MIGRATION AND THE ROMAN FAMILY IN THE FIRST TWO CENTURIES A.D.

Laurens E. Tacoma

Introduction

One of the famous 'laws' used in the study of migration in history is that there exists an inverse correlation between the scale and the distance of migration. Most people move only within a limited area, and only few move very far. Although the latter type of migration is certainly more spectacular, migration historians have started to realise that short-distance movement is very important as well. As the law stipulates, it occurs on a large scale, and it is precisely because of its large scale that it determines very much the rhythm of everyday life.

Rather unsurprisingly, it is precisely short-distance migration that is difficult to trace, however. If one likes laws, one could add a second one to the one stated above. There is not only an inverse correlation between distance and scale of movement, but there is also a positive correlation between distance and documentation. The shorter the distance travelled, the less likely it is recorded, and vice versa. In many respects short-distance migration will have been regarded as unproblematic or uninteresting and hence will have escaped documentation. A trader who is shipwrecked is more spectacular than a peasant moving to the next village.

The problem of a lack of documentation of short-distance mobility applies to all periods of history. But for many periods post-dating antiquity there are remedies available. Movement in bulk can be approached through statistical analysis of aggregate data. In early modern history, there are for example parish registers that allow a quantification of mobility.[1] It is precisely such data that are by and large absent for the Roman world. Even the papyri of Egypt do not offer the type of evidence that is needed, although I do suspect that more can be done on the basis of village registers of the Fayum then has been done so far and Bagnall and Frier have argued that the Egyptian census documents might be used for an indirect analysis of rural-urban migration.[2] Outside Egypt there is no such documentation available at all. The problem of charting short-distance migration is therefore especially pertinent in the case of Roman history.

---

[1] Osborne (1991) cites several such studies.
[2] Bagnall and Frier (1994) 160-169.

One possible way out has been shown by the migration historian Page Moch, who took her cue from work by the historical demographer Hajnal.[3] It is possible, she has shown, to approach short distance migration from the perspective of the family. Migration might not only influence patterns of family formation, but the reverse is also true. Marriage patterns and household composition create conditions which make migration possible for some members of the family and not for others. Some people remain tied to their kin, others become at certain points of their life available for the labour market or seek an education outside the hometown and move permanently or temporarily. Household composition therefore shows who is available for migration. In itself such an approach is indirect, because the availability of potential migrants does not mean that the candidates *actually* move. But household formation can be used to analyse the conditions that make migration possible.

It is certainly valid to apply Page Moch's ideas to the Roman world, and explore the relation between marriage, household structure and migration in the Principate. Not only because otherwise short-distance migration is bound to remain obscured from our view, but also because there is so much work on the Roman family already done. The lack of sources on Roman short-distance migration is amply compensated by the abundant scholarly literature on the Roman family. In the mid-eighties, scholars could still complain about the lack of serious investigation into the Roman family.[4] Such comments now seem strange in view of the virtual deluge of studies that have appeared since. An immense amount of work has been done on precisely those issues that are of importance for the subject of migration: patterns of marriage, mortality patterns, household composition, the position of women both within and outside the family, and so on. The subject is now firmly on the ancient historical agenda. Moreover, there is an added advantage in approaching migration through the family, in that it allows a discussion of migration of women. In the Roman evidence, it is not difficult to find travelling women attested in inscriptions, but the question is how their movement should be interpreted. In early-modern Europe, female migrants played a large role as servants. It is obvious that the role of women in the Roman world was different, but how should we perceive this difference?

In what follows, I will offer an exploration of the relation between migration and the Roman family of the Principate. In what ways does the form of Roman households produce

[3] Her work on the subject is not published yet, although some pertinent remarks can already be found in Page Moch (2003[2]). I base myself on a lecture given by her at Leiden in 2006 'Leaving the Family. Demography and Global Migration - China and Europe Compared'. For Hajnal see below.

[4] Saller and Shaw (1984) 145: 'The family was the fundamental unit of social reproduction in the Roman world, and yet it has received little systematic study by social historians.' Also Rawson (1986) 1-6, complaining about the lack of serious study of the Roman family, apart from the study of Roman family law. A good introduction on the Roman family is offered by Dixon (1992).

candidates for migration? Just like Page Moch, I will take my cue from the work of Hajnal. In section 1 I will discuss how Hajnal envisages the relation between marriage and household patterns on the one hand and migration on the other hand. This forms the background for a discussion how Roman household composition (section 2) and Roman marriage patterns (section 3) fit in with Hajnal's typology. In section 4 I will discuss the repercussions of high levels of mortality for household composition. In section 5 a category ignored by Hajnal will be discussed: that of widows.

Most of my discussion will be historiographical. I use as a starting-point a number of influential studies: those of Hajnal, several works by Saller and Shaw, and the work of Hopkins. This may appear like a retreat: I go over ground already covered by many others. But one of my arguments is that some of the central conclusions of these studies are in need of revision. Before any new research is attempted, it is important to reassess the value of these studies, and proceed from there.

I should perhaps also explain some of the definitions I use – and some that I do not. Three elements of the title 'Migration and the Roman family' might require preliminary exegesis: 'migration', and, in the phrase 'Roman family', 'Roman' and 'family'. What is meant by these terms?

I leave migration undefined – and deliberately so. In this article, the focus will be decidedly on the Roman family itself. Migration will only play a role in the background. As already stated, family forms may produce candidates for migration. Whether and to what extent family members actually migrate requires separate analysis. Such an analysis should not only focus on topics like the structure of the labour market, but also on the question what exactly constitutes migration. Should we regard a widowed woman returning to her paternal home as a female migrant? As has become increasingly clear in studies of migration, mobility and migration form in fact a spectrum of possibilities. There is little point to pre-empt further study by using an exclusivist definition.

What constitutes a 'family' is also left open initially, but that is because much of what follows will be devoted to precisely that topic. It has been aptly observed that *defining* the family is what family history is about, and that such definitional issues should come at the end, not at the beginning of the research.[5]

The definition of 'Roman' presents more of a problem. In itself it is not difficult to delineate the subject under inquiry, but the underlying issue is deeply problematic: to what

[5] Martin (1996) 50 n.39.

extent should we expect uniformity in family forms in the whole of the Roman population? The Roman Empire comprised some 60 million people dispersed over an enormous stretch of territory. As it consisted of many local cultures and enormous varieties in wealth and status it is certainly conceivable that with respect to family forms it also displayed variety. In fact, the existence of such variety seems to be suggested by the existence of divergences in marriage practices. The archive of Babatha suggests that polygamy occurred among the Jews; much better known is the uniqueness of brother-sister marriages of Roman Egypt. As it is the senatorial aristocracy[6] that dominates the literary evidence, and the literary evidence usually has had pride of place in historical analysis, aristocratic behaviour has always formed the backbone of the study of family life. It is still not always recognised that the aristocracy forms in fact a small subset of the population with a very peculiar position whose behaviour may or may not have been followed by the rest of the population.[7]

Although I would like to cover the general population of the Roman Empire completely, it is unpracticle if not impossible to discuss every particularity of all the various subsets within it. I will therefore base myself on studies of the inscription-erecting population of the western half of the Roman Empire, and more in particular of the free citizens within that population. Given the fact that most of their inscriptions originate from urban contexts, this means that my analysis will concern primarily the urban population.[8] I further assume that the population I discuss was either subject to Roman law or adhered to its basic principles. This assumption is in fact implicit in many scholarly works on Roman family life, not only in those who approach the subject from a legal angle. It is therefore perhaps something too obvious too mention, where it not for the fact that citizenship was far from universal until A.D. 212. On the other hand, legal concepts of family law were also used by persons who technically were not subject to it: slaves and soldiers. The inscription-erecting population can almost by definition be regarded as the most romanised part of the population, and is therefore likely to have consisted of either Roman citizens, or at least of people who closely adhered to the formulae of Roman law.

---

[6] In what follows, I will use the term 'senatorial aristocracy' in a loose sense. It comprises all persons belonging to the very top of the economic, social and political pyramid of the population, not only those who in a formal sense belonged to the senatorial order.

[7] Even otherwise excellent studies such as those of Corbier (1991) and Bradley (1991) do not escape completely from the assumption that what applied to the senatorial class also was applicable to the rest of the population.

[8] The social profile of the inscription-erecting population has been debated all too often. For a good and convincing overview, see Saller and Shaw (1984) 127-128.

The reasons for my choice are partly practical. This is a study based on secondary literature, not on independent research of ancient sources, and the inscription-erecting population of the Latin west is well covered by modern research. There are also negative considerations. In the first place, it is important to get away from the literary sources which are so biased towards the senatorial aristocracy. In the second place, at the other end of the social spectrum it is almost impossible to discuss the rural population (to the extent to which it did not erect inscriptions), unless one is prepared to venture into pure speculation. In the third place, the inscription-erecting population of the Greek East would form an excellent area for research, but it remains by and large unstudied and this is hardly the place to embark on such a vast project.[9] Lastly, although the census population of Roman Egypt is excellently documented and studied, in a number of cases it seems to have features that are particular to that population only. The inscription-erecting population of the Latin West certainly does not represent the total population of the Roman Empire, but it does in my view form the best starting point to study it.

1. The Hajnal Hypothesis

In 1965 and 1982 the historical demographer Hajnal published two articles which together presented a theory which in later years has been termed the Hajnal hypothesis. This famous hypothesis stipulates that there is a unique pattern of marriage (1965) and of household composition (1982) that prevailed for a long period in recent European history and that is not found elsewhere or in earlier periods of history.[10]

Hajnal's work forms an excellent base for an exploration into the relation between household formation and migration in the Roman world. In the first place, Hajnal's family typology has been extremely influential among historians of the family and has formed the basis of all subsequent discussion of family typology and marriage patterns.[11] Secondly, Hajnal also made a number of remarks on the social and economic implications of the differences in family formation. These remarks also concerned the issue of migration. Thirdly,

[9] To the best of my knowledge, the research project on the family in the Greek East announced by Martin (1996) has not been published.

[10] Hajnal (1965) and (1982). The (1982) article has also appeared with minor alterations as Hajnal (1983).

[11] Hajnal in his turn was quick to point out that his focus on the importance of late marriage was not new; it was already a major theme in Malthusian thought - Hajnal (1965) 130.

and not very well realised by ancient historians, Hajnal also discussed the ancient world.[12] Lastly, although there is little hint in their work itself, the studies of Saller and Shaw on the Roman family that will be discussed later on in this article can be read as a direct reaction on the Hajnal hypothesis.

The European marriage pattern as described by Hajnal has two characteristics: marriage occurs late both for women and for men, and there exists a substantial group of persons who never marry. Men and women delay their marriage up to the age of 30, and some 10% of the population remains single for the whole of their life. The actual ages and figures differ from country to country, but the general pattern is remarkably consistent. The pattern can be found west of the line Petersburg – Trieste and can be traced from the 1940s backwards all the way to about 1740. The demographic evidence for marriage *before* that period is so thin that it is difficult to be certain about earlier periods, but Hajnal considered it likely that at some moment between the Late Middle Ages and 1740 the European marriage pattern came into existence. Both medieval and ancient evidence suggests a different pattern. The modern European pattern is also unique in a geographical sense: it is not attested outside Europe.[13] To such marriage patterns belongs also a specific type of household: that of what Hajnal called the 'simple family'. Marriage was delayed because wealth had to be acquired in order to set up a separate household. This household is characterised by the fact that the married couple is in charge of its own household; that few relatives live in such households, and that instead lodgers and servants live together with the family. The reason why the pattern of late marriage and the type of household belong together is that precisely because the household is an independent unit, men (and women) had to possess the necessary means to establish it.

By contrast, the pattern found elsewhere shows early marriage for both men and women, and a situation in which marriage is universal. Hajnal termed this pattern 'Eastern European', but perhaps it is more appropriate to term it 'non-European', as Hajnal's main contention was that it prevailed in all societies outside Europe west of the line Petersburg – Trieste. He found it also in various African and Asian societies at different moments in the 19th century. Within Europe itself the non-European pattern was probably the norm before the

[12] Hajnal (1965) 120-125 on marriage patterns in the ancient world. Unfortunately, Hajnal (1982) does not offer a similar treatment of ancient household formation. Hajnal's work is only rarely cited by ancient historians, and these citations concern not his section on the ancient world but his general hypothesis – one wonders therefore how often he is read.

[13] It is however striking how little discussion is offered of the United States (only once, in a footnote on 103, in which the US is considered to show an intermediate pattern).

rise of the European pattern at an undetermined moment before the 1740s. To this pattern belongs a so-called joint household. This is a relatively complex type of household where two or more related couples live together. Marriage did not have to be delayed because the couple moved into an existing household: wealth was not a necessary condition for marriage.

Although rooted in solid empirical data, the European and the non-European household are in a sense ideal types. Hajnal was not really interested in interpreting differences within the two categories; his aim was to establish a major difference *between* the two and above all to point to the uniqueness of the European pattern. With respect to marriage patterns Hajnal pointed to some deviations from the patterns, but he interpreted these as intermediate cases between the European and the non-European patterns, not as patterns that were different altogether. In his second article he also allowed for more irregularity in the patterns of household formation, but his central argument was again the uniqueness of the European pattern.

Given the nature of Hajnal's model, the two different patterns can easily be summarized as follows:[14]

	Pattern of Marriage (1965)	Household (1982)
'European' pattern • pre-1740 to 1940s • W. of line Trieste - Petersburg	Late • late male marriage • late female marriage • a significant minority never marries	Simple (nuclear) • only one married couple • couple in charge of own household
'Non-European' model • all other periods • everywhere else	Early • early male marriage • early female marriage • universal marriage	Joint (extended) • Two or more related married couples • Newly married couple starts in a household in which an elder couple is in charge

Hajnal did not confine himself to establish the patterns in marriage and household formation, but also discussed their implications. Both articles yield a rich harvest of remarks on social and economic consequences of the uniqueness of the European family.[15] Hajnal offered also some discussion of issues relating to migration, though he certainly offered no complete treatment of the subject. It is in fact remarkable that Hajnal does not discuss the mobility of

[14] In the interest of clarity my summary leaves out some of the details mentioned by Hajnal. In the case of household typology, the terms 'nuclear' and 'extended' have not been used by Hajnal, but are used by Saller and Shaw and subsequently by other ancient historians - see for details the next section.

[15] Hajnal (1965) 130-135.

the relatively high proportion of people that do not marry at all. For Hajnal, the crucial group was formed by the persons aged 15 to 30 not yet married. These men and women formed a labour force of people at the peak of their physical capacity but not yet caught in a web of familial obligations. They were therefore able to migrate, and this gave rise to a system of temporal migration in which young men and women moved out to work for a significant period in their lives.

Hajnal emphasised that the European marriage pattern was not dependent on industrialisation or urbanisation. In fact, the pattern pre-dated these processes, and also occurred in countries where industrialisation or urbanisation were slow. The rise of the European marriage pattern could therefore not be the result of either process.[16] Hajnal went so far as to reverse the relation: late marriage created, especially in the case of women, a labour force at the height of its productive capacity without responsibility for children. There was a possibility to save and to acquire wealth and this in turn led to greater income equality. Hajnal went further: '[c]ould this effect, which was uniquely European, help to explain how the ground-work was laid for the uniquely European "take-off" into modern economic growth?'[17] The suggestion is extremely bold: marriage patterns are used to explain the rise of the West. I do not claim detailed knowledge about the historiography of the Industrial Revolution, but I remain sceptical.

Hajnal thus divided families into two types. The one type, found everywhere except in modern Europe, consisted of complex families of the 'joint' type where marriage occurred early and was universal. The other type, found exclusively in modern Europe, consisted of nuclear households and was characterised by late marriage. It was this late marriage that gave rise to a system of temporary migration, in which young men and women moved for a short period of their life to work elsewhere. In his typology, it was the European model of later marriage and nuclear families that formed a stimulus for migration.

2. The Roman Household: Size and Composition

If Hajnal's typology is accepted, it is very important to know how the Roman family fits in. According to Hajnal's reasoning, the European family was a modern occurrence. Somewhere before the 1740s it came into existence. The logical corollary is that the Roman family should

[16] Hajnal (1965) 106-113.
[17] Hajnal (1965) 132.

have belonged to the joint type. If so, the possibilities for migration must have been severely limited. But should we regard the Roman family as joint?

Over the past two decades, there has been a fierce debate about the composition of the Roman family. Although this debate has used terminology that is slightly different from Hajnal's ('nuclear' and 'extended' instead of Hajnal's 'simple' and 'joint'), the underlying typology is very similar – not surprising given the fact that they were formulated in direct response to the issues brought up by family historians of later periods.

It has long been thought that the Roman family was an extended, patriarchal, multi-generational and relatively large family - a 'joint' one, in Hajnal's terms. The eldest living male, the *pater familias*, exercised virtually unrestricted *patria potestas* over his sons, their wives (if married in a *manus* marriage), and their children. If a son married, he brought his wife into his father's household. In consequence, multiple sons lived (married or unmarried) next to each other in the same household, together with their children. The emblematic figure of the extended Roman family is the paternal grandfather, presiding over his sons, their wives and his grandsons and exercising his *ius vitae necisque* ('power over life and death') with - one would hope - restraint.

The extended family or, as it also termed, the joint agnatic family, clearly belongs to Hajnal's joint families, and it is obvious that it stands in clear contrast to Hajnal's simple family of modern Europe. As we have seen, Hajnal did not think in terms of evolution, but distinguished between two types, one of which occurred at a specific moment in history. However, the idea that the Roman family was of the extended type usually formed part of an argument about historical development in family forms. The development has been sketched in different ways. Initially, there was the quest for an Indo-European prototype of the family. The differences between Greek and Roman family forms were to be seen as permutations of a general *ur*-type of the Indo-European family. It was the task of what was termed comparative sociology to reconstruct this *ur*-type, and to explain the variety of forms that occurred in historical times. The research was backwards: it focussed on a reconstruction of the Archaic Roman family, and from there back to the Indo-European one.[18] In more forward looking approaches, there was a focus on development within the Roman period. In its crudest form – and directly dependent on statements of Roman authors – the development was presented as a form of moral decline. As time progressed, more and more wealth entered Roman society

[18] Crook (1967). See also Dixon (1988) and (1992). Dixon (1985) 355 points out that one of the side-effects of the fact that this approach fell from favour is that the anthropological perspective on the Roman family was lost.

through conquest, and increased wealth led to moral decay. Adultery became a serious problem, and this led to a higher incidence of divorces, a decline of family values, the rise of serial polygamy, and a drop in fertility rates.[19] Stripped from its moralizing overtones, scholars have pointed to the increasing complexity of family life: to the rise of *sine manu* marriage (a type of marriage in which the women remained under the control of her father),[20] to the high incidence of divorce and remarriage, and to a supposed rise in affectionate relationships, both between husband and wife, and between parents and their children. Although these are slow processes that are by definition hard to date, the general belief seems to be that most of the major changes occurred before the onset of the Principate. Somewhat surprisingly, no modern scholar of the Roman family has attempted to offer a coherent theory of change in which all these developments are connected with each other. The discussion usually focuses on the period of the Principate itself.

Although the idea that Roman family was of the extended type is under strong attack, it is important to emphasise that these ideas derive from the Romans themselves – they are certainly not a modern construct. Both Latin words that were most used to describe the ancient family, *familia* and *domus*, had a wide range of meanings. This is in itself easy to understand as the English word 'family' in contemporary speech also has a wide range of meanings. But it is striking that in the Roman period it never referred to the simple, nuclear family of husband, wife and children.[21] In fact, the Romans lacked a concept to describe such nuclear families.[22] One could perhaps – though this is much more open to debate – also point to the fact that stories circulating in the Principate about *patria potestas* and the use of the *ius*

---

[19] Such a model governs for example the discussion of the evolution of the Roman family by Dionysios of Halikarnassos, *Roman Antiquities* 2.24-27. For some general comments see Saller and Shaw (1984) 124, Dixon (1985) 358, Rawson (1986) and Dixon (1988) 17. I note in passing that the moral decay was believed to be the consequence of the import of large numbers of slaves from the East, whose moral degeneracy corrupted the otherwise virtuous Roman minds. This is a type of relationship between migration and family-life that I do not want to explore here (or anywhere else).

[20] *Sine manu* marriage seems to have become the norm around mid-first century BC. But note that *manus* marriages still occur in the *Laudatio Turiae* of ca 9 B.C.; cf. De Ligt (2001) for some explanation. De Ligt (2001) 47, 55 also points to the fact that marriage and transfer of *manus* need not occur at the same moment. In consequence, a marriage could be changed from *manus* to *sine manu* marriage and v.v. This incidentally also happens in the *Laudatio Turiae*; for reasons that have do with property transmission. By Tiberius' reign it was hard to find a patrician with *manus* marriage - see Tac., *Ann.* 4.16.3. The (by and large unknown) reasons for the shift are discussed by Dixon (1985) 354-355 and 362, who points to a simultaneous rise of conjugal love and 'nuclear identification'.

[21] For the various meanings of *familia*, the *locus classicus* is Ulpian, *Dig.* 50.16.195. Given the length of the text and the range of meanings discussed, the absence of the nuclear family is remarkable indeed.

[22] Saller and Shaw (1984) 124: 'The Romans had no word whose primary meaning was the mother-father-children triad'.

vitae necisque suggest that the concept of patriarchy was very much alive. This concept of patriarchy is usually associated with that of the extended family.[23]

However, the current consensus is otherwise. One of the central contentions in the debate on the nature of the Roman family has been that – in contradiction to what Hajnal predicted – the Roman family was of the nuclear type, just as the modern European one. It consisted of a husband, his wife, and their children. This radically different idea was most forcefully formulated by Saller and Shaw on the basis of a massive study of Latin epitaphs published in 1984, though parts of the attack on the arguments in favour of the extended family were formulated already earlier.[24]

Saller and Shaw studied the family relations in a sample of over 10,000 Latin epitaphs. These inscriptions consist of two types. Many people set up an inscription for the deceased at burial, and often they recorded on such inscriptions not only the name of the deceased, but also of the persons responsible for the erection of the inscription. There is also a second type of inscription, which was erected during the lifetime of the person by him- or herself. In such inscriptions there are often provisions made for the persons that are also to be buried in the same grave. Both types of inscriptions therefore give a sketch of the family of the deceased.

Saller and Shaw analysed statistically the commemorative activity recorded on both types of inscriptions: who was erecting the inscription, and for whom. It is from these relationships that Saller and Shaw tried to determine the character of the family. They emphasised that it was not a single mechanism that governed commemorations, rather, there were three factors at work: heirship (entering upon an inheritance might entail the responsibility for the burial), a sense of duty, or affection. There existed, in other words, more than one reason for setting up an epitaph, and precisely because of that, commemorative activity provides an insight into family relations. Saller and Shaw counted all the individual relationships recorded in the epitaphs and classified them according to type. Those of the inner circle of the family (e.g. a husband to his wife) belonged to the nuclear category, the extended category comprises family members at a further remove (e.g. a maternal

[23] I am concerned here with the existence of a discourse about patriarchy, not with the historicity of the stories in which the *ius vitae necisque* is supposedly invoked, which is (rightly to my mind) under strong attack. For discussion, see Harris (1986) and Shaw (2001). I do however not believe that the argument should be pressed - see below in this section.

[24] Saller and Shaw (1984). Predecessors are mentioned at n.51 and by Dixon (1992) 4. Martin (1996) 40 with n.1 and Gallivan and Wilkins (1997) 240 n.3 for further references to approval of Saller and Shaw's thesis. In what follows I concentrate on the free civilian population and ignore the discussion of regional variations within Saller and Shaw's data. These are in any case immaterial to their central argument, see e.g. 136 where they say that in *all* civilian data sets nuclear relationships are preponderant.

grandmother), and then there are commemorations made by outsiders (e.g. dedications by a friend).

In the thousands of relationships that Saller and Shaw recorded, there turned out to be a very heavy emphasis on nuclear family relationships. Everywhere we see wives setting up an epitaph for their husbands, fathers setting up inscriptions for their children, children setting up inscriptions for their parents. By contrast, dedications in which siblings or other members of the extended family occur are rare. In fact, in the absence of nuclear family relations we see that others, from outside the family, were preferred over members of the extended family. The paternal grandfather – *the* symbol of the extended family – is conspicuously absent in the epitaphs.[25]

At first sight the study of Saller and Shaw is very impressive. The sheer size of the data is daunting: thousands and thousands of inscriptions have been analysed and the results have presented in a great number of tables. The results are also very consistent: in different geographical areas the proportions of nuclear versus extended relationships may vary, but everywhere nuclear relationships are in the majority. Extended family relations are conspicuously absent from the epitaphs. The conclusion seems inescapable: we should revise our thoughts on the Roman family and start seeing them as predominantly consisting of father, mother, and children.

But on second thought the results of their investigation pose a serious problem. *Roman* ideas about the composition of their own families were very different. Romans seem to have believed that theirs was an extended family. The lack of a Latin word to describe the concept of the nuclear family is disturbing if we assume that precisely that was the dominant Roman family form.

Saller and Shaw occasionally pointed to the problem, but offered no coherent explanation. Their solution was of a different type. Saller and Shaw at the end of their article tentatively but boldly suggest that there may have existed continuity in family forms from ancient times to the modern ones.[26] In fact, their whole scholarly oeuvre on the Roman family can be read as one protracted attempt to modernise the Roman family. Although they never say as much, their version of the Roman family is closely similar to Hajnal's European family type. Phrased in Hajnal's typology, Saller and Shaw have consistently attempted to remove the Roman family from the non-European type into the European type.

[25] Saller and Shaw (1984) 136, emphasising further that there are even much less paternal grandfathers than predicted by demography and that even in dedications in which grandparents figure, the paternal *avus* is underrepresented.

[26] Saller and Shaw (1984) 145.

Such an attempt is of course not really an answer to the question what we should do with Roman ideas about the extended family. How should the inconsistency be explained?

There are several possibilities. We might assume that they are the product of a difference between ideal and reality. Such discrepancies are certainly observable elsewhere in Roman family history: there is for example a marked difference between the ideal of family continuity on the one hand and the discontinuity that occurred in practice on the other hand.[27] But in that case the ideal was a function of a different reality: precisely *because* discontinuity occurred so frequently, continuity was valued so highly. In the case of family types it is more difficult to explain what the function of a supposed ideal of an extended family would be.

We might also explain the discrepancy by relegating the extended family to the parts of the population not covered by the inscriptions of Saller and Shaw. This might take two forms: a chronological and a geographical one. We may assume that there was a historical development from the extended patriarchal family in the Republic to the nuclear family of the Principate as witnessed by Saller and Shaw's inscriptions. Or we may assume that the extended family occurred in the rural parts of the empire not covered by the epitaphs. Both possibilities are in fact considered by Saller and Shaw, though in passing only.[28]

If we would have the same sources as we have for the more urbanised areas of the Principate for the Republic or for the rural areas of the Principate, would the results be radically different from those found by Saller and Shaw? Before trying to explain the difference, we should first discuss whether Saller and Shaw's findings stand up to closer scrutiny. There are in fact many problems with Saller and Shaw's analysis, and these deserve close attention.

One relatively minor point of critique concerns the focus of the analysis. Bradley argued that the analysis was one-sided, as it focussed on one particular moment in the family cycle: the moment a member died.[29] Although this is in itself a correct observation, it can hardly constitute a serious point of critique. It should not be forgotten that death was all-around in the Roman world. Epitaphs are bound to be erected at very different moments in a

---

[27] See my own Tacoma (2006) Part 2 on discontinuity among the urban elites of third-century Roman Egypt.

[28] Saller and Shaw (1984) at 137 with n.53, suddenly (and quite unexpectedly) speak of 'the tension in the late Republic between the old values associated with the patriarchal household and the new values' that came along with the nuclear family. At 145 they suggest that the extended family may have prevailed among unromanised rural or tribal societies. For the latter suggestion see also Dixon (1988) 14: 'It need not follow that these practices [here: marriage patterns] have any bearing on the customs of the poorer classes in the city or the countryside. To say that the nuclear family was the norm does not mean that it was the only residential form'.

[29] Bradley (1991).

family's life cycle. It would be more to the point to say that the analysis of family life focuses on a peculiar moment: precisely at a moment in which the family could be dissolved or reconstituted.

A more important question to address is what governs acts of commemoration, and what commemorative acts tell about family structure. To begin with, it seems sensible to suppose that epitaphs would be set up by the *nearest* kin available, no matter what the family looked like. The fact that paternal grandfathers do not occur very often in the evidence would then be explained by the fact that they will not act as commemorators in cases where a father is available.[30] If the idea that the nearest kin is most likely to take up the responsibility for the erection of an inscription is correct, members of the nuclear family will automatically dominate the inscriptions. If relationships are counted, the number of nuclear relationships will be always higher than the number of relationships at a further remove. This effect occurs independent of family type, and the corollary is that a statistical analysis cannot be used to discover the character of the family.

But one might go further. In what constitutes the strongest attack on Saller and Shaw to date, Martin has shown that the statistical methodology employed by Saller and Shaw is highly questionable.[31] The problem is the way the relationships are counted. Saller and Shaw do not analyse family types by inscription, but by type of relation within the inscription. Each type of relation is counted separately: a husband - wife relation is counted as one nuclear relationship, father - children as one, and so on. As the number of nuclear relationships in the inscription is almost always much larger than the number of extended relationships, this results almost automatically in a numerical preponderance of nuclear relationships, no matter what type of family is analysed. In fact, even a classic example of an extended patriarchal family would in their count lead to a dominance of nuclear relationships!

It is, as Martin shows, inscriptions as a whole, not relationships within the inscriptions, that should be analysed. The question that should be addressed is: what family types can we see in the inscriptions, not: what type of relations can we see within the inscription? But Martin's arguments creates a new problem. If we want to analyse inscriptions as a whole, we should be certain that the inscriptions show complete families. However, there is no reason to assume that this is always the case. What is worse, we normally lack the instruments to judge the question whether families are complete. Our only way to judge completeness is the size of

[30] This argument admittedly applies to the *post-mortem* inscriptions only, not to the inscriptions set up during the life-time.

[31] Martin (1996), esp. 43-45.

families: only those families that are large stand a good chance of being complete. 'We must use those inscriptions that seem to include all those persons considered to be members of the family', writes Martin, but how can completeness be judged other than be size?[32] Although Martin is right to point out that extended families need not be large by definition,[33] it is hard to see how an analysis of inscriptions with large families will not result in a preponderance of the extended family type.

It is therefore difficult to think of a way in which the epitaphs can be used to determine the question whether the family was of the nuclear or of the extended type. Saller and Shaw's method of counting individual relationships leads automatically to a preponderance of nuclear relationships, and hence, to the idea that the Roman family was essentially a nuclear family. Martin's argument that we should analyse families as a whole on the basis of those inscriptions that seem to present complete families will produce a strong bias towards the opposite conclusion. Neither method therefore works.

What does this mean for the debate about the Roman family? It is important to realise that Saller and Shaw's method may have failed, but that their conclusions need not necessarily have been wrong. It should not be forgotten that their conclusion was met with approval by many historians of the ancient family, who thought that Saller and Shaw confirmed what they had already suspected for some time. Moreover, Martin himself certainly does not advocate the opposite conclusion that families were of the extended type. He rather pleads for a more complex typology. The question to what type the Roman family belong remains therefore unresolved.

One possibility is to refine our typology. Martin rightly argues that we should allow for more complexity, but he is not very specific on the analytical tools that should be employed in order to overcome the limitations of current approaches.[34] The suggestion therefore remains somewhat unsatisfactory, for we have to find words and categories to analyse the Roman family if we want to do social history at all. Before proceeding any further, I seems to me more fruitful to decide what our categories mean.

In the end, we should not so much question the Saller and Shaw's method but rather their questions. What exactly is meant by 'extended' and by 'nuclear' families? What does the

---

[32] Martin (1986) 51. One possible other solution could be to limit the analysis to ante-mortem inscriptions only. But these have not been separately analysed by either Saller and Shaw or Martin, and the proportion of ante-mortem inscriptions remains unclear. Note that Saller and Shaw(1984) 126, 132 point out that as a rule ante-mortem inscriptions show a much wider net of relations.

[33] Martin (1986); see also for more general observations Hajnal (1982) 449.

[34] Martin (1996) 57-58.

difference between the two exactly mean, and how can it be analysed? It is in fact remarkable that so little thought has been devoted to the definition of the two major concepts used in the debate.

The definition of 'nuclear' is relatively unproblematic. The simple family consists of the parents and the children. Bradley has warned that we should not suppose that 'nuclear' also automatically means 'affectionate'. We should not be led astray by modern analogies.[35] But this is problem of a different order.

Sometimes, extended is taken as 'large', and 'nuclear' by implication as small. Although this is likely to be correct in many cases, Martin is right to point out that size is not the defining criterion.[36] It is structure, not size, that matters. A family consisting of father, mother and 13 children is still a nuclear family, despite its large size.

The idea that size functions as a criterion might be wrong, but it leads to a different problem that is worth considering. In one sense the Roman family *was* large, because of the presence of slaves. How much slaves were used in the lower strata of society is open to debate, but that slaves were ubiquitous in the households of the wealthier parts of the population is without doubt. In Roman Egypt outside of Alexandria, slaves made up just over 10% of the population. Among the urban population of the towns that dotted the Egyptian landscape, ownership of slaves seems to have been more widespread than ownership of land.[37]

In the discussion about family typology, slaves have been consistently excluded from consideration.[38] Although this exclusion has not been discussed, it must be based on the idea that slaves did not really belong to the family. Their position is in that sense comparable to that of the servants of Early Modern Europe. In order to enable comparisons between Roman and later times, the slaves have been left out, just as the servants have been left out of consideration. However, this is only justified up to a point. Economically, the position of slaves was of course different from that of servants – though it need to be realised that the fact that a servant's labour was of a temporary nature might equally have applied to slaves, who were after all frequently manumitted. Slaves had a family life of their own, sometimes within, sometimes partly outside the household of their master.[39] But so did servants. However, the crucial difference between servants and slaves is that slaves in more than one sense really entered the Roman family. It is well-known that funeral monuments often allowed the

[35] Bradley (1991) 4 and passim.
[36] Martin (1996) 46-47.
[37] Tacoma (forthcoming) section 3.
[38] A notable exception is Bradley (1991), but his emphasis is somewhat different.
[39] Rawson (1986) 23-24, 37, Dixon (1988) 17 with further refs.

inclusion of slaves and freedmen. It can also be hardly coincidence that in some of the definitions of the Latin word *familia* slaves were included. More importantly, precisely at the moment of their manumission slaves came to belong to the family, through the fact that they were given the family name. In the absence of children, the family line was in some cases continued through former-slaves, and former slaves sometimes shared in the inheritance - or in what presumably constituted a small minority of cases, even inherited everything.[40] By taking over modern categorisations it seems as if Roman social historians have sacrificed an important aspect of the Roman family. The discussion is in that sense somewhat artificial. But as long as the unique position of slaves is remembered, there is no fundamental objection to continue the comparison.

Should we see the extended family primarily as a multi-generational unit? The extended Roman family is sometimes referred to as a 'three-generation' group, with the grandfather as *pater familias*. Taking the three-generation definition as a starting point, some have argued that demography shows that it is very rare that grandparents and grandchildren were alive simultaneously. Levels of mortality were too high. If this was the case the Roman family is unlikely to have been extended. However, even aside from the question whether 'extended' primarily means 'three generations', the demographic argument cannot be completely right. What demography primarily shows is the random character of mortality. Mortality produced gaps in the stemmata. The high mortality of the Roman world does not in itself lead to two- instead of three-generation families, but rather to families with missing members. There are in fact many occurrences in our evidence of grandfathers without living sons but with living grandsons.

There is also yet another perspective possible. Saller and Shaw (and many others in their wake) seem to operate from the assumption that 'extended' means first and foremost 'patriarchal'. Much of the work on the Roman family (not only on the Roman fathers but also on the Roman mother) has been devoted to a careful dismantling of the concept of strong patriarchy. However, to my mind too much focus on the figure of the *pater familias* has obscured many of the issues. It is as if scholars have been infected with the Roman obsession with the *pater familias* and his *patria potestas*. It has not well been realised that a concept of strong patriarchy can operate independent of family type. The powers of a *pater familias* will obviously in practice have been larger if the persons under his *patria potestas* were within his reach, but there is no reason - legal or social - why his powers would be really diminished if

[40] See e.g. Rawson (1986) 12-13.

this was not the case. If the concept of strong patriarchy can operate independent of family type, the logical corollary is that it cannot be used to make a distinction between the two.

A quick glance at Hajnal's typology shows that the crucial difference between the extended and the nuclear family should be located somewhere else.[41] The crucial difference between the two types is what happens when a son marries. In the extended family model, the son brings his wife into the household of his father. Upon the death of the father, the household is dissolved and the sons set up new ones. In the nuclear model, marriage is neo-local. The son sets up a separate household and leaves the household *before* his father's death. In the extended model, it is in other words the death of the father that forms the crucial moment in household formation, in the nuclear model, it is the marriage of the son. From this difference all other characteristics follow. In the extended family, the newly-wed couple might produce children, which then turn the *pater familias* into the grandfather famous from the concept of *patria potestas*. In cases where there are multiple sons, they all bring their wives into their father's household, and continue to live as siblings together. What happens to a son upon marriage is therefore the determining factor in the analysis of family type.

There can be no doubt that marriage was viri-local: the wife moved to the husband's home. Individually attested marriages show as much. The transfer of *manus* or the lack of it in *manus* and *sine manu* marriage respectively only make sense if marriage was viri-local. The ceremonies with which a marriage was celebrated suggest the same. The central element was a torch-lit procession in which the bride was led to her new home.[42] Marriage was supposed to begin when the wife had been led to the home of her husband, when she was *in domum deducta*.[43] The crucial question is then whether this 'home' was that of himself or that of his father.

The question has not had the attention it deserves. The few statements that can be found in the secondary literature offer differing opinions. Saller and Shaw quite unsurprisingly operate from the idea that marriage was neo-local, and they cite some examples which seem to corroborate the practice.[44] Dixon and Rawson assumed likewise,

[41] See also Hajnal (1982) 462f.
[42] For which see Williams (1958).
[43] Gardner (1986) 37, 44, 47.
[44] Saller and Shaw (1984) 137.

though offered no serious discussion.[45] Hopkins, however, in his reconstruction of an ancient marriage procession, has the procession ending at the father's house, not at a new one.[46]

The reason that the question whether sons set up a separate household upon marriage or when their father died is hardly ever discussed must be that the evidence to answer this seemingly simple question is not exactly abundant. Actual examples of sons setting up their own household are only sparsely recorded in the literary evidence and give conflicting impressions. There are certainly cases where a son sets up his own household, but there are also others in which the son remained in his original home.[47] Moreover, literary sources often seem to prefer the remarkable over the usual. Furthermore, it is difficult to see how the epitaphs can answer that question (no matter the way they are analysed). Would a son who had set up his own household not still commemorate his deceased father? Matters were further complicated by adoption. Among the senatorial elite the possibility existed for a father who had daughters only to adopt his son in law.[48] We can only wonder where such a couple would live.

The question to what extent sons already gained legal and financial independence before the death of their fathers through emancipation is surely also of relevance. *Emancipatio* was the act by which a son became *sui uiris* (legally independent, and hence capable of owning property) before the person under whose *patria potestas* he was died. *Emancipatio* might not have been absolutely necessary to set up an independent household, but it certainly will have helped to create favourable conditions for it. But although it is likely

[45] Dixon (1985); Rawson (1986) 14, who seems to suggest likewise that married sons set up their own household out during the lifetime of their fathers; Dixon (1988) 14 (normally sons left home upon marriage around age 18), 27 (the neo-local character of marriage helped to mitigate the potential severity of *patria potestas*).

[46] Hopkins (1999) 29-38, but Hopkins made his own sources.

[47] In my cursory reading I noted the following cases. Not all of them pertain to *married* sons.*Laudatio Turiae* 1.36, cf. 2.9a: 'Turia' lives with husband's mother and waits there for her husband's return, who is in the province Macedonia. Afterwards it seems likely (but cannot be ascertained) that they live in their own household. Cic., *Ad Att.* 12.32.2; 12.7, 15.20.4 and *Pro Caelio* 18 cited by Saller and Shaw (1984) 137 and Rawson (1986) 17: Cicero's son went off to Athens, but the alternative was to set up his own household, as Caelius had done. Cf. also Cic. *Pro Roscio* 43: the wealthy send out sons to manage part of their properties elsewhere, though this is better testimony to the fact that among the elite house and household are not co-terminous. In Seneca, *Cons. ad Marciam* 24.1 and Pliny, *Ep.* 9.9.2 sons are praised for staying in their own household – according to Saller and Shaw (1984) 137 exceptions that prove the rule. Val.Max. 4.4.9 and Plut. *Aem.P.* 5: the sons of the Aelii Tuberones of the second cent. B.C. continued to live at home after marriage; in consequence several married sons lived with their wives and children under the same roof as their father. This is described by Dixon (1988) 13-14 as a 'quaint and archaic system'. In Plut. *Crass.* 1 (cited by Dixon (1988) 14) the brothers Licinii Crassi lived after marriage in the parental home. Juv., *Sat.* 14.220-221 (cited by Dixon (1985) 369 n.44) clearly suggests in-living daughters in law.

[48] Corbier (1991) 67, 70-73; such adoptions required the emancipation of the daughter to avoid an incestuous union.

that *emancipatio* frequently occurred, we hardly ever know at which point in life, and with which frequency. Studying *emancipatio* therefore helps less than one might think.

A consideration that might offer support for the idea that sons remained living with their parents is that children were supposed to care for their parents. As in many societies, children were valued because they offered security in old age. This idea might imply that children remained living in their parent's household, or, conversely, that elder people moved into the new households of their sons. The problem, however, is that such behaviour is not the automatic result of a moral imperative to care for the elderly. The idea that children had obligations towards the parents is not in doubt, but the question what that implies for household formation remains open.

We then seem to end in a blind alley. According to Hajnal, the nuclear, simple family was a type that remained confined to modern European history. This idea seemed initially to fit very well with the Roman evidence, for that suggested extended, patriarchal families, not simple family forms. However, Saller and Shaw argued that in contradiction to the Hajnal hypothesis, the Roman family was nuclear.The problem is that their thesis cannot be proven in the way they did, and that other ways to analyse their data lead into blind alleys as well. Ultimately, the crucial problem turns out to be the position of sons. Would they leave the household upon marriage, or wait till it was dissolved by the dead of their father? This simple question turns out to be difficult to answer on the basis of the available evidence. The sources are hardly abundant, and further considerations lead in different directions. Before we give up our query, we might think of different ways to answer the question. An indirect answer, I would suggest, is to be found in Roman marriage patterns.

3. Roman Marriage Patterns

In Hajnal's model, it is not so much household types but marriage patterns that are important for migration. In Hajnal's European model, the fact that men and women marry late results in labour force consisting of temporary migrants. According to Hajnal's hypothesis, this pattern was unique for the modern European world. The implication is that the Roman world followed the pattern of early marriage, and as much is stated by Hajnal himself, who devoted a significant part of his first article to the ancient world. However, as we have seen in the case of household typology, Hajnal's assumptions about the Roman world should not be taken for

granted. In the case of marriage patterns, it turns out that there are very important amendments to be made to Hajnal's views.

The age at which Romans married has attracted lots of scholarly attention. Several publications are devoted partly or wholly to the topic. Discussions usually have focussed on women's age at marriage. The initial attention was generated by moral shock –the evidence for very early marriage was on the border of what was thought acceptable for civilized societies. Later discussions were written from the perspective of demography. Marriage itself is demographically not very interesting, but marriage has for obvious reasons a close connection to fertility and – something not to be neglected – it is relatively easy to measure. For the same moral and demographic reasons male marriage is apparently considered to be less interesting; it has been studied much less. Perhaps in consequence there are only short statements about the question how the Roman marriage patterns fit into wider historical patterns. Although Hajnal's work is sometimes cited by ancient historians, one wonders how often it is read. There is no real discussion by ancient historians of Hajnal's treatment of the ancient evidence.

As Hajnal already noted with some surprise, there is quite a lot of evidence for Roman ages of marriage. There are statements about the age at puberty by medical writers; there are legal sources about the age from which marriage was allowed; the Augustan marriage laws mention ages for marriage; there are generalising comments by ancient authors about the ages at which people usually marry; marriage contracts surviving on papyrus; epitaphs indicating marital status; evidence on individual marriages preserved in ancient authors, and so on.

For obvious reasons, an analysis of ages at marriage should focus on patterns, not on individual instances of marriages. Aggregate data-sets are therefore needed. For the purpose of this article, there is only one type of evidence that is really relevant: that from inscriptions. Individual marriages attested by literary sources are interesting, but even apart from the fact that they may focus on the exceptional rather than on the regular situations, the principal objection is obvious: the evidence pertains usually to the senatorial aristocracy.[49]

There exist Latin inscriptions in which the age at marriage can be calculated because both length of marriage and age at death are given. This type of inscription has been found

[49] The evidence is now systematically collected and analysed by Lelis, Percy and Verstraete (2003). The major advantage of the data is that it offers more or less precise evidence on age at *first* marriage. Their work raises the methodologically interesting question how the boundary should be drawn between statistical data and anecdotal analysis. Note in this context also that there are some cases of a match of literary with epigraphic evidence – see Friedländer (1919) 271 on Pl., *Ep.* 5.16.2, matched by *C.I.L.* 6.16631 = *I.L.S.* 1030, concerning a girl who died just before marriage at 12.

mostly in the city of Rome, but it is occasionally also found elsewhere, though hardly outside Italy. A systematic collection of these inscriptions was assembled by Harkness in 1898.[50] The material was again analysed by Hopkins in 1964/5 in what is generally regarded as a ground-braking article. He confined himself to Rome, whose epitaphs he recollected, to the exclusion of all other material. In order to enable comparisons, he added Christian material of somewhat later date also from the city of Rome, and separated this Christian material from the rest of the list. Hopkins presented the evidence for 144 pagan women and 86 pagan men in the form of a table. He focussed in his discussion on the marital age of women, for which he also produced a graph.[51] His main contention was that female marriage occurred early. Many girls were already married before the age of 15. His data are summarised in the first two rows of table 1.

age	0-4	5-9	10-14	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	55-59	60-64	total
Rome[52]	0	2	61	48	21	7	7	0	0	1	0	0	0	147
outside Rome[53]	0	1	34	25	6	2	4	2	0	0	0	0	0	74

Table 1. Marital ages of women

[50] Harkness (1896) at 35-51, with Hopkins (1964/5) 309 n.7 for the *CIL* volumes used by Harkness. The statement by Shaw (1987) 32 that Harkness relied partially on earlier work of Pelka is not corroborated by Harkness' article and cannot be right, because Pelka's work concerned the Christian epitaphs. The first collection of such evidence – at least for women - was presented by Bang in an appendix to Friedländer's *Sittengeschichte*. For some discussion of the variations between the several editions, see Hopkins (1964/1965) 4. Bang presented a list focusing on ages 12 to 18, but also gave references to ages above 18. The list presented 104 cases. How the list was composed was not stated, but it seems to have been more or less randomly compiled. Cf. also the remark Friedländer (1919[9]) vol. IV, 136: 'Es ist kein Grund anzunehmen, dass eine grosse Vermehrung dieser Sammlung wesentlich andere Altersverhältnisse ergeben würde'. It did present also some references outside *CIL* and included at least one Greek inscription. The critique of Harkness (1896) on the unsystematic character of their work was not taken into account by Friedländer or Bang in subsequent editions. See e.g. Friedländer (1919[9]) vol. I, 270-273; vol. IV, 133-141. Independent of Harkness, Macdonnell (1913) studied the marital ages of the city of Rome. He found 59 women with marital ages running from 7 to 56 and 29 men with marital ages running from 15 to 63. No references were given. For women a table was presented, for men only a description. As about three-quarters of Harkness' evidence concerned Rome, the only conclusion possible is that Macdonnell missed an enormous amount of information. Macdonnell's work is only cited by Hajnal (1965), not by Hopkins, and – hence? – has subsequently been lost again.

[51] Unfortunately Hopkins did not give references, with the consequence that anyone who wants to check the data is forced to use the incomplete list published by Harkness (1896).

[52] Data taken over from Hopkins (1964/5), pagan inscriptions. Hopkins' subtracted from his total the two first and the last ages. In his table, the total should read 144, not 145.

[53] Computed from the list of Harkness (1898) 40-45, ignoring Roman and/or Christian epitaphs.

There are some problems with the analysis of the Hopkins/Harkness dataset, but not in the direction where they are usually sought. Shaw has raised the objection that the data are in two respects not representative.[54] In the first place, they are geographically confined to Rome, and therefore may not represent marital practices in the rest of the empire. However, the data from outside Rome that Harkness assembled but Hopkins deliberately left out of his analysis do not offer a substantially different picture, as one can quickly see in the third row of the table above, in which I have summarised Harkness' data. In the second place, there is according to Shaw also a social bias. In his view, the Harkness/Hopkins dataset pertains to senators and their dependent slaves and ex-slaves, who imitated senatorial marital behaviour. Shaw did not corroborate his claim on the basis of a discussion of the evidence, and the suggestion is in fact highly implausible. The inscriptions were erected by the normal inscription-erecting population. In Rome, this population may have comprised both senators and freedmen (slaves are less likely, given the fact that they technically could not marry), but these certainly did not form the dominant groups. Furthermore, it is difficult to see why the servile population would imitate aristocratic marital patterns.

The problems should be sought elsewhere. Hopkins' article is rightly acknowledged as very important in the development of the study of Roman demography, but his treatment of the evidence warrants caution. I hasten to add that Hopkins has pointed to some of the issues raised here himself, but it is usually his conclusion that is cited, not the warnings with which it was surrounded.

The major problem is the data massaging that has been taken place. In the first place, a problem that has not often been noticed is that the evidence for *very* early marriage has been discarded. Hopkins followed previous scholars, who ignored ages at marriage below 10 on the basis of the argument that scribal errors must have been at work.[55]. Errors certainly cannot be ruled out, all the more so because the information about marital age is obtained in an indirect way, through subtraction of the length of a marriage from the age at death. Inscriptions therefore contained two figures, both of which could be wrong, and they were not meant to provide the information that *we* find interesting. But this problem applies not to any specific age category. The methodological problem is that discarding the very early ages of marriage is based on assumptions about historical reality that are precisely at issue. Phrased differently,

[54] Shaw (1987).

[55] Harkness cited a number of inscriptions for girls, and one for a boy, but suggests there are more of them. Harkness (1896) 38, 40 in which he also discarded evidence for early marriage in literary sources. He cites *CIL* IX.1530 (girl at 3), VI.3.21562 (girl at 7), IX.3710 (girl at 7), XIV.3178 (girl at 6), VI.4, fasc I.29544 (boy at 1), X.I.2251 (boy at 11).

on what grounds can we determine where plausibility stops? At 10? At 9? And what of the difference between men and women? Harkness started his female list at 10, but at the same time excluded a male age of marriage of 11. Given the fact that many female marriages were concluded in the ages 10 to 12, there can be little doubt that puberty (usually around 14) was not the determining factor. In fact, this is precisely the core of Hopkins' argument. He even suggested that such pre-puberty marriages were regularly consummated.[56] If Hopkins' argument that women often married before puberty is accepted, it is methodologically untenable to rule out the possibility of very early marriages, and this might have repercussions for the graphs Hopkins presented.

In a sense, the problem that very early ages for marriage have been ignored seems an academic one, as inclusion of such ages will only have strengthened Hopkins' contention that female marriages were concluded early. However, there are also problems at the other end of the range of figures. Some marital ages that are recorded are very high: we find women marrying beyond 20. As Hopkins points out, it is likely that many of such late marriages were not first marriages but remarriages. The problem is that in the great majority of cases it cannot be established whether a marriage was a first marriage or a remarriage. Remarriage is of course dependent on the termination of a first marriage through death of divorce. One need not know much about patterns of mortality to understand that the likelihood of termination increases with age. The later in life, the more often one of the spouses will have died.[57] It is however difficult to separate clearly between first marriages and later marriages in the data. The boundary is fluid: some marriages that seem late might still be first marriages, and some marriages that seem first marriages might be second ones.[58] Hopkins' solution was to take only marriages up to age 24 into account, but this should be regarded as a counsel of despair. Methodologically it is inadmissible, because it is again based on assumptions about historical reality that are precisely at issue. His often cited conclusion that half off all girls were married by the age of 15 (inclusive) was predicated on the assumption that very early marriages and very late marriages should not be taken into account.[59]

Whether we like it or not, we have to realise that the evidence for ages of marriage is simply not what we want it to be. It presents age at marriage, not age at *first* marriage. The

[56] Though see Rawson (1986) 21 for criticism.

[57] Termination of marriage might also have been caused by divorces, which were much more randomly patterned. However, I will argue below that the incidence of divorce among the inscription-erecting population is likely to have been much lower than among the senatorial population.

[58] Harkness notes that one marriage at 20 (*CIL* 5.2.7453) is in fact a remarriage!

[59] Compare in Hopkins (1964/5) 320-321 the graph (N = 130) with the table (N = 145, in fact 144 +3). All statements are based on the former, not the latter.

evidence suggests that remarriage did take place among the inscription-erecting population of Rome - both among men and among women. This means that the evidence cannot be used to determine age at first marriage, and there is no way getting around that obstacle.

Against Hopkins' method, Saller and Shaw have tried an alternative route.[60] They also studied epitaphs, but in quite a different fashion. They used shifts in commemoration to determine age at marriage. From a certain age onwards spouses take over the role of dedicants from the parents, and this point indicates the fact that people started to marry. As the analysis only requires the mentioning of a dedicator and the age of the deceased, Saller and Shaw could use a much larger dataset of epitaphs than Hopkins, with a much larger geographical catchment area. The outcome was different from that of Hopkins. Their main conclusion was that ages at marriage were late, both for women and for men. The implication – not stated but obvious – was that the Roman marital patterns were similar to the modern European ones found by Hajnal.

Saller and Shaw's method is ingenious, but it ultimately only provides vague indications, as 'shifts' are by definition slow processes. And there are some objections. In the first place, it is difficult to see why there could not be simply delays in the shifts. The first years after marriage the attachment to the parents might still be very strong, so in the early years of married life parents might still be inclined to take up commemoration. In the second place, parents might also feel a strong sense of *obligation* to commemorate. Marriages were normally *sine manu* marriages, which meant that the ties of a wife with her parents (esp. her father) were very strong. Likewise men were often still under the *patria potestas* of their father. It is therefore likely that the relatively late shift in commemorative patterns observed by Saller and Shaw is not only a function of marriage, but also of the availability of parents. The relatively late shift in commemoration is therefore likely not to have been produced by marriage alone.

Subsequent debate has focussed on the question whether female marriages were as early as Hopkins has stated. Similarly, although it has been generally accepted that male marriage occurred much later, it is a hotly contested issue what is meant by 'late' in the case of males.

For our purposes, the problem is not so much which method is right. The problem is rather that female and male marital patterns have been studied separately. Male marriage is often completely left out of account. In the case of studies of female marriage, it has not been

[60] Saller (1987), Shaw (1987).

well realised that the question whether female marriage was late (around 18) or early (around 14) is demographically only of limited importance. As has been stated already by Tim Parkin in a casual remark, the effects on fertility are very limited, as it takes several years after the onset of puberty before women are at full reproductive capacity.[61]

The fact that male and female ages of marriage have been studied separately has obscured the more important point that there existed a significant age gap between husband and wife. It is quite remarkable that the age gap between spouses is hardly a secret among ancient historians in general, but that specialist studies on Roman marriage patterns hardly have addressed the issue in a systematic vein. Perceptions may differ radically about what constitutes 'early' in the case of 'early female marriage' and what constitutes 'late' in the case of 'late male marriage', but all the evidence we have points to a substantial difference in the ages at which women married and the ages at which men married. This age gap is of fundamental importance. It is of more importance, I believe, than the by and large unanswerable question whether female age at marriage was 'early' or 'very early'. It is the age gap that is crucial for family formation.

As we have seen, the historical demographer Hajnal distinguished between two patterns of marriage. A European one in which men and women married late, and a non-European pattern in which men and women married early. The central question addressed here is how the Roman pattern fits in. The answer is simple: it does not.

Hajnal thought in fact otherwise. Although it is laudable that Hajnal himself seriously discussed the ancient data on marital age in his own work (how many historical demographers do?), the way he treated the evidence was remarkable, to say the least. Using Harkness' data for female marital ages he categorised the Roman pattern as non-European, because the ages at marriage of women were low.[62] Marital ages of Roman women were low indeed, but the same can hardly be said for Roman males. At the end of his discussion, Hajnal did observe that Egyptian men married substantially later than women, but he did not discuss the possible implications for his classification.[63] I do not know a better example of selective quotation.

---

[61] Though H.W. Singor points out to me that very early female marriage might strongly effect female mortality.

[62] Hajnal (1965) 120-122, concluding that '[t]he population whose deaths the tombstones record had a marriage pattern of 'non-European' type. The same conclusion emerges from an entirely different type of data, the 'censuses' of Roman Egypt'. At 121

[63] Hajnal (1965) 122. Hajnal did not analyse the non-Egyptian Roman data presented by Harkness (1896) for males, although he based his analysis on female marriage precisely on that article. Hajnal incidentally gives Harkness' figures as 171 for women and 191 for men. The latter figure is incorrect – the origin of the mistake remains a mystery, but again the error is indicative of the fact that the male figures were of no interest to Hajnal.

Roman males married relatively late, in a pattern that is similar to their later European counterparts. As there are no methodological grounds for privileging either the evidence for female ages at marriage (as Hajnal did) or the male ages of marriage, the conclusion is inescapable that the Roman marital pattern does not fall into either of Hajnal's categories.[64] This is a remarkable finding, which has not had the attention it deserves.

A reconsideration of the scholarly work on Roman marriage then leads to the conclusion that the focus has been misdirected. Too much attention has been paid to women's age at marriage. A discussion of one type of evidence, that of the inscriptions mentioning length of marriage and age at death, shows that it is not possible to determine how 'early' female marriage was. No one doubts that women married relatively young, but the question at what moment precisely cannot be answered. However, one simple observation has been ignored, and that is that there existed an age gap between husband and wife. Women married relatively early, and men married relatively late. Although this fact is hardly a secret among Roman historians, the implications for the Hajnal hypothesis have not been well realised. There exists a third marital pattern: one of late male and early female marriage.

4. Mortality

What is the relation between the findings on marital patterns and the question whether marriage would be neo-local? Before that question can be answered one further ingredient has to be added to the discussion. In recent years more and more attention has been paid to the consequences of the extremely high levels of mortality postulated for the ancient world. It has become clear that high levels of mortality are relevant for many aspects of social life. This includes obviously also family formation and marriage patterns.

The first consequence of high levels of mortality is fragmentation. Patterns of mortality produce random variation. This applies in the first place to the number of surviving children. There was a high incidence of childlessness, in many cases not because a couple did

[64] One word of caution is in order: I would not be too surprised if a similar pattern would be found in more of the samples Hajnal discusses from other areas and other periods of history. That makes it difficult to assume that the Roman (or better: ancient) pattern is unique. It should also be noted that at several points in his two articles Hajnal states in a matter of fact way that there existed an age gap between men and wife (in both marriage patterns); it is however obvious that he does not attach much importance to it.

not produce children, but because they did not survive.[65] The argument that couples ended up on average with two children is misleading, as the average will often have been far from reality. Many couples ending with a non-average number of children. Roman wills from third-century Egypt display exactly this type of pattern of random variation in the number of surviving heirs.

However, fragmentation does not only apply to the number of children. It went much further. Keith Bradley has in fact formulated a theory that major characteristic of the Roman family was its dislocation. High mortality in combination with many divorces and remarriages produced broken and reconstituted families. In consequence it is not possible to discuss family types, but one should rather focus on the dynamics of family formation.[66] The emblematic figure of this family is not the stern *pater familias*, but the child. The child should be essentially regarded as the victim of the situation: it was reared and educated without much parental interference and it was moved from household to household as new marriages were conducted and old household were broken up.

Bradley's dynamic and chaotic model is certainly worth close attention. There is an abundance of literary sources that seem to confirm Bradley's theory by showing fragmentation of family life. Seneca's mother Helvia can serve as a model: her mother died as she gave birth to her, later an uncle (either of Seneca or of herself) died, then her husband, then three grandchildren.[67] Likewise Pliny's family may be cited. Pliny's third (!) wife was raised by her paternal aunt in her grandfather's home.[68] And so we may add example after example.

But instead of adding examples, we should discuss how to proceed. One type of research focuses on compensatory mechanisms for dislocation. There were mechanisms available to create artificial ties of kinship, like adoption and fosterage.[69] One might also look into the question to what extent slaves through manumission were used to continue the family name. And one may look outside the family, and look for alternative institutions. It has often

[65] Dixon (1988) 23 with refs.

[66] Bradley (1991). Met with approval by Martin (1996) 56-57. In a similar though much briefer vein Dixon (1988) 32: 'even if the nuclear household was the norm, Romans must in fact have known a variety of family forms and children probably became attached to a number of individuals at successive stages of their lives'.

[67] Sen., *ad Helviam* 2.4. The fact that Seneca not only places his own exile in the same category as the deaths just mentioned but even calls it a worse fate (3.1) for his mother, forms a sobering corrective to all too modern interpretations of the Roman moral universe – or at least that of Seneca.

[68] Pl., *Ep.* 4.19.

[69] For adoption, see Corbier (1991) 63-76.

been pointed out that among the lower classes the burial clubs provided in more than one sense an insurance against the high mortality regime – they offered not only social company, but also placed the obligation for burial outside the confines of the direct family.[70] Research focuses, in short, on the wider set of relations that compensated the lack of relations in the inner circle.[71]

But there are also problems. A minor one is the question to what extent 'dislocation' was perceived to be a problem. The word 'dislocation' has a negative connotation. Children in Russian elite families of the nineteenth century almost never saw their parents, but Russian (elite) literature abounds with descriptions of happy childhoods. In the Roman case, Harlow and Laurence have argued that mothers and children did in fact have much contact for large parts of the day.[72] Dixon has also pointed out that separate residences need not have formed an obstacle for the continuation of social and affectionate relations between parents and children.[73] Dislocation may certainly have existed, but how this affected social life is therefore difficult to establish.

A more serious problem is to what extent the theory is applicable below the imperial aristocracy. Although details might be debatable, the literary sources unequivocally confirm the general outlines of Bradley's theory. But these sources hardly ever extend below the top layers of society, and it is precisely these lower layers that are of crucial importance for our subject. Bradley did not address the problem systematically.[74]

It is important to realise that Bradley's theory contains two different elements. A distinction should be made between demographic fragmentation and culturally induced dislocation. The former is the direct result of high levels of mortality. The latter is produced by divorce, remarriage, attitudes towards children, and more forms of social behaviour that are subject to human control. Mortality applied to all classes with equal vigour, but it is an open question to what extent the other factors applied to levels below the aristocracy.

The distinction between biology and culture is not always clear in all cases. Some types of social behaviour have a relation with mortality. If a wife dies and there are no surviving children, remarriage becomes a sensible option for the widower. In other cases

---

[70] Rawson (1986) 37.

[71] Rawson (1996) 8, passim.

[72] Harlow and Laurence (2002) 28.

[73] Dixon (1988) 9, arguing that ties between mothers and children did not alter after the marriage of the child, and at 15 emphasising that we should not be misled by legal categories: adopted males and married females 'showed a great awareness of their 'natural' agnates.

[74] Some chapters of Bradley (1991) (such as the one on child labour) are devoted to the lower classes, but precisely these chapters are not crucial for his general theory.

particular types of social behaviour might be a response to both mortality and culturally induced behaviour: nursing of children might be in the hands of slaves both because of high mortality among women giving birth and because it was a sign of status to use a nurse.[75]. The one phenomenon in Bradley's model that is clearly not dependent on mortality is divorce. It therefore forms the best starting point to discuss the problem.

The question how frequent Roman divorces occurred has been posed, but has not been adequately answered.[76] The sources are not really helpful. There are many sources in which we find adult women without husbands. Given the fact that marriage was universal, it is likely they were once married, but it is in most cases impossible to determine whether their marriage was dissolved through the death of their husband or through divorce. Even in the case of the senatorial aristocracy where divorce is certain to have occurred frequently, it is hard to track down.[77]

There seem to have been no real obstacles against divorce. Divorce was not entirely approved upon.[78] In the moralistic universe of Roman writers, stories circulated about a 'first divorce', implying that in the good old times no such thing had existed.[79] But it is clear that this was part of a counter-rhetoric against the high incidence of senatorial divorces. Technically, divorce was relatively easy, for both men and women (or for fathers of those in *patria potestas*). Its easiness can be explained with reference to the concept of marriage: marriage was based on consent of the partners or, if *in potestate*, on the consent of their respective *patres familias*.[80] If consensus stopped, the marriage did too.

Among the senatorial elite, three major explanations for a divorce should be considered: lack of love, lack of offspring and lack of expediency of a family alliance. Marriages were normally not concluded for reasons of love, but affection was supposed to develop after the marriage.[81] Lack of such affection was in itself hardly a ground for divorce, though in individual cases marital conflicts could result in divorce. Lack of offspring might have been a more serious factor. Some couples did not produce children, and as one of the

---

[75] So Dixon (1988) 32.

[76] Treggiari (1991).

[77] Corbier (1991) 58. Cf. the generalising (but no doubt moralising) statement in the *Laudatio Turiae* 1.27: most marriages are terminated through divorce, not death.

[78] Gardner (1986) 51.

[79] For 'first divorce' stories see Dion.Hal., *Rom.Ant.* 2.25.7 (231 B.C.). Scholars have sought to discover in such stories a kernel of historical reality. Supposedly, the stories contain an indirect reference to the establishment of the *actio rei uxoriae* by which a dowry had to be returned. See Gardner (1986) 48 and Dixon (1985) 357. Cf. also 364, where she states that somewhere during the Republic divorce had become bilateral and shame-free.

[80] Corbier (1991) 52.

[81] Bradley (1991) passim; cf. Dixon (1985).

main purposes of marriage was the continuation of the family line, childlessness must have been an accepted ground for divorce.[82] Most importantly, divorce and subsequent remarriage among the senatorial elite formed part of senatorial power games, and it would be naive to suppose that these disappeared with the establishment of the Principate.[83] If fact, Corbier makes the point that divorces among the power games of the aristocracy occurred *in order* to remarry. In other words, often the wish to remarry took precedence and divorce was only a consequence of that wish.[84] Divorces, then, were partly the product of lack of affection, partly of lack of children, but to a large extent the consequences of strategical considerations.

It is highly questionable whether the same rationale applies below the senatorial elite.[85] Power games were absent, and affection will have been a luxury item. Consider the third-century Egyptian elites: among them divorce was well-known as a concept, but it was hardly used.[86] If the rationale was apparently lacking among such groups, it is difficult to see why it would be used more in general by the local urban population of the Latin West.

It seems likely that among the urban population a simpler model obtained, based on mortality only. Mortality was as high as among senators. The incidence of divorce was much lower than among the senatorial elite. Most marriages were therefore terminated by the death of one of the spouses, not by divorce.

Mortality produced holes in the family stemmata. One current tendency is to explore the effects of the availability of parents for children and vici versa. Many sons lost their fathers at an early moment. Saller has pointed out that this mitigated the effects of *patria potestas* to a large extent.[87] Paternal power might have been potentially unlimited, but a fathers' existence was not.

Age	0	5	10	15	20	25	30	35	40	45	50	55	60	65	70
% with living father	100	88	75	63	51	39	28	17	9	4	1	0	0	0	.

Table 2. Percentage of children with living father

[82] For which see Gardner (1986) 81. See also *Laudatio Turiae* 2.25-51, in which 'Turia' out of love offered her husband a divorce, so that through remarriage he could produce an heir – a suggestion angrily rejected by him, again out of love.

[83] Dixon (1985) 353 and 371.

[84] Corbier (1991) 57-63.

[85] Likewise Rawson (1986) 32.Despite the fact that Corbier in her (1991) study of divorce explicitly states that she bases herself on the Roman aristocracy, one cannot escape from the impression that she thinks that her findings are applicable to much wider sections of Roman society.

[86] For which see my Tacoma (2006) Part 2 Ch. 2.

[87] Saller (1987). This shows that Ulpian is not making an academic point when he states in *Dig.* 50.16.195 that a minor can also be a *pater familias*.

Source: Saller (1994) 52 (males,[88] ordinary West level 3)

Saller's argument about the availability of fathers is also relevant for marital patterns. The fact that men married significantly later than women must have had repercussions for the functioning of the marriage market. The age difference between the spouses determined the freedom of choice. Technically, consent had to be given either by the marriage partners themselves if they were *sui iuris*, or their parents, if they were *in potestate*.[89] In the case of women marrying for the first time there is hardly any doubt that she would always be surrounded by parents or other relations to make choices for her.[90] In the case of men the situation was different. In many cases both parents will have been dead by the time of marriage. Upon the death of their father, they normally had become financially and legally independent, so they will have had every right to make their own choices. Bradley has pointed out that interference in marriage negotiations was in fact not limited to the direct parents, but the fundamental fact remains that parental interference (or more general: family interference) will have been much stronger in the case of women than in the case of men.[91] Bradley and others have also pointed to the seemingly paradoxical fact that conjugal love did not form the basis of a marriage, but was nevertheless supposed to grow afterwards. If it is assumed that men had more room for choice of a partner, the problem must have been more pertinent for women than for men.

But let us not drift too far away from the question whether the Roman family is to be considered nuclear or extended. At the end of section 2, the problem of the choice between the nuclear versus the extended family was redefined: the crucial question is what happened to a son as he was marrying. Would he set up a new household or would he remain living with his father?

At first sight, a marriage pattern of early female and late male marriage seems not very helpful to answer this question. But the fact that men married late in conditions of high mortality has important consequences. As Saller has shown in the context of *patria potestas*, many fathers were not alive anymore when their sons reached their twenties. Saller used this

[88] For obvious reasons, the percentages for sons are almost identical to those for women; the latter are therefore not reproduced here. For women, see Saller (1994) 49.

[89] Gardner (1986) 41-44. In the case of a *sine manu* marriage if she was *sui iuris* she could apparently act for herself, despite the presence of a tutor – so Gardner (1986) 46.

[90] For a glimpse, see the third marriage of Pliny with Calpurnia; the matter is arranged with her aunt, who brought her up.

[91] Bradley (1991) 177-204. The same argument is made by Dixon (1985) 367, incidentally also on the basis of an analysis of Cicero's family. Note also that technically it was a right of the father, not of the mother, but that in practice the mother had a large say - so Dixon (1985) 370.

argument to show that the practice of *patria potestas* was less harsh than the theory predicted.[92] But one could also make another argument. If sons married late, many of their fathers would have been deceased already, and many others would follow soon afterwards. In others words: the period in which the majority of sons married coincided with the period in which many of their fathers died.

The important consequence is that the supposed conflict between the nuclear and the extended model of the family is more apparent than real. In cases where the father was already dead, a son would automatically set up a household of his own upon marriage. But if his father was still alive, a son may decide to bring his new wife into his father's family, which thereby would be turned into an extended household. But such a situation would only be of short duration, as the chances were high that the father's household would be relatively quickly dissolved through the death of the father. This means that extended patriarchal families may certainly have existed, but that they were a fleeting occurrence. They quickly dissolved upon the death of the *pater familias,* which created new nuclear households of their sons. The boundary between extended and nuclear families was thus very permeable, and the one type could easily spill over into the other.

Mortality thus has important effects on family formation. If we can assume that the more elaborate types of social restructuring were of less importance among the lower classes, we still can observe the effects of mortality. The availability of parents in combination with differences in marital ages have important consequences for the freedom of choice that marriage partners had. More importantly, they also produce a pattern in which sons could easily move into their parental home upon marriage – just as easy as the death of their father would move them out again.

5. The Circulation of Widows

Thus far, the focus has been on men. The arguments apply to the setting up of households through marriage. Given the fact that marital ages differed sharply between husband and wife, the focus was necessarily on marrying men. It is their behaviour that is of real relevance for the question of potential migration. It is clear that the position of young women is of much less interest, for they married straight out of the parental home at a very young age. If we

[92] Saller (1987).

follow Hajnal's reasoning, the conclusion therefore seems obvious that Roman migrants should be primarily located among young unmarried males. But that is not the whole story.

The position of women is not without interest for the issue of migration, but at a different stage of their life. It is the position *after* their first marriage rather than before their first marriage that is important. As we have seen, because of the high mortality rates many marriages terminated at an early stage. Once again the difference in marital age is of relevance. As men were older than women, many will have died before their wives. This will have resulted in a large pool of once-married women. As women married young, they might still have a long life ahead of them when their husbands died. The question is what happened upon the termination of a marriage. To what extent did women move to different households, and how much freedom in decision-making did they have?

The crucial question is to what extent women remarried. The question has not received the attention it deserves. Especially the fact that male and female patterns of remarriage may have differed from each other substantially has not well been realised. There are at least two different scenario's possible: one that we may call senatorial, and one that may be called Egyptian.

The senatorial model is one in which female remarriage occurred on a large scale. The extent to which such remarriage was the norm is open to debate. Corbier in her research into senatorial family strategies assumed that remarriage was the norm among senatorial women, for their marital regime was one of serial monogamy.[93] However, this is surely an overstatement of reality. To be sure, there are certainly enough examples of female remarriage among the senatorial aristocracy to show that female remarriage was completely normal among the senatorial aristocracy.[94] But Corbier simply ignores counter-examples of women not remarrying. There are enough references to the existence of an alternative model among the senatorial class in which women refrained from remarrying. It is therefore saver to assume that among senatorial women more than one option may have prevailed.

The Augustan marriage laws are in accordance with such a scenario. Augustus' marriage laws stipulated that both men and wives whose marriage was terminated should remarry. The laws applied to unmarried women between ages 20 and 50, including widows who had not remarried within a year (later two years), and divorcées not remarried within six

[93] Corbier (1991) 50-51, 55-56 emphasises that the Roman discussion about remarriage was misogynistic and asymmetrical: only in the case of women it was an issue, not in the case of men. She also seems to think that whereas men had the option to remain bachelor (and/or enter into a relation with a concubine), or adopt a son, a woman did not have a choice.

[94] Dixon (1988) 30-31.

(later 18) months.[95] The relation of the law with reality is debated, and so is its effectiveness. Whereas the laws are usually regarded as an attempt to counteract a decline in aristocratic fertility rates, according to Bradley the Augustan marriage laws merely confirmed an already existing norm of remarriage.[96] As is well known, the laws were directed primarily at the elite, and it should also be realised that the penalties for non-compliance were relatively mild. The Augustan marriage laws certainly point to a strong pressure to remarry among the aristocracy, but they cannot be used to argue that remarriage was universal.

The evidence of the census population of Roman Egypt points to quite a different scenario. There, a pattern existed in which men did remarry but women did *not*. After the dissolution of their marriage, men remarried with young brides, whereas women married only once and remained widows for the rest of their life. Such a counter-intuitive pattern was the product of a large age-gap between spouses in combination with high mortality. There is no intrinsic reason why such a pattern could not be followed elsewhere in the Roman empire. In fact, individual instances of remarrying men seeking very young brides can also be found among the senatorial aristocracy. Pliny's third wife Calpurnia was certainly very young when she entered Pliny's third marriage.

The issue to what extent female remarriage occurred in the inscription-erecting population below the elite has hardly been addressed, but the few opinions that we have differ significantly. Corbier implicitly seemed to assume that her imputed pattern of universal female remarriage among the senatorial population was followed by the rest of the population.[97] Saller in his demographic micro-simulation used the same assumption.[98] With explicit reference to the Egyptian remarriage system, the extent of female remarriage among the lower classes of the rest of the Roman world has been questioned by Erdkamp.[99] It is therefore of real importance to know which scenario is right.

One issue can be solved immediately. There can be no doubt that remarriage *did* occur below the senatorial elite. The Harkness/Hopkins dataset discussed in the second section showed some relatively high female marital ages. Many marriages were concluded beyond age 24. Such high ages have been regarded as referring to a remarriage. The occurrence of remarriages formed precisely the problem that makes it impossible to derive age at *first*

[95] Rawson (1986) 31.
[96] Bradley (1991).
[97] Corbier (1991).
[98] Saller (1994) 9-69.
[99] Erdkamp (forthcoming).

marriage from the sample. There are also other scattered epigraphical instances showing remarriage.

But there are also indications that not all women remarried. Widows formed a separate category in society. It seems that widowhood was not regarded as a transient but as a permanent state. In the epigraphic evidence, the *univira* is a recurrent type. The word is referred to many times, and clearly not only in senatorial contexts.

How should the difference be explained? The *univira* (or, more general: the state of permanent widowhood) is often seen as exceptional. Dixon states that '[w]e cannot know how many widows actually resisted family and legal pressures (if they were child-less) to re-marry because of loyalty to the ideal or to an individual spouse'.[100] Gardner puts it somewhat more emphatically: 'Statistics are lacking on the incidence of remarriage, but the young or youngish widow who never remarried, like the woman who never married at all, was probably a relatively rare phenomenon in Roman society, which appears to have suffered from the chronic shortage of marriageable women'.[101]

One possible solution – already hinted at in the quotation of Gardner – might be sought in the age of remarriage. Hopkins' sample shows that almost all female marriages (including the remarriages) occurred before menopause. If the principle aim of marriage was producing offspring, we might assume that the same principle applied to remarriage. If we start from the assumption that the aim of remarriage was to produce new offspring, the social pressure on women who were widowed or divorced before menopause to remarry is likely to have been strong, whereas past that age the pressures will have been virtually absent. This would then result in a scenario in which widowed women before menopause would seek remarriage, and women after menopause would remain widows for the rest of their life. This is no doubt a crude and highly schematic scenario – but does it cover in its principle points reality?

It seems likely that one more factor should be added, and that is provided by demography. The presence of a living father would be a significant factor. If a father was alive, a woman would not be legally and economically independent. The dowry would return to the parents, and so would probably the woman herself. Conversely, it was the death of the father, rather than the death of the husband, that gave women legal and financial independence (with al due restrictions of *tutela*, of course). Independence need not result in

[100] Dixon (1988) 22.
[101] Gardner (1986) 56.

the univirate, but if seems safe to say that a woman's freedom of choice would be much greater. She could opt for remarriage, but it would also be easier to withstand the pressure.

The crucial question is once again how many fathers would be alive. The question can be answered on the basis of the same graph used earlier. Women married relatively young, so their parents might still be alive upon dissolution of a marriage. But given the age differential between the spouses, this applies more to mothers than to fathers. Later in a woman's life, the chances were high that her father would be deceased.

The two factors (age of the widow and presence of a living father) lead to a slightly more complex scenario than that sketched above. After menopause, the situation is relatively clear. The univirate would virtually be forced on a widow. It would be highly exceptional if her father was still alive, which meant that financially and legally a woman would be capable to set up her own household. Before menopause, widowhood offered more options. If a father was still alive, a woman would move back to the parental home, and there either remarry or remain widow. But many widowed women lacked a living father. According to Saller's simulation, already at age 25 over 60% of all women lacked a living father. Such women might seek remarriage, they might move in with other relatives, and they might also set up their own household.

The arguments just presented show that apart from remarriage there were several other possibilities for a widowed woman. They also show that the many women that we find operating on their own in the sources might be what they seem to be: independent.

The extent of *physical* mobility of widows is difficult to establish, but that widows circulated is beyond doubt. Some women moved into new households through remarriage. Such a move will undoubtedly have also contained some element of geographical mobility, but the extent of it could vary significantly. In many cases, remarriage will have meant a double move: a woman moved first back to her parents (or other relatives), and from there into the household of her new husband. Women not remarrying might have stayed in their deceased husbands' house, but it need to be realised that in many cases she will not have inherited the house and hence must have moved to property of her own. In all cases the distances travelled are likely to have been small. Much movement consisted of circling around the family households. A woman might move back to her parents, or move into her son's household which might be taken over from his father. If she would set up her own household she would do so presumably near the place where she had lived, or where her parents had lived.

No matter how much Roman widows moved physically, a different subject should be added to Hajnal's model of migration, and that is the circulation of widows. Patterns of mortality in combination with differences in marital ages result in a large category of women once-married. The young age at which women married inhibited their participation in migratory movements at young age, but at the same time lead to movement after marriage. The distances widows will have travelled are likely to have been small, but it is precisely the small-scale movement that is at issue here.

Conclusion

What can we conclude, then? What are the implications of my findings for Hajnal's hypothesis and for the issue of Roman migration? Who are the candidates available for migration?

Hajnal distinguished two types of households and two types of marriage patterns: a European one and a non-European one. Only the former created a labour force of temporary migrants. The European pattern of late marriage combined with simple households gave rise to a system of temporary migration. Hajnal's modern European marriage pattern of late marriage for both spouses created a labour-force of people in the prime of their life without social obligations. This lead to a massive stream of temporary migrants, mostly working in the service-sector of the economy.

The question how the Roman household fits in led to an analysis of household forms and of marriage patterns. Analysis of the former was unresolved, but analysis of the latter led to a remarkable conclusion: a third type of marriage pattern existed that deviates from either of Hajnal's types. Hajnal ignored the possibility of a third marital pattern consisting of early female marriage and late male marriage. This pattern in its turn produced a type of household that defies a categorisation as either extended or nuclear. The one type could easily turn into the other one, and the Roman household therefore was in some sense both.

This raises the question how we should envisage Roman migration. Hajnal's European pattern depended on late marriage. In the Roman pattern, late marriage existed, but it was confined to men. Male marriage was delayed, and this created a large time-span between adulthood and marriage. The crucial question, then, is what young Roman men did between, say, age 14 and age 25. They would have a long period for training, education, work or travel, and combinations of them. It is certainly conceivable that this group migrated on a temporary

basis. We may posit the existence of a large labour force of people not yet caught in a web of new ties, just as in modern Europe. The crucial difference is that it consisted entirely of males.

Women are most unlikely to have moved before marriage. Their early marital ages suggest that the only move they were to make was straight out of the parental house into a new household. Erdkamp has also aptly observed that the one most likely venue for their activities, being servants in the houses of the wealthier parts of the urban population, was closed, because of the presence of slaves. The concept of servants is in that sense absent from the Roman world.[102]

But women were not completely absent as migrants. Somewhat more promising candidates for female migration are formed by widows. Several options were available. If parents were available, it will have been quite normal for a woman to return into her parents' home. This might but need not necessarily have lead to a remarriage. At the age of widowhood, many fathers were already dead, and others would follow soon. It seems very likely that when the father was dead there were several options for women: they could move to other relatives, they could remarry, but they could also set up their own household and remain widow. Even in a crude and simple model in which availability of fathers and the age of widows form the determining factors, more than one option was available.

What cannot be in doubt is that there is a relation between the structure of the Roman family and the possibilities for migration in the Roman world. The movement of young men can be analysed with the help of Hajnal's model, though with important modifications. The circulation of widows forms a new and promising addition. Whether and to what extent men and women *actually* moved, and to what extent their movements should be classified as migration, is a question that requires further discussion.

Bibliography

Bradley (1991): K.R. Bradley, *Discovering the Roman family. Studies in Roman Social History* (New York 1991).

Corbier (1991): M. Corbier, 'Divorce and adoption as Roman familiar strategies', in: B. Rawson (ed.), *Marriage, divorce and children in ancient Rome* (Oxford) 47-78.

Crook (1967): J.A. Crook, '*Patria potestas*', *Classical Quarterly* 17: 113-122.

Dixon (1988): S. Dixon, *The Roman mother* (London 1988).

[102] Erdkamp (forthcoming).

Dixon (1992): S. Dixon, *The Roman family* (Baltimore en Londen 1992).

Friedländer (1919):

Gallivan and Wilkins (1997): P. Gallivan and P. Wilkins, 'Familial structures in Roman Italy: a regional approach', in: B. Rawson en P. Weaver (eds.), *The Roman family in Italy: status, sentiment, space* (Canberra en Oxford) 239-279.

Gardner (1986): J.F. Gardner, *Women in Roman law and society* (London and Sydney).

Hajnal (1965): J. Hajnal, 'European Marriage Patterns in Perspective', in: D.V. Glass and D.E.C. Eversley (eds.), *Population in History* (London) 101-143.

Hajnal (1982): J. Hajnal, 'Two Kinds of Preindustrial Household Formation System', *Population and Development Review* 8: 449-494.

Hajnal (1983): 'Two Kinds of Pre-Industrial Household Formation System', in: R. Wall, J. Robin and P. Laslett (eds), *Family Forms in Historic Europe* (Cambridge) 65-104.

Harlow and Laurence (2002): M. Harlow and R. Laurence, *Growing up and growing old in ancient Rome* (Londen and New York 2002).

Harris (1986): W.V. Harris, 'The Roman father's power of life and death', in: R.S. Bagnall en W.V. Harris (eds.), *Studies in Roman law in memory of A. Arthur Schiller* (Leiden) 81-95.

Hopkins (1964/5): M.K. Hopkins, 'The age of Roman girls at marriage', *Population studies* 18: 309-327.

Hopkins (1999): K. Hopkins, *A World full of Gods. Pagans, Jews and Christians in the Roman Empire* (London).

Lelis, Percy and Verstraete (2003): A. Lelis, W.A. Percy and B.C. Verstraete, *The Age of Marriage in Ancient Rome* (Lewinston, Queenston, Lampeter).

de Ligt (2001): L. de Ligt, 'De significatione verborum: Romeins erfrecht in de Laudatio Turiae', *Lampas* 34: 45-61.

Martin (1996): D.B. Martin, D.B., 'The Construction of the Ancient Family. Methodical Considerations', *Journal of Roman Studies* 86: 39-60.

Osborne (1991): R. Osborne, 'The Potential Mobility of Human Populations', *Oxford Journal of Archaeology* 10: 231-252.

Page Moch (2003^2): L. Page Moch, *Moving Europeans. Migration in Western Europe since 1650* (Bloomington en Indianapolis (orig. ed. 1992)).

Rawson (1986): B. Rawson, 'The Roman family', in idem (ed.), *The Family in Ancient Rome. New Perspectives* (Ithaca) 1-57.

Saller (1987): R.P. Saller, 'Men's Age at Marriage and its Consequences in the Roman Family, *Classical Philology* 82: 21-34.

Saller and Shaw (1984): R.P. Saller and B.D. Shaw, 'Tombstones and Roman family relations in the Principate: civilians, soldiers, and slaves', *Journal of Roman Studies* 74: 124-156.

Shaw (1987): B.D. Shaw, 'The Age of Roman Girls at Marriage: some Reconsiderations', *Journal of Roman Studies* 77: 30-46.

Shaw (2001): B.D. Shaw, 'Raising and killing Children: two Roman myths', *Mnemosyne* 54 (2001) 33-77.

Tacoma (2006): L.E. Tacoma, *Fragile Hierarchies. The Urban Elites of Third-Century Roman Egypt* (Leiden).

Tacoma (forthcoming): L.E. Tacoma, 'Urbanisation and Access to Land in Roman Egypt', in: R. Alston and O. van Nijf, *Feeding the City* (BICS Suppl) (forthcoming 2007).

Treggiari (1991): S. Treggiari, 'Divorce Roman style: how easy and how frequent was it?', in: B.Rawson (ed.), *Marriage, divorce and children in ancient Rome* (Oxford) 31-46.

Williams (1958): G. Williams, 'Some Aspects of Roman Marriage Ceremonies and Ideals', *JRS* 48: 16-29.

Leiden, 18-1-07