Essenes or Zealots?

Some Thoughts on a recent Book

by Roland de Vaux O.P.

We have all been aware, indeed for some years now, that Prof. G. R. Driver was preparing a work on the Dead Sea Scrolls. And we knew that it would be a major work, and that it would certainly tell us something new. It was a long wait, but now it is ended, and here is the book[1], of more than 600 pages, which is throwing to the ground certain positions which had come to be regarded as established. It is the work of a scholar, who is considered one of the finest Hebraists of our time, and of a courteous man, who has many friends, among whom I have the honour to include myself. I cannot forget his charm and humour as chairman at one of my *Schweich Lectures* on these very same manuscripts in 1959, nor his kind words in the preface which he graciously agreed to write to these lectures when they appeared in print[2], nor the long talks we had and the meals at Magdalen College and at his home at Oxford, nor the visit we made together to Qumrân. All this makes it the more painful to me to be compelled to disagree with what he has written. After I have shared bread and salt with him (and a little more too), he might well say with the psalmist (he would say it in Hebrew): 'He who did eat of my bread, hath lifted up his heel against me'. But this book is without question one of the most important which has been written about the scrolls, and judgement about it must be made with complete honesty and frankness.

Well then, according to Driver, all of us who hold that certain of these writings were composed before the Christian era, and all of them before 68 A.D., and that they are the work of a group related to the Essenes – we have all got it wrong. And this is because we have not made use of proper historical method; we are guilty of starting from preconceived ideas about the date of the manuscripts, and about the relationship of their contents with Essene doctrines, and we try to prove this by illogical reasoning, sometimes by mistranslations, and by holding to one element in the problems while neglecting all the others. In addition he reproves us for not quoting our sources exactly enough to enable a proper check to be made. If these faults in method are eliminated – and Driver thinks that he has succeeded in doing so – the conclusion reached is quite

[1]G. R. Driver, *The Judaean Scrolls*. 8vo, x–625 pp., Blackwell, Oxford, 1965. 70/–.

[2]*L'Archéologie et les Manuscrits de la Mer Morte*, London, 1961, which I shall refer to as *Archéologie*, as Driver does.

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different, and, he claims, much more satisfactory. The following is an outline of the argument.

The community of Qumrân forms a 'covenant', its members call themselves sometimes 'the men of the covenant', and Driver calls them the 'Covenanters'. If a comparison is made between their doctrine and rules and those of various parties or movements within Judaism, it appears that they have nothing in common with the Sadducees, possible connexions with the Dositheans and the Samaritans, certain resemblances to the Pharisees, superficial resemblances to the Essenes, but also fundamental differences. On the other hand, there is a striking resemblance to an offshoot of the Pharisees, the 'fourth philosophy' of Josephus, the Zealots.

If then one considers the historical indications provided chiefly by the *Habakkuk Commentary* and the *War*, and secondarily by the *Damascus Document*, the *Manual of Discipline* and the *Hymns*, one is led to reject all pre-Christian or Christian identifications which have been proposed for the events and people concerned. One positive clue is provided by the *War*. The military organisation described certainly corresponds to that of the Roman armies of the Republic, though some details point cogently to the time of the Empire: standards with inscriptions, trumpets, sacrifice offered to the standards. The *kidôn* is not the Roman *gladius*, but the *sica*, the curved dagger which is an assassin's weapon used by the *Sicarii*, the name given to the Zealots by their opponents. In the *War*, as in the *Habakkuk Commentary*, the principal enemies of the community are the *Kittim* of Assur and the *Kittim* of Egypt. These are not the Seleucids and the Ptolemies, but the Romans. Their 'rulers' (*moššlim*) are the Roman procurators of Judaea, their 'king' is the Emperor. The sacrifice to the standards, which is mentioned, is that which was offered by the Romans before the Temple in flames in 70 A.D.: and this is moreover the first mention of such a sacrifice. The 'kings of the North' allied to the *Kittim* are the princes of Syria, vassals of the Romans. The apocalyptic war lasts seven years, like the First Revolt of 66 to 73 A.D.

The historical background of the scrolls therefore is the war against Rome. At this moment, the only party or group to which the 'Covenanters' can be attached is that of the Zadokites (not the Sadducees of the New Testament, of Josephus etc.). Their movement goes back to the division that occurred among the partisans of the Zadokite line of high priests after the deposition or death of Onias III (170 B.C.): one side accepted the new situation (and finally became the Sadducees of the New Testament), while the other side fled into Egypt with Onias IV and reinstated a temple cult at On-Leontopolis; some of this group then perhaps took refuge in the Judaean Desert (Periods Ia and Ib of Qumrân?). After Pompey had put an end in B.C. 63 to Greek domination, the Egyptian group returned to Jerusalem, in particular the priest Boethus, who founded the party of the Boethusians. But after Boethus had compromised himself with Herod, the whole of his party disowned him and threw in their lot with the left wing Pharisees, who were hostile to the Romans and were organising themselves at the time under Judah the Galilean and Zadok: the Zealot party. After the execution of Judah by the Romans in A.D. 6, the Zealots settled at Qumrân (Period II), and become the 'Covenanters', or became identified with them, if a group of Zadokite-Covenanters had already been residing there in the II-I centuries B.C. At the moment of the Jewish War, the Zealots and the Sicarii were identical or closely related. The 'Covenanters' of Qumrân called themselves 'the sons of Zadok': their identity with the Zadokites-Boethusians-Zealots-Sicarii 'can be regarded as reasonably certain' (p. 266).

Granted that the background of the scrolls is the revolt of 66–73 A.D., and that the Zealots were then particularly active, it should be possible to throw light on the allusions in the scrolls from the history of the Zealots during those years. According to Josephus, at the beginning of the revolt against Rome, at the end of the summer of A.D. 66 in Jerusalem, Eleazar, son of the high priest Hananiah and captain of the Temple, suppressed the daily sacrifices for the Emperor and the Roman people, and took possession of the Antonia. For his part, Menahem, the son of Judah the Galilean, the Zealot, now arrived with his own men from Masada, where he had plundered the armoury, and took control of the revolt. Eleazar, thus deprived of the leadership, unleashed the people against him. Menahem was taken, tortured and put to death on Mount Ophel. His lieutenant, Absalom, was also captured and killed. But Eleazar, son of Jair, another descendant of Judah and therefore a relative of Menahem, fled with a group of partisans to Masada, where he resisted to the end, when he and his men committed suicide in A.D. 73. The attack on Menahem and his death took place in Tishri 66, a little before the Day of Atonement, the 10th Tishri, perhaps even on the day itself.

Now the *Habakkuk Commentary* reports a central fact in the history of the Qumrân community, which took place one Day of the Atonement: the Wicked Priest pursues the Rightful Teacher, and this ends almost certainly with the death of the Rightful Teacher; the 'House of Absalom' is then reduced to silence and gives no help to the Rightful Teacher; the 'House of Judah' will be saved. All this can be picked up in the history of the Zealots in A.D. 66: the Wicked Priest is Eleazar, captain of the Temple, the Rightful Teacher is Menahem, the 'House of Absalom' stands for Absalom, Menahem's lieutenant, and the 'House of Judah' stands for the group that fled with Eleazar, son of Jair, a descendant of Judah the Galilean. It then becomes possible to extend the identifications still further. According to the Qumrân texts, the Rightful Teacher had other opponents in a 'Man of Falsehood' and a 'Young Lion of Wrath': these are identified with John of Giscala and Simon bar Giora, who were, during the revolt, leaders of rival groups, opposed to Menahem's group.

And here we also have the solution to a chronological puzzle in the *Damascus Document.* The 390 years that are to elapse between 'Nabuchodonosor' and the beginning of the movement have to be reckoned from Alexander's conquest, and this brings us, according to the reckoning of the Jewish chronographers, to the year 44 A.D.; the 20 years (a round figure) during which the movement was deciding its direction brings us to 64 A.D. Now it is in 46–48 that the sons of Judah, Simon and James, revolted and were executed, and it is in 66 that Menahem comes upon the scene as leader of the revolt.

Upon this historical perspective, and with their mutual relationships in view, it becomes possible to arrange and date the principal documents as follows: The *Manual of Discipline* between 46/48 and 66 A.D.; the *Copper Scroll* (see below) in 66–68; the *Habakkuk Commentary* between 70 and 73; the *Hymns* between 73 and 81 (?); the *War* under Domitian 81–96, perhaps in 85; the *Damascus Document* between 106 and 115. These dates refer to the composition. The date when they were deposited in the caves is more difficult to determine. The *Copper Scroll* is a list of Temple treasures which were hidden at the beginning of the revolt, after winter 66; the scroll was deposited in the cave before summer 68, when the Roman army arrived in the Dead Sea area. The document has nothing to do with the community, nor with the other scrolls. These were all deposited later, perhaps at intervals and perhaps right until the Second Revolt of 132–135 A.D. The reason for depositing them in the caves remains uncertain: the most likely being that the caves were *genizôth*, and that the putting away of these texts, biblical and non-biblical, damaged or suspect, corresponds to the hardening of orthodoxy among the Rabbis after the set-back of the First Revolt.

G. R. Driver concludes (Epilogue, p. 584): 'The picture presented in the Scrolls as here interpreted is consistent and harmonious', and I recognise that the demonstration has been performed in a most impressive manner. But is this internal consistency 'consistent' with the external evidence? And is not this harmony the result of 'harmonization'? These questions lie at the centre of the problem. The historians of the Jewish sects will be able to decide whether this motley history of the Zadokites-Boethusians-'Covenanters'-Zealots-Sicarii has sufficient foundation in the texts. The historians of their doctrines will be able to judge whether what we know of the rules and religious ideas (and we know next to nothing of them) of the Zealots, precisely as distinct from the Pharisees, authorises their identification with the community of Qumrân. The exegetes of the texts of Qumrân will be able to say whether they can accept the interpretation which Driver puts on the passages which are the most important for his thesis. And I leave to the palaeographers the task of defending the earlier dates which they assigned to the writing of the manuscripts, and which contradict the dates proposed by Driver for their composition. I will confine myself to an examination of questions of method, especially since it is here that his gravest accusations have been made against us, and to the consideration of Driver's use of archaeology, since this is the field which is most familiar to me, and is also the field where he has got lost.

According to Driver, p. 6, the mistake we have all made is to start from 'preconceived opinions based on incorrect premises': we started from the idea that the scrolls were pre-Christian, at any rate some of them; and this idea was based on the fallacious witness of palaeography and archaeology. At this point I permit myself to answer that Driver has started from the preconceived idea that all the scrolls were post-Christian, and that this idea was based on the fallacious witness of orthography, language and vocabulary. The idea and its proofs are to be found in Driver's first two works on these documents: *The Hebrew Scrolls* (Friends of Dr Williams's Library, Fourth Lecture), 1951, and *Hebrew Scrolls* in JTS, N.S. II (1951), pp. 17–30. His conclusion was that the manuscripts could not be earlier than the period 200–500 A.D., and probably nearer the end of this period. Since then he has rejected his proofs and abandoned his conclusion, but he has clung to his idea: the documents belong to various dates between 70 and 115 A.D., but they remain post-Christian. Conversely the 'incorrect premises' accepted by his adversaries have been confirmed, for palaeography by the discoveries at Murabba'at and at Masada, and for archaeology by the excavations at Khirbet Qumrân. They may therefore be excused if they hold to their 'preconceived idea'.

They are accused of wanting to prove their idea by illogical reasoning, by misusing the argument from silence, and by using conjectures to demonstrate other conjectures (pp. 4–6). But Driver himself is not innocent of these things. It is illogical to reject evidence that goes against one's main thesis, and then to use this same evidence, and moreover to falsify it, in order to support one element of this thesis. The jars which were found in the caves and in the buildings, and some of which contained manuscripts, prove nothing at all, he says, with regard to the date of the manuscripts, nor with regard to the relationship between the caves and the buildings (pp. 402–403); but, because these jars resemble two jars found in Egypt and containing papyri, they come in useful for bolstering up the theory that the 'Covenanters' are the descendants of the Zadokites who had taken refuge in Egypt. The whole 'history' of the jars is then reconstituted: they were used for storing the manuscripts at Qumrân or elsewhere. In these jars the manuscripts were then transported to Syria (the exodus to Damascus?) or elsewhere along the desert tracks. And in these same jars they were brought back to Qumrân and hidden in the neighbouring caves (p. 407). This idea of a library making two journeys across the desert in jars is simply contrary to common sense. Furthermore, the parallel with the Egyptian jars is interesting, but it is not decisive and only concerns a type which is rare at Qumrân, the type with the small handle (DJD I, pp. 8-9), while the current type at Qumrân is not represented in Egypt. Finally, the discoveries of the caves and of the Khirbeh have shown that these jars were destined for – and normally used for – the storage of provisions and not of manuscripts (DJD III, pp. 32–35).

When Mrs Crowfoot says, in DJD I, p. 27, that her examination of the textiles brings her to conclusions which are in accordance with the date assigned by the archaeologists to the depositing of the scrolls in the caves, this is a worthless statement (p. 409); but when she says (DJD I, p. 19) that the yarn used in the cloths is spun in the manner of Egypt, this serves to confirm the group's Egyptian connexions (same p. 409). The trouble is that Mrs Crowfoot then goes on to say immediately afterwards that this is the only proper way to spin flax (of which the tissues are made), and indeed she adds, on p. 22, that the same method was used at Palmyra and at Dura, and that other aspects of the tissues seem to her to indicate that their origin is local and not Egyptian.

There are two ways of misusing the argument from silence. In one way it may be claimed that the silence of the literary sources disproves a conclusion which had been well established by other means. Thus the archaeologists claim to have established that the buildings at Qumrân were violently destroyed in 68 A.D. by the Romans. This says Driver (p. 397), is impossible, because Josephus says nothing about it. But Josephus did not mention everything that happened, and he is our only source for this period. In another way the silence of the literary sources may be used in order to insert one's own hypotheses. Thus the agitated history of the Zadokites-Boethusians-'Covenanters'-Zealots, which Driver reconstructs as described above, is built upon the silence of the literary sources, which say nothing of a return of the Zadokites from Egypt under Pompey, of their connexion with the Boethusians, of the *volte-face* of the Boethusians and of their union with the Zealots, and there is not a word about the 'Covenanters'.

The thing is an elaborate structure of hypotheses. Another such is his 'reconstruction' of the history of Qumrân, on pp. 46–47 (the italics are mine): 'The original buildings had *possibly* been built for political purposes; after the exile they *perhaps* came to be used as a refuge from persecution or oppression, from injustice or even from justice. Early in the 2nd century B.C. Maccabaean or Hasidaean refugees *might perhaps* have taken possession of them or have even made them into a permanent settlement, as the Essenes established themselves further south of Engadi for similar reasons; there they *would have* remained, more or less unmolested, until the buildings were destroyed by an earthquake in 31 B.C. These were abandoned till *c.* A.D. 4–6, when they were rebuilt. The new occupants, whom nothing connects with their predecessors, *would be* the followers of Judah and Saddock, dispersed after the abortive rising in A.D. 6, when Judah fell into the hands of the 'Romans'. This is presented as founded upon archaeological facts and 'written history'. We shall see presently whether archaeology has been sufficiently respected; but in any case, 'written history' certainly has not: it has been used to form conjectures, which are then used to prove another conjecture, namely, that the people of Qumrân are the Zealots.

At the end of his work, this initial conjecture is not yet proved. He has explained to his own satisfaction the contents of the scrolls, but he has not been able to explain their discovery. He does not know when they were deposited in the caves: 'this seems unfortunately to be one [problem] of which no exact solution can be found' (p. 377); he does not know why they were deposited there: 'All this reconstruction of the concealment of the Scrolls is naturally conjectural' (p. 392). The last page of the book, p. 591, contains the admission of a more complete checkmate: 'The hypothesis here put forward ... cannot of course be absolutely proved . . . and indeed, until the missing link connecting the Covenanters of Qumrân with the Zealots of Masada is found, it cannot be checked'. In short, he finds that the essential proof is lacking.

He could, however, have found this 'missing link' in the recent discoveries at Masada. He must have known about these, since he refers on p. 394, n. 7, to the article of Y. Yadin in ILN for 31.x.64 on the excavations at Masada. But he uses it in a most extraordinary way: this reference is given in support of an affirmation that *one* coin of the fifth year of the revolt had been found at Qumrân, while in fact Yadin is speaking of *three* coins of the fifth year found at . . . Masada. Furthermore he could have read in that same article, in the next column, of the discovery at Masada of a manuscript fragment of a work which exists also among the fragments of Cave 4 at Qumrân (the *Angelic Liturgy*, of which J. Strugnell published a section in Suppl. VT, VII), and which bears witness to the use of the calendar proper to the 'Covenanters', to which Driver devotes a whole section, pp. 316–330. C. Roth, who defends positions very close to those of Driver, did not fail to make the best of this windfall, when he concluded that 'the inescapable corollary is that the Qumrân sect belonged to the same body as the Zealot/Sicarii who had their military centre at Masadah'.

But it is not quite so simple as that. The work at Masada, in the course of two campaigns, and in different parts of the site used by the rebels of the First Revolt, have brought to light various manuscript fragments: fragments of several biblical books in Hebrew square character, but there is evidence of Leviticus both in square character and in archaic script, an important part of the Hebrew text of Ben Sira, a fragment of the *Book of Jubilees* in Hebrew, fragments of several non-biblical works, among which is the fragment of the *Angelic Liturgy* or *Chants for the Sabbath Sacrifice*, which has just been mentioned. The composition of the Masada collection is, in this sense, of the same character as that of the collections found in the various caves of Qumrân; but this does not mean that the occupants of Qumrân were the same as the occupants of Masada; it only means that both groups were Jewish. On the other hand, the presence of a work so typical of the literature of Qumrân as is the *Angelic Liturgy* indicates that there must have been – at least at some moment – a link between the two groups. But it is going much too fast and too far to conclude from this that the people of Qumrân were not Essenes, and that they were Zealots. In fact the notion of a Zealot occupation of Qumrân – *pace* C. Roth and G. R. Driver – fits neither with the contents of the texts, nor with the data of archaeology (to this last point I will return later). Already before the publications of Roth and Driver, J. T. Milik had suggested that the last phase of Essenism had taken on something of the character of the Zealots, the book of the *War* being the principal evidence of this. It seems to me, however, more correct to say with Fr M. Cross that a general apocalyptic trend of thought – present also among the Essenes – produced this work, whose sense of unreality is recognised by everyone, even by Driver: it is an eschatological war. When the Revolt broke out, a section of the group thought that the last days had come, and they resisted the attack of the Romans: for there was resistance and some destruction of buildings. It is amusing to me to notice that I admit – because the archaeological evidence leads me to – that the Essenes, who were pacifists, offered resistance to the attack; while Driver, against the archaeological evidence, thinks (p. 399) that his warlike Zealots withdrew from Qumrân without giving battle, at the approach of the Roman armies. It is Essenes who rallied to the revolt, whom we find again at Masada, together with one or other of their writings. We knew already from Josephus that the Revolt had not been the undertaking exclusively of the Zealots, and that moreover Essenes had taken part in it: in fact during the first victorious phase of the Revolt, in 66–67 A.D., a certain John the Essene had been appointed governor of the region of Thamna, Lydda, Jaffa and Emmaus, and with two other rebel leaders he had directed the ill-fated attack on Ashkelon, where he died.

If Driver had not made use of this argument, it is perhaps because – oddly enough – this 'Qumrânian' text found at the Zealots' stronghold at Masada was an embarrassment to his thesis. In order to make the date of the composition and actual writing of the scrolls as late as the end of the first century A.D. and the first third of the second century, he has had to invoke an absence (which he exaggerates) of comparable and exactly dated documents of the first century B.C. and the first century A.D. (pp. 410 ff.). Now, all the fragments of Masada are certainly earlier than the capture of the fortress by the Romans in A.D. 73. We then have, belonging to the subsequent period up to the Second Revolt of 132–135 A.D., the exactly dated documents found in the caves of Murabba'ât and the caves further to the south. These new witnesses confirm the dates assigned to the Qumrân scrolls by the palaeographers: like the new fragments of Masada, they are all earlier than A.D. 73.

It is archaeology, at least as much as palaeography, which is an embarrassment to Driver, and he admits, on p. 393, that it is from this quarter that he is most open to attack. It is a sad thing to find here once more this conflict of method and mentality between the textual critic and the archaeologist, the man at his desk and the man in the field. It happens, of course, in other areas: in the study of the ancient traditions of Israel, or of the homeric poems. Driver says on p. 394: 'The internal evidence afforded by a document must take precedence over any external evidence'. No – other things being equal, there is no precedence between the two kinds of evidence: a correct solution must make use of both, must prove the worth of both. Driver declares that he accepts the archaeological facts, but he rejects the conclusions drawn from them because they contradict the historical indications in the texts (p. 394). It is a little naive to suppose that it is only the archaeologists who indulge in interpretation, and that the textual critics never do this. Everyone has to interpret the evidence if history is going to be written, since an object found on a 'dig', or equally a fact stated in a text, have no meaning at all until they are set in a framework together with other objects or other facts, completed and if necessary corrected by other evidence. I would even be prepared to say that it is easier for the archaeologist to be objective: he is working with real material things, in real places, which have remained what they are and where they are, and he cannot alter this. A wall remains a wall, a pot remains a pot, and a coin remains a coin. If he knows his job, he can, with his coins and his pots, give a date to his walls. And he is usually more modest than his textual friend, for he is continually going to the written documents – if there are any – to guide or to test his conclusions. But the textual critic is not working on real tangible things: between him and the historical fact there are all the interpretations and possible mistakes of his author, and if – as is more usually the case – it is not an original he is working on, there are all the interpretations and mistakes of copyists and translators who have provided his text; and on top of all this there are his own interpretations ... and perhaps his mistakes. He should not overlook, nor set aside, nor deface the walls, the pots and the coins of his brother the archaeologist.

Like everyone else, of course Driver puts interpretations on his documents: their 'internal evidence' and their 'historical notices' are no more than the starting-point of his inferences. In fact, the manuscripts of Qumrân never speak of Eleazar, nor of Menahem, nor of Titus, nor of the Zealots or Masada; and conversely, the other texts which he cites never speak of the Wicked Priest, nor of the Rightful Teacher, nor of the 'Covenanters', nor of Qumrân. Furthermore, since he says that he accepts unreservedly the facts of archaeology, he is bound to interpret them to fit his thesis. Let us see whether he has succeeded.

I will now take up the various points that he has made, but in reverse order, so as to end with the point which in his eyes, and in mine, is the most important.

(1) Pp. 405–407. The archaeology of the caves proves nothing, because: (*a*) most of them had already been disturbed before the arrival of the archaeologists, (*b*) the date of the pottery is uncertain, and (*c*) new scrolls could have been deposited in old jars. I have already noted, above, the unlikely explanation which he offers regarding the use of these jars during the wanderings of his 'Covenanters' – Zealots.

Now for the objections: (*a*) If he had read the definitive publication on both cliff-caves and the terrace-caves of Qumrân (DJD III), and even if he had only read carefully the preliminary reports published in RB LX [1953], pp. 540–561; LXIII [1956], pp. 572–574, he would have known that caves 3, 5, 7, 8, 9, 10, had not been touched by clandestine diggers before the arrival of the archaeologists, and that the pottery that they contained is identical with that of the other scroll-caves. (*b*) The dates which I proposed for the pottery of Qumrân and of the caves have been accepted by all the archaeologists. They served as a basis for the only survey of Palestinian pottery as a whole of the first century B.C. and the first century A.D., and have been found in agreement with the discoveries on other sites. They have been confirmed by the excavations at Masada, where the group of pottery that is certainly Herodian has no equivalent at Qumrân, but fits into the lacuna corresponding to the reign of Herod, which I recognised between the pottery of Period 1b and that of Period II. (*c*) I have myself said that 'old manuscripts could have been put into new jars, and conversely that recent manuscripts could have been put into old jars' (RB LXVI [1959], pp. 91–92, and *Archéologie*, p. 79). But I also said: 'When we bear in mind that the manuscripts are numerous and the pottery abundant, and that the manuscripts form a single homogeneous collection and that the pottery all belongs to one period, it is difficult not to conclude that the manuscripts were deposited or abandoned in the caves at the same time as the pottery' (*Archéologie*, p. 79).

(2) Pp. 402–405. The connexion between the scrolls and the 'monastery'[3] as distinct from the caves, has not been proved: (*a*) the argument drawn from the religious content of the scrolls and from the religious purpose of the buildings is an *argumentum in circulo*; (*b*) the proximity of the caves has no significance; (*c*) the fact that jars were found in the buildings which are identical with those that contained some of the scrolls could be accidental; (*d*) the comparison of the handwriting of the scrolls and that of the ostraca in the buildings cannot prove anything, because the material on which the writing is done is not the same, and anyway the ostraca are not numerous enough.

To these I answer as follows: (*a*) The organised plan of the buildings, the common store-rooms, common workshops, common kitchen, common place of assembly and refectory and common cemetery, are signs of a community; the very elaborate system of water-supply and the orderly disposition of the large cemetery are signs of a disciplined community; the special religious rites which are manifested by the features of the tombs and the deposits of animals' bones, are signs of a religious community. I then observe that among the manuscripts in the caves there are religious rules, and moreover several copies of them, and also that the prescriptions laid down in these rules were capable of being carried out in the buildings of Qumrân (*Archéologie*, pp. 85–86). Is this arguing in a circle? Driver himself, after rejecting each one of the arguments in particular, recognises that, taken cumulatively, they do suggest a certain connexion between the caves and their contents on the one hand, and the buildings and their occupants on the other. The 'Covenanters' had lived at Qumrân, but those of their manuscripts which are later than the destruction of the 'monastery' cannot have been written at Qumrân, and if one of these manuscripts is found in one of the caves, the whole lot belonging to that cave must have been deposited there after the destruction of the 'monastery' (p. 405). Which of the two explanations is the most logically deduced, and the most in conformity with the archaeological facts? (*b*) With regard to the proximity of the caves, the richest, Cave 4, and its neighbour Cave 5, are but a stone's throw from the buildings, Caves 7 to 10 are on the terrace itself on which the ruins stand: is this of no significance? (*c*) That certain manuscripts had been deposited in certain jars may indeed have been accidental, but it rests with Driver to prove this. Meanwhile it is certain that the jars of the caves are contemporary with the jars of the buildings, and it is reasonable to admit that the date of the destruction of the buildings is also the date of the abandonment of the caves and the depositing of the scrolls. (*d*) It is exact

[3]In the course of his discussion of my conclusions, Driver often speaks of the 'monastery' of Qumrân: thus in 'quotes'. I am keeping the 'quotes', because I have never used the word when writing about the excavations at Qumrân, precisely because it represents an inference, which archaeology, taken alone, could not warrant.

that the comparison of the handwriting of the scrolls with that of the ostraca (and there are also inscriptions painted on the jars and the amount of evidence is greater than Driver imagines) is not decisive; but it must be remembered that the caves also yielded short inscriptions on the jars and an ostracon on which the handwriting is identical with that of the inscriptions in the buildings (*Archéologie*, p. 80).

(3) Pp. 400–402. The identification of Qumrân with the settlement of the Essenes, which Pliny the Elder fixes on the shores of the Dead Sea, is impossible, because: (*a*) although the translation of *infra hos* as 'lower down' could be stretched to mean 'further along' the coast, thus placing Engaddi to the south of the Essene settlement, the translation 'lower down' referring to altitude, is the only proper one, placing the Essenes not at Qumrân but above Engaddi; (*b*) archaeology has identified remains at Engaddi of the Roman period and contemporary with those of Qumrân.

My answer is as follows: (*a*) Driver quotes my words from a broadcast which I made from London in 1958, the text of which was afterwards published in *The Listener*; he also adds a reference to *Archéologie*, pp. 100–102, but this looks like an afterthought, and since this is the only scientific presentation of the matter, it is this that he should have consulted: but he seems not to have used it; (*b*) he would have found there the answer to his second objection, based on archaeology. He only uses the investigations of B. Mazar in 1950, and these did no more than touch the Tell el-Djurn, which was the headquarters of the Roman district of Engaddai, and which could not have been the settlement of the Essenes, who lived isolated above Engaddi, according to his interpretation of the text of Pliny. He should have taken into account the more recent exploration of the area round Engaddi, BIES, XXII [1958], pp. 27–45, and then he would have seen that there is no trace of any substantial occupation of the Roman period above Engaddi. On the problem in general he is not aware of the work of C. Burchard in RB LXIX [1962], pp. 533–569.

(4) Pp. 396–399. The hypothesis that the 'monastery' (Period II) was attacked and destroyed by the Romans in A.D. 68, and turned by them into a military post and maintained as such until A.D. 73 (Period III), is unproven because: (*a*) the reconstruction of Period III is much too rough to be the work of Romans; (*b*) such a military post would serve no purpose other than to keep watch on the coastal road which leads nowhere; (*c*) Josephus does not speak of an attack on Qumrân by the Romans; (*d*) if they had attacked, the Romans would surely have destroyed the buildings, and arrows would have been found outside the walls, and not gathered in certain rooms. Conclusion: 'The inferences ... are in the highest degree precarious, even impossible' (p. 398).

My reply is that: *a*) this military post was neither a legion's camp nor a permanent fort: it was a temporary accommodation for a detachment of auxiliary troops; (*b*) this military post kept watch on the

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whole northern half of the Dead Sea as well as the coastal road which leads to Masada, and moreover this watch was necessary as long as the siege of that fortress continued (*Archéologie*, pp. 33–34); (*c*) Josephus did not say everything; (*d*) all the buildings bear marks of violent destruction (*Archéologie*, p. 28), and the arrows were not found gathered in rooms, *pace* Teicher; some were found in the courtyards, and I think we can be pardoned for not having dug up the whole hillside outside the walls in search of others.

So Driver interprets as follows the facts that he accepts. The predecessors of the 'Covenanters' made the Qumrân site into a refuge from the persecution in the last part of the first century B.C. (this date corresponds archaeologically to nothing: should we read 'second century'?), and Qumrân was probably the first home of the 'Covenanters'. When a clash with the Romans became imminent, the pacifist members of the group (pacifist Zealots?) were evacuated with the elderly and sickly, perhaps to Damascus, and the militant members remained at Qumrân as an advanced post, while Masada served as their arsenal and base (the date of this whole operation is not given: it could only be the end of the year A.D. 66, when the Zealots settled at Masada). When, however, Vespasian and Titus invaded Palestine and even reached Jericho, the 'garrison'(?) of Qumrân withdrew to Masada, but upon leaving, demolished and set fire to the buildings (so there was a destruction after all?), or else alternatively a small Roman force burned or demolished the place which might have served as a refuge for the rebels in the event of their return (so the military nature of the destruction is not so unwarrantable? The date of all this is not given, but it could only be, according to Josephus, in A.D. 68: so the coins were not so useless after all?). When the main body of the Roman troops had left the area of the Dead Sea, a group of rebels would have returned and converted the 'monastery' into a fort (and would only have left behind Roman coins while their colleagues at Masada were using Jewish coins?). When Bassus and Sylva cleaned up the district, the little force would have found the position untenable, and would once more have withdrawn to Masada; the Romans did not raze the fort to the ground, because they found it empty (according to Josephus, the operations of Bassus and Sylva never came as far as Qumrân: their objectives were Masada, 30 miles to the south, the Herodium nearly 20 miles to the south-west, and Macherus in Transjordan). Which of the two explanations, Driver's or mine, is the most 'precarious' or the most 'impossible?'

(5) Pp. 394–396. The date summer A.D. 68, which I proposed for the destruction of Qumrân rests only on the evidence of coins. Now: (*a*) the fact that the Jewish coins of Qumrân cease in A.D. 68 does not prove that the Jews abandoned the place at that moment, for it is the moment when the Romans had besieged Jerusalem and the circulation of money was at a standstill; (*b*) if it is insisted that

coins of subsequent years were found at Masada, there were there only two coins of the year II and one of the year V, and their only significance is that somebody was there at a certain moment with those three coins; *c*) the Roman coins of Period III between 67–68 and 72–73 could have been acquired in plundering raids undertaken by the Jews. In conclusion, 'the evidence of the coins is consistent with either A.D. 68 or A.D. 73 for the abandonment of the buildings by the Jews and the occupation of them by a Roman garrison' (p. 396).

Let me begin with his conclusion. There is something new here: the acceptance of a Roman occupation – elsewhere strenuously rejected – but after 73, and an astonishing picture is thus obtained: before 68 the Jews are at Qumrân and, after coins of the Procurators and one silver coin of Nero of 62/63, there are only Jewish coins; between 68 and 73, the Jews are still there and there are only Roman coins; after 73 the Romans are there and there are no coins at all.

Now for the objections: (*a*) it is not exact to speak in a general way of the rarity of coins of the years IV (69–70 A.D.) and V (70 A.D.) of the Revolt. A distinction must be made between silver and bronze coins. Bronze coins of the year IV are common: the *Corpus* of Kadman includes 305, that is, twice as many as those of the year III. The silver shekels and half-shekels of the year IV are rare: about thirty examples are known, but of these at least five were part of a treasure found in 1874 at Jericho, not far therefore from Qumrân. The silver shekels of the year V (there were no bronze issues) are extremely rare: only ten examples are known, of which four come from Masada. (*b*) It is in fact wrong to say that only three coins of the Revolt were found at Masada. Driver is referring to the three bronze coins picked up at Masada before the excavations. To these should be added another chance discovery of bronze coins of the years II and IV, but most especially the coins found during the excavations: during 1963/64 season, in loc. 1045 numerous coins of the Revolt, among which a shekel of the year V, and, in loc. 1039, next to some manuscript fragments, many bronze coins and also a group of silver shekels: 10 of the year II, 2 of the year III, 2 of the year IV and 3 of the year V; during the 1964/65 season, numerous bronze coins and also 53 silver shekels and half-shekels, of which full details are not yet available. It cannot be admitted that, if Qumrân had at that time a Jewish garrison dependent on Masada, there should be no coins of these years IV and V, and only Roman coins. (*c*) When this has been said, the hypothesis that the Roman coins in Period III at Qumrân are the fruits of plundering raids carried out by the Jews seems entirely unwarranted and unhelpful.

All the objections that Driver makes against me are therefore fruitless, and I maintain my position. I can do no more here than summarise the demonstration which I have given, but which Driver seems not to have followed. The last coins of Period II of Qumrân are 83 coins of the year II of the Revolt (A.D. 67–68) and 5 coins of the year III (68–69). The coins of Period III that can be exactly dated begin with the coins of A.D. 67–68. It is obvious – and I have said this before – that the coins of Period II do not, *by themselves*, mean that this period finished in A.D. 68, and that the coins of Period III do not, *by themselves*, mean that this period began in A.D. 68. But since it is a fact that these coins are clearly confined to two superimposed archaeological strata, I concluded that this year 68 has a good chance of representing the end of Period II and the beginning of Period III. The end of Period II is marked by a violent destruction of all the buildings, and Period III is marked by a rebuilding which did not extend to the whole site and which brought considerable modifications to the plan and appearance of the buildings. I concluded that between Period II and Period III Qumrân had a change of occupiers. The coins of Period II are exclusively Jewish, and those of Period III are exclusively Roman. I concluded that the Romans replaced the Jews at Qumrân in A.D. 68. The last Jewish coins, those of the year III, had been put into circulation in the spring of 68 and only five have been found at Qumrân, compared to 83 of the year II. In June 68, as we learn from Josephus, the Roman troops, who had been quartered in 67 at Caesarea, occupied Jericho and their commander Vespasian visited the shores of the Dead Sea; the Roman coins of Period III at Qumrân, dated 67–68, had been struck at Caesarea and at Dora, quite near Caesarea, and they represent more than half all the coins of Period III. I concluded that June 68 was the date when the buildings of Period II were destroyed by the Romans, who then left a detachment on the site. The combined evidence of archaeology and the texts could not provide a proof that is more absolute, nor a date that is more certain.

This destruction marks the end of the Jewish occupation of Qumrân. The use of the caves by the Jews is contemporary with their occupation of the buildings, as the identity of the pottery shows, and the use of the caves ceased at the same time as the occupation of the buildings. Since the scrolls come from the Jewish community which occupied the buildings, and since it cannot be proved that they were brought at a later date to the caves, this same date also marks the depositing or the abandoning of the scrolls in the caves. No manuscript of the caves can be later than June A.D. 68.

Archaeology cannot say whether these writings are the work of Essenes, nor whether they are the work of Zealots; but archaeology does lay down a limit beyond which the commentator's researches cannot go: every hypothesis, which places the composition or the writing of the scrolls after this date, is wrong. From this point of view alone, Driver's theory is not 'as nearly valid as possible', as he says on the last page of his book it is impossible.