

DEMOGRAPHY, GEOGRAPHY AND THE SOURCES OF ROMAN SLAVES*

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I. INTRODUCTION

Where did a large-scale Roman slave-owner obtain new slaves? Varro in effect tells us: Ephesus.¹ And the answer would probably have been the same for many generations after his time. But can we work out more systematically and more thoroughly the relative importance of different *kinds* of sources? The sources which most require consideration are: (1) children born to slave-mothers within the Empire; (2) persons enslaved in provincial or frontier wars; (3) persons imported across the frontiers; (4) the 'self-enslaved'; and (5) infants abandoned at places within the Empire.

Several years ago, I argued on a number of grounds that the last of these sources, child-exposure, was more important than had previously been recognized.² Subsequent reconsideration of the problem has led me to suspect that the source-material under-represents the amount of slave-importation across the frontiers, but not to doubt that child-exposure was very widespread or that it made an important contribution to the slave supply. Of the many subsequent discussions, the most original is that of Ramin and Veyne, who, in an article of 1981 too little attended to in the Anglo-Saxon world, made it appear very likely that those who voluntarily sold themselves into slavery were a larger category than scholars usually imagine.³ More recently, Scheidel has attempted to revive the case, previously propounded by Shtaerman among others, in favour of the self-reproductivity of the slave population.⁴ Indeed he takes that case to extremes, claiming that after the abrupt deceleration of Rome's frontier wars which took place in the first two decades A.D. the fertility of the existing slave population itself was by far the most important source of slaves. It was 'at least five or six times as important as any other single source', and he is courageous enough to suggest a number for the percentage of slaves he thinks came from this source, a number which seems to be about 80 per cent. I shall call this opinion the SRH (self-replacement hypothesis).

Now, I have never denied that a large number of Roman slaves were the children of slave-mothers, nor did I in my paper on the slave trade write, as Scheidel alleges, that foundlings supplied more than half of the new slaves in this period.⁵ I consider that to be possible, but there is not enough evidence, in my opinion, to estimate the contribution from this source at all precisely. For the time being at least, as most scholars would probably agree, we can only fix the importance of sources (1), (3), (4) and (5) within rather broad bands of possibility. But Scheidel's position is extreme and implausible. He fails to make the high estimate of slave fertility which corresponds to his 80 per cent

* I thank the Editorial Committee for its efficiency as well as for its scholarly reactions to a first draft. My thanks also to Walter Scheidel for courteously sending me his 1997 paper in advance of publication, to the economist Michael Haines for his help with the demography of U.S. slavery in the nineteenth century, and to many friends, especially Richard Duncan-Jones, Keith Hopkins, Elio Lo Cascio and Brent Shaw for discussion.

¹ *De lingua Latina* 8.21.

² 'Towards a study of the Roman slave trade', *MAAR* 36 (= J. H. D'Arms and E. C. Kopff (eds), *The Seaborne Commerce of Ancient Rome*) (1980), 117-40. J. Ramin and P. Veyne, 'Droit romain et société: les hommes libres qui passent pour esclaves et l'esclavage volontaire', *Historia* 30 (1981), 475, were less cautious: abandoned children 'sont sûrement la source principale des esclaves sous l'Empire'.

³ Ramin and Veyne, op. cit. (n. 2), 472-97, repr. in P. Veyne, *La Société romaine* (1991), 247-80.

⁴ W. Scheidel, 'Quantifying the sources of slaves in the Roman Empire', *JRS* 87 (1997), 159-69. E. M. Shtaerman, *Die Blütezeit der Sklavenwirtschaft in der römischen Republik* (1969; the original edition was published in 1964), 70; E. Shtaerman and M. K. Trofimova, *La schiavitù nell'Italia imperiale* (1975; original edn 1971), esp. 17 and 24. Her conclusion is more moderate and credible than Scheidel's.

⁵ Scheidel, op. cit. (n. 4), 165. I wrote (op. cit. (n. 2), 123) that the enslavement of abandoned children was 'a far more important source' of slaves than any other Italian or provincial source apart from those who were slaves by birth. The paper of Ramin and Veyne makes me doubt whether I should have written 'far'. I have never by the way, *pace* Scheidel (156 n. 2), used the odd expression 'social life expectancy'.

estimate into a credible hypothesis, neglecting to take into account the reasons why the fertility of the slave population is likely to have been too low, including the powerful evidence (as it seems to me) which suggests that the sex ratio of the slave population was over a long period seriously imbalanced, with females in the minority. He underestimates the contribution from other sources, and comes to a fallacious conclusion concerning enslaved foundlings.

In 1980 it was mildly novel to correlate the problem of the slave supply with what little was known about the demography of the Roman Empire.⁶ Whether what we have learned on the latter subject since that date — and, in the eyes of some, what we have learned has been mainly the depth of our ignorance — has much bearing on the problem of the slave supply is one of the questions which this article confronts. The survival rate of foundlings is another question requiring attention.⁷ Nor will it be possible to skirt entirely the possible implications of Riddle's books on contraception and abortion.⁸ If Riddle were broadly correct and effective herbal methods of contraception and abortion were in widespread use in antiquity, our problem would become even more difficult, since it would be harder to believe in population 'surpluses' in the free population of the Roman Empire, and harder also perhaps to attribute a high level of fertility to Roman slaves. At all events, the purpose of this paper is not simply to reassert an earlier account, but to take stock of an undeniably delicate historical problem.

Ancient historians must imperatively learn from anthropology, economics, and demography, as has been obvious for a generation, indeed for two or three.⁹ It is not a matter of riding the wave of the future — historical studies have now notoriously turned away, to a great extent, from social history in its traditional forms — but of using the techniques which may help us to learn something interesting. One of the osmotic effects of the social sciences on ancient history has been a pronounced affection for the language of 'models'. Almost all of us have used this language, and a distinguished practitioner of the ancient-historical art has with persuasive words made its use a litmus-test of intellectually vigorous ancient history.¹⁰ But, to change the metaphor, too much of this heady liqueur is not good for you. Scheidel follows the fashion, and his paper neatly displays the unnoticed risk which 'model' terminology brings with it.

The *OED* somewhat optimistically asserts that a model, in this sense, is 'a simplified or idealized description or conception of a particular system, situation or process . . . that is put forward as a basis for calculations, predictions, or further investigation'.¹¹ In truth, there are two different senses of the word alluded to in this definition, one exemplified in the concept 'model life-table', tightly connected to empirical observations (though distinct from them) and multiple (Coale and Demeny offered eight different sets of models),¹² and the kind of model which is likely to stand on its own or be juxtaposed to a single alternative, and hence invites acceptance or rejection rather than modification.

⁶ But see K. Hopkins, *Conquerors and Slaves* (1978), 141. It is also worth consulting the remarks of Henri Wallon on this matter, written in the 1840s without the benefit of *CIL* or life-tables: *Histoire de l'esclavage dans l'antiquité* (2nd edn, 1879), I, 158 and II, 101–4.

⁷ cf. 'Child-exposure in the Roman Empire', *JRS* 84 (1994), 8–11. Only in classical fields, perhaps, would a degree of tension between what one wrote in 1980 and in 1994 be thought troubling.

⁸ J. M. Riddle, *Contraception and Abortion from the Ancient World to the Renaissance* (1992); *Eve's Herbs: a History of Contraception and Abortion in the West* (1997). But according to B. W. Frier, 'Natural fertility and family limitation in Roman marriage', *CPh* 89 (1994), 318–33, there was little family limitation within marriage outside the upper class.

⁹ Which is not to suggest that all ancient historians must follow this path, or that such study is a vaccine against bad social history. And the question always

remains — which economics, which anthropology? As for the present problem, Scheidel is in error in asserting that its difficulties derive solely from 'lack of demographic conceptualization' (156) — they also derive from among other things poor evidence, poor interpretation of the evidence, and poor weighing of historical probability.

¹⁰ K. Hopkins, 'Rome, taxes, rent and trade', *Kodai* 6/7 (1995/96), 41. Contrast R. S. Bagnall and B. W. Frier, *The Demography of Roman Egypt* (1994), xvi–xvii.

¹¹ 1989 edition, s.v. model I.2.e. Cf. the definition from R. J. Chorley and P. Haggett, *Socio-Economic Models in Geography* (1968 edn), 22, quoted by M. I. Finley, *Ancient History: Evidence and Models* (1985), 60. This definition stresses simplicity more, and hence covers the second sense mentioned above better than the first.

¹² A. J. Coale and P. Demeny, *Regional Model Life Tables and Stable Populations* (2nd edn, 1983).

The latter kind of model can be 'good to think with'. The 'consumer city' may not have turned out to be an appropriate model for the cities of the Roman Empire but at least it stirred some discussion of a topic which many of us thought worth talking about. But such models can rather easily become strait-jackets. What is unproductive is the model as a substitute for refining a historical description or hypothesis. Some historical problems do admittedly invite the choice of Model A or B (or C or D — but all too often we content ourselves with dichotomies), there being fundamental disagreement about how to approach the topic and about what is admissible evidence. Other historical problems are matters of degree, matters of adjustment, and the prize — or rather the baton — goes to the historian who can offer the account which is most precise or convincing or stimulating (tastes vary). In such cases the simplification inherent in model construction can be quite harmful.¹³

But to return to the particular case: the first matter to be discussed is the fertility of the Roman slave population. Then we must reconsider the contributions to the slave supply made by the other sources, and then comment briefly on the geography of the slave trade, which is in itself enough to render the SRH very improbable. We shall also glance at the controversial matter of economic integration.

II. THE FERTILITY OF THE SLAVE POPULATION

One can understand why no one has recently reviewed the evidence about absolute slave numbers in the Roman Empire: not only is the evidence hopelessly inadequate for any but a very approximate conclusion, but the problem of the free population, which one might expect to be easier to resolve, is once again warmly disputed territory.¹⁴ The present significance of the question of absolute slave numbers, apart from one's general wish to know how many people we are talking about, is that the more slaves there were, the more difficult it is to identify sufficient sources other than children born to slave-mothers within the Empire; or rather, the higher the ratio of slaves to free inhabitants, the harder it is to identify such sources. But that does not mean that we should necessarily prefer a low estimate of the slave population: the question is which hypothetical numbers are more unacceptable, a low number for the slave population,¹⁵ or high numbers for all or some of the principal slave-producing sources.

It is disappointing, however, that recent works on both Roman slavery and Roman demography have avoided discussing or drawing conclusions about the size of the slave population.¹⁶ Scheidel has the real merit of facing the question. He says 'six million in a population of sixty million' (p. 158), on the eve of the Antonine plague, but without much in the way of justification. We are offered a second-hand guess about Italy — two to three million slaves — and for the rest of the provinces an extrapolation from Egypt, where, it has been agreed for some time, the likely proportion of slaves in the *chora* was about 10 per cent (at Alexandria things may have been different).¹⁷ The only city in the

¹³ Finley op. cit. (n. 11), 61, contrasted model-construction with the meaningless accumulation of facts in books about ancient cities, and that is probably a case in which model-construction can help, partly because the evidence is so unmanageable, as it is not, for example, in the case of the sources of Roman slaves. But Finley's dichotomy was itself seriously misleading.

¹⁴ See for instance E. Lo Cascio, 'The size of the Roman population: Beloch and the meaning of the Augustan census figures', *JRS* 84 (1994), 23–40; W. Scheidel, *Measuring, Sex, Death and Age in the Roman Empire: Explorations in Ancient Demography* (1996), 167–8. F. Coarelli has now argued for a population of the city of Rome as high as 1.2 million in early imperial times ('La consistenza della città nel periodo imperiale: *pomerium, vici, insulae*', in *La Rome Impériale. Démographie et logistique* (1997), 107).

¹⁵ I see the high ratio of slaves as having gradually taken hold in Italy over the course of the middle Republic; the addition of new provinces will have had varying effects on the overall ratio.

¹⁶ Thus nothing on this in E. M. Shterman *et al.*, *Die Sklaverei in den westlichen Provinzen des römischen Reiches im 1.-3. Jahrhundert* (1987; original edn 1977), L. P. Marinovich *et al.*, *Die Sklaverei in den östlichen Provinzen u.s.w.* (1992; original edn 1977), T. G. Parkin, *Demography and Roman Society* (1992), E. Herrmann-Otto, *Ex Ancilla Natus* (1993), or K. R. Bradley, *Slavery and Society at Rome* (1994).

¹⁷ See esp. I. Biežuńska-Małowist, *L'Esclavage dans l'Égypte gréco-romaine II* (1977), 156–8; cf. 'L'Égypte et l'histoire de l'esclavage dans l'antiquité', in L. Criscuolo and G. Geraci (eds), *Egitto e storia antica dall'ellenismo all'età araba* (1988), 264.

Eastern provinces where we have any information is Pergamum, where Galen, in a not fully lucid passage, seems to tell us that slaves amounted to about a quarter of the total population.¹⁸

While we no longer suppose that Egypt was an exception to every generalization that can be made about the economy and society of the Roman Empire, it seems reasonably obvious that it will have had a lower ratio of slaves than almost any other province. On large rural estates in Egypt, as Rathbone has shown in a single but scarcely unrepresentative case,¹⁹ slave labour was sparse, which cannot have been the case in any Western province, and is most unlikely to have been the case in the highly Hellenized provinces. Wealthy Romans, including those of provincial origin, bought land and other assets in many provinces, and often had a strong incentive to change the labour system. An exhaustive discussion of the results would have to consider the extent of quasi-capitalistic agriculture in each province, and should also consider the extent of the urban *familiae* of local bigwigs and senators.²⁰ We might attempt a triage of provinces, differentiating those which had a ratio of slaves near to that of Italy, those which like Asia cannot possibly have had a ratio as low as 10 per cent, and those which resembled Egypt. In any case 10 per cent is far too low an overall ratio for the extra-Italian empire as a whole. My earlier guess that the actual figure fell within the range 16.6 to 20 per cent still seems to me about as close as we can get.

The Scheidel model, or SRH, contends that the slave population produced a large proportion of its own replacements, 80 per cent of them according to Scheidel's conjecture. This might be considered implausible even if the slave population had a natural sex-ratio, since it seems generally to be true that high fertility and tolerably low child mortality depend on the existence of family structures.²¹ In the Roman world, large numbers of female slaves of child-bearing age must have had irregular sexual lives, and for every *ancilla* who was made pregnant in a fleeting encounter there must have been another whose wish to find a tolerable love-life was frustrated by her owner or the owner's agent. More slave-women than free-women nursed their own babies, with the consequence that they lactated longer and had longer intervals between pregnancies. Nor should we brush aside the fact that in significant portions of the Roman Empire, the offspring of slave-women were not deemed to be slaves if the father was free.²² The children of slaves could also suffer exposure (hence there was some overlap between source (1) and source (5)).²³ And in my view the sex-ratio of the slave population was in any case far from the natural one: it was very high, that is to say that males far outnumbered females,²⁴ with very negative consequences for slave fertility.

An isolated ancient text appears to suggest that Roman slaves were at one time not less but more fertile than free people: Appian attributes to Ti. Gracchus the assertion that the supposed infertility of the free Italians was matched by the *πολυπαιδία* of the

¹⁸ In *De propriorum animi cuiuslibet affectuum dig-natione* 9.13 (p. 33 De Boer (CMG 5,4,1,1)=5 p. 49 Kühn), Galen lets it drop that the number of slaves was the same as the number of male citizens and as the number of women (citizens), namely 40,000. (See S. Mitchell, *Anatolia* (1993), II, 244, for the view that these numbers referred to the city itself without its *chora*.) The absolute value of this figure is slight (cf. W. Scheidel, 'Finances, figures and fiction', *CQ* 46 (1996), 222–38, on the Graeco-Roman passion for the numbers 400, 40,000, 400,000), but the proportions may be roughly right. If the free population was 3.5 times that of the male citizens (R. P. Duncan-Jones, *The Economy of the Roman Empire: Quantitative Studies* (1974), 264 n. 4), that would mean that slaves made up 22.2 per cent of the total, but it seems unlikely that Galen intended to include very young slave children (since he is discussing the financial assets of the three groups); hence the proportion was probably higher than 22.2 per cent. Parkin, op. cit. (n. 16), 175 n. 187, prefers a multiplier of 4, not 3.5, but Caesar, *BG* 1.29 does not support this: the men who could bear arms (among the Helvetii)

must have been a smaller set than the adult males. Those who believe in low estimates for the slave population regularly belittle Galen's information.

¹⁹ D. Rathbone, *Economic Rationalism and Rural Society in Third-century A.D. Egypt* (1991), 89–91, 106–7.

²⁰ See further 'Between Archaic and Modern: some current problems in the history of the Roman economy', in W. V. Harris (ed.), *The Inscripted Economy. Production and Distribution in the Roman Empire in the Light of Instrumentum Domesticum* (JRA Supplementary Series 6) (1993), 25–7.

²¹ See for instance B. W. Higman, 'Household structure and fertility on Jamaican slave plantations: a nineteenth-century example', *Population Studies* 27 (1973), at 527 (repr. in H. Beckles and V. Shepherd (eds), *Caribbean Slave Society and Economy* (1991), 250).

²² Ramin and Veyne, op. cit. (n. 2), 481. See for instance Dio Chrys. 15.3–5.

²³ cf. 'Child-exposure', op. cit. (n. 7), 14.

²⁴ 'Towards a study', op. cit. (n. 2), 119–20; and see below, p. 69.

slaves.²⁵ Opinions will continue to differ as to whether the notion goes back to the 130s B.C. or whether it was a later embellishment. Given the relatively high quality of Appian's information about the era between 133 and 70, a genuine comment by Ti. Gracchus or at least an invention of that period may have been involved. If so, the allusion may have been pure rhetoric, aimed at discrediting wealthy latifundists; or it may have had some basis in Gracchus' observations, in which case he may have been thinking of the consequences of the great acts of civilian enslavement which took place during the imperial expansion of the mid-second century B.C., in Spain, Greece, and North Africa, which had few parallels (there were some of course) in post-Augustan times.²⁶

Nor does Varro really encourage us to believe in a high level of slave fertility.²⁷ He shows no interest in slaves having children except when they are *pastores*, who have a quite exceptionally responsible job. Columella on the other hand, as is well known, favoured the fertility of slave-women on country estates, and that may well be a symptom of the changed conditions of imperial times. He is not likely to have been eccentric in this respect, but his system of rewards is represented as his own idiosyncrasy.²⁸ To have a large demographic effect, slave-owners would have had to mobilize a widespread system of serious rewards, an unproven though not an impossible hypothesis. In any case neither these texts nor any others lend support to the suggestion that large slave-owners also effectively encouraged fertility in their urban households.²⁹

Slave-owners' attitudes towards their victims' fertility have to be given their proper context, which is the general problem of slave management. This required a mixture of rewards and severity — but mainly, in Roman eyes, severity. Xenophon was probably expressing a common slave-owner's attitude when he made his hero Ischomachus say that 'good slaves generally become more loyal when they have had children, but when the bad ones form sexual relationships they become more liable to misbehave' (εὐπορώτεροι πρὸς τὸ κακουργεῖν γίνονται: *Oec.* 9.5); Xenophon represented Ischomachus as preventing slaves from having children without his permission. This would have made good sense to Romans too.³⁰ As for acquiring slave women with the intention of profiting from their fertility, it seems to have been rare.³¹

The validity of Scheidel's theory depends on a high level of slave fertility, and it is therefore surprising to see the matter exiled to a footnote and an appendix. We are offered the familiar observation that the influx of slaves into a slave system may affect its fertility. 'One might therefore wonder', the argument goes on,³²

²⁵ *BC* 1.7.29, an assertion rejected by E. Gabba ad loc. Herrmann-Otto, who discusses this passage without coming to any firm conclusion, errs in implying (op. cit. (n. 16), 234 n. 6) that the scholars she lists, including me, have gone so far as to suppose that there was no natural reproduction of slaves 'worth mentioning' under the Republic, which would be a bizarre position.

²⁶ It could be argued that Appian's comment is better evidence if it is *not* authentically Republican; but it is in any case a slender reed.

²⁷ Varro, *RR* 2.10.6, and Colum. 1.8.19 are invoked by Scheidel, 169.

²⁸ 'Nos quidem . . . feminis quoque fecundioribus, quarum in subole certus numerus honorari debet, otium nonnumquam et libertatem dedimus, cum complures natos educassent. Nam cui tres erant filii [sons or children?] vacatio, cui plures libertas quoque contingebat'. Cf. Parkin, op. cit. (n. 16), 122. Such a rule must have put female infants at risk. On the desirability and affordability of *vernae* see 'Towards a study', op. cit. (n. 2), 118–19, 120.

²⁹ The implications of *Fragmenta de iure fisci* 13 (*FIRA*, ed. Riccobono II, 629) are obscure.

³⁰ Cicero translated this book in his youth (*De off.* 2.87), and the translation was widely read; see S. B. Pomeroy's commentary, p. 70. She is mistaken, how-

ever, in saying (p. 299) that for Xenophon slaves 'born at home' are the only acceptable ones: as far as I can see, he nowhere implies any such opinion, nor does *Oec.* 7.34 provide any evidence that, even in Xenophon's imagination, 'Ischomachus' slaves evidently do more than reproduce their numbers'.

³¹ With respect to pre-Severan times, this rests on an argument from silence and probability. Then there is the comment of Ulpian in *Dig.* 5.3.27.pr.: 'quia non temere ancillae eius rei causa comparantur ut pariant ...', 'since slave-women are not commonly acquired so that they may produce children'; for the sense of *non temere* cf. Suet., *De gramm.* 4.5, Gell. 20.5.4, and *OLD* sense 3. It used to be debated, not unreasonably, whether *non temere* was interpolated: F. De Martino, *Storia economica di Roma antica* (1980), 265–6. (T. Kinsey in A. Watson (ed.), *The Digest of Justinian* (1985), erroneously translates 'slave girls are not acquired solely as breeding stock'.) There is no contradiction between Ulpian's words and *Dig.* 21.1.14.1 (Ulpian again) or 19.1.21.pr. (Paulus), texts which confirm the obvious fact that when women slaves were purchased the purchasers were sometimes (as they must normally have been) interested in their ability to bear children.

³² Scheidel, op. cit. (n. 4), 157 n. 14.

to what extent the slave populations of the Caribbean and Latin America which were shaped by continuous selective import and failed fully to reproduce themselves were intrinsically more 'typical' than the self-contained and highly reproductive slave population of the United States.

But the issue is not the demographic 'typicality' of these populations, rather their demographic similarity to the Roman slave population. It so happens that U.S. slavery between 1808 and 1865 was in fact *atypical*, since it was an almost closed system, whereas in most slave systems there are accretions to the slave population from outside. It may also have been atypical in another respect: during the explosive growth of U.S. cotton production after Whitney's invention of the cotton gin in 1793, the prevailing preference of the slave-owning cotton producers in the Carolinas and Georgia was for female slaves, 'since it was supposed that the sensitive harvesting of cotton demanded female labor'.³³ On the other hand, the occupations for which Roman slave-owners preferred female slaves — and they thought in terms of occupations³⁴ — were strikingly few (see below).

The Roman system resembled 'open' western-hemisphere systems into which new slaves were imported, in that the sex-ratio of the total slave population was liable to severe distortion. As is well known, the eighteenth- and nineteenth-century Atlantic slave trade commonly carried far more adult males than adult females. The orthodoxy used to be that differential demand was the cause of this; the more recent orthodoxy is that the determining factors were at least in part to be found in the source regions,³⁵ but since no one, so far, has been able to give an altogether persuasive account of what those factors were, it is not the moment to be dogmatic about this matter.³⁶ In any case the Roman evidence, spotty though it is, supports the notion that demand was stronger for male slaves than for female ones.³⁷

No one has ever doubted that a closed slave population *can* have a positive net reproduction rate (NRR),³⁸ or that it *can* experience long-term natural increase; the nineteenth-century American case settles that. The question is whether the Roman slave system was more similar to the nineteenth-century American one, and to the few other known instances of self-reproducing slave populations, or to the systems which predominated in the Caribbean and elsewhere in the western hemisphere which were demographically very different. I contend that to assimilate the Roman system to any relatively mild system is a serious error of historical perspective. Scheidel has forgotten the *servi vincti*.

Working out why some nineteenth-century slave populations were better than others at reproducing themselves is a complex matter. Two-parent families almost certainly helped,³⁹ and that factor alone is powerful evidence that the Roman slave population did not reproduce itself. But there is much more to say about differences in the living conditions of slaves between the Roman Empire, the Caribbean, and the

³³ H. Thomas, *The Story of the Atlantic Slave Trade 1440-1870* (1997), 572. But other historians such as H. S. Klein dismiss this claim (personal communication).

³⁴ See Tac., *Germ.* 25.

³⁵ See, for instance, D. Eltis and S. L. Engerman, 'Fluctuations in sex and age ratios in the Transatlantic slave trade, 1663-1864', *Economic History Review* 46 (1993), 308-23, who say that the sex imbalance is normal for a migrating population. For an interesting attempt to combine factors in the source regions with differential demand see R. Olwell, *Masters, Slaves and Subjects: the Culture of Power in the South Carolina Low Country 1740-1790* (1998), 28 n. 44. See now H. S. Klein, *The Atlantic Slave Trade* (1999).

³⁶ cf. D. Eltis and D. Richardson, 'West Africa and the Transatlantic slave trade: new evidence of long-run trends', in Eltis and Richardson (eds), *Routes to Slavery: Direction, Ethnicity and Mortality in the Transatlantic Slave Trade* (1997), 32-3.

³⁷ cf. Harris, 'Towards a study', op. cit. (n. 2), 119-20.

³⁸ That is, 'the number of daughters that a cohort of newborn girl babies will bear during their lifetime assuming a fixed schedule of age-specific fertility rates and a fixed set of mortality rates', H. S. Shryock, J. S. Siegel *et al.*, *The Methods and Materials of Demography* (1976 edn), 315.

³⁹ Scheidel, op. cit. (n. 4), 169 (end) is wrong to imply that we know such families to have been common among Roman slaves, and also mistaken in implying that Bagnall and Frier, op. cit. (n. 10), lend support to this view (see 156-9 for their most pertinent comments). See below for the argument that the sex-ratio detectable in the census-returns from the Egyptian *chora* (more female slaves than male) reverses the pattern prevailing in the Roman Empire as a whole.

United States — and a whole host of other slave systems. In an era which has been re-awakened to the life-conditions of slaves in the latter milieu by such books as Toni Morrison's *Beloved*, any attempt to argue that one slave regime was worse than another may seem distasteful. On the other hand there is a long and tiresome tradition among classicists of softening the realities of the Roman slave system. A good antidote is to read any account of the way the ancients tortured slaves for legal testimony. More to the point is that in the extremely unpleasant world of Caribbean slavery in the eighteenth century, which was characterized by under-nourishment and corporal punishment, all sorts of measures were nevertheless taken for the physical well-being of the slaves which would have been unthinkable in the Roman Empire. There can be no doubt that conditions in many places in the West Indies were severe enough to have a major negative effect on fertility. But there were sometimes slave codes aimed at limiting the exploitation of slave labour, in Trinidad for example in 1789 and 1800.⁴⁰ There were quite often more or less scientifically-minded doctors (sometimes getting rich quickly, if they survived).⁴¹ As early as the 1640s, Curaçao had two hospitals for slaves, 'aimed at enhancing the exchange value of the slaves', and later in the century there was a doctor there charged with treating slaves.⁴² Much of this medicine did more harm than good,⁴³ but of course that became less true as the nineteenth century progressed; and we can presume that the motives which led slave-owners to pay for such medical care also actuated them to take thought for the slaves' nutrition and shelter. The medical care applied to slaves in the antebellum U.S. was extensive,⁴⁴ and once again important for what it suggests about the general treatment of slaves. Now, the contrast with the classical world is not total, and we cannot suppose that the slave infirmaries, *valetudinaria*, mentioned by Columella (11.1.18, 12.3.7 and 8) were unique — he sees no need to explain them to his readers. But it would probably be right to suppose that Roman attitudes towards the health of slaves were generally harder than those of Caribbean and North American slave-owners.⁴⁵

There is a real contrast to be drawn between Roman and antebellum U.S. slavery with respect to the crucial matter of family life. It hardly seems necessary here to parade the evidence that only a very small proportion of Roman slaves lived in families of their own (which was plainly the reason why freedmen, according to the evidence of their funerary monuments, exulted in family life),⁴⁶ but it may be worth pointing out that North American slaves in the nineteenth century, the self-reproducing slave population par excellence, normally lived in family dwellings and in long-term relationships. In this respect, *Time on the Cross* was hardly misleading.⁴⁷ We hardly know enough to be sure that Roman slave-owners, when they came to sell slaves, took less notice of such family ties between slaves as did exist than American owners did, but their ruthlessness in this respect has struck at least one investigator.⁴⁸

⁴⁰ For details see A. M. John, *The Plantation Slaves of Trinidad, 1783–1816: a Mathematical and Demographic Enquiry* (1988).

⁴¹ See for instance M. Craton, 'Death, disease and medicine on the Jamaican slave plantations: the example of Worthy Park, 1767–1838', *Histoire Sociale – Social History* 9 (1976), 237–55, repr. in H. Beckles and V. Shepherd (eds), *Caribbean Slave Society and Economy* (1991); R. B. Sheridan, *Doctors and Slaves: A Medical and Demographic History of Slavery in the British West Indies, 1680–1834* (1985).

⁴² H. Lamur, 'Demographic performance of two slave populations of the Dutch speaking Caribbean', *Boletín de Estudios Latino Americanos y del Caribe* 30 (1981), cited from Beckles and Shepherd, op. cit. (n. 41), 216.

⁴³ For some of the fatal remedies favoured by doctors during the Atlantic journey see K. F. Kiple and B. T. Higgins, 'Mortality caused by dehydration during the Middle Passage', in J. E. Inikori and S. L. Engerman (eds), *The Atlantic Slave Trade* (1992), 321–37, esp. 327.

⁴⁴ See most recently K. O. Bankole, *Slavery and Medicine: Enslavement and Medical Practices in Antebellum Louisiana* (1998).

⁴⁵ There were ephemerally official attempts in the second century A.D. to protect slaves from certain extremes of punishment and overwork; cf. P. Garnsey, *Ideas of Slavery from Aristotle to Augustine* (1996), 93–7.

⁴⁶ B. D. Shaw, 'The cultural meaning of death: age and gender in the Roman family', in D. I. Kertzer and R. P. Saller (eds), *The Family in Italy from Antiquity to the Present* (1991), at 87, making use of the work of P. Zanker and D. E. E. Kleiner. Some slaves did of course live within stable family structures.

⁴⁷ R. W. Fogel and S. L. Engerman, *Time on the Cross* (1974), 126–44. The classic treatment, also much discussed, is H. G. Gutman, *The Black Family in Slavery and Freedom, 1750–1925* (1976), chs 2–4; see too P. Kolchin, *American Slavery, 1619–1877* (1993), 138–43. Scheidel, op. cit. (n. 4), 163 n. 29 gives quite the wrong impression on this matter.

⁴⁸ Herrmann-Otto, op. cit. (n. 16), esp. 264.

Another extremely important factor affecting the ability of the Roman slave population to reproduce itself, perhaps the most important factor of all, was that population's sex-ratio.⁴⁹ The Egyptian census evidence tells us that, in the very small sample of the non-Alexandrian population in question (N = 102), the ratio of male slaves to female was 1:2 (in conventional terms, the sex-ratio was therefore 50.0). The most recent commentators observe, however, that while there may have been many more female than male slaves in Egypt (better: Egypt outside Alexandria),⁵⁰ the census returns may exaggerate the female preponderance, 'since it appears that male slaves were typically manumitted earlier than females'.⁵¹ The same scholars also remark that there is no sign in these documents that the slave-masters pursued a policy of encouraging slave families.⁵² But the sex-ratio which obtained in Egyptian towns and villages has very little bearing on Alexandria or on the provinces where agricultural slavery was common.

All the evidence, and all the acceptable arguments, about the sex-ratio of the slave population in the big cities and in the provinces where slaves represented a sizeable proportion of the rural and the work-shop labour-force point to a heavy imbalance in the other direction, i.e. far more males than females. The same obviously applies in places where mines and quarries and related economic activities were important. In domestic service, male slaves seem to have outnumbered female to a possibly surprising degree.⁵³ One could never, I think, have said about the Roman Empire, as has been said about the British Caribbean in the early nineteenth century, that 'the towns always had low slave sex ratios [i.e. a low proportion of males to females] because of the demand for females in domestic employment'.⁵⁴

A lengthy case could be made for the proposition that the slave population of the Empire as a whole was disproportionately male. One might start with urban *familiae*, since it is not inconceivable *a priori* that, there at least, the pattern was different, especially in the Greek world. The epigraphical evidence admittedly requires much more cautious handling than it received twenty years ago, and may in fact be unusable. Not that scholars were unaware, even in those remote times, that in many ancient milieux males were thought to be worthier of epitaphs than females were.⁵⁵ More detailed work has now been done which shows that habits of commemoration can skew the apparent sex-ratio — in either direction.⁵⁶ Furthermore the slaves of the super-rich might be atypical in this respect. It is none the less striking that the epitaphs of such households several times show male slaves outnumbering female by about three to one,⁵⁷ an imbalance greater than Shaw's researches on Latin epitaphs have uncovered in any tranche of the free population. And in so far as we are dealing in these cases with privileged slaves, with unusually good chances of arranging sexual and domestic relationships for themselves, we might reasonably guess that the sex-ratio we find among them is actually lower, i.e. more nearly natural, than it was in some households. Still more striking as evidence about urban *familiae* is the inventory of the town slaves of the rich Alexandrian Ti. Iulius Theon, who died in 111: of the fifty-nine slaves whose gender can be determined from this damaged document, only two were female.⁵⁸ All this is supported by the evidence that for many occupations which women could have performed at least as well as men, men were preferred.

In pursuit of facts about the sex-ratio among slaves, De Ste. Croix claimed to find Columella more interested in female slave labour than the earlier agronomists had

⁴⁹ This matter is ignored in Scheidel's account.

⁵⁰ This case has been reinforced by R. S. Bagnall, 'Missing females in Roman Egypt', *Scripta Classica Israelica* 16 (1997), 121–38.

⁵¹ Bagnall and Frier, *op. cit.* (n. 10), 94.

⁵² *op. cit.* (n. 10), 157.

⁵³ 'Towards a study', *op. cit.* (n. 2), 119.

⁵⁴ B. W. Higman, *Slave Populations of the British Caribbean 1807–1834* (1984), 118.

⁵⁵ e.g. S. Treggiari, 'Family life among the staff of the Volusii', *TAPhA* 105 (1975), 395.

⁵⁶ See esp. Shaw, *op. cit.* (n. 46), at 81–2.

⁵⁷ 77 per cent of the commemorated household staff

of Livia appears to have been male (S. Treggiari, 'Jobs in the household of Livia', *PBSR* 43 (1975), 58), as were 66 per cent of the commemorated town slaves of the Statilii and Volusii (these numbers include freedmen) (Treggiari, *op. cit.* (n. 55), esp. 395, who hesitantly argued, n. 10, that since burial clubs open to women were in question 'they should have had an equal chance of being commemorated'). At Carthage, 76 per cent of the recorded members of the imperial household were male: P. R. C. Weaver, *Familia Caesaris* (1972), 172.

⁵⁸ *P.Oxy.* XLIV.3197. No other Roman inventory of comparable size seems to have been published.

been.⁵⁹ That would be a quite logical thing to expect, but the pair of texts to which De Ste. Croix refers will not bear anything like this weight.⁶⁰ The likelihood that any landowner who had any choice in the matter (including small landowners who were able to cause slave-born children to be 'exposed') preferred male slaves is overwhelming. Female slaves working on the land in the high Roman Empire may have been a rarity.

The obvious counter-argument is that over time the natural sex-ratio is likely to have reasserted itself, as in the Americas.⁶¹ And anyone who models such a sequence of events mathematically will realize how ineluctable such a trend is — on two conditions.⁶² One is that the female children of slaves survived to adulthood in roughly the same numbers as the male children. In reality it is quite possible that the mortality of slave-born girls was much worse than that of slave-born boys. Since the 'exposure' of infants was commonplace in many areas, it was possible to distort the natural sex-ratio (and child-rearing practices may also have favoured the survival of the males). Since slaves were treated as commodities, relative survival rates will have depended in part on demand. The slave-owner who went to market purchased males if it pleased him to do so. The other precondition of a return to a natural sex-ratio is that there should not have been a disproportionate addition of males to the slave population from outside. But in the Roman Empire there is likely to have been precisely that. Males were probably in the majority both among the external recruits to the slave population (slaves imported across the frontiers) and among the internal recruits (foundlings and the 'self-sold'). Male war-prisoners are likely to have been more numerous than female. In all these circumstances, it is *possible* that the sex-ratio remained high over a long period.

A negative NRR in a slave population can come from an imbalanced sex-ratio, infertility, high mortality, manumission, or from any combination of these; in the Roman Empire all four factors probably had their effect. Only manumission requires a further comment here. There is no need to reiterate refutations of the view that the duration of slavery was often only 'a couple of years',⁶³ which would have made the slave system unsustainable and is in any case supported by no evidence whatsoever. Cicero seems to assume that a good slave obtained freedom after six years, and it is perhaps not quite enough to dismiss that as a rhetorical distortion (as most scholars seem to).⁶⁴ But the *average* period of servitude, including the best-treated urban slave and the most despised *vinctus* on a remote country estate, must have been much higher, even when due account is taken of the slaves' undoubtedly poor life expectation. But what matters most here is whether manumission impeded the slave population's ability to reproduce itself.

Freedom, like death, could come to a slave at any age. One would like to know whether it affected women within the reproductive cycle to a disproportionate extent; it

⁵⁹ G. E. M. de Ste. Croix, *The Class Struggle in the Ancient Greek World* (1981), 588, with proper reservations about the potential significance of such evidence.

⁶⁰ 12.4.3: some authorities said that food should be served by persons without sexual contacts, i.e. male or female children; 8.2.7: a boy or an old woman should be put in charge of stray chickens. This is not a rich harvest from many hundreds of pages. In 12.3.5–9 he describes the duty of the *vilica*, and she seems strangely isolated from other women.

⁶¹ cf. O. Patterson, *Slavery and Social Death* (1982), 134. Scheidel, *op. cit.* (n. 4), 169, attributes to Eltis and Engerman, *op. cit.* (n. 35), 321, the view that the demographic effect of an unnatural sex-ratio on a slave population 'should not be overrated', implying apparently that it was never important, which is not at all what they say.

⁶² If we descended to the era of Justinian, or even of Diocletian, the story might be different (see the end of this article); the point is not to deny that the natural sex-ratio ever reasserted itself, but that it did so quickly. The Aezani text of Diocletian's Price Edict shows that female slaves received the same valuation as males in only one age-group, from eight to sixteen; prospective fertility is likely to be one of the causes

(cf. W. Scheidel, 'Reflections on the differential valuation of slaves in Diocletian's Price Edict and in the United States', *Münstersche Beiträge zur antiken Handelsgeschichte* 15, 1 (1996), 67–79).

⁶³ G. Alföldy used this expression in 'Die Freilassung von Sklaven und die Struktur der Sklaverei in der römischen Kaiserzeit', *RSA* 2 (1972), 97–129, but stepped back from it in the *Nachträge* accompanying the reprint in *Die römische Gesellschaft* (1986), 286–331, at 330; these *Nachträge* do not address the problem of the slave-supply. Against Alföldy's theory of easy manumission: 'Towards a study', *op. cit.* (n. 2), 118; T. E. J. Wiedemann, 'The regularity of manumission at Rome', *CQ* 35 (1985), 162–75.

⁶⁴ *Phil.* 8.32: 'cum in spem libertatis sexennio post sumus ingressi diutiusque servitutum perpassi quam captivi frugi et diligentes solent' (49 B.C.–43 B.C. = 6). This cannot be nonsense. Rather, certain kinds of slaves could hope for freedom after six years. Perhaps wealthy Romans already made a mental division of slaves into quasi-classes, as they certainly did later on (Ulpian in *Dig.* 47.10.15.44). Incidentally Scheidel's assertion (*op. cit.* (n. 4), 158) that I hypothesize a 'staggering amount of social mobility' is spun out of nothing.

might be expected for example that women of marriageable age would be at an advantage in gaining manumission. But there seems to be no quantifiable evidence. The Egyptian census-returns show a *lower* chance of manumission for females (in an extremely small sample), but it would be better not to rely on these texts for a generalization about this question. The epigraphical evidence from Delphi studied by Hopkins is far more extensive and more likely to be typical of Graeco-Roman slavery in general; 63 per cent of those manumitted were female.⁶⁵ Weaver has argued that in the epigraphically commemorated population of Rome itself, female slaves were manumitted at an earlier age than males.⁶⁶

Let us imagine the fertility of a typical cohort of Roman slave-women (those born in a single year), and compare it with that of a contemporary cohort of free Roman women.⁶⁷ With respect to the latter group, we can state the following correspondences between NRR and GRR, based on the Coale-Demeny life-table Model West Level 3 Female and Model South Level 3 Female:⁶⁸

NRR	0.95	1.00
GRR (West)	2.21	2.54
GRR (South)	2.29	2.64

If the population of the Roman Empire remained stable in the High Empire (conquests aside), the long-term overall NRR of that population will have been < 1.00 (since there must have more immigration than emigration); I use the figure 0.95 purely for illustrative purposes. In reality it seems likely that there was spasmodic growth throughout the period from Augustus to the Antonine plague,⁶⁹ so we shall not go far wrong thinking of the NRR as 1.00. That means that according to Model West the average woman who survived throughout her reproductive years will have had about 2.54 girl babies and 5.16 babies in total.⁷⁰ Nothing impossible about that. But let us turn to our cohort of slave-women. A certain percentage (m) of its children will either have been born after the mother's manumission or have been free at birth in virtue of non-Roman laws. Life expectation will have been lower than in the free population: I consider even $e_0 = 20$ to be an improbably high figure,⁷¹ but to avoid fruitless disputes (and also for the practical reason that I have not found a model life-table for an e_0 value lower than 20), we can apply Model West Level 1 Female.

It rapidly emerges that the SRH is untenable. How many live births per woman would such a population have to produce in order to remain stable, or to replace 80 per cent of itself (Scheidel's supposition)? For 80 per cent replacement, if $m = 10$, specific sex ratios translate into a need for live births in the following fashion:⁷²

⁶⁵ op. cit. (n. 6), 139.

⁶⁶ P. R. C. Weaver, 'Children of freedmen (and freedwomen)', in B. Rawson (ed.), *Marriage, Divorce and Children in Ancient Rome* (1991), at 179–82.

⁶⁷ For the concept of cohort fertility see, e.g., C. Newell, *Methods and Models in Demography* (1988), 52–62, S. S. Halli and K. V. Rao, *Advanced Techniques of Population Analysis* (1992), 42–5. The cohort we are considering will not, of course, all have been born into slavery.

⁶⁸ Coale and Demeny, op. cit. (n. 12), 57, 82, 399, 449. The applicability of Model West (or South) to the Ancient World needs to be re-examined in the light of the history of causes of mortality (none of the 130 tables underlying Model West goes back earlier than 1870 (Coale and Demeny 12), a date later than, among other things, Lister's discovery of antiseptics; five of the twenty-two tables underlying Model South are from Italy, 1876–1910, all the others are from 1900 or later (ibid.)), but the problem cannot be

pursued here. It is unlikely, for example, that many if any of the countries whose statistics went to make up Model West had nearly as high a level of infant mortality from child-abandonment. Level 3, incidentally, means that the table concerns a population in which $e_0 = 25$.

⁶⁹ H. W. Pleket, 'Wirtschaft', in W. Fischer *et al.*, *Handbuch der europäischen Wirtschafts- und Sozialgeschichte* (1990), I, 57, B. D. Shaw, review of T. G. Parkin, *Demography and Roman Society*, CPh 89 (1994), 190–1.

⁷⁰ On the assumption that there were about 105 live male births for every 100 female ones.

⁷¹ cf. R. Duncan-Jones, *Structure and Scale in the Roman Economy* (1990), 100–1.

⁷² If the sex-ratio of the slave population was really as high as 300, self-reproduction would have required even more absurd levels of fertility; but I do not dismiss the possibility that after some decline such a population might reach the sex-ratios used in the text.

sex ratio	males as per cent of the total slave population	GRR	GRR + 105%
150	60	3.51	7.20
100	50	2.81	5.70

If $m = 20$, which is not improbable, the following are the consequences:

sex ratio	males as per cent of the total slave population	GRR	GRR + 105%
150	60	3.95	8.10
100	50	3.16	6.48

It is a most implausible double to suppose both that the slave population had a natural sex-ratio and that it had such a high level of fertility.⁷³

Between 1820 and 1860 the slave population of the United States grew by 156 per cent, far faster in other words than the slave population of the post-Augustan Roman Empire can ever have grown. The GRR of this population was obviously high, apparently 3.9–4.0 in the period 1850–1859 (in other words, a woman who lived to menopause bore an average of about eight children) and earlier on somewhat higher still.⁷⁴ For the reasons already explained, the Roman figure has to be much lower and certainly lower than 3.51.

Orlando Patterson observed that ‘even if a slave population is biologically nonreproductive, birth may still remain the single most important source of slaves’,⁷⁵ which I take to be both self-evident, and also irrelevant to the claim that 80 per cent of Roman slaves were the children of slaves.

III. IMPORTED SLAVES, THE SELF-ENSLAVED, ENSLAVED FOUNDLINGS

What about other possibly important sources? Imports, first of all. They have not normally been thought of as a major source of supply in imperial times, though there is quite a lot of scattered evidence. Scheidel contributes the useful consideration that the areas which are most in question were not densely enough populated to be able to fulfil the Roman Empire’s need for slaves. He lists these areas as Ireland(!), Scotland, Germania, south Russia, the Caucasus, the Arabian peninsula, and the Sudan; Mesopotamia and Iran, he says would have had to supply slaves for the Parthian Empire.⁷⁶ We should also factor in slaves who emerged from Indian Ocean trade (perhaps in the main from Somalia) and from Saharan trade.⁷⁷ It is suggested that the area from which the Roman Empire imported its slaves will probably have had a population as low as fifteen million.⁷⁸ That is a highly speculative figure, which may be thought to include an element of special pleading, but Scheidel is undoubtedly right to suggest that sheer lack of population would have prevented the external periphery from satisfying a large proportion of the Roman Empire’s appetite for slaves. The European and African populations in question were relatively un-urbanized, and in the same areas even the extent of peasant agriculture may have been quite limited. The weakness of the argument is that it assumes that populations in the ‘catchment’ areas were stable. In reality they may well have decreased under the impact of proximity to the Romans; and another possibility is that the areas immediately beyond the Roman frontiers were

⁷³ Applying Model South would require an even higher GRR.

⁷⁴ Michael Haines’s extrapolation (personal communication) from the estimate of A. J. Coale and N. W. Rives that the Total Fertility Rate of the whole black population, slave and free, of the US in 1850–1859 was 7.90 (*Population Index* 39 (1973), 26).

⁷⁵ op. cit. (n. 61), 133.

⁷⁶ Scheidel, op. cit. (n. 4), 159.

⁷⁷ Somalia: see esp. *Periplus Maris Erythraei* 13. Sahara: *CIL* VIII.4508, with D. J. Mattingly, *Tripolitania* (1994), 156; M. Brett and E. Fentress, *The Berbers* (1996), 68–9. For Mauretania see ‘Towards a study’, op. cit. (n. 2), 126.

⁷⁸ Scheidel, op. cit. (n. 4), 159–60.

repopulated from without. The Roman economy may have had a vast magnetic effect, saving the closest external regions from depopulation.⁷⁹

The rather abundant evidence for slave importation does very little to help us quantify it.⁸⁰ The problem is intertwined with the not to be neglected question of prisoners-of-war. Some have estimated that the average number of peacefully imported slaves fell within the range 20,000 to 25,000, but absolutely no reason has been adduced for excluding a lower number or a higher one.⁸¹ Apart, that is, from the fact that without imports there would probably have been a supply crisis of which we see no sign.

How much of a part was played by self-enslavement? Historians have sometimes neglected this practice,⁸² presumably because it was one of those *atrocissimae* which dignified Roman society did not readily write about (which is precisely why it *does* appear in Petr., *Sat.* 57.4). But in the startling passage from which Ramin and Veyne begin their discussion, Seneca includes *mangones* among those who *even though they are useful to others* do not really convey *beneficia* (since they reap advantage for themselves in the process). He takes it to be obvious that *mangones* are useful to those whom they sell ('mango venalibus prodest').⁸³ These beneficiaries can only be those who *wish* to be sold. Seneca's assumption that his comment will make sense to the Neronian reader is valuable evidence. Both Clement, Bishop of Rome, and Papinian refer to the frequency of this practice, while Petronius, Dio Chrysostom, and Ulpian make it entirely plain that self-sale was commonplace.⁸⁴ If the literary references are judged to be few, that is understandable in the light of the fact that self-sale offended one of the cardinal principles of Roman law, the inalienability of freedom.⁸⁵ The legal experts had to grapple with this difficulty and duly created a massive loophole.

It was essential to do so for two separate reasons which are both elucidated by Ramin and Veyne, one being life-threatening poverty and the other the desirability — in the eyes of many people — of the position of slave *actor*, the slave who in every substantial Roman household handled financial transactions.⁸⁶ A job as an *actor* was a specialized inducement, which can only have affected a few hundred men a year, but the threat of hunger will in bad harvest years have affected many thousands. One notes that the authors just cited came from North Africa and the Greek East as well as from Italy, and that they allude to both Roman citizens and non-citizens.⁸⁷ We are now well aware that hunger periodically attacked diverse areas of the Roman Empire, and that the response of officials was often inadequate.⁸⁸ The effects to be expected are child-abandonment and self-sale, rather as when in seventeenth-century India, as Braudel records, a Persian ambassador acquired 'innumerable slaves . . . for almost nothing because of the famine'.⁸⁹

The enslavement of foundlings is a phenomenon which historians have sometimes attempted to evade, but there is no need to cite yet again the evidence that it was widely

⁷⁹ The notion that in Roman times there were major population movements in NE Europe, having gone through a period of unpopularity, seems to be taking hold again; see e.g. P. Heather, *The Goths* (1996), 48–50.

⁸⁰ For a survey of the evidence see 'Towards a study', op. cit. (n. 2), 124. If as Tacitus says (*Germ.* 19) the Germans did not expose infants, that may have been because in case of necessity they, in essence, exported some of them as slaves; they supposedly exported persons who were enslaved for gambling debts (*Germ.* 24).

⁸¹ There is no demographic reason why there should not have been 40,000 slaves imported every year over a long period, though if compelled to guess I would opt for a lower figure. Scheidel, op. cit. (n. 4), 164 n. 34, seems to imply that the figure of 70,000 a year, said to be the maximum reached in the Atlantic slave trade, means that 40,000 is too large a figure, but the one figure has no bearing on the other.

⁸² But see, e.g., J. Crook, *Law and Life of Rome*, 90 B.C.–A.D. 212 (1967), 59–60; Alföldy, op. cit. (n. 63), 125 (315 in the 1986 repr.).

⁸³ *De ben.* 4.13.3. They are among those who 'summam utilitatem aliis adferunt'.

⁸⁴ Clem. Rom. 1.55.2; Papinian in *Dig.* 41.3.44 pr. ('frequenter ignorantia liberos emimus'); Petr., *Sat.* 57.4; Dio Chrys. 15.23 (?); Ulpian in *Dig.* 21.1.17.12, 28.3.6.5.

⁸⁵ For a soldier, self-sale was not surprisingly a capital offence, *Dig.* 48.19.14 (Macer).

⁸⁶ cf. J.-J. Aubert, *Business Managers in Ancient Rome: a Social and Economic Study of Institores*, 200 B.C.–A.D. 250 (1994), 194.

⁸⁷ Ramin and Veyne, op. cit. (n. 2), 496, consider it to be the third great source together with foundlings and self-reproduction.

⁸⁸ See P. Garnsey, *Famine and Food Supply in the Graeco-Roman World* (1988).

⁸⁹ F. Braudel, *Civilization and Capitalism, 15th–18th Century*. I. *The Structures of Everyday Life: The Limits of the Possible* (1981; original edn 1979), 77.

practised in the Roman Empire, or the evidence that, like self-sale, it served as a mechanism for transforming the freeborn into slaves.⁹⁰ There may sometimes have been a degree of connivance between the exposers and the 'rescuers',⁹¹ unless allusions to such behaviour are part of the self-exculpatory mythology of the subject.

Two rates are important for the theory that foundlings met a major part of the Roman Empire's demand for slaves, the rate of child-exposure and the rate of survival. As for the former, Scheidel tries to shock us into incredulity by writing that, on the above hypothesis, 'every other mother [or rather, every other mother who survived to menopause] would have exposed one of her children'.⁹² But even higher rates of child-abandonment than this are well-attested for a number of milieux and are likely to have obtained among many other populations. As David Kertzer has observed, drawing on recent work,⁹³

levels of abandonment ranged from 20% to over 35% of all births in such cities as Paris, Vienna, Milan, and Florence.

Why should we deny the possibility that a comparable rate obtained in many urban and rural environments in the Roman Empire?⁹⁴ This is not to ignore the Romans' openness to what Apuleius calls *insita matribus pietas* (*Met.* 10.23). As for the rate of survival, we have very little to go on. I may have over-reacted against the poor arguments which John Boswell used in favour of a high rate of survival, and I do not now regard the disastrous mortality experience of foundling hospitals as strong evidence that few *expositi* survived. If, by way of illustration, the free population of the Roman Empire in A.D. 100 was 50 million, if it had a birth-rate of 47.38 per 1,000 (Frier) and abandoned 20 per cent of the new-born, and one third of these passed into slavery, that would mean 157,933 new slaves a year.

IV. 'INTEGRATION' AND PATTERNS IN THE SLAVE TRADE

Another grave weakness of the self-replacement hypothesis is that it is entirely inconsistent with the extensive evidence that there was a large-scale slave trade within the Roman Empire, and one which led from certain areas, Thrace, Asia Minor, and Syria above all, to Italy and the other areas of overseas demand. Ephesus, at the end of the Republic and in high imperial times, was the hub, as the new customs law tends to confirm.⁹⁵ According to the SRH this long-distance trade had very little reason to exist after the deceleration of the wars of expansion. The SRH implies that the vast majority of the slaves needed in, for instance, Italy were born in Italy. It thus entirely fails to explain the westward flow of slaves from the provincial regions just mentioned. The years have added to what was already an ample body of texts,⁹⁶ showing for example

⁹⁰ cf. 'Child-exposure', op. cit. (n. 7), 1, 20-1.

⁹¹ Ramin and Veyne, op. cit. (n. 2), 477.

⁹² Scheidel, op. cit. (n. 4), 164.

⁹³ In L. A. Tilly *et al.*, 'Child abandonment in European history: a symposium', *J. of Family History* 17 (1992), 15. See also 'The theoretical possibility of extensive infanticide in the Graeco-Roman world', *CQ* 32 (1982), 114-16. P. Brulé has hypothesized that more than 50 per cent of female infants were exposed in some Hellenistic cities, 'Enquête démographique sur la famille grecque antique', *REA* 92 (1990), 233-58.

⁹⁴ Scheidel, op. cit. (n. 4), 165 n. 37, attempts to dispose of the small proportion of the comparative evidence which he takes notice of, but it is not clear what reason or reasons he advances for doing so. Not a demographic one certainly. His point seems to be that high levels of abandonment were brought about by the existence of foundling hospitals, which were of

course unknown in antiquity. But very high levels of abandonment are known from, indeed commonplace in, other worlds without foundling hospitals (M. Dickeman, 'Demographic consequences of infanticide in man', *Annual Review of Ecology and Systematics* 6 (1975), 130); and the basic premise is faulty, for we must assume that abandoning parents had at least a rough idea that foundling hospitals were dangerous — and being cauldrons of disease, it is quite possible that traditional foundling hospitals led to a higher mortality rate than Graeco-Romana exposure did.

⁹⁵ H. Engelmann and D. Knibbe, 'Das Zollgesetz der Provinz Asia', *Epigraphica Anatolica* 14 (1989), ll. 11-12, 98-9, 117-22.

⁹⁶ cf. 'Towards a study', op. cit. (n. 2), 126-8. I should also have mentioned the allusion to the slave trade in the first-century customs law of Caunos (*JHS* 74 (1954), 97-105 = *SEG* XIV (1957), no. 639).

that there was a *σπατάριον*, a slave-market, at Sardis,⁹⁷ as well as at Ephesus, Thyatira, Magnesia-on-Maeander, Acmonia, and probably dozens of other places in this region.⁹⁸ These buildings did not exist for the occasional trading of a few score of slaves (that could have been done in an ordinary market-place), but for a large-scale and rather regular business.⁹⁹

Some will also consider it a serious disadvantage of the SRH that it makes Caesar Augustus into a blunderer. In order to provide for the pay of the 7,000 *vigiles* of Rome and for some other expenses, he introduced (A.D. 7) a two-per-cent tax on slave sales (Dio 55.31), which means that he believed that hundreds of thousands of taxable slave sales (I once hypothesized 250,000) took place every year. At some date between A.D. 7 and 43, the tax-rate was doubled, perhaps by Caligula.¹⁰⁰ But once again, the whole phenomenon is unintelligible if large households were able to fulfil most of their need for slave labour from within.

Finally, integration. Both Finley and Duncan-Jones have suggested in different ways that the economy of the Roman Empire was little integrated, and should rather be seen, in consequence, as a congeries of local or regional markets. The nature of the slave trade might be an additional reason to reject this model — if we knew more about it, and in particular if we knew more about prices. I would suppose that slave prices in Rome, Carthage, Ephesus, Alexandria, and inland Asia Minor reacted to each other as quickly as Roman information technology permitted — there is no reason to believe the contrary. But it cannot be proved.

V. CONCLUSION

It is both true and untrue that the relative importance of different sources of Roman slaves ‘cannot be gauged from ancient texts’.¹⁰¹ Enough is known, however, to establish the improbability of the SRH in the terms in which Scheidel has stated it, and to rehabilitate the importance of importation, and more particularly of self-sale and the enslavement of foundlings. But this is a story about the high Roman Empire, and something like Scheidel’s model (let it be reiterated that we should not see the choice as a stark ‘either/or’) must in the end have imposed itself. When?

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⁹⁷ See the probably Flavian inscription published by P. Herrmann, ‘Neues vom Sklavenmarkt in Sardis’, *Arkeoloji Dergisi* 4 (1996), 175–87 (the text has been quoted elsewhere, e.g. *SEG* XLIII (1994), p. 311).

⁹⁸ The question of the characteristic architecture of Graeco-Roman slave-markets will be re-examined at a conference organized by Elizabeth Fentress which is due to take place at the American Academy in Rome in June 2000.

⁹⁹ About the slave-trade there is more to say elsewhere in view of such studies as F. Coarelli, ‘“Magistri capitolini” e mercanti di schiavi nella Roma repubblicana’, *Index* 15 (1987), 175–90.

¹⁰⁰ ‘Towards a study’, *op. cit.* (n. 2), 121. For detailed but inconclusive discussion of the pay of the *vigiles* see R. Sablayrolles, *Libertinus miles. Les cohortes de vigiles* (1996), 333–42.

¹⁰¹ Scheidel, *op. cit.* (n. 4), 156.