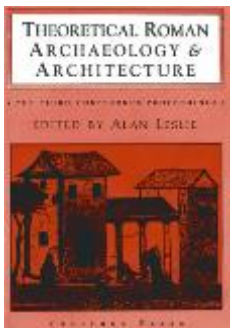

Paper Information:

Title: Slavish Nonsense or the Talking Tool
Author: Ross Samson
Pages: 122–140

DOI: http://doi.org/10.16995/TRAC1993_122_140
Publication Date: 16 April 1999

Volume Information:

Leslie, A. (ed.) 1999. *Theoretical Roman Archaeology and Architecture: The Third Conference Proceedings.* Glasgow: Cruithne Press.

CHAPTER 8

Slavish nonsense or the talking tool

Ross Samson

I *should begin with my excuses* for being a contributor amongst this august body of learned, but no less trendy for that, Romanists, for I am at heart a medieval archaeologist. The main excuse is that it seemed pointless leaving the subject of slavery to Roman archaeologists as there is no sign of any bothering themselves with it. Let me demonstrate just how silent Roman archaeologists are on the subject.

Occasionally, as in the case of J. T. Smith (1978; 1982) or Ramsay MacMullen (1987) (although he is no archaeologist, but his work is cited by them), the scholar takes the hard line that there were scarcely any slaves in the north-west of the Roman empire. But more commonly the issue of slavery is simply skirted.

In Richmond's revision of Collingwood's *The Archaeology of Roman Britain*, for instance, no form of dependent labour features in the index (while "spokeshave" does!). And while I managed to find one abstract reference to slavery (p. 133), all references to actual dependent labourers on villas (chapter seven) are to "farm-workers". John Percival is similarly quiet on the question of slave residence in *The Roman Villa*, mentioning slavery only twice according to the index: once to mention its demise and once while quoting the social historian Rostovtzeff. In *The Archaeology of*

the Roman Economy, Kevin Greene (p. 69) explicitly admits that the question of slaves on villas will not be addressed, being too intractable. In *The Roman Villa in Britain*, edited by Rivet, slaves appear in the index four times only, three times in Rivet's own contribution on social and economic aspects. Look them up and three of the four entries are to Strabo and the export of slaves from Britain in the Iron Age. And the fourth reference is in the index by mistake! Rivet mentions servile labour but in the medieval sense of serfdom not slavery. While slaves make the index of Anthony King's *Roman Gaul and Germany* some thirteen times, most of these instances recount the same anecdote about the Gauls willing to exchange a slave for an amphora of wine or stress that there were no slaves. Slaves, for example, were *not* involved in mining (p. 121) and had *no* part in the production of Samian pottery (p. 130). We also hear that "villas were often large enough to accommodate a substantial number of farmworkers" (p. 101) but that there is "little support to the idea that there were many slaves in Gaul and Germany, particularly in the countryside" (p. 95). So, who were King's "farmworkers" if not slaves? "Clients" (*clientes*) in the early empire and "tenants" (*coloni*) in the late empire. Martin Millett's *The Romanization of Britain* has slaves down in the index five times. Numbers one and two are references to Strabo again and the exportation of slaves in the Iron Age. Turn to Millett's references three and four and we find that slaves appear in figures tabulating social personae recorded in epigraphic evidence. Millett mentions slaves one more time, when they are being replaced by the *coloni* in the late empire.

Almost comically, when mentioning slavery many archaeologists make reference to Strabo and an Iron-Age slave trade (before Roman Britain) and a further reference to the "end of slavery"[1] (after Roman Britain). There is, seemingly, no middle! If this were not odd enough, the most common aspect of slavery mentioned by archaeologists is in the context of trade. Note that mentions of slaves by Roman archaeologists regularly accompany illustrations of amphorae and other Roman goodies, in the context of trade goods, or the mentions accompany plans of hillforts; slavery is equated with tribal warfare, it is almost never suggested that these Iron-Age slaves might have been part of an indigenous institution of slavery. Thus, slaves spring from somewhere and are traded, but then disappear from the narrative. One must assume that British scholars see them all being swallowed up by that cesspit of human excesses known as Italy. Moreover, it is extraordinary that a Romanist should characterise the late empire as a time when slavery disappeared as the institution gave way to a more medieval form of serfdom when the same scholar has

already said that slavery was uncommon, and that rural slavery was almost unknown in the western provinces. Would this not mean that the fourth and fifth century saw the transformation of a few urban domestic slaves into a multitude of servile peasants? In any case, I (1992b; 1994) have tried to show that rural, back-breaking agricultural slavery did not end at all in the late Roman period, but thrived in the early Middle Ages, at least it did in Gaul and Spain.[2]

Now, I have an idea that rural slavery was much more common in the western empire than is usually accepted, certainly much more common than is accepted by British archaeologists, and even more common than is accepted by most Marxist historians. I suggest that there were millions of slaves in the western empire at any one time. Perhaps I over argue the point for polemical reasons, but it is sadly in need of far more strenuous arguing than even I can manage.

How to prove my point? Well, there seem to be three major lines that might be followed.

PROOF 1: HISTORIOGRAPHY

The first is to do some historiography, deconstruct the work of past and contemporary historians and archaeologists, the truly great and Martin Millet too. Archaeologists certainly have little to say about slavery. A historical critique would have us ask whether the cause is indeed the scarcity of ancient slaves, simply the absence of evidence, or something else. And if there are other reasons why archaeologists have ignored slavery, what are they and why are they.

One could probably show that anti-Marxist and anti-Communist sentiment, conscious or unconscious, not the absence of evidence has largely been to blame for the minute amount of archaeological research on slavery. You may wish to note the next time you read why an archaeologist is going to skip over the question of slavery whether there is any mention of the wrong-headedness of Marxist theory. In the typical western historian's misunderstanding of what is meant by a "slave economy", it is generally taken to mean that a huge amount of general production was undertaken by slaves. The Marxist notion of "slave economy" is rejected on the grounds that the larger portion of all ancient production was performed by the free, by tenants, and by servile peasants. Ramsay MacMullen (1987) went one step further and suggested that most slaves did not perform real productive labour at all, being primarily cleaners, barbers, and sex-workers. In short: there were not that many slaves and

those few there were did not count anyway. By a slave economy orthodox Marxists mean (or meant, since they are probably now all dead or converted) only that the rôle played by slave production was somehow crucial in determining the structure of the society. In other words, slaves need not have been numerous to have been significant to historical developments. As it happens, most orthodox Marxists have assumed that slaves in the western empire were not at all numerous. And I believe them too to be wrong.

It is worth noting that in Branigan and Miles's edited *Villa Economies* there are only two mentions of slaves and they coincide with reference to Carandini's excavations at Settefinestre. Carandini is overtly Marxist scholastically and emphasises slavery in all his works. Indeed, the word is in the very title of his book on the villa excavations. Note too that there is the unconscious non-reflexive parrot-fashion recital of what others have already said. Thus, although there is nothing remarkable about Köln-Müngersdorf, the villa appears time and time again when an archaeologist wishes to briefly mention slaves and pass on to other things. There is nothing so special about it that the same could not be said of hundreds of other villas in western Europe. When a Roman archaeologist wishes to pay lip service to slavery, he or she digs through a few handy books and, consulting the index and without being any the wiser, reproduces either Settefinestre in Italy or Köln-Müngersdorf in Germany or Montmaurin in France just as dozens of others have done before, and only because their original excavators had the courage to mention slaves. This original mention of slavery is then faithfully duplicated in a synthetic work.

In the typical British villa excavation report or synthetic work, there are few if any mentions of slaves. Villa workers are called field hands or farm workers. If the British archaeologist mentions slavery, the chances are almost odds on that the passage is quoting or paraphrasing a foreign archaeologist or just maybe a social historian. Most British archaeologists make the mistake of believing that the terms farm hands and field hands and labourers are neutral and cover up our ignorance of the exact form of their dependency on the villa grandees. In fact, the terms are appropriate to the modern world and carry with them the modern implications of contractual employment.

Of course, this is the result of more than overt or unconsious shyness towards Marxism, for these very bourgeois forms of dependent labour so regularly ascribed to villa workers fit neatly in the long disciplinary tradition of western historical scholarship, which has concentrated on things

of bourgeois values, such as the doings of great men and the world of the mind rather than social history (which only today is beginning to rival political and intellectual history) and the oppression of women and the poor. Yet, even today the rich and their mansions are far more often the object of study than the poor, who lived such uninteresting drab lives in uninteresting and drab houses.

Oddly, despite the slow rise of social history and feminist studies in the last three decades, this period has seen further influences on Roman archaeologists which have neutralised these gains. And the result is an even greater tendency to overlook ancient slavery. In particular, there has been a marked reaction against old-style classicism as archaeology has come more into its own. The organiser of the very first TRAC went so far as to say that Roman archaeologists and Roman historians should work independently, only coming together from time to time to check results. This was a natural reaction against the "archaeology-as-handmaiden" syndrome (one already experienced in medieval studies). The attitude is perfectly understandable, utterly indefensible, but perfectly understandable. Imbued with the classics, it was once easy for Romanists in Britain to paint a picture of the villa using the pallet of Columella, Cato, Pliny, and Varro. Twenty years ago, John Burke in *Life in the Villa in Roman Britain* was still able to write in the old style (p. 41):

> But the slaves remained constant; or, rather, there were new generations and perhaps different races of slaves. The civilising influences which led the Romano-British to ape their conquerors and practise some of the refinements of living, including the art of philosophical conversation over the dinner table, did not extend to the treatment of those unfortunates whose labours made possible the accumulation of so much wealth. There were slaves in Rome and in the provinces, and always had been.

But the easy use of classical texts has ended. Brooches are the new texts.

Burke was happy to stress the monstrosity of Roman masters towards their ubiquitous slaves. And this image of Romans is something of a cliché drawn from the classical texts, the result of reading too much Suetonius and Juvenal. It clearly needed some revision, but the "New Archaeology" wanted rid of all that old-fashioned culture history. Romanists were the least affected (or infected) by New Archaeology, but they were nevertheless touched, shared some of the seventies' enthrallment with "continuity", experienced some of the infiltration of a smidgin of social anthropology into British archaeology, and a few youngs ones

have even adopted the post-processual cockiness that everything before Foucault was wrong. This has conspired to produce images of Romano-British society regulated or dominated by kinship ties, ideological meanings, and "central-place" geography. Down with the toga! In Britain, Romans were just down-the-line gift-exchanging Iron-Age tribesfolk in rectangular houses or, worse, contextual manipulators of ideological cosmologies.

So, you see, Roman archaeologists are happy not seeing slaves for several reasons. Slavery falls outright into the arena of social history, and a Marxist circus at that, a place where few Roman archaeologists stray. Traditional, conservative, bourgeois Romanists prefer military and imperial political history, armies and administration; New Archaeologists prefer Thiessen polygons, positive systems' feedback, and any number of mathematical equations; both utilise economic models that are often capitalistic in their assumptions and applications; Hodderite contexualists are largely trapped in the mind game of "meanings" and seem to believe that ideological thought is the moving force in history; the trendiest post-processualists rarely get beyond a reiteration of all the essential theoretical litany of agency, recursivity, *différance*, and phenomenological hypertextual routinisation; while most practical Roman archaeologists, like practical archaeologists of all periods, spend most of their time counting, measuring, and typologically ordering their pottery, brooches, and sites. To those who do not believe that the disciplinary tradition plays a dominant rôle in Roman archaeologists not mentioning slavery, I ask this question. How is it that almost unreadable abstractions on "fields of discourse" may end up in the bibliographies of Roman archaeologists, along with references to French semioticians, while the superb works of the social historians Keith Bradley and Keith Hopkins are almost never to be found?

PROOF 2: POSITIVE EVIDENCE

The second line of proving the existence and extent of slavery is the simplest. We could just muster all the positive evidence there is. For archaeologists this is not easy. Can we recover what might be interpreted as the archaeological paraphernalia of slave markets, such as the market itself or slave tallies for accounting? Are ownership tags known in the archaeological record? Now, this question I invented as a rhetorical one, not being a Roman archaeologist and shying away from anything as common as evidence. So, you can imagine my surprise when I promptly

came across three examples of slave collar tags which read, "Stop me from running away and bring me back to my master, Viventius in the yard of Callistus" and "Restrain me so that I don't escape, and take me back to my master Pascasius in the colonnade in Trajan's market" and "I have run away, so keep hold of me. When you return me to my master Zoninus you will receive a reward" (all quoted and referenced in Massey and Moreland 1978, 62). Remember Juvenal's Rutilus in the *Satyricon* (XIV. 15), "To his trembling household he is a complete monster, never happy until he has called for a torturer and some poor wretch is being branded with a hot iron – and all because of two missing towels." Have we yet found any branding irons bearing the initial FHE, for "this is a fugitive"? How frequently do we find chains for humans? It took me only a moment to find a list of twelve sites in France yielding slave chains (Famechon, Somme; Moulets-et-Villemartin, Gironde; Annecy aux Ilettes; Condé-sur-Iton, Eure; Liberchies, Belgium; Epiais-Rhus, Val d'Oise; Vertault, Côte-d'Or; Compiègne, Grand, Vosges; Saintes; Alesia; Strasbourg). And what about that impressive chain from near Cambridge with collars for six? And what was the purpose of the *cryptoporticus* often found under the forum (as at Arles, Narbonne, Reims, etc.)? Some suggest this was a slave barracks for the town's public slaves. Can we spot slaves in Roman sculpture? Many of the household servants depicted around the representation of the deceased on tombstones must be slaves. On a fragment of a French stone we see a carving of what is clearly a slave, for he is being freed by a touch of the *vindicta*, a rod, used in manumission ceremonies (fig. 1).

Feminists will be most acutely aware that the infrequent reference to women in antiquity and the even less frequent reference to women by modern scholars does not imply that they did not exist. The same is probably true of slaves. So, finding little evidence for slaves does not necessarily mean there were no slaves. My impression is that there is scattered and inferential evidence for slavery, but it is generally ignored, and that there is not much of what you might call solid evidence for slavery, not much solid evidence, that is, if we overlook one source. Tomb inscriptions reveal some eye-popping facts. Sure, there are few slaves commemorated, and those are almost all domestic slaves of high standing. But then there are few enough of the common rural, but free peasants, such as the *coloni*. Although historians (including Marxist historians) consistently accept that in the late empire *coloni* probably vastly outnumbered slaves, they are even rarer in the epigraphic evidence. Almost non-existent to be honest. For a period of three hundred years

Figure 1. Sculpted fragment of a manumission scene. The slave is about to be touched by the vindicta, the rod, which is the same rod that probably beat him on many occasions before this ceremony (Musée Royal de Mariemont).

from Britain to Spain, from Sardinia to Dalmatia, and from Hungary back to Britain (more or less all of Europe except Italy) there are only fourteen *coloni* recorded epigraphically. (There are more epigraphic references to *servi* than *coloni* in Britain.) But there are huge numbers of inscriptions remembering the deceased freedman and freedwoman, the *liberti*.

If we did some simple mathematical modelling, in the style of a New Archaeologist, we might say that one in ten slaves were freed by the time of their death (and here I am exaggerating in order to *weaken* my proposal in the spirit of the null hypothesis, for it is likely that a much smaller percentage were freed). Freed slaves, on the whole, must have been poorer than ordinary citizens, but let us say that free people were neither more or less frequently commemorated with tomb stones (again I am purposefully *weakening* my own proposal, for I could legitimately suggest that freed slaves were more likely to lack the wherewithal of the free to put up stones). A quick calculation based on the actual figures of social groups commemorated on stones would make the slave population, on average, somewhere between forty and eighty per cent of the total population in the western empire. And this is a conservative estimate.

Now, it is no accident that *liberti* are well and truly over-represented and that the free did in fact make up much more than a third of the population. Freed slaves, and even more so their children who probably set up most of the stones, had a vested interest in making public statements that recalled their manumission, not wishing former masters or their successors to try to reassert their former ownership. (This, I trust, you will find a more reasonable explanation than MacMullen's suggestion that somehow the *liberti* were simply addicted to epigraphy.) But even if *liberti* were over-represented, there must have been relatively large numbers of slaves in some provinces in the west for freed slaves to be so common among our tombstone inscriptions.

PROOF 3: THEORY

The third approach is the least straightforward, the most theoretical, and therefore the most appropriate to TRAC. We can investigate or theorise about the nature of slavery itself or of slave societies as entities and look for corroborating evidence for slavery. For example, what is the rôle of the state in the maintenance of slavery and does it manifest itself materially? The ability of one person to sell another is the most classic element of slavery. The acquisition of new slaves and the vitality of the slave trade is regularly assumed by bourgeois historians as all important to the

maintenance of slavery. Without a source of new slaves, many historians believe that slavery is doomed to die out. Be that as it may, can archaeology suggest widespread slave trading by plotting a one-way flow of traded goods and hypothesise that the archaeologically unrecoverable objects traded in the other direction were slaves? I believe it was Andy Fitzpatrick who showed that three rim sherds of Dressel 1A *amphorae* are the trade equivalent of one strong adolescent male slave or an older woman with two children.[3]

Here, however, my interest is in the nature of slave labour, particularly in its organisation and control by masters. While the evidence is not unequivocal, we have masses of it – the villa excavations.

Slave labour: theoretical premises

Time itself was not ordered by slaves. Time to eat, time to rise, time to sleep, even time to fornicate. Slaves' labour was not their own, nor was the produce of that labour. Certainly slaves did not own the means of production, the livestock, equipment, or buildings. Here I have gone from the least easily demonstrated archaeologically to the most easily. We surely cannot demonstrate the chronological patterns at the daily level. At the next level, and despite the pessimism and dismissiveness of some British archaeologists, I believe that the organisation of labour can be demonstrated. But more of that shortly.

MacMullen is only one in a long list of bourgeois historians who have insisted on legal definitions to distinguish *coloni* from slaves, maintaining that they cannot be lumped together. Archaeologists naturally cannot recognise legal status, with the result that, like Harald von Petrikovits nearly half a century ago, they abandon the effort to distinguish between "dependent peasants". Although Richard Hingley is not afraid to mention slavery in his *Rural Settlement in Roman Britain*, a typical pasage runs like this (p. 67):

> This model [of two unequal "houses" on one villa] specifies that aisled houses were the homes of labourers, tenants and slaves on a villa estate. The alternative model [is] that aisled houses were the homes of extended kin groupings and that the evidence does not appear to suggest a rigid division between landowner and tenant.

Almost every mention of slaves is in a bundle of other forms of dependency, usually, as here, including tenants (is this a translation of *coloni*?) and labourers. Thus, Hingley allows for the existence and presence of

slaves in rural Roman Britain, but he refuses to try to recognise them. This is an improvement on Millett and Greene but ultimately is a capitulation to the bourgeois historian.

In fact, the ancient legal definition of freedom and unfreedom or *colonus* and *servus* is largely irrelevant. Or it is to us, at any rate. Medieval history demonstrates that thirteenth-century peasants hated the slur of being a villein. Even if economically better off than some neighbouring free tenants, villeins might well pay good money to escape their status. But medieval history also demonstrates that legal definitions of an absence of "freedom" hardly made a villein a "slave" in any sense of the word that a good social historian would use today. For us, the real importance is the degree and nature of control a Roman lord or master had over his or her "labourer". A *colonus* was no "tenant" if he lived in special quarters on the villa of his *dominus*, was not free to leave, and did not organise his own labour but did what he was told. Such a man was very nearly a slave, and extreme forms of servile dependency may well have left obvious spatial and material traces.

Villas

When archaeologists investigate villas for possible slave quarters, they start with Columella and his description of the *ergastula*. Well, this prison block clearly was not common. And one reason why Settefinestre features so commonly in archaeologists' references to Roman slavery is that it has something that passes for the *ergastula* (fig. 2). But very few pay attention to the passage of Columella's in which he assumes that slaves will be quartered according to their jobs. Cattle herds might be expected to sleep in the byres, others in the barns. In a sense, this is how Köln-Müngersdorf became so well known. A few buildings were postulated as perhaps holding slaves in addition to their obvious agricultural functions. When I first started looking at villa plans in search of possible accommodation of slaves I was particularly excited by the French double-courtyard villas. At Warfusée-Abancourt you can see that a pale imitation of the great house is sometimes to be found. It is interpreted as the residence of the *actor*, the overseer, who was more than likely a slave. Roman writers, such as Varro and Columella, have much to say about the *actor*, his qualities and age and personality and his duties. They even say something of his mate or "wife" in inverted commas – for slaves could not legally marry – and how good looking she should be if the master picked her out for him, and how she was to behave and who she was to supervise. Reading between the lines there appears to be a division of

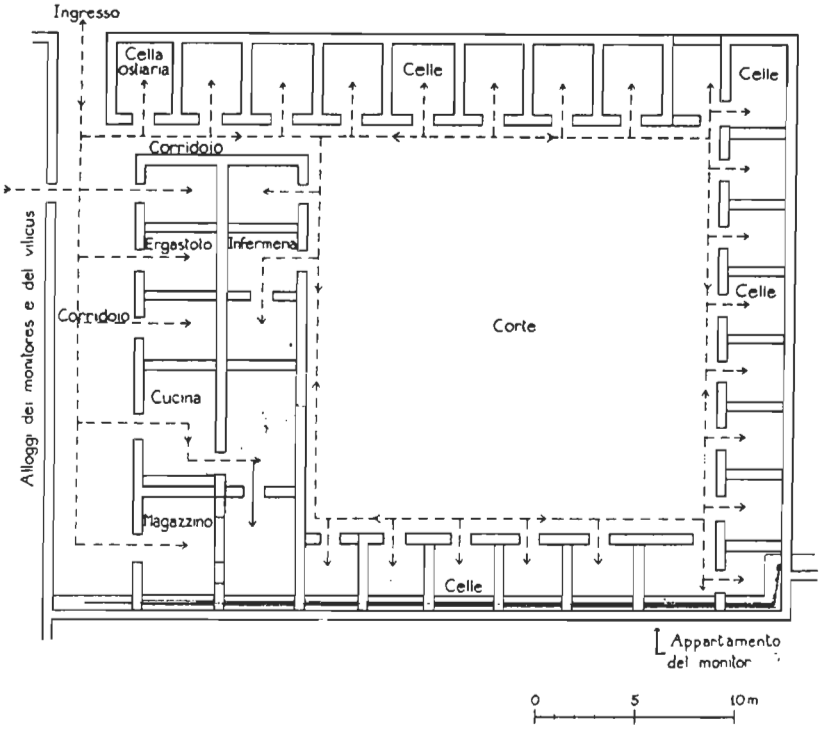

Figure 2. Plan of the slaves' quarters at Settefinestre (after Carandini 1988). Labelled rooms include cells (celle), infirmary (infermeria), kitchen (cucina), store room (magazzino), "porter's cell" (cella ostiaria), and the overseer's lodgings (alloggi del vilicus).

labour between genders, which also had a spatial element and something of an agricultural-domestic division as well.

Well, my initial excitement stemmed from the belief that the *actores* were more likely to be organising and supervising slave than free labour, but for the moment let us look at the *actor*'s home. It was probably built by and owned by the master(s) in the great *domus*. Indeed, all the buildings in the outer courtyard were surely owned by them. It seems to me that the outer buildings are surprisingly well built to house animals and "farm hands". The French villas found by Agache through aerial photography, for example, are only recognised because their foundations, at least, were in stone. If we are to accept the presence of labourers on the villa we probably should begin by accepting that they did not live in their

own homes, that the buildings they inhabited were more substantially built than those of their social equals who might live in their own homes far from any villa, that they lived within a compound in which an overseer possibly dwelt, whose residence mirrored that of the landlord and lady, and whose status might well be unfree. So, what sort of dependency held these "hands" to the villa owners?

Can the spatial organisation of the villa throw light on the nature of the control of that labour? I have argued (1992a) that the enclosure wall had more than the rôle usually imputed to it: of preventing access or demarcating where access was no longer legally or socially permissible. The enclosure wall was more than some cosmological symbol, the beloved explanation of the ideology-crazed post-processualists. More than keep out animals, robbers, and taxmen, it kept people, particularly slaves, in. The simple expedient of an enclosure wall meant not that slaves were trapped in, as if in prison, but should they be found outside when they were not supposed to be, after curfew perhaps, they could be instantly recognised or charged with trying to escape. They could not plead ignorance. According to Columella (1.8.12), no slave should leave the farm without the overseer's permission. Cato, in his *Agricultura* (5), may just give us evidence of an evening curfew, when he wrote of the overseer's duties:

> He must be the first to get out of bed, and the last to go to bed. Before that he must see that the farm is shut up, that everyone is asleep in their proper place, and that the animals are provided with fodder.

Varro, in his *Agricultura* (2.10.1 and 2.10.6), makes the distinction between herdsmen that go back to the farm every night and those who stay away – who are of special concern. Conversely, Columella (12.3.7) stresses that the female *actor* is to make a close inspection that none of the field slaves is still inside the farmyard when the team has gone out to work, for it is symptomatic of the malingerer.

The existence of an enclosure wall is almost a necessary pre-requisite to the day-to-day maintenance of slavery if flight is at all a common form of slave resistance, and there are whole books devoted to the subject of slave flight in the Roman period (e.g. Bellen 1971). It is no accident that medieval servile labour – serfdom – did not involve such enclosures.

Now, almost every villa in the north-west of the Roman empire had an enclosure. It may have had nothing to do with social control of labour and everything to do with penning in rare, colourful and exotic animals,

such as peacocks and peahens, or it may have been thought essential to signify the *axis mundi* lest the sky fall on top of them. But the fact that the accommodation for the workers surely belonged to the villa and was located so close to the master or overseer makes it hard to see the labourers as anything other than closely controlled servants, presumably slaves. It is a pity that British archaeologists have made no effort to describe how they envisage the relationship between villa owners and so-called farm workers. Could the workers have been indentured servants? Perhaps. Itinerant waged farm labourers taken on at an annual hiring fair seem unrealistically anachronistic. A tenant, in the sense of a cash- or tribute-paying renter of land, is surely out of the question, for these workers most assuredly worked the villa's home fields rather than their "own". Indeed, this is implied by British archaeologists when they use terms like field or farm hands. And yet, "tenant" is one of the most common terms used by Roman arcaheologists (to translate *colonus* one must assume) to describe the labourer housed in a villa outbuilding.

It is not true that archaeologists in Britain have totally ignored social relationships among the inhabitants of villas. Twenty years ago, J. T. Smith first drew our attention to villas with what appears to be two houses and suggested joint ownership, presumably of close kin (siblings one might suppose). This opened the way to seeing villas as perhaps accommodating extended kin. The argument is interesting, not least because it holds out the possibility of seeing Romano-British society as essentially prehistoric or early medieval. Villas would have been more egalitarian than I envisage them, but the vision is not one I can share, for it seems to suffer from two deficiencies.

One problem with the argument is that the supposed duplication of domestic "units" is only rarely encountered on villas. Many of the supposed examples are just not very believable. For instance, Hingley (1989, figures 33 and 34) offers the plans of villas with "two relatively equal houses" and further draws attention to villas with separate aisled halls, which are suggested to hold either estate workers (following Richmond) or a separate branch of the family (following Smith). The problem is that the "second" house is seldom obvious and that which is presumably being posited as a second house (in some cases I cannot tell) is usually quite different in plan and even in size. This is most extreme in the case of aisled halls, which regularly shelter one room into which alone could be fitted four or five of the biggest rooms of the main luxurious villa *domus*. At Winterton and Sparsholt, for instance, the *domus* and aisled hall are so different in plan that one really has to imagine the buildings func-

tioning quite differently. Even to suggest they might be two variations on the same theme is to admit that we really have not a clue what was happening in the various rooms of Roman villas. But the idea that there was little differentiation between the lesser kin who all bunked down in the hall of the aisled building of a villa such as Winterton does seem like nonsense. While we may not be able to label each room and space at Winterton villa, the layout in plan simply reeks of specialisation. The space was highly ordered and one might presume also hierarchically ordered. Moreover, rooms in Roman houses have quite a special nature compared to prehistoric and to medieval rooms which seems to be little appreciated by archaeologists, too happy with their plan drawings. Roman rooms are quite small. Many rooms are under twelve feet wide and would be at home on a "Brookside"-style estate. If you place the plans of an Iron-Age round house, a Roman villa, a Dark-Age hall, and a Jacobean manor all side by side, what is most remarkable is how the space of a Roman house was so much more divided up into small rooms. To see a Roman master as some Brian Blessed-like character throwing bones over his shoulder while a packed crowd of dependants, the *familia*, slobbered, belched, and snored in a large communal hall is to borrow a thoroughly fanciful medieval image and make it even more inappropriate.

The second problem with the egalitarian extended family interpretation is that a large villa may well have held a hundred people or more. More, at any rate, than an extended family. At Winterton some fifty rooms have been excavated. In his introduction, Hingley (1989, figure 3) introduces us to the extended family. The model of a three-generation family, borrowed from Thomas Charles-Edwards, has only twenty-five members, which include the two great grandparents who were probably no longer living when the youngest grandchild was born and three women who presumably would have married and gone to live with their husbands, assuming a patrilocal society. In other words, the villa would have been populated with just six kids, two teens, eight young adults, and four middle-aged adults. That makes fifteen able bodies, but surely they did not all plough and hoe and harvest and muck out and weave. Villa owners were at the apex of society and presumably did not often break into a sweat. At least some of these fifteen must have swanned about, and, when not bathing, been fed grapes while reclining. Perhaps a villa estate could well have been worked by ten strong women and men; I am no farmer and have no practical knowledge of these matters.

If a Winterton-sized villa needed far more workers (ancients and

archaeologists alike asume a rough figure of one hundred, cf. Samson 1989), to stock it with yet more extended family would have required all the descendants from an ancestral pair six or even seven generations back in time. Now, this is not an extended family as anthropologists know it, this is a true lineage. This postulation turns villas estates into some sort of tribal kibbutz, containing cousins five times removed, which while quite a sweet picture of Romano-British society is also quite daft.

CONCLUSIONS

The most unsatisfactory aspect of villa archaeology must be our almost total inability to ascribe functions to the dozens of rooms we find on any plan (this observation has also been made by Millett 1990, 197). Where is the master bedroom, the study, the childrens' play room, the library, the billiard room, or the guest's room? Where did the nanny sleep? Where was the maid's room and where did she keep the linen and mops? Was there a granny flat? We are ignorant of the layout of most of the big house (where past excavations have concentrated), so it is not surprising that little can be said about the arrangements for agricultural staff. But one thing seems almost unanimous among scholars and that is the assumption that many labourers did shelter somewhere in the buildings in and around the villa enclosure wall.

While I cannot mystically divine slavery from a villa enclosure wall and a well-built stone agricultural building against it that may or may not have afforded accommodation to workers, at least I have a plausible theory that says that both of these bits of architecture point to intensive control of the labourers by the villa owners, which implies slavery rather than tenantry. Almost all Roman archaeologists make no explicit theoretical statement about slavery or dependent labour. Most do not even mention slaves but prefer terms that better fit modern capitalism, deluding themselves that they are somehow "neutral" terms. I may be wide of the mark supposing that this is the result of an explicit loathing or an unconscious shared western political and cultural fear of Marxism, a backlash against culture history and classicism by New Archaeologists and post-processualists alike, or just the stupid parroting of what is found in the secondary sources. But one thing is certain, most archaeologists' thin descriptions of dependent labour on Romano-British villas, devoid of reference to slavery but full of references to "farm hands" and "tenants", are, frankly, pure nonsense.

NOTES

1. Here is the seldom-seen bedrock of most Romanists' assumptions on slavery in the western empire: the slaves produced from early imperial expansion died out naturally (somehow they did not reproduce in large enough numbers), the opportunities to recruit new slaves dried up in time, slave owners finally realised that it was in their economic interest to let their slaves go, because as slaves they were lazy and good for nothing, and that, in any case, there was no slavery anyway. Thus, we find Peter Salway, who rarely mentions slavery in *Roman Britain*, arguing on the page headed "slaves, labourers and tenants" (p. 605) (1) that "under the Empire, as conquests became less frequent ... the availability of new slaves must have become less", (2) that "there is no direct evidence in Roman Britain for the great slave-run estates or *latifundia* of Republican Italy", and (3) that "by imperial times Roman writers were doubting their economic efficiency and preferring tenant farmers".

   This stereotypical explanation for the end of slavery is also employed for the Middle Ages (see Samson 1994). The "explanation" sees slavery as somehow uneconomical and therefore even almost unnatural, which beggars belief and can only be ascribed to a mad free-market capitalist mentality that says that people only work hard if working for themselves. It makes me wonder how, if Roman masters and medieval lords were able to work out that they would receive increased profits from less direct exploitation but greater productivity after freeing their slaves, plantation owners of the American South were unable to make the same calculation. Moreover, I know of no abolitionist literature that ever put forward this argument that slavery was "uneconomical". It seems just possible that abolitionists refrained from making such claims for fear that plantation owners would, rightly, have seen them as economic simpletons.
2. I apologise for referencing no fewer than five different pieces of my work, the medieval ones of debatable relevance. I hate this self-indulgence in others, but a bout of paranoia has convinced me that no one has ever read anything I have written. This is no more than a desperate bid to remind others of these often-overlooked gems.
3. Dear reader, this text contains many instances of the first person pronoun because I actually did write these words and think these thoughts. I believe there is nothing wrong with admitting that the thoughts are my own and not some objective analyses that sprang from a disembodied undefined author-scholar. (Indeed, the thoughts are more completely my own than might be the case of other scholars because my friends do not seem to have the time or inclination to read my work or pass comments any more.) As it stands, Dr Reviewer, the paper does not read like an unrevised lecture. It was almost completely rewritten for publication. I simply prefer clear and unpretentious prose, especially if it occasionally amuses. The reference to Fitzpatrick, as far

as my memory now fails me, is totally spurious. I apologise to Andrew for any distress this may cause him.

REFERENCES

Bellen, Heinz 1971. *Studien zur Sklavenflucht im römischen Kaiserreich*. Wiesbaden: Steiner.

Bradley. Keith R. 1984. *Slaves and Masters in the Roman Empire: a study in social control*. Tournai: Latomus.

Branigan, Keith and David Miles (eds) 1988. *Villa Economies: Aspects of Romano-British Villas*. Sheffield: John Collis.

Burke, John 1978. *Life in the Villa in Roman Britain*. London: Batsford.

Carandini, Andrea 1988. *Schiavi in Italia. Gli strumenti pensanti dei Romani fra tarda Repubblica e medio Impero*. Rome: La Nuova Italia Scientifica.

Collingwood, R. G. and Ian Richmond 1969. *The Archaeology of Roman Britain*. London: Methuen.

Demarez, J.-D. 1987. Les bâtiments à fonction économique dans les fundi de la provincia Beglica. *Amphora* 50, 1–36.

Greene, Kevin 1986. *The Archaeology of the Roman Economy*. London: Batsford.

Hingley, Richard 1989. *Rural Settlement in Roman Britain*. London: Seaby.

Hopkins, Keith 1978. *Conquerors and Slaves: sociological studies in Roman History*, vol. 1. Cambridge: Cambridge University Press.

MacMullen, Ramsay 1987. Late Roman slavery. *Historia* 36, 359–382.

Massey, Michael and Paul Moreland 1978. *Slavery in Ancient Rome*. London: Macmillan.

Millett, Martin 1990. *The Romanization of Britain. An Essay in Archaeological Interpretation*. Cambridge: Cambridge University Press.

Percival, John 1976. *The Roman Villa*. London: Batsford.

Rivet, A. L. F. (ed.) 1969. *The Roman Villa in Britain*. London: Routledge.

Salway, Peter 1981. *Roman Britain*. Oxford: Clarendon Press.

Samson, Ross 1989. Rural slavery, inscriptions, archaeology and Marx: a response to Ramsay MacMullen's "late Roman slavery". *Historia* 38, 99–110.

Samson, Ross 1990. Comment on Eleanor Scott's "Romano-British villas and the social construction of space". In Ross Samson (ed.), *The Social Archaeology of Houses*, 173–180. Edinburgh: Edinburgh University Press.

Samson, Ross 1992a. Knowledge, constraint and power in inaction: the defenseless medieval wall. *Historical Archaeology* 26, 26–44.

Samson, Ross 1992b. Slavery, the Roman legacy. In Hugh Elton and John Drinkwater (eds), *Fifth-Century Gaul: a Crisis of Identity*, 218–227. Cambridge: Cambridge University Press.

Samson, Ross 1994. The end of early medieval slavery. In Allen Frantzen and Douglas Moffat (eds), *The Work of Work. Servitude, Slavery and Labor in Medieval England*, 95–124. Glasgow: Cruithne Press.

Smith, J. T. 1978. Villas as a key to social structure. In Malcolm Todd (ed.), *Studies in the Romano-British Villa*, 149–156. Leicester: Leicester University Press.

Smith, J. T. 1982. Villa plans and social structure in Britain and Gaul. *Caesarodunum* 17, 321–351.