

CHAPTER TWO

MODELS AND EVIDENCE

IN THE STUDY OF RELIGION IN LATE ROMAN EGYPT

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The title of this paper may seem too restrictive for an opening lecture in a colloquium concerned with the entire East of the Roman Empire, in which only half of the papers concern Egypt, and yet simultaneously far too ambitious in its scope. In the course of the colloquium, however, it became clear that the methodological issues that I was trying to confront were broadly relevant across the geographical span covered by the colloquium and to some extent raised fundamental questions about the very formulation of some of the organizers' questions to participants, questions which came back into focus in the lively concluding discussion. This in my view was to be anticipated, because part of my argument is that Egypt is not any more exceptional than anywhere else in the Roman Empire of late antiquity, and that the questions at stake there are broadly applicable, even if the answers vary.

I should begin by recording two rather different debts that this paper owes to the work of other scholars. The greater one is to David Frankfurter's *Religion in Roman Egypt*.¹ This book appeared four years prior to the colloquium in Münster, early in a semester when I was teaching a class on the social context of Egyptian Christianity to a seminar at Union Theological Seminary. It was a great source of stimulation for that seminar. As readers will find, there are many fundamental assumptions and arguments in that book with which I disagree;² but it was reading it that led me first to formulate some of the methodological points set out below and to force myself to clarify matters that I had left until then unanalyzed. This is a stimulus for which I remain grateful. The second obligation is to the brilliant conference paper given by [p. 24] Stephen Emmel at a colloquium in Leiden later that same fall entitled "Perspectives on Panopolis," the proceedings of which were published just before the Münster colloquium.³ The analysis of Shenoute and Gessius – two central characters in a late antique real-life drama – given in that paper embodied and exemplified many of the precise points that I had been thinking about for the preceding months and helped move them from pure abstraction to exemplification. Gessius will come back a number of times in my remarks; but I should in fairness add that Professor Emmel is in no way responsible for what I have made of his analysis.⁴

¹ FRANKFURTER 1998.

² Since Frankfurter's book is at many points written in opposition to chapter 9 of BAGNALL 1993, this is unavoidable.

³ EMMEL 2002.

⁴ See also EMMEL's paper in this volume.

Models and evidence: they make a nicely balanced pair, but I have never found it rewarding to think of them separately. As a product of the American pragmatist tradition, I tend to assign utility to models only to the extent that they help me to make sense out of both the evidence that exists and the evidence that does not exist.⁵ Before turning to more specific subjects, I should say that I remain convinced of the centrality of not only an accurate and critical treatment of the evidence, but serious respect for explaining the contours of that evidence, even though such concern is likely today to be disparaged as “positivism.”⁶ I cannot [p. 25] resist quoting Robert Darnton’s description of the late Lawrence Stone, a prominent historian of early modern British society:⁷

He understood history in the British manner, as argument from evidence – endless argument, from boundless information available in archives. Although he loved to pilfer, as he put it, from the social sciences and claimed some mastery of theory, Lawrence conceived of knowledge as ultimately grounded in facts. . . . He did not use “positivism” as a pejorative. He even described himself as “the last of the Whigs.” Far from suffering from epistemological *Angst*, he scorned postmodernism and warned that historians would sell their birthright if they mixed fact and fiction.

I shall begin with a subject where models and evidence are inextricably related. This is the fact that our literary evidence for the entire subject of this colloquium is, with rare exceptions, made up of Christian literature. Indeed, this is true not merely for the destruction or reuse of temples, but for the entire shape and character of the relationship between Christian religion and the traditional religions of the Eastern Mediterranean world in late antiquity. Except for a handful of intellectuals, who cannot safely be taken as representative of anyone but themselves, we have only Christian depictions of – of what? Even the choice of a term like struggle, renewal, destruction, transition, or anything else betrays an evaluation made possible by looking back on the events of late antiquity, something not possible for contemporaries.

⁵ The term “model” is used in a considerable range of meanings, from explanation to Weberian ideal type to middle-level theory. I am speaking here mainly of the last of these.

⁶ Misuse of evidence is, regrettably, a pervasive feature of Frankfurter’s book. One among many striking examples, and relevant to the subject of the present volume, is the claim (FRANKFURTER 1998, 122) that the Thoëron of Oxyrhynchos was still active in 462, “when a symposium is to be held in it. The space evidently continued to maintain at least some holiness. The Thoëron had been the site of ritual dinners through the third century, so a symposium may not have constituted a break with the temple’s ritual tradition.” This was evidently borrowed without verification from QUAEGBEUR – CLARYSSE – VAN DER MAELE 1985, 225, which Frankfurter cites. In reality, the document in question (PSI III 175) is a lease of a *symposion* in a house located in the sanctuary of Thoëris for an indefinite period on a monthly basis. A *symposion* in such a context simply refers to a room in a house, often used for habitation but sometimes for other purposes; see HUSSON 1983, 267–271. The lease provides no information about the intended use of this room, and there is no basis for the assertion that a “symposium” was to be held in it. References to sections of cities by the names of temples located there are *not* evidence for the continued activity of those temples. A beautiful example occurs in a lease from Hermopolis, dated to 555, in which one of the lessors is “the Holy Martyrion of the Three Holy Martyrs of the Holy Sarapeion”: the Sarapeion, which has picked up the title “holy” from its newer occupants (the three youths in the furnace of Nebuchadnezzar, from Daniel 3), is not validated as an operative pagan sanctuary in 555 by such an attestation. This papyrus (P.Vindob. inv. G. 13288) has been published by me as *P.Horak* 10 (see BAGNALL 2004, 54–57, p. 7).

⁷ DARNTON 2001, 382–383.

This fundamental characteristic of our evidence should be a matter of deep concern for all of us. There is, to be sure, documentary and archaeological evidence to contribute to the dossier, with its own set of opportunities and difficulties, to which I shall come in due course. But the difficulties involved in interpreting a literature that is largely rhetorical in character, whether in sermons or hagiography, are very great, and in my view they have not been taken seriously enough by many scholars working in late antiquity. That is, I find much scholarship that uses Christian sources to reconstruct the state of paganism and, even more, of Christian-pagan relations to be remarkably reticent about asking rude questions concerning the objectives and methods of the ancient Christian writers who created these texts. On some fronts this has begun to change; for example, Malcolm Errington has recently demonstrated that Christian writers, and non-Christian writers as well, no matter how much they talk about imperial religious policies, are [p. 26] almost without exception completely ignorant of the actual imperial legislation on the subject of religion issued in their own period or that preceding them.⁸

The general reluctance to deconstruct Christian literature is a curious phenomenon, one which seems to me strikingly at odds with the otherwise widespread suspicion, in our postmodern age, of the trustworthiness of Christian sources, indeed of all sources. Why is it not “triumphalist” – the most vicious accusation one can level nowadays – to swallow whole a Christian writer’s claims about the strength of the pagan opposition that has been beaten down? Arietta Papaconstantinou has pointed to the large element of exaggeration in Christian claims about the destruction of temples.⁹ Is it really so much more difficult to understand that magnification of the strength of the opposition is largely a rhetorical ploy to exaggerate the accomplishments of one’s hero or, for that matter, oneself? I find the tendency to accept such statements at face value naive in the extreme. Such texts are indeed historical evidence, but evidence for themselves as cultural artifacts and for the author’s own context and objectives; use of them as evidence for some other historical events that they purport to describe must proceed with extreme caution.

It is true that the past generation has shown signs of the rise of a more critical attitude. In a recent article, Ewa Wipszycka has described the point from which this change of attitude proceeded.¹⁰ It is worth quoting her remarks, because she knows the Christian literature of Egypt as well as any scholar and has used it with care and insight in a series of recent studies:¹¹

Il fut un temps, pas tellement éloigné du présent, où l’historien exploitait à son aise les textes hagiographiques, sans se soucier de leur caractère littéraire. Généralement admise comme licite, cette approche semblait particulièrement justifiée dans les cas où l’on savait quand et où le texte donné était né et où l’on pouvait avoir l’impression que le récit n’était pas trop chargé de ces *topoi* qui traînent à travers la

⁸ ERRINGTON 1997b. Ulrich GOTTER (in this volume) has suggested that the “ignorance” is in fact part of a narrative strategy in some authors.

⁹ PAPACONSTANTINO 2001, 244–245. She notes acutely that a distinction is to be made between the earlier sources, where competition between religions is still a live issue, and later ones, where competition with other shrines is more prominent.

¹⁰ WIPSYCKA 2002, 61.

¹¹ Many of them now collected in WIPSYCKA 1996.

littérature hagiographique. Une fois les miracles mis de côté, le reste des informations contenues [p. 27] dans la vie d'un saint pouvait être directement utilisé pour reconstituer, sinon la biographie du saint, du moins les *realia* de l'époque où la vie avait été composée.

Wipszycka follows this description of the benighted attitudes of the past (but hardly only the past) with a far more critical engagement with the life of John the Almoner, in which she demonstrates that the author did not actually know the subject of the life and that there is thus no eyewitness value to be attributed to his statements. What then, she asks, are we to make of the purported information about the economic life of the Alexandrian patriarchate contained in the life? Her own method, which she explicitly describes, is to compare this information with our other evidence for the same subject, to see if it is inherently credible. She concludes, “Son ouvrage [the life of John] est certainement une source précieuse, mais pour pouvoir utiliser ses informations, il faut effectuer continuellement des opérations critiques, il faut examiner de façon critique son récit chapitre par chapitre, phrase par phrase.”¹²

There is, however, a logical problem inherent even in Wipszycka's critical method which she does not consider, one of circularity. In short, the information in the life of John the Almoner is usable only on her view if it is confirmed by other, more reliable, evidence. But if it is, then is it of any real use? In other words, it can be trusted only when it tells us what we already know and cannot be used for the addition of any new knowledge, or at least no new knowledge that challenges anything we already knew. It is not of any independent value for the economic condition of the patriarchate in John's time. What it can tell us is the interests, motives, and strategies of its author, and what that author thought he might say without his readers finding the background color implausible. At best, then, it tells us about what readers and hearers might have found plausible in their own time. Given the gross distortions published every day in the contemporary press, to an audience with far readier access to information than those who would have heard or read a hagiographic work of this sort in late antiquity, that constraint of plausibility can hardly be taken to be very strong. In general, information that is not the focus of the narrative is more likely to be reliable than anything relevant to the point the author is making, but it is impossible to obtain reliable information by deciding that some numbers in the life are inherently possible and others are not. [p. 28]

The *appropriate* use of literary works is a point on which Emmel's treatment of Shenoute and Gessius advances the level of sophistication markedly, as he shows that the label “pagan” affixed to Gessius was not, as most scholars have assumed, an element in Gessius's self-presentation, but instead a description that Shenoute kept insisting on applying to Gessius despite the latter's own profession of Christianity – part of the author's strategy in local politics and in his writings, rather than part of any independent reality. I would indeed go farther and ask how secure the inference is that Gessius had *ever* actually seen himself or portrayed himself as a pagan at any stage of his life. This point returns in Emmel's paper in this volume.

¹² WIPSYCKA 2002, 80.

Two fundamental aspects of our model, whether stated or unstated, of the religious world of fourth-century Panopolis emerge from the questioning of the rhetorical, self-serving character of Shenoute's portrayal of his opponent as a pagan. The first is the recognition that the kind of strategy employed by Shenoute is characteristic of Christian polemical discourse across time and space. I cannot enter here into a broad exploration of this question, but Christian polemicists have long been fond of depicting their adversaries in extreme terms. A mild doctrinal disagreement suffices to label the opponent a heretic, a larger one earning the term pagan or, in the case of some doctrinal tendencies, a Jew. These labels are terms of abuse, not sober historical or even theological description, and we will be prudent to regard them as evidence for the writer's rhetorical strategy rather than for the self-identification of the targets, let alone some supposed objective fact.

We should also bear in mind a second characteristic of Christian writing, its fondness for certain types of stories. Averil Cameron has written persuasively about the tendency of Christian texts to tell "stories people want" to hear.¹³ These narratives form a domain of discourse popular in preaching and instruction. The most appropriate object in the study of such stories is not factual information contained in the narratives but the social role of the narratives themselves. It is urgent that we should look at the hagiographic literature from the point of view of its own imperatives, rather than according to the stories it tells a status like that of a defendant in Anglo-American law, believable until proven false. Several of the papers in this volume, indeed, [p. 29] identify elements in the temple-destruction narratives as playing a role in intra-Christian discourse.

More generally, Christianity in its diachronic sweep is a necessary context for the reading of the Christian sources of late antiquity. They cannot be seen as peculiar to a provincial world of limited duration. The authors in question would certainly have rejected any such limiting description, seeing themselves as the heirs of the earlier patristic writers and part of the catholic church. That Shenoute had an excellent education, particularly in rhetoric, is by now widely accepted, and he, like other writers, must be taken as part of a much larger phenomenon of Mediterranean antiquity with heirs in much later periods as well. I do not, in saying this, mean to suggest the existence of uniformity in Christianity across time any more than in antiquity; far from it. But some types of argument seem deeply embedded in Christianity and to survive differences in time, place, and doctrines. One of them is precisely the presumption that in fact Christianity is essentially characterized by doctrinal uniformity and that maintaining such uniformity is of vital importance. Deviations are then treated as aberrant. Far from being merely an "ideal in Coptic sermons," as Frankfurter dismissively put it,¹⁴ the drive for unity and uniformity is a central ideological tenet and motive force in the history of Christianity. The fact that in reality Christianity as a movement has, from its earliest days, been characterized by diversity of doctrine in no way negates the centrality of debate that takes for granted that there is only one true set of doctrines.

¹³ CAMERON 1991, 89–119.

¹⁴ FRANKFURTER 1998, 7.

That, then, is one interpretive model and axis: the Christian texts of late antique Egypt and other lands of the Greek world seen as part of a continuum in space and time with the works of other Christian writers and thinkers. The other model is equally important: the understanding of the environment in which these writers worked as part of the larger Roman world, and the insistence that the eastern Roman Empire is the correct framework for analysis. This is one important respect in which I find Frankfurter's book to mark a large step backward, in its insistence on the peculiarity of Egypt: "Egypt in almost every way stands apart from the rest of the Mediterranean and Near Eastern worlds."¹⁵ That is a posture once widely adopted in historical studies of Hellenistic and Roman Egypt, but it has collapsed in the [p. 30] last generation or so under the onslaught of abundant evidence that Egypt was no more different from other parts of the Roman Empire than any other province was.¹⁶ I am prepared to argue that this is true to a large degree even in the Hellenistic period. But whether that is right or not, it is abundantly clear that Egypt in the fourth to sixth centuries was an integral part of the late antique world, and it cannot be treated as an aberration to be analyzed outside the larger picture of the contemporary Roman world.¹⁷ This is as true in religion as in any other domain of life. There is certainly nothing distinctive about the fact "that religion in Roman Egypt was essentially a localized phenomenon."¹⁸ This was in the very nature of ancient religion and forms the empire-wide basis for the stresses of the religious environment of the third century, when Christianity was far from the only movement seeking to construct a more universal religious framework.¹⁹ As Leslie MacCoull put it, "In our salutary awareness of the local particularity of Egypt we do well to remember that it was Hellenism that gave the local, the particular, the chance to flower and the ability to endure,"²⁰ citing Glen Bowersock's *Hellenism in Late Antiquity*.

I shall exemplify the intersection of these two interpretive frames – Christianity and the late Roman Empire – by returning to Gessius. Emmel has now shown beyond any doubt, contrary to the skepticism of earlier scholars, that Gessius the local magnate in Panopolis attacked by Shenoute is to be identified with Flavius Aelius Gessius, the *praeses Thebaidis* (the regional governor) of 376–378, just a few years before Shenoute became abbot of his monastery (probably ca. 385).²¹ It is worth thinking about the context of Gessius's governorship. It fell under the reign of Valens, an emperor whose ecclesial alignments and theological preferences, aimed essentially at preserving the homoian stance (that is, seeing Christ's nature as like the Father's rather than identical to it) widely accepted in the East under Constantius, caused him to be libeled by the hard-core Nicene adherents as an Arian – but who was certainly for some time an

¹⁵ FRANKFURTER 1998, 14.

¹⁶ I have dealt with this issue briefly in BAGNALL 1995, 11–12. The topic is a central theme in CRISCUOLO – GERACI 1989.

¹⁷ On this point see MACCOULL 1992, 74 (with earlier bibliography) and BAGNALL 1995.

¹⁸ FRANKFURTER 1998, 8.

¹⁹ See RIVES 1995 for the interplay of local and imperial in Carthage.

²⁰ MACCOULL 1992, 74.

²¹ EMMEL 2002, 101–105.

enemy of Athanasius and his [p. 31] supporters.²² Gessius's time in office, in fact, coincides with Valens's last years, which ended with the emperor's death at Adrianople on 9 August 378. Athanasius's successor Peter, expelled under Valens, had returned to Alexandria in 378 after Valens left Antioch to meet the Goths, according to Jerome on the basis of an edict of toleration issued by Valens. Once Gratian appointed Theodosius, a supporter of Nicaea from the West, to take charge of the East, homoousian Christianity was once more securely in power.²³

Emmel has called attention to the fact that Gessius appears never to have obtained any higher official position after his term as *praeses*.²⁴ The conclusion seems evident that Gessius was aligned with Valens and the homoeans, and that after the death of the emperor and the change in religious policy in the eastern court, Gessius no longer had any source of patronage. Theodosius had few links to the existing power networks of the East and no incentive to favor someone like Gessius. In connection with this it seems worth reconsidering the relevance of the statement quoted by Shenoute as having been made by Gessius, "Jesus was not divine" or (to be literal) "Jesus was not a god."²⁵ This slogan is used by Shenoute as a weapon to accuse Gessius of paganism, but it could perfectly well be instead a short-form sloganistic version of a Christian theological position – adoptionist or subordinationist, Emmel has suggested, but perhaps one would instead say, from a neo-Athanasian polemical perspective, "Arian" or something close to it. Such an affiliation would make good sense for a man who won patronage under Valens. "Arian" itself, of course, was a "term of abuse," as Barnes has remarked, pointing out that "no fourth-century thinker who is normally regarded as an 'Arian' or 'Neo-Arian' would ever have applied this term to himself."²⁶ Indeed, the contents of the term even in Athanasian invective are variable over time.²⁷ In fact, even Athanasius's opponents, in the synodical letter written by the eastern bishops in 343 after their departure from Serdica, stigmatized "Christ is not God" as heretical: [p. 32]

Those who say that the Son is from "that which was not," or is from another *hypostasis* and not from God, or that there was a time or period when he was not, the holy catholic church condemns as heretics. Similarly also, those who say that there are three Gods, or that Christ is not God, or that before the ages he was neither Christ nor Son of God, or that the Father and Son and Holy Spirit are the same, or that the Son is unbegotten, or that the Father did not beget the Son by his choice or will, the holy and catholic church anathematizes.²⁸

Note that those who say that Christ is not God are not denounced by the bishops as pagans, but as heretics. In this case, the imperial politics and the larger currents of Christian polemics converge to provide a legitimate and Christian explanation for a remark of Gessius used by Shenoute as evidence of paganism. In case anyone thinks that it is un-

²² See ERRINGTON 1997a for the ecclesiastical politics of this period.

²³ See BARNES 1993, 180–182 for a brief summary.

²⁴ EMMEL 2002, 102–103.

²⁵ See EMMEL 2002, 99.

²⁶ BARNES 1993, 15.

²⁷ BARNES 1993, 134–135.

²⁸ Translation by BARNES 1993, 75, combining versions (see p. 261 n. 28).

likely that high-level imperial politics would have affected a provincial governorship of this mid-level sort, I remind you of the case of Abinnaeus, commander of a modest military installation on the edge of the Fayyum in the middle of the fourth century, who had great difficulty with the anti-Athanasian prefect Valacius, ending only with the prefect's death; Abinnaeus obtained security in his position only after the imperial order for Athanasius's restoration, and just before Athanasius's actual return to Alexandria in 346. Barnes has plausibly argued that Abinnaeus was an Athanasian partisan. Certainly it would be a mistaken undervaluation of the pervasiveness of patronage in late Roman society to fail to see that governorships were closely linked to higher political developments.²⁹ Jill Harries has recently pointed out how vulnerable such governors could be to the attacks of local ruling elites.³⁰

Now, it is time to turn from literary to documentary and archaeological sources. I shall take first the archaeological evidence. It is in this domain that my uneasiness at the concept of "From Temple to Church" (the theme of the colloquium and of this volume) arises most concretely. Although there are helpful ambiguities in the phrasing of this title, the manner in which the questions posed to participants were phrased led me to think that my concern was justified; for example, "What was the temporal relationship between abandonment, destruction, or closure of the shrines, and their transformation into Christian [p. 33] churches?" The question struck me as unnecessarily teleological; it implies taking for granted a directionality and a relationship that to my mind are at best a hypothesis to be tested and more likely are a minor element in the overall situation.

What I mean by this is that on the whole the reuse of temples and their compounds for purposes other than the cults to which they were originally dedicated seems to me to have no obvious relationship to the building of churches; these do not appear to be closely connected topics. Where we have sufficient evidence to establish the approximate date of the reuse of temple spaces – and this is true in only a minority of the temples³¹ – it comes sometimes at a date before the building of identifiable churches began in Egypt, at other times far later. Abandonment and reuse are separate issues.

Now I know that many critics have been outraged by my picture of an early decline of institutionalized Egyptian religion,³² but however much room there may be for disagreement about how far there still were active pagan cult centers in the fourth century, there is no way of evading the fact that major temples like the Triphieion across from Panopolis or the temple of Luxor were taken over by the Roman government for official or military purposes during the third century. These reuses took place at a time when Christianity was still disapproved of, and from time to time repressed, by that same government. I would thus rephrase the question to ask instead how far Egyptian temples

²⁹ BARNES 1993, 96.

³⁰ HARRIES 1999, 170.

³¹ The same is true of destructions, unfortunately. A textbook example is provided by SCHMID 2001, where there is scarcely any evidence for the destruction of a temple of the imperial cult at Eretria, but a priori grounds lead to "on balance we would prefer to see the destruction of the temple at Eretria as falling later, within the 5th c., or even later" (SCHMID 2001, 141): this despite the fact that there is only one coin in the debris later than 300, and that probably belongs to Constantius II.

³² Even so otherwise sympathetic a critic as CARRIÉ 1995, 324–325.

were reused for *any* purpose once they were no longer active cult centers, along what timeline such reuse took place, what the new purposes were, and whether those purposes themselves can be chronologically stratified. This will in the end bring us to the question of when and how some cult places were reused as churches, but it will avoid the tendency to assume from the start that such Christian reuse was normal or to accept too casually the common – but sloppy – modern assumption that once a place was regarded as holy it always remained so. [p. 34]

I have said enough in print about the interpretation of papyri that I do not need to remark at length on that subject here.³³ One thing that does need emphasis, however, is that the silences of the record need to be taken as seriously as the presences. One often hears or reads that it is simply a matter of the chance of survival that we do or do not have evidence of something. This is untrue. The patterns of preservation of evidence are not simply a matter of randomness. On the contrary, they reflect deep patterns: in choices about what to record in writing; in ancient treatment of papyrus documents, both in retention and in disposal; and in significant facts about the history of the archaeology of Roman Egypt. All of these are, in principle, capable of elucidation, and taken together they determine the broad contours of our evidence. That is not to say that at present we understand all of these patterns, for we do not; nor is it to give chance no room at all for operation within those contours. But it is to say that we must try to understand these patterns, and it is to claim that chance cannot be given nearly as large a role as is commonly supposed.³⁴

Of course we must not suppose that the documents, either epigraphic or papyrological, give us some kind of photographic snapshot of ancient society. For reasons I have discussed in detail elsewhere, this is not the case, and I do not suppose that there is now anyone who thinks so, any more than serious archaeologists operate on a “Pompeii hypothesis” about their sites – that is, the notion that ancient cities were abandoned just as they stood in the midst of life.³⁵ But accepting that the documentation was and is selective does not give us license either to avoid thinking about the value and limits of the evidence or simply to dismiss it when it is inconvenient for preconceived theories.

To add a bit of concreteness to these remarks: for the fourth century we are extremely well informed about some aspects of the public administration of the cities, reasonably well informed about the administration of villages, and adequately instructed about some areas of agriculture and taxation in the villages, although mainly the villages of the Fayyum.³⁶ Private correspondence is well represented from [p. 35] Oxyrhynchos and Hermopolis, but very poorly known for the villages, with the single exception of Kellis, in the Dakhla Oasis. We also know quite a bit about some monastic milieus and can see an early dissenting view of the Athanasian establishment. For the fifth century, our doc-

³³ BAGNALL 1993, 4–13; 1995.

³⁴ I have explored some of these issues in the Gray Lectures for the Faculty of Classics, Cambridge (May 2003) and plan to return to them in a future book.

³⁵ See, e.g., MORRIS 2002, 53.

³⁶ The reader will find sufficient documentation for most of the generalizations in this paragraph in BAGNALL 1993.

umentation is much worse. The reasons for this situation are still not clear, but the consequences certainly include the need for a great deal more circumspection in generalizing about any matter concerning this century.

These facts all have important consequences for the kinds of information we can expect to find about religious life. I shall give only a couple of examples, but the kind of analysis presented could readily be expanded to other topics. The very uneven presence of private correspondence is one key point. The evidence for active and hardly concealed Manichaean activity in the Dakhla Oasis in the third quarter of the fourth century, for example, is something that we could hardly have guessed without the Kellis letters, especially the Coptic ones (*P.Kellis V*), although there are slight clues in the Kellis Agricultural Account Book (*P.Kellis IV Gr. 96*). The humdrum cohabitation of paganism and Christianity in the Theophanes correspondence is equally striking for a somewhat earlier period. We must suppose that if we had more such material we would find equally unexpected insights. In the area of civic administration, by contrast, Oxyrhynchos, Hermopolis and Panopolis ought to be giving us a fairly representative picture. It would be far more astonishing to discover a lot of new information concerning otherwise unknown civic priesthoods, for example, or to find that our picture of the expenditure of public and euergetic funds was significantly mistaken, than it would be to discover something strikingly new about individual religious practices or the presence of lightly-institutionalized sects. It is for this reason that it is impossible to dismiss the significance of the evidence for the movement of funds from religious to civic purposes in the third century.³⁷

This point may be connected with the observations made earlier about Gessius to emphasize that the documents probably give us a fair, [p. 36] even if very incomplete, picture of the state of things in the sphere of public power. The Roman state was undoubtedly not as strong and effective as a modern one, but on the whole I think the documentary evidence gives us a sense of it as nearly omnipresent, even if only through local governments, in possession of detailed documentation about the population and its property, and capable of ensuring that the reins of power were in the hands of those in favor with the current imperial government. The degrees of separation between the tenant farmer and the emperor were probably fewer than five.

These observations reinforce my earlier emphasis on Roman Egypt as an integral part of the Roman world. We are not dealing with an isolated, undeveloped society with local traditions newly in contact with metropolitan ones. Far from it. Egypt in the fourth century had behind it a millennium of foreign domination, consistently exercised by developed, paper-producing bureaucracies. The same is true of Asia Minor, Syria, Palestine and for the most part of Mesopotamia too.

³⁷ Contrast FRANKFURTER 1998, 75 (“one cannot extrapolate anything more from this evidence than the most general reflection of economic hardships”), apparently ignorant of the large sums being spent at precisely this period for civic construction; see particularly DREW-BEAR 1997 and VAN MINNEN 2002 on Hermopolis. (The fact that the Hermopolitan expenditures were necessitated by war damage in no way diminishes the fact that the city was capable of raising enough funds to repair that damage, in this case – as Van Minnen emphasizes – by taxation rather than by euergetism.)

Nor can any of these countries be looked at as if they existed in a timeless present. This is, I believe, the most important respect in which my framework for looking at religion in late antiquity differs from Frankfurter's. In my view, his anthropological interpretive lens tends to collapse time and to result in an ahistorical approach, where much attention is given, for example, to Egyptian religion in the New Kingdom,³⁸ but the Ptolemaic period is virtually ignored. The relationship between city and country, perhaps most centrally from the point of view of the history of Egyptian religion, is treated as if it were static throughout the Roman period, when in reality it changed continuously and materially between the first and fourth centuries.³⁹

The point of view that I am arguing here is not identical with the rejection of anthropology that Leslie MacCoull propounded with great [p. 37] vigor a decade ago,⁴⁰ in response to Deborah Hobson's equally strong argument for a greater use by papyrologists of models derived from anthropology. MacCoull, although perfectly conscious of the key role of change in history, was intent on emphasizing the entire Mediterranean world, but particularly of course the eastern Mediterranean, as *the* appropriate context for the study of late antique Egypt. She was led by this emphasis to downplay the value of the study of Egypt over the *longue durée*, in part because such approaches tend to shy away from change in favor of durable patterns of life. For my part, as I have said on other occasions, I do not see a need to choose between these axes; I find both rewarding in different ways and see no reason to impoverish our studies with an unnecessary choice. But it is true that the historian cannot shy away from change.

A few more words about anthropology may be worthwhile. Although this discipline is generally classified with the social sciences, it is sometimes today referred to as one of the humanistic social sciences, in the company of history. Although this seems intended almost as a compliment, what it often means is that anthropology is, like literary criticism, interpretive in the sense that anthropologists present viewpoints that are not capable of disproof. There are historians who see their craft as analogous, and of course history is in an important sense an interpretive activity. Facts do not speak for themselves, nor do we encounter them without a conceptual framework. But, like Lawrence Stone, I think it is a blunder to think that history is no more than that. There *is* evidence, and not all of it is merely a construct. This evidence needs to be used as far as possible to attempt to control hypotheses.⁴¹

³⁸ For example, the use of New Kingdom evidence in FRANKFURTER 1998, 98 to demonstrate a practice "which emerges only idiosyncratically from the texts and archaeological sites," i.e., for which there is no evidence from the period under discussion.

³⁹ Nowhere is this more striking than in Frankfurter's omission of funerary practices, whose "vivid continuity throughout the Roman and Coptic periods" he alleges (FRANKFURTER 1998, 10). In reality, funerary practices underwent dramatic change during this period. Compare KAPER 2001, 131: "There were in fact profound changes taking place in this sector of religion. In fact, it is especially in the funerary beliefs of the Roman period that the complex interaction of the Egyptian tradition with Hellenistic ideas became clearly visible in the art, architecture and material culture as well as in the ideas expressed in the texts."

⁴⁰ MACCOULL 1992.

⁴¹ For a discussion of the differences between scientific and literary discourse, and the affinity of cultural anthropology with the latter, see (from a Popperian perspective) MEDAWAR 1982, 42–61.

It may be helpful to look at the question of change from the standpoint of another province. I quote from Seth Schwartz's recent book on the impact of Roman imperialism on Judaism. After pointing out that most scholars have seen the political impact of the destruction of the Second Temple in Jerusalem as mainly an internal Jewish matter, Schwartz argues, in contrast, that the implications of direct Roman rule were great.⁴²

[p. 38]

To be sure, the government did nothing to prevent Jews from patronizing their native legal experts for advice and arbitration. Yet by failing to recognize their jurisdiction, they made them effectively powerless to compete with the Roman courts and the arbitration of Jewish city councillors and landowners for most purposes. We may in a general way compare the Palestinian situation with the deleterious effects on the native priesthood of the (far less radical) Severan reforms of the ancient nome system in Egypt: the transformation of the old nome capitals into more or less normal Graeco-Roman cities, in which both political power and religious authority were concentrated, apparently seriously undermined the financial well-being of the rural temples and the authority of their clergy.

Schwartz thus sees the actions of the Roman state, and of those persons and bodies carrying out its wishes at a local level, as decisive for many aspects of Jewish society from the second century of our era onward. He argues, in fact, that the rabbis occupied positions of far less influence than is commonly claimed today, and that Roman actions were responsible for the state of affairs. It is important to point out that the Romans were not necessarily, or even probably, seeking this outcome when they instituted direct rule and its trappings, any more than Septimius Severus was interested in weakening the temples of villages like Karanis or Soknopaiou Nesos (both in the Fayyum) when he granted municipal charters to the Egyptian metropoleis. But consequences followed all the same. If ever there was a society that practiced resistance to Roman power, it was Jewish Palestine. But Schwartz's book makes it clear just how modest the impact of *religious* resistance was once the political revolts were ended; and even the rise to prominence of new structures in Judaism in late antiquity was again the product of actions taken by an external power.

If this is correct, and I think it is, it seems very implausible that resistance, in the sense popularized by anthropology, is of any value as a concept for interpreting religious change in Roman Egypt, at least down to the fourth century. It represents in my view quite the wrong set of questions. The distribution of power in Egyptian society changed substantially from Augustus to Constantine, and the village temples and their priests were major losers in this change. So, I believe, were the urban temples, perhaps in large part because the newly-constituted urban elites of the Severan period concentrated their resources on competitive civic public life rather than religious institutions. To the degree that religion figured at all in their priorities, it was in the form of the constitution of a world-class set of athletic competitions, which absorbed resources and energy on a large scale. [p. 39]

⁴² SCHWARTZ 2001, 111.

All of this involved neither any necessary hostility to traditional religion on the part of this elite nor any advocacy by them of an alternative. Even if one inclines to a high estimate of Christianity's numerical importance and institutional development in the second half of the third century, it is not likely to have played much of a role in the changes that I have been sketching.

What I think is most important in all of this is not any particular view of the rate at which the temples declined, nor any specific estimate of the trajectory of Christianity. What is central is a rejection of the view of their relationship in simple binary terms, a discarding of the idea that religious history in this period is a zero-sum game with two players.⁴³ If we can free ourselves from this simpleminded notion, we can look at issues like the abandonment of the traditional uses of temples without presuming that Christianity is the cause.⁴⁴ It is indeed a curiosity and a paradox that those scholars most desperate to be seen shunning any hint of Christian triumphalism have been incapable of imagining that the decline of the temples is not driven by Christianity.⁴⁵ On the contrary, both religious traditions – or, better, one might say all three, for the formation of late antique Judaism comes about in the same environment⁴⁶ – were profoundly affected by the political realities of the times, and every question concerning religious change must be looked at with a keen sense of the impact of politics and political change in particular.

How far we can distinguish a sphere of private piety and religious practice separable from that of public manifestations of religion is harder to say. Views on this are evidently shaped by our conceptions of private life, indeed how far we think such a concept is viable for antiquity. To return to Gessius once more, it seems to me in no way implausible that his professed, and perhaps believed, Christian allegiance (even if perhaps only as a catechumen, as Ulrich Gotter suggested during discussion) coexisted with practices that to a more professional and less tolerant Christian eye were incompatible with that allegiance. [p. 40] In general, however, I think we are still far from a real understanding of the extent and nature of home religious observance, particularly of how far it was tied to temple cults in the Roman period.⁴⁷ Nor do we yet have a satisfactory account of religious change over the millennium from the Saïtes to the fourth century. Much of the material for such a study lies in personal names, and many of the elements were traced in the work of the late Jan Quaegebeur, but there is a great deal still to be done. Just to give one example, names formed on the god Shai scarcely exist before the Roman

⁴³ This emphasis in BAGNALL 1993 was at least understood by GASCOU 1996, 348.

⁴⁴ It is well known that the Herulian invasion (267 CE) is responsible for much of the decline of temples in Greece, for example; cf. SCHMID 2001, 140 with references.

⁴⁵ The self-contradictions in KAPER 2001, 131–132 in this respect are remarkable.

⁴⁶ See CHANIOTIS (in this volume) on Judaism as the third major force in the religious life of Aphrodisias in this period. The role of Judaism in late antique Egypt was probably not as large as in Asia Minor, but there was certainly a significant recovery from the depths that followed the suppression of the Trajanic revolt.

⁴⁷ KAPER 2001, 129 points out that Frankfurter's description of cult in the home is not based on evidence.

period but are wildly popular *in* that period.⁴⁸ What, if any, is the connection between the rise of popular cults of this kind and the visible decline of the large temples? Many questions of this kind could be asked, and I do not know what the answers would be. But if we want to understand the context of the abandonment, the destruction, or the re-use of temples, we cannot avoid focusing our study of Egyptian religion, and on religion in other parts of the eastern Mediterranean, on change rather than on continuity.

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⁴⁸ And, indeed, survive the coming of Christianity like a number of other pagan names thanks to the existence of a revered monk bearing the name, witness the monasteries of Bishoi (= Psois), one in Wadi Natrun and another across from Panopolis.

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