

Missing Females in Roman Egypt

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On a late summer day in the year 362 of our era, in a village in one of the remotest corners of the Roman Empire, three Egyptians executed a contract for the sale of a slave girl.¹ The agreement was drawn up in the village of Kellis, today Ismant el-Kharab, in the Dakleh Oasis — then called the Mothite Nome after its capital, Mothis — of the western desert of Upper Egypt. This village lay more than 300 km from the nearest parts of the Nile Valley, itself already a distant and exotic locality to Roman tastes.² The contract is, in terms of law and form, nothing remarkable, although it is the latest such sale of a slave from Egypt to include a price. The participants — a married couple as sellers, a village carpenter as purchaser — look equally unremarkable. But in this transaction is encapsulated, I believe, an entire pattern of behavior that seems likely to have been typical for Egypt as a whole and probably much of the Roman Empire. Most of the elements of this pattern have been noted by students of ancient history, but the way in which they fit together into a pattern has generally been either ignored or denied. Neither this single contract nor the other evidence I shall cite can fairly be said to prove the reality of the pattern beyond all doubt, but I hope to offer persuasive arguments for its reality.

First, let us examine the details of this sale. The sellers were Psais son of Pekysis and his wife Tatoup, officially from Kellis but, they say, actually living in an *epoikion*, hamlet, the name of which is damaged beyond readability. The purchaser, Tithoes son of Petesis, is a resident of Kellis. Their names are all Egyptian, Psais coming from the god of fortune Shai and Tithoes from the local god Tutu.³ Tithoes, a carpenter, lived in part of a large and elaborate house in

¹ *P.Kellis* I 8, August-September, 362. I am indebted to Klaas Worp and John Whitehorne for knowledge of this text in advance of its publication. This article draws heavily on the work by Bruce Frier and me cited in note 5, and I must acknowledge particularly my debt to my co-author's demographic expertise. Equally, however, he is not to be held responsible for the speculations in this article which go beyond the account given in our book.

² For the excavations at Kellis, in my view the most interesting and important excavation of a site from Roman Egypt since the Michigan work at Karanis, see the introduction to *P.Kellis* I and the excavation reports cited there.

³ Tatoup is in form also a theophoric name, but I do not know its derivation.

Kellis which has been excavated, and several other texts from the excavation concern him; the sellers lived, as I have mentioned, in a nearby hamlet but used Kellis as their market center and are otherwise unknown to us. At least one of Tithoes' children bore a distinctively Christian name, suggesting that Tithoes himself was probably Christian. Whether he was born into that faith or converted, however, we do not know. The sellers, who execute the contract, are stated to be illiterate, and the contract is written in a competent documentary hand by one Timotheos son of Harpokration, a former magistrate — presumably of Mothis, the local nome's capital city.

The slave is described carefully as 'the slave girl belonging to us, picked up from the ground (*χαμαίρετος*, an otherwise unattested Greek word),⁴ nursed by me the aforementioned woman with my own milk.' She is not, however, given a name in the document, and the comparatively low price of two solidi suggested to the editor that she must have been 'little more than a toddler at the time of the sale.' Presumably, however, she was old enough that the purchasers thought she had a reasonable chance of surviving; two solidi, although a low price for a slave, was enough to support a small family for a year, and one would not want to risk it on a baby still likely to suffer from the enormous infant mortality that this society experienced.⁵

The sequence of events indicated by the sale contract is important. A baby girl was exposed, left in the open, either in Kellis itself or in the small hamlet where the couple lived. We cannot tell which of these from the information given. They took her up and brought her to the point at which she is sold, perhaps a couple of years later,⁶ with the wife in the couple nursing the baby herself. The child was then sold to a relatively prosperous artisan in the local center for what would have been quite a lot of money to most Egyptian villagers — a reasonable return, they may have thought, for their personal involvement and perhaps their investment in earlier foundlings who had not survived infancy. Although juridically a village and not the metropolis of the nome, Kellis should probably be seen more as a small urban center than as a rural village. It had large houses, the residences of important landowners, a culture that included Greek and Coptic literary texts (both classical and Manichaean), and monumental architecture — not the characteristics of ordinary Egyptian villages in this period.⁷

⁴ One usually finds *ἀναίρετος* or *κοπριάρετος*; cf. I. Biezunska-Matowist, 'Die expositio von Kindern als Quelle des Skavenbeschaffung im griechisch-römischen Ägypten', *Jahrbuch für Wirtschaftsgeschichte* 1971/II, 129-33.

⁵ A third dead by the age of a year, roughly. See R. S. Bagnall and B.W. Frier, *The Demography of Roman Egypt*, Cambridge 1994, 32-6, 151-3.

⁶ Perhaps at not much more than a year, even. See below, Appendix I, for a discussion of the economics of the business.

⁷ See on this point R.S. Bagnall, *Egypt in Late Antiquity*, Princeton 1993, 310-9.

A building complex most of which is yet to be excavated is apparently even larger than what has been brought to light so far.

As I remarked earlier, some of the phenomena we see here have long been described as normal for the Roman Empire. A recent article by William Harris on 'Child-Exposure in the Roman Empire' has argued that exposure was relatively common, that more girl babies than boy babies probably were exposed, and that exposure was an important source of slaves.⁸ Harris is, however, reluctant to see any overall pattern in which such exposure has a significant effect on the demographic structure of society, particularly on the sex ratio. It is precisely such a pattern that I am going to try to elucidate. Strictly speaking, I shall be talking about evidence from Egypt, and one could argue — as has often been claimed — that this province was peculiar. But this claim has suffered significant blows in recent years and to my somewhat biased view no longer seems even intellectually respectable.⁹ For most demographic purposes, I think it fair to say that 'the basic demographic attributes of Roman Egypt are, at the least, thoroughly at home in the Mediterranean; they tend to recur in historical Mediterranean populations with considerable regularity. Nor is there any strong *a priori* reason why most of these attributes should be regarded as unique to Egypt among Roman provinces.'¹⁰

The attributes in question have been discovered by an analysis of the information provided by the census returns from Roman Egypt, from which we know some 1100 persons. This body of information is not without its difficulties, and I must sketch these briefly. The declarations are very unevenly distributed in space and time, coming predominantly from the second and early third century and from the Arsinoite Nome, today's Fayum province. These non-

⁸ *JRS* 84, 1994, 1-22; see 3-11 for the commonness of exposure, 4-5 and 11 for the prevalence of girls among those exposed, and 18-19 for exposure as source of slaves. Cf. also Harris's earlier note on the possibility of widespread exposure in *CQ* n.s. 32, 1982, 114-6. The 1994 article has extensive bibliography of other discussions of the subject, which I do not repeat here; for the papyri, however, see the remarks of I. Bieżunska-Małowist, 'Les enfants-esclaves à la lumière des papyrus,' *Hommages à M. Renard II* (= Collection Latomus 102), Brussels 1969, 91-6 and her article in *Jahrbuch für Wirtschaftsgeschichte* 1971/II, 129-33, concentrating on the profitability of the raising of children as slaves (on which see Appendix I, below).

⁹ See, for example, Dominic Rathbone, 'The Ancient Economy and Graeco-Roman Egypt', *Egitto e storia antica dall'Ellenismo all'età araba. Bilancio di un confronto*, ed. Lucia Criscuolo and G. Geraci, Bologna 1989, 159-76; A.K. Bowman and Dominic Rathbone, 'Cities and Administration in Roman Egypt,' *JRS* 82, 1992, 107-27.

¹⁰ Bagnall and Frier (n. 5), 173. T. Parkin, in his view of this book (*BMCR* 6, 1995, 88-98 at 94) quite reasonably views this statement as unproven, but he does not argue that it is wrong.

random characteristics, however, do not seem to have important consequences for the analysis. The sample is also unduly biased toward returns originating in cities, but this can be corrected by a simple weighting of numbers. More serious is a clear tendency for males to vanish in the years just before and around the age at which one became liable for capitation taxes, either by simple failure to register them or by their migration from their home villages to the more anonymous cities. We shall have to return to this distortion. Overall, though, the single greatest deficiency of our data base is simply that it is not large enough. For the most part, conclusions drawn from the entire population have proven to be fairly robust, but those drawn from subsets diminish in reliability very sharply as one moves to smaller numbers.¹¹

My concern here is not with any effect that a *general* tendency to expose infants might have had on the demography of Egypt. If infants were exposed without regard for sex, and equal proportions of boys and girls died as a result, the only effect would have been to change slightly the shape of the fertility curve; exposure would in this case have been only a postnatal form of birth control. This actually does not seem to have been the case, however, for the fertility curve derivable from the census declarations shows the distinctive shape of a population in which no such control is exercised.¹² The Egyptians used other means to prevent overpopulation, particularly breastfeeding and the failure of women to remarry after being widowed or divorced.¹³ My interest, rather, lies in what results *differential* exposure might have had: If it is true — as the commonplaces of the literary sources would suggest — that more girls than boys were exposed, what results would follow for the sex ratio in the free and slave populations, and can those results be identified in the data we have? And would the results even be visible against the backdrop of extremely high infant mortality?

The first problem we must acknowledge is that a *small* differential probably could *not* be identified in our data. It should manifest itself in a higher than natural sex ratio for the free population: the sex ratio is generally expressed as

¹¹ The problems that we faced with the Egyptian data are in many respects paralleled by difficulties in the study of the Florentine *catasto*: see David Herlihy and Christiane Klapisch-Zuber, *Tuscans and their Families. A Study of the Florentine Catasto of 1427*, New Haven 1985, chapters 5 and 6. But the size of their data set is enormously greater than ours.

¹² This has been shown in detail by B.W. Frier, 'Natural Fertility and Family Limitation in Roman Marriage', *CP* 89, 1994, 318-33. Where birth control (including postnatal) is used to control family size, the fertility curve drops off much more sharply than in populations where no such controls are in use, because these measures are usually adopted after families have reached the desired size, not in the early years of marriage.

¹³ For a discussion of overpopulation as the major threat to the stability of ancient populations, see Frier, *CAH XI*, forthcoming.

the number of males per 100 females in the population, and in most modern populations it is around 105 at birth and declines subsequently toward 100 as the higher mortality rate of males affects it. We do find a sex ratio in the overall Egyptian data diverging sharply from this norm, namely a startlingly high 120.4.¹⁴ But two caveats are needed. First, that ratio comes from a body of material in which the nome capitals are overrepresented compared to the villages. These metropoleis, as they are called, show a much higher sex ratio than the villages — where the ratio is only 88.2, more females than males — do, and when we correct for this bias, the ratio drops to a less dramatic but still high 111.4.¹⁵ Second, the ratio is not consistent through the period for which we have declarations — the relatively few first-century declarations greatly underrepresent women. Nor does it remain constant through the lifespan — the reported sex ratio drops steadily as boys approach the age at which they become liable to taxation and only recovers thereafter. Its pattern contains enough swings to appear chaotic at first glance, but it shows that the male edge grows with increasing speed as the population ages. In all likelihood, the largest component of the early distortion results from the deliberate concealment of young village males approaching the age of taxation. Part — offsetting the preceding — also probably comes from the underreporting of very young females, especially in the cities. Part, too, comes from the tendency of random fluctuations to be greater in small samples of the evidence than they are in reality.¹⁶ At all events, it is very probable that the sex ratio in the free population was rather higher than natural levels, but the data are subject to enough ‘uncertainties and biases’ that a skeptical observer may not feel a great deal of confidence in our assessment of the sex ratio.¹⁷

A more detailed analysis of a limited body of the best-preserved declarations helps to clarify matters. For this purpose, only those returns with relatively young parents — 35 or under — with their children were used.¹⁸ In this way one gets some sense of what those families which were still pretty much intact, from which children had not yet moved out, looked like; only free persons are taken into account. The metropolitan families had more than twice as many sons as daughters; those from the village reported 34 daughters to 19 sons. Both of these inspire caution, even suspicion, for both underreporting of daughters (in the

¹⁴ Bagnall and Frier, *Demography* (n. 5), 93.

¹⁵ Florence also had a much higher sex ratio than the Tuscan countryside and even than other Tuscan cities.

¹⁶ Bagnall and Frier, *Demography* (n. 5), 103 shows the male age structure in the census declarations compared with that visible in tax lists from Fayum villages; the tax lists show less violent swings while displaying much the same shape graph. If we had more data, these swings would gradually diminish.

¹⁷ So Parkin (n. 10).

¹⁸ See Bagnall and Frier, *Demography* (n. 5), 152-3 for this discussion.

metropoleis) and concealment of sons (in the villages) seem very probable. They do, however, survive enlargement of the data to include all free persons with preserved ages: in the villages, females under 15 outnumber males 57 to 42, but in the metropoleis males outnumber females 62 to 33. We note, 'the lopsided juvenile sex ratio in villages is probably not significant, since it is implausible that village parents practiced active postnatal sexual discrimination in favor of daughters; concealment of sons is the more obvious explanation. By contrast, the metropolitan sex ratio for juveniles cannot be brought into balance even if large allowance is made for underreporting of very young girls.'

Despite all of the deficiencies of our data, then, it seems very likely that a certain number of free female children in the metropoleis were eliminated. There are at least three significant methods by which this may have occurred, and all three may have been operative. First, female babies may have been exposed and died. Second, they may have been exposed and turned into slaves, like the object of the sale from Kellis. Third, girls may have been treated less well and fed less generously, resulting in an even higher mortality rate in infancy and early childhood than that from which boys suffered. The last of these, if it was operative, probably had most of its effects before the age of five, for the gap between boys and girls does not widen after that age. But we are unlikely to be able to tell the difference between the effects of exposure followed by death and death resulting from poor treatment at an early age; the robustness of the data from year to year in the population under five is simply inadequate.

If the entire shortage of young females in the metropoleis was caused by death, either through exposure or through maltreatment, it would in principle have no impact on the sex ratio in the slave population. One might even expect to find that slave-born girls were also differentially eliminated, in which case the slave population would have a sex ratio like that of the free population, with males overrepresented.¹⁹ If part of the explanation for the missing females is exposure followed by enslavement, however, it ought to be the case that the slave population would be more female than could have resulted from births to slave women, as these should have yielded roughly equal numbers of males and females. *Such a pattern of femaleness is in fact present.*

Our population sample in the census declarations includes a total of 118 slaves, or about 11 percent of the population. The declarations from the cities, which are about half of the total, include about 60 percent of all slaves. But, because village households are on average larger than metropolitan ones, slaves are 13.4 percent of the urban population and only 8.5 percent of that of the villages. Given the significant concentration of wealth in the nome capitals, this is not a particularly surprising state of things. Overall, slaves will have made up just

¹⁹ This seems to be the view of Harris, 'Child-exposure' (n. 8), 6, although he does not believe there is any evidence for a high sex ratio in the free population. Cf. the next note.

about exactly 10 percent of the total weighted population. This is in line with most estimates of this figure by other scholars.

When we look at the sex ratio among slaves, however, the picture becomes more interesting. Men and women occur in roughly equal numbers in the servile population of the metropoleis, but in the villages women are overwhelmingly predominant, 36 women but only 6 men. It is possible that these numbers are somewhat unduly influenced by a few households, but it seems unlikely that the pattern would disappear with more evidence. Overall, women slaves outnumber men by two to one, 68 to 34.

If we remember that the villages are greatly underrepresented in the data and correct the raw figures accordingly, the weighted population looks somewhat different, with femaleness enhanced. The total female to male ratio is now about 2.5:1 (104 to 40). But the great preponderance of women is changed only in degree. Why are there more women slaves? We cannot leap immediately to the conclusion that exposure is responsible. One immediately evident answer, indeed, is that there are no male slaves older than 32, and only one older than 29, whereas women continue to appear into their forties. It is impossible to escape the conclusion that women were often retained as slaves as long as they appeared to be fertile, whereas social expectations and economic decisions led to the manumission of men by about age 30. (That does not mean that there were not some male slaves over 30, only that they must not have been numerous.)²⁰ In our weighted population, using only slaves for whom ages are preserved, there would be 69 women to 29 men; eliminating those over the age of 30, there would be 48 women to 28 men.

The differential pattern of manumission, therefore, accounts for a bit over half of the surplus of female slaves visible in the raw numbers. But even after we restrict our inquiry to slaves aged 30 and under, women still outnumber men by better than three to two, and by nearly three to one in the villages.²¹ Although

²⁰ There is not enough documentation of the ages of freedmen in the papyri to provide confirmation of this view. I. Biezunska-Małowist, *L'esclavage dans l'Égypte gréco-romaine* II, Wrocław 1977, 146 notes that despite the lack of specific ages preserved, freedmen generally seem relatively young and certainly active, a sign that they were manumitted when a considerable part of their working life lay ahead of them. She also points out (p. 145) that some 60 percent of attested freedmen are male, although the statistical value of this figure may be limited not only by the quantity of data but by the fact that men's activities are more likely to have generated documentation.

²¹ Harris (n. 8), 6 remarks that 'an unbalanced sex ratio probably did prevail in the population of slaves, and one of the mechanisms by which this was brought about was perhaps the selective exposure of girls who were born to slave mothers.' He thus apparently supposes that the sex ratio among slaves was skewed toward masculinity, the reverse of what the data show. This view is presumably at the root of

births to slave women were probably the largest source of slaves in Roman Egypt, then, there must have been some other source to have yielded the remainder of the young female slaves.

Before we go on to pursue the implications of this statement, we ought to ask how vulnerable it is to attack. Its main weakness is simply the rather small size of the population under study and the very serious possibility that the results are not statistically reliable. Until such time as new evidence allows a considerably larger number of households to be described, particularly in villages, direct improvement of the situation is impossible. But the essential point is the numerical preponderance of females in the slave population. If *this* could be confirmed from other sources, the trustworthiness of this essential link in the argument would be greatly strengthened.

Sales of slaves are one such source of information. The tabulation of these sales (for the period up to the end of the third century) in Hans-Joachim Drexhage's recent book on prices²² shows 58 female and 41 male slaves. If only those with preserved ages are counted, the gap is 41 to 22. For slaves under 30, females outnumber males 37 to 15.²³ (Curiously enough, more sales of males over 30 than of women are found, but the numbers are small and probably reflect some circumstances we cannot determine.) Not too much reliance should be put on the exact ratios; if these are open to doubt in the case of the census declarations, they are that much more doubtful here and will no doubt change as more data are published. But the central tendency of the data is the same and is not in doubt. All of the female:male ratios in both sets of data fall in the range from 3:2 to 5:2. Such numbers are consonant with the general conclusion reached by modern scholars that slaves of working age in Egypt were owned primarily for personal use, not for economic exploitation: they were, in other words, items of consumption and not of investment.²⁴

If, then, we need a source of young female slaves other than birth to a slave mother, it is hard to see what this source can have been except the exposure of free female infants unwanted as members of their birth families but valued by those who rescued them from death as potential salable assets, exactly what we

his failure to see some of the connections for which I am arguing here, which otherwise fit rather well with his views.

²² *Preise, Mieten/Pachten, Kosten und Löhne im römischen Ägypten*, Sankt-Katharinen 1991, 271-9.

²³ Drexhage (n. 22), 254. The mutability of such numbers may be seen by looking back at the statement of I. Biezunska-Małowist (n. 20), 145 that men and women occur with roughly even frequency in sales; that statement rested on a much older list published by O. Montevecchi.

²⁴ See generally I. Biezunska-Małowist (n. 20), 73-108; some slaves were employed in agriculture, few in artisanry.

see in the Kellis sale.²⁵ And there is other evidence that the Kellis sale is not an isolated phenomenon. The most salient is the corpus of contracts for hire of a wet-nurse, which have been collected and studied by Mariadele Manca Masciadri and Orsolina Montevocchi.²⁶ Of the 31 infants put out for paid nursing of whom we know the legal status, 23 were slaves, or three-quarters. For 19 of those 23 slaves, we are given information about the way in which they became slaves: 12 of 19 were picked up from exposure, and only 7 are described as something else, mainly *engona*, i.e., born in the household. And they are predominantly female. Slaves also account for a high percentage of infants put out to nursing in contracts that have not survived but are listed in summary form in the registers of the record office of Tebtunis.²⁷

There is no point in pushing the exact percentages here, but the evidence is certainly consonant with our hypothesis that exposure of girl babies was a major source of slaves and, specifically, of a disproportionately female slave population. Assessing the impact of enough exposed girls to account for these surplus female slaves is tricky, and given the problems in our data base an attempt at great precision is probably not particularly valuable. A rough calculation, however, might go as follows: The total weighted population of females under age 30 in the declarations would have been 326; of these, 48, or 14.7 percent would be slaves. Arriving at a comparable figure for males is very difficult because of what appears to be the concealment of village males, to which I have already referred. If for the sake of hypothesis we imagine that the sex ratio for the under-30 population in villages was really 100, we would obtain a restored weighted population of 359, of whom only 28 were slaves, or 7.8 percent. Applying that percentage to the female population would have yielded a slave population of about 26, rather than the actual 48.

This result then indicates that 22 additional females in this sample population had been enslaved beyond the number that one would expect. Once again, this figure leaves out of account any undifferentiated impact of exposure. For all we know, a considerable part of the male slaves may come from exposure as

²⁵ One might of course hypothesize large-scale importation into Egypt of slave girls from elsewhere, but this is most unlikely: (1) It would have had to begin with infancy to produce the pattern we see; (2) there is no evidence for such a trade in the documentation; and (3) such a hypothesis only displaces the problem from Egypt to some other place. The other possibility is sales of infants by their own parents, which in a sense would be another means of accomplishing the same general transfer of females from the free population to the slave that exposure produced. Just such a pattern, at least in times of economic stress, has been suggested by M. Manca Masciadri and O. Montevocchi, *I contratti di baliatico* (= *Corpus Papyrorum Graecarum* I), Milan 1984.

²⁶ *I contratti di baliatico* (= *Corpus Papyrorum Graecarum* I), Milan 1984.

²⁷ See below, Appendix II, on the problems connected with the interpretation of these registers.

well, but that is invisible to us, and I am considering here only that portion of the female slave population in excess of the male. Those 22 females are 6.7 percent of the total females in the under-30 bracket. If that represented the percentage differentially exposed, the result would be — and this is both assuming an inherent move toward 100 and leaving out of account all other sources for skewing of the sex ratio — a sex ratio for the free population of about 107. If, on the other hand, we assume that preferential treatment of male children cancelled the normal drift of the sex ratio from the 105 found at birth down towards 100, a sex ratio among the free of about 113 would result.

At this point we may recall that a weighted sex ratio of 111.4 for the free population is actually found in the declarations. This number does not, however, add back in some village males to compensate for underreporting, as we have just done. When this is done, the result should be about 115. That would be compatible with an overall ratio for the population somewhere in the range of 108, a figure that we arrived at on other grounds as a reasonable approximation.²⁸

The reader may at this point feel that I have pushed the numbers entirely too far. But I think that one further point about the Egyptian situation is worth making briefly. This is that female slaves in the villages are also on average younger than those in the cities (mean age of 18 in the village vs. 22 in the city). This fact also is perfectly compatible with the hypothesis that villagers made a practice of taking in exposed infants from the metropoleis, reared them, and eventually sold some of them back to urban residents.²⁹ This hypothesis would, of course, help to explain why the missing young females are a facet of the metropolitan figures but not of the village numbers. In this way the cycle would have been completed. Metropolitans would be rid of unwanted daughters, shift the risk of rearing them as slaves to villagers for whom the costs were low, and then buy them back once they had passed infant mortality, were no longer recognizable, and might even be starting to be useful as servants.

There are obvious difficulties remaining. In the absence of more than approximate sex ratios, we cannot tell if some allowance needs to be made for exposed babies who died rather than being enslaved. We cannot be certain if some of the missing males in the villages are actually in the metropoleis, registered

²⁸ Bagnall and Frier, *Demography* (n. 5), 95; cf. 108 for reluctance to argue strongly for anything except a range of 100 to 110.

²⁹ The number of wet-nursing contracts for slaves collected from exposure is not large enough to verify or disprove any hypothesis about the ownership of the slaves being reared in this fashion. If one supposed that villagers operating on their own account usually did their own nursing (as in the Kellis contract) and metropolitan entrepreneurs usually hired nurses, one would at least find some support in *CPGr.* I 15 and 24; but in most of the relevant contracts some element of the needed data is lacking.

apart from their birth families. The examination of young families, however, with their surplus of males over females, suggests that the high metropolitan sex ratio exists independently of migration and cannot entirely be explained by it.

At this point it seems useful to try to provide a wider perspective for the patterns that I believe can be seen, even if imperfectly, in the Egyptian data. The first such context is that of Ptolemaic Egypt. We have a significant body of census-related accounts from the third-century B.C. Arsinoite Nome, which are currently being edited or reedited by Willy Clarysse and Dorothy Thompson; I am indebted to them for the use of some of their data before publication.³⁰ The figures we have are from a papyrus dating between 253 and 230 and are almost complete for adults, omitting only a small number of soldiers. They do not, however, include children, an important omission from our point of view. At least one may note that they concern the same region from which come most of our census declarations in the Roman period.

The families of members of the army, both military settlers and those on active duty, show a sex ratio of 115.8; given the masculine character of military service, this is perhaps not surprising. The civilian population, however, shows a sex ratio of 90.9. This is remarkably low, but it will not escape notice that it bears a certain resemblance to the village figures for the Roman period. It is of course possible to argue that concealment of males is again at work in the Ptolemaic data, but Ptolemaic capitation taxes, although bewilderingly numerous, were far less burdensome in amount than Roman taxation, and the argument is thus less persuasive. Also interesting is the fact that within the civilian population there is a distinction between those persons legally designated as Hellenes and those classified as Egyptians. These categories are ones of legal status, and many — perhaps most — of those legally listed as Hellenes were of varieties at which most classical Greeks would have turned up their noses: Macedonians, Thracians, Paeonians, Jews, and so on.³¹ Still, their sex ratio is 97.5, while that of the Egyptians is 88: identical to the sex ratio in village returns of the Roman period.

But matters are more complex. We also have a number of census lists giving not aggregate numbers but the members of individual households. When these are classified and counted up, *they* yield a sex ratio of 105. To be sure, the numbers involved are much smaller than the aggregate nome figures in the account cited earlier. But they are still puzzling. I do not think that the lists of names include children under 14, either, so a much different sex ratio under the age of 14 cannot be the explanation. All in all, however, the Ptolemaic evidence makes me wonder if our belief that the Roman village sex ratio is to be ex-

³⁰ To appear as *Counting the People* in *Collectanea Hellenistica* (Leuven).

³¹ On this matter see my article on 'The People of the Roman Fayum', in *Portraits and Masks: Burial Customs in Roman Egypt*, ed. M.L. Bierbrier (London, British Museum, forthcoming in 1997).

plained by concealment of teenaged males is entirely justified. Even the 'Hellenes' have a much lower sex ratio than seems probable for the Roman period. It should be remembered that nearly all of the population in the Ptolemaic period lived in villages; the metropolis of the nome had not yet become a significant city. It is therefore at least possible that, even with some allowance for underrepresentation of males, the figures for Egyptians in the Ptolemaic period and for villagers in the Roman period together point to an underlying demographic reality very different from what the population of the Roman period as a totality experienced.

The data from the Florentine *catasto* of 1427 provide another interesting point of comparison. The sex ratio graph by age is remarkably similar to that for Roman Egypt as far as the mid-forties, after which they diverge very considerably.³² Very high (120 and above) ratios for ages under 10 drop toward equality as the age of tax liability approaches; the ratio then rises to a new peak in the mid-twenties, only to drop off again into the middle thirties (this may be only a blip), after which it again rises. In the case of the Florentine population, the incidence of plague needs to be taken into account, but still the similarity is striking. So too are the explanations offered, including nonreporting of those who died young and devaluation of female infants.³³ And Tuscans also had incentives to conceal young men reaching the age of liability for the head tax, or at least to make sure that girls — whose gender had hardly mattered until now — were reported as such.³⁴

Despite all of these distortions in reporting, however, the authors of the principal study of the Tuscan data conclude that 'it is impossible to believe that the recording of women would have been uniformly poor at nearly every level of life. Rather, it appears incontrovertible that the Tuscan population was marked by a true deficit of females. Social factors of some sort must have deprived girls of their normally better chances of survival.'³⁵ They go on to canvass particularly infanticide and abandonment, the latter of which led to being a foundling, left to be raised by charitable institutions. Girls made up 70 percent of the foundling population.³⁶ We may be reminded that this is about the same percentage that females formed of the slave population of Roman Egypt, according to the census declarations (72.2, to be precise, in our weighted population).

Still more striking is the fact that 'wealthy Florentines (and Tuscans) declared in their households substantially fewer women than did the poor, and ur-

³² Herlihy and Klapisch-Zuber (n. 11), 133, 143: Egypt shows a sharp rise in the sex ratio, Tuscany a much gentler one.

³³ Herlihy and Klapisch-Zuber (n. 11), 135-44.

³⁴ Herlihy and Klapisch-Zuber (n. 11), 138.

³⁵ Herlihy and Klapisch-Zuber (n. 11), 144.

³⁶ Herlihy and Klapisch-Zuber (n. 11), 145.

ban families also showed fewer females than do homes in the countryside.³⁷ As to the latter, the difference (112 urban vs. 109 rural) was not as sharp as in Roman Egypt; but for Florence itself the ratio was 117.6 vs. 108.9, and there was considerable variation from town to town and district to district.³⁸ In part this difference reflects migration, a factor we have supposed played some role in Roman Egypt as well, but in part it was a matter of differential nutrition, abandonment, and infanticide.

The Tuscan data certainly remind us just how variable by local circumstances these matters must have been, and they may make it less surprising that we find in the Ptolemaic Fayum a discrepancy between overall numbers and the lists for particular villages. They are also useful in showing that similar patterns in the population figures may have roots in a mixture of similar and different causes. Roman Egypt had no foundling hospitals or nunneries, and young children were not put out in the households of others as free servants. Although slavery existed in Tuscany, it was a negligible factor. Tuscany may, however, have been almost as urbanized as Roman Egypt.

What assessment of the situation in Roman Egypt are we to offer, then? All of the data to which I have called attention may point to a particular pattern of treatment of females in the society of Roman Egypt: less care in upbringing than for males, shorter life expectancy, higher rate of exposure as infants, prolonged time in slavery, maximum exploitation as breeders of slaves. This pattern seems particularly connected with the cities, but the villages are directly implicated in it by reciprocal roles in the cycle, acting as collectors of exposed infants, and as rearers and sellers of slaves. We have become accustomed to a picture of Egyptian society as far less male-dominated and less prone to subjugate women than was normal in classical Greece. Egyptian women were, already before the Hellenistic period, able to own property and to marry and divorce by their own decision. I think it is fair to say that the common wisdom among historians of Hellenistic and Roman Egypt is that the Greek immigrants into Egypt in the Ptolemaic period absorbed and appropriated much of the juridical and social freedom ascribed to Egyptian women. It is possible that the low sex ratios we have seen in the village populations of both Ptolemaic and Roman periods reflect an underlying Egyptian regime which even after all allowances for faulty reporting shows a demographically more favorable situation for women than in, let us say, the pre-1960s rural population of China, where excess female mortality in infancy ranged from 5 to 15 percent.³⁹

For the Roman period, the melioristic process is usually assumed to have continued, benefiting from women's ready ownership of property, including

³⁷ Herlihy and Klapisch-Zuber (n. 11), 151.

³⁸ Herlihy and Klapisch-Zuber (n. 11), 156-7.

³⁹ See Ansley J. Coale and Judith Banister, 'Five Decades of Missing Females in China,' *Demography* 31, 1994, 459-79.

land, and freedom of divorce under Roman law. This view has become characteristic of scholarship based on the papyri. Sarah Pomeroy, for example, has reached the 'inevitable conclusion' that 'under Roman rule women gained in economic and legal capacity.'⁴⁰ How are we to reconcile this picture of economic and legal gains with the darker demographic view sketched here?

It would far exceed the scope of a single paper to attempt a general assessment of the situation of women in Roman Egypt. The explanation or explanations of the higher rate of infant exposure of females, and of their upbringing as slaves, may well be complex. But two points in the evidence seem to me to suggest the direction in which we ought to look. The first is that the Ptolemaic situation seems to have been very different; it is only with the Roman period that exposure begins to be a significant phenomenon. The second is that it is mainly an urban phenomenon, remaining foreign to the Egyptian population of the countryside. These two facts suggest that we are looking here at one of the consequences of the more systematic imposition of Graeco-Roman patterns of the organization of social and economic life that we find in Egypt from the reign of Augustus on. Paradoxically, the Ptolemaic period saw relatively little development of characteristic Greek institutions in the Egyptian countryside, above all because the Ptolemies created few Greek cities in Egypt. It is unfortunate that the Ptolemaic records lack individual ages, making it impossible to calculate the age-specific sex ratio, an important tool in reconstructing the total picture. But the fact that our information is also based on nearly complete figures for one nome, amounting to more than 58,000 individuals, in large part outweighs such regrets. These figures probably indicate that women in this Ptolemaic population had slightly higher life expectancies than men.

The Roman period, on the other hand, saw from the beginning a development of the metropoleis of the nomes as Greek cities with a privileged class of landowning notables, whom the Romans treated juridically as Egyptians but fiscally as a group apart.⁴¹ By the third century this class was ready to become a full-fledged curial order like those in Greek cities throughout the Roman East. In keeping with this pattern, the Roman period in general saw a growth in the hierarchical relationship of city and countryside, and I do not think it is far-fetched to see the differential demographic patterns I have tried to describe as a part of that transformation. It is entirely possible — and the Florentine data would suggest very strongly — that the urban elites, followed to a lesser degree by the rest of the urban population, showed significantly different patterns of childrearing; exposure may have been both a strategy of the rich to minimize the

⁴⁰ 'Women in Roman Egypt,' *ANRW* II 10.1, 1988, 708-23 at 723.

⁴¹ See Bowman and Rathbone (n. 9); J. Méléze Modrzejewski, 'Entre la cité et le fisc: le statut grec dans l'Égypte romaine,' *Symposion 1982*, Valencia 1985, 241-80 = *Droit impérial et traditions locales dans l'Égypte romaine*, Aldershot 1990, chapter I.

partition of wealth and a response of the poorer residents of the cities to the survival pressures they faced. Greater differentiation between rich and poor was characteristic of the Roman empire in many respects, of course, and it would not be surprising to see it operating as well in the realms of life, reproduction, and death.

In any event, we should probably be cautious about assuming that the role of women in the transmission of property that we see in the papyri of the Roman period actually represents their well-being; it is entirely possible, in my view likely, that the Ptolemaic period was more favorable for women — and perhaps for some other less privileged social groups — than was the Roman.⁴² We historians may still be too prone to our desire to like the people we study, and the people of the Roman papyri, especially the members of the propertied classes who produced most of the papyri we read, are perhaps just a bit too sympathetic to our own middle-class tastes. Lurking in our all-too-imperfect data are signs that stratification of wealth and privilege in Roman Egypt may have been linked to a deterioration of the daily lives of those left behind. To an American, familiar with the differences in such indexes of social welfare as infant mortality between the United States and those countries with a higher rate of income redistribution, this pattern cannot be very surprising.

Appendix I: The Economics of Bringing up Exposed Infants

Diodorus Siculus famously tells us that bringing up children is so cheap in Egypt that the Egyptians do not expose any of their children. The cost for Tatoupe to nurse their foundling was presumably minor — a bit more food for her to eat — and the actual cash invested in the young slave was therefore small. But it is evident from the wet-nursing contracts discussed above that not all slave infants were nursed ‘free’ by an owner. Manca Masciadri and Montevecchi (*I contratti di balatico* 12) express surprise also at the fact that the *engona*, children of slave mothers, are not being nursed by their mothers; surely, they suppose, we would not expect so many mothers to have died or be dry. They therefore suspect that the designation might be used to cover some other reality (sale of children by their parents, they think; cf. Appendix II). But it must be remembered that allowing the slave mother to nurse the child might have delayed her becoming pregnant again, and to the extent that one owned female slaves partly for breeding of slave children, such a delay would be undesirable. It may have been more profitable to pay someone else to nurse the child than to sacrifice some of the reproductive potential of the slave.

⁴² See J. Méléze Modrzejewski, *The Jews of Egypt from Ramses II to Emperor Hadrian*, Philadelphia 1995, 161-83 on the degradation of the status of the Jews under Roman rule.

In either case, the question arises: How much did it cost to raise an infant taken up from exposure and turn it into a salable slave? And was it still profitable to take up exposed infants if one could not have them nursed without cash outlay? Opinions have been expressed on the subject (cf. above, n. 8, for I. Biezunska-Małowist's repeated assertion of profitability), but without even a rudimentary analysis. For present purposes I use figures from the first century A.D. from the *chora*. In Alexandria, both wages and the price of slaves were higher; but the documentation is also earlier in the Roman period (from the reign of Augustus) than what we have from the Arsinoite and Oxyrhynchite nomes (Tiberius and later), so the difference (Alexandria is something like 40 percent higher) may not be explained entirely by location.

The average cost of a hired nurse was 6 drachmas per month; almost all values fall in the range 5-7.⁴³ After age 2, when nursing was no longer required, the cost of support presumably fell; I have assumed that it was half the expense of nursing, which is probably not off the mark by a large amount. If we use the model life table for women (Level 2, West) argued in *Demography of Roman Egypt* to be the closest representation of Egyptian experience, we find approximately the following cost for a cohort of 100 infants collected from exposure at birth:⁴⁴

<i>Age</i>	<i>Survivors</i>	<i>Year cost</i>	<i>Cumulative cost</i>	<i>Unit cost</i>
1	67	6,012 dr.	6,012 dr.	90 dr.
2	62	4,644 dr.	10,656 dr.	172 dr.
3	58	2,160 dr.	12,816 dr.	221 dr.
4	54	2,016 dr.	14,832 dr.	275 dr.
5	50	1,872 dr.	16,704 dr.	334 dr.

The cumulative unit cost per survivor to a given age is shown in the right-hand column. A large operator would need to exceed this figure to make money. Obviously not many, if any, operators actually put cohorts of a hundred babies out to nurse even over quite a few years; individuals with one or a few nurselings would have had fewer over whom to spread the risk of death and would thus probably have needed a higher premium for risk. After the age of 5, when mortality became a smaller factor, the unit cost rose by the actual cost of support for each child.

The number of surviving slave prices for children under the age of 10 is not large, and one should be cautious in using them. But at these ages skills do not

⁴³ All data here derive from the information given in the corpus of Manca Masciadri and Montevecchi.

⁴⁴ It is assumed that infants dying during a given year live for exactly half of that year; that may not have been true in the first year, in which case the cost figure is somewhat too high.

play the part they may have later, and children are thus probably more fungible than adults. At all events, the prices collected in Drexhage, *Preise* 265, show the following (omitting one outlier):

<i>Age</i>	<i>Price range</i>
2-3	300 dr.
7	500-700 dr.
8	600-1200 dr.
9	1600 dr.

It looks as if anyone who could afford to take risks had a fair chance of making a good profit margin on a child whose acquisition cost was zero. We have no evidence what the market value of a neonate was; not high, I would judge, or exposure would have been uncommon; but it might have been as much as 100 dr.

Appendix II: Exposure or Sale in the Tebtunis Register?

Manca Masciadri and Montevecchi have argued⁴⁵ that the numerous contracts for nursing infant slaves listed in the registers from Tebtunis, which give summaries of them, are best interpreted not as showing exposed infants being put out to nurse but as witnessing to a practice of sale of neonates by their financially distressed parents. Although I see no way of excluding this possibility, I also see no grounds for this hypothesis. The view that ἀναίρετοι generally were not really exposed infants but children sold by their parents had in fact already been proposed by B. Adams; Manca Masciadri and Montevecchi say 'l'ipotesi è gratuita' (149). So it is; but they go on to offer a virtually identical hypothesis for the Tebtunis registers.

Their reasoning is essentially as follows (154-7 of the article cited): It is incredible that a master would pay for a nurse for the child of his own slave, so we may exclude the notion that these slaves (whose origin is not given in the summaries) are *engona*. But the Egyptians rejected the entire notion of exposure, being firm believers in the value of life. Tebtunis was a largely Egyptian milieu, and its Greek population cannot have produced such a large crop of exposed babies for Egyptians to pick up. Therefore the slaves can be neither *engona*, nor the exposed children of Egyptians, nor the exposed children of Greeks. The only plausible hypothesis is therefore that they were sold by their own parents as a result of financial distress. Because a large portion of the contracts date from years 6 and 7 of Claudius, which we know to have been hard times in Egypt, the oc-

⁴⁵ In summary in the introduction to *I contratti*, in more detail in *Aegyptus* 62, 1982, 148-61.

casation for such distress is known. The mechanism used is likely to have been the ὤνη ἐν πίστει, the sale subject to redemption, where the child becomes the property of the creditor at the end of the loan period if it cannot be paid back.

This argument is not solid.⁴⁶ First, we have already seen that a master might indeed have good reason to prefer to have his slave woman get pregnant with the next slave child rather than nursing, with the partial protection against pregnancy that nursing provides. Indeed, if the master was the biological father of the slave children, he might not wish to have the slave woman observe the sexual abstinence during nursing normally called for in the nursing contracts. Second, the view that there would not have been an adequate supply of exposed infants in Tebtunis takes it for granted that all of those raised in Tebtunis were exposed there. There are no grounds for such a view; they could quite well come from Arsinoe. Once one admits the possibility that urban infants who had been exposed might have been taken to villages to be nursed, indeed, the entire argument about the Egyptians' cultural preferences becomes moot.

On the juridical side, the argument also seems to me suspect. The child is already treated as a slave of the hiring party in the nursing contract, if we may judge from our brief summaries; this would not already have been the case unless a sale contract had intervened, and no such sale contract is ever recorded in the register. Nor is it obvious where in the two contracts that *are* recorded (nursing contract and loan agreement, the latter of which made the wages paid in advance to the nurse more readily recoverable if necessary) a provision for redemption of the child would have been found. Certainly none of the surviving contracts has any such provision.

It may well be true that there is a concentration of these contracts in the period of economic distress, but that is no argument for infant sale. It can just as well point to an increased likelihood of abandonment of unwanted infants in the nome metropolis or elsewhere, and to a need for funds that would have made village women readier to take on wet-nursing jobs.

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⁴⁶ It is accepted without discussion by D.W. Hobson, *EMC* 28 = n.s. 3, 1984, 373-90 at 378-9.