

Jewish-Greek conflicts in Eretz-Israel and in the diaspora could be examined both horizontally, for the same historical period in the different places, and vertically, from the age of Julius Caesar to that of Titus, or even later, to the beginning of the second century C.E.

Kasher's book opens new horizons to investigation: for this reason, too, we owe him our gratitude.

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J.J. Price, *Jerusalem Under Siege: The Collapse of the Jewish State 66-70 C.E.* (Leiden-New York-Cologne 1992), 361 pp.

Josephus' description of the Great War against Rome in 66-70 C.E. is the longest such account of any war of similar duration in antiquity. Price's book, which is based on his Ph.D. dissertation, is an excellent detailed study of Jerusalem during that war.

Price is very systematic in his approach. He starts the study with a discussion about rebels and aristocrats. This leads him into a detailed history of the struggle until the destruction of Jerusalem. After an epilogue there are fourteen detailed appendices in which he discusses various matters such as the *BJ* as an historical source, questions of the Jewish army, and archaeology. Price is a careful scholar who does not take any risks and does not tend to plunge into unnecessary scholarly or emotional polemics. His study is cautious and balanced and concentrates on the information available from Josephus' *BJ*. At times Price alludes to the war which was going on in Palestine, but the war in the rest of the country is really just background to the situation in Jerusalem during 66-70. Throughout his book the author attempts to introduce some "order" into the sequence of events during the four years of *stasis* in Jerusalem. It is evident that as a classicist he is aware of the phenomenon of *stasis* in the Greek world, but nevertheless he does not reach hasty conclusions as a result of facile comparisons, and this is one of the book's merits. Neither does he make elaborate comparisons with modern revolutions, such as the French or the Bolshevik. This again is in his favour.

My criticism of Price concerns only matters of interpretation; the Great War can indeed be discussed *ad nauseam*, because our only source remains the *BJ* of Josephus (with minor pieces of information in other sources, in particular Tacitus; cf. Appendix 2). I will give a few examples. First, Price puts a great deal of emphasis on the distinction between "revolutionaries" and "the Jewish leaders and high priests" at the very start of the war in 66, before the incident with Cestius Gallus (p. 31). This seems to me too schematic because the term

“revolutionaries” can, as Price himself must know from other revolutionary incidents in antiquity, include everything. Second, the first stage of the revolt, after the famous Temple meeting (pp. 51ff.), is depicted by Price as being led by a central government which operated from Jerusalem and functioned like the central government of a state (although at times he uses looser terms such as “mafia” to depict it). He is not alone in thinking that a real Jewish state operated in Palestine (recently Martin Goodman argued for this but in a much more extreme manner). On the one hand, one should admit that it is impossible to speak of a “state”, even in the classical sense of the word. I would perhaps use the expression “the leading group of the revolution”, because this leading group was not really a central government of a state with a central army, central administration and a strategy (*contra* Price). The minting of coins by local political entities can be found elsewhere in the Roman empire, and not necessarily in the context of war against the empire. On the other hand, there was indeed a short period after the crushing of the legion led by Cestius Gallus in 66 when one can, possibly, speak of a broad support for the war by aristocrats and priests. The rifts and feuds of the aristocracy in the late fifties and early sixties cannot be taken as evidence for Josephus’ “distortion”. Price argues that although the rifts and feuds between the aristocracy emerge from the *AJ* (did these exist at the beginning of the war?), Josephus wants to present the unity reached at 66 as a consensus of the aristocracy and the priestly order. I do not see any particular problem here: according to Josephus the aristocracy and the so-called high priests were split in the years before the war started, but then some of them wanted to do all they could to stop the war. When they did not succeed, most of the high priests and aristocrats very wisely decided to do all they could do from a position of strength — but not at any cost — to prevent an overall war from spreading (*BJ* 2.651, 4.319-321). Hence they managed to gain the support of the Zealots who did not participate in this regime.

Third, Price at times builds too much on a popular concept, held by many scholars, that Josephus distorted his account in certain places (Price in this respect is much more balanced than most historians who deal with Josephus). For instance, he accepts much of what Josephus relates about the second stage of the revolt (after the so-called Temple Meeting), but claims that there could not have been any sort of constitution (*politeia*) formed at the time by the revolutionaries (Price admits, however, that Josephus mentions the term “constitution” as well as other “institutional” terms within this context in the *BJ*, pp. 63ff.). But, he says, in the *Vita*, written in Rome long after the events, he *invents* a constitutional basis for this same juncture in order to whitewash himself, *i.e.*, to emphasize the legality of his own actions at this stage of the war. This argument, however ingenious, does not make sense. Josephus described certain elements which can be perceived as components of a “constitution” because he already thought in *BJ* that the central government had some sort of a legitimate position

(for example, the assembly in the Temple itself granted the government a great deal of political legality). Now if Price does nevertheless argue on the side of an unconstitutional base for the first regime, why does he then talk about a “state” and “central government”? On the other hand to use these latter terms throughout the description, and yet to say that the “Jerusalem government” “seems to have operated more in the manner of mafias than formally constituted and structured governments” (p. 67), seems to me incorrect.

I would claim that there is no doubt that the first regime created after the defeat of Cestius Gallus was moderate, and did not include the Zealots (nor the Sicarii), but because of this remained very weak. It did have some constitutional base (it started to mint coins and sent generals to different places in the country), but it was not a sovereign government in the sense of the regime which Simeon the Hasmonean created in 143/2 B.C.E. This is also the reason, I believe, why the generals who were sent out to the field very quickly ignored the leaders in Jerusalem who had previously sent them out. The activities of Josephus and Simeon bar Giora in the Galilee are famous examples of the poor authority exercised by the central regime; I would like to give yet another example.

The three generals who went to war against Ascalon did this on their own initiative; Price believes that they launched this operation as part of a grand strategy of the central government (pp. 72ff.). There is no evidence in the source to indicate that they received orders from Jerusalem. Price starts his section about “strategic planning” by saying that “if a coherent nation-wide strategy was designed by the leaders of 66, it is not to be found in the existing evidence” (p. 72). Fair enough; but on the next page he says concerning the attack on Ascalon: “In addition, the government’s strategy called for quick offensive strikes at Roman targets, such as that against Ascalon ...”. I do not think the government had any strategy at all besides the nomination of generals for the war, who were in any case “political” figures rather than eminent military experts. The initiative of the three generals, which proved to be futile, was not merely motivated by “hatred” (p. 73). I would suggest that Ascalon was the only place in Palestine which even during Alexander Jannaeus’ regime did not become part of the Hasmonean state. This may be the reason for the attack on this strategically unimportant city. But it was a local initiative and did not spread. Price may, however, be correct in his statement that “attacks on such places — as with the Ascalon attack, had it been successful — would have helped maintain the momentum of revolution” (p. 74).

Notwithstanding Price’s arguments (*e.g.*, p. 32), I still think that the central government in 66/7 tried to gain time and was ambivalent towards the war. Price suggests, along with some other scholars, that Josephus distorted its role, and that it is impossible to maintain that it played a double game. Against Price I would say that Josephus, for this stage of the war had no other material than his oral sources and the communications he himself had with the central regime, and

thus he could not report on the “other”, more secret channel of policies of the regime in Jerusalem aimed at avoiding the war. This would explain why this kind of information is missing in Josephus’ description; not that it did not exist, as Price wants us to believe (pp. 32-34). Thus we hear more of their overt policies; the regime of this first stage indeed started to prepare for war, perhaps as a last resort. This could easily be observed on the surface. But their hesitation also points to the fact that they propounded a more peaceful solution, which naturally could not be discussed openly, and Josephus, in the Galilee, could hardly know of this. The same holds true of a later stage, after the *coup d’état* where Price claims that Josephus had a task “set for himself: portraying committed revolutionaries as pursuers of peace” (p. 94). Again, during impossible situations like the one under discussion revolutionaries cannot be portrayed in black and white. Basically many of the aristocrats were “pursuers of peace” but were driven to adopt more aggressive policies because of the chain of events, and vice versa. A historian should know that at many junctures of history one finds people such as Polybius who are ambivalent, or not sure of themselves concerning the course they should take, or change their minds in the middle of some action due to pressure. Price and others censure these kinds of “contradictions” in Josephus, whereas I would say that they are a sign of Josephus’ trustworthiness which was at times undermined by misinformation, together with the imperfect editing of the *BJ*, rather than any deliberate intention to distort the facts.

Another point I would like to comment on is the messianism of the first century, which is commonly said to have been disparaged by Josephus. This is usually based on the assumption that Josephus wished to tone down or even ignore the messianism of Christianity (emphasized in the narrative sections of the New Testament, which were written during the same period that Josephus wrote his works). I am not sure that there is enough evidence to claim that Josephus distorted the facts here. One can merely say that in Josephus’ own circles present messianism (namely that the messiah had already come) was not a major issue. Price is very balanced on this problem (pp. 15-17), but nevertheless one gets the impression that he does go along with the idea that Josephus toned down messianism (which is associated with a king from the House of David). In line with Horseley and others he claims that the various pretenders after Herod’s death in 4 B.C.E. actually were messianic figures (p. 15). This is very doubtful. Whereas the Egyptian prophet or Theudas may have had some messianic overtones, there is no evidence whatsoever in Josephus’ accounts, in neither *BJ* nor *AJ*, that Simeon and Athronges “behaved as messianic figures”. They, *pace* Josephus, were the common pretenders found all over the Hellenistic world. The fact that they “crowned themselves” does not prove anything. Herod the Great was a king, but never — however megalomaniac he was — thought of himself as a messiah!

Price at times speaks of distortions — again, much less than scholars usually do regarding Josephus — and says at one point that Josephus “maintained

the true order of historical events while modifying them according to larger purposes" (p. 98). I do not think that this can be decisively proven, or that it has been proven in the narrative of the chapter ("The *Coup d'État*"). He also says that he does not believe that Josephus organized an army in the Galilee on the Roman model, and that this description (*BJ* 2.577ff.) was invented by him in Rome. The reasons given are: a) Josephus dealt with rebel groups and these groups show no signs of Roman organization; b) the "training program he boasts to have undertaken would have required far more time than he had"; c) an army trained in the Roman fashion would have needed much more equipment than was available to Josephus; and d) Josephus' troops did not show "much organization or fortitude in battle" (p. 60, n. 21). Price's statement that he does not believe Josephus in this particular matter seems to me somewhat dangerous as this can jeopardize the truth of many other details Price does accept as "facts" in his book. Why does Price disbelieve this, and yet believes many other details Josephus describes throughout the years 68-69, when the historian was kept prisoner in the Roman camp and had no direct evidence for what was happening in Jerusalem, as Price himself admits on p. 108? (This "fact", I believe, can also not really be proven). The weakness of Josephus' army and the poor equipment it had, as well as its swift disappearance the moment the Romans started their conquest of the Galilee, does not prove anything about Josephus' credibility. There is no reason to disbelieve Josephus in this particular matter. Roman legions passed through Palestine, as we know from Josephus himself, and Roman troops were stationed there all the time. It was easy for people like Josephus to study a cohort or a legion, albeit in a superficial manner. If the Essenes knew something about the Roman army (*1QM*), so could Josephus. One must add that we have reliable information about other revolts against Rome where the same thing happened, that is, revolutionaries attempted to organize their armies in a way similar to that of the Roman oppressor. Tacitus did not invent the revolutionary armies. That bandits were converted into a "national" army by the central "government" seems plausible to me, but cannot be proven. As said above, it was more a local militia than a real central "national" army; for example, Price himself admits on pp. 67ff. that there is no decisive evidence that the central government supplied the army with weapons.

In his Appendix 1 ("The *Bellum Judaicum* as an Historical Source"), Price concurs with all those who think that Josephus' apologetic purpose made him a cheap bluffer. Thus Price says that Josephus "explains the Jewish revolt just as this viewpoint would require: a relatively small collection of hardened, insane, godless rebels — militant activists — drove the nation unwillingly into war, persisting in their hopeless, self-destructive resistance, despite the efforts of the Jewish leaders (which included Josephus) to prevent war, and despite the many chances to save not only themselves but also the Temple and the whole Jewish people. The importance of this explanation for Roman propaganda but even

more so for the surviving remnants of the Jewish nation, especially the former ruling class, ensured that it would be stated clearly and often" (and here comes the statement by Josephus from *BJ* 1.10 and 5.442-4, pp. 180-181). Three points should be made here: first, scholars who deal with Hellenistic historiography know that general statements by historians in antiquity and their narratives are two different matters. Such statements should therefore not be taken too seriously by the modern reader. Second, there is no reason to doubt the above-mentioned description by Josephus, and that it was indeed a small collection of mad rebels who drove the nation unwillingly into war. During this war many changed their minds, because of the belief that success meant God's approval, and defeat His disapproval. Third, why would Josephus have presented such a "distorted" picture if he makes it very clear throughout his *BJ* that he himself participated in the war and fought the Romans? The latter were not interested in whether Josephus and his fellow aristocrats fought reluctantly or wholeheartedly. They fought them, and as Jotapata has proven, it was not so very easy for the Roman army to fight the historian Josephus. The narrative of Josephus should be taken — with the right critical reservations — at face value.

A last point should be made. Price wisely does not take the rabbinic sources as historical evidence for the war. Neither does he try to learn from them about the ideology which motivated the rebels. Hengel tended somewhat to the other extreme, *e.g.*, learning about Zealot ideology from late rabbinic sources. Price mentions *The Assumption of Moses* as a document for studying the ideology of the moderate group (p. 16). There exists, however, another composition which may possibly shed light on the ideologies of this period. I mean the *Biblical Antiquities* of Pseudo-Philo, which some scholars have already associated with this particular war, or with its aftermath. I believe that in this document one can trace some of the ideologies prevalent in the period under discussion (*cf.* my "Pseudo-Philo's *Biblical Antiquities*, the 'Fourth Philosophy' and the Political Messianism of the First Century C.E.", in *The Messiah*, ed. J.H. Charlesworth [Minneapolis 1992], 261-275).

To conclude. Price succeeded in his aim "to accomplish the Thucydidean first step, that is, to describe one 'event', the fall of Jerusalem during the Jewish rebellion of 66-70 C.E., in as much detail as possible, adopting a perspective different from that of most previous research ... " (p. 177). He has presented us with a solid balanced piece of work and with a coherent picture of these four stormy years. His description of the role of the Idumeans (pp. 89-94, and elsewhere), the different waves of desertions, and the conditions in Jerusalem during its two last years (pp. 102, 174) are, in spite of various reservations that I have on certain details of interpretation, done in a very detailed and clear manner. The book is well-written, and it was a wise idea to leave more lengthy scholarly discussions for the appendices, which makes it easier for the general reader to fol-

low the argument. It is an important book not only for Judaists but also for ancient historians and the general historian who is interested in revolutions.

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Rev. Peter Schenk, *Die Gestalt des Turnus in Vergils Aeneis. Beiträge zur klassischen Philologie* 164 (Verlag A. Hain, Königstein/Ts. 1984), 420pp.

It is unusual for a German Ph.D. thesis (Köln) to be published as a book within the year. It is also unusual that a reviewer takes up such a volume (in typescript, not print; the technique used apparently excluded the correction of the very numerous misprints) seven years after publication, when at least five reviews are already in print (*vidi*) and registered in *Aph*. The detailed summary by S. Harrison (*CR* 36 [1986], 40-3) and the telling remarks of E. Karaggerud (*Gnomon* 59 [1987], 61-2) exempt me both from a minute study of the text and from a detailed critique of its extraordinarily narrow and extreme thesis. I limit myself therefore to (1) a minimum of observations on Schenk's treatment of the book (7), upon which I have (mis-)spent, intermittently, twenty-five years (288-332 and four shorter passages), and (2) some even briefer remarks on Schenk's position and method.

(1) Schenk's German is lucid and his thesis simple. On the ideology and morality of the *Aeneid* he has read widely, especially in German and English. Thus (*e.g.*) on the use of *audax* in literature (28ff.) he is quite useful — or so it might appear until you realize that he has not read Hellegouarc'h's indispensable study of political terminology (*Le vocabulaire latin des relations et des partis politiques* [Paris 1963]); that he ignores Miss Taylor and Prof. Wirszubski goes almost without saying. You cannot understand Turnus without Catiline and Mark Antony. S. tells us nothing, either, of Turnus' helmet and shield, of his allies, or of the significance of the torch hurled at his breast (*cf.* 310). With the help of a concordance and a little ingenuity it is only too easy to impose a moral argument on the text of the *Aeneid*. Given industry and the resources of a good library that argument can be adorned with the appearance of learning. The result need, may, can have little to do with the actual text of the *Aeneid* as one struggles through its subtleties, ambiguities, allusions, depths, complexities. A few moral terms, or an analysis based on certain values, cannot be wrenched from its context and studied in isolation. S. has not studied *Aeneid* 7 in terms (*e.g.*) of the historiography of the outbreak of a civil war, or as an antiquarian reconstruction of military practice in primitive Italy. Eduard Fraenkel's classic article in *JRS* 1945 would have led him to both ideas, but Fraenkel's bibliography he has not followed up. Discussion of Turnus' previous "engagement" to Lavinia (296)