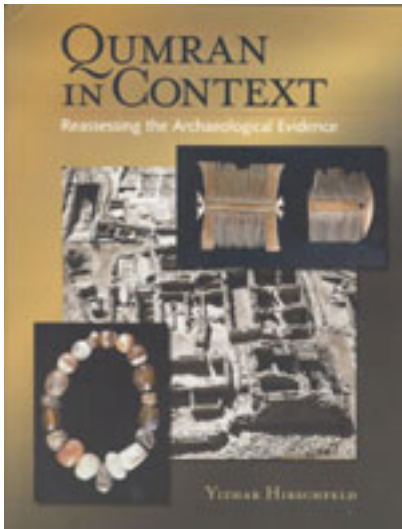


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**Hirschfeld, Yizhar**

***Qumran in Context: Reassessing the Archaeological Evidence***

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In the last decade, the interpretation of the site of Qumran has become one of the most hotly debated issues in the field of Second Temple period archaeology. Was Qumran a sectarian (and specifically, Essene) settlement, as the original excavator, Roland de Vaux, proposed (and as I believe, following de Vaux), or was it something else—a villa, manor house, fort, commercial entrepot, or pottery manufacturing center—as others have suggested? In the present book, Yizhar Hirschfeld presents a sweeping revisionist interpretation, identifying Qumran as a “field fort and road station” in the Hasmonean period and as a manor house in the Herodian period. The book is divided into five chapters: Qumran’s physical setting and the history of fieldwork and research (ch. 1); an overview of the Dead Sea Scrolls, focusing on the question of their place of origin (ch. 2); a description of Qumran including Hirschfeld’s interpretation of the stratigraphy and chronology (ch. 3); a description and discussion of the nearby site of Ein Feshkha (ch. 4); and Hirschfeld’s reconstruction of the settlement picture in the Dead Sea region at the time of Qumran (ch. 5). The book is richly illustrated with maps, plans, drawings, and black-and-white and color photographs.

As John Collins notes in his endorsement on the dust jacket: “Hirschfeld’s description of the archaeology of Qumran is so different from the influential accounts of de Vaux and Magness that it is sure to be controversial.” The alternative interpretations of Qumran proposed by Hirschfeld and others have caused a great deal of confusion among

archaeologists, Dead Sea Scrolls scholars, and the general public, for how can anyone—and especially those who do not specialize in archaeology—judge the validity of the various interpretations when the same evidence is interpreted so differently? In this review, I hope to demonstrate that Hirschfeld’s interpretation is methodologically flawed and factually erroneous and that the debates surrounding the alternative interpretations add nothing to our understanding of Qumran.

The interpretation of Qumran as a sectarian settlement depends on whether one accepts or rejects the association of the scrolls with the site. Simply put, scholars who accept the connection between the scrolls and the site identify Qumran as a sectarian settlement (this is true regardless of whether or not these scholars identify the sectarians as Essenes). This school of thought is sometimes described as “consensual” (or consensus). Scholars who reject the identification of Qumran as a sectarian settlement (the “nonconsensual” or nonconsensus school of thought) must necessarily argue that there is no connection between the scrolls and the site. According to this view, the inhabitants of Qumran did not own and use the scrolls or deposit them in the nearby caves. This approach is articulated as follows by Hirschfeld: “By reexamining the vestiges of Qumran on their own terms, in isolation from the content of the scrolls, we can release Qumran from the interpretive impasse in which it has become trapped, whereby the scrolls explain the finds and the finds explain the scrolls” (6).

The rejection of any connection between the scrolls and the site of Qumran might seem to be a simple matter of prioritizing the archaeological evidence over literary sources—a question of how to weight the evidence. As Jurgen Zangenberg writes in the foreword, Hirschfeld’s book is distinguished by “following a strictly archaeological agenda ... [showing] how to read the archaeology of Qumran as *archaeology* for the first time” (xiii). Zangenberg is right because Hirschfeld can support his identification of Qumran as a manor house only by ignoring the information the scrolls provide about the community that deposited them in the caves.

The prioritizing of archaeological evidence over literary sources might seem to be innocent and even laudable. Why not let the archaeological evidence speak for itself? As Zangenberg puts it: “In the Qumran-Essene-Theory” the role of archaeology is largely affirmative and illustrative.... The so-called ‘consensus’ never really took the archaeology of the site seriously as an independent source of information and knowledge, but rather, concurred with a widespread tendency of scholars who mainly deal with texts to deny archeology its very own power to formulate concepts and ideas about the past” (xii). Zangenberg’s claim that scholars who identify Qumran as a sectarian settlement have not seriously considered the archaeological evidence belittles the contributions made by such scholars as Roland de Vaux, Yigael Yadin, and Magen Broshi. More

importantly, Zangenberg and Hirschfeld do not just prioritize the archaeological evidence but dismiss the literary evidence altogether. Why should one type of evidence [archaeological] have priority over another [literary]? Why not use both types of evidence together, to complement each other? Qumran represents an extraordinary discovery that provides an opportunity to use archaeological and literary evidence together to understand the lifestyle and beliefs of this community. For example, archaeologically we can identify the remains of ten *miqva'ot* at Qumran, while literary sources (the scrolls and Josephus) describe the purification rituals of this community. Taken together, these two types of evidence are complementary. In fact, Qumran is an excellent example of a site that provides physical, archaeological evidence for the religious beliefs and practices of its inhabitants. Why would anyone want to disregard or prioritize one type of evidence over another? Of course, the answer is that this is necessary if that evidence does not support one's interpretation.

A pronounced element of postmodernism underlies the nonconsensual school's interpretation of Qumran. This is evident in the prioritizing of the archaeological evidence and the rejection of any connection between the scrolls and the site. As a result, Qumran is ripped from its social-religious-historical context, leaving us with the archaeological remains alone: stones, potsherds, coins, glass, and the like. Although these remains provide certain types of information (e.g., the large number of *miqva'ot* suggests a concern with ritual purity), they cannot inform us about the religious beliefs and ideology of the inhabitants (e.g., only from the scrolls do we learn that the sect was concerned with ritual purity because they conceived of their community as a substitute temple). In other words, without the scrolls the archaeological remains are ambiguous enough to support a variety of possible interpretations: that Qumran is a villa, manor house, fort, commercial entrepot, pottery manufacturing center, and so on. As Hirschfeld states: "Qumran has been variously defined as a fortress, a road station, or the center of an agricultural estate.... these interpretations are not necessarily mutually exclusive" (xv).

Denying any connection between Qumran and the scrolls automatically creates ambiguity, a situation in which Qumran can be interpreted in any one of a number of ways and all interpretations are equally valid. This is another reflection of postmodernism, according to which no one interpretation is correct and all interpretations have equal value. I would argue that the exact opposite is the case. Qumran could not have been a sectarian settlement, a villa, a fort, or a commercial entrepot—at least, not all at the same time. Only one interpretation can be correct, and not all interpretations carry the same weight or are equally legitimate. Although it is possible to interpret both the literary and archaeological evidence in different ways, only one interpretation is supported by a majority of the evidence and creates a minimum number of problems. For example, if one rejects the connection between Qumran and the scrolls, one must explain how the scrolls

came to be deposited in caves immediately below the settlement at the same time the site was occupied. Hirschfeld claims that the scrolls were brought from the Jerusalem temple before the Roman destruction in 70 C.E. and deposited in the caves around Qumran (45). However, much of the literature from Qumran expresses opposition to the (non-Zadokite) priestly establishment in Jerusalem and therefore could hardly have originated there. The works from Qumran reflect a distinctively sectarian outlook, including a different calendar than the one used by other Jews at the time and different interpretations of halakah. In support of his claim, Hirschfeld states that “More and more scholars are convinced that a small sect such as the Essenes could not have been solely responsible for the huge literary scope of the scrolls discovered in the caves near Qumran and that the site itself cannot have been the place where such numerous and varied compositions were written and copied” (45). This statement creates a false impression, since even scholars who identify Qumran as a sectarian settlement do not identify all of the works as sectarian compositions or claim that all (or most) of the scrolls were written or copied at Qumran. To the contrary, the fact that some of the scrolls antedate the establishment of the settlement indicates they were brought to Qumran from elsewhere.

Similar postmodern tendencies underlie the minimalist-maximalist debate raging over the origins of the Israelites and the nature of the united kingdom. The minimalists reject any element of historicity in the biblical accounts. This removes the archaeological remains from their historical and religious context, making it possible to argue for different ranges of dates and for different interpretations (e.g., the kingdom of David and Solomon was just a small chiefdom, and the monumental building remains traditionally associated with them date to a later period). On the other hand, the anti-Zionist or post-Zionist and postcolonial aspects of the minimalist-maximalist debate have no analogues in Qumran archaeology, although there are religious and antireligious elements, as illustrated by Hirschfeld’s statement, “By suggesting that Jerusalem is the source of the scrolls, we liberate Qumran from the burden of religious significance. 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” (5).

Hirschfeld argues that the water level of the springs by the Dead Sea has fluctuated significantly in historical periods, affecting the quality of the water and the accessibility of the road along the shoreline. According to Hirschfeld, the climate in the Second Temple period was wet, causing the level of the Dead Sea to rise to ca. -395 m and turning brackish water in springs such as Ein Feshkha to sweet water. Against this backdrop of a more hospitable environment, Hirschfeld paints a picture of dense agricultural settlement and bustling economic activity along the shores of the Dead Sea.

Of course climate and environment affect human settlement patterns, but Hirschfeld's environmental determinism is overly simplistic and his sweeping claims demonstrably wrong. Arlene Rosen and Steven Rosen have recently highlighted the problematic use of environmentally deterministic models in archaeology:

models of climactic and environmental change, especially those trotted out for use by archaeologists, have remained on the simplistic level of wet/dry cycles, with little true environmental reconstruction.... Even the establishment of the length of an environmental episode must be considered, especially since time and intensity may often be confused in the proxy data. It is notable that in some cases, climactic changes have been assumed to correlate with the social changes, and the social changes thus used to date the climactic events.... our handy charts showing sequences of hot/cold, dry/wet, alleviation/erosion, or trees/grasses, usually modeled on archaeological periodization schemes, are fundamentally simplistic, and in fact tell us almost nothing about the real events that influenced human behavior. (A. M. Rosen and S. A. Rosen, "Determinist or Not Determinist? Climate, Environment, and Archaeological Exploration in the Levant," in *Studies in the Archaeology of Israel and Neighboring Lands in Memory of Douglas L. Esse* [ed. S. R. Wolff, Chicago: Oriental Institute of the University of Chicago, 2001], 538–39)

Although some scholars have suggested that the Roman period was wetter than today (this has been proposed for other historical periods as well), the evidence is far from unequivocal, and there is no consensus. Hirschfeld states that flash floods are the most important water source for the Dead Sea and that "In dry periods like the present-day one, the level drops below -402 m and the southern basin dries up" (7). In fact, the Jordan River is the Dead Sea's main source. The sea's current low level (including the disappearance of the shallow southern basin since the 1970s) is due to the fact that Israel and Jordan have been siphoning off nearly all of the water from the Jordan River before it reaches the Dead Sea. In other words, today's drying up of the Dead Sea is a result of man's effect on the environment, not the environment's effect on man!

The archaeological evidence also contradicts Hirschfeld's claim that the Roman period was significantly wetter and that the springs of Ein Feskha, Ein Ghuweir, and Ein et-Tureiba were sweet, not brackish. Remains of permanent, multiperiod settlements on the western shore of the Dead Sea are found only at oases that have always had perennial sources of fresh water, specifically at Jericho and Ein Gedi. In contrast, permanent settlements existed at Ein Feshkha and Ein Boqe only in the late first century B.C.E. and first century C.E. (until 68) (with a brief and limited Byzantine presence at Ein Feshkha and a Byzantine fort at Ein Boqe). The settlement at Ein el-Ghuweir dates to the first century C.E. only (until 68). Changes in the quality and level of these springs are measured

in centuries and even millennia, not in decades, since they are fed by underground aquifers that have formed over the course of geological ages. Had the climate fluctuated as dramatically as Hirschfeld claims, making the spring water at Ein Feshkha and Ein el-Ghuweir sweet, we should expect to find remains of permanent, multiperiod settlements at these sites instead of a brief occupation lasting no more than seventy-five years. Furthermore, the water system at Qumran is clearly adapted to the same sort of desert conditions that exist today. The *miqva'ot* (which were filled by flash-flood waters from Wadi Qumran) served the needs of a community concerned with maintaining a high degree of ritual purity. Even so, the number of *miqva'ot* is disproportionately high relative to the size of the community (which perhaps consisted of 100–150 members). The large number of *miqva'ot* (and their large sizes) was clearly designed to take into account the long dry season and the reality that flash floods in Wadi Qumran might occur only one day a year. Because of these conditions, the inhabitants attempted to maximize the amount of water captured and stored in pools (cisterns as well as *miqva'ot*).

Hirschfeld claims that balsam was a major product of the oases along the western shore of the Dead Sea, including Ein Feshkha and Ein Boqeq (214), and that it was processed at Qumran (138). Similarly, Niger Hepper and Joan Taylor have recently argued that the opobalsam (Hebrew *afarsamon*, the plant from which balsam was extracted) was cultivated widely in the Jordan Valley and along the Dead Sea (F. N. Hepper and J. E. Taylor, "Date Palms and Opobalsam in the Madaba Mosaic Map," *PEQ* 136 [2004]: 35–44). Hepper and Taylor base this on the identification of a plant represented in the sixth century C.E. Madaba Mosaic Map as the opobalsam bush. However, their identification is undermined by the fact that not a single bush is depicted on the Madaba Map at Jericho, which was famous for its opobalsam plantations (see Hepper and Taylor, "Date Palms and Opobalsam," 38 fig. 2). Furthermore, the literary sources cited by Hepper and Taylor do not support their claim that the opobalsam was cultivated on the western shore of the Dead Sea outside of Jericho and Ein Gedi. According to Hepper and Taylor, "Diodorus Siculus assigns opobalsam groves to the Aulon, i.e. Jordan valley, but does not link the growing of opobalsam to Jericho alone" ("Date Palms and Opobalsam," 41). Instead, Diodorus's statement that "In a certain valley in this region there grows what is called balsam" (19.98) only informs us that opobalsam was grown in the Jordan Valley but does not indicate that it was cultivated outside of Jericho. And even if Hepper and Taylor's literal interpretation of this passage is correct, Diodorus refers to the Jordan Valley, not the Dead Sea. Hepper and Taylor bolster their argument by citing Pliny's reference to the spread of the cultivation of the opobalsam ("Date Palms and Opobalsam," 41), but an examination of the relevant passage contradicts their interpretation:

But every other scent ranks below balsam. The only country to which this plant has been vouchsafed is Judaea, where formerly it grew in only two gardens

(*horti*), both belonging to the king [that is, in royal plantations at Jericho and Ein Gedi].... The balsam-tree is now a subject of Rome, and pays tribute together with the race to which it belongs; it differs entirely in character from the accounts that had been given it by Roman and foreign writers, being more like a vine than a myrtle: it has quite recently been taught to grow from mallet-shoots tied up on trellises like a vine, and it covers whole hillsides as vineyards do.... It is now cultivated by the treasury authorities, and was never before more plentiful. (Pliny, *Nat. Hist.* 12.111–114).

Nowhere does Pliny mention the cultivation of the opobalsam outside of Jericho and Ein Gedi. His description of opobalsam growing on hillsides accurately describes Ein Gedi, where the remains of ancient agricultural terraces can still be seen covering the slopes. The other sites mentioned by ancient authors in connection with the cultivation of the opobalsam (which are cited by Hepper and Taylor)—Livias and Zoara—are located on the *eastern* side of the Jordan Valley and Dead Sea.

There is thus not one shred of archaeological or literary evidence to support Hirschfeld's claim that balsam was cultivated at Ein Feshkha and processed at Qumran. We do not know what was produced in most of the workshops at Qumran or at Ein Feskha, but there is no indication that it was balsam. Even if a juglet from Cave 13 contained balsam (which is a matter of dispute), the balsam's origin is unknown. The cave in which the juglet was found is not a scroll cave and is located at a considerable distance from Qumran—to the north of Cave 3, the northernmost of the scroll caves (see Hirschfeld, 22). Therefore, this juglet has no definite connection with Qumran, as the archaeologists who discovered it concluded: "it is doubtful if they [those responsible for hiding the juglet] were members of the sect" (J. Patrich, "Khirbet Qumran in Light of New Archaeological Explorations in the Qumran Caves," in *Methods of Investigation of the Dead Sea Scrolls and the Khirbet Qumran Site: Present Realities and Future Prospects* [ed. M. O. Wise et al.; *Annals of the New York Academy of Sciences* 722; New York: New York Academy of Sciences, 1994], 91). Surely it is no coincidence that our ancient sources attest to the cultivation and production of opobalsam at Jericho and Ein Gedi, which are the only perennial fresh-water springs on the western side of the Dead Sea. Hirschfeld's reconstruction of dense agricultural settlement based on the cultivation and production of balsam along the shores of the Dead Sea is pure fantasy, based on a problematic and unsubstantiated assumption of a wetter climate and sweeter springs and an absence of supporting archaeological and literary evidence.

Hirschfeld and Zangenberg have criticized me for not considering Qumran within a broader regional context, claiming that this has led to my understanding (or misunderstanding) of Qumran as a "self-contained, isolated, and ascetic community." As Zangenberg states,

“Hirschfeld’s book marks a new step in Qumran archaeology because, for the first time, he takes the *region* as prime context of the settlement. Not isolation, but analogy and comparison, are the appropriate ways to look at Qumran” (xiii). Although Qumran is the focus of my book (as reflected in its title, *The Archaeology of Qumran and the Dead Sea Scrolls* [Grand Rapids: Eerdmans, 2002]) I did examine parallels between the architecture and material culture of Qumran and other sites. And as Hirschfeld notes (and I have discussed), there are some similarities, such as the square towers at Qumran, Hilkiah’s palace (Khirbet el-Muraq), and other sites. However, it is the differences, not the similarities, that define the nature of the settlement at Qumran.

A comparison between Qumran and other sites—that is, a consideration of Qumran within its larger regional context—reveals just how different it is. Qumran appeared to be unique in de Vaux’s time because few sites had been excavated in the Dead Sea region. The fact that many more sites have been excavated and published since then—including Herodian Jericho, Ein Boqeq, Ein Gedi, Herodium, Masada, Kallirrhoe, and Machaerus—only highlights the unique nature of the settlement at Qumran. None of the anomalous features at Qumran is paralleled at any other site. These include the large number of *miqva’ot* (and their large sizes), the animal-bone deposits, the large adjacent cemetery, the communal dining rooms with adjacent pantries containing hundreds of dishes, the numerous workshops, and an unusual ceramic repertoire (including the distinctive cylindrical jars, with only a single example attested from Jericho and none published from any of the other sites mentioned). Examining Qumran within a larger regional context actually works against Hirschfeld and Zangenberg by showing just how unique it is.

To interpret Qumran as anything but a sectarian settlement, Hirschfeld, Zangenberg, and others must explain away each one of the anomalous features, often with interpretations that are far-fetched and unsubstantiated. For example, according to Hirschfeld, the animal-bone deposits prove that the inhabitants could not have been Essenes because the Essenes were vegetarians. Hirschfeld bases this on Josephus’s statement that the Essenes lived a “Pythagorean way of life”; since the Pythagoreans were vegetarians, the Essenes must also have been vegetarians! Hirschfeld is undeterred by the fact that none of our ancient sources describe the Essenes as vegetarians, although they go to great lengths to point out the peculiarities of the Essenes’ lifestyle (including their toilet habits!). Hirschfeld’s curiously literal and unsupported interpretation of this passage reflects a basic misunderstanding and misuse of Josephus. Josephus (and Philo) described Jewish sects in Greek philosophical terms to make them understandable to Roman readers and to demonstrate the superiority of Judaism over Greco-Roman religions (similar agendas and biases account for the emphasis these authors place on male celibacy among the Essenes).

The book is riddled with erroneous or problematic claims and assertions, of which I cite just a few examples. (1) Contrary to Hirschfeld, none of the published pottery from Qumran (with one exception) represents types that appear to antedate the first century B.C.E. Hirschfeld cites Andrea Berlin's comparison of the pottery from Qumran with late Hellenistic pottery from other sites (59–60), but this means only that the Qumran pottery *postdates* the mid-second century. In fact, Hirschfeld does not mention any specific examples of pottery types found at Qumran that support his early dating. Similarly, the handful of Seleucid coins only provide a rough *terminus post quem* (literally, the date after which, not the earliest possible date, as Hirschfeld states!) for the establishment of the sectarian settlement (59), especially since coins often remained in circulation for long periods in antiquity.

(2) The fact that the sloping rampart blocked two windows in the tower and obstructed the open passages surrounding it proves that it was added as reinforcement after the earthquake of 31 B.C.E. and is not part of the original construction (the rampart is identified by Hirschfeld as a *proteichisma* or forewall [69]; see Magness, *Archaeology of Qumran*, 57). Hirschfeld's photograph (73 fig. 29) does not support his claim that the outer walls of the tower were left unfinished, and they probably were covered with mud plaster anyway.

(3) Hirschfeld confuses and conflates stucco with plaster. Stucco is plaster molded to imitate architectural shapes (such as cut marble blocks or eggs-and-darts) and is a relatively costly type of interior decoration. In contrast, the plaster still adhering to an inner wall of the tower is just that: plaster, not stucco (see Hirschfeld, 70 fig. 27). The recent publication of stone decorative elements from Qumran has not altered my observation that “the small number of these elements shows that interior decoration is almost completely absent from Qumran” (Magness, *Archaeology of Qumran*, 100; see A. Chambon, “Catalogue des blocs d'architecture localizes ou erratiques,” in *Khirbet Qumran et 'Ain Feshkha II* [ed. J.-B. Humbert and J. Gunneweg; Fribourg: Academic, 2003], 445–65). Aside from the column drums, bases, capitals, and voussoir stones known previously from de Vaux's publications, Alain Chambon illustrates a small battered fragment of a limestone cornice block carved with an egg-and-dart motif and seven limestone tiles of different shapes and sizes (square, circular, triangular). These elements are made of local limestone (*calcaire*, as described in French by both de Vaux and Chambon, not sandstone, as Hirschfeld states [142]). Since none of them was found in situ, their origin within the settlement remains unknown. I have hesitantly suggested that these elements belonged to the pre-31 B.C.E. phase of the settlement, but this question is unresolved. There is no evidence at Qumran for stucco, frescoes, or mosaic floors.

(4) Hirschfeld states that the Hasmonean-period inhabitants of Qumran “were already meticulous in their observance of the religious law calling for the separation of an

inhabited area from a place reserved for the dead” (83). But there is no other example of a Hasmonean-period fortress with a large cemetery next to it!

(5) Hirschfeld repeats his earlier claim that “many examples of fine wares” were found at Qumran (93). Had Hirschfeld read J. Magness, “The Community at Qumran in Light of Its Pottery” (in Wise et al., *Methods of Investigation*, 50 [not listed in his bibliography]), he would have seen that Pauline Donceel-Voute acknowledged there is very little fine ware from Qumran:

Magness: “My impression from what I saw in the basement of the Rockefeller Museum (which was, I think, most of the pottery that’s still around) is that there was very, very little in the way of fine wares. The vast majority is plain, undecorated pottery, and that does sharply contrast with contemporary assemblages at other sites.”

Eric Meyers: “I concur; my visits also corroborate that. *I see an affirmative nod from Professor Donceel-Voute*” (emphasis added).

(6) Although stone vessels cost more to produce than pottery, their presence indicates a concern with the observance of Jewish purity laws and does not necessarily indicate wealth (contra Hirschfeld, 142). Examples of the relatively costly lathe-turned stone vessels are attested at Qumran (how many is not clear; only two fragments of a single vase are illustrated by the Donceels), but most of the pieces belong to the less expensive chip-cut vessels common at towns, villages, and farmhouses throughout the country, not just at villas and estates of the wealthy (for the stone vessels from Qumran, see R. Donceel and P. Donceel-Voute, “The Archaeology of Khirbet Qumran,” in Wise et al., *Methods of Investigation*, 10–12; for a recent study of stone vessels, see Y. Magen, *The Stone Vessel Industry in the Second Temple Period* [Jerusalem: Israel Exploration Society, 2002]). Similarly, glass is not always a luxury item; like pottery and stone vessels, some types are more expensive than others. On the other hand, the fact that the sectarians chose to live a communal lifestyle does not mean they were impoverished, since some members undoubtedly came from prosperous families and surrendered their possessions when they joined the sect.

(7) Except for Roman luxury latrines, ancient toilets lacked flushing devices and were not connected to drains or sewage systems (contra Hirschfeld, 100). The toilet at Qumran is typical in consisting of a pit dug into the dirt floor of a room that would have been emptied periodically as necessary (see Magness, *Archaeology of Qumran*, 106–7).

(8) Instead of working with the chronological framework and stratigraphic information provided by de Vaux while recognizing the limitations, Hirschfeld constructs a new

periodization framework (55). Hirschfeld's periods are not actually "strata" (as he calls them) but are based almost entirely on architectural (as opposed to stratigraphic) considerations. Specifically, Hirschfeld claims that the original settlement had a square layout, with later additions surrounding it (see 89 fig. 41). He believes that the original neat square layout reflects the military nature of the Hasmonean period occupation, while the less rigid planning of the additions show that the Herodian period settlement was civilian (88). However, careful readers will notice that Hirschfeld's neat square has been artificially created by the addition of wall lines (indicated in bold dashes) on the north side and at the southwest corner of the square (see 89 fig. 41). Furthermore, some of these walls are apparently Iron Age in date (see R. de Vaux, *Archaeology and the Dead Sea Scrolls* [London: British Academy, 1973], pl. III).

(9) There is no stratigraphic basis for Hirschfeld's periodization, which collapses and muddles the evidence for the different occupation phases that de Vaux identified. Additional confusion is caused by his refusal to accept a date of 31 B.C.E. for the earthquake that damaged the settlement. For example, Hirschfeld dates the broken dishes found in the pantry or annex (L86) next to the dining room (L77) to the time of the First Revolt in 68 C.E. instead of to 31 B.C.E. (104). But Hirschfeld's date is contradicted by the pottery types themselves, which are visibly different from the types found in loci associated with the destruction of 68 C.E. And Hirschfeld does not account for the changes made to the pantry after the earthquake of 31 B.C.E. (when the area with the collapsed dishes was walled off and the rest of the room continued in use; see Magness, *Archaeology of Qumran*, 57), which clearly antedate the Roman destruction of 68 C.E. Photographs show that burnt ceiling beams from the pantry that were dated by means of radiocarbon to the first century C.E. lay on the floor of the Period II (first century C.E. phase) room and are not associated with the destruction caused by the earthquake of 31 B.C.E. (contra Hirschfeld, 104; see Magness, *Archaeology of Qumran*, 17).

(10) Hirschfeld claims that the number of *miqva'ot* at Qumran "is large but not exceptional. For example, a complex of the early Roman period was exposed near Shoham on the coastal plain. Smaller than the complex at Qumran, with an area of 2,800 m cubed [*sic*], it contains four miqvehs of various sizes." Let us examine this statement more closely. The report on Shoham was published by U. Dahari and U. Ad, "Shoham Bypass Road" (Hebrew) *Hadashot Arkheologiyot* 108 (not 109, as it appears in Hirschfeld's bibliography) (1998): 79–83. According to the report, an area measuring about 130 x 75 m was excavated, whereas Qumran covers an area of about 80 x 100 meters (see de Vaux, *Archaeology and the Dead Sea Scrolls*, 1). This means that the site at Shoham is larger, not smaller than Qumran. Two of the *miqva'ot* at Shoham are located within a fortified enclosure, and two are in an area of agricultural installations. Although the size of the *miqva'ot* is not mentioned in the report, the plan shows that all

four are small, rock-cut pools measuring no more than 2 meters long (including the steps) and about one meter wide. In contrast, the *miqva'ot* at Qumran include at least two examples that are between 17.5–12.5 meters long (L56 and 71) and four that are between 5.0–1.5 meters long (L48/49, 43, 117, 118, and 138), and they are all between 2–3 meters wide (for the dimensions of the Qumran *miqva'ot*, see K. Galor, “Plastered Pools, A New Perspective,” in Humbert and Gunneweg, *Khirbet Qumran et 'Ain Feshkha II*, 291–320). In other words, Hirschfeld’s comparison with Shoham only highlights the unusually large number and large sizes of the *miqva'ot* at Qumran.

In his footnote to the Shoham site (128 n. 144) Hirschfeld states that “Another Second Temple period settlement, with an area of four dunams and featuring four rock-cut *miqvehs*, was found at the site of Nahal Yarmuth in the southern coastal plain.” The source for this site is E. Eisenberg, “Nahal Yarmut,” *Hadashot Arkheologiyot* 112 (2000) (not 2001, as it appears in Hirschfeld): 91\*–93\* (English); 115–17 (Hebrew). The settlement at Nahal Yarmuth covers about four dunams, half of which was excavated (for a length of 80 meters). This means that the excavated area at Nahal Yarmuth is roughly comparable to the site of Qumran. The buildings in the center of the excavated area surrounded a courtyard. Agricultural installations including storage pits and an oil press were found inside these buildings. Four *miqva'ot* were found in open spaces by these buildings; two of them shared a common staircase and were located by the oil press. Although no plan of the site is published, an aerial photograph (115) shows that the *miqva'ot* at Yarmuth were small, rock-cut pools like those at Shoham and other sites. This comparison again highlights the uniqueness of the Qumran *miqva'ot*. Finally, the large number of *miqva'ot* at Jericho noted by Hirschfeld (128) is hardly surprising, considering that Jericho was the center of a priestly community, including the Hasmonean kings who also officiated as high priests (see J. Schwartz, “On Priests and Jericho in the Second Temple Period,” *JQR* 79 [1988]: 23–48).

(11) Hirschfeld’s claim that the animal bone deposits were fertilizer for a garden area is as absurd as it is unsubstantiated (106–8). Of course animal bones (and even human bones) are often found in archaeological excavations, usually in fills mixed with other debris. But no parallels exist for the animal-bone deposits at Qumran. The bones represent the remains of “kosher” species of animals (but significantly no poultry) that were slaughtered, butchered, roasted or boiled, and eaten. The bones were placed on top of the ground in the open-air spaces around the buildings (especially to the north and south of the site), either inside pots or covered with potsherds. De Vaux’s descriptions contradict Hirschfeld’s claim that the animal bone deposits were buried (108–10): “As a rule these deposits have hardly been covered with earth. They are flush with the level of the ground. Some of them even seem to have been laid on the ground” (de Vaux, *Archaeology and the Dead Sea Scrolls*, 12–13). Hirschfeld criticizes me for describing the deposits as

having been laid “carefully” on the ground (108), but this was de Vaux’s observation: “The care with which the bones were set apart after the flesh had been cooked and eaten reveals a religious preoccupation” (de Vaux, *Archaeology and the Dead Sea Scrolls*, 14). Whereas Hirschfeld claims that the bones were buried as fertilizer in a garden, he attributes the pottery vessels found with them to “the ‘craze’ of the Jewish people at this time regarding ritual purity,” which he says “was a subject of concern for the entire Jewish population of Second Temple period Judea, not only for sectarian groups” (108). If this is true, why is Qumran the only site where these animal-bone deposits have been found? Hirschfeld explains that it is “only because Qumran was so extensively excavated over the years was such a large amount of buried pottery discovered” (108). This claim is patently false—Hirschfeld himself conducted excavations at Ein Gedi for seven years, and if he had a single animal-bone deposit, he would surely have mentioned it (in comparison, de Vaux conducted excavations at Qumran between 1951–1956). Other sites in the Dead Sea region—including Masada, Jericho, Ein Boqeq, and Ein Feshkha—also have been extensively excavated (the last was reexcavated by Hirschfeld, but again, with no reference to the discovery of animal-bone deposits). Like so much else in this book, Hirschfeld’s reconstruction of a garden to the north of the site (113 fig. 57) is pure fantasy.

(12) In identifying L30 as a triclinium, Hirschfeld ignores a study that has demonstrated that the benches are too narrow to have been used for dining (R. Reich, “A Note on the Function of Room 30 (the ‘Scriptorium’) at Khirbet Qumran,” *JJS* 46 [1995]: 157–60; see Magness, *Archaeology of Qumran*, 60–61, 71, for a discussion and references to articles by other scholars who have identified L30 as a triclinium). The discovery of three inkwells (two in L30 and one in L31) supports de Vaux’s suggestion that some sort of writing activity took place in this room. As I have noted, inkwells are not common finds on archaeological excavations in Israel. Aside from repeating my observation that two inkwells were discovered by Nahman Avigad in Jerusalem’s Jewish Quarter (Hirschfeld, 96; Magness, *Archaeology of Qumran*, 61), Hirschfeld does not cite any other examples (apparently there are no inkwells from Ein Gedi or his other excavations). Interestingly, de Vaux’s discovery of an inkwell at Ein Feshkha suggests a possible connection with Qumran, although in my opinion there is no definite evidence that Ein Feskha was a sectarian settlement (see Magness, *Archaeology of Qumran*, 220–23). Of course, inkwells are not an indicator of sectarian presence. The point is that the discovery of three inkwells is unusual and supports de Vaux’s identification of L30 as a scriptorium, that is, a room in which writing activity took place.

(12) In referring to “ovoid bag-shaped jars,” Hirschfeld confuses and conflates two distinct types: ovoid jars and bag-shaped jars. Ovoid jars are a variant of cylindrical jars (the latter are sometimes referred to as “scroll jars”). Ovoid jars are similar to cylindrical jars in having a wide mouth with a very low neck and a low ring base, but they are broad

at the shoulders and narrow toward the base. In contrast, bag-shaped jars have a taller neck with a narrower mouth and a bag-shaped body that widens towards a rounded base (for descriptions and discussion, see Magness, *Archaeology of Qumran*, 82).

Hirschfeld vigorously (and sometimes viciously) criticizes me throughout his book (as well as in a review of my book that he published; see Y. Hirschfeld, “Qumran: Back to the Beginning,” *JRA* 16 [2003]: 648–52; for a rebuttal, see M. Broshi, “Response to Y. Hirschfeld’s review of J. Magness, *The Archaeology of Qumran*,” *JRA* 17 [2004]: 761–63). Of course Hirschfeld is entitled to express his opinions. More disturbing is the fact that some of the sentences in Hirschfeld’s book are identical or nearly identical with sentences in my book but without any indication or citation of the source. A few examples will suffice.

(1) Describing the reason for the location of a tower on the north side of the site:

Hirschfeld (90): “This was because most people approaching Qumran in antiquity, like today, would have come from the direction of Jerusalem and Jericho to the north.”

Magness (*Archaeology of Qumran*, 50): “This is because, like today, most people approaching Qumran in antiquity would have come from the direction of Jerusalem and Jericho to the north.”

(2) Describing the pottery from Qumran (which is based on my personal inspection):

Hirschfeld (146): “Most of the local vessels from Qumran are made from a pink, light red, or gray clay, often covered with a whitish slip. The presence of a potter’s workshop at the site implies that at least some of the vessels were locally manufactured. The pottery from the caves is identical to that from the site except that the repertoire is more limited.”

Magness (*Archaeology of Qumran*, 74, 73): “Otherwise, most of the vessels from Qumran are made of a pink, light red, or gray clay, often with a whitish slip covering the exterior. The presence of a potters’ workshop at Qumran indicates that at least some of the vessels were manufactured at the site.... The pottery from the caves is identical with that from the site except that it is more limited in repertoire.”

(3) Discussing de Vaux’s basis for dating the destruction of Qumran:

Hirschfeld (163): “De Vaux used the numismatic evidence and Josephus’s description to date the destruction of Qumran to 68 C.E.”

Magness (*Archaeology of Qumran*, 61): “De Vaux used the numismatic evidence and Josephus’s testimony to pinpoint the date of the destruction to 68 C.E.”

I was surprised to find these and other examples of my prose in Hirschfeld’s book without any reference to their source.

To conclude, Hirschfeld’s interpretation and other alternative interpretations of Qumran are contradicted by the physical connection between the scroll caves and the settlement and by the presence of numerous features that are unparalleled at other sites. These features—including the large number of *miqva’ot* (and their large sizes), the communal dining rooms with pantries containing hundreds of dishes, the animal-bone deposits, and the unusual ceramic assemblage including cylindrical jars—are physical expressions of this community’s halakah, which involved maintaining the highest possible level of ritual purity. This accounts for the absence of these features at other sites. Rarely does archaeology so clearly reflect a system of religious beliefs and practices.

Many aspects of the archaeology of Qumran remain poorly understood, and it is difficult to say anything definitive without the full and final publication of de Vaux’s excavations, which we still await fifty years later. Even so, the archaeological remains can be investigated productively, especially in light of the recently completed publication of the Dead Sea Scrolls as well as the publication of other excavated sites in the Dead Sea region. One hopes that future studies that are methodologically sound and scientifically rigorous will help us better understand this unique and important site.