exclusively in Valentinian's reign — and indeed Ammianus would be guilty either of serious error or an extraordinary and wilful deceit if the authors' chronology were correct on this point.

The most unequivocally successful aspects of the commentaries are philological: in explaining usage, in detailing intertextuality, in exploring the nuances of pronouns they cannot be bettered. There are many fresh observations, including at 28.4.21 the fact that editors have printed a sentence with no main verb, simply two present participles: perhaps an authorial error? I turn to their textual choices. As in the previous volumes, Den Boeft *et al.* diverge frequently from the standard Teubner edition of Seyfarth from which they take their lemmata. I counted over sixty divergences, excluding patently corrupt and lacunose passages where they reject overly optimistic attempts at rescue (there is a marked increase in such passages in Book 28). At only three points, by my count, do they vindicate the manuscript reading of the Vaticanus against other readings printed by Seyfarth (27.1.2, 28.2.4, 28.4.28); at another dozen they argue for readings of Gelenius' edition of 1533, which may represent either the readings of the lost Hersfeldensis or simply his conjectural acumen. In just over forty they argue for the conjectures of others (ten by Petschenig, six by Henri de Valois), and they make about ten conjectures of their own (personally I would alter his text still further). In half a dozen or so cases where they disagree with Seyfarth, Ammianus' prose rhythm, which is remarkably regular, is mentioned as favouring their change, but in another half dozen cases, they do not mention the fact that their solutions repair the rhythm. At 27.7.7 their solution breaks the *cursus*, but justifiably, given Ammianus' practice in pithy excerpts of direct speech. There are also places where *cursus* should have been taken into account and was not: at 27.4.10 in favour of Clark's <*de*>*fluentem*; at 27.7.9 perhaps tipping the balance in favour of Adrien de Valois' *efficere* rather than Madvig's *effici*; at 28.1.37 as an obstacle to their proposed punctuation. Whereas some of their disagreements attest Seyfarth's perverse conservatism more than their good judgement, there are countless astute choices and some outstanding conjectures: at 28.1.22 *tutus* for V's *tectus*, while rescuing the ms reading *tectius* a line before; at 28.1.47 *coartato* for *V*'s *contracto* makes lurid sense of a Roman matron's suicide by self-suffocation. Of course, my focus on emendation does not mean that they do not just as often explain the unexplained: for example by identifying *eiusdem* in 28.1.27 as Lollianus mentioned in 28.1.26 (the two sentences therefore should form a single paragraph). I read through the commentaries while writing a translation of the two books, and can rarely remember learning as much about Latin in as short a time.

A few minor corrigenda. 27.3.9: Gelenius' reading is not *fremitu* but *fremituque*; 27.3.15: lemma and commentary have been accidentally duplicated from 27.4.14; 27.5.9: Augustus' grandson Gaius Caesar is confused with his namesake and nephew the emperor Caligula; 27.6.2: the emperor Gratian is better described as 'assassinated' than 'executed'; 27.12.2: the praetorian prefect 'Sallustius' (or to be precise, Saloustios) described in John Lydus, *Mag.* 3.51.6–52.4 should have been identified with Saturninius Secundus *Salutius*; 28.2.10: the villa Murocincta, normally identified as Parndorf near Vienna, is certainly nowhere near Sirmium. The authors probably assume that readers will have a critical text, but if they do not, they will not realize that at 27.2.6 *insueta* is the reading of Accursius and Gelenius, not C. F. W. Müller's conjecture; at 28.2.4 *His* is not added in Gelenius' edition but *is* a conjecture by Müller; and at 28.1.38 Valesius' conjecture *implacabilitate* is anticipated by the scribe of manuscript E.

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K. HARPER, *SLAVERY IN THE LATE ROMAN WORLD, AD 275–425*. Cambridge: Cambridge University Press, 2011. Pp. xiv + 611, illus. ISBN 9780521198615. £85.00/US$140.00.

Weber, Bloch, Finley, de Ste. Croix, McCormick, Wickham: the decline of the Roman slave system in Late Antiquity has been central to every major modern account of the transition from antiquity to the Middle Ages. Kyle Harper's monumental study of late Roman slavery hence has implications far beyond its ostensible focus on the 'long' fourth century (A.D. 275–425). Lucid, sophisticated, and beautifully written, it deserves the widest possible readership.

The place of slavery in the late Roman economy is the subject of Section I (1–200, chs 1–4). H. plausibly argues that slaves made up some 10 per cent of the population of the fourth-century

Roman Empire (5,000,000 in total), half of them owned by élite senatorial and decurial households, the other half by 'middling' urban and rural households (38–60). One of the great strengths of the book is H.'s awareness of the differences in kind between these two broad classes of slave-owners: the kinds of benefits that one or two slaves brought to a well-off peasant household (128–41) had little in common with the economics of slave labour on huge dispersed decurial estates (162–79).

H.'s account of the late Roman slave-supply (67–99) makes excellent use of a fourth-century tax-register from Thera (first published in 2005) listing 152 agricultural slaves with their names and ages. The age- and sex-profile of this sample (numerous children, and a ratio tilted towards females among slaves over the age of thirty) is taken by H. to reflect an agricultural workforce 'shaped by natural reproduction, family life, and some adult male manumission' (76). This would offer striking support for Walter Scheidel's hypothesis (*JRS* 87 (1997), 156–69) that natural reproduction within the household was the most important single source of Roman slaves. However, a sceptic might reasonably point to the prevalence of child exposure and infant sale in the eastern Roman Empire (78–83, 391–423), which could easily create a 'bottom-heavy' age-profile of this kind without the need for large-scale slave breeding.

In his enormous chapter on agricultural slavery (144–200), H. explains the persistence of large-scale slave-ownership in the Late Empire in terms of four determinants: 'supply, demand, formal institutions, and the dynamics of estate management' (152). Slavery flourished because it represented the most efficient way of maximizing household revenues within a diverse labour-market: 'land-owners *could choose* from three categories of labor: slavery, tenancy, and wage labor' (156, my emphasis). When a fourth-century land-owner weighed up direct costs (slave prices vs wages) against transaction costs (efficiency, the need for oversight, the seasonality of Mediterranean agriculture), slavery would have presented itself as the 'rational' choice. Whatever one thinks of this unashamedly neoclassical approach — the influence of the New Institutional Economics is patent — H.'s account is the most coherent and sophisticated analysis of the economics of ancient slavery that we currently possess. It raises the bar.

Section II (201–348, chs 5–8) is concerned with the subjective experience of Late Roman slavery. It may not be news that slaves and free men ate different kinds of bread (237), that a slave was forbidden to look his master in the eye (332), or that an owner expected to know the complete sexual history of his slave-women (295). But thanks to the massive wealth of fourth-century homiletic evidence, the social realities of the Roman master-slave relationship have never been better documented, and seldom more vividly evoked, than they are here. One of the things that comes out most strongly is the fundamental *compatibility* of Christian ethics with slave-ownership (212–14). As H. shows, the Christian critique of slavery focused on the ethical consequences of mastery, not on the human rights of the slave (300–4). If the sexual exploitation of slave-girls was a bad thing, that was because Christianity emphasized the value of sexual exclusivity *tout court*; it was the spiritual welfare of the slave-owner, not of his human property, that worried Augustine, Gregory of Nyssa and John Chrysostom (346).

Legal norms concerning enslavement and manumission changed radically in the course of the fourth century, but H. is unwilling to use this as evidence of social change: 'in late antiquity, the values behind public law changed less than the mechanisms used by the state to enforce those values' (431). As a result, Section III (349–494, chs 9–12), on the pronouncements of the Late Roman state on slavery, is 'more about change within the state than change within society' (423). So the increased willingness of Constantine and his successors to countenance the sale of children into slavery does not mark a change in social relations, but 'is simply a reflection on the material limits of the state's power and its unwillingness to exert great energy in this arena of social activity' (414). Likewise, when Constantine permitted the re-enslavement of freedmen, he was simply bringing Roman statutory law 'into harmony with the natural balance of social power' (488). As a result, the legal sources add less to our picture of fourth-century slavery than one might have hoped: neither Christianization, nor changes in the Roman status system, seem to have had any significant impact on the public law of slavery.

H. is better at illustrating the vitality of the fourth-century Roman slave system than he is at accounting for its dramatic fifth- and sixth-century decline. Readers seeking a new interpretation of the end of ancient slavery, or of the transition from antiquity to feudalism, will be disappointed. For the West, H. argues (following Chris Wickham's *Framing the Early Middle Ages*) that the decline of estate-based agriculture and long-distance commodity exchange was the decisive factor: 'Roman slavery was situated in the sectors of the economy most influenced by elite land-ownership and market-orientated production' (198). Hence, when inter-regional

Mediterranean exchange declined sharply in the mid-fifth century, large-scale slave-ownership simply withered away. For the East, where it is harder to show economic simplification in the fifth and sixth centuries, H. tentatively suggests — true to his 'rational-choice' model — that population growth among the free peasantry may have rendered slave production 'less attractive, less necessary' (506). But this 'demand-side' explanation of the end of Roman slavery only works (if at all) for large-scale estate-based agricultural slavery. What induced the millions of fourth-century 'middling' slave-owners, in city, town and village, to give up the weavers, nurses and pig-keepers on whom their social status depended? A supply-side crisis must surely be part of the answer; but where does that leave the thesis of a self-reproducing Late Roman slave population?

This is the most important book on Roman slavery to appear in many years. No social or economic historian of the Roman world or early medieval Europe can afford to ignore it.

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J. HAHN (ED.), *SPÄTANTIKER STAAT UND RELIGIÖSER KONFLIKT: IMPERIALE UND LOKALE VERWALTUNG UND DIE GEWALT GEGEN HEILIGTÜMER* (Millennium-Studien 34). Berlin/New York: De Gruyter, 2011. Pp. 227, illus. ISBN 9783110240870. €79.95.

Published in 2011 but originating in a conference held in Münster in 2005, this volume illustrates the ever increasing interest in religious violence in Late Antiquity. It focuses on a classic subject, the destruction of temples, which, as the introduction argues, symbolizes the violent side of the transition from a pagan to a Christian world in Late Antiquity. One can take issue with this starting point (see L. Lavan's introduction to L. Lavan and M. Mulryan (eds), *The Archaeology of Late Antique Paganism* (2011)), but the volume approaches the topic from an original angle: how did the Roman state respond to the destruction of temples, and more broadly, to religious violence? For such a research question much hinges on what one understands by 'state'. In particular, Anglo-Saxon scholars such as J. Harries, C. Kelly, F. Millar, and J. Matthews have underlined the complexity of the functioning of the later Roman state. Even if emperors did have some general ideas and intentions, government often happened through response to specific situations. Moreover, the 'state' was composed of different, often competing, actors such as the emperor(s), palatine officials, governors, and generals — to name but a few.

This complexity is well brought out by E. Meyer-Zwiffelhoffer and J. Hahn. Meyer-Zwiffelhoffer offers what may well become the definitive treatment of the rôle of the governor in religious conflicts. Because of their precarious position within the state structure (often embattled in their province but envious to rise in the ranks), they were generally hesitant to execute imperial laws that might cause unrest. Hahn cautions against using the Theodosian Code as evidence for imperial policy, as it creates an idealized, *post-factum* image of imperial actions. At the same time, laws were needed as bishops who wanted to proceed against temples needed legal justification. Not everyone in the volume, however, takes this methodological perspective on board. G. Bonamente's useful catalogue of fourth-century policy on the confiscation of temple land by the state seems predicated on a reading of the Codex Hahn counsels against. In line with much recent scholarship, violence is especially attributed to the actions of Christian bishops, thus generating the image of a passive state that responds to new social groups. This is most strongly put by U. Gotter, who draws a contrast between the 'imperial tradition' that rejected violence as a solution for conflict and the later Empire that condoned acts of violence from Christians. He then relates this contrast to the opposition between an inclusive paganism and an exclusive Christianity. The contrast seems overdrawn (see, e.g., the events in Alexandria under Claudius and Caligula) and neglects the fact that the ancient Church also principally rejected violence outside the context of warfare (illustrated by Libanius' jibe that Christians should not become governors because they do not want to execute anybody (*Or.* 30.20)). It may simply be the consequence of the scope of the volume that the complexity on the side of the Church is underestimated: violence against the 'other' is not as natural as it may seem. Moreover, the essay of Bonamente provides evidence that emperors and high officials could condone acts of violence and even initiate them, thus suggesting that their attitude was not always so passive. An argument in this direction was provided by N. McLynn, 'Christian controversy and violence in the fourth century', *Kodai: Journal of Ancient History* 3