2. City and countryside: Economic and social aspects

In introducing the city of Trimithis and its oasis surroundings in the first of these lectures, I called attention to the distance of the Great Oasis from the Nile valley. I also signalled the apparently dramatic rise in the extent of settlement in Dakhla during the Roman period. The growth in accurate knowledge of the chronology of oasis pottery in the last two decades has led to the redating of some wares found in the survey of the oasis, and this redating has led to the identification of more Ptolemaic sites once thought to be only Roman. As a result, the rise from Ptolemaic to Roman seems a bit less marked now than it did when the survey was carried out by the Dakhleh Oasis Project (1977-1987), but it is still evident that growth in the Roman period was striking. Given the high cost of overland transport in antiquity, a theme familiar to anyone with even a passing acquaintance with ancient economic history, one might well wonder how such growth was possible. Why would anyone settle in so remote and seemingly forbidding a place as Dakhla, and how could they earn a living there, let alone become rich? Why would rich people invest there?

The oases are constituted essentially of soil and water, plus some stone, mostly sandstone of moderate quality. Actually, that description sounds at first like much of the Nile valley, too. But there it was relatively easy to supply anything not available locally, because transportation on the great river was cheap; for the same reason it was easy to export the agricultural produce of the rich soil of the valley to places from which other goods were to be acquired. The oases lacked this easy and cheap transportation network. By ancient direct road, Dakhla is about 365 km from the valley at its closest point.

But the oases, if they were to live above the level of subsistence farming, needed many things they did not produce, notably metals, but also any finer and harder stones, papyrus, and luxuries like the fine glassware found at Kellis, fish sauce, or other foods not available locally. In addition, the population had to pay taxes in money to the Roman government, and to the extent that this cash was not locally recycled, it had to be earned. There were only two routes to acquiring the means to pay for these. One was to export something (or a combination of things) that is valuable enough that it can be sold profitably in the valley, even after the cost of transporting it over 365 km of desert; the other is to have a strategic or prestige value to the ruling power that causes it to supply the oases even at a loss to itself.

The second of these is certainly the explanation for much of the activity in the Eastern Desert of Egypt in the Roman period: quarrying of granodiorite at Mons Claudianus, porphyry at Mons Porphyrites, and other expensive stones and gems at other sites, about which much more is known now than it was a quarter-century ago. These stones were needed for imperial projects and acquired at vast expense out of imperial resources, or destined for consumption by the wealthy. The situation with the trade routes via Myos Hormos and Berenike to points south and east, above all India, is slightly more complex. There was a highly profitable and well-taxed luxury trade, in both directions, that furnished a market basis for the large imperial investment in roads, stations, and ports in support of the trade.

In the Western Desert, there was probably some comparable justification in prestige and rarity for the exploitation of the region under the Old Kingdom, when expeditions scoured the deserts for valuable mineral products. Only in such a way could the elaborate relays of donkeys necessary to cross this desert in this era have been maintained, and the best known Old Kingdom site in Dakhla, Ain Asil, certainly was a royal project that at best can have covered its own

operating costs within the oasis. In the Roman period, there is no sign of any imperial interest in such high-value minerals in the western desert. There was certainly exploitation of alum resources in Kharga and Dakhla, mainly through shallow mining, it appears. Alum was kept as an imperial monopoly, but at the same time it was a highly valuable product with a broad market, used as a mordant in cloth dyeing. It may have been a significant component in what made Dakhla profitable in the Roman period. But it is hard to see that it can explain the enormous agricultural growth in the oasis in the period, because the population needed for alum extraction would not have been large. Nor, for that matter, is it easy to see why the demand should have been so much greater in the Roman period than before, even with a rise in textile production. It does not seem a good candidate for an important vector of change.

The major technological change that made the Great Oasis more viable as a destination and source had in fact taken place long before, with the introduction of the camel into the Western Desert. We do not have an exact date for this change. The camel seems to have reached Egypt before the Saite period, but the earliest mention of the camel in any text from the Saharan region probably dates to the fifth century BC, during the Persian domination, in a Demotic ostracon from Ain Manawir in the Kharga Oasis. We do not, as far as I know, have any secure archaeological confirmation of the camel's presence at an earlier date, but it is possible that the camel did come to the western desert under the Saites. The camel can cross the desert without an elaborate network of stations and wells to allow frequent watering, as donkeys require. This is not to say that camels will not drink more frequently when that is possible, but it is not necessary. A camel could go from the Nile valley to Kharga or Dakhla without any intermediate food or water stops, if none were available.

The impact of this fact on the economic realities of crossing the desert is dramatic. Not only does the camel not need to make fueling stops, it does not need to use part of its payload to carry fuel—that is, food and water—with it, although probably in most cases some food was brought along. It is not only a matter of replacing a short-range aircraft with a long-haul jet, but as if one had invented an airplane that did not need to use any of its payload for fuel. The fact that the number and wealth of settlements in both parts of the Great Oasis, as well as in the Small, or Bahariya, Oasis, rise very sharply during the Saite and Persian periods is very likely to be the result in large part of the introduction of the camel as the main freight-carrying vehicle. In Kharga, in fact, the growth in the Persian period is even more striking than in Dakhla. But all of this means that the camel was not, at all events, something new in the Roman period.

The camel is thus necessary to explain the Roman takeoff, but not sufficient. This condition had been satisfied more than four centuries before the Romans arrived. We must ask, what did the oases produce, or what could they have produced, that would be profitable when transported to the valley by camel? And, even more important, why did their production of it in the Roman period assume so much larger dimensions than in the Saite, Persian, and Ptolemaic periods?

A significant part of the answer emerges from the *Kellis Agricultural Account Book*, the 1786-line account of receipts and outlays for three years, kept by a steward of a unit of a large estate during the 360s. This text, as I remarked in the previous lecture, was my personal gateway to the study of the western desert and the oases. According to my analysis of this account, it showed a two-part estate economy. One part consisted of crops grown for local consumption by humans or animals, or for disbursement in payment for services, mainly wheat, barley, fodder crops, and wine. The other part was provided by high-value tree crops: olives, mainly in the

processed form of olive oil; dates; figs; and jujubes. These are not expended locally in the account, and this part of the estate's income therefore seems to constitute its effective surplus. These are all products that could have been transported to the valley by camel without their cost to the purchaser there being increased by more than 10-20 percent above production expense or at any rate their market value in the oasis, compared to the more than doubling of cost that would have taken place in the case of bulk commodities like wheat or wine. Just like Roman Tripolitania, as analyzed by David Mattingly, so Roman Dakhla apparently experienced an olive oil boom.

The hundreds of ostraca now yielded by the large House B1 at Trimithis are in general in accord with this overall picture, although they cannot offer the kind of synoptic picture of estate management that we find in a comprehensive account book. Instead, they show us many dozens of snapshots of individual economic acts: deliveries of hay in one set of accounts; many tiny ostraka used as tags, set in mud jar stoppers atop containers of wine or oil, giving at most a year, a place, and a personal name, and not always all three of these; receipts for hay or barley used as donkey feed; letters requesting the provision of olive oil or wine. [**Figure 5, ostrakon in stopper, about here**] In effect, we are seeing much the same structure and range of crops as in the Kellis accounts, but from different vantage points, and no doubt more of the local consumption side of the ledger than of the surplus for export.

This agricultural structure included a landlord, in our case Serenos, whose home is referred to as "the house" throughout the texts, along with his wife the *oikodespoina*, "mistress of the household," never named; a set of landholdings centered around wells in the surrounding countryside; and a process for collecting at least part of the produce centrally, evidently in Trimithis itself. At least one ostrakon shows that the wells themselves were an important part of

the property portfolio, as had been the case in Dakhla since the Old Kingdom, with farmers paying regular amounts of rental per day of water supply. The exact nature of the settlements around the wells is not made explicit, in general, but they are called by the Egyptian word for "water" or "well", *pmoun*, "the water of," followed by a name or description. In the ostraka in the first volume resulting from the Amheida excavations, there are more than forty such *pmoun* names. In many cases, the Greek word *hydreuma,* "well," appears, rather pleonastically, before *pmoun*, which for Greek-speaking people no doubt had come to be part of the name rather than a descriptor.

This description based on the ostraka tallies fairly well with what still seems to me to be a justified picture drawn from the Kellis account. There is one difference of standpoint that increasingly strikes me, not only from the ostraka referring to the activities of Serenos' household in B1 but from those deriving from other households and used as fill under Serenos' house and the adjoining street. In the account we see the goods of all kind coming into the possession of the pronoetes, the "I" of the Kellis account, and only some of them going out as disbursements. In the ostraca from Trimithis, by contrast, the goods that were consumed locally are overwhelmingly dominant, that is, grains and especially wine. The greater visibility of wine compared to the grains is easily explained: grain traveled in sacks, wine in jars with labels set into the stoppers. The dominance of consumables in the Trimithis documentation is probably to be explained by the fact that our ostraka come from the city house, where the family lived and things were consumed, rather than from a storehouse where goods for export would be concentrated.

But since the publication of the Kellis Account book, the picture has also been complicated in a different way. The account book also mentions cotton, although not in terribly

large quantities. Now, however, we have mentions of rather large quantities of cotton also in a few ostraka from Trimithis, and it begins to be clear that cotton production was not on a small scale. I showed in a recent article that all of our papyrological evidence for cotton production in Roman Egypt comes from or concerns the western oases; in Lower Nubia there was also production around Qasr Ibrim, as John Peter Wild has demonstrated. But the Nile valley in Egypt seems not to show any signs of cotton growing. This is actually a near inevitability, not a surprise, for cotton is a summer crop, and Egypt's arable land was not available during most of the summer, because it was underwater. Only after the Nile's flood receded could the land be sown with the winter crops, especially wheat and barley, which grew well in basins moistened by the flood. In the oases, by contrast, water flowed, but did not flood, 365 days a year. It would have been perfectly possible to grow cotton in the oases during the summer on the land that in the winter produced wheat or fodder crops.

Moreover, that two-season advantage was not limited to cotton. Tosha Dupras has shown that the bone collagen of Kellis residents buried in the cemeteries of that village contains an isotope of carbon that cannot be derived from the staple carbohydrates that are typical in the ancient Old World diet, that is, wheat and barley, but rather must involve maize (which as a New World crop not known in Egypt until modern times is out of the question in this context) or millet. And in fact millet has been found in significant quantities at Kellis, although it is never mentioned in the documents from any of the oases, only in one passage of Olympiodorus (FHG 4.64.33), pointed out many years ago by Guy Wagner. Olympiodorus tells us that millet is sometimes sowed three times a year in the oasis. We do not know if it was eaten directly by the people of Kellis; it is very rarely mentioned in the papyri from the Nile valley, either, and it does not seem to have been a valued part of the Egyptian diet. But its consumption could be an oasis

peculiarity. Even more likely, perhaps, is that it was absorbed indirectly, through feeding it to animals whose meat or milk was then consumed by humans. But either way, it was obviously another crop that could be grown in the summer and thus allow land to produce two crops a year, or even, as Olympiodorus says, three.

Some interesting conclusions may be drawn. First, the possibility of producing two crops a year could have helped offset the remoteness of the oases from the valley. The additional income would essentially compensate for the overhead on oasis production caused by transportation costs and eliminate the built-in disadvantage that the distance of the oases from the valley gave them. Second, neither cotton nor millet is known in Egypt before the Roman period, as far as I can determine. Their introduction may be a key, along with increasing consumption of olive oil, to the attractiveness of the oases as places for investment in land development in the early Roman period.

Seen in this light, the oases can have profited from a two-part strategy: on the one hand, autarky in essential foodstuffs, of which they grew just enough to feed themselves and their animals; on the other hand, a highly export-dependent focus that tied them in to a globalized economy in high-value tradable commodities, in their case particularly olive oil, cotton, and the more traditional dates, figs, and jujubes—along with alum, in all likelihood. The ability to use such a dual strategy would have constituted a remarkable competitive advantage for the oases. The one caveat I would offer is that Bahariya oasis, the Small Oasis of the papyri, probably was not self-sufficient in cereals. Because it was both much smaller and nearer to the valley than Dakhla, the resulting need for imports was not as significant a cost for this oasis. We have a considerable amount of evidence from customs declarations and letters showing that wheat was sent to the Small Oasis from the Fayyum and from Oxyrhynchos. It was more expensive in the

oasis than in the valley, but not enough to make the economics of growing wheat in that oasis attractive. Even today, Bahariya is almost totally dominated by fruit crops.

Now, we return to the organizational structure of the countryside. The Kellis papyri attest several villages, *kômai*. Kellis itself and its important counterpart Mesobe, the location of which is not known, both had the juridical status of the chief place of a toparchy, and some other villages are mentioned more casually without a clear indication of their status other than being a village. It is likely that Trimithis was also, during some part of its history, a village at the head of a toparchy, although it seems that it was a polis in the fourth century—and proud of it, to judge from the personification of polis (city) standing in the corner and watching the divine shenanigans in the paintings of room 1 in the House of Serenos that I will present in the next lecture.

As I noted, there are numerous mentions of places named after their well in both the Kellis account book and the ostraka from Trimithis, as well as in the Kellis papyri and ostraka. We know that some of these were referred to as *epoikia* ("settlements"), using a term well attested in the valley: Thio (*P.Kell.* 45), Pmoun Tametra (*P.Kell.* 41), and a couple of others the names of which are not preserved (*P.Kell.* 8, *P.Sijp.* 11a). But the term *epoikion* is not consistently used with either Thio or Pmoun Tametra, and it is entirely possible that some, even many, of the Pmoun names in the documentation were in fact classified as *epoikia* in more official parlance.

The oases today still show many remains of smaller rural settlements of the Roman period. As these have in general not been excavated, we are usually lacking the ability to link them to the place-names found in the documents and to connect a particular physical manifestation with a verbal description. Opportunities for future work are very great.

It is in this respect that our excavtions at Ain el-Gedida, located a few kilometers from Kellis, have a potentially important contribution to make. After the Supreme Council of Antiquities excavations of the 1990s, which revealed an area with communal cooking facilities but no recognizable houses, Colin Hope suggested that it might have been a monastery. That was part of what led Nicola Aravecchia to want to excavate there. But it has become clear that we need a different explanation for the existence of the community and of its communal facilities. Despite the presence of the church, which I described in the previous lecture, monasticism is not a sufficient explanation, because the settlement goes back at least to the second century. That is the probable date of what has now been recognized as a temple refitted as a pottery workshop in the fourth century. This temple matches in its form a number of other mud-brick temples in the Dakhla oasis. It means that Ain el-Gedida was not a newly-created community in the fourth century, but one that had existed for at least a couple of hundred years.

When you add up communal facilities, a pigeon-house, a pottery workshop, extensive storage areas, and production zones, the picture begins to take a different shape. Late monasteries can be found with such extensive economic facilities, like Deir Anba Hadra, or St. Symeon, on the west bank at Aswan, but there is no evidence that monasteries were so developed in the fourth century. This is why the discovery of an ostrakon that gives the name of a place, and probably of this settlement, as Pmoun Berri, the precise Coptic equivalent of Ain el-Gedida, or "New Well", is so important. And it is described in this ostrakon as a *georgion*, or an agricultural estate.

Here, I believe, we have a large estate, that model beloved of late antique Egyptian social history, and usually referred to as *epoikion*, amply documented in the papyri of the sixth century. As far as I am aware, never before has one been identified and excavated. We cannot say for sure

that *georgion* and *epoikion* have the same meaning. It is possible that a *georgion* was smaller than an *epoikion*; but it is also possible that something called one of these terms from the start might have kept its name regardless of its subsequent development. (We may be reminded of the fact that some *epoikia* clearly developed into full-scale villages.) A large part of our evidence for the term *georgion* in the papyri comes from the sixth-century Aphrodite papyri, especially from the land leases of the archive of Dioskoros. In those, the size of the whole *georgion* is only occasionally stated; certainly a *georgion* could be at least as large as 70 arouras (about 20 hectares) and include numerous capital installations—cisterns, reservoirs, outbuildings, vineyards, irrigation machinery, and no doubt much more.

Amheida surely had such settlements around it, whether large or small. Certainly many of these places lurk in the Pmoun names. If Pmoun Berri (a name also found in the Trimithis ostraka) was a *georgion*, so may Pmoun Arau, Pmoun Emboou, Pmoun Osire, and others have been. Or they may have been *epoikia*. Vestiges of these settlements can be found on the ground in the surroundings of Amheida. Not many kilometers away is a field full of these farm buildings, pigeon-houses on the upper floors and storage underneath . [**Figure 6, farm building, about here**]. Ain es-Sabil, where, as I mentioned in the first lecture, the SCA has found another fourth-century church, may have been a large example of the genre, if it was not a village; we do not yet know its ancient name. We have only scratched the surface in this respect; much even of Ain el-Gedida remains unexcavated, and we hope to return to it in the future.

These oasis settlements certainly differed from their counterparts in the Nile valley, if only because their existence depended on their own water source, unlike subsidiary agricultural enterprises in the valley, and their size was essentially a product of the flow of that source. But the same questions arise, especially whether they were fundamentally operated by wage labor, as

Jairus Banaji has argued for the *epoikia*, or by tenants, or by a combination. The communal facilities at Ain el-Gedida would certainly tend at first sight to push us in the direction of seeing this place as populated by wage laborers rather than permanently resident tenant families. It is not yet clear to us what parts of the site were sleeping facilities and how these were configured, but there is little to remind us of the family-style houses we find at Kellis or Trimithis.

One other point must be kept in mind, however. In Dakhla, the *ezba*, as such settlements are called in Arabic, was associated until relatively recent times with seasonality; the work there was done by people who lived in a larger village during most of the year but moved to the *ezba* without their families for the periods of intense seasonal agricultural work—planting, cultivation in the vineyards and orchards, harvest, processing of fruit, and storage of the products. It is possible that the people of Ain el-Gedida did not form a discrete population at all but were in the main residents of Kellis who spent only part of the year in the *georgion*. In the case of the many pigeonhouse-storehouse complexes near Amheida, there is nothing still visible recognizable as habitation, and they were quite likely dependent on a larger village or even the city itself for the residences of most of the people, most of the time.

An export-based economy located at a great distance from the Nile entailed an extensive land transportation industry. The ostraka are saturated with mentions of donkeys, mainly involved in local transportation aimed at concentrating the surplus in the hands of the landlord and seeing that provisions get into the hands of those who need them. They also mention camels and cameldrivers sufficiently to show their presence, but the records concerning how the surplus was sold and moved to the valley have not come to light at any of the excavated sites so far, just as they are largely lacking in the papyrus-producing cities of the valley. But just as we can glimpse in the ostraka from the Eastern Desert the outlines of a large corps of camel and donkey

drivers based in Coptos, we can infer the existence of a similar body of men who moved the oil, cotton, dates, figs, and alum from the oases to the valley. In the case of Dakhla, this may have meant Lykopolis (modern Assiut), but there were routes also to the Panopolite (now Akhmim) and to the Antaiopolite, and probably the cross-desert caravan route sent out many fingers into the valley cities.

Each round trip must have meant close to four weeks' absence from the oasis. It is likely that some men were away from homes half of the time or more. The letters from Kellis show, moreover, that even apart from the transportation business many oasites inevitably spent a considerable amount of time in the valley for all sorts of business. The traces of these absences—for example, to register documents in Alexandria—can be found as well in the archive of the nekrotaphoi, the funerary workers of Kysis, which I have assembled and hope to publish before long. Extensive absence was part of the price to be paid for the economic advantages I have enumerated. And many families had relatives living in the valley, whom they no doubt visited from time to time.

We know little about the consequences for the society of the oases resulting from this structural situation. Although I move very much into the realm of speculation here, it seems worth adducing the presence in the *nekrotaphoi* archive of several people without patronymics, whose only identification is their mother's name—what are elsewhere often referred to by scholars as *apatores*, although that term never occurs in the dossier. Indeed, Herbert Youtie pointed out in his celebrated article on the subject that this word becomes scarcer as the third century goes on and disappears after 314, by coincidence the end-date of this archive. Apart from one man who as a slave could not have had a legal father, there are at least 5 instances of non-paternal identification among the members of the nekrotaphic families whom I have been able to

identify. These stand against 26 individuals with patronymics and 12 for whom the parentage is not preserved. The fatherless thus come to 16.1% of those whose parentage is attested. By contrast, Youtie estimated from the Karanis money-tax registers compiled by the collector Sokrates in the second century that the percentage of *apatores* there was in the range of 2.l-2.6 %. Some self-consciousness of fatherless status is shown in the nekrotaphic archive, despite the absence of the term *apator*, by the fact that in every case except that of a woman appearing as a grandmother, the word *huios* (son) or *thugater* (daughter) is added before the mother's name, where otherwise a father's name would be given without any relational word; by contrast, *metros* ("of mother") is used only where a patronymic is also given.

The latter point is also true in the ostraka from Douch, ancient Kysis, in the southern part of the Kharga oasis, where the five volumes published to date do yield a number of people with mothers' names but no patronymics; any attempt at quantification there is made difficult by the fact that the ostraka come from a military milieu and patronymics are thus often not given. A count of the names in the Dakhla documentation, as collected by Klaas Worp in the oasis onomasticon, and including the first volume of the Trimithis ostraka, yields about 4.64% of mothers' names; Trimithis alone is 5.3%, but this difference is probably not significant. A figure around 5% seems likely: double that of the earlier Karanis registers, but much less than in the fairly small sample of the *nekrotaphoi*.

The interpretation of this information, even apart from legitimate questions about the statistical value of such small numbers, is difficult to assess. We may reasonably be reluctant to go back to the kind of view that Youtie rightly characterized as too judgmental, seeing illegitimacy as "a natural consequence of the loose morals of a low-class population." But given the date and the social milieu, we can exclude the reasons that produced the kind of illegitimacy

Youtie was most interested in, namely the technical lack of a legal father that resulted from Roman restrictions on legitimate marriage between some groups: the fact that soldiers on active service could not marry, and that the Gnomon of the Idios Logos (an elaborate code regulating legal status) prohibited marriage between some classes of the population. The nekrotaphic archive belongs entirely to the period of universal Roman citizenship after the Antonine Constitution of 212, and after the end of the marriage prohibition for soldiers, under Septimius Severus (ruled 193-211), thus the termination of the two regulations that Youtie cited as artificial generators of legal illegitimacy. The phenomenon still needs further exploration and explanation.

The mention of soldiers leads to the subject of security. As I remarked already, we know from the *Notitia Dignitatum* that Trimithis was the base of the Ala I Quadorum, and the military camp, *ta kastra* (the Greek word is a borrowing of the Latin *castra*) is mentioned in a fair number of documents from Kellis and in a graffito from a side room of the pillared hall at Amheida, excavated in 2011. But there is nothing at Amheida that looks anything like a military camp, and there was long among archaeologists working in Dakhla an instinct to think that the camp should have been at nearby El-Qasr—even though we know that the word *qasr* was, although derived from the Latin *castrum*, applied to many places in the eastern Roman world that were not in fact *castra*, so it cannot be taken for granted that a Qasr had once been a *castra*.

In 2006, Fred Leemhuis, who has for several years been doing conservation and reconstruction work on two of the larger Ottoman houses in El-Qasr, made an important discovery. He was walking one day from these houses toward an area where he was doing some trial excavation, in what he thinks was the oldest part of the town, when he noticed that a large but rather eroded wall along his path, probably originally part of the town wall, was built in bricks of a different size from that typical of the Islamic town. He had seen the wall many times,

its surface is highly eroded, and it took a slightly different light to bring the lines dividing the bricks to his attention.

The brick size was Roman, and he soon found a couple of other massive bastions built of the same bricks. More have now come to light elsewhere in this part of El-Qasr. It now seems to us beyond doubt that the reason we have not so far spotted anything at Amheida that looks even remotely like a fort for the Ala Quadorum is simply that the unit's headquarters was stationed at El-Qasr, just a few kilometers from the main urban area of Trimithis and obviously considered part of it. The oldest part of El-Qasr **looks** like a Roman fort in plan. Further excavations by the Egyptian authorities have found more of the wall, and it now appears that El-Qasr was a fort of dimensions and shape more or less identical to those of El-Deir in Kharga, and indeed to other tetrarchic forts in Egypt. It was called *pkastron nperro* ("the imperial camp") in Coptic, as we know from some ostraka excavated by our Egyptian colleagues.

At the same time, we can now say with confidence that the garrison of Dakhla was more complicated than the entry in the *Notitia Dignitatum* would suggest. As is increasingly recognized to be normal Roman practice, it included detachments from other units. One of these, the Tentyrites, turned up in an ostracon excavated at Amheida in 2008; the second is the Legio II Traiana, represented by the mention of an "optio of Asphynis" in an ostracon found in 2009. Asphynis, in the Latopolite nome, is where this legion was stationed in the fourth century, as we know from a centurion's will in the Columbia papyrus collection.

To these may be added an ostrakon found at Ain el-Gedida, not far from Kellis, which is a receipt for annona for the *hippotoxotai*, horse-mounted archers, engaged in *angareia* (i.e., probably maintaining the official postal service). So at least one of these units contained cavalry archers, no doubt useful in assuring the security of the desert roads. Perhaps these were the

Tentyrites, since the *Notitia* indicates that *equites sagittarii indigenae* ("native cavalry archers") were stationed at Tentyra. It is noteworthy that Dakhla has so far not given much sign on the surface of the numerous small forts characteristic of Kharga. But this may be deceiving. Like the fort at El-Qasr, these may yet be found.

A central question about the military is what it was doing in the oases. There is, as far as I know, no evidence at all for the military in the Great Oasis before the Tetrarchy, and probably not in the Small Oasis either. It appears that the building of a series of fairly uniform forts at Qaret el-Tub in Bahariya, El-Deir in Kharga, and El-Qasr in Dakhla is part of the same wave of military construction in the late 280s and continuing for a couple of decades that we can find elsewhere in Egypt. Nowhere is it clear if there was any direct threat that we would describe as military, or if instead we are dealing essentially with the assertion of control over lines of communication and perhaps a policing function. The building of forts in the Eastern Desert under Vespasian in the mid 70s, where before there had been unfortified stations, has been connected with the increasing nomadic raiding activity that is recorded in the ostraka from the station at Krokodilo. It is possible that the same thing was true in the Western Desert in the later third century. This was a much more difficult environment for nomads, but also for defenders, than the Eastern Desert, because water sources do not lie as near the surface in the western desert as they do in the eastern, meaning that a dense network of well-based stations is not feasible. The exposed lines across the desert were much longer in the west, and the forts are in any case in, sometimes on the edges of, the oases rather than out in the open desert of the high plateau.

It is then natural to wonder if the steep decline of the oasis settlements in the second half of the fourth century and beginning of the fifth is connected with greater insecurity. It will be evident that only secure transportation lines across the desert to the Nile valley can have enabled

the existence of the economy that I have tried to describe. There are, of course, other possible explanations for the decline, which have been canvassed from time to time. These include a decline in water supplies, the invasion of sand dunes, or the possibility that the decline is illusory and produced by our failure to recognize the later settlements archaeologically and to excavate them. There are good arguments against all of these hypotheses. First, the water level is not likely to have failed in widely dispersed locations at the same time, and Mut and Amheida even today have a fairly high level of humidity near the surface, even though throughout the oases water must in general be sought from much deeper wells than in antiquity. Second, sand dunes are local and never cover more than a fraction of any settlement at a given moment; one would not react to a dune invasion by abandoning a city or village, but by moving house. Third, Dakhla is well surveyed, and the pottery of the period after the fourth century is recognized. There is no good reason to think that our perception of occupation is seriously flawed. There are, after all, sites such as Deir Abu Metta, where occupation into the fifth century is known from the ceramic evidence.

It is possible that the question of security, in this case, affected not so much the security of the oasis itself as that of the roads between the oasis and the valley. If we remember that the Roman flourishing of the oases seems likely to have depended very heavily on the ability to export goods in the production of which the oases had a comparative advantage to the valley, we can see that even a modest reduction in the security of the desert roads could have had a devastating impact on the economic viability of the oases.

In his book on the oases, Guy Wagner cites a passage of John Moschus (112; PG 87C, 2976-8), in which a number of monks in the oasis were taken prisoner by marauding Mazikes. The dramatic date of the scene can be no later than the reign of Tiberius II (578-582). One of

these monks was taken to Hibis, the capital of the other part of the Great Oasis (modern Kharga), in order to try to get 24 solidi (one-third of a Roman pound of gold, or about 108 grams) to ransom a group of elderly and ailing monks. But the bishop could find only 8 solidi, and the Mazikes refused this; they took a senior monk instead. We have no way of assessing the typicality of this sixth-century vignette, but it may point to a greater level of insecurity in late antiquity and, eventually, an inability to sustain a society that depended on the security of its communications to the world of the Nile valley, hundreds of kilometers away.