

Evidence and Models for the Economy of Roman Egypt

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One of the most striking aspects of the colloquium to which this volume owes its genesis was the diversity of reactions expressed by contributors to the role played by a retrospective view of the work of Moses Finley. For Egypt, Finley certainly represents at best an ambiguous inheritance. Most readers of Finley's works on the ancient economy and on historical method are likely to develop a negative view of the contribution that the study of Hellenistic and Roman Egypt might make to any larger historical horizon—if, that is, the land of the Nile does not escape their notice entirely in reading Finley, for it does not play a prominent role in his discussions. Even the knowledge that Finley's hostility to papyrological documentation comes at least in part from his experiences with an uninspiring introduction to the field at the hands of William Linn Westermann during his time at Columbia is insufficient to justify what to a papyrological historian can only seem exceptional wrongheadedness.¹ Finley's aversion to documentary studies extended also to epigraphy.² And yet the reality is somewhat more ambiguous than pure negativism, as we shall see.

Certainly the negative remarks are easily found. The uniqueness of Egypt is stressed repeatedly: "In the long history of the Graeco-Roman world, massive documentation characterized only the peculiar society of Egypt and to a limited extent the imperial courts of the later Roman Empire" (Finley 1985b: 15). Similarly: "Only Graeco-Roman Egypt, conceivably the Seleucid empire (about which we know next to nothing on this aspect) and in a

rudimentary way Rome under the emperors were sufficiently bureaucratic." The documentation of the papyri is called "a paperasserie on a breath-taking scale and an equally stupendous illusion" (Finley 1985b: 33–34). The papyri are not only an illusion but too miscellaneous to be very useful (Finley 1985b: 30, quoting A. H. M. Jones). They do not provide data (Finley 1985b: 32), and they have no analytic character.³

But two passages suggest a different approach. Finley acknowledges (1985b: 45) that groups of related documents can in fact provide real information about the ancient world. And at one point he uses a papyrus (a lease of pottery works) to provide the explanatory model for archaeological data—thus contradicting his own claim that evidence cannot yield models, not to speak of undermining the notion of the uniqueness of Egypt (Finley 1985b: 24). Finley's criticisms of the papyri, I shall argue, are ill founded. From these more positive approaches, however, there is something to be learned, and it may be suggested that Finley's partial recognition of the uses of papyri had been blotted from view by his frustration with the quality of most historical writing using the papyri.⁴ Indeed, even his attempts to describe abstractly just what he means by the term *model* sound mostly like the anguished cries of someone who has read one too many books consisting of an amassing of facts without any attempt at explanation; for "model" in these passages comes remarkably close to meaning "explanation" (Finley 1985b: 60, 66). In his own practice, however, "model" seems sometimes to mean Weberian ideal type, sometimes middle-level theory (Finley 1985b: 60, 106).⁵ It is largely in the latter sense that I shall pursue the issue of models.⁶

Whether the problem of the distinctiveness of Egypt still needs belaboring at this point is hard to say. Among those who know anything about the subject, the viewpoint that Finley expressed is long dead. But there are plenty of signs that his attitudes are still commonplace in the larger world of ancient history. The uniqueness argument has collapsed under two burdens. The first is the discovery of Egyptian-style documentation in other places. The second is the successful demonstration that the evidence even of Egyptian papyri can be brought to bear on major issues in the study of the Roman economy.⁷ The second will occupy most of the rest of this chapter, but the first deserves a few words before we pass on to that task.

We now have a considerable body of written material on papyrus, parchment, wood, and clay from non-Egyptian sources. A recent survey of the Near East yielded some 609 inventory numbers, including both the older finds from Dura and Nessana and postwar discoveries, like the various archives from around the Dead Sea, the recently found and still largely unpublished papyri from the Euphrates valley from the mid-third century, and

the sixth-century rolls from Petra still in the course of preparation for publication (Cotton et al. 1995). Hardly any part of Palestine, Syria, and northern Mesopotamia is now entirely lacking in papyrological finds. Not less important is the fact that the range of document types is similar to that in Egypt: family documents concerned with property, letters, legal matters, the census, military business, and the like all appear. Written documents, letters, and accounts have an air of overwhelming ordinariness, in exactly the manner we are accustomed to with the Egyptian papyri. Documentary formulas are similar: the handwriting could come from Egypt, the language is familiar but sometimes with Semitic flavoring. In short, the conclusion that written documentation functioned in the Near East as it did in Egypt is inescapable.

Problems of preservation make it unlikely that the West will ever be quite so generous. But two important recent finds have shown what earlier discoveries of tablets in Dacia and at Herculaneum had suggested: Writing was used in the western provinces with much the same degree of ordinariness. These are the ostraca from the fort at Bu Njem in Libya and still more the writing tablets from Vindolanda on the northern frontier in Britain.⁸ Particularly in the case of Vindolanda, detailed analysis has shown that the use of writing was not the preserve of some few individuals but was, even in the early years of Roman rule of Britain, widely spread and deeply ingrained; once again, absolutely normal and unremarkable. As Alan Bowman has observed, "It is enough that several hundred individuals could [perform the act of writing] within a framework of convention and expectation which would have rendered their texts easily comprehensible at the other end of the Roman Empire" (Bowman 1994: 125).

Nor is that the only lesson to be derived from these far-flung discoveries. Added to Egyptian documentation and archaeology, the discoveries highlight important aspects of the ancient economy. An example is water management. Anyone acquainted with the Mediterranean soon notices that ancient settlement and cultivation extended well beyond the limits of habitation and farming in subsequent centuries, sometimes even beyond present-day limits. As consensus develops that world climate has not changed dramatically from the Roman period to the present—or at least the very recent past—the task of explaining the cultivation patterns of the Roman period becomes more acute. The papyri from Petra, combined with archaeological observation in the vicinity, show that a sophisticated combination of use of spring water and control of water from irregular rainfall allowed extensive agriculture and arboriculture in its area (see provisionally Koenen 1996: 183–85). They thus join such disparate evidence as the tablets and ostraca from the Kharga and Dakhleh Oases (see Bagnall 1997b)

and survey work in Libya (Mattingly 1994, especially ch. 7) in illuminating the wide range of tools available to landowners in the Roman east who tried to support themselves, and ultimately the whole structure of urban life, on marginal lands. The Egyptian documentation, in this light, seems no more particular than any other; it reveals a specific ecosystem, or rather several specific ecosystems, but so would evidence for any other region. There is no basis for any assumption that one region is normal.

At this point it might seem tempting to give a cheerful shout of triumph and claim that Egypt's evidence is thus fully ready for the mainstream of historical study of Roman antiquity. But this is not the case. Finley and those who, like him, have denigrated the use of papyrological evidence or simply ignored its existence have to a considerable degree produced a self-fulfilling prophecy.⁹ Although much work in papyrology has always been driven by no questions at all, but simply by the accumulation of data on some point, there have always been exceptions.¹⁰ Interestingly, however, the impact of larger ideas was for the first three-quarters of this century considerably greater in studies of the Ptolemaic and so-called Byzantine periods than in those of the Roman period. There has never been an equivalent for the Roman period of the great interwar syntheses of Ptolemaic Egypt by Claire Préaux and Michael Rostovtzeff, dominated by a general theory of Egypt as a centrally planned, dirigiste economy with a dominant role in production in the hands of the state (Préaux 1939; Rostovtzeff 1922, 1941). Nor has there been any equivalent to the pre-1960s consensus that Egypt after Diocletian was essentially a protofeudal state, in which spatial, occupational, and economic mobility had been suppressed for most of the population and great magnates dominated the scene.¹¹ These broad conceptions were certainly large-scale models of particular ancient societies; neither has much credibility among specialists today, although they will still be found in derivative works. The impetus for the demolition of the Ptolemaic model came to a large degree from inside papyrology, in considerable part by Préaux herself and by her student Jean Bingen, but Finley's writing on the ancient economy has certainly affected the discussion considerably. In the case of late antiquity, the revisionist impetus of A. H. M. Jones's magnum opus (1964) took time to percolate into papyrology, and when it did the discussion was also strongly affected by the movement in late antique studies led by Peter Brown.¹²

Nothing like this, as far as I can see, happened with Roman Egypt. James Keenan (1982: 23–24) has suggested that the reason for the lack of grand syntheses of Egypt from Augustus to Constantine (or Theodosius, for that matter) is in part that the pace of publication of papyri from the Roman period

has hardly slackened from the earliest days of papyrology to the present. For the period from the fifth century on, there was a substantial lull after 1924, when relatively few substantial new volumes appeared; and after the early 1930s, the amount of new material of Ptolemaic date also dropped off substantially. In effect, the pauses created the space for synthesis, for taking stock and for the creation of major research tools. No such pause occurred for papyri of the Roman era.

This hesitation was in fact expressed by the most important single exception to this generalization, the Princeton historian Allan Chester Johnson, who remarked in 1936 that “it may be premature to attempt an economic survey of Roman Egypt while the ancient sites continue to yield their treasures of papyri and ostraca,” but who proceeded immediately to say, “yet the main lines of Roman exploitation may now be traced with some accuracy” (A. C. Johnson 1936: vii). His 1936 survey was in fact mainly descriptive, but it contained substantial analytic passages as well. With his students and colleagues, Johnson continued to produce the only substantial body of synthetic work on economic issues, including Wallace's study of taxation (1938), West and Johnson (1944) on currency, and Johnson and West (1949) on the Byzantine economy. This series culminated in 1951 with Johnson's book on Roman Egypt, based on his Jerome Lectures of 1947–48, *Egypt and the Roman Empire*.

This book, however, was not a general synthesis but a series of six interconnected studies, discussing fiduciary currency, inflation, land tenure, “serfdom” and taxation in Byzantine Egypt (this in the usual papyrological manner referring to everything from Diocletian on), and the administration of Egypt. Nowhere does it offer an overall model of the economy of Roman Egypt. We can, nonetheless, sketch Johnson's view of the subject from remarks scattered throughout the book. Egypt, for him, was largely self-sufficient, needing to import little but iron. The peasantry and urban “proletariat” lived barely above a subsistence level and were largely absent from any other economic transactions. They produced the large wheat surplus that gave Egypt a generally favorable balance of trade and paid its taxes but did little else. The urban population consumed some imported luxuries, but these were of marginal significance and highly regulated by a strongly centralized government. The currency was fiduciary until Diocletian, and the “creeping” inflation visible at least until 269–76 is largely to be attributed to rising external demand for Egypt's wheat as production elsewhere fell. The more dramatic rise in price levels around 275 was also the product of external circumstances rather than changes in Egypt itself. Fourth-century inflation, by contrast (and Johnson is very tentative about this period), seems to reflect

changes in the denomination of billon coinage and in their metal content compared to the denominations.¹³

The Roman land-tenure system, Johnson thought, was essentially a continuation of the Ptolemaic one, with the overwhelming bulk of the land in imperial ownership and farmed under hereditary lease; private land was scanty. In this situation, large estates did not grow up. Not until the fourth century was much land turned over to private ownership, and even then really large estates did not become a feature of the Egyptian landscape (see A. C. Johnson 1951: 67–86). In Johnson's view, the period from Diocletian on was a big improvement, with the removal of many inequities, so that "the peasant enjoyed greater liberty and greater prosperity than ever before" (1951: 156).¹⁴

Johnson's contemporary Westermann also did some writing on Roman Egypt, but his great preoccupation in the 1920s and 1930s was the Zenon papyri and Ptolemaic Egypt. He therefore never produced a work on Roman Egypt with the broad scope of Johnson's.¹⁵ Nor did Michael Rostovtzeff, whose major contribution to the study of Egypt was on the Zenon archive from the Ptolemaic period, and who when engaged with the Roman period was largely taken up with the material from the excavations at Dura-Europos in Syria.¹⁶ Arthur Boak, working throughout this period on papyri of the third and fourth centuries, especially from Karanis, produced a number of articles setting forth a view indebted to the pessimistic school of thought represented by Bell.¹⁷ And it is difficult to point to any British or continental scholar of their period who was engaged to the same degree with the Roman period and attempted broad-gauged synthesis. Rather, the questions asked by most papyrologists working on Roman Egypt continued to be driven not by the larger field of ancient history but by the internal dynamics of papyrology, a field largely shaped by philology and law. It is perhaps not surprising in these circumstances that Johnson's relatively optimistic view of the society and economy of late Roman Egypt drew little support; as we have seen, he had in any case no coherent overall model for the earlier Roman centuries to match the powerful descriptions of Ptolemaic and Byzantine Egypt that were prevalent in the field, and both Westermann and Bell had low opinions of Johnson and his work.¹⁸

Nor in the intellectual climate of the period up to the 1960s could one ignore what was said about the Ptolemaic and Byzantine periods in forming an opinion about the Roman era. For papyrology was dominated by the view that a high degree of continuity affected Egypt throughout the centuries, and above all that Augustus largely took over a going concern in 30 BC and changed it as little as possible. Once that premise is accepted, it is evident that

Roman Egypt also should have been a period with a high degree of state control over the productive apparatus and an oppressive bureaucratic and taxation regime. Although detailed studies began to challenge this continuity by the early 1950s, it was a paper of Naphtali Lewis's at the 1968 Congress of Papyrology that demolished it with a head-on and widely accepted attack.¹⁹ At least for the last thirty years, then, the intellectual climate has been such as to allow for the need of models to explain the economy of Roman Egypt as a distinct phenomenon.

Lewis's own general book on Roman Egypt, published in 1983, does not attempt to do this. It is a largely descriptive work, aimed at a general reader, and argues no general thesis. Once again, however, one can elicit from it some broad conceptions. These include a largely subsistence economy in the rural villages, except for a handful of the more affluent; geographical mobility mainly as the result of unbearable fiscal pressure; a high incidence of flight and manpower loss as the result of heavy and inflexible taxation; a slave population mainly engaged in domestic service or wage-earning occupations; and a mainly private water-transport fleet.²⁰

It would, alas, be idle to suppose that recent progress has now given us a model for the Roman period of an attractiveness comparable to that of the older ones for the Ptolemaic and Byzantine periods, not to speak of greater explanatory power. Many questions essential to understanding the economy of Roman Egypt still lack any serious discussion. If one asks about studies that take on matters of class or markets, for example, it is hard to give much of an answer. But some subjects have fared better, typically those where the usability of the evidence of the papyri is more obvious. In what follows, I shall describe some of the areas where real progress has been made, then try—very tentatively—to summarize the overall view of the economy emerging from them, and finally turn to assess the potential for further work, particularly of a quantitative nature.²¹

It is not surprising that land and agriculture have been the largest beneficiaries of inquiries based on the conscious use of models. Of course such models were present even from the earliest times, but in the earlier literature they come largely from the late Roman legal sources. The heavy reliance on the Theodosian and Justinianic codes, and on older concepts of medieval feudalism, to explain the status of land ownership and of agricultural labor, not only in late antiquity but throughout the Roman period (and sometimes even earlier), has come under strong attack in the last two decades (especially by Carrié [1982, 1983]). But replacing the models of the colonate and feudalism has still required coming to terms with the legal sources, and even though more recent literature on feudalism has not figured much in

discussions of late Roman Egypt, the development of a view of the Great Houses as carriers of burdens on behalf of the state surely owes something to changing views of later periods (see especially the classic treatment of Gascoû [1985]).

The very notion that land ownership was highly concentrated, and ever increasing, throughout Byzantine Egypt has been shown to require considerable modification. In part this shift in thinking has come from the application of statistical techniques borrowed from modern economic history and development economics to the data provided by land and tax registers from Egypt (Bowman 1985; Bagnall 1992). In part, it has come from a revisionist understanding of the relationship between city and country, in which there are no one-way streets and in which villagers are seen as actors in their own interest and not only as acted upon (see Keenan [1980, 1981] for examples). Although this latter strand has not been self-consciously presented as part of a larger intellectual current, it is hard not to see it, at least in retrospect, as one aspect of the attempt, as it were, to empower ordinary people of the past, to see them as autonomous individuals negotiating their fates in the context of the available choices. Microhistory has been particularly devoted to this approach, which has also influenced much feminist work.²²

Earlier Roman Egypt has been affected by models in quite different directions. Two major works address the use of tenancy in agriculture: Kehoe (1992) consciously applies to a limited number of archival groups a model based originally on study of Pliny the Younger and the inscriptions concerning imperial estates in North Africa to the use of leasing by larger landowners; Rowlandson (1996) surveys synoptically the scattered documentation from Roman Oxyrhynchus. The two complement one another well, with Kehoe's driven primarily by economic questions, Rowlandson's by social interests.

If operation through leasing has become much clearer from these works, so too has direct operation of large estates through Dominic Rathbone's book (1991) on the Appianus estate in the Fayyum, where the center of attention is a centrally managed but geographically dispersed array of holdings devoted to producing a marketable surplus of wine but also meeting expenses in part through growing other crops. Rathbone explicitly sets his book in a context of bringing major questions about the Roman economy, including the sophistication of accounting and the role of economic rationalism, to bear on the Egyptian evidence and in turn testing hypotheses about them with that evidence.

Rathbone's book is the bridge to another major area of the economy—urban production and redistribution—because his description of the

Appianus estate demands a means of turning its wine surplus into cash; it implies, that is, an extensive city-based trade in wine, even if only within Egypt (we have no reason to think that the quality of Egyptian wine would have attracted any external market). For example, the Fayyum may well have shipped most of its produce to Alexandria. It could well, however, have sold part to upper Egyptian cities where vineyards were less extensive.

Other studies have begun the task of understanding urban production (see van Minnen [1986] for a general treatment). As Finley recognized, it was above all the involvement of government in certain economic activities that led to their being documented in great detail, and the relative lack of information about craft, or even small-scale industrial, production in the cities of Roman Egypt is not evidence for the unimportance of this work. That is, one cannot both regard documentation as the product of government and at the same time use the lack of documentation as evidence for a consumer city that had little productive capacity. Against Finley's assertions to the contrary (1985a: 135–37), it has been shown beyond reasonable doubt (van Minnen 1986) that Oxyrhynchus had an extensive textile industry, producing cloth somewhere on the scale of an early modern European city.²³ All of this work on the urban economy has been rooted in attempts to bring the papyri into the larger discussion about the character of the ancient city, particularly a wish to break out of the producer/consumer dichotomy that has so dominated the subject.

In the domain of status, studies have focused in two areas. One is the *coloni adscripticii* of the late period, who form part of the investigation of large estates already described. The other is of course slavery. The centrality of discussion of slavery in ancient historical studies has been such that the papyri have never stood outside the debate as they have in other areas, and it requires only a brief description for it to be obvious that the questions debated are derivatives of the models of ancient slavery contested for many decades. There has long been a consensus that Egyptian slavery was largely domestic in character and thus mainly a matter of consumption rather than production.²⁴ In the urban economy this view still holds, and slavery is above all an urban phenomenon in Egypt. But although the number of rural slaves engaged in agricultural work was not very large, it may nonetheless have made a significant difference to some types of households (Bagnall 1993a: 123–27). And I have argued recently that Egyptian villages were significantly involved in the activity of collecting exposed city babies, bringing them up as slaves, and selling them back to the cities (Bagnall 1997c). The supposed decline of slavery in late antiquity, another staple question on a larger canvas, has been posited for Egypt as well. This has been both called into question

and defended during the last decade.²⁵ The issue is partly structural (Did the Egyptian economy change in ways that would have made slaves less useful?) and partly evidentiary (Does the decline in attestations of slaves point to a decline in the actual number of slaves, or is it, as I have argued, an artifact of the character of the papyrological evidence for late antiquity?).

Another critical area of the ancient economy is transportation. Egypt has long stood outside the raging debates about the feasibility and economy of overland transportation, because almost all parts of it were reached by the Nile and the connected network of waterways, surely the most cost-efficient transportation system of any sizable part of the Roman Empire. Although the existence of land transport by donkey and camel, and even to a limited extent by wagon, has long been recognized, it has been assumed to be a marginal element in a water-dominated system.²⁶ No doubt that is basically right, but the last couple of decades have seen an enormous amount of exploration of the Egyptian deserts and oases, which can only have depended on transportation by pack animal, and these studies have opened new perspectives. The Eastern Desert was the land of two important economic phenomena: in its southern sector, of the Red Sea trade via Myos Hormos and Berenike; and in its northern sector, of the work of mining and quarrying centered on Mons Claudianus and Mons Porphyrites.²⁷ These are both exceptional cases, to be sure, driven by luxury import and export goods in the one case and by high-value products, of which the government was the principal consumer, in the other. In both cases the government was directly involved. But it was not simply a matter of a few bags of spices or of gold crossing the desert on camel. Just the supplying of the Red Sea outposts and of the extensive network of small desert forts and quarrying operations was a gigantic business, and the goods the Romans exported to India and East Africa included such relatively bulky items as wine.²⁸ Obviously cost was not a determinative factor in some of these desert operations—one had no choice but to pay the going cost to transport a porphyry column to the Nile—but it is unlikely that cost was irrelevant in getting Italian wine to Berenike for shipment to India, even if the market at the other end was a royal court.

The Western Desert offers an entirely different perspective on this problem. The first four centuries of Roman rule saw a vast expansion of settlement in the Kharga and Dakhleh Oases, both of which ultimately became independent nomes (the Hibite and Mothite) with their own metropoleis and civic elites. There was evidently a sizable increase in wealth to support this governing class. At the same time, we find units of the Roman army being stationed in the region. There is now a strong hint from the Kellis Agricultural

Account Book (Bagnall 1997b) that the foundation of the wealth that allowed the full participation of the oases in the Roman economic and social system was olives and olive oil, as was the case in Tripolitania (Mattingly 1994), another region where marginal land came under cultivation in this period. Only the ability to export this produce to the Nile valley can have generated wealth on this scale. The private letters from Kellis show that the Dakhleh Oasis had close ties to the part of the Nile around Lykopolis and Antaiopolis, directly across three hundred kilometers of desert rather than via the longer but easier route through the Kharga Oasis.²⁹ It seems increasingly clear that it was possible to transport high-value but not luxurious goods, like olive oil, by land across vast stretches of desert and still have them competitive in Egyptian markets—where, to be sure, olive oil was always in somewhat short supply.

Even from a cursory look of the sort we have given to recent work, it becomes obvious that quantitative arguments play a key role in many of these subjects.³⁰ Some of these are questions of scale, which are important in the works of Rathbone and van Minnen, for example. Some are applications of specific quantitative techniques, which are present in the studies of the distribution of land (Bowman 1985; Bagnall 1992). In my work with Bruce Frier on demography, standard demographic modeling is central to many of our main positions (Bagnall and Frier 1994).

Most of the quantitative investigations have been based on one of three types of evidence. First come synthetic compilations of data carried out by ancient authorities, usually to provide a basis for taxation or to demonstrate that taxation had been properly carried out; the Hermopolite land registers are a preeminent specimen of this genre. Second there are assemblages of similar texts that can be treated as a sample, even though in no case do we really have anything like a representative sample; the census declarations are a good example of this type of data. Third come the large archives, where we can get complete enough information about an individual family or enterprise to assess key economic aspects of its life, even if comparability remains a perennial problem; the Heroninos archive may represent this group fairly.

Even where these types of data are not available, all is not lost. Smaller archives also can yield less complete but still useful information, and having enough of them allows comparison; Kehoe (1992) shows something of what can be done with such bodies of material. And even nonarchival material, if collected in enough quantity and carefully analyzed, yields insights complementary to those provided by the archives, as Rowlandson (1996) demonstrates.

The remarks above have probably given the impression that a lot of progress has been made in the last decade or so in the deployment of the

papyri for purposes of economic history, both through the application of questions and techniques from the mainstream of ancient economic history and through detailed analysis of bodies of papyrological evidence. That impression seems to me correct. A new model for the economy of Roman Egypt has in fact emerged in specialist circles, even if it has not yet been set forth in comprehensive fashion; it underlies many of the detailed studies I have cited. The next few years should see at least two attempts to offer an overall synthesis or a generalized model for the whole period in question.³¹ One of these is van Minnen's forthcoming book on the economy of Roman Egypt, announced but not yet published. Rathbone has also now undertaken to produce a large-scale synthesis, with a quantitative bent, on the subject. With these coming, it is impossible and inadvisable to do more in the present chapter than to sketch some of the main outlines of what recent scholarship would suggest a synthetic view of the economy of Roman Egypt might contain.

First, the Roman government set out to dismantle the remains of the Ptolemaic state and its more interventionist approach (Rathbone 1994). The professional bureaucracy was largely replaced by liturgical service over the period from Augustus to Hadrian. To make this possible, land was turned over to private ownership on a large scale, particularly from the old Ptolemaic military allotments. A landowning elite quickly grew up in the nome capitals, which were given gradually increasing powers of self-management and responsibility for the surrounding countryside. In this way Egypt was assimilated to the patterns prevalent in the rest of the eastern Roman Empire. The process was certainly not instantaneous, but its completion—not beginning—in the third century with the grant of city councils reflects how far the change had gone. Although some landowners had substantial holdings, there was a much more substantial group of middle-range owners, and very large, consolidated estates never developed in Egypt. These elites managed their dispersed estates in a variety of ways. As elsewhere in the Roman world, tenancy played a central role, particularly for arable land; but capital-intensive fruit crops tended to draw more direct management, with the larger operations run for efficiency and profit. The rural economy was highly monetized and closely tied to that of the cities through leasing, credit, and marketing. The villages, too, had a substantial stratum of midrange landowners, well above the subsistence level and often very closely linked to the operations of the urban economy. The productivity of the agricultural sector was very high and seems to have risen during the Roman period.

The cities, in their turn, were far from being merely the residence of these landowners and their dependants. At least the larger of them were also

scenes of substantial and specialized craft production, with export in mind. This too is probably the work of two centuries of development. It is harder to pinpoint the extent to which this trade was internal to Egypt and to what degree it was directed at foreign markets, but certainly some of the goods sold abroad, like linen and papyrus, were produced in the nomes and not in Alexandria. In any case, the efficiency of the Nile as a transportation route made it possible for the entire valley to operate as a single unit to an extent rarely found elsewhere in the ancient world. The desert extremities were tied into this Nilotic backbone—to the east because the mines, quarries, and trade routes were imperially managed and dealing in high-value goods, to the west because the oasitic ecosystem was able to produce olive oil worth transporting by land to the valley.

It is now widely considered that the Egyptian economy became extensively monetized already in the Ptolemaic period. The Roman government did not change that fact, despite its continuation of the Ptolemaic habit of maintaining a distinctive system of coinage in Egypt. It is becoming clearer that inflation was a far more limited aspect of the economy than historians—haunted by the specter of interwar inflation in Europe—were prepared to believe. In the third century it was (as Johnson already seems to have grasped) missing until about 275, and then again absent until Diocletian; even in the fourth century, when it seems to have been massive if one simply judges by the changes in prices, it was a monetary phenomenon—the combined product of debasements and retariffings of coins—with limited impact on other economic activity.³²

Within these broad outlines there are plenty of questions on which syntheses are likely to differ. From all indications, van Minnen's book will take a position at the "optimistic" end of the spectrum of opinion, arguing for a large population and a highly productive urban economy. That is, it will offer a near-total revision of the model of the economy summarized above from Johnson and other works of the past sixty years. If these positions are likely to be controversial, it will (I expect) be more because of the extreme lengths to which they are pushed rather than because the basic views expressed are different from those of most specialists today.

There are also plenty of aspects of the economy missing from the brief summary above, mainly because they have not yet received the same kind of attention. It is therefore natural to ask if this kind of progress can be sustained. I shall suggest several directions in which I believe that it can. First, of course, the great attraction of documentary and archaeological disciplines is that they keep bringing new material to light. Although the pace of papyrological publication is irregular, over the course of a decade or two

quite a bit of relevant new material is published. Almost the entirety of the material mentioned above concerning the role of the desert economy and its relevance to the analysis of transportation economics in Egypt was found since the mid-1980s, to offer just one example. It is true that established Western collections have fewer dramatic novelties to offer now than was once the case, but even they are by no means exhausted; and excavations continue to shed light on regions of Egypt hitherto poorly known, showing that we assume uniformity at our peril.

Second, there is a lot of published material that has never been subjected to serious analysis. Sometimes this is a matter of the scattering of connected items through many separate publications, sometimes a matter of the limited questions with which editors approach their texts. Many of the archives treated by Kehoe (1992) would individually repay more detailed studies, and the recent resurgence of studies of the Apion papers of the sixth century shows that even the best-known of archives often have scarcely been touched.

Third, any number of topics concerned with the economy have never had an intelligent study. Perhaps the single most important for Roman Egypt is taxation. There is, of course, a voluminous bibliography about taxation, but it is almost all concerned with identifying the existence of taxes, understanding the ways and means of collection, and establishing rates. And much even of this bibliography is now out of date. But no one has ever made a serious attempt to understand what the overall burden of taxation was like for different actors in the economy and what kinds of incentives and disincentives were created by this particular system of taxation. The details of liturgical service, too, have been studied in great detail, but there has been no real attempt to ask similar questions about the impact of the system.³³ Still less has anyone correlated these systems and tracked their development over time. This is a daunting project but an important one. Egypt's imports, so readily dismissed by Johnson, demand reexamination. Egypt certainly needed to import wood and charcoal as well as iron. Our evidence is very poor, but one might begin by pondering the essential resources that were not indigenous to Egypt and formulating estimates of their quantitative importance.

This leads me to pose the question of the feasibility of creating a model in a more explicitly quantitative sense: Could we build a working model of the economy of Roman Egypt, of a particular Roman province, that is, with both its universal and its particular dimensions? I do not, of course, mean a set of spreadsheets in which most of the cells are filled in with hard data, which might not really be a model anyway, but one that embodied a set of hypotheses. There are sufficient data, I think, to test enough of these hypotheses to assure some rigor in the model. Egypt may be the only part of

the ancient world for which there is enough evidence to test a reasonable number of the assumptions.³⁴ Constructing it would be a large-scale collaborative venture involving a lot of groundwork as well as the model-building itself.

Notes

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1. I am grateful to Naphtali Lewis for his recollections of Finley's time at Columbia. He writes (letter of 9 February 1998), "He was for a few years in the '30s WLW's favorite, prize student, before the relationship soured. Moe, Meyer Reinhold and I were the [total] student body in WLW's 1932 papyrology seminar on the Zenon papyri. We taught ourselves to decipher, which is easy enough in that script where the writing is intact; but what we did with our deciphered texts was rudimentary and uninspiring—little more than *explication de textes* and search for parallels. Much given [all his life] to snap judgments, Moe must have decided then and there, I suspect, that this stuff was unimportant—out of the mainstream and therefore negligible."

2. See *Proceedings of the British Academy* 94 (1996): 587, on Finley's role in the election of George Forrest rather than David Lewis as Wykeham Professor at Oxford. For a more general account of the origins of Finley's aversion to emphasizing evidence, see 466–70.

3. Finley 1985b: 36: "However, all the evidence satisfies me that such notions [synthetic accounts] were alien to him [Zenon] and his society, that his mass of paper consisted of day-to-day documents intended for day-to-day purposes and little more." He sees the Roman picture as more of the same.

4. Much of Finley (1985b) is in fact a polemic (see 63 ff.) against Peter Fraser's *Ptolemaic Alexandria* (1972), with its (as Finley saw matters, and not unjustly) positivistic and unreflective approach to not merely the papyri but other evidence.

5. Somewhere in between is Keith Hopkins's definition (1995/96: 41): "A model is, roughly speaking, a simplification of a complex reality, designed to show up the logical relationships between its constituent parts." Hopkins (64 n. 2) misrepresents me as "explicitly den[ying] the utility of models" (citing Bagnall 1993a: 310), but there I say only that "the complex reality of the agricultural economy of Egypt is not readily represented by broad generalizations or simple models," which is not the same thing.

6. One case of the use of models in the study of Ptolemaic Egypt deserves mention here, as I shall not discuss it below because it is neither concerned with Roman Egypt nor mainly economic in character. This is the proposal by E. Will to adopt modern colonialism as a model for approaching Hellenistic

kingdoms and particularly the Ptolemies. The results of attempts to put Will's proposal into practice have been rather mixed, consisting partly of careless handling of the facts and partly of careful analysis of a traditional sort in which the colonial theory is applied like a veneer. For a discussion with bibliography, see Bagnall (1997a), where it is argued that a use of colonialism as an ideal type is of little use, but a sensitivity to mid-level explanations of specific phenomena is helpful.

7. See esp. Rathbone 1989. He focuses there on "the economic position of the rural population, the character of ancient economic behaviour, and the nature and scale of Roman taxation" (161). Recognizing the individuality of all provinces, he shows the usability of Egyptian data for key questions concerning the Roman economy.

8. O. *Bu Njem*; Bowman and Thomas 1994.

9. Examples appear constantly; the most recent to cross my desk is Alcock (1997), which manages not a single contribution on Egypt (no doubt one of those "inevitable and unfortunate gaps in coverage" mentioned on p. vi).

10. See Jördens 1995: 37 n.; despite her disclaimer, however, her article is by no means an example of the type.

11. For discussion, see Keenan 1993; Hardy (1931), especially, embodies this view, but Keenan cites abundant literature supporting it, especially many works of H. I. Bell.

12. Keenan (1993: 141) refers to articles by Rémondon and Keenan himself in the mid-1970s as marking a shift.

13. Currency occupies pp. 1–36, with many of Johnson's general views on pp. 18–29; inflation, pp. 37–66; see esp. p. 39 on *monetary velocity*.

14. See also pp. 88–99 on the status of the peasants and p. 118 for Diocletian's removal of inequities in the tax system.

15. It may be noted that E. R. Hardy, whose dissertation on Byzantine Egypt has been mentioned in note 12, was Westermann's pupil. As Keenan (1993: 141) has pointed out, however, the dominant influence is that of Bell, and it may be doubted that Westermann had any original contribution to make to the study of the sixth century.

16. That is not to say that Rostovtzeff was uninterested in the Roman papyri, which he uses throughout his *Social and Economic History of the Roman Empire* (1926).

17. See Boak and Youtie (1957), the title of which is by itself sufficient. In Boak's Roman history textbook (many editions, see, for example, *History of Rome*, 3rd ed. [1943]), Egypt gets no more than passing mention. The same is true (naturally enough, given the title) of his *Manpower Shortage and the Fall of the Roman Empire in the West* (1955).

18. "Relatively" is to be emphasized. The preface to A. C. Johnson (1936) is full of the decline of agriculture and of villages, the "anarchy of the third century," and the "flight of the peasant from the soil." Westermann's views,

which can be found in his correspondence with Bell about the operations of the papyrus cartel, are confirmed for me by Lewis: Westermann "never missed an opportunity to denigrate Johnson." For Bell on Johnson, see Keenan 1993.

19. N. Lewis 1970; see also N. Lewis (1984) for further development of the theme, surveying work in the intervening years. Braunert (1964) offered the first major detailed and nuanced study of a major question of social history of Roman Egypt in quite some time, and the wider historical context is important to his framing of questions; he also treated the Ptolemaic and Byzantine periods. But in his examination of migration and population movement he explicitly rejected the possibility of using statistical methods.

20. N. Lewis 1983: 57 (slavery), 67–71 (villages), 142 (transport), 159–65 (taxation and flight), 203 (mobility). His account of agriculture (107–33) and trades (134–55) is essentially enumerative.

21. It should need no special emphasis—but it is a matter of some pleasure to observe—that in a chapter like this it is impossible to mention many valuable contributions in which models from the larger world of ancient studies are brought to bear on the papyri. Some of these are discussed throughout Bagnall 1993a; on the general question of the sources of the questions discussed in papyrology see Bagnall 1995: 90–108.

22. On the roots of this approach in microhistory, see G. Levi, "On microhistory," in Burke 1991: 97–117.

23. See Bagnall (1993a: 82–83) for a discussion accepting the main lines of van Minnen's argument but suggesting that his numbers may be too high.

24. Biežuńska-Małowist (1977) is the classic statement of this position, with full description of the evidence as it stood then.

25. Bagnall (1993b) argues that the supposed quantitative evidence for decline is worthless; Fikhman (1997) remains in favor of a decline.

26. See Bagnall (1985a) for a general assessment and Jördens (1995) for a detailed examination of the trade in transport animals.

27. For a brief survey with bibliography, see Maxfield 1996. On Berenike, see most recently Sidebotham and Wendrich 1996.

28. On the supply of goods like wine and oil to Mons Claudianus, see Tomber 1996. The ostraca from Berenike (published by R. S. Bagnall, Christina Helms, and Arthur Verhoogt in *Papyrologica Bruxellensia*) show extensive export of fine wines just as described in the *Periplus Maris Erythraei*.

29. See *P. Kell*. I (ed. K. A. Worp) for Greek letters; *P. Kell*. V (ed. A. Alcock, I. Gardner, and W.-P. Funk 1999) has numerous Coptic letters showing the ties to this part of the valley (not least that the dialect of the incoming letters is characteristic of that region).

30. For a general discussion of quantification in history written from papyri, see Bagnall 1995: 73–89.

31. In Bagnall (1993a), I have developed a general view of the post-Diocletianic situation.

32. See Rathbone (1996) for the third century (his title is slightly misleading, for he does not in fact claim that there was no price inflation, only that it was absent until 274/75); see Bagnall (1985b) for the fourth.

33. Drecoll (1997) deals only with the third and fourth centuries, and without any serious economic analysis.

34. One may ask if the discovery of papyrological documents outside Egypt will make it possible to construct comparable economic models for those regions as well. This seems improbable unless substantial new discoveries are made. The Dead Sea archives from the early second century provide information on private landowning, but the volume of material is still relatively small. When the Petra papyri are published, they may contribute significantly to our assessment of the operation of the agricultural economy of that area. From preliminary reports there do not seem to be any texts capable of contributing to a quantitative appreciation of the Petra economy, but at least some important qualitative information will offer a new basis for interrogating the archaeological record, including the remains of water-control systems and paleobotanical materials.