

A few assertions seem tenuous. He several times refers to the Polyphemus encounter as creating a rift between Odysseus and his crew, but this already occurs in the two preceding episodes. Chapter 7 is the least successful for me. Zeus, ruler of the cosmos, has, in the *Odyssey*, as in the *Iliad*, a big-picture view, compared with Poseidon or Hera, who usually cannot see beyond their emotional reactions. Is his vision a “competing” one, or a correcting one? An overreliance on Vladimir Propp’s “quest” formulation may limit consideration of interconnected episodes of greater length. Does Odysseus “take on features of Polyphemus” when he slays the suitors (pp. 69–73)? Does he get drunk, eat his guests, express contempt for Zeus? I don’t see it. B. offers intriguing analysis of Zeus, Poseidon, and Helios, not to mention Circe, but little focus on Athena, the deity most closely involved in the slaying of the suitors, which may skew some observations on Odysseus’ slaying of them. Is Eupheithes’ speech “representative of the Ithacan community at large” (p. 130)? I suspect not.

These very specific, and minor, criticisms notwithstanding, the study has significant strengths. Analyses of the *Odyssey* too often focus primarily on the *Apologue*; B. covers much of the poem. Rightly, he sees events on Thrinacia as key (“the offence of eating the Cattle of the Sun is much more serious than scholarship has tended to acknowledge,” p. 115): it is the most significant intersection of his two themes. His contributions on “interformularity” are noteworthy, and could be expanded and integrated into the rest of the argument. On occasion, especially in chapter 5, and in his considerable facility and dexterity in analyzing and juxtaposing the various dimensions of feasting and its intersections with poetic performance, B. himself performs a shamanistic function, leading us into previously un-glimpsed other-worlds in the *Odyssey*.

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Violence in Roman Egypt: A Study in Legal Interpretation. By ARI Z. BRYEN. Philadelphia: University of Pennsylvania Press, 2013. Pp. [x] + 363.

The phenomenon of low-level, interpersonal violence in Roman Egypt has long provoked scholarly interest, thanks to the survival on papyri of over a hundred petitions complaining about such acts.¹ After something of a hiatus in scholarly discussion, Ari Z. Bryen presents the first monograph focusing on these papyri—a focus that precludes detailed discussion of riots, revolts, or episodes of late antique religious violence attested in literary texts. The book is offered as an “historical essay” (pp. 7, 203, 207), which frees the author from having to fret about papyrological minutiae. Instead, he ranges across a wide terrain, with “violence” often serving more as a case study to illustrate general points about law and society in Roman Egypt. Following the analytical part of the book, Appendix B gives translations of all of the core texts on which B. relies, namely 135 petitions relating to violence from the Roman conquest to the early sixth century C.E. As one would expect from an essay, the argument is not always linear, and the language oscillates between the grandiose and the colloquial.

1. See esp. Roger S. Bagnall, “Official and Private Violence in Roman Egypt,” *BASP* 26 (1989): 201–16; Richard Alston, “Violence and Social Control in Roman Egypt,” in *Proceedings of the 20th International Congress of Papyrologists*, ed. Adam Bülow-Jacobsen (Copenhagen, 1994), 517–21; Brian McGing, “Bandits, Real and Imagined, in Greco-Roman Egypt,” *BASP* 35 (1998): 159–83.

One suspects that reading *Violence in Roman Egypt* will be something of a Rorschach test for papyrologists and Roman historians, revealing a great deal about individuals' attitudes toward evidence, their methodological preferences, and their tolerance for theory-oriented social history.

The introduction positions the book partly as one about the cultural and social history of violence: how it was practiced, how it was described and experienced by those involved, and how it impinged on interpersonal relationships. But we are also promised fundamental insights into what law meant for those who petitioned and litigated. The central thesis of the book is said to be that when individuals' social worlds become "overwhelming, unlivable, or intolerable" law can rectify the situation by offering a simplified framework and language that allows them to "rethink themselves and their relationships" (p. 3). A methodological standpoint is sketched, too. This involves reading petitions "seriously" and "sympathetically" within local and comparative contexts. B. judiciously eschews the use of comparative materials to fill evidentiary gaps, and prefers it as a means of illuminating differences between societies.

Chapter 1 ("Ptolemaios Complains") employs a second-century petition (*P.Mich.* 3.174) to introduce the attributes and potential uses of petitions, and to illustrate some key institutional features of the second-century legal system. The author expresses ambivalence about using them to say something about grand questions of Roman imperial history, such as official corruption and its regulation, and the impact and operation of law and legal rules in society. Instead, he suggests that the texts be used *to generate* theories—especially theories about individuals' perceptions—rather than to test pre-fabricated hypotheses (pp. 21–23). B. therefore takes an optimistic position on how historians' minds work: that we can let the sources raise their own generalizations, and to some extent avoid the unconscious imposition of our own a priori hypotheses.

Chapter 2 ("Violent Egypt") addresses the origins of the stereotypes of Egyptians as prone to sedition, crowd violence, and banditry that are found in Roman literary sources from around the start of the second century C.E. There is a (rather summary) rejection of the possibility that these were a reflection of the Alexandrian riots and Jewish revolts in the first and early second centuries C.E.; the long history of riot and revolt in the Ptolemaic period is ignored. Instead, B. points to the fact that there are substantially more published petitions from the second century than the first, and suggests that literary authors were responding to an increase in the overall quantity of litigation in Egypt.² This ingenious hypothesis will require a good deal more argument to convince. How did petitions about petty assaults give rise to stereotypes about crowd violence and sedition? By what process did the Roman authorities collect data about litigation rates over a period of many decades, and how was this then disseminated to literary authors? The balance of the chapter then discusses how the inhabitants of Egypt were integrated (or not) into the Roman administrative and legal order. This involves discussion of the official status categories imposed on the population, of administrative structures and ideals, and of the interplay between Roman law and local law. There are sound comments about the impossibility of discerning "violence levels" or "crime rates" from Roman Egypt; this distances B. from the crude "crime history" approaches of several earlier scholars of Egyptian violence.

2. It would perhaps have been more germane to have presented the statistics for the chronological distribution of violence petitions, with the archival distortions removed.

In chapter 3 (“Violence, Ancient and Modern”) B. introduces some of the words that he sees as signaling violence in the petitions. He then discusses the problem of just whose words these are, a problem raised by the fact that a high proportion of these documents was presumably written by scribes. The main concern of the chapter is how one might define “violence” in a way that makes it a useful analytical category. This involves a meandering tour through twentieth-century critical thought, but the fundamental observations are important. First, the tendency to view violence as a *magnum mysterium* that surpasses all understanding is a major impediment to serious analysis. Secondly, attempts to theorize violence have to deal with the fact that the threat of violence underpins many relationships of power and hierarchy. It is, therefore, hard to prevent the category of “violence” from expanding to the point of being meaningless, since so many social relationships can be analyzed as “violent,” especially if one dislikes some aspect of them (pp. 60–73). B.’s attempt to avoid this theoretical conundrum is to suggest that we should study “violence” not as an objective reality but as a label imposed by victim and observer, which depends on ethical criteria and the statuses of victim and perpetrator (pp. 74–79).

Next there is a discussion about what petitions have to say about the perpetrators of violence and the techniques and weapons that they used (chapter 4: “Narrating Injury”). A petition narrative is fruitfully suggested to be the outcome of a process of “translation” from the act of violence as it happened to the categories and commonplaces of the petition. Important for the argument is what is missing from the petitions. There are convincing observations about the lack of reported sexual violence and the tendency not to emphasize gender or ethnicity. B. also argues that while the line between slave and free was important, socioeconomic status differences between assailants and their targets did not matter to the authors of petitions (pp. 114–17). This claim jostles somewhat uneasily with the standard petition topos that the offender was a person of local power (pp. 96–100). As for descriptions of the assaults themselves, B. argues that “[t]he emphasis in these narratives . . . is largely a discourse of wounded bodies, and in particular, bodies with wounds that were publicly visible” (p. 120). Appendix B adds nuance to this: mentions of publicly visible injuries are rather rare before the mid-third century (4, 6, and 39), and only become somewhat more common after this (81, 96, 109, 110, 116, 117, 118, 121, and 127). One also notices that the vast majority of the 135 petitions say nothing whatsoever about which part of the victim’s anatomy was injured.

Chapter 5 (“The Work of Law”) is concerned with “what petitioners were trying to achieve when they wrote their narratives, with what those in charge of them were willing to do on their behalf, and with the results . . . of these encounters” (p. 127). Rightly rejecting suggestions that victims of violence petitioned only as a last resort to try to restore the status quo, B. produces evidence that shows that petitions tend to ask for punishment, or at least for court proceedings. He notes, however, that there is little evidence of people actually receiving punishments for violent acts, a silence that prompts him to see a paradox: if violence petitions tended not to result in punishments, why did victims submit them? Here the thesis mentioned in the introduction emerges: going to law served to redefine the relationship between victim and perpetrator, to “flatten” that relationship (pp. 139–40).

Discussion then reverts to the question of the different legal traditions existing in Roman Egypt, with emphasis on Roman judges’ unwillingness to apply Egyptian

law (pp. 143–48; cf. 47). The evidence for this consists of three cases cited in a single petition in which judges refused to apply one particular rule of “Egyptian” law; in the absence of other evidence, one wonders how typical the judicial treatment of this rule was. The discussion of legal pluralism also introduces us to a useful distinction made by the legal sociologist John Griffiths between “weak legal pluralism,” in which the state recognizes and allows different legal traditions for different sections of the population, and “strong legal pluralism,” in which semiautonomous social fields (e.g., professions, families, voluntary associations, etc.) regulate themselves according to their own “legal” or normative orders, independently from state authority (pp. 142–43). B. is of the view that in Roman Egypt written law concerning violence was fairly imprecise in defining what counted as an actionable violent wrong. This afforded the authors of petitions quite free rein in crafting their narratives, allowing them to participate in defining what issues were justiciable. This freedom is deemed by B. to be an element of “strong pluralism” (p. 164)—a drastic stretch of Griffiths’ category.

The final substantive chapter (“Fusion and Fission”) begins with the question: “What were people ‘doing’ when they used violence?” There is a polemical attack on the alleged tendency of ancient historians to see individual violence in terms of “feuding” behavior; some readers might detect the rustle of a straw man here. The later part of the chapter is given over to a detailed examination of four richly attested family disputes which involved attempts to settle the quarrel by contract, but ended in violence (and hence delictual, not contractual liability). The argument of the chapter again focuses on the role of law and violence in restructuring relationships, but the discussion (perhaps unintentionally) creates the impression that four different versions of the argument are being offered. At various points it is apparently claimed that (1) making a legal claim about an act of violence *in fact* tended to transform relationships by defining interpersonal relationships in terms of the categories of delict (pp. 190–91, 197, 199, 200, 205–6), and that (2) committing an act of violence *in fact* produced a similar transformation, because of the emergence of a delictual liability (pp. 178, 180, 191–92, 205). On the other hand, the argument sometimes seems to be that (3) petitioners *hoped for* such a transformation (pp. 197, 200–201), or that (4) the perpetrators of violence were animated by such a strategy (pp. 171, 178, 202).

The first two versions of the argument seem fairly uncontroversial, although one must remember that some victims of violence were simply not in a position to choose to enforce the rights created by the commission of a delict. One wonders whether the possession of an abstract, unenforceable right would have really transformed relationships. There are more substantial doubts in relation to the claims that B. seems to be making about the subjective mental states of complainants and attackers. As B. ably demonstrates in chapter 5, the authors of petitions talk of punishment and vengeance when they try to articulate the goals of these complaints. There is little language in the documents concerning the rethinking and restructuring of relationships. How do we know that we are not in the presence of the fruits of modern scholarly ratiocination, rather than genuine ancient *mentalité*? Further, any claims about *offenders’* strategies as they launched their attacks have even less textual support: as B. rightly points out on several occasions, we rarely have any texts from their perspectives. Yet he creates the impression that we are seriously being asked to believe that when disputants snapped and started hitting their opponents, they were most often thinking something like “it

would be splendid to transform my dispute, my relationship, and my position vis-à-vis this chap” (cf. p. 202).

Aside from stimulating discussion and debate on a series of detailed questions, B.’s essay raises two large methodological challenges that will be of interest to social historians of ancient law. One relates to the solution proposed to the difficulties with “violence” as a category of analysis. By adopting an essentially emic approach—violence is whatever participants and contemporary observers label as such—B. has found a way to stop the uncontrollable expansion of his category. But actually applying this apparently straightforward approach to a distant society turns out to be somewhat difficult. For one thing, in the case of Roman Egypt, one has to decide which Greek words can be translated as “violence.” We are given at least some of these in an ex cathedra pronouncement (p. 55), although it is clear from Appendix B that there are other words that B. would include on the list. Devising this working list of “violence labels” requires B. to invoke his own intuition about what violence actually is. Thus, he flatly declares that ἀηδία means “violence” (p. 55; cf. 154), even though the standard dictionaries give no such meaning.³ Ὑβρις for B. is always “violence,” even when purely verbal insults are involved (pp. 105–12). He deems threats to count as violence, even if they do not end in physical harm,⁴ but is less clear on false imprisonment, declaring that one complaint about such mistreatment (*PSI* 7.807 recto) does not involve violence (p. 312 n. 21), only to include it in his appendix of “petitions concerning violence” (doc. 86). On the other hand, βία concerns harm to financial and property interests, so for B. it is never violence.⁵ None of these choices is necessarily unreasonable, but the point is that each is the author’s choice: the dead are not labeling their actions and experiences as “violence” without the involvement of the living.

A second aspect of the work that will certainly provide a stimulus for discussion relates to B.’s desire to read the words of the petitions as the petitioners’ subjective responses to the wrongs they had suffered and the process of going to justice. Petitioners are repeatedly said to have “written” their petitions (pp. 76, 92–93, 127, 163) and to “speak” in them (p. 101; cf. 128). They are “savvy” in their complaints, drafting and redrafting them in a conscious and calculated way using legal language (pp. 155–56; cf. 57, 194). The violence narratives of these documents are said to be “a valuable source for how individuals living in an ancient empire chose to describe and classify themselves” (p. 84; cf. 91, 112, 205, 213). Yet, as B. acknowledges, there are very good reasons to believe that scribes wrote down most of these documents (pp. 9, 56; cf. 21–22, 65). Thus, how do we know (either in a particular instance or in the general run of cases) that what we see in petitions does not reflect ideas about how to narrate violence existing either in the mind of a particular scribe or in the prevailing legal culture more generally? The problem is especially acute when we remember a vital fact, passed over by B.: some petitioners who had the Egyptian language as their mother tongue would have known little or no Greek. They literally would not have understood the words appearing in their petitions.

3. See, e.g., LSJ, s.v. ἀηδία; *DGE* I, s.v. ἀηδία; Preisigke, *WB* I, s.v. ἀηδία, IV.1, s.v. ἀηδία.

4. See p. 109, with *BGU* 2.589 = Appendix 44; *P.Mich.* 18.793 = Appendix 115.

5. See p. 55. Note, however, the rather different understanding of βία in the petitions offered by Roberto Mascellari, “La descrizione di atti criminosi e violazioni nei papiri: ὕβρις, αἰκία, πληγαί, βία,” in *Recht haben und Recht bekommen im Imperium Romanum: Das Gerichtswesen der Römischen Kaiserzeit und seine dokumentarische Evidenz*, ed. Rudolf Haensch (Warsaw, forthcoming).

B.'s solution to the problem of scribal involvement is to state that "[c]ertain kinds of legal boilerplate show up in all petitions, but to attribute this to the hand of the scribe alone is to enter dangerous territory" (p. 63). Quite right. But it is also to "enter dangerous territory" to attribute the words or rhetorical strategies in a particular document to the petitioner, yet B. does precisely this throughout the book. The fact is that, in the vast majority of cases, we just do not know whose subjective mental states we are seeing in these texts, or if certain phrases or narrative features were repeated through sheer force of habit. The fact that B. produces two private letters (p. 64), each with a word that he deems "legal" (without any argument about what identifies a word as "legal"), does not prove much about the extent to which the general public could deploy legal jargon—especially since private letters were sometimes written by paid scribes as well. Thus, if petitions are to be used to peer inside the minds of petitioners, better arguments will need to be found.

The above reflections on *Violence in Roman Egypt* have suggested, I hope, that B. has produced a stimulating and thought-provoking book. Work needs to be done to clarify and provide evidentiary support for his detailed arguments, and to strengthen the claims he makes for the meaning of his evidence. But the purpose of an essay is surely to provoke debate and raise new research questions. This essay will certainly achieve such a purpose.

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Greek in Talmudic Palestine. By DANIEL SPERBER. Ramat-Gan: Bar-Ilan University Press, 2012. Pp. 266.

The systematic study of the numerous Greek and Latin loanwords found in classical rabbinic literature, written between the first and the seventh centuries C.E., began in earnest in the eleventh century, when Rabbi Nathan ben Yehiel of Rome wrote his magisterial *Arukh*. This alphabetically arranged dictionary-cum-encyclopedia made use of earlier glossaries and commentaries on rabbinic literature, and of R. Nathan's own extensive knowledge of Latin and Greek, to which he had been exposed as a Jew living in Italy.¹ His work became an essential tool for all medieval and later readers of rabbinic literature, most of whom had no knowledge of the classical languages, and therefore no other way of dealing with the endless stream of transliterated Greek words, and less numerous Latin words, found in their central religious scriptures. In fact, it was one of the first Hebrew books ever printed, and supplements to it were written from the twelfth to the nineteenth centuries.

With the rise of the *Wissenschaft des Judentums* in the early nineteenth century, one of the scholarly desiderata for the scientific study of the classical Jewish texts was a new dictionary of Greek and Latin words in rabbinic literature, one that would make use of all the tools of modern textual and philological scholarship. A first major step in this direction was taken in the 1880s, when Alexander Kohut produced a critical edition of the *Arukh* that has not been replaced since.² A second step was

1. For Nathan of Rome and his *Arukh*, see Shifra Sznol, "Medieval Jewish Greek Lexicography: The 'Arukh' of Nathan ben Jehiel," *Erytheia* 30 (2009): 107–28.

2. Alexander Kohut, *Aruch completum, sive lexicon vocabula et res, quae in libris targumicis, talmudicis et midraschicis continentur, explicans* (Vienna, 1878–92).