mazur's reconstruction of it was based on parallels of size and layout with other houses on the island, specifically *GD* 57, the House of the Poseidonists, on the western side of the island. Having looked at the extant houses on the island, it is my view that *GD* 80 is more comparable with the House of the Hermes (*GD* 89) (Fig. 11) near the theatre, which had at least three storeys, accessed from various external and internal stairways. The ground floor plan of this house appears to be very similar to the ground floor plan of *GD* 80, as well as to the ground floor plans of the House of the Dauphins (*GD* 111) and the House

Fig. 11. *GD* 89 – The House of the Hermes

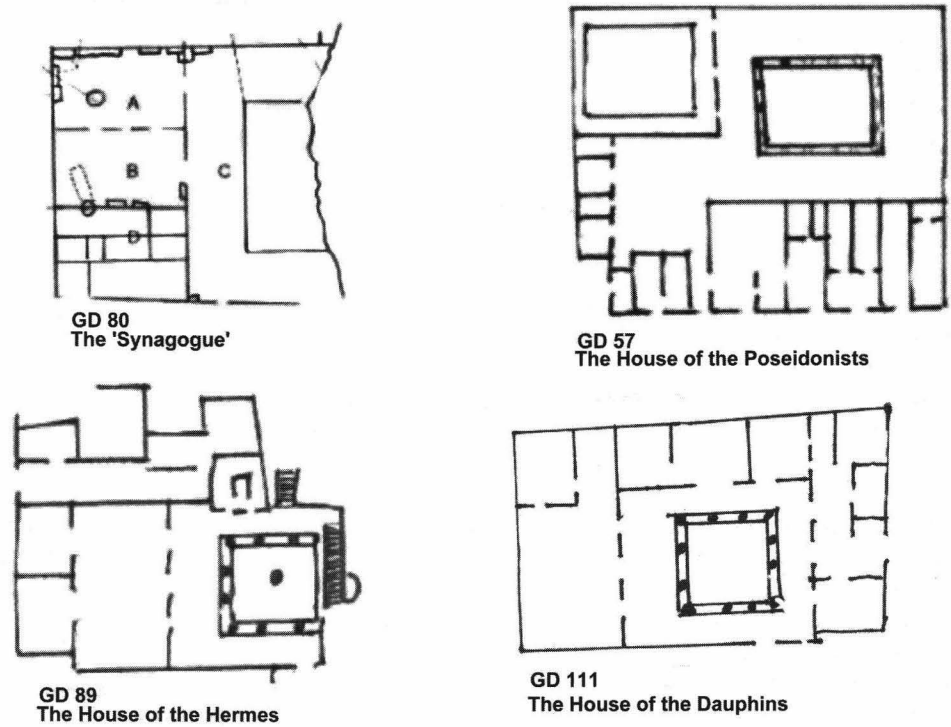

Fig. 12. Four Similar House Plans (after Ducat and Bruneau 1966)

of the Poseidonists (*GD* 57) (Fig. 12). In any event, without any evidence to corroborate its identification, none of these interpretations of the original layout of *GD* 80 have anything to do with it ever having been a synagogue.

Belle Mazur's was the only dissenting voice on the subject of the so-called synagogue on Delos, and, while her interpretation of the physical structure of the building was very similar to that of Plassart and others, her interpretation of the inscriptions and statue bases found in the building was not. She argued that they were not consistent with a Jewish context, that Plassart's inscriptions were not Jewish and that *GD* 80 was therefore not a synagogue, but some sort of establishment belonging to the Greek cult of *Theos Hypsistos* (Mazur 1935: 22).

Eleazar Lipa Sukenik (1934) accepted André Plassart's interpretation of *GD* 80. However, once he had read Mazur's 1935 analysis of the evidence, he changed his mind. Writing in 1949, he said: 'the case of the so-called "Synagogue" at Delos shows how misleading incomplete research can be', and went on to conclude, based on Mazur's argument, that the word προσευχή could only mean 'prayer' and not 'synagogue' because of the absence of the definite article in the inscription; that the deity referred to as '*hypsistos*', was the Greek

god Zeus; and that the form of the inscribed bases was pagan and not Jewish (Sukenik 1949).

Bruneau (1970; 1982) accepted Plassart's identification and dismissed Mazur's rebuttal of Plassart's work, along with Sukenik's later acknowledgement of the correctness of her rebuttal. Bruneau insisted that the inscriptions showed that *GD* 80 was a sanctuary of the Jewish God Most High, *Theos Hypsistos*, since the name *Zeus Hypsistos* does not appear on the inscriptions and since the cult of *Zeus Hypsistos* had its own sanctuary on Mount Cynthus.[35] He also rejected Mazur's argument concerning the format and style of the inscribed bases, saying that the Hellenized Jews of the Diaspora assimilated certain pagan customs which over time became established in their religion. Peculiarly, even though he agreed with Mazur's translation of the phrase ἐπὶ προσευχῆι as 'for a prayer/offering', he accepted Plassart's reading of it as 'for the synagogue' and insisted that προσευχή remains 'an essentially Jewish term' (Bruneau 1970: 488), concluding that *GD* 80 was a synagogue of an exceptional type, and that the endurance of the Jewish cult on Delos even after the destructions of 88 and 69 BC confirms the references in the literary sources (Bruneau 1970: 485). However, those texts, as I have shown, do not refer to any structure at all, let alone to a synagogue. At the very best, they confirm the presence of Jews on Delos (and other *neighbouring* Jews), and indicate that the Jews on Delos were for some time unable to follow their customary religious practices.

Michael White (1987: 80; 1990: 138) concluded that because there is some external evidence of a Jewish community on Delos, *GD* 80 would have fitted their needs and that in all likelihood it was a Samaritan synagogue that was founded. I have no argument with this. Like many other buildings on the island, *GD* 80 *could* have been a synagogue. It is only that there is no evidence that it *was* a synagogue, be it Jewish or Samaritan (White 1990: 152).

Alf Thomas Kraabel (1992) came to the conclusion that *GD* 80 was a synagogue on the basis of the earlier debate (rejecting Mazur's critique and Sukenik's support of it), and relying on Bruneau's presentation of the material. His main argument for the identification of *GD* 80 as a synagogue rests on the epigraphical references to *Theos Hypsistos* in the inscriptions found by André Plassart (Kraabel 1992: 491) which, he says, 'do not offer an obviously pagan use of the term at a time when references to one or other pagan deity as *Hypsistos* are not uncommon'.[36] As I have shown above, the inscriptions are out of context and unrelated, and one of them only contains the epithet *hypsistos* and not *theos hypsistos*.

Whilst Kraabel (1992: 493) acknowledged the ambiguity of the *proseuchē* inscription, he concluded it was nonetheless Jewish. He did not remark on the form or style of the inscribed bases, nor did he note or refer to the cuttings for lead fixings.

Hudson McLean (1996) took the two Samaritan inscriptions as proof that *GD* 80 was a synagogue, but a Samaritan one. In McLean's interpretation of

the physical structure (wholly adopted from White), he noted that there was no provision in *GD* 80 for cultic rites, that there was no altar or shrine and that therefore the congregation 'related to a remote external cult, namely the Samaritan cult practiced at Mount Gerizim' (McLean 1996: 195).

Peter Richardson (1996) interpreted *GD* 80 as a 'remodelled house adapted to the needs of the worshipping community' (Richardson 1996: 97). He accepted that Plassart and all those who followed on from his work were correct and that *GD* 80 was a synagogue.

Binder (1999) made what is probably one of the most ambitious of all the interpretations of the building. Based only on the letter preserved in Josephus (*AJ* 14.213–216) and on Plassart's and later, Bruneau's interpretation of the material he found, he described *GD* 80 as 'a synagogue with an ancillary banquet hall used to hold feasts on sacred days' and argued that the dividing wall between Rooms A and B presented 'the first serious architectural evidence suggesting the division of the sexes within the synagogue' (Binder 1999: 299). He deemed that access to the cistern from rooms B and D was part of the proof for this claim, on the basis that it was possible that the cistern might have functioned as a *mikveh* (Binder 1999: 316–317) which, as I have shown is unlikely, both physically and domestically.

Lee I. Levine (writing in 2000), accepted Bruneau's conclusion that *GD* 80 was a synagogue, and referred to the 1970s as the point at which a scholarly consensus was arrived at (Levine 2000: 100; apparently on the basis of Philippe Bruneau's publication of the site). Levine described *ID*s *2328, 2330, 2331* as having been inscribed on 'column bases', which is incorrect. These inscriptions are actually on carved stelae, some in the shape of horned altars, some rectangles with lead fixings. Levine further mentioned *ID* 2329 (the *proseuchē* inscription), noting that it could have been used in a pagan context but that, combined with the other ancillary evidence and the discovery of the two Samaritan stelae in 1979 by the École française d'Athènes, it added up to sufficient evidence to identify *GD* 80 as the earliest synagogue thus far found (Levine 2000: 100–101). But, as I have shown, the Samaritan inscriptions and the inscriptions found by André Plassart are unrelated and, while the Samaritan inscriptions are unquestionably evidence of some sort of Samaritan community on Delos, Plassart's inscriptions are not Jewish.

Levine (2000: 103) asked whether there were two separate synagogues (one Jewish, one Samaritan) or one synagogue serving both communities. He went on to conclude that the location of the Delian Jewish community was in a 'relatively isolated part of the island'. In fact, *GD* 80, the *proseuchē* inscription and the two Samaritan inscriptions were found in densely populated areas, each not more than 100 m or so from the others, abutting a heavily occupied residential area on the east side of the stadium. This area has not been fully excavated yet, but it is evident from Bruneau's plans, my own observations in October 2003 and by cursory examination of satellite views of the site

from the *Google Earth* website, that there are sub-surface and above-surface walls all over the area, so that there is practically no unused ground in that quarter. There was simply no room in the town and town-adjacent areas of this small island for isolation of any sort.

Trümper (2004) acknowledged that the identification of *GD* 80 as a synagogue was made primarily on the basis of the inscriptions and furnishings. She cited just three scholars, Bruneau, White and Binder, as being sufficient to explain the history and use of *GD* 80 'because no substantially differing views have been presented in the literature' (Trümper 2004: 569). In a footnote she goes on to qualify this with the extraordinary statement that the earlier opponents to the 'synagogue' argument (Mazur and Sukenik) 'can be ignored here'. She is ultimately drawn into a circular argument of her own making and cannot then acknowledge the full force of the Mazur argument against the identification of *GD* 80 as a synagogue. Of course, she is hindered in her view by not actually having read Mazur's article.[37]

Trümper goes on to cite the *four* inscriptions found in *GD* 80 that bear the name *theos hypsistos*. She is incorrect in this detail: only three of the inscriptions bear the epithet *theos hypsistos* (*ID*s *2328, 2330* and *2331*). One of the inscriptions bears only the epithet *hypsistos* (*ID 2332*). She goes on to say that the use of this epithet is still debated, although it is now generally agreed that it was used (although not exclusively) 'by Diaspora Jews (and also Samaritans) to refer to their god' (Trümper 2004: 569). This may well be the case from about the middle of the first century AD for the use of the epithet *theos hypsistos*, but it is by no means certain in the first century BC or earlier – the period to which Trümper refers. By using later evidence to support earlier data without any corroboration, she creates yet another circular and potentially misleading argument.

Trümper goes on to make another incorrect statement, saying that there is an ongoing discussion about the *three other* Jewish and Samaritan inscriptions: 'One was discovered in a private house nearby, in the *Quartier du stade*, and the other two, on stelae, were found in an unexcavated area some 90 m north of *GD* 80' (Trümper 2004: 571). Here she has confused two separate things. The two Samaritan stelae to which she alludes were discovered in 1979 by Philippe Fraisse of the École française d'Athènes (see the section above on inscriptions). However, the so-called *third* inscription to which she refers is the original *proseuchē* inscription that Plassart found back in 1912 (*ID* 2329), which was indeed found in the stadium district, in Habitation IIA of *GD* 79 (see section on inscriptions, above) and which she refers to separately and earlier in her article. Thus, she has introduced more obfuscation into the argument by duplicating a piece of evidence and treating it as though its existence supports her argument that it and the Samaritan inscriptions may have originated in *GD* 80.

There are a number of other claims made by Trümper to which I must also refer. One is that the niche in the wall of room A postdates the construction of

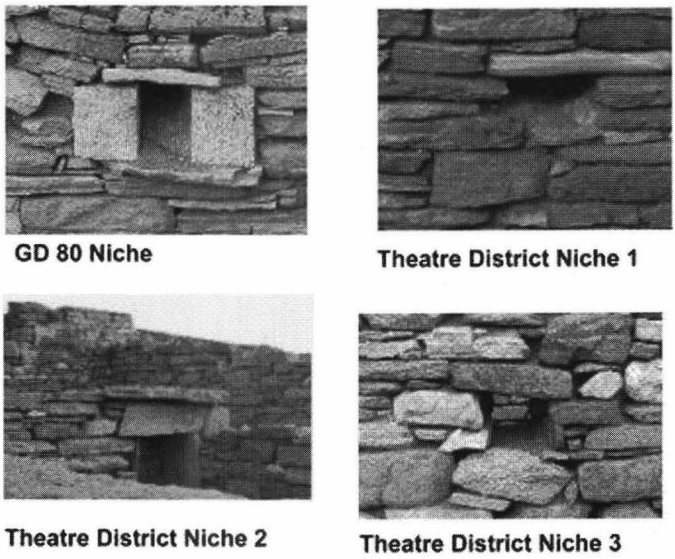

Fig. 13. *GD* 80 niche and two other niches on Delos

the wall and is 'rather crudely made' (Trümper 2004: 585). This is incorrect, as anyone who has looked at other structures on the island will see. The niche is an integral part of the original Hellenistic construction. I found numerous other such niches, constructed and dressed in precisely the same manner and these niches are common all over the island and elsewhere (Fig. 13).[38] They could indeed have been used, as she suggests, for placing lamps to light building interiors. It is also possible that they were shrines of some sort, as were those recorded by Renfrew during his extensive excavations on Phylakopi (see Renfrew 1985: 11–12, Pl. 12b).[39]

Trümper (2004: 585) also says that the inscription bases resemble 'altar incense burners', and were probably used in the 'synagogue' on Delos, a claim, for which, again, there is no evidence whatsoever. Trümper cites Anders Runesson (2001: 437) here as support for this argument, but Runesson does not offer any support for this specific contention, and indeed his comments on meal and incense offerings relate only to the petition to restore the Temple at Elephantine some time before its ultimate abandonment[40] and not to any purported synagogue usage, then or later.

Conclusions

Because we know so very little about early synagogues, it is important to proceed carefully with the available evidence and not to reach into inherently

teleological solutions to explain what we do not as yet have answers to. The problem with these and other interpretations of the structure, identification and internal furnishings of *GD* 80 (other than Mazur's) is that they are predicated on the pre-existing belief, following Plassart, that *GD* 80 is a synagogue. They are not based on the physical, literary or epigraphic evidence. The argument, for instance, that the Samaritan inscriptions provide *additional* proof that *GD* 80 was a synagogue is spurious since it is clear from all the evidence that the initial identification of *GD* 80 as a synagogue was made on the basis of the tenuous association of two inscriptions by Plassart, and that *that* initial association is clearly not supported by the evidence.

Plassart's identification of *GD* 80 as a synagogue may have given rise to an historical distortion in the chronology of the development of synagogues in the diaspora. Indeed, some scholars have dated the 'Delian synagogue' not even to the last phase of the building (when the benches were added), but to its Hellenistic origins in the third century BC, and all on the basis of the first inscription that Plassart discovered 90 m north of *GD* 80.

The question to ask must surely be, if Plassart had not originally associated the inscriptions from *GD* 79 and *GD* 80, whether such an identification could ever have been made. The answer to that question is clearly 'no'; such an identification of *GD* 80 as a synagogue on such tenuous material would be deemed implausible.

It is safe to say that while there is nothing that would exclude *GD* 80 from being a synagogue, there is not one piece of evidence that would suggest that it actually *was* a synagogue. Each of the hypotheses arguing that *GD* 80 was a synagogue is based on the association of those first two inscriptions by Andre Plassart back in 1912. Nor does the discovery in 1979 of the two Samaritan inscriptions go to support the identification of *GD* 80 as a synagogue, though it raises many other questions and makes other interesting interpretations possible.

All that can be said with certainty is that there were Jews or Samaritans (or both) on Delos from some time in the first (or possibly second) century BC, and that they were prevented from following their traditional customs for an unknown period of time during the first century BC.

While it is possible that there was a synagogue (Samaritan or Jewish, or both) on Delos, there is as yet no evidence that it has been found. Because of the restrictions on the traditional practices of some cults and associations, including the Jews, in the first century BC, it is also possible that if Jews assembled for religious purposes, they did so in private dwellings, not in cultic establishments, in which case they would have remained hidden and unidentifiable. Moreover, the letter preserved in Josephus (*AJ* 14.213–216) relating to the Jews being forbidden to follow their religious traditions and customs is dated to precisely the time that it is argued *GD* 80 functioned as a synagogue, that is, to the middle of the first century BC.

As I have already said, the issue of physical evidence is complicated because in this period it is not certain that we should be looking for synagogues since religious structures are bound to be of an ambiguous nature if that worship was forbidden by local law. An obvious example would be that when Christians were being persecuted under Roman rule there were no purpose-built Christian churches or basilicas. Private houses, bath houses, crypts and even catacombs were used as meeting places, and overt architectural statements of identity only emerged when the political climate of religious tolerance made safe for them to develop.

All in all, it is impossible to identify *GD* 80 as a synagogue on the available evidence. It is furthermore impossible to identify any other structure on the island as a synagogue. It is also clear that other than the two Samaritan (Israelite) inscriptions, nothing specifically pertaining to Jews or Samaritans has been found on the island. I have shown that the names *Lysimachus* and *Agathokles* are not indicators of 'Jewishness' on the island and appear elsewhere in very specifically non-Jewish contexts on the island. The only names associated with a Jewish or Samaritan context on Delos are those of Jason of Knossos and Artemidoros of Heraclea, both apparently from Crete. And again, we do not know if these were Samaritan benefactors or pagan donors or patrons.

I have to conclude, therefore, that the vexed question of the existence of a synagogue on Delos must remain open, and that we must hope for specifically Jewish and/or *more* Samaritan material to be found to help with any potential identification.

Notes

1 The École française d'Athènes maintain a number of houses on the island for the purpose of accommodating their archaeologists during the digging seasons and I am most grateful to their Director of Studies, Mde. Michèle Brunet for arranging to open one of their dig houses for me, and to Panayotis Chatzidakis of the 21st Ephoreia of Prehistoric and Classic Antiquities, for giving me permission to stay on the island in October of 2003.

2 For the essentials of the debate on the chronology, see Goldstein 1998: 84–100.

3 Josephus, *Jewish Antiquities, Books XII-XIV*, The Loeb Classical Library, edited and translated by Ralph Marcus (London/Massachusetts: 1943).

4 Ibid.

5 Roussel and Launey 1937: 295; Plassart 1913:, 205, n.2; Plassart 1914: 526, n.1; Frey, *CIJ*, I, n.726.

6 My translation.

7 Roussel and Launey 1937a: 119.

8 Roussel and Launey 1937a: 188.

9 Roussel and Launey 1937b: 395.

10 Roussel and Launey 1937b: 374.

11 Roussel and Launey 1937a: 1.

12 Roussel and Launey 1937b: 278.

13 Roussel and Launey 1937b: 295; Plassart 1913: 205, n.2; Plassart 1914: 527, n.2; Frey, *CIJ* I, n.729.
14 My translation.
15 Roussel and Launey 1937a: 122.
16 Roussel and Launey 1937b: 389.
17 Roussel and Launey 1937b: 296; Plassart 1913: 205, n.3; Plassart 1914: 527, n.3; Frey, *CIJ* I, n.728.
18 My translation.
19 Roussel and Launey 1937b: 401–2.
20 Roussel and Launey 1937b: 296; Plassart *1913:* 205, n.4; Plassart 1914:, 527, n.4; Frey, *CIJ* I, n.727.
21 My translation.
22 Roussel and Launey 1937b: 296; Plassart 1913: 206, n.5; Plassart 1914, 528, n.5; Frey *CIJ* I, n.730.
23 Roussel and Launey 1937b: 296; Plassart 1913: 206, n.6; Plassart 1914: 528, n.6; Frey, *CIJ* I, n.731.
24 My translation.
25 Deonna 1938: Pl. CXII, photographs 969 and 970.
26 Translation based on Bruneau 1982: 469.
27 My translation.
28 Bruneau 1982: 471–474.
29 My translation.
30 Levine 1982: 6; 1998: 23; 2000: 581 rightly makes the point that Jews, wherever they may be, have borrowed from and adapted what they saw around them.
31 Around 200 m distant from *GD* 80. The *Ephebium* is where the education of *ephebes* took place under the supervision of the *Gymnasiarch*. The construction of the benches there is very similar to the construction of the benches in *GD* 80, and the throne would probably have been used by the *Gymnasiarch* who instructed the *ephebes*. The throne could, alternatively, have come from the theatre as it is identical to other theatre 'VIP' chairs.
32 Boardman, Griffin and Murray 1988: 388.
33 Binder 1999: 306.
34 The cistern of *GD* 79 (the building where *ID* 2329 was found), for example, has a stone staircase leading down into that cistern.
35 Bruneau 1970: 486–487. However, one of the inscriptions (*ID 2332*) contains has the epithet *hypsistos* and not *theos hypsistos*.
36 Kraabel 1992: 493.
37 Trümper 2004: 519 n.17, says that she had no access to Mazur's 'book'. However, I had no difficulty in obtaining a photocopy of what is actually a short article in 2003 from the École française d'Athènes while I was staying in Athens before travelling south to Delos.
38 Niches from other buildings on Delos. Unfortunately, I have lost my references to exactly which houses I took the photographs in, but they were taken in the Theatre District in October 2003.
39 Renfrew 1985: 11–12 and plate 12b.
40 Runesson 2001: 437.

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Greek Inscriptions from Khirbet el-Jiljil and Beit Jimal and the Identification of Caphar Gamala

LEAH DI SEGNI AND SHIMON GIBSON

In an article published in the *Revue Biblique* (2006), Professor Émile Puech of the École Biblique et Archéologique Française in Jerusalem suggested an interesting reading for a Greek inscription engraved on a lintel, which was discovered during a survey at Khirbet el-Jiljil near Beit Jimal (for further details: Strus and Gibson 2005: 34, Figs. 4: 2 and 6). Puech based his reading on a squeeze made from the surface of the lintel. On the basis of this reading, and on the assumption that Beit Jimal (Bet Gemal) is to be identified with Caphar Gamala, the place where the body of St. Stephen the Protomartyr was purportedly discovered in AD 415, Puech reached the conclusion that the inscription must have come from a large circular structure nearby, which he identified as the mausoleum of the Protomartyr. However, the lintel in question has been re-examined by the present authors, as well as the squeeze of its surface, and the examination does not lend support to the interpretation proposed by Puech. Moreover, the large circular structure was used solely as a wine press during the two stages of its existence.

The Khirbet el-Jiljil inscription

The lintel was discovered in 2003 during the course of a field survey that preceded the archaeological excavations at Khirbet el-Jilil (Strus and Gibson 2005: Fig. 3:9) (Fig. 1). It was found lying on the surface of the site, upside down, and judging by its position it could have come from any one of the adjacent buildings situated within the lower ruins of the site, or, indeed, from the monumental circular building which is located further upslope as suggested by Puech. The surface of the lintel was carved with a *tabula ansata*. The central rectangular area measures 0.70×0.46 m, with triangular 'handles' extending for 0.22 m on either side (Fig. 2).

A careful surface examination was made of the stone at the time of the discovery by the late Father Andrzej Strus and Shimon Gibson, but except for a few faint indentations on its surface and a possible incised cross visible at the bottom of the main frame, the excavators were unable to

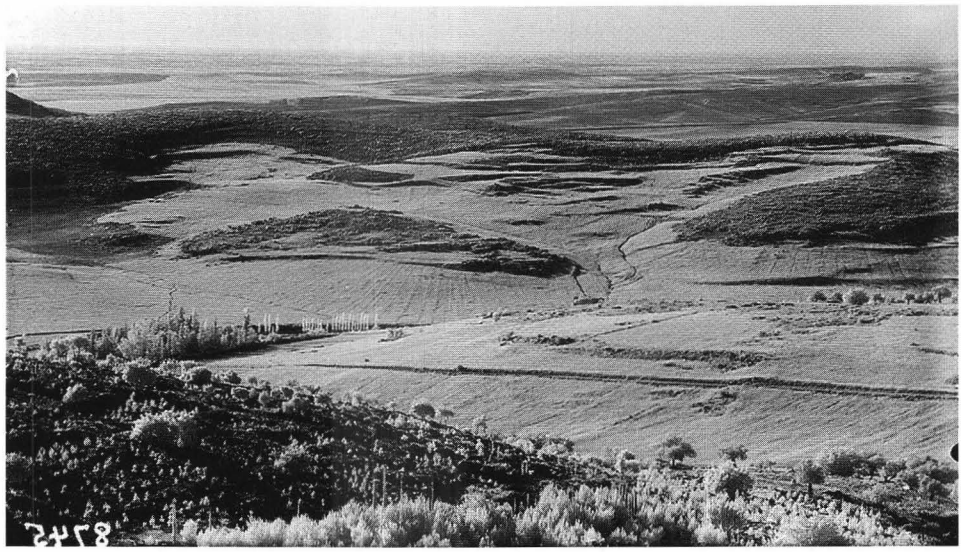


Fig. 1 A general view of the countryside to the north-west of Beit Jimal, with the ruins of Khirbet el-Jiljil in the lower right of the picture, taken in the early twentieth century (No. 03787v, courtesy of the Library of Congress Photographic Archives, Washington)

determine the existence of an inscription on it (see the photograph and drawing in Strus and Gibson 2005: Figs. 4:2, 6).[1] However, in order to establish some certainty in this matter it was decided to make a squeeze of the surface of the stone and this was subsequently undertaken by Professor

Fig. 2. The lintel with a *tabula ansata* frame from Khirbet el-Jiljil (photo: S. Gibson)

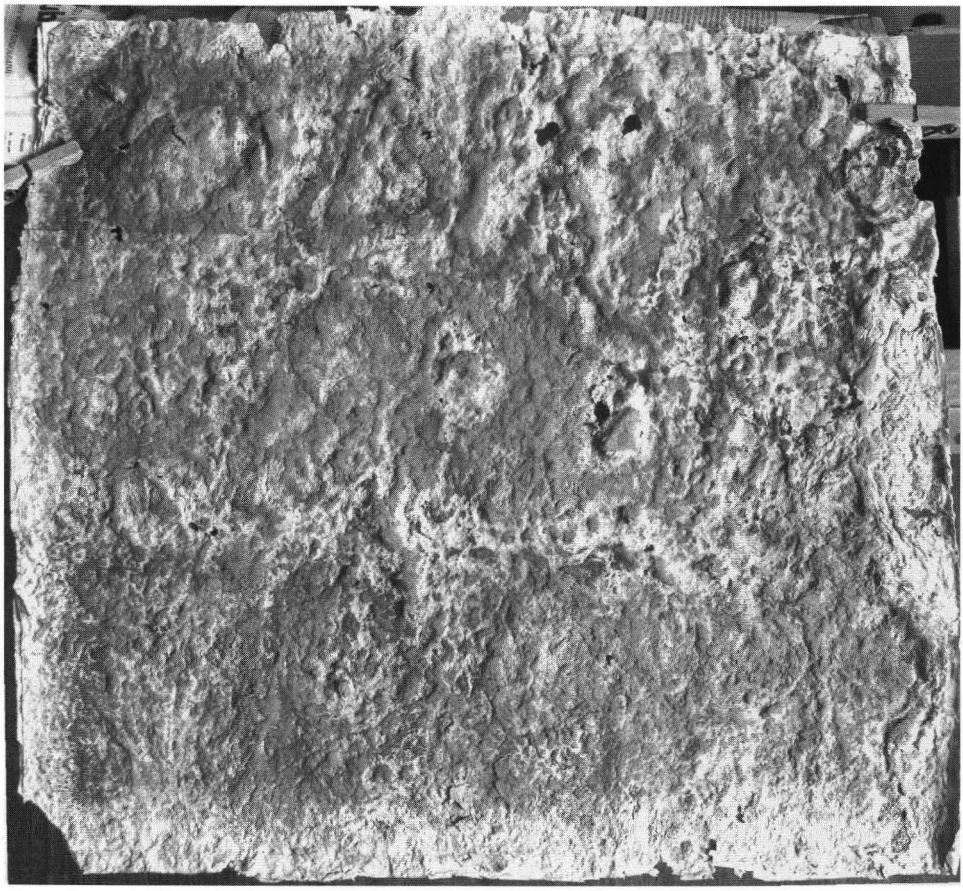


Fig. 3. The squeeze of the lintel (photo: S. Pfann)

Puech. On 29th June 2004 the excavators were informed by Puech that his analysis of the squeeze indicated that at least one line of faint Greek letters was discernable, incised into the top register of the surface of the stone, but that his results were still at a preliminary stage (Strus and Gibson 2005: note 5).

Eventually, a full reading of the inscription was published by Puech (2006: 110, Fig. 3), as follows:

ΤΟΔΙΑΚ^{s} - -	Τὸ διακ(ονικὸν) [Στε-]
ΦΑΝΟΠ - -	φάνου π[ροτο-]
ΜΑΡ^{s}+	μάρ(τυρος) +

The diakonikon of Stephen Protomartyr

Analysis of the squeeze

The squeeze made by Puech was examined by the authors and found to contain additions made with orange-coloured chalk in order to emphasize the letters which Puech believed he could make out. Therefore it was decided to inspect the lintel again and to make a proper photographic examination of the squeeze using special angled lighting (Fig. 3). A careful inspection of the stone and the squeeze ascertained that there are indeed faint Greek letters visible in three registers on the surface of the lintel (Fig. 4). However, we were unable to confirm the existence and clarity of several of the Greek letters proposed by Puech (Fig. 5B). Indeed, some of the indentations on the squeeze would appear, following a new examination of the surface of the stone itself, to be natural fissures and weathered grooves caused by environmental factors, namely wind and rain. It is also possible that some of the pitting and battering evident on its surface may have resulted from human hands.

In his decipherment, Puech did not distinguish between intentionally made marks and natural fissures, or between the frame and some of the letters. For instance, the initial *tau*, which in Puech's words 'n'est pas de lecture totalement assurée', is simply not there at all: The incised vertical and horizontal grooves meeting at the upper left corner of the squeeze are nothing but the upper left corner of the *tabula ansata* frame. The ΦA in the squeeze, and

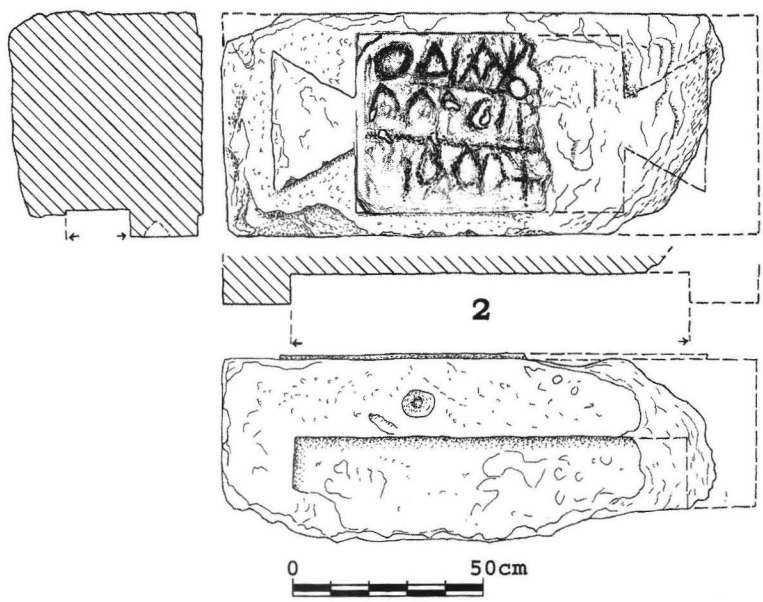
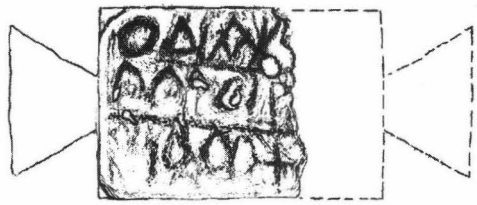
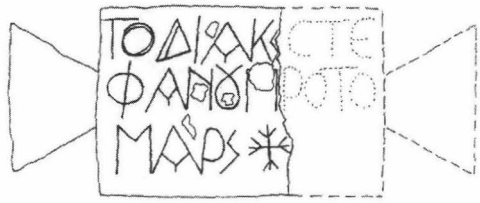


Fig. 4. A drawing of the lintel with superimposed markings from the squeeze (drawing: S. Gibson)



A



B

Fig. 5. Two drawings of the lintel based on the squeeze: (A) by S. Gibson; (B) by É. Puech (after Puech 2006: Fig. 3)

there is no trace of the following *nu*, ligature of *omicron-upsilon* and *pi* as recorded by Puech. In the third line, of which Puech writes 'cette ligne est plus faiblement préservée, mais la lecture *mu-alpha* paraît la seule possible', the first two letters in the squeeze seem to be rather *pi* and possibly *omicron*. Therefore, we suggest a minimal reading of the inscription, as follows:

ΟΔΙΑḲ - -
Μ̣ - - - - -
Π̣ - - - +

One might suggest a possible reading along the lines of: ʿΟ διακ(ὼν) [ὁ δεῖναε]πο̣[ιησε] but it would certainly be nothing but a wild guess. Nothing further can really be made out of these marks, except that they lead us to reject Puech's overall reading.

In Puech's reconstruction he crowds too many letters than is feasible, taking into consideration the overall size of the *tabula ansata* and the relative size of the few existing Greek letters. Even if one accepts his restoration of the first line and the beginning of the second, there is simply not enough space for the four letters necessary to complete line 2, especially since ΠΡWΤΟ is spelled with an *omega*, a much larger letter than the *omicron* Puech erroneously put there. Of course, misspellings are not unusual in Byzantine

inscriptions (though πρότος for πρῶτος is not among the common ones). Moreover, it is very unlikely that any Christian would have referred to St. Stephen using only the words 'Stephen Protomartyr' without adding ἅγιος, ἔνδοξος or both attributes.[2] Another problem is that the word *diakonikon* means sacristy, a space used, among other things, for the safekeeping of relics and precious objects; but the term by itself cannot in any way be used to signify a mausoleum, despite the information – partly irrelevant, anachronistic, or even erroneous – provided by Puech (2006: 111–118).

Unfortunately, it would appear that Puech's interpretation of the inscription was dictated not by his examination of the lintel, but by certain preconceived ideas (following Strus); firstly, that the circular building was a mausoleum, and, secondly, that Beit Jimal, situated in the near vicinity of Kh. el-Jiljil, must be ancient Caphar Gamala. But, while the first statement is unproven, to say the least, the second has been a matter of scholarly dispute and should now be rejected (for reasons which we shall state below).

Is the circular building a mausoleum?

The site of the circular building was surveyed in 1993 by Strus, but apart from the general outline of the structure nothing more could be seen. Strus, however, even before any excavation was commenced at the site, was of the opinion that the building had to be either a baptistery or a mausoleum (Strus 2003: 37). Full excavations at the site were subsequently conducted there in 1999 (Strus 2001: 270–271), resulting in Strus putting forward the proposal that the structure must be interpreted as a fourth-century mausoleum/religious building, that was later converted into a wine press during the course of the Byzantine Period (Figs. 6–7). The final publication of this interesting building was not completed, unfortunately, owing to the untimely death of the excavator in June 2005. The identification of this structure as a mausoleum was also adopted by Puech (2006: 119–123), suggesting that it should be identified as the 'diakonikon' of St. Stephen.[3] But what is the archaeological evidence to support such an interpretation?

According to Strus, the circular structure was originally built in the fourth century AD to serve as a 'religious monument', with a central 'memorial room' (Fig. 7: I) and with a special room to the south (Fig. 7: II) giving access to a 'water pit' (used, he says, as a 'ritual bath') sunk into the floor and that it had a walled-up 'sepulchral entrance' leading from it to a subterranean grotto (visible behind the 'water pool': Fig. 7: III). Strus went on to say that since the circular structure was evidently 'built above a tomb, it is reasonable to interpret the monument as an early Christian mausoleum dedicated to an important Christian personality' (2001: 271).[4] However, an examination of the so-called 'grotto' indicates it to be nothing more than a

Fig. 6. The circular structure during the 1999 excavation season, looking north. Father A. Strus is visible standing on the right (photo: S. Gibson)

natural cavity (0.50 m high) of geological origin, and since tool marks are not evident on its sides we must discount the existence of a 'tomb' at this location. Moreover, the finds from the excavation did not reveal, as far as we are aware, reliquaries or human bones. The so-called 'water pool' was the vat associated with the wine press, as we shall see below.

Strus' dating of the earliest phase of the circular structure is also problematic. His fourth century AD date for the founding of the structure was based on an analysis of ceramics and coins derived 'from the first stratum around the monument and from the former pit's plaster...' (2001: 271). Presumably, the reference here is to the reddish-brown fills seen extending down to bedrock in the exterior areas around the building (Fig. 8: 2 and 4), on the one hand, and more importantly to fills found sealed beneath the lowest mosaic floor in the vat (Fig. 8: 13), on the other, as well as from some of the natural rock cavities situated behind the vat walls which Strus also cleared. On stratigraphical grounds this means that the structure could not have been built *before* the fourth century, but, at the same time, one cannot ignore the possibility that the early pottery from these fills might be residuals (i.e. fourth century artifacts deposited there during building activities in the fifth century). Furthermore, some of the fourth-century type coins that were

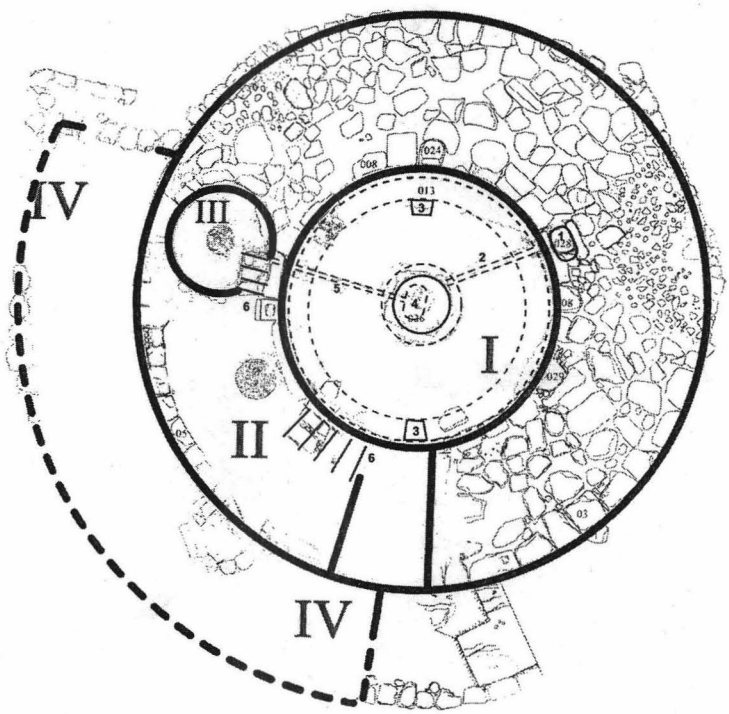


Fig. 7. A plan of the circular structure by K. Koszewski, illustrating Strus' interpretation of the building (courtesy of the late A. Strus).

found (see a reference to these coins in Arslan 2005: 107) are known to have continued circulating throughout the fifth century and into the first half of the sixth century (Bijovsky 2000: 208). Indeed, similar types of fills, containing fourth century pottery (including Beit Nattif type lamps), were uncovered beneath the earliest phase of an adjacent rectangular building complex that Strus and Gibson excavated in 2003; there too, fourth-century type coins were found (Strus and Gibson 2005: 59).

During the second phase of the monument, according to Strus, the building was converted into a large wine press, with a circular treading floor and a screw-press device, a large side-chamber, and a circular collecting vat; this phase he dated to the fifth-sixth centuries AD (Strus 2001: 271). On face value, the suggestion that a Christian mausoleum could be converted by Christians into a wine press seems implausible, to say the least. The argument made by Puech (2006: 123) that the 'mausoleum' must have been abandoned at the end of the fifth century (and subsequently converted into a wine press) in consequence of the publication of the *Decretum Gelasianum* (ca. AD 494) that classified the legends of *inventiones* among the apocrypha, is unacceptable. Had this decree had any practical effect, it would have been a demotion of the cult of St. Stephen's relics; but these, as we know, continued to be venerated, translated from one place to another, new churches were built in honour of the Protomartyr and new *encomia* written for his festivals (Wilson 1888; Bovon 2003; and the bibliography in Strus and Gibson 2005, note 11). There would have been no reason to abandon his burial place, if it was locally venerated, just because the story of its discovery had been declared apocryphal in Rome.

One wonders, therefore, what the basis was for Strus' dating of the second phase of the building. After all, ceramics finds were derived from only five specific locations in the excavations: (1) fills surrounding the exterior wall of the building down to bedrock; (2) fills sealed beneath the lowest mosaic floor of the vat and in the natural rocky cavities behind its walls; (3) fills sealed between the lower and upper mosaic floors in the vat; (4) fills from the final phase of the building, on the floors of the central circular chamber, in the side chamber, and in the vat; and (5) fills overlying the ruined building. Based on information provided by A. de Vincenz (pers. comm.), who prepared a report (unpublished) on the ceramics from the site, the fills from the final phase of the building (i.e. from location 4) were undoubtedly of Umayyad date, whereas the pottery from the other contexts (excluding location 5) was from the Byzantine Period (fifth or sixth centuries). The pottery found on the floors of the structure can be used to date the final use or alternatively the abandonment of the structure, since we are unable to ascertain whether or not the Umayyad pottery from the excavation included *in situ* vessels or not.

A new examination of the building shows quite clearly that it was designed from the outset as a wine press, and that both phases of the building had an identical function (Figs. 8–10). The original building works associated with the construction of the first phase of the building included the levelling of irregular rocky areas of the ground surface with a spread of small stones (Fig. 8: 24) and the hewing of rock footings (0.20 m high, visible between 11 and 16 in Fig. 8) as foundations for walls; the hewing of a large circular basin in the centre of the circular chamber as a socket for the stone base for the mortise of an upright screw press device (Fig. 8: 17); and the hewing of

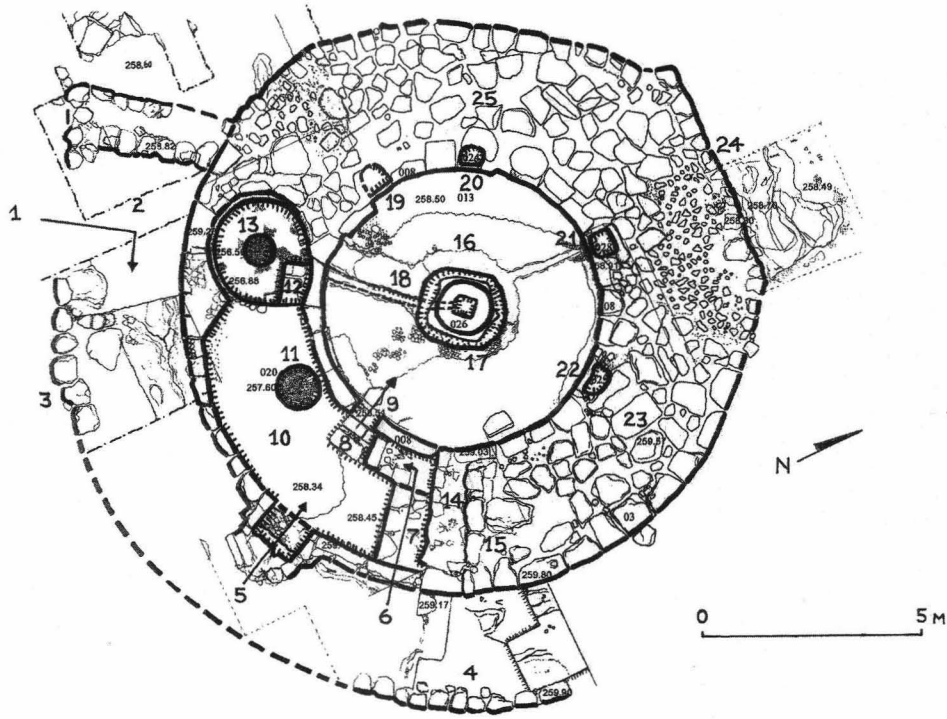


Fig. 8. A plan of the circular structure based on an original drawing by K. Koszewski, which was checked and corrected in the field by S. Gibson

a circular receiving vat (Fig. 8: 13). The existence of these features is conclusive proof that this structure was intentionally designed as a wine press and not for any other purpose. Usually industrial-size wine presses, such as this one, are square or rectangular in plan, but one large circular wine press was uncovered by Hirschfeld adjacent to a Byzantine rural estate at Ramat Hanadiv (Hirschfeld 2003: 70–73).[5]

Access to the structure was from the south, from a large fenced court, fronting a large rectangular building complex situated further to the south (Strus and Gibson 2005: Fig. 3: 5). Clearly the circular structure and the building complex were inter-related and contemporary; it is not surprising therefore that the stratigraphical phasing of these two buildings is similar. A fenced path (2.34–2.66 m wide) surrounded the southeast quadrant of the circular building (Fig. 8: 1–2); the fence wall was built of one (or perhaps two) courses of fieldstones facing outwards (Fig. 8: 3–4).[6] The surface of the path had eroded and was not visible at the time of the excavation.

The circular structure must have been impressive seen from a distance, with a total diameter of 13 m. The height of the structure is not known but the

Fig. 9. The circular structure in 2007, looking west (photo: S. Gibson)

thickness of the walls (3.5 m) would definitely have allowed a second storey. The wall of the structure was better preserved on the south side (0.74 m above bedrock), with the wall substantially eroded to the north. The entrance to the circular structure (0.60 m wide?) was via a flight of steps (0.87–1.15 m wide) descending into a side chamber (Figs. 7: 6–7; 10); only the lower step (0.77 m wide, with a tread 0.17 m) has been preserved. Consolidation walls were observed on the north side of these steps (Fig. 8: 14–15). A small square paved area (0.80 × 0.80 m) situated at the foot of the steps (Figs. 8: 8; 10) and in front of the door (0.52 m wide) leading from the side chamber into the main circular chamber (Fig. 8: 9), ensured that anyone descending the steps from outside would not tread on the mosaic floor of the side chamber (Fig. 8: 10), which presumably was used to hold the grapes that were deposited there through a chute/window (1.02 m wide) situated in the external wall (Fig. 8: 5). The chute/window was blocked up (0.61 m thick) during the second phase of the building and a coat of plaster covered the interior side of this entry.[7] The side chamber (5.30 × 2.40 m) had a levelled rock floor and was originally covered with a mosaic pavement and with a coating of plaster on its walls (Fig. 8: 10). On one side of this chamber there was a circular sedimentary basin or vat (0.93 m diameter, 0.72 m deep) with plaster-coated walls and with a floor paved with white tesserae (4 × 4 cm) (Fig. 8: 11). Direct access existed between the side chamber and

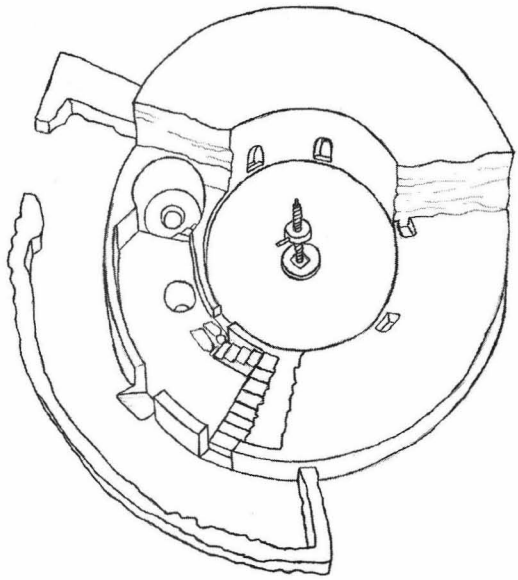


Fig. 10. An axiometric drawing of the circular structure (drawing: S. Gibson)

Fig. 11. Room 10 in the circular structure, looking north-east (see Fig. 7). Visible is the square paved area (8) leading to the first step (6) of a flight of steps (not preserved) behind. On the left is a doorway (9) leading to the central chamber (photo: S. Gibson)

the large circular vat, via a flight of plastered steps: 0.94 m in length and 0.57 m in width (Fig. 8: 12).

The vat (2.37 m in diameter) has two courses of flat dressed ashlars (averaging 0.30–0.69 m in length) surrounding the upper part of its interior wall, with an external coat of plaster (Fig. 8: 13).[8] The lower part has superimposed mosaic floors belonging to the two phases of the building: the lower with a white mosaic made with plain white tesserae (2.5 × 2 cm), the upper with large industrial-type white tesserae (5 × 4 cm) (Fig. 12). The depth of the upper mosaic in the vat from the floor in the side chamber (No. 10) is 1.35 m. The lower mosaic floor had a sump at its centre (0.60 m diameter). The walls of the vat were plastered. The floor of the circular vat is at a depth of 1.30 m below the level of the lower floor of the central circular chamber.

Access to the central chamber was solely through the side chamber on the southeast side of the building. A narrow doorway (0.52 m wide) linked the two chambers (Fig. 8: 9); the interior wall of the central chamber was built of flat-dressed ashlars (averaging 0.45–1.82 m in length and 0.45–0.66 m in height),

Fig. 12. The vat of the circular structure showing the two superimposed mosaic floors, in a photograph taken during the excavations in 2003 (see Fig. 7: 13). (photo: S. Gibson)

Fig. 13. A compartment in the wall of the circular structure (see Fig. 7: 20) (photo: S. Gibson)

two courses of which are preserved, coated with plaster.[9] The inner diameter of the central chamber is 6.20 m, with a distance of 3.13 m from the edge of the interior wall to the socket of the stone base in the centre of the chamber. Four compartments were cut into the lower interior wall (Figs. 8: 19–22; 12), of which three were plastered.[10] In the centre of the chamber is a rock-cut basin (2.08 m in diameter, 0.59–0.69 m deep) which was used as a socket to contain the stone base for the mortise of an upright screw press device (Fig. 8: 17). The stone base was irregular in shape, but its upper part was circular (1.30 m in diameter), and its top surface would originally have been more or less flush with the surface of the mosaic pavement of the chamber (Fig. 14). The wooden mortise would have been anchored in place inside the square socket (0.40 × 0.41 m, 0.54 m deep) in the centre of this stone base, the lower part of which had been widened (4.5 cm) towards the north (Fig. 10). Suitable downward pressure would have been applied with a wooden nut that was rotated with the help of a handle inserted into holes located in its side. The grape pulp (pomace) was piled on top of this stone base and kept in place with a wooden frame or a coiled rope, with the pressed juice running across the surface of the mosaic floor and through a small aperture to drain into the circular vat. During the second phase of the building the mosaic floor was raised by 0.30 m (white tesserae: 4 × 4 cm) and the technology of the upright screw press was changed. The stone was raised within

Fig. 14. The stone base in the centre of the circular structure, looking north (see Fig. 7: 17). (photo: S. Gibson)

the basin and propped up on a number of stones, and the interior of the socket was paved with large white tesserae (6.4 × 5.5 cm). Clearly, the base of the upright screw did not plug the mortise, and pressure placed on the baskets containing the grape pomace forced the juice to accumulate within the mortise socket below. A channel carved into the south-west side of the mortise socket (0.11 m wide and 0.18 m deep) led to a rock-cut channel (0.15 m wide and 0.18 m deep) beneath the mosaic floor of the chamber, bringing the juice into the circular vat (see Frankel 1994: 75–77).

The production process within this wine press may be reconstructed fairly well based on the archaeological evidence available to us. The grapes were brought into the structure not through the main door, but down a chute/window situated in the external wall leading to a side chamber. This side chamber was a holding area or a working area for the first treading or crushing of grapes, in addition to the work subsequently undertaken within the central circular chamber. The grape juice would accumulate within the circular sedimentary basin or vat situated on one side of the room. The fact that one side of this chamber was open to the main circular vat must indicate that it served for the treading or crushing of grapes, as otherwise it would have been blocked up. The central circular chamber seems to have been used specifically for the pressing of the grape pulp resulting from the first

treading in the side chamber (see Dray 2003: 221). Small compartments (usually semi-circular and arched) have been found in the walls of many large-scale wine presses of the Byzantine Period and their function has been debated (Sidi, Amit and 'Ad 2003: 261–262). The fact that the ones in the Jiljil building are so small precludes their having being used to store grapes. Hence it seems likely that they were used to store the *huwwar* (marl) which was used during production for the refining of the must derived from grapes (Frankel and Ayalon 1988: 20). According to Gal's calculations (1985–86: 137) it is possible to process between 300–400 kg of grapes at any one time on a floor area of 12 square metres. Hence, we may estimate that the side chamber (with a floor area of 12 square metres) was used to process this amount, and that the central circular chamber (with a floor area of 24 square metres), if indeed it was used for treading grapes, could have been used to process double that amount.

To sum up, we may conclude that this winery was not built *before* the fourth century, and that its construction most likely occurred in the fifth century AD. It was built as a wine press for large-scale wine production, presumably as an installation belonging to the *villa rustica* building complex at Khirbet el-Jiljil. The building was in use during the course of the Byzantine Period (fifth-sixth centuries) before being abandoned or destroyed. Based on stratigraphic evidence derived from the excavation of the adjacent building complex, this probably took place in the late sixth or early seventh century (Strus and Gibson 2005: 72). The building was eventually reconstructed in the Umayyad Period (probably in the mid-seventh century), with the re-building of walls and with the raising of mosaic floors within the central chamber and in the vat. The reason for the final abandonment of the structure is unknown, but based on the evidence from the adjacent building complex, this probably occurred towards the end of the Umayyad Period and before the quarrying of stones from the structure took place in the Abbasid Period (Strus and Gibson 2005: 76).

On the identification of Caphar Gamala

The reason why the late Father Andrzej Strus chose to excavate the circular building at Khirbet el-Jiljil in the first place, is stated in the last sentence of a booklet he wrote about Beit Jimal (Bet Gemal), as follows: 'If the identification of Bet Gemal with the villa of Gamliel is correct, then Kh. el-Jiljil must be the most probable place in which to search for the tombs of the saints and the remains of the first sanctuary' (Strus 2000: 63). We have shown above that the circular structure at Khirbet el-Jiljil was planned, executed and used solely as a wine press, probably part of a large agricultural estate, and that there is no evidence whatsoever to suggest it might have been used at an earlier stage as a mausoleum. But what of the identification of Beit Jimal as ancient Caphar Gamala?

Fig. 15. Beit Jimal in a photograph taken in the early twentieth century (courtesy of the archives of the École Biblique et Archéologique Francaise, Jerusalem)

In the Epistle of Lucian, the document in which the story of the *inventio* of St. Stephen's relics is given, the narrator, Presbyter Lucian, describes himself as the priest of the village of Caphar Gamala, in the territory of Jerusalem, 20 Roman miles from the Holy City (*Epistula Luciani*, *PL* 41: 807–809).[11] The distance more or less fits the location of Beit Jimal (Figs. 15–16), and the modern toponym does indeed recall Caphar Gamala. There are even reports that this place was known locally in the early twentieth century as Kafr Jimal (Stephan 1937: 46; see further discussion in Strus and Gibson 2005: n.12). The identification of Beit Jimal as Caphar Gamala was already given credence by the explorers C.R. Conder and H.H. Kitchener during their work in the region in the 1870s (Conder and Kitchener 1883: 24), though this was not an identification accepted by all.

When the Salesian fathers discovered a Byzantine church in their property, in 1916, they promptly identified it as the memorial church built over St. Stephen's tomb, despite the fact that none of the four caves found beneath it corresponded to Lucian's description (Gisler 1917; Abel 1919; Mallon 1922; Fergnani 1934; more bibliography in *TIR*, s.v. Caphar Gamala). Moreover, none of the caves resemble types of tombs known from Second Temple or Byzantine periods. At least two of these caves appear to be typical stepped ritual bathing pools (*mikva'ot*) (Fig. 17), the third is a cave of nondescript appearance (see Abel 1919: Fig. 1: A, B and D; for the various forms of *mikva'ot* in the region, see Zissu 2001), and the

Fig. 16. Aerial view of Beit Jimal (courtesy of Richard Cleave)

fourth is apparently a cistern.[12] Indeed, the archaeological remains uncovered and recorded at Beit Jimal suggest scattered remains from the Second Temple Period across the entire summit and on the slopes, including a tomb with *kokhim* which was reused in the Late Roman Period (Strus 2003: 472). The phenomenon of Byzantine churches and monasteries erected above caves and tunnels, tombs, and ritual bathing pools (*mikva'ot*) of the Second Temple Period, is quite common in Palestine (Di Segni 2006–2007; Zissu 2001). Therefore, the coincidence of the similarity of ancient and modern names, and the supposed existence of a tomb beneath a church, is not enough to prove the identification of Caphar Gamala with Beit Jimal.

An important consideration is that Beit Jimal (map ref. 147/125) is sandwiched between villages said by Eusebius to belong to the territory of Eleutheropolis: Zanoua to the east (Kh. Zanu', 150/125) and Saraa (Ṣor'ah) to the northeast (Ṣar'ah, map ref. 148/131: Eusebius, *Onomasticon* 92, 13–14; 156, 15–17; see *TIR*). Both Zor'ah and Zanoua are located farther from Eleutheropolis and nearer to Jerusalem than Beit Jimal. Two

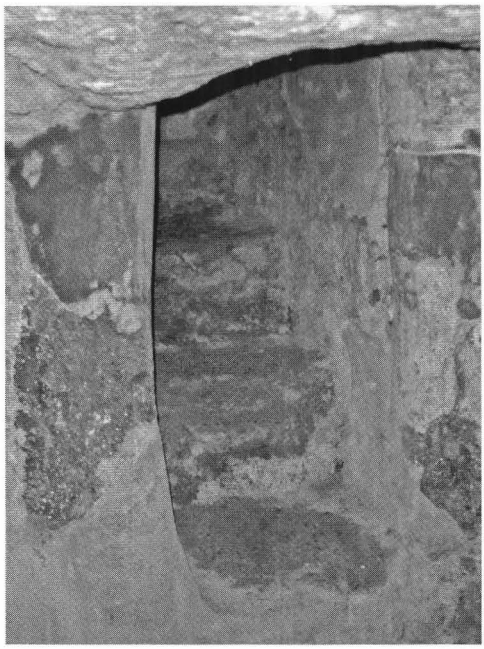


Fig. 17. Stepped entrance to one of the plastered *miqva'ot* beneath the Byzantine church at Beit Jimal (photo: S. Gibson)

other villages situated along the Eleutheropolis-Jerusalem road, Iermucha and Enadab, both nearer to Jerusalem than Beit Jimal, were located by Eusebius through their distance from Eleutheropolis, an indication that they belonged to the territory of that city (Eusebius, *Onomasticon* 95, 28–29; 107, 24–25. Iermucha is Kh. Yarmuk, 147/124, and Enadab is Beit 'Itab, 155/126, see *TIR*). Three other villages, Beth Shemesh, Iarimuth and Eshthaol, all northeast of Beit Jimal and nearer Jerusalem, are also located by their distance from Eleutheropolis (Eusebius, *Onomasticon* 54, 11–13; 88, 12–14; 106, 9–10; see *TIR*). The territory of Jerusalem began east of Eshthaol: the village of Chasalon (Kasla, 154/132) was located 'in the territory of Jerusalem' (Eusebius, *Onomasticon* 172, 16–17, and see *TIR*).

This was the territorial situation in the late third century, when Eusebius wrote his *Onomasticon*, which was probably based on road maps of the Roman administration (Isaac 1996), and this was also the situation in the sixth century, as is indicated by an inscription in the mosaic pavement of the church at 'Ain Fattir, northeast of Beit Jimal, which mentions a bishop called Anastasius (Alpi 1992). The choice is between Anastasius, who occupied the throne of Jerusalem in AD 458–478, and Anastasius, bishop of Eleutheropolis from around AD 536 (Fedalto 1988: 1001, 1021), but the palaeography undoubtedly indicates a date in the first half of the sixth

century (a dating Puech himself accepts: 2006: 107, n.15), and the use of the term ἐπίσκοπος, not ἀρχιεπίσκοπος or πατριάρχης also points to the bishop of Eleutheropolis.[13] It seems futile to contend, with Puech, that 'si Beit Gimal relève de territoire d'Éleuthéropolis à un moment de l'époque byzantine, rien n'empêche qu'il ait pu relever d'abord ou un temps du territoire de Jérusalem' (Puech 2006: 124). As ecclesiastical and administrative borders coincided since at least the fifth century, and probably much earlier, as attested in canons 12 and 17 of the Council of Chalcedon (Mansi 1960: 364–365), a shift of urban boundaries would have involved not only a change in the ecclesiastical authority to which the villages of the area were subject, but alterations and adjustments in the fiscal and administrative system. That this may have been done, for no conceivable reason, between the late third century and AD 415, and then back again between AD 415 and the early sixth century, is outside the realm of credibility.

Abel (1924) and Vincent (1926) rejected the identification of Beit Jimal with Caphar Gamala, and proposed an alternative location, at Jemmala northwest of Jerusalem (map ref. 159/153, and see *TIR*, s.v.; Dauphin 1998, III: 829–830, Map 7–8, no. 314).[14] Its distance from Jerusalem (25 km) fits the 20 miles indicated by the *Epistula Luciani*; its modern name recalls the ancient toponym; moreover, near Jemmala there is a ruin called Kh. Selemya (map ref. 169/148), whose name recalls Caphar Selemia, mentioned in the Epistle as another property of Gamaliel's in the vicinity (*PL* 41: 812; cf. Abel 1938: 293–294; *TIR*, s.v. Caphar Salama; for another possible candidate, Kh. Selma, 167/140, see Dauphin 1998, III: 887, Map 11–12, no. 5). In Rosenfeld's opinion Jemmala is located too far west to have been included in the Jerusalem toparchy (Rosenfeld 1997: 204, n.22) and so Strus (followed by Puech) suggested it must have belonged to the Thamna toparchy. However, nothing is known about the extension of the toparchy of Thamna. Moreover, this argument is anachronistic, for after the urbanization of the province, which was completed long before the *Epistula Luciani* was written, there were no longer toparchies administered under village headquarters. Thamna itself in the late third century belonged to the territory of Lydda (Eusebius, *Onomasticon* 96, 24–26; *TIR*, Thamna I), but Jemmala, situated more than 4 km due south, and separated from Thamna (Kh. Tibna) by chains of hills and by Nahal Shami, may well have been included in the territory of Jerusalem, from which Jemmala is less than 25 km distant to the north-northwest. A site at the identical distance towards the north-northeast, Kh. Samiyye (map ref. 181/154; *TIR*) was in the Hellenistic period included in the toparchy of Apharaema and in the Roman period in that of Gophna; in the sixth century it belonged to the territory of Jerusalem, as is shown by a Greek inscription mentioning Patriarch Eustochius (dated AD 557: Macalister 1907; Abel 1907; Di Segni 1997: 582–583, no. 200).

On the basis of the information available to us at the present time in regard to the delineation of the Jerusalem territory, the Jemmala identification

appears to be a much stronger candidate for Caphar Gamala, but absolute certainty in this regard remains elusive. For reasons given above the identification of Caphar Gamala at Beit Jimal thus appears to be invalid.

Additional inscriptions

In his article, Puech seems to be in two minds: on the one hand, he is anxious to demonstrate that Beit Jimal represents ancient Caphar Gamala; on the other, he has doubts about the sepulchral interpretation given to the caves situated beneath the Byzantine church, and now under the present-day church (2006: 103, n.6). These doubts do not deter him, however, from restoring the mosaic inscription, whose remains were discovered in the pavement of the nave of the Byzantine church, as a text which can only be justified if Puech accepts the interpretation of the church as a memorial erected at the site where the priest Lucian and the monk Megetius discovered the bodies of Stephen, Gamaliel and Abibos. On the other hand, Puech proposes a reading for the Kh. el-Jiljil inscription based solely on the identification of the circular building at the site as the mausoleum in which the three would have been buried. To effect this identification, Kh. el-Jiljil, situated 1 km away from Beit Jimal, becomes a combined location, Khirbet Jiljil – Beit Jimal, and the toponym Jiljil, an Arabic form of the Aramaic Galgal ('circle, ring'), becomes a translation of the Greek name of the martyr, Στέφανος, 'crown' (Puech 2006: 121).

Perhaps being unable to make up his mind, Puech proceeded in his article to restore and/or to interpret every single one of the inscriptions known from the two sites, Kh. el-Jiljil and Beit Jimal, as directly relating to the burial of the Protomartyr. However, since the identification of Beit Jimal as Caphar Camala is no longer valid, what else may be said about Puech's restorations/interpretations of the inscriptions?

The first inscription examined by Puech (2006: 101–102) is the one engraved on a capital with crosses which was discovered in the nineteenth century at Kh. el-Jiljil, and is now lost. Séjourné (1892: 262–263) and Germer-Durand (1892) read it: Εἷς θεὸς ὁ βοηθῶν τὸν δεσπότ(α) Ἀντωχιανοῦ, 'One God who helps the master of Antochianus'; according to this interpretation, the man Antochianus would be offering a prayer to his master. Di Segni (1994: 104, no. 30; 2005: 103) has suggested to correct) Ἀντιοχιανοῦ, and to interpret it not as a personal name but as a neuter, referring to a *villa* or landed estate named after its owner, Antiochus or Antiochius. Puech rejects both the correction and the interpretation, and makes of the master of Antochianus 'sans doute... un bienfaiteur de l'édifice pour lequel était sculpté ce chapiteau', of Antochianus the marble-worker, and of the acclamation εἷς θεός 'sans aucun doute une réminiscence du *credo*'. In other words the capital, which most likely was used in a portico in the complex excavated by Strus and Gibson, together with its companion found nearby (see Strus and Gibson 2005: 33–34, Figs. 4.1 and 5; 81, Fig. 60), has been promoted

to the rank of an architectural element in a sacred building – for benefactors are commemorated only in sacred buildings in the Byzantine period – and the acclamation εἷς θεός ὁ βοηθῶν, well known in Palestine as an apotropaic formula of pagan origin, mostly used in a gnostic or Samaritan milieu (Di Segni 1994), has suddenly been promoted to almost a quotation from the Credo, as is suitable in an inscription engraved on the capital of a column in a church. However, the building complex at Khirbet el-Jiljil was undoubtedly part of a rural estate and a search for the 'chapel' referred to by Gisler (1918: 20) was made during the recent excavations at the site, but not found (Strus and Gibson 2005: 31). The only sacred building, therefore, is the so-called 'mausoleum of Stephen the Protomartyr', which, as we have shown, served only as a wine press.

The second inscription considered by Puech (2006: 102–105) is that of the mosaic pavement from the nave of the Byzantine church at Beit Jimal (Abel 1919: Fig. 1). This was set in a round medallion of which only the extreme right side has survived, with two letters at the end of five lines, out of six to eight lines of the original text (Fig. 18). Perhaps 15% of the letters, possibly even less, have survived, and since these are insignificant groups (OY for genitives, ΘΕ that can be part of θεός or of an epithet or a name beginning

Fig. 18. Greek inscription in a mosaic floor from the nave of the Byzantine church at Beit Jimal (photo: S. Gibson)

with θεο-) they cannot provide a key for an interpretation. Puech rightly rejects the implausible restoration offered by the excavator (Gisler 1923: 20; Puech 2006: 102–103, n.5), but then proceeds to offer an even wilder guess, in which the names of Lucian the priest and Megetius the monk are somehow fitted in, together with unidiomatic phrasing, and a misspelling for good measure. Puech reads the inscription as follows:

[+]
2 [KEIYBWH]ΘE
[CONΛOYKIAN]OY
4 [EYCEBECTAT]OY
[ΠPECB^{s}KAIHΓ]OYME
6 [NOYMEΓEΘIOYΘ]EO
[CEBECTATOY]
8 [+]

2 [Κ(ύρι)ε ’Ι(ησο)ῦ βωή]θε-
[σον Λουκιαν]οῦ
4 [εὐσεβεστά́τ]ου
[πρεσβ(υτέ́ρου) καὶ ἡγ]ουμέ́-
6 [νου Μεγεθίου θ]εο-
[σεβεστά́του]

‘Lord Jesus, help Lucianus most pious priest and Megetius most devote hegumen’

According to Puech, ‘cette proposition respecte de bout en bout la phraséologie habituelle’. We beg to disagree. Some examples: the usual abbreviated invocation is Κ(ύρι)ε ’Ι(ησο)ῦ Χ(ριστ)έ́: Κύριε ’Ιησοῦ or Κύριε Χ(ριστ)έ́ are both known, but usually not in abbreviated form. For the spelling βοήθεσον with *epsilon* instead of *eta* (we shall pass over the fact that both in the drawing and in the copy Puech writes βωήθεσον with an *omega*), Puech cites three examples (2006: 104, n.7), which when checked are revealed to be just one, a graffito from Wadi Haggag in Sinai (*SEG* 26: 1661) cited three times, under different headings, by Meimaris (1986). The invocation to God to help Lucian and Megetius must be understood as inscribed in the lifetime of the two, otherwise it makes no sense; but the abbreviation ΠPECB^{s} marked with a *stigma* is not found in inscriptions of the early fifth century. The sequence ἡγουμένου Μεγεθίου εὐσεβεστάτου is unacceptable, even if one takes for granted that the humble monk, after the discovery of the relics, was promoted to the abbacy. In all Byzantine inscriptions mentioning members of the Church, the order is: name, epithet and title *or* epithet, name and title; the position of the name can vary, but the epithet always precedes the title. The only way to read ll. 5–7 in Puech’s reconstruction would be to attach the title of hegumen to Lucian (‘most pious priest and

hegumen'), but the Epistle provides no hint that he was a monk, and certainly not an abbot. By the way, the reading ἡγ]ουμέ[νου, already suggested by Abel (1919: 244), is by no means as certain as Puech would have us to believe; in ll. 5–6, a masculine genitive followed by με[τὰ συμβίου Θ]εο[δώρας (or another name) or με[τὰ τέκνων Θ]εο[δώρου καὶ – would do just as well. In short, anyone with a knowledge of Greek and a creative mood might produce a perfectly acceptable, idiomatic text in accordance with the miserable remains of letters, without calling into play Lucian and Megetius. The only justification for their appearance in Puech's restored text is his preconceived idea that this was the site of the discovery of Stephen's bones (or it wasn't, if the Protomartyr was buried in the 'mausoleum' at Kh. el-Jiljil).

The third inscription discussed by Puech is the one in the mosaic pavement of the church at 'Ain Fattir, near Beit Jimal (2006: 106–107). Luckily, thanks to the good edition provided by Alpi (1992), there is not much scope here for Puech's creativity, and he is wise enough not to evoke the bizarre theory presented by Strus (1995) about the 'cryptogram' in l. 5. However, in consideration of his assumption that Beit Jimal is Caphar Gamala, and in spite of his better understanding, leading him to favour a date in the sixth century, Puech leaves open the option of identifying bishop Anastasius, mentioned in the inscription, with Anastasius of Jerusalem (AD 458–478), instead of with Anastasius of Eleutheropolis (*ca.* AD 536). The solution is then indicated later, in the passage discussed above, according to which Caphar Gamala would have swung between two bishoprics, in the course of the Late Roman and Byzantine periods.

The next inscription considered (Puech 2006: 107–109) is the one discovered within the entrance of a corridor leading to the *triclinium* of the building complex excavated by Strus and Gibson at Khirbet el-Jiljil. The inscription, published by Di Segni (2005), was actually still forthcoming when Puech's paper was printed in 2006; having seen the manuscript, through the good offices of Strus, Puech published the text and interpretation without the author's knowledge or permission. Here, as in the case of the 'Ain Fattir inscription, Puech could not contest the reading: Εἴσιθι χαίρων, 'Enter rejoicing!' – an augural formula used at the entrance of private and public buildings, not of sacred character, as was explained and illustrated with examples by Di Segni. But can such a prosaic explanation be held valid, with St. Stephen's mausoleum looming in front (or rather at the back) of the building complex, not a hundred yards away? Therefore the hall cannot be a *triclinium*, but must be a meeting place for celebrations in honour of the Protomartyr (an idea already voiced by Strus at the time of the excavations, but not accepted by Gibson), and the formula must be a quotation from the Book of Tobias, where phrases like Εἰσῆλθε χαίρων, ἐξῆλθε χαίρων and the like appear several times.

However, the Book of Tobias is not among the most frequently quoted in inscriptions. In his collection of biblical passages found in inscriptions in the entire Christian *oecumene* from the third to the eighth centuries, A.E. Felle

cannot identify more than two solitary examples (both from Macedonia: Felle 2006: 264, no. 574; 266, no. 578). Nor can any casual combination of words be taken as a quotation: a descriptive passage containing the verbs ἐλθεῖν (in any composition) and χαίρειν; is quite a different thing from an augural 'Enter rejoicing!' In fact, notwithstanding his liberal approach to the concept of a quote, which enabled him to include in his collection also 'reminiscences' of biblical phrases, Felle did not include the combination ἐλθεῖν + χαίρειν in his monumental volume. Nor can the difference between εἴσιθι χαίρων and the combinations in Tobias be covered, as Puech tries to do, by maintaining that the Greek inscription would be a quotation not from the Greek Tobias, but from the Aramaic, for Palestinian Christians were bilingual.

Conclusions

The use of *papier maché* squeezes to help elucidate the reading of inscriptions has been a boon for scholars for almost two centuries, but squeezes of heavily weathered surfaces can sometimes lead to misunderstandings and misinterpretations (see, for example, the extremely wise comments on the process of squeeze-making in Woodhead 1967: 77–83).

The analysis we have made of the squeeze of the stone lintel from Khirbet el-Jiljil indicates that the reading proposed by Puech does not stand up to subsequent scrutiny. There was undoubtedly a Greek inscription on the surface, but the surface of the stone is so weathered that an exact determination of the various Greek letters appearing there is impossible. Moreover, the interpretation of the circular building at the site as the mausoleum of St. Stephen can be shown to be groundless, and the identification of nearby Beit Jimal with Caphar Gamala, contrary to Puech's assertions, is now shown to be incorrect.

Acknowledgements

Our thanks to Father J.-M. de Tarragon, Director of the Archives of the École Biblique et Archéologique Française in Jerusalem, for permission to publish the early photograph of Beit Jimal (Fig. 15), to the Library of Congress Photographic Archives in Washington (Fig. 1), and to Richard Cleave of Rohr Productions Ltd (Fig. 16). We are also grateful to Dr. Anna de Vincenz for information regarding the ceramics from the site, to Stephen Pfann (Junior) for his photographs of the squeeze of the *tabula ansata* lintel inscription, and to Mareike Grosser for assisting us in a renewed examination of the plan of the Khirbet el-Jiljil circular structure.

Notes

1 For greater clarity we should note that the photograph of the lintel as published in Strus and Gibson (2005: Fig. 6) was shown upside down the way it was found.

2 See, for example, the inscriptions containing saints' names dealt with by Meimaris (1986: 114–139).

3 Circular mausolea are not known in Palestine. An exception perhaps is a 'memorial' tower which was built above the tomb of a Christian hermit at Khirbet Tabaliya, south of Jerusalem, but it is not necessarily Byzantine and could be of medieval date (Kogan-Zehavi 1998).

4 A fanciful reconstruction of the monument made by G. Matteoni is reproduced by Puech (2006: 120, Fig. 4). We have two objections to this reconstruction: a) the flight of steps leading directly to the central chamber, cutting the side chamber into two, is not mirrored in the preserved archaeological remains; b) there is no evidence that the inner wall of the central chamber, on the side of Strus' Chamber II, served as a stylobate for three columns as presented in this drawing.

5 See also the comments made by Strus and Gibson (2005: n.16) in regard to the probability that the Ramat Hanadiv wine press was also surrounded by a circular tower and was not a free-standing installation as reconstructed by Hirschfeld (2003: 70–73, Fig. 145).

6 The stone wall (0.65 m thick) was inserted into the brown soil fill 0.63 m above bedrock and clearly had no function other than curbing the path extending to the entrance to the building.

7 Two coats of plaster were visible at different locations within the building and they appear to correspond respectively to the Byzantine and Umayyad building phases. The earliest layer is white-grey in colour with gravel inclusions; the later layer is pinkish with gravel and grog inclusions. Plaster was evident at the following locations (see Fig. 8): on the internal walls of the side chamber (10) and over against the edges of the flight of steps (7); on the walls of the circular pit (11); on the flight of steps leading into the vat (12); within the vat (13); on the internal wall of the central chamber (16); and within the compartments (20–22). The earliest layer of plaster in the vat (13), is 4 cm thick against a rubble-and-plaster backing extending back to bedrock, and it was visibly associated with the earliest mosaic floor (with 2.5×2 cm tesserae). The latest coat of plaster in the vat, is 5 cm thick with a backing of plaster and potsherds, and it was associated with the latest mosaic floor (with 5×4 cm tesserae).

8 These ashlars were dressed with a comb-pick. One of the ashlars had a surrounding comb-picked margin (5.5 cm wide) and a flat chiselled centre.

9 These ashlars also had signs of comb-picking on their surfaces.

10 The fact that the compartments were cut into the pre-existing stones of the interior wall suggests that they did not belong to the original stage of the building, but were inserted later. Their sizes are as follows (Fig. 8): No. 19: 0.48×0.60 m; No. 20: 0.47×0.46 m; No. 21: 0.48×0.45 m; and No. 22: 0.80×0.44 m. The opening to Compartment No. 20 was apparently arched, based on the groove for a springer visible in the wall on the right, suggesting that the opening had a height of 1.15–1.20 m.

11 The epistle tells the story of the discovery of the tomb of the Protomartyr, of the High Priest Gamaliel and of his son Abibos, who had accepted the Christian faith and taken charge of burying Stephen in a tomb in the family estate at Caphar Gamala, 'Gamaliel's village'. The *inventio* was the result of visions that appeared to the priest of Caphar Gamala, Lucian, and to the monk Megetius, during the Council of Lydda in AD 415. The epistle is preserved in Greek, Latin, Armenian, Georgian and Slavonic: see Charbel (1978). Two Latin versions are in *PL* 41: 807–817; for a critical edition see Vanderlinden (1946).

12 The supposed 'tomb' beneath the church (F) matches the width of the nave and so was probably a cistern that was linked in antiquity to the stepped *mikve* (B) from

the south, perhaps even serving as an *otsar* (a water-replenishing tank) for the *mikve* (Gibson 2005: 276). See the corrected plan of the church and its underlying cavities in Strus (1988: Fig. 3).

13 In order to defend his hypothesis that Beit Jimal was Caphar Gamala, Strus insisted on the earlier date (Strus 1992; 1995; 2003), even going so far as to quote Di Segni (1992: 462) in support of this date, despite the fact that Di Segni had actually expressed an opposite opinion on the date to Father Strus.

14 The suggestion had already been put forward by the Dominican fathers J. Marta and M.-J Lagrange: (Lagrange 1894: 58–59).

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Abbreviations

GCS	Die griechischen christlichen Schriftsteller der ersten Jahrhunderte, Kirchenväter Kommission der königlichen Preussischen Akademie der Wissenschaften (Leipzig).
PL	J.P. Migne, *Patrologiae cursus completus*, Series Latina (Paris).
PPTS	Palestine Pilgrim's Texts Society.
TIR	Y. Tsafrir, L. Di Segni, and J. Green, *Tabula Imperii Romani. Judaea-Palaestina. Maps and Gazetteer* (Jerusalem, 1994).

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Reassessing the Judean Desert Caves: Libraries, Archives, *Genizas* and Hiding Places

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In December 1952, five years after the discovery of Qumran cave 1, Roland de Vaux connected its manuscript remains to the nearby site of Khirbet Qumran when he found one of the unique cylindrical jars, typical of cave 1Q, embedded in the floor of the site. The power of this suggestion was such that, from that point on, as each successive Judean Desert cave containing first-century scrolls was discovered, they, too, were assumed to have originated from the site of Qumran. Even the scrolls discovered at Masada were thought to have arrived there by the hands of Essene refugees. Other researchers have since proposed that certain teachings within the scrolls of Qumran's caves provide evidence for a sect that does not match that of the Essenes described by first-century writers such as Josephus, Philo and Pliny. These researchers prefer to call this group 'the Qumran Community', 'the Covenanters', 'the *Yahad*' or simply 'sectarians'. The problem is that no single title sufficiently covers the doctrines presented in the scrolls, primarily since there is a clear diversity in doctrine among these scrolls.[1]

In this article, I would like to present a challenge to this monolithic approach to the understanding of the caves and their scroll collections. This reassessment will be based on a close examination of the material culture of the caves (including ceramics and fabrics) and the palaeographic dating of the scroll collections in individual caves. While the results of this examination are preliminary, it is hoped that such an exercise will open the study of the Dead Sea scrolls on a new level, by allowing each cave to tell its own, nuanced story, rather than imposing upon it *a priori* an 'Essene hypothesis' or any other all-encompassing theory.

Of the numerous manuscript collections that have been found in the Judean Desert, not one has been found in its original library or archive room, with the exception of caves 4Q and 5Q, which may have served as *genizas* for the community (see below). The contents of the libraries at Kh. Qumran were evacuated, perhaps on sundry occasions, as refugees fled with the manuscripts and hid them in caves for safekeeping. Due to the quality

of the scrolls left behind and the manner in which they were deposited, it is safe to assume that the original intention was to leave them hidden until a safer moment presented itself for the owners to return and retrieve the precious manuscripts. In all of the cases where scrolls have been discovered, we can likewise assume that the original owners did not consider it safe or did not survive to return for them, likely due to the calamities and harsh reality of their own times.

This begs the question of just how in-use libraries would have been kept in the Judean Desert or elsewhere in the first century. To answer this question it would be helpful to survey the available information on other sundry but parallel collections of manuscripts that existed in the contemporary Roman world. This will be followed by a survey and comparison of the manuscript collections presently available from the Second Temple Period, especially from the area of the Judean Wilderness.

I. The libraries, archives, *genizas* and hiding places of the Judean Wilderness in the context of the Roman world

At the outset, a distinction should be made between manuscripts found in caves and manuscripts kept in buildings. The scrolls found in the caves in the cliffs do not represent functional, working libraries. Rather, they held the contents of various libraries or archives that had been hidden, most likely to protect them from the threat of theft or destruction. In antiquity, as today, books and scrolls within functional or 'in-use' libraries were generally stored on shelves in special rooms within a building, as the following survey indicates.

A. Libraries

Public libraries

The most famous were the Library of Alexandria at the Museon and its 'daughter library' at the Serapion, Hadrian's Library (Athens), the Celsus Library (Ephesus), the library of Attalus I (Pergamon), and Augustus' library on the Palatine Hill (Rome; which was enlarged by Tiberius and Caligula). Among its numerous holdings, Vespasian's Library of Peace in Rome, established in AD 76, contained many volumes taken as booty from Jerusalem's main library, including Hebrew Torah scrolls.

Institutional libraries

These include Galen's medical library at Pergamon's Asclepion and the hieratic library at Delphi.

Personal libraries

These represent personal holdings, which range from a few scrolls to collections, in certain cases, of enormous size. The library of L. Calpurnius

Piso (Julius Caesar's father-in-law) at Herculaneum contained at least 1800 volumes. Certain personal libraries later became institutional (e.g., Galen's Library) or public. The greatest library of Rome, built by Trajan in AD 114, was based upon the personal library of a certain Epaphroditus of Cherlones. Although no functional libraries were found *in situ* in the Judean Wilderness, the partial contents of libraries were found in caves 1Q, 2Q, 3Q, 6Q, 11Q and Masada. Since their contents represent the collections of specific sects or interest groups, these apparently contained the remnants of institutional libraries. Caves 4Q and 5Q apparently held the worn remains of a much larger institutional library (see below).

B. Archives

Public archives

Examples include the Temple archives in Jerusalem, which were stored separately from the main library and were burned by revolutionaries, likely Sicarii; the Elephantine papyri, and the recently discovered Idumaean ostraca archive (limited in general to receipts and lists of produce).

Institutional archives

Eighteen archival documents seem to have been mixed among the remains of an institutional library found in Qumran cave 4 (4Q342–4Q359), at the time they were discovered by Bedouin. However, certain of those have been proven to derive from the personal archives of the Bar Kokhba period found in Nahal Hever (especially 4Q347 and 4Q359), and not from cave 4Q at all. This has led some to conclude that most, if not all of the papyrus archival documents presumed to have come from cave 4Q actually came from sites elsewhere in the Judean Wilderness. If any of this group of archival documents (such as 4Q350 and 4Q355) should prove to have actually derived from cave 4Q, it might be an accidental addition. By no means could these few fragments definitively represent the actual remains of the institutional archives of any of Qumran's inhabitants. Moreover, not a single fragment of these documents was found among the 72 manuscripts recovered by de Vaux and his team when they excavated cave 4Q.

Other manuscripts from the caves and site of Qumran have the appearance of being institutional documents but seem to be reproductions of the original documents. These include 4QMMT (Letter), 4Q477 Rebukes of the Overseer, 4Q340 Lists of Netinim and 3Q15 the Copper Scroll (a list of hidden Temple treasures).

Personal archives

Examples of personal archive collections include the Babatha archive (Cave of Letters), the En Gedi archive (Cave of Letters), the Bar Kokhba correspondence (Cave of Letters, Wadi Murabba'at), and, in Egypt, the Hermopolis papyri and the Arsham Correspondence from the Persian Period.

C. Scroll and book storage

Public and institutional libraries normally stored the scrolls in tall wall niches, as at Celsus' library in Ephesus, at Nessana in the Negev, also at Masada, and apparently at Qumran's locus 2 (see Figs. 1–3).[2] Scrolls would be labelled by either a tag fixed to the exposed end or by the title written on the outer sheet of the scroll toward one end. Personal libraries were also often kept in wall niches, as in the Library of Lucullus (after 66 BC) in Rome, but also in more diverse ways such as in wooden boxes at Herculaneum. Personal archives were known to have been kept in jars whose lids were sealed and tied, such as at Deir el-Medineh in Egypt (see Pfann 2002).

D. Protective safes and hiding places

At Nag Hammadi in Egypt the Gnostic papyrus codices were hidden in jars, as were the papyrus codices of the Chester Beatty and Bodmer libraries

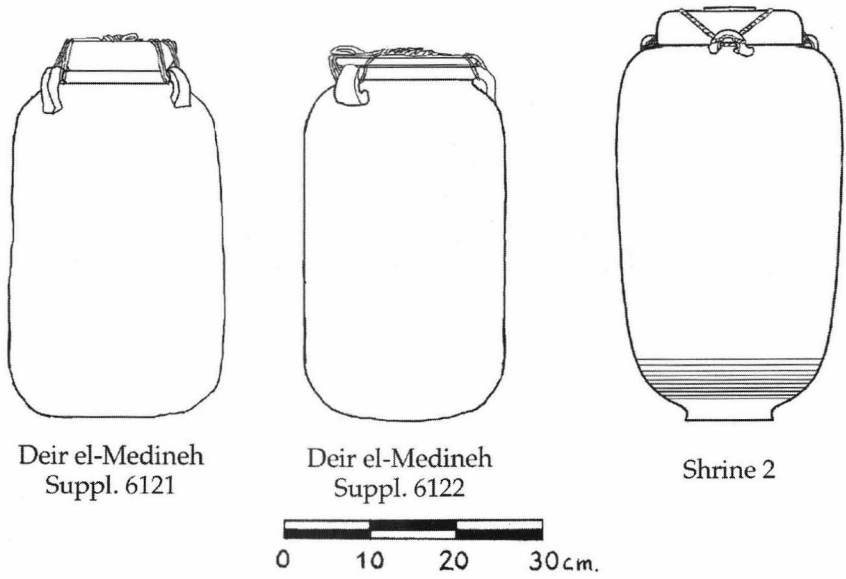
Fig. 1. Library at Nessana (note the grooves to hold shelves) (photo: S. Pfann)

Fig. 2. Masada library niches (note double door sockets in each niche)

Fig. 3. Masada library niches according to Y. Hirschfeld (illustration: D. Porotsky)

Fig. 4. Shelved area showing niches in Qumran's locus 2 (photo: S. Pfann)

– in all cases, almost certainly to conceal them from invaders. Jars similar to those from Deir el-Medineh were used to hide scrolls from an active library in Qumran cave 1. At the site of Kh. Qumran, cylindrical jars, possibly serving as safes for sundry valued items, were embedded in the floor at various locations in the site, including the room identified as the library. Such safes could have easily been used to hide precious manuscripts or documents. However, all such jars were found empty.



The Deir el-Medineh Jars with tied lids as found.
The Jar and lid "Shrine 2" from Cave 1Q with its tie restored. **S. Pfann**

Fig. 5. Archive storage jars (see Pfann 2002)

The personal archives and Bar Kokhba letters found in the Cave of the Letters were left by the refugees in a leather pouch and in a wine skin.

E. Which library types can be identified among the Qumran caves?

Closest in breadth of contents to national or public libraries are caves 4a and 4b at Qumran (though it may be that certain censorial limits were observed, since Pharisaic and other sections of Second Temple Period literature were apparently excluded; e.g., Ben Sira, 1 and 2 Maccabees, etc.). However, the fragmentary, worn, and even repaired state of the manuscripts seems to indicate that these caves together served as a *geniza* for retired manuscripts from a variety of sources.

An institutional library could have included (1) collections of authoritative, external sources – e.g., the Bible, certain books of the Pseudepigrapha; (2) collections of internal documents of the group authored by members or predecessors of the group; and (3) miscellaneous external texts and resources originating from non-members or other organizations, limited to benign, amicable or agreeable content and doctrines. This is likely the case for caves 1Q, 2Q, 3Q, 6Q, 11Q and Masada. For each, the heart of the library is the Torah (Books of Moses) and the rest of the library suits

the concerns and the needs of a specific interest group (concerning which see below: 'On determining the nature of a library by its contents and duration').

Composite libraries and genizas (evidenced by diversity of script, language and doctrine derived from a number of divergent sources) could also be evidenced. Based simply upon script, the Cryptic A corpus of manuscripts in the broader library in cave 4Q helps to distinguish at least one part of the library from the others. The palaeo-Hebrew manuscripts in this cave may present another special segment in which manuscripts are included selectively instead of representing the adoption of a cohesive library that was once separate. The other caves from Qumran may contain one, or at the most two, manuscripts in palaeo-Hebrew script, but this again makes a case for selective inclusion (particularly of the Book of Leviticus; 1QpaleoLev, 2QpaleoLev, 6QpaleoLev, 11QpaleoLev). *Genizas* are typically composite, often mixing manuscripts from various sources, including both libraries and archives. Compare, for example, caves 4Q, 5Q, and potentially, Masada, where there is a surprising mixture of various texts and documents (containing even documents of both lay and priestly character together; see below).

II. The Libraries, Archives, *Genizas* and Hiding Places of the Judean Wilderness within the Context of the Late Second Temple Period

It seems certain that the vast majority of the recovered manuscripts from the Judean Wilderness are united by the fact that they were originally hidden under difficult circumstances. However, the contents of these collections and associated materials indicate that the various collections were not all homogeneous, were not from the same source and not all from the same period.

Studies of the Judean Desert caves and scrolls in the last decade have focused on various attempts to discern the origins of the collections and to reassess, to the point of dismissal, the connection between the scroll caves and the site of Kh. Qumran. Most recently, Yizhar Hirschfeld argued strongly against such a connection, and indeed, against the assumption that Kh. Qumran was ever home to a religiously oriented group such as the Essenes (Hirschfeld 2004).[3]

Hirschfeld often cited the theories of other researchers who failed to deal adequately with the stratigraphic challenges of the site and drew eclectically from the scrolls and the historical sources to support weakly developed hypotheses.

In this section of the article, I would like to address the difficulties in the theories of those scholars, which is a necessary prelude to reassessing the caves, the scrolls, and their owners. I will then present a fresh synthesis of the material, a synthesis which I feel incorporates as much physical and archaeological data as possible.

Should the scrolls be disconnected from the site?

Following N. Golb (1995), Hirschfeld states, 'Since not a single scroll was discovered at the site itself, but only in the nearby caves, it can be assumed that the scrolls originated in Jerusalem' (Hirschfeld 2004: 230) and, 'By suggesting that Jerusalem is the source of the scrolls, we liberate Qumran from the burden of religious significance that has clung to it. It allows us to give the site a secular interpretation, not as a monastery but as a complex of utilitarian buildings constructed for some commercial, military, or administrative purpose' (Hirschfeld 2004: 5).

First of all, it is implausible that the inhabitants of any site would leave a sacred scroll, or even small fragments of such a scroll, lying around on the floor, only later to be discovered by archaeologists. Worn sacred manuscripts were customarily interred in a repository for sacred objects, known as a *geniza*. This was likely the function of caves 4Q and 5Q, which contained the oldest and most fragmentary of the scrolls. In contrast to scrolls that became worn through daily use and were sequestered in a *geniza*, are those scrolls which, during a time of threat or potential destruction, were carried away from a community's library shelves, wrapped in linen or sealed in jars, and hidden in safe places for protection and in order to avoid profanation. This was likely the case with the scrolls hidden in caves 1Q, 2Q, 3Q, 6Q and 11Q. It is most likely that the owners hoped that one day they would return to retrieve them.

Secondly, it is simply not true that no scrolls were found at the site. It is well known that the complex of scroll caves 4Q, 5Q, and 10Q lies only 80 m from the buildings of Qumran. Even more importantly, the 7Q, 8Q, and 9Q scroll cave complex, located at the end of the esplanade extending south from the buildings, lies within the protective wall of the site itself. It would not have been possible to enter those caves without first entering the enclosure walls of Kh. Qumran. Thus this would seem to be an unlikely place for outsiders to hide sacred scrolls. Therefore, one can safely suggest that scrolls found in the caves at the end of the Kh. Qumran esplanade and the peninsula of caves 4Q, 5Q, and 10Q were placed there by individuals who inhabited the building complex of Kh. Qumran during one of its phases. The question that remains is how to identify these individuals.

Could the scrolls have been brought from Jerusalem on the eve of its destruction?

To state that the scrolls came solely from Jerusalem (Golb 1995) or partially from Jerusalem and partially from Jericho (Cansdale 1997) is to assert that none of the scrolls from the caves were produced at Qumran. To suggest that all of the scrolls, especially those that issued from the adjacent caves, would have been rescued from the central libraries of Jerusalem, seems to

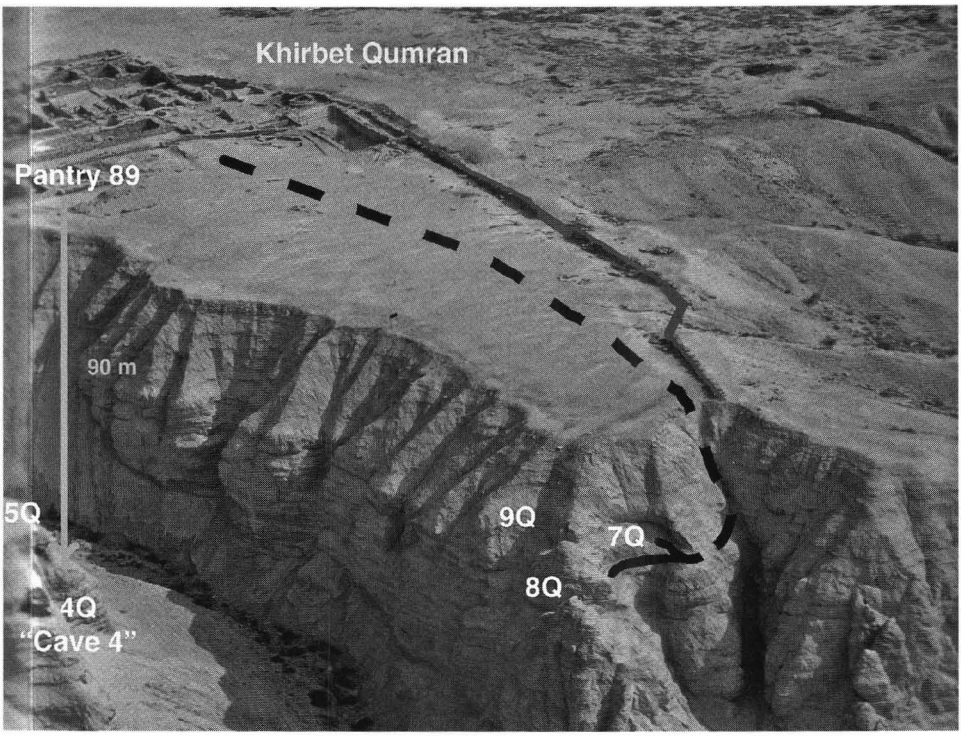


Fig. 6. Scroll caves at the site of Qumran; caves 7Q, 8Q and 9Q are within the enclosure wall (photo: R. Cleave)

be implausible (and potentially scandalous within the context of Jerusalem), particularly since no scrolls which could clearly be defined as being of the Sanhedrin, especially the Pharisees, were found among the caves.[4] In fact, at least 20% of the scrolls found in the *Yahad*/Essene caves of Qumran were produced by a group who derided the Sanhedrin, Pharisees, Sadducees and all other non-members of their sect as 'Sons of Darkness'.

Could the scrolls of all of the Qumran caves have been produced and collected solely by the Essenes?

On the one hand, most scholars who hold to the Essene hypothesis would agree that not all of the scrolls from the caves were Essene compositions and that at least some of the scrolls that are found in the caves were not originally copied at Qumran. On the other hand, these same scholars would still support the idea that the vast majority of the scrolls were penned at the site (keeping in mind that fragments of up to nine inkwells have been identified from the site; Humbert and Gunneweg 2003: 32) and they would still assert

that the entire collection of literary scrolls from Qumran (and even Masada!) was once collected and owned by the Essenes themselves.

In the end, the global statements made on both sides of the divide have only created an impassable rift between them. Thus it appears that a certain myopia or naivety has developed on both sides. There are those who consider all scrolls to be connected with a single group who inhabited Qumran (the consensus view) and those who believe no scrolls were connected with the site. Although one side might confidently snub the idea that the sum total of all of the scrolls came from the libraries of Jerusalem, and the other dismiss the potential Qumranian origins, it would be prudent not to be so quick to dismiss a suggestion that at least some of the scrolls were derived from each source. With these cautions in mind, it appears useful to evaluate the material remains and literary contents of each cave on its own, assuming the possibility that each individual cave might represent a single coherent library.

On determining the nature of a library by its contents and duration

Ostensibly, an initial separation of the caves into two main groups can be proposed: firstly, those which contain manuscripts providing typical *Yahad* doctrine (caves 1Q, 4Q, 5Q, 6Q), and, secondly, those caves or sites which do not contain scrolls with *Yahad* doctrine (caves 2Q, 3Q, 11Q and Masada).[5]

As it turns out, although the group of twelve scroll caves (i.e., caves 1Q-11Q, remembering that 4Q is actually two separate caves, 4a and 4b) are united by the presence of epigraphic finds, each has a distinct profile, sharing only some characteristics with one cave or another. These characteristics include aspects such as palaeographic date, genre, and content of the scrolls. A quick glance at the palaeographic dates of caves 1Q, 4Q, 5Q and 6Q reveals a muddled picture (Fig. 7).[6] However, by examining the chart more closely, two distinct patterns emerge, one shared by caves 4Q and 5Q (Fig. 8), and one shared by caves 1Q and 6Q (Fig. 9).

Through assessing such criteria more closely, profiles of distinct libraries begin to emerge. Setting aside for the moment the caves in the marl terrace, especially 4Q and 5Q, as potential *genizas* (see above), let us examine caves 1Q and 6Q, located in the central cliffs.

The Yahad priestly and lay characters of caves 1Q and 6Q

Caves 1Q and 6Q are similar in that they contain certain scrolls that are typically *Yahad* in doctrine (e.g., in cave 1Q: the Rule of the Community, the Thanksgiving Scroll, and typically *Yahad* oriented commentaries; in cave 6Q: the Damascus Document; Fig. 12A) and are written exclusively in Hebrew and Aramaic. However, the scrolls of cave 1Q are written exclusively on parchment while those of cave 6Q are written for the most

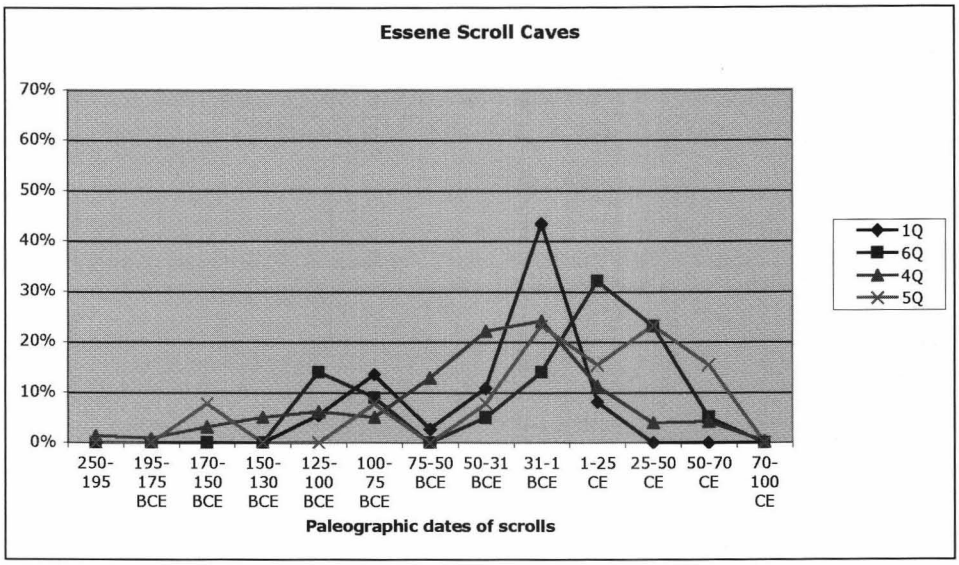


Fig. 7. Palaeographic dates of caves 1Q, 4Q, 5Q, and 6Q

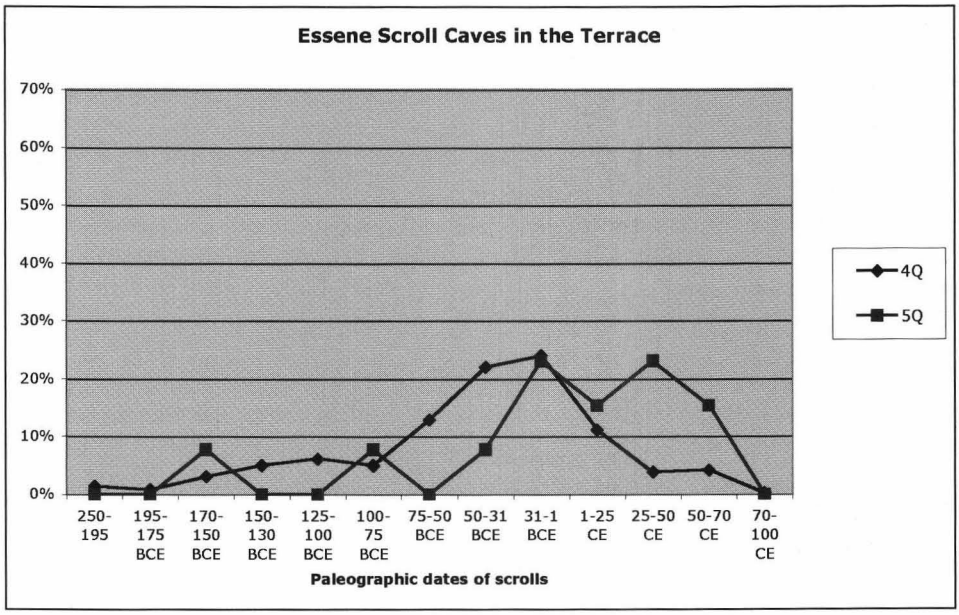


Fig. 8. Palaeographic dates of caves 4Q and 5Q

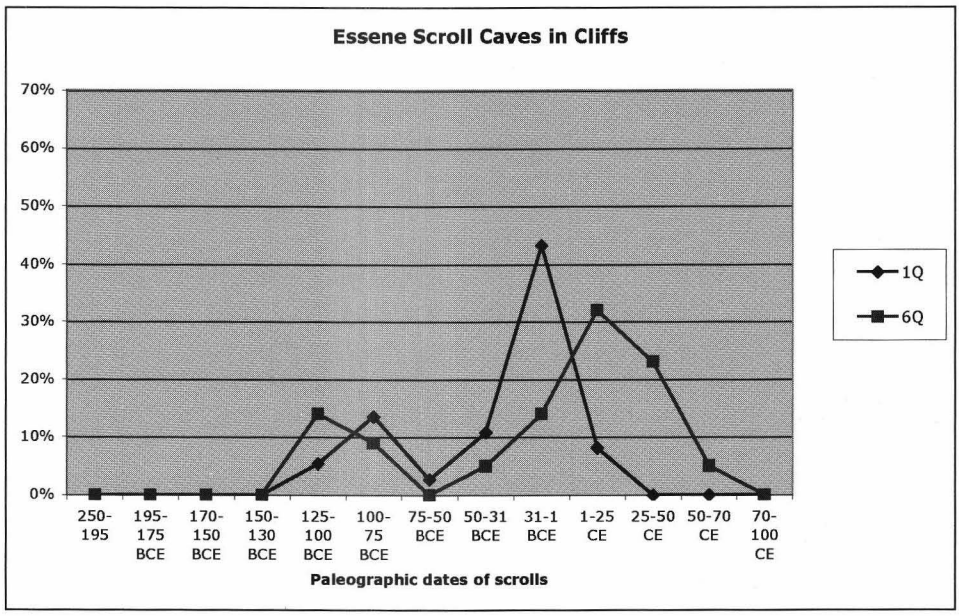


Fig. 9. Palaeographic dates of caves 1Q and 6Q

part on less costly papyrus (Fig. 10A). Cave 1Q contains scrolls of a more liturgical nature, including multiple copies of the Book of Psalms and two copies of the Thanksgiving Scroll and the Community Rule (which would point to a priestly *Yahad* library; Fig. 10D). Cave 6Q, on the other hand, reveals a library of a more lay character, a library containing the Damascus Document (with rules for lay members of the movement), a number of apocryphal or legendary works and a *megillah* (a pocket scroll carried by laity during festivals) of the Song of Songs (Fig. 10B). One other enigmatic difference between the two caves is that cave 6Q has no remains of phylacteries, while cave 1Q contains the remains (either parchment slips or leather cases) of eight phylacteries (as do all other verifiable Essene- type caves, i.e., 4Q and 5Q).

Furthermore, the striking parallel between the palaeographic dates of the scrolls of caves 1Q (at least 77 scrolls identified) and 6Q (at least 26 identified) is noteworthy. Figure 9 indicates that the period of manuscript collection (or production) for both caves 1Q and 6Q was from the late second century BC until the first quarter (1Q) or first half (6Q) of the first century AD. At least in the case of cave 1Q, which contained the major community compositions, the small early peak may well indicate an early history of this library, since the *Yahad* community's document 1QS derives from that period. The high peaks of the later period represent the apex of library expansion for both libraries. On the other hand, there is one distinction that may be significant: the final

	Primary Content	A Scroll materials	B Festal Pocket Scrolls	C Phylacteries and Mezuzot	D Psalms	D Liturgies and Hymns	D Calendars
Priestly Libraries							
1Q	Yahad/Priestly Essene c70mss	parchment only	—	1 head phyl; 2 head & 5 hand empty phylactery cases	Psalms 3x	H 2x, Sb, 3 tongues of fire, Lit Pr 2x, hymnComp 7x (18% of mss)	Serekh haYahad (364 day Biblical Festal)
11Q	Priestly Zealots c30mss	parchment only	—	—	Psalms 5x	ApPs, Ber, Hymns a & b, SOS (16% of mss)	Temple (364 day Pentacontad Festal)
Lay Libraries							
6Q	Lay Essene c33mss	mostly papyrus	Song of Songs 1x	—	Psalms 1x	hymnic comp, benediction?	
2Q	Revolutionary Simon b. Giora? c33mss	parchment only	Ruth 2x	—	Psalms 1x	—	
3Q	Lay Zealots c15mss	parchment (except Copper Scroll)	Lam	—	Psalms 1x	hymn (?)	
Mixed collections							
4Qa, 4Qb	Mixed; Geniza c700mss	parchment and papyrus	Ruth 2x, Cant 3x, Qoh 2x, Lam 1x	10 head & 2 hand phyl; 8 head & 3 hand empty phylactery cases; 7 Mezuzot	Psalms 23x	SOS 8x	S/Otot (364 day Biblical Festal); T & MMT (364 day Pentacontad Festal)
5Q	Mixed; Geniza c25mss	parchment only	Lam 2x	1 phylactery	Psalms 1x	curses	
Masada	Sicarii c16mss (literary)	parchment for Hebrew texts; others papyrus Samaritan,	—	—	Psalms 3x	SOS	

Fig. 10. Priestly vs. lay libraries

decline of collecting ends before AD 25 for cave 1Q and about AD 50 for cave 6Q, possibly indicating separate *termini* at which each group was forced to abandon the Qumran site (Fig. 9).

The zealot character of caves 11Q and 3Q

The caves of the north cluster (caves 3Q and 11Q, associated with certain nearby contemporary caves which lacked scrolls), share important similarities with one another and stand at a significant distance from the other caves in the central and southern cliffs (Fig. 14). With respect to doctrine, the genuine *Yahad* scrolls (e.g., the Damascus Document, the Thanksgiving Scroll, or the Rule of the Community) are completely lacking in these two caves. Instead, three copies of the Temple Scroll were found, a composition that is known to contain doctrines that are at variance with those of the *Yahad* (Fig. 12A). The main texts of caves 3Q and 11Q share a priestly and/or a Temple orientation, focused on defining and protecting the Temple and its contents (e.g., 11QTemple^{a-c}, 3QCopper Scroll).

Certain scrolls from caves 11Q and 3Q which were once considered to be *Yahad* compositions are no longer believed to be so. The Song of the Sabbath Sacrifice (Fig. 10D) and New Jerusalem (Fig. 12E) are now generally held by scholars not to be *Yahad* compositions. The commentaries 11QMelchizedek and 3QpIsaiah are anomalous and too ill-defined to be

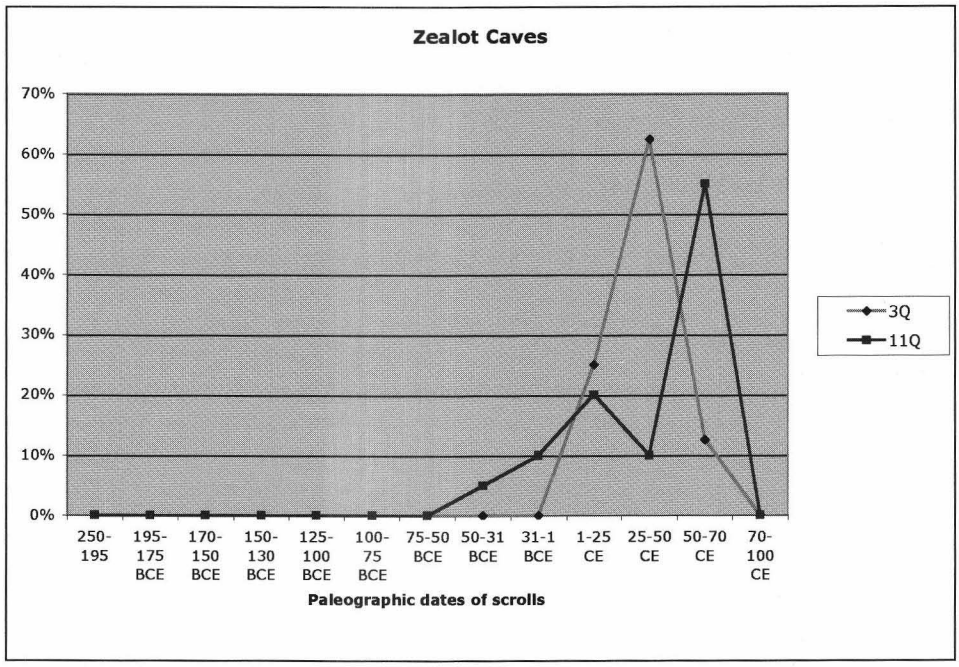


Fig. 11. Palaeographic dates of caves 3Q and 11Q

confirmed as *Yahad* compositions. Although 11QMelchizedek uses the word '*pesher*', it does not comment on the text of a biblical book in the order of its verses, as do other Qumran commentaries. 3QIsaiah starts with verse one of the Book of Isaiah, but the small piece of text that follows the verse is illegible. Also, there is no reason to believe that groups other than the *Yahad* did not write their own commentaries.

The scrolls from both caves 3Q and 11Q represent the remnants of relatively young libraries (Fig. 11). The scrolls of these two caves are among the latest from Qumran; 83% of the 11Q scrolls and 100% of the 3Q scrolls date to the first century AD. In fact 65% of the approximately 50 combined manuscripts from caves 11Q and 3Q date from the last 25 years before the fall of Jerusalem. The first-century scripts of these scrolls also tend to be particularly elegant and stately, perhaps linking them to the finer scribal schools of Jerusalem. All the scrolls from both caves were written on parchment (with the exception of the Copper Scroll), but none on papyrus (Fig. 10A). In addition, the corpus from both caves is exclusively in Hebrew and Aramaic; no Greek scrolls have been found in either cave.

Furthermore, in terms of both location and material culture, caves 3Q and 11Q stand apart from the other caves. The pottery, which includes distinctive oil lamps, is late (mid- to late first century AD). Five cylindrical jars, out of the 35 found in cave 3Q, were subjected to provenience testing. Results of neutron

activation analysis of the clay from at least four of the jars (and one lid) show that the jars were made from *Jerusalem clay*, indicating that they were brought from Jerusalem to the Dead Sea region (Humbert and Gunneweg 2003: 13–14). Furthermore, the textiles of cave 11Q are distinctive from those of caves 1Q and 4Q, for example, in that they are *bleached* white with indigo stripes (Bélis 2003: 236, pl. III:1–7). Bleaching is not found elsewhere at Qumran, where natural 'off-white' cloth is the norm.

Based upon the distinctive doctrine, the late dating, and the scribal elegance of the manuscripts, together with the Jerusalem source for the pottery, the bleached textiles, and the contents of the Copper Scroll, it is very possible that Golb and Cansdale might be correct in their suggestion that at least certain scrolls were brought from the libraries of Jerusalem, or even from the Temple, to the Qumran caves, and to caves 3Q and 11Q in particular. But if they are right, they are only partially so. The scrolls from caves 11Q and 3Q might, in fact, have been brought there from Jerusalem, but certainly not by the *Yahad* group, whose doctrines, sectarian compositions, and textiles are lacking in these two caves. It is far more likely that the scrolls of caves 3Q and 11Q were brought there by the revolutionary priestly protectors of the Temple and its treasures, more commonly known as the Zealots (a suggestion already made by both C. Rabin [1956] and J. Allegro [1964]).

I would suggest that these, and the other caves of the northern cluster, were inhabited briefly at the end of the First Revolt. The best candidate for ownership of these manuscript collections would seem to be the group of rebels led by the Zealot general Yehudah ben Yair, who came down from Jerusalem to the forest/thicket of the Yarden to make their last stand, along with another group of refugees from Machaerus.

> When Bassus had settled these affairs, he marched hastily to the forest of Jarden, as it is called; for he had heard that a great many of those that had fled from Jerusalem and Macherus formerly, were there gotten together. (211) When he was therefore come to the place, and understood that the former news was no mistake, he, in the first place, surrounded the whole place with his horsemen, that such of the Jews as had boldness enough to try to break through, might have no way possible for escaping, by reason of the situation of these horsemen; and for the footmen, he ordered them to cut down the trees that were in the wood whither they were fled. (212) So the Jews were under a necessity of performing some glorious exploit, and of greatly exposing themselves in a battle, since they might perhaps thereby escape. So they made a general attack, and with a great shout fell upon those that surrounded them, (213) who received them with great courage; and so, while the one side fought desperately, and the others would not yield, the fight was prolonged on that account. But the event of the battle did not answer the expectation of the assailants; (214) for so it happened, that no more than twelve fell on the Roman side with a few that were wounded; but not one of the Jews escaped out of this battle, for they were all killed, being in the whole not fewer in number than three thousand, (215) together with Judas, the son of Jairus,

> their general: concerning whom we have before spoken, that he had been captain of a certain band at the siege of Jerusalem and by going down into a certain vault underground, had privately made his escape.
>
> (Jos., *JW* 7.6.5.210–215; Whiston translation)

Shortcomings in the traditional 'consensus' theory

If the *Yahad* is identified with the Essenes, then the consensus of the majority group of scholars, the champions of the Essene hypothesis, appears to be correct, but only partially so. Most, but not all, of the caves and their scrolls can be identified primarily with the *Yahad* who lived at the site of Qumran (though only during two or three of the site's phases). However, judging by the presence of typical *Yahad* compositions, only caves 1Q, 4Qa, 4Qb, 5Q and 6Q can be connected to the group with any certainty.

Having segregated 3Q and 11Q as a sub-unit of caves, and 1Q, 4Qa, 4Qb, 5Q and 6Q, as a second group, can we recognize other unique libraries in the Judean Wilderness? What can be said about cave 2Q and about Masada? In fact, it can be suggested that a third category of library can be connected with First Revolt rebel groups. This sub-group shares some features in common and other features in contrast with the *Yahad* libraries, which from henceforth will be defined as Essene.

Features of the rebel caves (2Q, 3Q, 11Q, Masada) shared in common with the Essene libraries

All the caves, whether Essene or not, treasure the Torah. With regard to pseudepigraphic and non-biblical texts, the Book of Jubilees is found in 2Q, 3Q, 11Q and potentially, Masada (also in Essene caves 1Q and 4Q; Fig. 12E); New Jerusalem is found in 2Q and 11Q (and also in Essene caves 1Q and 4Q; Fig. 12E). The Song of the Sabbath Sacrifice is found in 11Q and Masada (as well as in 4Q; Fig. 10D). The Book of Giants appears in 2Q but is also found in Essene caves 1Q, 4Q and 6Q (Fig. 12E).

Some common features of the overall rebel group (2Q, 3Q, 11Q, Masada) over against the Essene libraries

Concerning the Prophets (Fig. 12B). 3Q, 11Q and Masada have only Ezekiel; the Temple Scroll of cave 11Q quotes almost exclusively from Ezekiel, a book that has a Temple orientation and is supportive of the Zadokite priesthood. (On the other hand, cave 2Q has Jeremiah and 3Q has a commentary(?) on Isaiah.)

Concerning the Apocrypha (Fig. 12C). Ben Sira, which runs contrary to Essene teaching on a number of points, including its support for the lunar calendar, is found only in 2Q, Masada and 11Q (quoting a chapter in 11QPsa[a]). This book is not found in the other caves.

		A	B	C	D	E		
	Primary Content	Sectarian	Prophets	Ben Sira	Phylacteries and Mezuzot	Jubilees	EnGiants	New Jerusalem
Essene Libraries								
1Q	Yahad/Priestly Essene c70mss	S, SE, H 2x, pMic, pHab, pZeph, pPs; (M)	Isa (2x), Ezek, XIIProph, Pss	—	1 head phyl; 2 head & 5 hand empty phylactery cases	Jubilees 2x	EnGiants 2x	NJ 1x
6Q	Lay Essene c33mss	D	Daniel	—	—	—	EnGiants 1x	—
4Qa, 4Qb	Mixed; Geniza c700mss	S 10x, SE 9x, H 6x, pIsa 5x. pHos 2x, pMic?, pNah, pZeph, pPs 2x; (T 1x, M 7x)	Isa 17x, Jer 5x, Ezek 3x, XII 7x, Pss 23, Dan 5x	—	10 head & 2 hand phyl; 8 head & 3 hand empty phylactery cases; 7 Mezuzot	Jubilees 8x	EnGiants 6x	NJ 2x
5Q	Mixed; Geniza c25mss	S, D	Isa, Amos, Pss	—	Phylactery	—	—	NJ 1x
Rebel Libraries								
11Q	Priestly Zealots c31mss	(Temple 3x, Melkizedek; M, S?)	Ezek only	Ben Sira (last chapter in Pss)	—	Jubilees 1x	—	NJ 1x
2Q	Revolutionary Simon b. Giora? c33mss	—	Jer only	Ben Sira	—	Jubilees 2x	EnGiants 1x	NJ 1x
3Q	Lay Zealots c15mss	(comIsa?)	Ezek only; (Isaiah comm?)	—	—	Jubilees 1x	—	—
Masada	Sicarii c16mss (literary)	sectarian doc?	Ezek only	Ben Sira	—	Jubilees? 1x	—	—

Fig. 12. Essene vs. rebel libraries

This evaluation of the manuscript collections on the basis of content, exclusion and inclusion of books, leads to tentative identifications with specific movements and groups in the First Revolt. It may be suggested that, just as a distinction can be made between Essene priestly (1Q) and Essene lay libraries (6Q), so, too, a distinction can be made between rebel priestly and rebel lay libraries.

Concerning the Liturgy and Calendar (Fig. 10E). Although there is a reasonable predominance of multiple copies of the book of Psalms and liturgies in libraries which are devoted to priestly practice (more than 15% of the manuscripts in caves 1Q and 11Q) there are also certain distinctions in liturgical practice that can be discerned between the main Essene priestly and rebel priestly caves 1Q and 11Q. The most obvious distinction is found in the definition of the liturgical year itself. Although the 364-day solar calendar is predominant among the extant scrolls from the Qumran caves, the calendars that are attached to or embedded in the central rulebooks vary when comparing the actual feast days that are observed during the course of the liturgical year. The calendar attached to the 4QS^{e} manuscript of the Community Rule limits its acknowledged feast days to those which are commanded in the Bible, including Second Passover. The Temple Scroll (11QT) observes a pentacontad festal cycle which adds a sequence of additional harvest festivals including the 'Feast of New Wine' and the

'Feast of New Oil', each separated by 49 days, but does not mention the Second Passover.

Concerning Phylacteries and Mezuzahs (Fig. 10E). The remains of 33 phylacteries and eight mezuzahs were found in the caves of Qumran. Curiously, all were found in Essene caves (in 1Q, 4Q, 5Q and suspected Essene cave 8Q; only lay Essene cave 6Q lacks them).[7] The fact that no phylacteries were found in the suspected rebel caves 2Q, 3Q or 11Q may be of significance with regard to *halakhic* practices among the sects of the late Second Temple Period.[8]

Rebel priestly vs. rebel lay libraries?

Rebel priestly libraries

Limited to 11Q (but to some extent, also Masada's library, which appears to have a mixture of priestly and lay components). As in the case of the Essene priestly libraries, the rebel priestly libraries have liturgies, multiple copies of the book of Psalms, and texts that focus on the Temple structure and service (Figs. 10D, 12A–B).

Potential rebel lay libraries

Caves 2Q and 3Q both have copies of the typical *megillot* Ruth (two copies) and Lamentations, which are normally associated with lay participation in the yearly festivals (Fig. 10B). 2Q and 3Q also have legendary texts/apocrypha, which are often found in lay contexts, presumably since they bolster lay participation in the divine plan. (This is also the case for the lay Essene cave 6Q.)

Although cave 2Q is in the same cluster as cave 1Q, it is not likely connected with the Essenes, since it contains no community documents. It also includes *Ben Sira*. Like 3Q, it lacks liturgies and other scrolls normally associated with priestly groups.[9]

The case of Masada

Early during the Revolt, Masada became the sole stronghold and residence of the Sicarii. The founder, Judah the Galilean, and his successors were called 'teachers' by Josephus (*JW* 2.118). There is no reason to believe that this group would not keep an institutional library. The cache of scrolls found at Masada that were once thought to be connected with the Qumran scrolls (i.e., the Song of the Sabbath Sacrifice and the New Jerusalem text), are no longer considered to be either *Yahad* or Essene in character. The corpus of manuscripts from Masada should be viewed as the remnants of a Sicarii library, written mainly on parchment, with certain lay and priestly components (Figs. 10, 12). (The various extraneous papyrus documents derived from the Roman occupation of the site must be treated separately.)

In contrast to the collection profiles of caves 11Q and 3Q, the period over which scrolls were introduced into the collections at both Masada and cave 2Q lasted from the late second or early first century BC until at least the mid-first

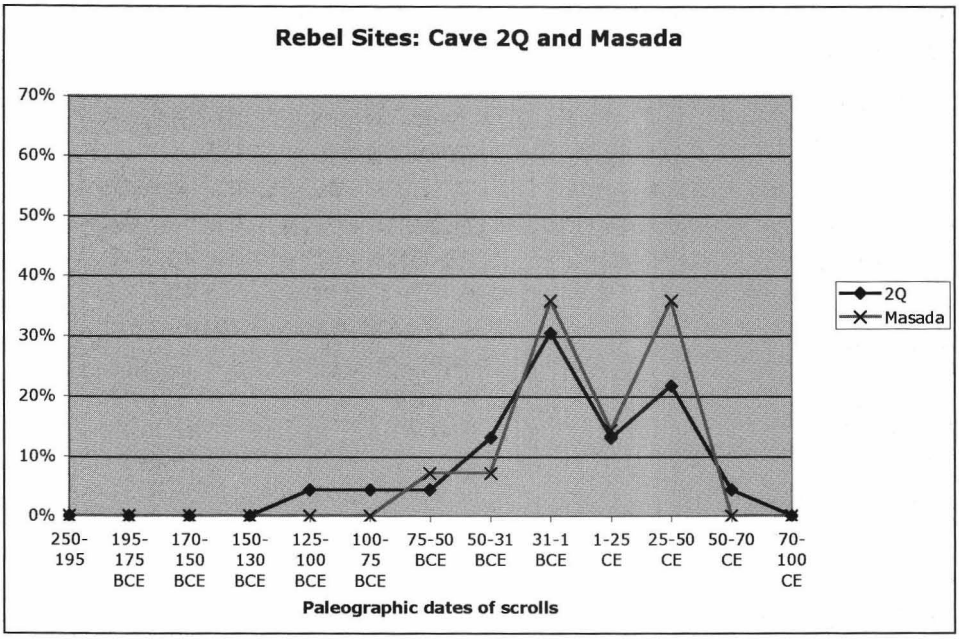


Fig. 13. Palaeographic dates of cave 2Q and Masada

century AD. There were two major peaks during this collection period at both sites: 31–1 BC and AD 25–50 (representing 73% and 52% of the total, respectively; Fig. 13). Although this may indicate groups with longer histories, the earlier peak might simply represent the incorporation of a group of manuscripts collected for an earlier, unconnected library.

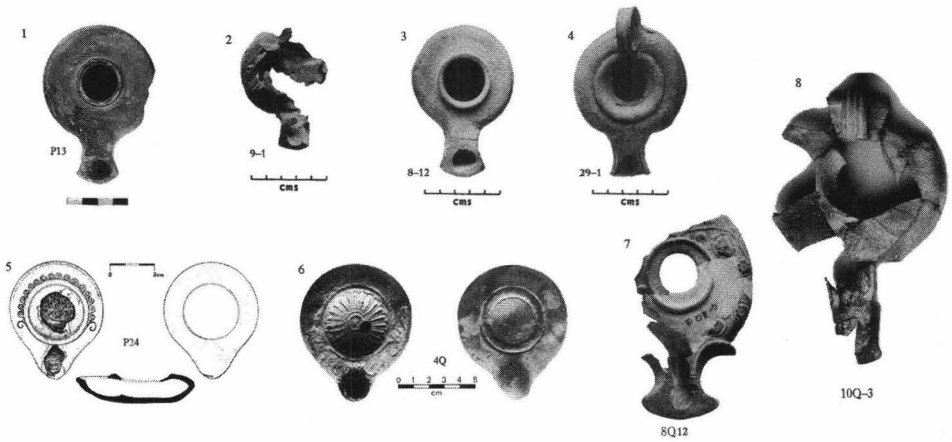


Fig. 14. The elusive character of caves 7Q, 8Q, 9Q and 10Q

The elusive character of caves 7Q, 8Q, 9Q, and 10Q

The manuscript remains from caves 7Q, 8Q, 9Q, and 10Q are quite meagre and assessment of their character is thus highly tentative. It may be suggested that the remains from cave 7Q, which contained only Greek biblical and literary documents, written on papyrus, reflect the remnants of a Hellenistic Jewish scroll collection. The manuscript remains from cave 8Q, which contained Psalms, a liturgical work, a phylactery and a mezuzah, although having a definable character, are too sparse to connect with the other caves.[10] The manuscripts from caves 9Q and 10Q are not definable since they each contain one fragment of indecipherable text. However, in light of the food remains and lamps from all four of these caves, they all appear to have been used at the end of Period IIb (AD 66–68) as residences for the rebels.[11] This is also true with respect to most of the caves throughout the Qumran cliffs as well as in the caves of Wadi Murraba'at where at least one papyrus divorce document (Mur. 19) dating to year 6 (!) of the First Revolt and an ostracon (Mur 72) derive from a refugee from Masada.[12]

Too many scrolls?

Hirschfeld, following Golb and Cansdale, suggests that there are too many scrolls and too great a diversity of texts to be owned by the Essenes or connected with the site of Kh. Qumran.

As demonstrated above, not all the caves find their origins in the Essenes. As for the potential that the diverse documents from the remainder of the caves (1Q, 4Q, 5Q and 6Q) had been deposited there by the Essenes, one should remember, first of all, that the group that collected the scrolls were known to themselves as Sons of Light, the *Yahad*, and other terms. They never called themselves 'Essenes', a term used only by outsiders (just as the titles 'Pharisees', 'Sadducees', and even 'Christians', were at first only used of those groups by outsiders). Secondly, according to Philo and Josephus, the Essenes were absorbed in studying the sources. They interpreted the sacred writings, the law and the prophets. They produced their own rulebooks and by oath were devoted to them. They also studied the 'works of the ancients' for the sake of the 'body and soul', for healing of diseases and protection.

> There are some among them who, trained as they are in the study of the holy books and the <sacred> writings, and the sayings of the prophets, become expert in foreseeing the future: they are rarely deceived in their predictions.
>
> (Josephus, *War* 2.8.159)

> One of them then takes up the books and reads, and another from among the more learned steps forward and explains whatever is not easy to understand in these books. Most of the time, and in accordance with an ancient method of inquiry, instruction is given them by means of symbols.
>
> (Philo, *Quod Omnis Probus liber sit* 80–82)

> In addition, he swears to transmit none of the doctrines except as he himself received them, abstaining from all <alteration>, and to preserve the books of their sect likewise, as also the names of the Angels. Such are the oaths by which they secure the fidelity of those who enter the sect.
>
> (Josephus, *War* 2.8.142)

> They apply themselves with extraordinary zeal to the study of the works of the ancients choosing, above all, those which tend to be useful to body and soul. In them they study the healing of diseases, the roots offering protection and the properties of stones.
>
> (Josephus, *War* 2.8.136)

Conclusion

It appears to be high time to abandon the monolithic approach to the caves that assumes a common owner or origin for all the caves. Rather, each cave must be assessed on its own merits. Salient features of the scroll collections found in each cave include doctrinal content, date range, scroll material, language, and scribal protocols. In addition, the material culture associated with each cave and its scrolls must be examined and compared, including pottery forms, clay source analysis and textiles. Last but not least, the location of the caves with respect to one another and with respect to Qumran itself may be an indicator of ownership. On this basis, the foregoing study has suggested that caves 1Q and 6Q derive from priestly and lay Essene groups, respectively; that caves 4Q and 5Q served as *genizas* for the Essenes, both priestly and lay, during their phases of occupation of the site; that caves 11Q and 3Q derive from priestly and lay Zealot parties at the end of the First Revolt; that caves 7Q, 8Q, 9Q and 10Q date as well to the First Revolt; that Masada finds its owners among the Sicarii, and that cave 2Q is potentially connected with Simon bar Giora, whose troops were known to be in the area of the Judean Wilderness at the time.

Notes

1 The appellation 'Qumran Community' is insufficient since the primary group among the scrolls was not limited to the area of Qumran. 'The Covenanters', preferred by Shemaryahu Talmon, could be used to define a number of different groups. 'Sectarian' is a generic term which can fit a number of sects which existed during the Second Temple Period. The term '*Yahad*' does represent the priestly group connected with the Community Rule, but does not represent the lay group(s) connected with the related Damascus Document, also found at Qumran. None of these terms sufficiently defines the group connected with the Temple Scroll or other distinctive documents found among the caves. For this study, the Essene character of the groups represented by the Community Rule and the Damascus Document is considered highly probable since no other material remains of the well-attested Essenes have been found outside of these documents and the Qumran site connected with them. It is also highly implausible that the same historical sources would have entirely overlooked or ignored such an otherwise Essene-like

group with such an extensive manuscript collection and archaeological remains, as is found at Qumran and associated sites like Ein Feshkha. For the purposes of this essay the group treated in the Community Rule will be called '*Yahad*' or 'priestly Essenes', and the group(s) of the Damascus Document will be called the 'lay Essenes'. 'Sectarian' will be utilized to convey its generic meaning related to Jewish sects in general.

2 The fact that locus 2 lay below the room with plastered benches and ink-wells (locus 30) and was adjacent to locus 4, the benched room, indicates that it was a meeting room associated with the production and storage of scrolls.

3 With the premature death of Prof. Hirschfeld, the archaeology community suffered the loss of a fine colleague. He was in the prime of life and of his academic productivity. He will be remembered for the valuable contributions made to our field of study by his publications and his excavations at Hammat Gader, Ramat HaNadiv, Ein-Gedi, Tiberias, Shivta, and throughout the monasteries in the Judean Wilderness.

4 A distinction should be made, however, between compositions with an interest in a Zadokite priesthood and those which scholars could identify as belonging to the party of the Sadducees in Jerusalem. Apparently the factional nature of certain parties such as the Essenes (described by Josephus, Philo and Hippolytus), the Pharisees (e.g., the schools of Hillel and Shammai detailed in Rabbinic literature) and the rebels (e.g., the Sicarii, Zealot, Simon bar Giora, and John of Gischala factions as described by Josephus) is potentially true also among the Zadokites (which includes the Hellenized form known as the 'Sadducees' and the other subgroups represented by MMT and the Temple Scroll).

5 Caves 7Q–10Q must be eliminated for this part of the assessment due to the insufficient quantity of manuscript remains in those caves.

6 This survey is based on the paleographical dates for the scrolls published in the *editio princeps*. While some minor adjustments in paleographical dates can be anticipated in the future, for the moment these remain the accepted dates for the production of the scrolls.

7 It may be significant that all phylacteries and *mezuzahs* were found in caves which could be defined as priestly Essene or 'mixed priestly and lay' Essene in character.

8 It is widely known from the literature that at least one other group from the Second Temple Period, the Pharisees, did wear phylacteries. A head phylactery case was found at W. Murabba'at and hand phylacteries were found both at W. Murraba'at and at N. Hever, both sites generally taken to be from the Bar Kokhba Period. It should be noted that Murabba'at does, however, have at least some materials identified with the First Revolt.

9 Cave 2Q is likely connected with revolutionaries, perhaps with the group led by Simon bar Giora. This group is known to have had a presence in the area. The two copies of the Book of Ruth would have had extra significance for this group whose leader, bar Giora ('son of a proselyte'), like Ruth, was a convert to Judaism who originally came from across the Jordan.

10 Since a Psalm scroll and a hymn have already been identified in 8Q, the existence of a phylactery and *mezuzah* in the same cave might lend support to the idea that this cave conveys a priestly Essene character. However, the evidence still remains admittedly meagre to support such an assertion.

11 R. de Vaux understood Kh. Qumran to be a site with multiple phases of occupation. His assessment was based both on changes in stratigraphy and in material culture, including ceramics, coins, and fabrics. The presence of weapons, stoneware, hoards of Revolt coins, and new additions to and distribution within

the pottery repertoire in the latest stratum at Qumran argues for the presence of rebel occupants. I would suggest that such a presence came about at the beginning of the Revolt in 66 AD and ended in 68 AD (based upon the modest number of 'year 2,' 68 AD, Revolt coins in the debris). Although cave 2Q may be linked with this occupation, I have proposed that the deposits in the northern cluster of caves – which are located 2 km from the site and which contain the latest scrolls – were left there in AD 70 by members of the Zealot party, fleeing from the besieged temple in Jerusalem, two years after the site of Qumran had already been destroyed and was at that point still largely in ruins and temporarily unoccupied (see Fig. 15). For an updated assessment of de Vaux's multiple periods of occupation of Kh. Qumran, see Bruce and Pfann 2006.

12 *DJD* 2. pp. 104–109, Fig. 28, pl. XXX.

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Review Article

Qumran in Context: Reassessing the Archaeological Evidence

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In recent years the Qumran-Essene hypothesis – most comprehensively presented by Roland de Vaux, who excavated the site of Qumran (Fig. 1) in the 1950s – has come under repeated attack. While initial doubts were expressed by historians such as Norman Golb (1995) and Lena Cansdale (1997), the battle is now raging within the realms of archaeologists. In his

Fig. 1. View of Qumran from the pass to the west of the ruins

book, *Qumran in Context: Reassessing the Archaeological Evidence* (2004), the late Yizhar Hirschfeld has examined the site from its earliest to latest forms, providing a radical revision of how it may be viewed within the context of the Dead Sea region. The argument and language of this book is clear and accessible and the illustrations superb, all of which will continue to invite many scholars and the general public into Hirschfeld's alternative scenario. Hirschfeld was innovative in his approach to the site and presented a cogent challenge to supporters of de Vaux. This book was one that I was happily willing to endorse as a serious thesis from a worthy and gracious opponent. It is a great shame that he is no longer with us to engage in debate about the site of Qumran or see the impact of his work.

In *Qumran in Context* Hirschfeld presents the view that the Qumran-Essene hypothesis is no longer sustainable in the light of archaeology, taken on its own terms. Roughly following the chronological guidelines first proposed by Jean-Baptist Humbert (1994), he defines four strata. Stratum I is an Iron Age establishment. Stratum II dates to the Hasmonean Period. Stratum III is defined as 'Herodian' though it stretches from the time of Herod the Great to the fall of Qumran in AD 68. Stratum IV is the period of Roman occupation dating from AD 68 till as late as the Second Revolt (a theory I suggested at the Brown Conference on Qumran archaeology in 2003, and one which I discussed very profitably there with Prof. Hirschfeld, whose comments were very astute and valuable to me).

It is the period represented by Strata II and III (corresponding to de Vaux's Period Ia, Ib and II) that is relevant for the Qumran-Essene hypothesis, since it is during this time de Vaux and Humbert suggest that Essenes built up and occupied the site. Hirschfeld argues that in the Hasmonean period (late second century BC to mid-first century AD) the structure existing at Qumran was a kind of fortress, while in the 'Herodian' phase of Stratum III the site was a manor house or estate in which a variety of industries took place, partly involving the processing of opobalsam. In all periods, Hirschfeld sees a connection between Qumran and Ein Feshkha, so that they formed one agricultural zone, joined by a long wall. Hirschfeld finds no reason to associate this estate with Essenes, and suggests that the Dead Sea Scrolls were an important library deposited in caves close to Qumran at one time by refugees from Jerusalem who hid them with the help of local Jews who were non-sectarian. The Essenes are to be detached from Qumran and placed instead in the hills behind En Gedi, where Hirschfeld has excavated small huts in which they purportedly lived. Hirschfeld accepts no peculiarities of the site of Qumran that cannot be explained in terms of a rural manor house model, for which he draws on a number of archaeological parallels.

It is difficult to review such a rich counter-hypothesis and all the wealth of evidence Hirschfeld brings to his argument concisely, and a detailed critique of Hirschfeld's archaeological proposals in regard to the buildings alone would involve a lengthy discussion. Jodi Magness, in the *Review of Biblical*

Literature 8 (2005), and also Hanan Eshel (2005), have already pointed out a number of errors and issues for consideration and these need not be repeated here. However, one thing to note is a tendency by Hirschfeld to use 'either/or' language that attempts to refute the identifications made by other scholars by means of de-emphasizing their observations and results and over-emphasizing an alternative. Therefore, the emphasis on the tower in the Hasmonean period leads Hirschfeld to reject completely the suggestion by Humbert that the architectural remains represent a villa. However, in the Herodian Period Hirschfeld can easily accommodate the tower as part of his rural manor house, seeing it as an appropriate defensive inclusion in the total plan. Indeed the revetment that Hirschfeld associates with the Hasmonean stratum is from this time: Magness (2002: 57) notes that the rampart was an addition to strengthen the tower after the earthquake of 31 BC (so also Magen and Peleg 2006: 102).

Hirschfeld judges the construction to be a fortress on the basis of the tower (Fig. 2), a square plan and the strategic location, yet his example of manor houses – which he considers clinching comparisons for the *Herodian* phase of the site – are all fairly neatly square/rectangular with each having their own single tower, and would match the Hasmonean phase of Qumran – as

Fig. 2. The tower of Qumran from the west

Hirschfeld identifies it – much better than the expansive site he identifies in Stratum III. The point is that just because something is fortified it does not make it a fort, *viz.* something designed *primarily* for defence.

De Vaux (1973: 5–7) already noted that the two-storied tower was built defensively since the lower walls are also thickened at the base and very robust, though it should be remembered that the tower did not loom above any other part of the building complex since much of this complex was two-storied (clearly seen in locs. 1, 2 and 30), in fact Hirschfeld has it as three-storied in his Figure 40 and four-storied in Figures 42 and 57. Nevertheless, de Vaux concluded that the building complex of Period Ib is not *as a whole* built defensively, as a fort. What we can identify in Qumran's tower surely – as with the other local manor houses/villas – is strengthened fortification for defensive purposes, but this does not necessarily make the entire architectural assembly into a fort with a single military *raison d'être.*

If we resist the 'either/or' dichotomy, it is easy to see that structures can have multiple rationales at any one time. There is literary evidence to corroborate the building of towers to defend agricultural resources from theft (e.g. Isa. 5: 2; Matt. 21: 33, Luke 14: 27–30), and the study of the Nazareth Village Farm in this issue of the *Bulletin* well illustrates the use of towers to guard agricultural domains. The tower at Qumran might also have enabled watchmen to guard the pass and roadways, which is indeed a strategic consideration, and one which Magen and Peleg (2006: 102–104) also see as decisive in identifying early Hasmonean Qumran as a kind of military post. The similarities between Qumran, Rujm el-Bahr and Kh. Mazin may well mean that there were a string of settlements established during the early Hasmonean Period around the Dead Sea, but they were surely not purely military posts, but designed for the exploitation of the economic resources of the lake and supervision of the trade that resulted, as Hirschfeld himself has pointed out.

How long this form of Hasmonean Qumran existed remains a controversial point. A critical issue of chronology in Hirschfeld's (and Humbert's) schema is when exactly the buildings and water systems were expanded. It is at the time of the expansion that Humbert considers Essenes first occupied the site. De Vaux identified this expansion with Period Ib, dated to the initial years of the first century BC, while Humbert is doubtful that this took place so early, placing the expansion later in the first century BC, a chronology followed by Hirschfeld, who suggests that this phase began in either 40–37 BC (Parthian invasion) or 31 BC (post-earthquake). Jodi Magness considers the expansion part of the early growth of the site (which has no Period Ia as de Vaux defined it) long before the earthquake of 31 BC. Magen and Peleg have now presented a different chronology in which there was a later Hasmonean (pre-Herodian) expansion. They reason that Essenes could not have arrived at this time because the residential space was cut back by the construction of pools (*loci* 56, 58, 48–9), though

without any apparent consideration of upper storey modifications. Full publication of the results of de Vaux's excavations (now being prepared at the École Biblique under the direction of Jean-Baptiste Humbert), along with those of Magen and Peleg, will undoubtedly enable the discussion to proceed with more clarity.

It should be noted that Hirschfeld's title, *Qumran in Context*, is quite telling. It is right methodologically to situate a site within its context and ensure that any analysis takes place with everything that is known about that context firmly in mind. It needs to be remembered, however, that one can be right methodologically but still come unstuck in terms of an argument by over-emphasizing some factors while de-emphasizing others, for the sake of a polemical 'either/or' approach.

In terms of emphasis, Hirschfeld draws on good studies to define the roadways or tracks around the Dead Sea, and also pays careful attention to harbours. It is absolutely the case that traffic went by way of rivers, lakes and seas in the ancient world, and the Dead Sea was no exception (Nissenbaum 1991; Hadas 1992; Shimoni, Yucha and Werker 1992). One sees on the Madaba mosaic map, dated to the 6th century AD, that there are boats on the Dead Sea carrying cargo. But the question of whether Qumran was a commercial hub is not solved by noting connections without careful attention to the date of these paths, tracks and shipping routes. Accessibility does not mean that Qumran was a commercial *entrepôt* and a hive of busy trading activity throughout all periods of its habitation. We do not always know who controlled trading routes, or what access was permitted in any given zone, especially in the case of precious commodities, in particular the opobalsam trade that Hirschfeld is most interested in emphasizing. If, for example, it was always a royal monopoly, what does that mean for trade access to the region? Qumran aside, the reasons why it is hard to consider the northwestern Dead Sea busy with commercial activity is provided by the very archaeological evidence Hirschfeld is keen to rely on: the Dead Sea scrolls were hidden in certain caves in the region (Caves 1–3, 6, 11), just as people themselves would hide in caves in the region in both the First and Second Revolts, surely *because* this area was not well-travelled, but rather wilderness in which hidden things/people would not easily be found, whether by Romans or anyone else (who might betray them). In addition, as Humbert has pointed out, 'except in urban contexts, cemeteries were generally established in areas that were useless, abandoned or remote' (Humbert 2006: 23).

In terms of Qumran's industrial and agricultural dimensions in the context of the Dead Sea, Hirschfeld in Chapter 5 gives a very good presentation. As Zangenberg and others have reminded us, from the fourth century BC onwards the Nabataeans had collected bitumen from the surface of the lake and sold it to the Egyptians for embalming.[1] There was a trade route leading from the lake to Egypt as well as across the Judaean wilderness to the western part of Palestine via Zoara (Har-El 1978). The production of

opobalsam, dates, salt, sulphur and soap involved the towns and settlements in labour and trade. In the Roman and Byzantine periods, the two major towns actually on the lake were Zoara, in Nabataea/Arabia, and En Gedi, both being centres for the opobalsam industry and date propagation (see Hepper and Taylor 2004). R. M. Bloch (1962, cf. Rosenson 1986) has suggested that the boats depicted on the Madaba mosaic map are carrying cargoes of salt, shown as different colours to illustrate the distinction between sea and rock salt. In addition, there was the production of soap in this region. Hirschfeld notes (p.138) that a 'cleansing product' was manufactured here. Zohar Amar (1998) has identified the installations and materials found by Vendyl Jones in the so-called 'Cave of the Column' as being connected with the production of soap from potassium-rich plants (from the family of *Chenopodiacae*) which grow wild in the vicinity, which might fit with the fact that the occupants of Qumran were also washing in the basins of *loci* 34–5 (De Vaux 1973: 7; Magness 2002: 123, though Magen and Peleg 2006: 65, assign these basins to the perfume industry). All this suggests that it is reasonable to consider that there were one or more such industries taking place at Qumran, along with the propagation of palms, processing of dates and some pottery production (without making it primarily a pottery production centre as do Magen and Peleg 2006). In the crunch, however, Hirschfeld is clearly most intrigued by the opobalsam-cultivation or processing possibility: '[it] is most likely that many of the installations at Qumran were connected with the processing of the unique resources of the region, the valuable perfumes and ointments produced from balsam' (p.138).

Ein Feshkha, which Hirschfeld himself has excavated, is a critical element in his identification of the industry of Qumran since it is considered part of the total farmed estate. Here there is an oasis covering originally over 1 km in length crossed by both natural and artificial channels where a mixture of brackish and not-so-brackish springs fed a basin of 150–200 m^2, up to 120 cm deep, with a maximum temperature of 27°C. Nevertheless, the fertility of the oasis of Ein Feshkha and its suitability for growing certain plants has still not been tested by practical botanic experimentation, nor has there been a serious landscape archaeology analysis of the area or of the Qumran plateau. Discussions on the springs of Ein Feshkha can highlight the sweetness (so Hirschfeld, 7–11) or the salinity (Broshi and Eshel 2006) of the water, since in fact the whole area is pock-marked with springs of varying salinity levels. Moreover, no geological study of where ancient – now dried up – springs were located has yet been done. Archaeology does indeed indicate that some moderately sweet(ish) water could be harnessed, since north of the main structure at Ein Feshkha was an installation Hirschfeld identifies as being related to opobalsam processing, though more likely it is a date-wine press (so Netzer 2005), and water was fed to a reservoir next to this installation from a now extinct spring north of the site (Fig. 3). But it is another thing to propose that this whole region could actually support the

Fig. 3. Water channels at Ein Feshkha

growing of opobalsam. Date palms, on the other hand, are quite resistant to salinity, and experiments could confirm what else could grow here and provide perhaps some vegetation necessary to feed the number of animals resident in Ein Feshkha, as well as the human population. From the archaeological evidence it must be the case that animal husbandry was a critical concern of the residents of Ein Feshkha. South of the main building was a large animal pen (34 × 34 m) with a stable running along the northern side. How many sheep/goats could be contained here has not been estimated, but they surely provided meat that was consumed both here and at Qumran, the remains of boiled meals being buried (for purity and hygiene?) throughout the latter settlement.

However, as with the case of roadway and lake access, the fact that there was agriculture (date palms, animal fodder and vegetables?), animal husbandry and industry (date processing for wine/honey, soap production, pottery production etc.) at Qumran and Ein Feshkha is in no way inconsistent with the Qumran-Essene hypothesis, despite the way that Hirschfeld has presented his case so beautifully. Many scholars who support the Qumran-Essene hypothesis are also interested in the industries and agriculture that may have taken place in the vicinity of the site. When full publication is

completed, the various agricultural metal tools found at Qumran will undoubtedly illuminate the range of agricultural work considerably. De Vaux himself was fascinated by the industrial installations at Qumran and Ein Feshkha, as well as agricultural matters (suggesting for example that the occupants farmed the Buqei'a at the top of the pass, to furnish the wheat then ground in the mills of *locus* 100 and 104; de Vaux 1973, 28–9). De Vaux knew very well that the Essenes in the classical sources are described not as contemplative ascetics unengaged in any productive activity but repeatedly as the very opposite. They are actively working at various enterprises. Hirschfeld, unfortunately, read these sources quite narrowly.

For example, Hirschfeld took it as literally true that the Essenes lived, *sine pecunia*, 'without money' (Pliny, *Historia Naturalis* 5:15:4/73), so that any money or commercial activity found on the site of Qumran was interpreted as evidence that contradicts the Qumran-Essene hypothesis. Hyperbole aside, it is evident elsewhere in our sources that Essenes earned money, which they would then deposit into a communal fund (Philo, *Prob.* 86; *Hypoth.* 11:4, 10) and so we might expect some coin hoards in a site occupied by Essenes. Philo states that the Essenes labour in agricultural and artisanal work (*Prob.* 76). In the *Hypothetica* Philo mentions cultivators, shepherds, and bee-keeping (11:8) as well as technical skills (11:9). Josephus would write, after his comment that the Essenes have a different ritual of purification for their sacrifices: 'Otherwise, best are [the] men who have directed their way and all to work hard in agriculture' (*Ant.* 18:19). He sees Essenes earning money (*Ant.* 18: 22), and in *War* 2.129 he mentions *technai* – crafts, skills – in which the Essenes were proficient, which would easily have included processing of agricultural products, soap manufacture or pottery making. Since opobalsam sap had a highly esteemed medicinal use in antiquity, any manufacturing process for this – if ever proven – still would not contradict the Qumran-Essene hypothesis, since Essenes were interested in medicinal plants (*War* 2: 136; see Taylor 2006:144).

This points to an issue that underpins Hirschfeld's entire critique of the Qumran-Essene hypothesis. There is no attempt in the book to describe exactly who the Essenes were, and yet at every turn we are supposed to see the identity of this group as somehow self-evident from the occasional citations of the sources he uses. In fact, it is Hirschfeld's idiosyncratic conception of the Essenes that governs the paradigms he creates in his work. We have mention of 'a small sect such as the Essenes' (p. 45), or, concerning the Hasmonean tower, 'the revetment was clearly designed and built as an integral part of the tower. And since the function of the revetment was clearly defensive, its presence at Qumran is an indication that its inhabitants were far from being pacifists, as were the Essenes' (p.72, cf. 241). Essenes apparently did not eat meat, being ascetic and veritably identical to Pythagoreans (p.111, cf. *Ant.* 15:371): '[it] is absurd to think that the inhabitants of Qumran, who were obviously meat eaters, could also have been Essenes'. The presence of some

women's skeletons in the cemetery rules out the identity of the population as Essene because 'according to Pliny, they shunned the company of women' (p.161). The Essenes are described 'as freely choosing poverty and a frugal life' (p. 230, citing Josephus, *War* 2:122; Philo, *Prob.* 689). They are a 'small sect living on the periphery of Jewish society, without access to the Jewish administrative establishment in Jerusalem' (p. 231).

Therefore we have a picture of a small, ascetic, male, vegetarian, pacifist, isolated sect who are linked, by Hirschfeld, with wandering ascetics like John the Baptist and Bannus: '[t]he sources tell of figures, such as John the Baptist and the Essenes, who lived an isolated and ascetic life near the Jordan river or in the cliffs above the Dead Sea' (p.211). Hirschfeld conflates Essenes with Bannus-like ascetics, but the Essenes are actually not described in the sources in the way Hirschfeld defines them.

There is nothing that requires us to imagine that the Essenes adopted extreme asceticism, even if they were frugal. It is not stated that they were vegetarian, or that women could not be involved with any of their communities, or that they were a small sect who only lived an isolated existence far away from Jerusalem. In our sources they are characterized by communal living and particular attention to purity, characteristics that have been shown to fit well with Qumran's archaeology, as de Vaux argued and Magness has explored further. Even the identification of the Essenes as pacifist is highly questionable, since Philo mentions only that they do not manufacture instruments of war as part of their particular commercial operations, as they also reject a trading market, retail business or ship-owning (*Prob.* 78). In *Prob.* 76 Philo indeed states that they live in villages and shun cities because of the iniquities found in city life, but Philo – correcting himself – in the *Hypothetica* writes that the Essenes live in 'many cities of Judaea and many villages' (11:1), which coheres with Josephus, *War* 2:124. When Philo gives the number of Essenes as being over 4000 (*Prob.* 75, as also *Ant.* 18:20), the emphasis is on just how many of them there were; they are in *Prob.* 91, a *homilos*, a 'crowd' or 'throng'. Moses trained 'multitudes' of his pupils for a life of community, namely the Essenes, and 'they dwell... in great and much-populated throngs' (*Hypoth.* 11:1, cf. 11:5). Pliny too refers to the Essenes as a *turba*, 'throng, large multitude'. Wherever they had their main bases or communities, they are defined by Josephus as one of the three main legal parties in Second Temple Judaism. The Essenes appear in the Jerusalem Temple at certain points of Josephus' narrative (*War* 1:78–80; 2:562–567; *Ant.* 13:311–313; 18:19), where they appear as teacher-prophets (see also *War* 2:113; *Ant.* 15:370–379; 17:346–348), perhaps living in proximity to the Essene Gate in the city. In *War* 2:140 Josephus notes their humility and honesty in public office. The small, isolationist model of Essenes adopted by Hirschfeld is not borne out by the classical attestations.

Hirschfeld's removal of the scrolls from the relevant archaeological context is a radical de-emphasis of a critical piece of evidence. He notes

that no fragments have been found at the site of Qumran (p.46), and yet – given that he has defined the site of Qumran as encompassing not only the buildings on the plateau but also the associated farm of Ein Feshkha, so that the entire area is one large estate – the scrolls are clearly on site in the artificial marl caves 4, 5, 7, 8, 9 and 10. According to Hirschfeld, these scrolls, which are directly related to others found elsewhere in terms of their content, are also to be included in the quick hiding operation from Jerusalem that apparently accounts for the scrolls' appearance in the estate and its environs. But the quick hiding scenario is not the only one that has been suggested (see Doudna 2006, and Pfann in this issue of the *Bulletin*), and it remains rather hard to know how Jerusalemites would possibly have transported a huge library from the city during the First Revolt. These scrolls may have been stored in the artificial marl caves of Qumran – which are cooler – already as a library. Since these caves are visible from the roadway below the cliffs, it remains most plausible to think that the marl terrace cave finds represent scrolls that had not yet been transported to even more isolated hiding places nearby.

Hirschfeld defines as an essential view of the Qumran-Essene hypothesis that all the scrolls were produced and utilized in Qumran (p.45). This is not so. It has long been recognized that scrolls could have come from other places, but the character of the *yahad* scrolls indicates a cohesive point of view that differs from those currently in charge of the Temple in Jerusalem. As Geza Vermes (1995, xxxi–xxxii) has noted, '[o]ver a dozen manuscripts contain sectarian calendars, yet not one mainstream calendar figures among the 575 compositions found in that cave [4]!' and Vermes asks why Jerusalemites went to the trouble to find caves far away by the Dead Sea when 'equally inaccessible caves could have been found closer to home?'

Furthermore, de Vaux's explanation of the peculiar long tables found in *locus* 30 as being used for laying out long scrolls and writing them (de Vaux 1973: 32) still stands. To recap on this evidence, the room extending southwards from the tower, *locus* 30 had two storeys, and the top storey collapsed at the end of de Vaux's Period II (Hirschfeld's 'Herodian' stratum III). In this debris were discovered structures made of mud-brick covered with plaster (which were taken to Jerusalem and reconstructed to make them into benches and a table 5 m long, 40 cm broad and 50 cm high) as well as fragments of smaller tables. In addition to this two inkwells were found in the debris: one bronze and another pottery, one of which still contained dried ink. De Vaux then identified that this upper room was a *scriptorium*. Hirschfeld considers the plastered tables as benches that were part of the furniture of a *triclinium* (p.95–96). There are no parallels for such reclining benches. As de Vaux (1973: 30) states, the structures would probably not have borne the weight of someone sitting on them. It is also argued by Hirschfeld that the inkwells were part of the debris from the downstairs room rather than upstairs, and inkwells do not necessarily mean

you have scroll-writers. Perhaps, but, added to the two inkwells in the debris of *locus* 30 a further four have now come to light from Qumran, one made of bronze in a private collection (ex Bedouin; see Goranson 1994). This large number of inkwells suggests that some sort of scribal activity was taking place. While it may well be that this scribal activity was administrative, it just so happens that there are no administrative records found on site but rather a large number of fragments of religious writings.

In Hirschfeld's view, Essenes could not be located at Qumran because they lived in the hills above En Gedi (p.232). Pliny, *Hist. Nat.* (5.15.4/73) writes: '*ab occidente litora esseni fugiunt usque qua nocent, gens sola . . . socia palmarum . . . infra hos engada . . . inde masada*', 'in/at the west [of the Dead Sea] the Essenes flee all the way from the shores which hurt, a people alone . . . in the company of palms. . . . Below them is Engedi . . . from there Masada'. Hirschfeld notes: '[b]efore the discovery of the scrolls, there were no doubts among scholars that the Essene settlement should be located in the En-Gedi area' (232, n.82). This is incorrect. Long before the discovery of the Dead Sea scrolls and the excavation of Kh. Qumran, the northwestern sector of the Dead Sea coast was considered to be the region in which a group known as the Essenes lived during the Second Temple Period. For example, Christian D. Ginsberg (1864: 26) wrote in his essay on the Essenes that 'the majority of them settled on the northwest shore of the Dead Sea'. The traveller William Hepworth Dixon (1866: 279–280) noted that the 'chief' seats of this sect [of the Essenes] were pitched on the western shores of the Dead Sea, about the present Ras el Feshka'. No one positively identified the site of Kh. Qumran as Essene on account of the view that it was more modern than Second Temple (see Taylor 2002),[2] but the association between this vicinity and the Essenes was clearly made on the basis of Pliny.

In sum, the archaeological re-assessment by Hirschfeld does not successfully dent the Qumran-Essene hypothesis because even if some of his proposals are accepted, the site remains perfectly suited to being a residence for people (largely adult men) who were committed to ritual purity, community, common meals, and possessions and a fairly simple work ethic. As this site developed – before the Herodian Period – its adaptations make it distinctively different from any parallel cited by Hirschfeld. The location by the Dead Sea where ancient sources place Essenes; Jewish occupancy; communal dining halls; a very large number of pools that are suitable as *miqvaot* (regardless of any other uses); cemeteries with poor burials and a significantly greater number of adult males than adult females and children (if any children should be dated to the time of the settlement); a table and inkwells appropriate for some scribal activity; cylindrical jars that are *identical* to those in which some scrolls are buried in nearby caves; a collection of religious texts – indicating a particular type of legal and scriptural analysis – located in on-site artificial caves (4–5, 7–10); collections of (community) money; a range of agricultural operations – including date propagation and animal

husbandry – and simple industries that would enable self-sufficiency and economic viability; a location away from the bustle of city life yet still connected with transport systems: these elements, *cumulatively* considered, strongly support a hypothesis that would have Essenes *appropriately* living at Qumran in the first century BC to first century AD.

On balance, despite the argument of *Qumran in Context*, the Qumran-Essene hypothesis remains the most plausible of any yet proposed.[3] That many features of the Essene school of thought appear to fit the peculiar theology evidenced in the ancient texts of the Dead Sea scrolls is, in addition, a factor of *archaeology itself* that should not be pushed aside. I continue to support Hirschfeld's book as being the best critique that anyone has yet devised against the Qumran-Essene hypothesis, and any proposal must be able to withstand worthy challenges of highly qualified experts. Nevertheless, as Jean-Baptiste Humbert (2006: 19) has recently stated, although the hypothesis 'has not been irrefutably proven, it nevertheless remains the most likely explanation'.

Notes

1 To Hirschfeld's citations one can also note Diodorus Siculus, *Hist.* 19.99.3; Galen, *De Simpl. Med.* 9.2.10; Josephus, *War* 4. 481, and see Safrai 1994, 187–8.

2 Apart from de Saulcy, who thought the ruins were of Biblical Gomorrah, there was a tendency to think of the site as post Second Temple and military, e.g. Van der Velde (1856, 257): 'The ruins called Ghomran are those of a small fortress which has been built to guard the pass above; and around it, on the E. and S., a few cottages have stood, which probably afforded shelter to the soldiers, the whole having been surrounded by a wall for defence.'

3 See my review of Magness (2002), in *PEQ* 136 (2004), 81–7. While modifying some of de Vaux's arguments, Magness provides a careful defence of the Qumran-Essene hypothesis in terms of the archaeology.

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Book Reviews

A.F. Rainey and **R.S. Notley**, with contributions by **J. Uzziel**, **I. Shai** and **B. Schulz**, *The Sacred Bridge: Carta's Atlas of the Biblical World.* Carta: Jerusalem, 2006. Pp. 448 + 300 maps and illustrations. Price £76.99. ISBN 978-9652207036

This atlas is presented as a successor to *The Macmillan Bible Atlas* by Yohanan Aharoni and Michael Avi-Yonah. According to the Foreword, it was written 'under the conviction that understanding the ecological, social and ideational experience of the ancient peoples on the Land Bridge is a valuable component for comprehending the intellectual and spiritual results of their experience'. Anson Rainey is responsible for the introductory matters and the story from the Bronze Age to the Persian Period. Steven Notley covers the ground from the Hellenistic Period to the time of the Jewish revolts against Rome.

Emphasis throughout is on the written sources, interpreted from the ancient language. A particular attitude to modern critical research into the biblical sources is loudly proclaimed in the Foreword: 'on those [biblical] texts, modern scholars have worked their critical legerdemain. It may be said of them as Anatole France said of a certain historian, "He has enriched us with a new uncertainty".' This attitude, it must be said, seems somewhat at odds with the general employment of critical scholarship throughout the book, but one does detect that the authors have some concern to support the general historicity of the biblical account. Archaeological evidence is taken into account whenever it seemed relevant. The Foreword claims that in this work 'biblical texts are evaluated mainly for their geographical content'. There are indeed some superb analyses of texts with geographical and topographical content, especially in the *excursus* (see below). Sometimes, however, one gets the feeling that Rainey at least is equally concerned to write a history of the 'biblical period' in the manner of a John Bright, with a particular interest in chronological problems. The general focus of this book is totally biblical. It is a very different book from George Adam Smith's *Atlas of the Historical Geography of the Holy Land.*

The work begins with preparatory chapters outlining the disciplines of physical geography, philology, toponymy (a very useful section, pp. 14–20) and archaeology (with the standard critique of the Wheeler-Kenyon system, p. 23, but some useful remarks in 'Linkage and Synthesis', p. 24), the ancient world view, the general geography and topography of the Land Bridge (with a useful discussion of the geographic borders of the land of Canaan, whose southern border is the Wadi el-'Arish, distinct from the

Land of Promise whose southern border is the River of Egypt, the Pelusiac branch of the Nile [pp. 34–35], and whose northern border does not include Alalakh and Ugarit [p. 36]). Pages 36-41 describe the Land of Israel, but here we meet a problem that surfaces throughout the atlas: the text, which is full of references to particular places and topographical details, is not adequately illustrated by the map (p. 38), which omits many of the places mentioned in the text.

Chapter 4 deals with the Early and Intermediate Bronze Ages, noting the absence of epigraphic evidence for the identity of the Intermediate Bronze Age people (p. 47), with maps clearly showing the Chalcolithic, Intermediate Bronze and Early Bronze sites in the southern Levant. Chapter 5 turns to the (Amurrite) Middle Bronze Age (MB I = MB IIA), with which (and not with the Intermediate Age) Rainey links the Sinuhe story. The Dynasty XV Hyksos are firmly identified as Amurrite/Canaanite rulers of foreign city states, and the popular belief that Joseph is to be associated with them denied (p. 60). Excursus 5.1 (p. 58) usefully gives the contents of the Execration Texts, just as in Ch. 6 Excursus 6.1 gives the topographical list of Thutmoses III and Excursus 6.4 the Taanach Letters. These *excursus* sections increase the value of the book to the student, making readily available texts otherwise hard to find, and Rainey is to be commended for providing them.

Chapter 7 gives a detailed account of the Amarna Age (LB II), with maps and texts. An important and extremely useful feature of this book is that historical texts are often given in their original text or (transliterated) language (in light blue print), with translation (dark blue print), and source reference (in red). Excursus 7.2 (pp. 88–89) tackles the problem of the *'Apiru*, who are not to be identified with the Hebrews in Egypt or proto-Israelites (whom Rainey links with quite distinct tribal pastoralists called Sutû in cuneiform or Shâsu in Egyptian texts [see p. 103]). Chapter 8 illustrates the Ramesside Period (LB III), with a section on the Merneptah stele (pp. 99–100), with its reference to the socio-ethnic group (*not* territory) of Israel, and with an Excursus (8.2) on Papyrus Anastasi I.

With Chapter 9 we reach the twelfth century BC and the question of Israel's origins. Rainey argues from recent surveys and excavations that the new immigrants to the Samarian Hills came from Transjordan (pp. 111–12), and a small map shows the most prominent Early Iron Age sites (EI I? or EI I and EI II?), but much more space is given in the following pages to larger maps illustrating the Genesis traditions of Abraham, Isaac and Jacob, though, in the case of Gen. 14, 'no confirmed contacts with the sources of the Bronze or Iron ages have been found' (p. 116). Excursus 9.2 (pp. 116–18, 'a textbook example of how historical geography really works') considers the location of Bethel = Beitin (*en passant* correcting G.S.P. Freeman-Grenville's recent translation of Eusebius on Ai and Bethel) (p. 118). Excursus 9.3 attempts to combine the Exodus and the wilderness wanderings of Num. 33 in one coherent itinerary; the key to Rainey's reconstruction is the location

of Mt. Sinai/Horeb in southern Sinai, and 'the recognition that Israel's encounter with Edom took place on the *western* side of the Arabah' (p. 121), reflecting either the early Shâsu presence or a seventh-sixth century BC Edomite presence in that region. But map and text are not in total agreement, for Abronah, map-located west of Elath, 'cannot be identified' (p. 120), and Ezion-geber is associated in the text with 'Ain Ghadyân. This whole exercise is fraught with difficulty, and, assuming an essential unity of narrative, ignores the problems of the text's historical development. Excursus 9.4 (Conquest traditions) inevitably raises problems. The well-known problem of Heshbon is glossed over with the comment that '[t]here are no Late Bronze remains and the Iron Age levels were badly disturbed.... The results of the excavations again suggest a twelfth-eleventh-century BC date for the tradition of its conquest' (p. 124), which is surely wishful thinking (on this problem see recently S. Timm, '*Gott kommt vom Teman...*' AOAT 314 [2004]). As a general point it might be noted that the biblical construction of the conquest is not made demonstrably more historical by the identification of locations associated with it.

Chapter 10 brings us to the eleventh century BC and the biblical judges. The map of the Neo-Hittite and Aramaean states is useful, though text and map have differing spelling conventions. The maps of the judges' campaigns illustrate the stories, but remain somewhat impressionistic, with large coloured arrows indicating supposed routes (cf, e.g., *The war of Jephthah*, p. 140). In describing campaigns in a particular area and landscape, it would aid accurate comprehension to relate these events to the topography as seen today on modern maps, complete with contours, heights, rivers, roads and settlements. It would also help to give grid coordinates to known sites (as indeed Aharoni did in his *The Land of the Bible*).

Chapter 11, on tenth-century territorial states, begins with Northern Mesopotamia and Syria, but the text (p. 157) mentions numerous places not shown on the map (and vice versa), prompting the question whether the map was drawn from the text or imported from somewhere else. For the whole monarchic period, from David to the Judaean exile, Rainey accepts the evidence of the books of Chronicles as supplementing or correcting that of the Deuteronomistic History, and reconstructs the history from both sources (see the important Excursus 11, pp. 171–74), using Chronicles 'without apology' (p. 174). Also basic to his reconstruction throughout is the chronology of E.R. Thiele (*The Mysterious Numbers of the Hebrew Kings*). At times it seems that Rainey is far more interested in the chronology and the historical narrative than in the geography, and one might be forgiven for comparing this work in genre both with maximalist histories of Israel illustrated by maps and with modern archaeologically minded commentaries on the books of Samuel and Kings. Throughout this whole central section there is an impressive amount of detail, and selectivity in reviewing is inevitable. In Ps. 60:8 Rainey follows the Targum and reads 'Aram' for

'Edom' (p. 161). Excursus 11.2 (pp. 174–79) treats the Solomonic administrative districts in detail, reconstructing the geography with help from the books of Judges and Joshua (cf. p. 185: Rainey sees pericopes in Joshua as deriving 'from the same archival sources that preserved the list of Solomon's commissioners' districts'). Excursus 11.3 (pp. 179–85) examines the tribal border descriptions of the Book of Joshua. (Discussing Ephraim's borders [p. 182], the text gives Wâdî Qanah and Nahr el-'Aujā, while the map shows Kanah Brook, without the Nahr el-'Aujā; map and text here and elsewhere should correspond better.) Excursus 11.4 (pp. 185–89) presents Shoshenq's campaign, dated to 925 BC (*pace* Finkelstein), with the topographical list in full. Biblical historians will be extremely grateful for these sections of the work, which take full note of previous scholarship (though note that the reference to Levi *et al.* 2004 [p. 189] should be to Levy).

Chapter 12 turns to the 'regional conflicts' of the ninth century BC. The discussion of the topography of Baasha's reign in Israel (p. 195, col. 2) would be more intelligible if the associated map were based on the modern map. We also need a date-chart for the kings of Israel and Judah at this point (the one inside the book's back cover is inadequate – for example, it omits Baasha). Page 201 (col. 3, top) assesses evidence from the Chronicler by the comment that '[t]he naming of specific individuals, officials and family members *looks quite authentic*' (emphasis added); on such intuitive judgments is history written! The discussion on p. 202 lacks a map explaining the Ramoth-Gilead campaign. On p. 203, a comment explaining 'ships of Tarshish' would have been helpful. On p. 204 the village of Kerioth is identified in Moab on the same ridge occupied by Ataroth, which seems inaccurate, to judge by the map on p. 203, and there is reference to a strange wadi 'Unhealed' between Baluah and Aroer. There is no sign of Libb (p. 204) on the map of p. 203. On p. 210 we meet the surprising Rejuvenates and Monazites (= Reubenites and Manassites?) from Aroer in the valley of the Arnon. Excursus 12.1 (pp. 211–12) studies the text of the Mesha stele, and Excursus 12.3 that of the Tel Dan inscription.

Chapter 13 moves on to the eighth century, with a helpful map, Excursus (13.2) on the Samaria ostraca, and an Excursus (13.4) on Kuntillet 'Ajrud. Chapter 14 covers the Assyrian Period; in 2 Kgs. 17:4 Rainey translates '[Hoshea] sent messengers to So < to the > king of Egypt', in which 'So' is a toponym = Sais. Pp. 245–46 require a map to illustrate the campaigns of Sennacherib. On pp. 246 and 250, Rainey reconstructs the career of King Manasseh and his relationship with Assyria. Excursus 14.1 explains that the 'Way of the Sea' (Isa. 8:23 MT = 9:1 EVV) has nothing to do with an illusory *Via Maris* supposed to link Mesopotamia and Egypt via Damascus. Excursus 14.2 studies the *lmlk* stamps on royal wine jars, now dated to Hezekiah's preparations for Assyrian invasion in the late eighth century BC; the four places named on the jars represent royal wineries in the hill country of Judah, one town in each of three southern districts, and *mmšt* (Jerusalem?)

in the north. This Excursus cries out for an illustrative map, whose absence is surprising. Excursus14.3 studies the Siloam tunnel inscription. Chapter 15 turns to the final century of the kingdoms of Israel and Judah. The discussion of Jehoiakim's reign (pp. 260–61) is not easy to follow. On p. 270 it is noted that 'Reconciling the Greek and Babylonian sources [for events surrounding the death of Hophra (Apries)] is a difficult task under present circumstances' – what does Rainey mean? The map of Neo-Babylonian conflicts (p. 272) should relate better to the text it purports to illustrate. Where are Cilicia, Hume/Adana, Ura, Seleucia/Silifke, the Göksu river, and so on?

Chapter 16 ('Persian Domination') closes Rainey's contribution. On p. 281 the text states that Poseideion is located just south of the mouth of the Orontes river; the map (p. 280) shows it just north. On pp. 280–83 he presents a confusing attempt to determine the boundaries of Syria, Phoenicia, Philistia and Egypt which would be greatly helped by a purpose-drawn map showing the places named. On p. 286 a map shows the Israelite 'Return to Zion'; this map creates facts, as it were, on the ground in the absence of any hard evidence, and is more illustrative of an idea than of history (compare maps of the journeys of Abraham). It should be compared with the similar map on p. 290, and further questions asked about hard evidence for the route there shown.

With Chapter 17 we turn to the Hellenistic Period, leading via the Hasmonaeans and Herods to the historical geography of the Gospels and early church writings, and the Jewish revolts. These chapters are the work of Professor R. Steven Notley, and are equally impressive, making full use of the evidence of Josephus and other Hellenistic writers. Excursus 17.1 illuminates the urban revolution in the Levant after Alexander the Great with particular reference to architecture. Chapter 18 focuses well on the topography of the Maccabaean campaigns, with a good section on Judas' campaigns in Gilead (again, the identification of places like Raphon, Alema, Bosora, etc., with modern sites on a map with coordinates would be a great improvement), and another good essay on the topographical problems of 1 Macc. 9:2 (p. 315–16). The fourth district of 1 Macc. 11:57 is identified as Ptolemais, not Akrabattene (with Abel and Goldstein) or Peraea (with Avi-Yonah) (p. 320). Chapters 19–21 detail the political geography of the later Hasmonaean and Herodian Periods, including the region of Qumran.

Chapter 22 provides a new and useful historical geography of the Gospels. Notley explains Nazoraios (Mt. 2:23) as from Hebrew *natsori*, 'the one [whom I have] protected', with no relationship to Nazareth (p. 349), finds Bethany beyond Jordan (Jn. 1:28) with Lightfoot and Conder in Batanaea/Bashan (p. 351), sees 'the Sea of Galilee' as an early *Christian* toponym alluding to Isaiah 9:1 (p. 354), rejects the identification of et-Tell with Bethsaida (p. 359), locates the story of the drowning of the swine at el-Kursi (p. 360). (N.B. on p. 360, col. 2, line 24, read Gerasa for Gergesa, an unfortunate error here.) Notley argues that Jesus' strange route in Mark 7:31 owes

much to Mark's knowledge of the Old Testament coupling of Tyre and Sidon (in that order) and his understanding of Jesus as fulfilling the prophecy of Isa. 9:1. Jesus started out for his crucifixion not from the Antonia, but from the praetorium reported by Philo and Josephus to be at Herod's palace on the western hill. Excursus 22.1 (pp. 368–69) demolishes the 'myth of the Essene Quarter' at Jerusalem. This is perhaps the place to note – or have I missed or forgotten something? – that there is no Excursus (surely desirable) on or map of the physical development of the city of Jerusalem and its walls in biblical times.

Chapter 23 provides the familiar details of the travels of St. Paul and the seven churches of the Apocalypse. Plans of cities visited by Paul – e.g. Corinth – could have been usefully incorporated. Chapters 24 and 25 tell the story of the Jewish Revolts of AD 66 and 132. The map on p. 389 is inadequate to illustrate the confusing discussion about Gamala on the same page; we need to see exactly where Tel el-Ahdab, Khirbet es-Salaam, el-Yehudiye, and Solyma are located to make sense of the text. Again, the modern map is necessary for the location of ancient sites. The map of the siege of Bether (p. 399) needs more explanation in the text.

Having worked through some 400 pages of text, in three columns per page, I feel that some comment on matters of presentation is in order. The layout is pleasing to the eye but the print is often too small for comfort, especially for periods of extended study. The use of different print colours is successful, and references leap to the eye when required. The English is sometimes curious, and would not always meet with the approval of H.W. Fowler or even his more liberal successors. There are a number of malapropisms – 'the upstart of all this' for 'upshot' (p. 265), 'magnet' as a translation for the title *rubû* when surely 'magnate' was meant (p. 273), and 'Hypodamic' presumably for 'Hippodamian' (p. 227, col. 3) – come to mind immediately. 'Rejuvenates' and 'Monazites' have been mentioned; are they the result of a computer spelling check? Frequent references to Liddell and Scott of Greek lexicon fame consistently misspell Liddell, even in the bibliography (p. 418, col. 2). On p. 153, col. 2, line 7, for Williams read Williamson. On p. 223, col. 1, *re* Teman, read Knauf 1992b for 1992a. There were too many misspellings for a seriously academic work, not all of which were mere typos; in either case, the sub-editorial eye had slipped. On p. 148, col. 1 there is the strange omission of Mizpah from the text, a blank space remaining. The use of the slang 'moniker' for 'name' on p. 397 surprised me.

This huge atlas is designated as an Atlas of the Biblical World. How far does the biblical world extend? This atlas focuses very closely on the biblical narrative and perspective, and within that on the region of the southern Levant, with occasional forays into Egypt and Syria. What is an atlas? A compilation of maps, to be sure, but what sort of maps are these? These maps limit themselves for the most part to displaying named ancient sites in relation to each other and to the main geographical features. Disappointingly,

the ancient sites are not given modern co-ordinates by which to locate them precisely, and are not set against contoured relief (in map work, the change from drawn contours to photographic impressions of relief is not a universal change for the better). Horizontal and vertical relationships are imprecise at best. The ancient sites are here not shown in their relationship to modern sites and modern place-names, which is unfortunate because in the text they are often so identified. Why is it so impossible to use more detailed modern maps as the base on which to display our knowledge of the past? We could then set our knowledge of past events against our modern experience and knowledge of the land, and see the ancient world and its actors on a more intelligible stage.

In spite of the faults (often minor) and limitations outlined above, it must be said that this atlas is a most valuable work. The breadth of learning and the range of scholarly literature cited in the *References* is indeed impressive. The many *excursus* sections in particular provide most helpful summaries of well-known problems of biblical history and geography. The colour-coded citation of texts in the original language, with transliteration and translation where required, the full provision of source references, and the presentation of toponyms in both Arabic and Hebrew forms for the sake of clarity, demonstrate the effort put into the project by authors, editors and publishers alike. The *References* (pp. 403–32) and *Index* (pp. 433–48) conceal an enormous amount of labour by person or persons unknown; one hopes they were properly rewarded. Anson Rainey and Steven Notley and their editors and publishers deserve our congratulations and thanks on the production of such an informative volume.

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Leen Ritmeyer, *The Quest, Revealing the Temple Mount in Jerusalem*, 2006. Pp. 440, figs. and plates on 399. Carta, Jerusalem and the Lamb Foundation. Price £60. ISBN 978-9652206282

There is no doubt that Leen Ritmeyer is one of the finest illustrators of Israeli archaeology in recent times. His reconstruction sketches of the library at Qumran, his drawings of the finds in the Herodian Jewish Quarter in Jerusalem, his cut-away plans of ancient tombs, of early synagogues and many other finds, are clear, accurate and aesthetically pleasing. Many historical sites in Israel feature copies of his drawings on their explanatory boards and many popular reports of local excavations are illustrated by them.

His drawings of the Temple Mount are particularly good and extensive and give the public what they require, a picture of something that has been lost forever, of a long-lost glory that is still venerated today. It is something that the public hankers after, the glory that was built by Solomon, by

Zerubbabel, and by Herod, and Ritmeyer supplies the visual representation to feed that hankering. It is clear that he has had to work under instruction from the archaeologists that directed each expedition and no doubt in some cases other experts would have seen the remains differently. Some archaeologists would say there was no library at Qumran, but that does not detract from Ritmeyer's reconstruction; he was acting on the evidence given to him by the excavators with whom he was working, and he illustrated their ideas perfectly.

In the case of the Temple Mount, the externals were examined in the excavations of the late Benjamin Mazar and Nahman Avigad, and Ritmeyer worked with both of them illustrating their interpretations. But what of the Temple area itself? Work there was out of bounds, taboo to all excavators, but Ritmeyer has set himself the goal of examining the literary evidence for the three Temples, trying to find archaeological clues and, by putting the results together, determining their location and details. And that is what he has presented in this book.

The first literary clue is the sanctified area of 500 by 500 cubits mentioned in the Mishnaic sources, as recorded in about AD 200. Ritmeyer has usefully examined twelve interpretations of this area by different scholars, working over the last 140 years, and finally comes up with his own suggestion, based on the discovery that the base step of the north-western staircase up to the present podium around the Dome of the Rock is part of a pre-Herodian wall aligned with the outer eastern wall. Thus it neatly forms the north-west corner defining the sacred area.

Although Ritmeyer claims that his is the first reconstruction of the Temple of Solomon based on archaeological evidence (p.279), this step is really one of the few pieces of hard evidence that he can supply for any of the Temples. Two other pieces of evidence are based on his evaluation of the actual Rock and the indentations on it, which are facts on the ground, but not necessarily related to Solomon's Temple or any other. He has done us a service by making a detailed visual survey of the 'Rock' (pp. 251ff.) and has come up with the idea that one particular hollow served as a base to the Ark of the Covenant in Solomon's Temple, and another levelled section was the foundation trench of the wall around the 'Holy of Holies' (p. 266). These finds are hardly conclusive for, as he himself illustrates, the Crusaders made considerable adjustments to the 'Rock' when they converted it to the High Altar of their church.

Although Ritmeyer's sanctified square (the 500 by 500 cubits) appears to be a sensible evaluation, there is one major objection. It is his location, and those of many others, of the 'Holy of Holies' of the Temple on the Rock, the *Even Shetiyah* where, by Hebrew tradition, Abraham nearly sacrificed his son Isaac and where, according to the Moslems, Muhammad arrived and left on his steed El-Buraq. This Rock, the centrepiece of the Dome of the Rock, stands over an ancient hollow, which dates back to the Middle Bronze Age and, according to some scholars, was originally a burial cave. It is certainly

of ancient date, well before the time of David and Solomon, and that would have made it unsuitable for use as the base of the holiest part of the Temple. The Crusaders, when they captured Jerusalem may, as good Christians, have been happy to see their new church based on a venerated tomb, as many cathedrals were, but Jewish tradition would have frowned on that, and even if it was not a tomb, there would be no logic in selecting a site based on a hollow, whether it was tomb, cistern or hiding place. So the location of the 'Holy of Holies' of the Temple on this famous rock is out of order.

The real crux of the whole question of Temple reconstruction is that it has to be based primarily on literary sources. In spite of his claim to be using archaeological evidence, Ritmeyer has to rely on literary sources for nearly all the evidence for the three Temples, those of Solomon, Zerubbabel, and Herod, that he describes in such detail, and frankly Ritmeyer is no expert on the literature.

The literary evidence that he, and others, have to draw on is contained in the Bible, in the Tractate of *Middoth* ('Measurements') of the Mishnah and in the works of Josephus Flavius. Starting from the end, the accounts of buildings by Josephus, in both his *Antiquities* and *Wars*, have generally proved to be correct, but disagree in many fundamentals with the description given in Tractate *Middoth*. Ritmeyer gets over that problem by claiming ('we now know' he states on p. 85) that *Middoth* is describing the Temple Mount before its rebuild by Herod, and Josephus is describing it after Herod. But this is by no means accepted by Hebrew scholars of the Mishnah, for it is unlikely that the authors of the Mishnah knew the earlier Temple Mount well enough to describe it in detail. Not many of them were even alive before the last Temple, that of Herod, was destroyed in 70 AD.

Ritmeyer, and many others, accept the account of the Temple layout in *Middoth* and base the internal arrangements on its description of the courtyards. This makes the area around the ramped altar, the area for slaughtering the animals, and the courtyard of the male Israelites excessively cramped. There would have been little room for the priests to go around and perform all their duties, nowhere for the animals to be held before and after they were slaughtered, and hardly standing room for all the men who had brought sacrifices to watch the proceedings. In fact the writers of *Middoth* were not there at the time nor were they practical men. One could say that they ignored the requirements of space as well as the changes of level imposed by all the steps that they described. It is surprising that so many scholars have based their reconstructions on this work though admittedly there was little else to go on.

When it comes to the Bible, the position is even less clear. The description of the Temple of Solomon in the First Book of Kings clashes with that in the Second Book of Chronicles, and Ritmeyer is prepared to accept both of them, saying that the latter refers to reconstruction works by the later kings. This is possible, but it is difficult to find someone today who would

take either account as architectural evidence. Neither description can be taken as a blueprint, they are both just literary descriptions but Ritmeyer for some reason is prepared to take them at face value. He goes so far as to reproduce in full Chapter 6 of First Kings, describing Solomon's Temple, in the King James's version (pp. 282–83) and tries to give it authenticity by including some words in Hebrew, but he fails to point out the difficulties, such as the obscure description of the windows in verse 4, or how the cedar beams could span the Temple width of 20 cubits.

Even more so, when it comes to the Tabernacle of the Wilderness, which Ritmeyer draws on for some details, it is difficult to argue that such an elaborate construction could have existed in the Sinai. We need no longer claim with Wellhausen that it was a retro-model of the Solomonic Temple; it was more likely based on the later shrines that existed at the Tabernacle at Shiloh or even the Tent of David in Jerusalem, but of course this is all literary conjecture, as nothing has been found of these buildings, and that is why it is surprising that Ritmeyer is prepared to draw on such a source for an archaeological reconstruction of the Solomonic Temple.

On the other hand, Ritmeyer completely ignores the fact that the Bible implies the Temple was an integral part of Solomon's Palace, and in his reconstruction model there is no suggestion that the Palace stood adjacent to the Temple. It is quite likely that, on the north Syrian model, the Temple was part of the palace complex and that it was indeed a temple reserved for royalty before it became one for the people.

He describes the vicissitudes of that Temple up to the time of its destruction by the Babylonians, but has only the biblical sources for reference. This is fair enough but he treats the sources quite uncritically and illustrates some of the main events, such as the Temple renovation by the young Jehoash (Joash), with nineteenth-century illustrations, which are attractive but have no authenticity. Even less sound are the many illustrations that he has taken from the Temple Institute of Jerusalem, whose artists have produced attractive renderings of scenes conjured up by Talmudic imagination as, for instance, the enormous viaduct built to bring the scapegoat and the ashes of the red heifer across the valley between the Temple Mount and the Mount of Olives (pp. 64 and 112). There really is no factual evidence for this extravaganza, as Ritmeyer says himself, so what place do these illustrations have in a serious study?

Ritmeyer even goes so far as to illustrate some of the Temple vessels with the models produced by the Temple Institute, which resemble artifacts of the Victorian era more than any of ancient times.

When it comes to describing the Herodian temple we are all on safer ground, as the external walls of the platform still exist as do many of the gates and other features. Here Ritmeyer is at his best and his illustrations of the intricacies of the Barclay Gate and the double and triple gates are very useful. It is in these small details that Ritmeyer excels and one can even say

it is in these smaller constructions that Herod's architects were at their best. The beautiful ceilings of the double gate, which Ritmeyer rightly says are the first use of a shallow square-sided dome with integral pendentives, are exquisite, though today in a rather parlous state, but his drawings give some idea of their original splendour. It may be worth noting that 1800 years later, Sir John Soane used a similar square shallow dome with panelled rose decoration over the dining room of his house, now the Soane Museum, in London.

Herod built on a vast scale, with vast resources and vast effort and everyone praises the results. But was it really necessary to extend the Temple platform across the Tyropoeon and into the Kidron valleys at such great effort, and was it even aesthetically pleasing? I think not, and I imagine that Herod's architects and engineers would have at first advised against it. The idea of planting a colossal rectangular box on top of a beautiful mountain must be anathema to all lovers of the organic in architecture. What was wrong with additional terraces, even if the Hasmoneans had already started the box idea? Theirs was on a smaller scale and it was unnecessary to repeat their mistake. Ritmeyer has no criticism of Herod's work nor, to be fair, has any other scholar. No one has voiced concern over the dictator's megalomaniac construction, but is it really necessary to constantly gawp at the 40 m high retaining wall, with its admittedly beautiful dressed ashlars, stepping back slightly at each course? The technical achievement is superb, but the aesthetics is awful. Herod's work at Masada and the port of Caesarea also shows him dominating Mother Nature, but in a much more pleasing way.

Ritmeyer seems to have caught some of Herod's megalomania with his reconstruction of the Royal Stoa (p. 94) which is so colossally tall that the human figures hardly rise above the column bases. Who could have built this? Who would want to walk in it?

What is impressive at the Temple Mount was the ability to move the colossal stones of the so-called master-course, weighing up to hundreds of tonnes, and here Ritmeyer unfortunately gives the reader very little guidance, though it is a subject that should have attracted his expertise. He could have referred to the work of Warszawski and Peretz (in *Cathedra 66,* Dec. 1992), who have given a detailed account of the practicalities of building the Herodian Temple, and he seems to have borrowed from their ideas for his picture of tiny oxen drawing stones towards a crane (p. 137). Nor does he credit our knowledge of the lifting devices that have been illustrated to that great early source Vitruvius.

This brings me to the illustrations, which are numerous. The figures by Ritmeyer are first class, but many of them have already appeared elsewhere, and here they are repeated in a smaller version, sometimes not easy to read. Ritmeyer may be a brilliant draughtsman but he is not a brilliant photographer and many of his pictures are unclear and rather dark, though that may be the fault of the printer. Other photographs in general are taken from the

New Encyclopedia of Archaeological Excavations in the Holy Land (1993) and this is a pity as it should have been possible to take more up-to-date ones. The illustrations from the paintings of the nineteenth-century are only sourced to the Carta Archives, and better information might have given them some authenticity. Many illustrations and maps are taken from Dan Bahat's excellent book on Jerusalem, also by Carta, and admittedly many are originals by Ritmeyer, but one wonders why new versions could not have been provided.

The problem with this book is that the author has chosen an impossible subject. None of the researchers of the last 140 years can have any claim to authenticity for the location and details of the Temples, as reliance has to be mainly based on literary sources, which must remain obscure until they can be backed up by facts on the ground. Excavation on the Mount has hardly progressed beyond that already achieved by Charles Warren, some 140 years ago, and today, further excavation is out of the question. Ritmeyer has done us a service in bringing together a detailed review of previous attempts to solve this intractable problem, but his resolution of the Quest is no more satisfactory than that of many others seeking this particular Holy Grail. On the other hand some of his drawings are superb.

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S.S. Brooks, *Saul and the Monarchy: A New Look*, 2005. Pp. 222. Society of Old Testament Study; Ashgate Publishing: Bodmin, Cornwall. Price £50. ISBN 0 7546 5204 1

This book is an adaptation of the Ph.D. dissertation submitted to University College London and published in 1997. The aim of the thesis was to take a 'New Look' at the beginning of the Israelite monarchy and in particular the story of King Saul. The book is intended as a fresh appreciation of the biblical text and the relevant archaeological finds. At the outset of this review it is important to note that one of the main problems of this work is that it does not fulfil its main intention, that of being relevant as a 'New Look'. Such an endeavour would need to contain pertinent new information and this work does not. In fact, it does not offer any substantial evidence beyond the mid-nineties of the last century and most of the information and the articles the author relied on were written before 1996.[1] This is a problem augmented by the fact that it is precisely in the last two decades that archaeological finds from the biblical period have developed research so substantially as to push historical studies of this period into entirely new spheres. This point can be easily represented by considering just one example, that of the low chronology debate which began in 1996. This important debate, which rocked the historical understanding of the substance of the United Monarchy, is not dealt with by Brooks.[2] In the light of this

debate and together with recent research, King Saul can no longer be unequivocally accepted as the founder of the pan-Israelite monarchy. A possible portrayal of King Saul, in the light of the archaeological evidence, is that of a local leader who was able to establish his rule beyond the confines of the Benjaminite territory and thus control the encompassing areas around the tribal domain. This form of rule would be similar to the Canaanite kings in the El-Amarna Period (fourteenth century BC).[3] Thus it can be seen that the discussions regarding the low chronology together with archaeological evidence have such important ramifications on the understanding of the establishment of the Israelite Monarchy that they need to be considered in any discussion concentrating on the early kingship of Israel. It is the lack of this discussion and the lack of such deliberation that casts a shadow of uncertainty on the substance (pp. xiii) and the title of the project declared to be a 'New Look'.

The first chapter of this book introduces the reader to a general discussion of the various approaches to biblical history. At the outset of this chapter the author states her aim: to present a point of view that counters those of the 'minimalists', those that reject the Hebrew Bible as a historical source (pp. 8–10). The author discusses the literary genre of biblical historiography and states the main problems of considering the Bible as a source of history (pp. 5–8). Brooks rightly points out that one of the problems for a biblical historian is the gap between the time that events actually occurred and when they were finally written down (p. 5). However, Brooks points out that the lack of historical details in the biblical text is the result of the biblical author's lack of concern for factual history. This was, in her opinion, due to the biblical author's interests being purely in ideological matters (p. 7). What Brooks does not take into consideration is that the lack of historical details may quite simply be the result of the author's absence of sources and gaps in his knowledge. This conclusion contains a much more problematic premise which leads Brooks' research into the supposition that the biblical author had at his disposal a varied supply of reliable information but used only part of it, thus manipulating his sources selectively because of lack of interest in pure historical reporting (p. 8). At the end of this chapter Brooks presents her main theory, which is the result of her analysis of the Book of Samuel. She comes to the conclusion that this book is intended to vindicate David from all blame as to Saul's death and above all to justify David's kingship (pp. 10–19).

The second chapter presents the historical background of the epoch in which Saul lived. This is set in the transitional time from the Late Bronze Age to the Iron Age I Period. This chapter contains in part a discussion of the appearance of the Philistines on the Canaanite coast at the height of the Bronze Age (pp. 23–34); and in part the appearance of the Israelite tribes in the central highlands of Canaan. Here the social and economic developments which fostered the birth of the institution of the monarchy in the central highlands are presented (pp. 32–40).The lack of current research, archaeological

and historical, is painfully obvious here. Current research debates the appearance of the Philistines and various aspects of the development of the Israelite kingdom in the central highlands on the basis of patrimonial 'tribal kingdoms' (chiefdoms) becoming organized kingdoms.[4] Also missing are the archaeological updates of the Philistine cities of the Judean foothills which have a crucial importance for an understanding of the political and social developments of the Canaanite Iron Age I Period. Although Brooks realizes the importance of these developments, the recent excavations of the other Philistine cities are nowhere to be found. For Brooks, Ashdod is the only Philistine capital she knows to have been excavated (p. 31) and thus important information from the excavations at Ekron and Gath is lacking.[5]

The third chapter concentrates on 1 Samuel and deals with the Deuteronomist and his account of Saul. The professed aim here is to show that the sins of Saul are those ascribed to him by the author/authors of 1 Samuel (pp. xv, 41–47). According to Brooks most of the biblical commentators fail to understand Saul's kingship because they were influenced by the negative biblical portrayal of this king (p. 34). After a short summary of the main sources of the Book of Samuel (pp. 43–47), Brooks deduces that the presentation of Saul as a negative character is the result of three main sources: that coming from the 'court of David', that of the prophetic circles as they opposed the monarchy on principle, and finally anti-Israelite propaganda that came after the schism of the United Monarchy. It is these conclusions that then become the basis for the analysis of these sources about King Saul (pp. 47–67). Brooks' conclusion, on the basis of her tripartite dissection of Samuel, is that Saul did not commit any sin that would justify such condemnation. Rather, 'the Deuteronomist discredited Saul by portraying his strength and virtues as madness and failure (i.e. 1 Sam. 16 when the victory over Amalek is reported as a sinful episode). Second, the figure of Saul as the first king was used by later Prophetic circles, to emphasize that the power of god (or of the prophet) is more important than that of the monarch' (p. 67).

The fourth chapter discusses the negative presentation of Saul in the light of his relationship to David. Here Brooks analyses the literary unit dealing with 'David's rise to power' (1 Sam. 16:14– 2 Sam. 5:5). In the light of Brooks' diagnosis, that this unit is originally apologetic, her conclusions are extremely far reaching. In Brooks' opinion Saul was a popular leader while David was an outlaw (p. 71). Casting Saul as mentally unstable was in fact a way of acquitting David of subversive activity while Saul's reaction to David's sedition was in fact logical: Saul was simply trying to protect himself when David openly rebelled against him, caused quarrels within his family and went out to war on his own private campaign (against Amalek) without the King's permission (p. 72). After the analysis of 1 Sam. 24, 26, 29, 30 and 31 (pp. 72–82) Brooks concludes that David is guilty of Saul's death on Gilboa: David refused to be 'discharged' from Achish King of Gath's army (1 Sam. 29:8). This accusation is substantiated with an explanation for of why only 400 out of David's 600

men took part in the raid on the destroyers of Ziklag (1 Sam 30:10): David had slipped away with 200 of his men to go north and fight with the Philistines against Saul. David was going to ambush Saul on Gilboa (p. 76). The topographical analysis of the battleground on Gilboa (p. 77) shows that Saul was apparently killed by a hail of arrows that were fired from the mountain above and not from the valley below. This in Brooks' opinion could only be due to David's ambush, and if the arrows were from David, then he killed Saul. In order to support this reconstruction Brooks maintains that Saul's plea for mercy from David (1 Sam 24:21) should be placed at Gilboa and not as it is at Ein Gedi. Moreover the young Amalekite's confession of the murder of Saul (2 Sam 1:10) is in Brooks' opinion a later addition, whereas in the original story David does not continue the interrogation of the Amalekite as he did not want the truth about Saul's death to be known (p. 77). Concluding this chapter is a comparison between the story of David and Saul and the story of Samson and Delilah (pp. 81–87). From the similarities Brooks assumes that the story of Samson and Delilah was written by a pro-Saulian author whose aim was to show David as a traitor in the same way as Delilah was a traitor for the Philistines. It is worth noting that this conclusion is not based on any factual evidence but simply on the similarity of the plot's narrative in both stories.

In the fifth chapter, relying on her axiom that Saul's kingship was neglected by research (p. 89), Brooks offers a presentation of the 'real Saul' based on her former analysis of the biblical stories from the preceding chapters. After examining the various stories regarding Saul, Brooks' conclusion is that Saul was a successful king, who was able to form an army, to defeat Israel's neighbouring enemies and to create the first Israelite Monarchy in the central highlands of Canaan (p. 118). However, this is not a new look, but the view held by quite a few biblical scholars.[6]

The sixth chapter discusses the archaeological discoveries connected to King Saul (primarily the excavations of Tel el-Ful identified as 'Givat Shaul') which she uses to verify her historical and chronological conclusions. Brooks discusses the identification of 'Givat Shaul' (pp. 122–26) and after that she surveys the excavations (pp. 126–35). Brooks' main innovation in this discussion is the contribution of new chronological dates for the various strata of this site. These differ from Albright and Lapp who dated the final layer of the site to the latter part of the eleventh, even possibly the early part of the tenth, century BC, while Brooks dates this level to an earlier period of mid-eleventh century (p. 140). In this way Brooks manoeuvres the dates of the site to suit her theory of the era of David and Saul. As stated above Brooks' archaeological and chronological discussions do not take into account the raging debate about the chronology of the Iron Age.

The seventh chapter searches for the reasons behind the schism of the United Monarchy in the days of David's grandson Rehoboam and after the death of his father Solomon. Here Brooks examines those traditions in the Second Book of

Samuel associated with David (especially the wars of David and Joab against Abner and Ishboshet, the rebellions against David and the accession of Solomon in 1 Kings 1–2). Her assumptions, which differ from the biblical conclusions, are that the seeds of the schism began in the days of David. David in fact sabotaged the work of unification undertaken by Saul (p. 178).

The main problem with this study is (and this is apart from her lack of information on the recent discoveries in research but may well be the result of that) the lack of a systematically methodological approach to historical discussions and her selective use of the sources. When reading this book it becomes apparent that Brooks has set for herself the aim of exonerating Saul. This book is her opportunity to vindicate his damaged reputation and in fact the author herself says as much several times during her book. It sometimes seems that the redemption of Saul's damaged image may have caused a lack of objective appraisal of the various sources. Unfortunately, as Brooks' declared purpose is to exonerate Saul it seems that the only way this could be achieved successfully was by simultaneously slandering David. Slandering David is as important a theme in Brooks' work as that of exonerating Saul. Certainly David is represented as the villain of her work; he is to blame for the schism of the United Monarchy, which according to Brooks '*started off well. It was only David's competition to the throne that sabotaged it and brought to an end Saul's successful reign. Neither David nor Solomon could continue the work of unification started by Saul*' (pp. 177–8).

Actually, Brooks seems in general to regard the biblical narrative as historical reality. The characters of David and Saul, their rise and fall, the unification or division: all are considered quite simply as facts of history that need no proof. In other words, Brooks' reading of the Bible is not critical. For instance, Brooks totally accepts the existence of Edomite, Moabite and Ammonite nations as political entities that existed already in the period of Iron I and this despite the very clear archaeological evidence that these nations could not have appeared before the period of Iron II (pp. 32, 114). Brooks accepts without question the biblical description in Judges and Samuel that suggests that the tribal communities had similar identities and therefore only needed someone to come along and unite them in order to form a nation, and Brooks equally accepts that Saul was that person (pp. 35–40, 71, 109–10, 118). It is therefore not surprising that in her treatment of the area Saul ruled, Brooks is sure that this was a kingship over all the tribes, despite the fact that most scholars today understand Saul's kingdom as encompassing only the tribal area of Benjamin.[7]

Occasionally descriptions that are clearly literary innovations employed by the biblical author for narrative purposes are used by Brooks as factual evidence to reach historical conclusions (for instance, when the biblical narrator describes Saul's fear (1 Sam. 28:5) before the battle with the Philistines, Brooks accepts this as evidence that Saul was not the perpetrator of the war with the Philistines, p. 111). For Brooks it often seems that the fact

that something is described in biblical narrative as 'having happened' is proof of its historicity. If the Bible indicates in 1 Sam. 28:1 that it was not an Israelite initiative to go to war, then 'if the Bible said so', that is proof enough (p. 111). See also her attitude to how lame Mephibosheth became an invalid (p. 148).

However, in this 'belief in the written word as fact' Brooks is not constant. There are times when this author does not hesitate to disregard the biblical account totally and maintain that something is written purposely to fulfil a desired aim. Thus, for example, when Saul dies on the slopes of Gilboa, Brooks nullifies any historical reality that David was with the Amalekites on the basis of 1 Sam. 15, which indicates that Saul had annihilated the Amalekites. This approach disregards the fact that some biblical scholars consider these verses as a late intrusion into the Book of Samuel.[8] Second Samuel 30 was written, according to Brooks, in order to provide David with an alibi for the murder of Saul. How can the inconsistent disparity between historicity and invention be accounted for? Why, when it comes to an account of Saul, is it 'historic fact' and when the story pertains to David is it an 'invention'? Based on the inconsistent analysis of the biblical testimony, she comes to some far-reaching historical conclusions. These are based on the author's own reasoning and not on hard evidence. That is how David suddenly stood at the head of 200 of his people at Gilboa killing Saul. Despite the ingenuity of Brooks' logic, her judgment alone is simply not enough. If an event is created in order to explain a theoretical conviction, it must be stated as such. Many modern scholars assume that David probably was involved with Saul's death, but to state how exactly he was involved cannot be more then a supposition.[9]

In conclusion this study is problematic and does not greatly contribute to the research of the early Israelite monarchy. It is outdated and can even be considered a burden on those searching for the 'New Look'. As far as her biblical research goes, methodological problems further complicate the issues since deductions are made from literary accounts. The suppositions of this work as to what, if anything, went on between David and Saul remain just that, suppositions.

It is worth noting that most of the information about King Saul is literary and was heavily edited by the Deuteronomist. It is the very lack of concrete information from extra-biblical sources, combined with the fact that Iron I is an intermediary period in archaeological terms, that allow scholars a free hand in creating the character of King Saul. Thus Saul has become for some the charismatic leader similar to the heroes of the Book of Judges (Liverani 2005: 88–91) while for others he is the 'last Labayu' who bravely fought the Egyptian King Shishak (Finkelstein 2006). Finally we are left with the predicament that far more is concealed about Saul than revealed. This is exactly why historical conclusions regarding Saul must be carefully considered while acknowledging the problem of sources and distinguishing between historical conclusions and suppositions.

Acknowledgements

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Notes

1 The only publications following 1997, i.e. after Shalom Brooks' doctoral thesis, are: Astour 1999; Whitelam 2000; Finkelstein and Silberman 2001; Gibson 2001; Shalom Brooks 2001–2002; Dever 2003; Kitchen 2003. These publications, however, do not deal specifically with King Saul; rather their main theme is the ethnic origins of the Israelite settlement in the hill area. Kitchen's article on the credibility of the Bible was written as an answer to the minimalists. In addition to the above mentioned, in the list of bibliographical abbreviations can be found a number of small items from the years 1996–97, all of which deal with subject of the reliability of the Bible as opposed to the minimalist's school (Hurvitz 1997; Shanks 1997).

2 See Finkelstein 1996; 1998; Mazar 1997.

3 See for example Na'aman 2002: 106–8, 110; Liverani 2005: 88–91; Finkelstein 2006; Finkelstein and Silberman 2006: 31–91.

4 See Finkelstein 2003; Finkelstein and Na'aman 2005; Bunimovitz and Lederman 2001; Lehman 2003. For the latest updates on the Philistines and their culture see Ehrlich 1996; Oren 2000.

5 For the excavations of Tel Mikne/Ekron see Meehl, Dothan and Gitin 2006 and the relevant books mentioned there; for the excavation of Tel es-Safi/Gath see Maeir 2003, Maeir 2004 and the relevant bibliography. For the importance of the Philistine cities of the Judean foothills in the development of the early Israelite monarchy see Bunimovitz and Lederman 2006.

6 See for example the works of Tsevat 1967, 1979 or Malamat 1983 on whom Brooks relied in her research.

7 See for example Liverani 2005: 88–91; Finkelstein 2006. The view of a greater Israel from Dan to Beer Sheba is considered a later Deuteronomistic idea.

8 See for example Foresti 1984.

9 cf. Halpern 2001: 78–81.

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Summaries of Lectures

Religion in Israel in the Period of the Judges

Garth Gilmour

Recent archaeological research has revealed that the Early Iron Age (*c.* 1200–1000 BC) in Palestine witnessed a dramatic increase in settlement and population in the central hill country as farmsteads, small villages and towns were rapidly established in hitherto sparsely settled areas. The origins and identity of the new settlers have been the subject of debate. Some scholars, such as Israel Finkelstein, have suggested that they are local pastoralists and nomads who settled down in response to the changed political and social environment, while others, like William G. Dever, insist that they are more eclectic, incorporating nomads, survivors from the collapsed Canaanite cities and other elements living in the region.

A study of the archaeology of religion in these settlements reveals little uniformity in religious practice, and a variety of influences, both local and foreign, clearly demonstrates that the newcomers are much more diverse in their origins and traditions than formerly proposed. Their religious behaviour appears to be restricted to domestic, workshop and open-air sites, with temples declining in use and eventually disappearing. The absence of temples reflects the absence of centralized authority, both political and religious, in the region during this period, which is in marked contrast to the Bronze Age Canaanite culture that it replaced and the Israelite monarchy that succeeded it. This privatization of worship and ritual in the biblical period of the Judges is also evidenced in the artifacts, both in type and distribution, which display a marked individuality absent in previous and subsequent periods. The results of this study of religion support the spatial archaeological research that has been undertaken in the highlands of Israel in recent years, casting further light on the dramatic collapse of urban society and power vacuum that followed the decline of the Canaanite civilization, a situation that was only redressed with the rise of the United Monarchy in Israel in the tenth century BC.

This research has implications too for the identity of the biblical 'Israelites', who can now be considered to be but one of a number of peoples or groups that moved into the region at the time of the Canaanite collapse at the end of the Bronze Age. The conservative nature of religion and its cultural longevity, even in the face of severe assimilation, render it a useful indicator of cultural origins and roots, and the archaeological study of religion in this period confirms not only the wide variety of religious expression but also the diverse origins of these religious practices.

Is it Possible to Write a History of Ancient Israel?

Siam Bhayro

It should be possible to write the history of ancient Israel. The Land of Israel has been excavated more than any other area on earth and scholarly enquiry into the peoples, customs and languages of the region has been a constant feature of the Western Academy. And yet, even the

existence of ancient Israel has been called into question. Every piece of evidence is scrutinized, and every interpretation is bitterly contested in an increasingly politicized approach to the subject. So have we now reached the point of no return? Can a history of Ancient Israel be written?

Sex and Ladders in the Monastic Deserts of Late Antique Egypt and Palestine

Claudine Dauphin

'*Imagine two ladders: one leading up to heaven, and the other down to Hades, and then imagine yourself standing on earth between the two ladders*' (Dorotheus of Gaza, *Mystic Treatises* 245).

Tracing in this lecture the ascetic struggle against the demons (embodied by snakes, horned vipers, hyenas, chacals and lurid women personifying earthly and sinful desires) in the monastic deserts of Egypt and Palestine between the third and seventh centuries AD, in this lecture Claudine Dauphin investigated the links between food and sex, against the background of the monks' ultimate goal: to wear out the body and dominate the temptations of the senses (*apatheia*), attain mental impassibility (*hesychia*), and, reaching the top of the 'divine ladder', commune mystically with God.

The Archaeology of Biblical-Talmudic Medicine

Mark Geller

This lecture concentrated on medicine contemporary with the Bible, from Mesopotamia, which provides the most complete evidence of medical and healing practices of that time. Passing reference was made to Talmudic medicine, which also reflects earlier Mesopotamian medicine. The evidence presented was from actual cuneiform medical tablets and iconography.

Love Poems, Schooldays and the Alphabet that Never Was: Egyptian Influences on the Hebrew Monarchy

John Ray

The first part of this lecture recalled the passages in the Old Testament which are generally agreed to show the influence of Egyptian literature. These include Psalm 104, with its strong echoes of the Great Hymn to the Aten (mid-fourteenth century BC), Proverbs 22–24, which show a strong affinity with passages from the thirteenth-century BC Wisdom of Amenemope, and the Song of Songs, which has many points in common with Egyptian love poetry of a similar date. Transmission of these ideas may well have been indirect rather than direct, but transmission of some sort has clearly occurred. There are other cases in ancient literature where the influence of Egypt is clear, even if the route of that influence cannot be traced. One analogy is the strong resemblances between Egyptian love poetry and the work of poets such as Theocritus in later Alexandria. Here too there must be influence at work, even though we do not know how it came about.

The second part of the lecture described the unusual alphabetic scheme which appears in several Egyptian texts from the demotic period (latter half of the first millennium BC). This starts with the letters HLḪ, rather than the ABG or ABC which is familiar to us. This alternative alphabet has now been recognized as the order which appears in South Arabian texts, and which is also known in Ethiopic, and it has therefore been argued that the concept was borrowed by the Egyptians from Arabia. However, this scheme is far older than the South Arabian script, since it has now been recognized on a tablet from Ugarit and on a near-contemporary tablet from

Beth Shemesh. The most likely explanation for this, according to the speaker, is that the HLÓ alphabet originated in Egypt. If the Egyptians had waited until the fifth century BC before adopting an alphabetic scheme, they could easily have adopted the ABG-order which was familiar to them from the Phoenician, Aramaic and Greek scripts. Maritime trade will have taken the Egyptian alphabet to Ugarit and Beth Shemesh, since the latter is not far inland, and along the Red Sea to the Yemen. The Egyptian scheme might have originated in a hymn to the ibis, the sacred bird of the god of writing, since the word for ibis in Egyptian began with the letter *h*.

Samaria: Royal Citadel of the Kings of Israel

Rupert L. Chapman

Samaria was not only the capital of the kings of Israel from 880 to 721 BC but also the first palace complex of the two Hebrew kingdoms to be excavated, in two pioneering campaigns, the first by Harvard University and the second by the Palestine Exploration Fund and the British School of Archaeology in Jerusalem. Study of this royal complex has revealed a great deal about the art and architecture of the Hebrew kings, and given us a comparative standard by which to judge them against their neighbours in Phoenicia and Western Syria, as well as the biblical accounts of the opulence of the kings of Israel. With the excavation of the later royal complex at Ramat Rahel, it continues to provide insights into royal administration and material for the study of the chronology of the Hebrew kingdoms. Samaria was also an important site during the Assyrian and Babylonian Empires and was rebuilt as a showpiece city by Herod the Great, as part of his grand project for the glorification of Israel. Partly due to these later periods of major construction, less survives of the Israelite levels than on many other sites; however, these levels are important in their own right.

The Identification of Qadesh

Jonathan Tubb

In April 1881, Claude Conder visited the region of Homs in Syria in an attempt to identify the site of Qadesh, where the great battle between the Egyptian pharaoh Ramses II and the Hittite king, Muwatallis took place in 1289 BC. The event is depicted and described in detail on reliefs at Ramses's mortuary temple (the Rameseum) at Thebes, and also at Luxor, Karnak, Abydos and Abu Simbel. Conder, informed by the Egyptian pictorial representations of the site, which show a fortified city surrounded by water, considered two sites as suitable candidates, Tel Nebi Mend, which is partly encircled by the Orontes river on one side and its tributary, the Wadi et-Tannur, on the other, and Tel et-Tin situated within the present Lake of Homs. Rejecting the latter on the basis of its smaller size and its isolation from the shore, Conder chose Tel Nebi Mend as ancient Qadesh, and this identification gained general acceptance. Indeed, confirmation appeared to have been provided by subsequent excavations at the site. Maurice Pezard's 1921–22 excavations uncovered a stela of Seti I, indicating the importance of the site, and Peter Parr's excavations (1975–96) produced tablets addressed to Niqmadu, king of Kinza (Qadesh).

Largely overlooked, however, are the excavations undertaken by Joseph-Etienne Gautier in 1895 at the 'rejected' site of Tel et-Tin in the Lake of Homs. His meticulously conducted work was truly outstanding for its day, a model of scientifically objective research. Beginning with the hypothesis that, contrary to Conder's belief, Qadesh might be identified with Tel et-Tin, he set about testing it through well-controlled excavation. His analysis of the results led him ultimately

to reject his initial thesis and to accept instead Conder's identification. With the benefit of more recent information, however, an examination of Gautier's report suggests that he might have been too hasty in rejecting his initial hypothesis.

Human and Divine Attendants at the Royal Courts of Assyria and the Near East

Paul Collins

Beginning in the ninth century BC, the small kingdom of Assyria in northern Iraq began to reassert its authority across the steppe lands of Syria and gradually, over two centuries, a series of able kings created an empire stretching from Egypt to Iran. The palaces of these rulers, built at successive royal centres of Nimrud, Khorsabad and Nineveh, were elaborately decorated with images of kingship. The most spectacular form of decoration was the huge slabs of carved alabaster that adorned the walls of important rooms and courtyards. Among the images frequently found carved in relief at Nimrud are various depictions of the king attended by both beardless officials and protective spirits. This talk investigated the meaning of such imagery, found not only in the reliefs but also in Assyrian wall paintings and, on a smaller scale, in cylinder seals, metalwork, terracottas and carved ivories and explored its relationship with similar imagery known from Syria and the Levant, especially in Phoenician art and descriptions in the Old Testament of cherubim. By the height of the Assyrian empire such imagery became less important, perhaps reflecting a growing interest in representing a more naturalistic world as well as the need for images of kingship which were meaningful to all the people encompassed by the empire. Nonetheless, the courtiers of the king, whether human or divine, remained important symbols of power under the greatest of the Near Eastern empires, that of the Achaemenid Persians whose power stretched from Libya to India.

Copper-based Metalwork from the Jewish Quarter Excavations in the Old City, Jerusalem

Matthew J. Ponting

This lecture presented an overview of recently conducted analytical work on finds from the excavations of the Jewish Quarter of the Old City area of Jerusalem. The material studied comes from a number of different contexts including closely dated Herodian domestic structures. The study uses chemical analysis of the artifacts to identify the alloy types used and the trace contaminants present. This information is then used to investigate changes and innovation in the alloys selected for different purposes and other technological considerations. The trends identified were discussed in relation to other data from the region and with broader archaeological issues.

Sheba to Gashmu: Ancient Arabia as Background to the Hebrew Bible

Kenneth A. Kitchen

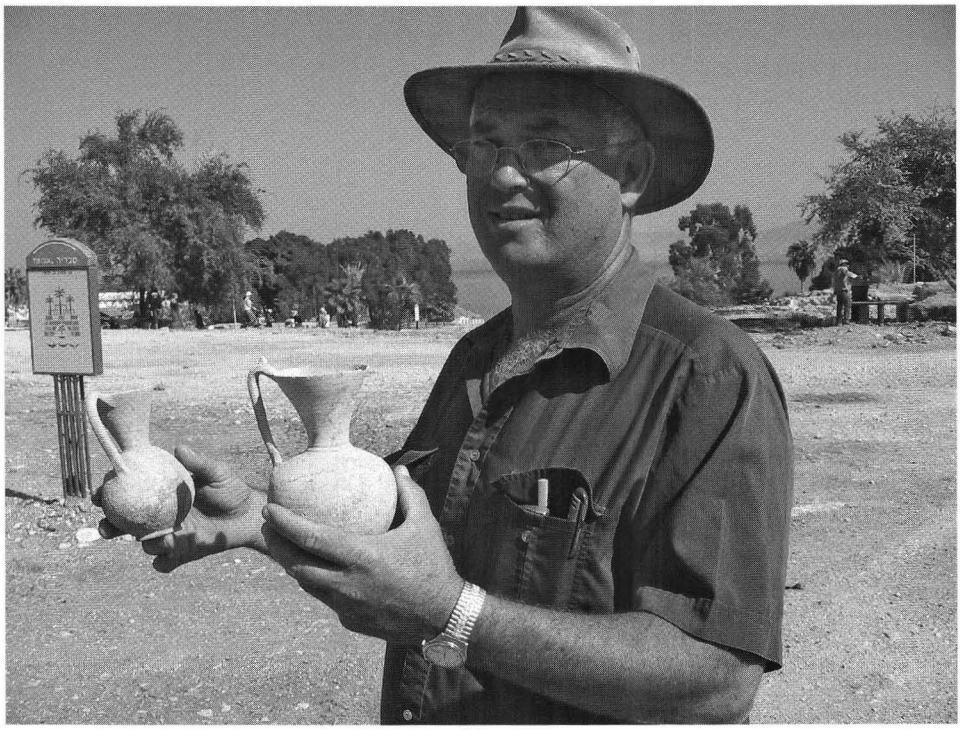
In pre-classical antiquity, there were four 'Arabias' – (1) Eastern Arabia (along the Gulf) with the Dilmun and Magan civilizations, (2) SW Arabia, famous for the kingdoms of Saba (Sheba), Qataban, Main and Hadramaut, and (3) NW Arabia centred on Al-Ula and other oases, with the smaller kingdoms of, for example, Qedar and Lihyan, – besides (4) the great north–south stretch of Syro-Arabian desert that separated (1) in the east from (2) and (3) in the west.

In the Hebrew Bible, we find links with Midian and Qedar in the NW, and with Sheba in the SW, all the way from Solomon (most famously the queen of Sheba) down to Nehemiah (versus Gashmu). The

archaeological and inscriptional discoveries of the last 130 years (not least since the 1970s) enable us to appreciate the variety and richness of these ancient civilizations both in their own right and as illuminating background to the scatter of Biblical references to the exotic worlds of Sheba, Hadramaut and others, and to the traditions of Arabian wealth in gold, and aromatics such as incense and myrrh.

Obituary

YIZHAR HIRSCHFELD (1950–2006)



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Yizhar was born in Kibbutz Keshet and from a very early age had a great interest in the archaeology and history of the land of Israel. His formal studies were undertaken at the Institute of Archaeology in the Hebrew University, Jerusalem, where he later taught. His MA thesis was on traditional housing in the Hebron Hills, later to become the subject of a major book dealing also with Roman and Byzantine dwellings in Palestine.

Yizhar was a man with a zest for life, a scholar with a broad mind and a thirst for knowledge, always in pursuit of the ultimate archaeological discovery. He

conducted numerous excavations and surveys, and loved being in the field. His first major excavation was of a monumental bath-house at Hammath Gader. This was followed by excavations at Khirbet ed-Deir, Ramat ha-Nadiv, En Gedi and Tiberias, and surveys of Byzantine monasteries in the Judean Desert. He once told me that he was a fan of Derwas J. Chitty, who wrote about desert monasticism in his influential book *The Desert a City*.

Yizhar was extremely prolific, producing many books and articles, in Hebrew and in English; I was constantly surprised by the fact that all of this scholarship was produced notwithstanding the fact that his writing was done long-hand and without the use of a computer. He was never ever afraid of putting forward a controversial idea (for example that Essenes lived at En Gedi rather than at Qumran); indeed he positively enjoyed doing so, and was frequently outspoken in a quiet and distinguished way. Some of his ideas and archaeological finds are extremely significant and will undoubtedly eventually show him to have been a pivotal person in the development of Israeli archaeology over the past three decades.

In addition to his professional achievements Yizhar was a kind and discerning individual, who loved food and travel, and had a wonderful sense of humour. Yizhar was struck down well before his time; I shall miss him greatly.

Shimon Gibson

Grants Given by the Society

CHANGYOP LEE

In Korea Biblical Studies is mainly a study of texts. There is little done to introduce students to the ancient culture and real life of the people of Israel and Judah, or its neighbours. I am an ordained minister and have always wanted to visit the Holy Land but have never had the opportunity or funding to do so. I felt I would benefit tremendously from experiencing the excavation process first-hand, so that when I complete my studies at Sheffield University and return home, I can speak with confidence about the archaeology and culture in both an academic and church setting. My sermons and teaching would be enhanced by my being able to include personal impressions of sites and objects. The Anglo-Israel Archaeological Society Travel Grant made my dream come true this summer, so I could take part in Tell es-Safi/ Gath Archaeological project. I chose this site because my supervisor, Dr. Diana Edelman, is a staff member in the Department. of Biblical Studies at Sheffield.

According to my proposed project, I spent three weeks in Israel. One week I visited Jerusalem and included visits to the Israel Museum and the Rockefeller Museum, the Mount of Olives, and the Old City.

I spent the last two weeks excavating at Tel es-Safi (from 8 to 21 July 2007). The digging itself was not easy for me because it needs patience under the scorching sunshine to find artifacts. The best thing I learnt from this summer season in Israel was how to record the finds in as careful a manner as possible, such as measuring heights and keeping a daily graphic diary, because without ongoing documentation, the final report cannot be published. The extensive late ninth- or early eighth-century BC destruction level in Area A continued to be exposed in the 2004 season of excavations. This level is most probably connected to the capture and subsequent destruction of Philistine Gath by Hazael, king of Aram Damascus (2 Kgs 12: 18). Dr. Edelman's explanation about the process of the destruction level was very good.

During two Saturdays I visited major excavation sites, such as Lachish, Tel Beersheva, Tel Arad, and the Dead Sea. On the last day before my return to Sheffield, I heard news from Revadim kibbutz (base camp) that an extremely interesting cache of cultic vessels had been found in the destruction debris, including several phallic-shaped vessels, pomegranate-shaped vessels, rhyta,

zoomorphic vessels and other objects. It was said that it was a unique find, coming from the ninth-century BC destruction level in Area A.

I think my short trip to Israel and experience in the field was just one step in the long journey of learning. I cannot thank the Anglo-Israel Archaeological Society enough for awarding me a student travel grant.

Errata

Y. Stepansky, 'Rock-hewn Channels near Tel Hazor: Evidence of Middle Bronze Age Long-Distance Water-Carrier', *BAIAS* 24 (2006), 51–76:

1. P. 53, line 3: instead of – '(Figs 1–4)', it should be '(Fig. 2:1–4)'.
2. P. 53, bottom line: instead of – '(Figs 2,4)', it should be '(Fig. 2:4)'.
3. P. 54, line 2: instead of – '(Fig. 4)', it should be '(Fig. 3)'.
4. P. 54, line 8: instead of – '(Figs 2,3)', it should be '(Fig. 2: 2, 3)'.
5. P. 55, figure 5 caption: instead of – '(1992; view towards the east)', it should be '(1992; view towards the west)'.
6. P. 55, line 6: instead of – '(Figs 2–4)', it should be '(Fig. 2: I–V)'.
7. P. 55, line 14: erase the word '(vessels)'.
8. P. 59, line 1: instead of – '241.22–241', it should be '241.22–244.23'
9. P. 59, line 3: erase the word 'point'.
10. P. 59, line 19: instead of – '(Figs 2,4)', it should be '(Fig. 2:4)'.
11. P. 59, line 36: instead of – '(Figs 2,5)', it should be '(Fig. 2:5)'.
12. P. 59, line 42 (3rd line from the bottom): instead of – 'only bedrock did not exhibit any evidence of cutting', it should be: 'only bedrock that did not exhibit any evidence of cutting was found'.
13. P. 61, Figure 11 caption: instead of – '(view towards the west)', it should be '(view towards the east)'.
14. P. 62, Figure 12 caption: instead of – '(view towards the east)', it should be '(view towards the west)'.
15. P. 66, line 14: instead of – '(Figs 1,2)', it should be '(Fig. 1:2)'.
16. P. 66, line 18: instead of – '(Figs 1,2)', it should be '(Fig. 1: 2 a–b)'.
17. P. 67, line 36 (6th line from the bottom): instead of '(Figs 1,3)', it should be '(Fig. 1:3)'.
18. P. 68, line 13: instead of – '(mainly winepresses of different type date)', it should be '(mainly winepresses of different type and date)'.
19. P. 69, line 27: instead of – 'thousands of metres', it should be: 'tens and even hundreds of metres'.
20. P. 70, line 27: instead of – 'a hewn 1km tunnel', it should be: 'hewn tunnel 1000'.
21. P. 73, note 5: instead of – 'Middle Bronze Age I (2200–1950 BCE), Middle Bronze Age II (1950 – 1550 BCE)', it should be: 'Intermediate Bronze Age (2300–2000 BCE), Middle Bronze Age II (2000 – 1550 BCE)'.

Notes for Contributors

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