Chapter 8

THE PALEODEMOGRAPHY OF CENTRAL PORTUGAL AND THE MESOLITHIC-NEOLITHIC TRANSITION

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Abstract: Newly available information on the excavation of the Portuguese Mesolithic shell middens, Cabeço da Arruda and Moita do Sebastião, has allowed reassessment of the paleodemography of the sites. Following the restudy of Arruda and an examination of Moita site structure, we now discuss the problem of arriving at a minimum number of individuals (MNI) for Moita and use the age distribution of the dead to estimate the total fertility rate (TFR). We confirm the difference between Moita and Arruda and note their divergence from the Neolithic site of Casa da Moura. Our method of estimating TFR, deriving from the use of West model tables, is explained and is tested by reference to historical data sets and by calculation of equivalent demographic values using the Brass relational table approach. Our focus is the Mesolithic-Neolithic transition and we establish the context of demographic change in the late Mesolithic and early Neolithic of central Portugal by reference to changing environmental, nutritional and disease conditions. The low level of population growth at Moita would have increased during the occupation of Arruda. However, subsequent changes in climate and sea levels led to unfavorable conditions and we hypothesize that the Tagus lowlands were abandoned in favor of healthier uplands where there was a rebound and an increase in population growth in response to changed lifeways

INTRODUCTION

This paper continues work that has interested us for over twenty years, the interpretation of the demography of the Mesolithic and Neolithic of central Portugal, which falls within the much broader sphere of the interpretation of demographic variables in human populations during the late Pleistocene and Holocene. The continuing debate over what Deevey (1960) called the

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demographic transition (but see Schulze and Mealy, 2001) is the focus of our interest. Here we will concentrate on the interpretation of the Portuguese mid-Holocene record and touch on other areas only to clarify our concerns about the assessment of population samples and the difficulties of global interpretation.

We begin with a brief introduction to certain aspects of paleodemographic data and methods, followed by a mention of our earlier work on European Mesolithic and Neolithic demography, in order to explain our interest in the Portuguese data. We introduce and discuss the skeletal sample from Moita do Sebastião that is central to this paper. Methods allowing us to estimate the relative fertility levels of the key Portuguese sites are detailed and evaluated. Finally, we place the sites within the general context of the demographic aspects of the transition to agriculture in Portugal.

Paleodemographic reconstruction

The potential uses of demographic data in bioarcheological interpretaton have undergone a revolution over the past thirty years. When Peterson wrote his critical review of paleodemography (1975) his concerns were with the accuracy of sex and age determination, and with sample bias. His conclusion was that paleodemography was founded in quicksand and unlikely to yield meaningful results.

While Peterson's concerns have not been fully answered, the focus of the enquiry has changed and matured, following upon the work of Bocquet-Appel and Masset (1977). Our work has centred upon an examination of what the age profile tells us about population growth (or decline) and fertility.

However, there are three critical aspects that need examination if paleodemographic age at death distributions are to be interpreted accurately. The first is that there must be accurate age assessment of the osteological sample. This has been extensively reviewed (e.g. Jackes, 1992). Very careful assessment of possible sample bias is also necessary. A further concern is the structure of the age profile to be used. For reasons discussed elsewhere (Jackes, 1986, 1992, 2000; Jackes et al., 2001a) we believe an age profile that divides the sample into five year categories for the period from birth to 24 years, and groups all adults aged 25 and over, provides the most satisfactory results. In this way, errors in adult age assessment are largely avoided with minimal loss of explanatory power.

The second aspect concerns the completeness of the sample. Are we dealing with a full collection as excavated? What is the relationship of the excavated sample to the site as a whole? This is an aspect that has especially concerned us in our examination of the Portuguese data since excavation has been ongoing since the mid 19th century.

The third aspect has to do with the issue of which demographic values can be considered representative of the original biological population. We plot the ratio of subadults to adults, as first proposed by Angel (1969, 1971) and the mean probability of death during childhood (Jackes, 1986). These serve as summary values that will allow us to make comparisons among sites, to interpret the data by setting site values within a wider context in order to make sure that the data are valid and biologically possible and in order to provide an interpretation.

Paleodemographic research and the shift to food production

The transition from food gathering to food production (in Europe the Mesolithic-Neolithic transition) has produced an extensive literature. The demography of the transition, first discussed by Deevey (1960), was subsequently elaborated by, for example, Spooner (1972) and Cohen (1977). The central question asked was whether or not population growth could be seen as a cause or a consequence of the transition.

We stress that paleodemographic exploration of the transition requires a good deal of self-discipline on the part of the researcher because there are clear pitfalls in such analysis. Data must be from sites which are stringently chosen. Firstly, they must be from the appropriate time period. Secondly, we cannot ignore strictures levelled against the use of data from sites with enormous time spans and tiny samples, sites that were incompletely excavated, sites in which only a percentage of burials were used to develop the life tables, sites with clear biases introduced by the burial patterns (e.g. Jackes, 1992). If the basic data, the number and age distribution of the skeletons, are biased or flawed, the conclusions rest on shifting sand.

We also suggest that global modelling of transitions such as that from food gathering to food production is a questionable exercise because it assumes that the transitions are similar in different parts of the world. Clearly the transition in the Near East, where it is earliest, differs from that found in Europe where there is debate over its very nature. What parts of Europe may have undergone colonization by farmers from the Near East (see e.g. Scarre, 2003; Zvelebil, 2003; Di Giacomo et al., 2004 cf. Semino et al., 2004)? To what extent were there real changes in subsistence? What was the effect, if any, on the social organization and settlement patterns of the immigrant and/or the indigenous populations? Answers to these questions will have major influence on the interpretation of demographic changes.

Earlier attempts to explore transitions in North American contexts (Jackes and Lubell n.d.; Jackes, 1986; Jackes, 1993) were problematic. Many recent papers focus on the difficulty of identifying a clear transition to horticulture in the Americas (B. Smith, 1989, 1995, 1998, 2001a; Chapdelaine, 1993; D. Smith in prep.; O'Shea, 2003): the most trenchant statement is that by Bruce Smith as he fulminates against the "reductionist, essentialist, and dichotomous world view that societies are either hunter-gatherer-foragers or farmers" (Smith, 2001b:4). Even more recently Bellwood, in a general summary, has characterized the shift to agriculture in Central and North America as "diffuse" (2005: 155, 177).

Our attempts to identify and analyze transitions other than in Europe and North America have also been problematic. We unsuccessfully sought suitable East Asian samples (Jackes, 2004; Jackes and Gao, n.d.). The North African Maghreb, sometimes used in analyses which include the circum-Mediterranean Basin, shows a set of very specific problems (Lubell, 2001, 2005; Rahmani, 2003, 2004). Included in these are the demonstration of change within the Epipaleolithic, the analogue but probably not homologue of the European Mesolithic, the late introduction of the Neolithic into the Maghreb, and the long time period represented by the sites. One core site sometimes seen as analogous to the European Mesolithic is Taforalt. This site falls completely within the Iberomaurusian period but probably represents a 10,000 year time span. It also involves two cemeteries, only one of which is directly dated (one date at ca. 13,730 calBP), perhaps 7,000 years before the Neolithic of Capsian Tradition appears. Use of such a site to model the demographic situation for the pre-Neolithic is problematic. There is very limited evidence for subsistence change until the Neolithic of Capsian Tradition, and even then the long-established Epipaleolithic pattern continues.

Our work on the Near East (Jackes et al., in prep. b, contra Eshed et al., 2004) suggests that it is an over-simplification to assume there was a clear-cut Mesolithic-Neolithic transition directly associated with a demographic transition (see also Davis, 2005).

Paleodemography of the shift to food production in Europe

We began studying paleodemographic changes at the shift to agriculture in Europe by analyzing the human skeletal remains recovered from the Mesolithic shell middens of the Muge estuary in central Portugal (see Lubell et al., 1989). Our initial reconstruction of the demographic profiles of two Mesolithic sites and one Neolithic site (Jackes, 1988: see further below) suggested firstly that rates of growth and apparent fertility were higher in the Neolithic than in the Mesolithic, and secondly that the two Mesolithic sites were different from each other.

Since our initial publication of these data we have been involved in two further preliminary studies of demographic profiles in the European Mesolithic and early Neolithic (Meiklejohn et al., 1997; Jackes et al., in press), the first centered on samples from Northern Europe, the second on collections from Đjerdap (Iron Gates Gorge). The results are congruent with the findings of Jackes (1988), but the samples are not satisfactory. Some samples are small, some incomplete, some biased, some are agglomerated samples from more than one site, and there are uncertainties about the archeology and archeological provenance in some. Few of the individual samples stand up to close scrutiny, and therefore we cannot consider these a solid basis for interpretation of the agricultural transition.

We are convinced that central Portugal is one of the only geographical regions in which we can examine the transition with some degree of assurance that the data are sufficiently robust. We began work here in 1983 (Lubell et al., 1989), focusing our attention on radiometric dates and stable isotope analyses, and attempting to gain a fuller understanding of the sites (e.g. Jackes and Lubell, 1999a,b; Lubell et al., 1994). We have looked at metric and non-metric indicators of group relationships, and whether or not the groups were homogeneous over space and time. We have concluded that there was no large influx of population at the time of the subsistence transition (e.g. Jackes et al., 2001b). However, our work on the extraction and analysis of ancient DNA is continuing and may provide further information (e.g. Bamforth et al., 2003).

Most recently we have been examining means of assessing the population structure of those sites we believe can provide the most reliable data for assessment of the transition. We have been working in great detail on questions of numbers of individuals in the sites and their age profile, and enquiring how representative the burials are of the original group.

Unfortunately, we can work only with what the archeological record makes available to us, so there can never be the rigor we would desire in the selection of samples suitable for reaching definitive answers. However, central Portugal provides us with the numbers, the sites, and a long history of archeological exploration so that we can at least approximate a reasonable research methodology. This is not to gainsay that further archeological research in central Portugal may provide evidence of a "fuzzy" transition, and we very briefly touch upon this below. Archeologists may well discover that there are as many questions in central Portugal about the nature and timing of the transition as there are elsewhere (Oosterbeek, 2004), but our ongoing work on dating and stable isotope analyses provides a fair measure of certainty about the suitability of the sites we will discuss and their relation to a subsistence transition. We are using sites that appear to fall on either side of a subsistence divide as demonstrated by stable isotope analyses and which represent a fairly limited time period on either side of that divide (see Jackes and Meiklejohn, 2004 for information on stable isotopes and radiocarbon dates, with details on methods of calibrating the dates).

The samples available to us are from two Mesolithic sites and one Neolithic site. The Mesolithic sites (Cabeço da Arruda and Moita do Sebastião) come from a restricted area along the Muge, a southern tributary of the Tagus River. The Neolithic material is from a cave (Casa da Moura) in a karstic limestone area to the north of the Tagus (Figure 1). Casa da Moura is the earliest among a complex of such ossuary cave sites which includes others we have studied, Furninha, Feteira and Fontainhas. It is unfortunate that we do not have Neolithic material which is geographically closer to the Mesolithic sites, but suitable material is not available from Portuguese sites south of the Tagus. Only the completely inadequate sample from Melides Lagares is of the right time period[1]. For Escoural, also south of the Tagus in the northern Alentejo, the excavation history and a summary of what we know (Cauwe, 1996) would suggest that the MNI, while over 34, is insufficient. Furthermore, the dating, based on human bone fragments from Galleries 4 and 12 (Gilot, 1996), is at least 1,500 years after the end of the Muge occupation. Escoural is quite certainly younger than 5500 calBP and therefore ca 1,000 years after the transition.

A full discussion of the sample from Cabeço da Arruda has been published (Jackes and Meiklejohn, 2004) and does not require reiteration. Casa da Moura has been the subject of re-excavation and analysis (Straus et al., 1988) as well as discussion on the methods of dealing with the human sample (Jackes, 1992; Jackes and Lubell, 1996). Examination of documentary evidence on Moita do Sebastião and its excavation has begun (Jackes and Alvim, 2006, Alvim et al., in prep.; Jackes et al., in prep. a), and it is appropriate to undertake a complete re-evaluation of the Moita sample, so that we have a firmer basis for our discussions of Muge paleodemography. The publication of Veiga Ferreira's field notes and sketches from the excavations at Moita do Sebastião between 1952 and 1954 (Cardoso and Rolão, 1999/2000) allows this complete re-evaluation of the site and the human skeletons found there. This newly published material is supported by

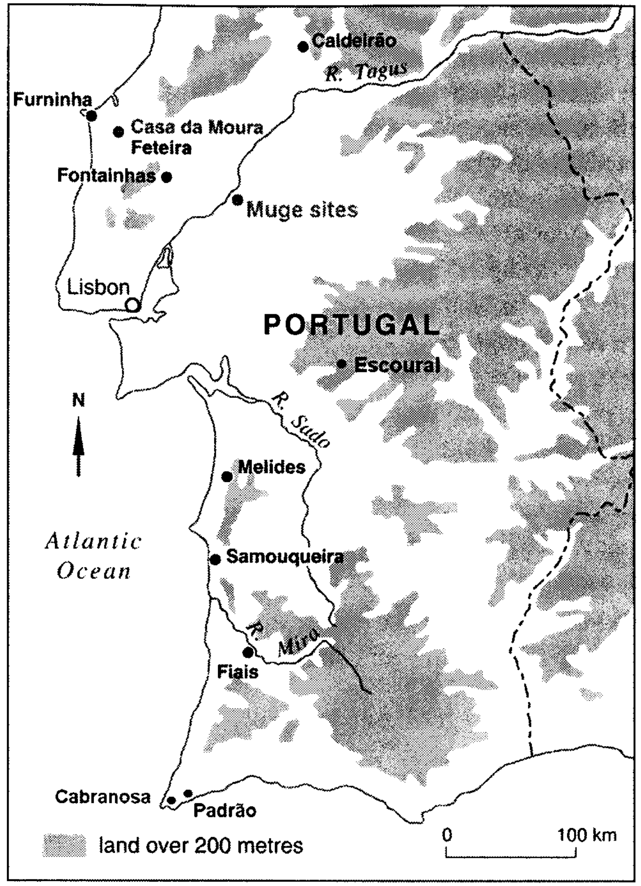

Figure 1: Map showing locations of sites mentioned in text

new evidence on the skeletons excavated in 1954 (Jackes et al., in prep. a). Together with archival material from the 19th century, the evidence from the 1950s allows us to re-examine Moita do Sebastião as a whole.

MUGE AND MOITA DO SEBASTIÃO – THE BACKGROUND TO THE PALEODEMOGRAPHY

The Cabeço da Arruda and Moita do Sebastião shell middens are located on terraces of the Muge River which flows into the upper estuary of the Tagus from the east. Other middens were present, including the largest still in existence, the less well known site of Cabeço da Amoreira[2]. A number of factors must be considered in order to provide the context for the occupation of the Muge valley, which was extensive in the Mesolithic and apparently largely absent in the succeeding Neolithic.

The occupation of the three Muge sites appears to have lasted from shortly after 8000 years calBP to sometime prior to 6500 years calBP (see Figure 2). Moita was the earliest, and was occupied for about 700 years. Arruda was first occupied a few hundred years later and the occupation lasted for almost a millennium. Amoreira, the least known of the three, appears to be roughly contemporaneous with Arruda but perhaps beginning a century or so later and lasting a century or so longer.

Figure 2 provides a composite image which places the Muge human burials within their maximal probability ranges (at least 95.4%) using calibrated dates adjusted for a reservoir effect of 253 ± 29[3]. Those burials for which we have both stable isotope values and radiocarbon dates provide the date range for each site. Amoreira is least well documented, only two human bone collagen dates being definitively available[4]. The "whiskers" show the maximum range for the use of this site, derived from recent dates on faunal material (Roksandič pers.com.). Charcoal dates on basal levels at

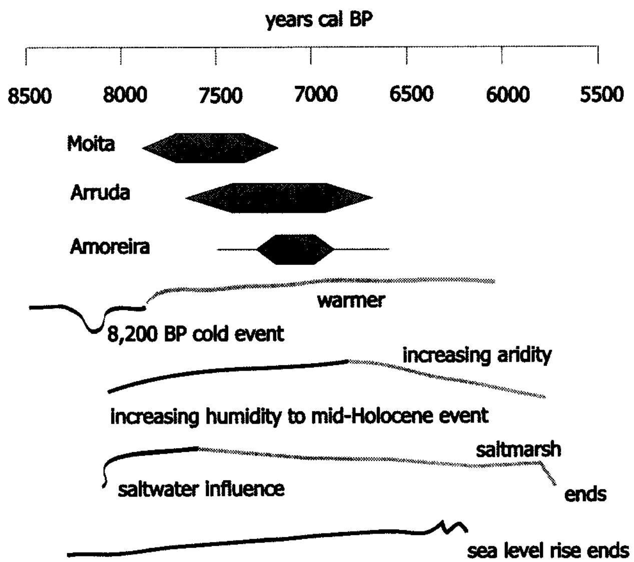

Figure 2: Diagrammatic summary of changes in the Muge valley at the Mesolithic-Neolithic transition

Moita and Amoreira (see Jackes, 2004: Figure 8) seem to confirm the age ranges suggested here for the burial levels.

The work of van der Schriek (2004; van der Schriek et al., 2003) allows us a more fine-grained look at conditions within the Muge valley, particularly based on Core MDS6N (20) between Moita and Arruda. This is well dated and shows the start of saltwater influence at ~8100 calBP, dated on plant material associated with foraminifera and fine-grained sedimentation. A foraminiferal peak indicates the maximum tidal influence at 7500 calBP, after which pine pollen begins to decrease and the assemblage indicates intensification of the salt marsh flora. The peak of salt marsh vegetation was reached by 6000 calBP and all salt water indications disappear within a few hundred years of this date.

These data suggest that the primary occupation at Muge occurred within the last 1,500 years or so of sea level rise in this region. The end of the postglacial rise in sea-level along the Portuguese coast is dated by lagoons which formed behind sand barriers. The sea was no longer rising by 6200–6300 calBP (dated on wood; Freitas et al., 2003). Up until that time, the sea had risen at an average rate of 85 cm per 100 years (Boski et al., 2002; but see also Long, 2000 and Psuty and Moreira, 2000 with regard to variations or oscillations).

A primary reason for occupying the Muge valley during the late Mesolithic would have been the abundant estuarine molluscs found in the salt marshes. The major species in the archeological deposits are *Scrobicularia plana* (peppery furrow shell) and *Cerastoderma edule* (cockle) (Lentacker, 1991). *C. edule* will tolerate a range of salinity (Tyler-Walters, 2003); *S. plana* can survive in settings with very low salinity (Pizzolla, 2002), such as would have occurred when the terrestrial sources of moisture were greatest, just after 7000 calBP. *S. plana* is found in mud flats and salt marsh creeks (Costa et al., 2001). It is "detritivorous", feeding on the detritus by sucking it from the mud surface using an inhalant siphon when the tide is in. High densities can be supported, as suggested by the faunal remains from the Muge sites which include huge numbers of *S. plana*. *C. edule* which is both detritivorous and suspensivorous, burrowing less deeply into the mud and able to tolerate a wide range of salinity, may have been able to survive more of the changes that occurred towards the end of the Mesolithic occupation of the Muge. Figure 2 suggests that, with increasing temperatures and aridity, the Muge salt marshes could have become too saline for *S. plana*. It is perhaps relevant that Mendes Corrêa (1932) reports that *S. plana* is less abundant at Amoreira than at Arruda, *C. edule* being more frequent in the deposits at Amoreira than at Arruda. Finally, the tides would have ceased to reach the Muge and estuarine species would have disappeared completely. Thus, the abundant bivalve molluscs which drew the Mesolithic population to the Muge are likely to have found the fluctuations in conditions intolerable and disappeared.

Besides shellfish, the salt marshes would have provided another resource. Chenopodiaceae pollens are dominant in the area of Moita and Arruda during the period of occupation (van der Schriek et al., 2003, Figure 5). Chenopods provide easily gathered and extremely nutritious food derived from both their green tips and from their seeds (e.g. glassworts or marsh samphires which are succulent halophytes, able to live in salt marsh habitats). Liguliflorae and Poaceae come second and a more distant third in the pollen frequency counts. While the latter grasses (e.g. *Spartina* spp.) would have provided useful materials for daily life, it is likely that the former daisy-type plants provided food in the form of golden samphire (a type of aster adapted to saline conditions). Chenopods would have reduced in number as the frequency of tidal inundation decreased, to be replaced by grasses.

The importance of the marsh swamp species to the people of Moita is stressed by the findings of Roche and Veiga-Ferreira (Roche, 1972) during the excavations of the early 1950s to be described below. Six Moita skeletons were buried with shells of *Neritina fluviatilis*, a gastropod species (= *Theodoxus fluviatilis*) which can tolerate salinity. Lentacker (1991) found a large number of these shells in the collections from Arruda and Amoreira. Moita skeleton No. 3 was buried on a bed of unopened *Tapes decussata* (a bivalve mollusc).

Because it is at the interface of terrestrial, marine and freshwater environments, an estuary has outstanding richness of resources. But with the changes through time indicated in Figure 2, there is likely to have been pressure on the population to move away from the Muge into the uplands where the reduced resource diversity would have encouraged changing to Neolithic life-ways, as happened elsewhere in Iberia (Straus et al., 2000:14).

With a warm, more arid, climate and lower tidal activity, the possibility of toxicity increases: diarrhetic shellfish poisoning (DSP) is most likely to occur with low rainfall and high salinity during the summer (Vale and Sampayo, 2003). Furthermore, the combination of lower tides, higher temperatures and greater aridity would have increased the possibility of other diseases.

The Tagus valley with its marshes and rice growing areas, established over many hundreds of years, was an area of high malaria incidence until after World War II (Bruce-Chwatt and de Zulueta, 1977; Bruce-Chwatt, 1988; Howorth, 1988). That, of course, does not mean that malaria was present prior to European contact with tropical regions and the importation of *Plasmodium falciparum*. Iberia is considered to have historical evidence of malaria by the 11th century AD (Bruce-Chwatt, 1988:12). There is, however, a growing body of evidence for the historical presence of indigenous European malaria, even in the northern parts of the continent and in the British Isles, irrespective of contact with imported tropical diseases. The existence of indigenous European malaria has been documented (e.g. Hackett, 1937:201–231; Bruce-Chwatt, 1988: 12–16; Huldén et al., 2005) and has been studied in great detail in the salt marsh regions of south eastern England (Dobson, 1997). Estuarine stagnant waters were the major factor in increased death rates in these areas. *Anopheles atroparvus* breeds in such areas (Reiter, 2000) and serves as the vector for the transmission of *Plasmodium vivax* (but is not susceptible to *P. falciparum*, see also Bruce-Chwatt and de Zulueta, 1977 and Bruce-Chwatt, 1980:99). Steadily warm temperatures, such as those which occurred during the Mesolithic occupation of the Muge are an essential factor: ". . . .as temperatures climb so will the rate at which mosquitoes develop, adult mosquitoes will feed more frequently on blood (and so pick up and pass on the infection faster) and malaria parasites will develop quicker in the mosquito" (Lindsay and Joyce, 2000). Historical records demonstrate that summers with low rainfall led to autumns in which "the ague" was most prevalent in the estuarine swamp areas of England (Dobson, 1997:323). Relapse and latent primary infections could also cause spring deaths (op cit. 325, see also Paul et al., 2004). As documented by Dobson, clearly described periodic tertian fevers occurred in the spring and autumn, intermittent illnesses which are consistent with infection by *Plasmodium vivax*, which causes fever, chills, headache, weakness, vomiting and diarrhea. *P. vivax* is milder than *P. falciparum* malaria (though still capable of causing anemia[5]): nevertheless, the documented differential death rates between areas of estuarine swamp and the uplands in historical England is extremely significant.

While the most severe form of malaria (caused by *P. falciparum*) is likely to have been absent in Europe until around 2500 years ago (Tishkoff et al., 2001), it is no more than a guess that milder forms of malaria did not enter Europe until domestic animals were present. Indeed, the absence of domesticated mammals is very likely to increase malaria rates in human beings, since *A. atroparvus* is zoophilic, i.e. would bite animals in preference to human beings (see e.g. Hackett, 1937: 230; Bruce-Chwatt, 1980:110 and passim.; Kuhn et al., 2003). However, if Mesolithic groups constructed some form of shelters and used fire to warm themselves during the winters (as seems likely from evidence at Moita), then, in the absence of domestic animals, they are likely to have been subjected to the attention of *A. atroparvus*. Bruce-Chwatt (1980:125–126) states that *A. atroparvus* is common in Portugal and the basic Iberian vector, indicating that Iberian malaria was not, in fact, necessarily caused by *P. falciparum* throughout history.

Changing conditions, dwindling resources as the salt marshes disappeared, and increased possibility of disease would have decreased health and fertility and made the Muge valley unattractive to Mesolithic groups who were no doubt already knowledgeable about the attractions of neighboring regions: the lower estuary of the Tagus or the Sado, the sea coast, the adjacent uplands. The depopulation of the Muge at the end of the Mesolithic was predictable. But where did they go? Calado (2002) has suggested a connection between the Tagus tributaries and Sado estuarine Mesolithic midden sites and the uplands of the central Alentejo which were more or less empty of people at the time when occupation of Moita began.

Across the Tagus, to the north, elements of a Neolithic lifeway were already becoming established in Portugal. Despite limited evidence, this is documented by the data from the isolated inland cave of Caldeirão where a date of over 7000 calBP overlaps the younger Muge dates, and Casa da Moura, for which the oldest date overlaps the youngest Arruda date. In the Upper Alentejo there are very early dates possibly associated with megalithic monuments (Oliveira, 2000). The most conservative date (6210 ± 50 bp) would place these at 7000–7200 calBP – a little younger than the oldest Caldeirão Neolithic date. But, as pointed out by Jorge (2000:59), these dates must be treated with caution. Further south, there are sites which indicate coastal and estuarine occupation with continuing "Mesolithic" activities at the same time as the youngest Arruda date (Lubell et al., 2007), for example, one of the two human burials at Samouqueira and a well-provenanced mammal bone at Fiais dated at ~7100 calBP. Gonçalves (2003) provides us with many instances of newly excavated or dated Early Neolithic sites throughout Portugal. For example, at Cabranosa and Padrão (Cardoso and Carvalho, 2003), in the extreme south west of Portugal, we find Neolithic sites with Cardial ware dated on shell to between 6540 and 6870 calBP[6], again contemporaneous with Samouqueira and the latest Arruda date (both calibrated with the reservoir effect for a diet very heavily based on marine resources).

In other words, it appears likely that Portugal at around 7000 calBP was a mosaic of different late Mesolithic/Early Neolithic influences.

Oosterbeek (2004) has pointed out that even along the immediate drainage of the one river (the Tagus) within the one period/culture (the Mesolithic during the time of the first burials at Moita), there is variation. How much more likely, then, that during the late Mesolithic and early Neolithic, groups were doing different things in different places. We have previously emphasized Neolithic and Mesolithic physical heterogeneity (Jackes et al., 1997; Jackes and Lubell, 1999b). Those populations living along the Muge seem to have found it necessary to disperse, for reasons which we have suggested above. Those living further upriver along the Tagus stayed and began to display some Neolithic cultural elements (Oosterbeek, 2004): the difference between the two areas may well relate to the particular circumstances of the Muge salt marsh alterations. A simple unilineal approach to this most important and complex period of human history has never been less acceptable.

It is likely then that we can only guess whether our samples from central Portugal do actually tell the full story about the Mesolithic-Neolithic transition in Portugal. At best, the picture will be incomplete. We have seen that it is likely that the Mesolithic resources of central Portugal during the Mesolithic could sustain a reasonably high and probably increasing level of fertility. What do the human skeletons tell us?

The site of Moita

Moita do Sebastião was excavated first in the 1880s and then, as a rescue excavation, from 1952 to 1954. In the earlier campaign, skeletons were excavated in 1880 and again in 1885: Jackes and Alvim (2006) have discussed the documentary evidence on the 19th century excavations. The 1950s excavations by the Abbé J. Roche and O. da Veiga Ferreira came after bulldozing and construction had revealed additional human skeletons.

Jackes and Alvim (2006) have reconstructed the relationship of the various excavations to each other and to the original topography of the mound. The location of the 1880 excavation has been pinpointed to the centre of the northern margin of the conical mound which rose to an estimated height of 24.5 m ASL on the south bank of the marshy course of the Muge. The excavation lay within encircling paths leading from the high point of the mound down across the river flats to the Amoreira bridge over a drainage ditch into the Muge. The paths no doubt detoured around some feature, which we can speculate was the location of post-Mesolithic disturbance. Roche (1972) described the mound surface before it was bulldozed as much disturbed. In 1880, Roque plotted a quarry pit on the south of the mound (Jackes and Alvim, 2006) and Paula e Oliveira (1889) noted that a concentration of post-Mesolithic pottery was found – no doubt in a large depression – in the surface layers in 1884.

In 1885, the excavations were continued along the north east face of the mound and this trench was redefined in 1954 by Roche and Veiga Ferreira. The reconciling of the 19th century and 20th century records with aerial and satellite images has allowed the "reconstruction" of the mound and the placement of the excavations (Jackes and Alvim, 2006), thus making it possible to assess whether the entire burial area was excavated and whether skeletons were lost to bulldozing in the early 1950s during the removal of the mound summit. The conclusion must be that, by the end of the excavations in 1954, the mound was completely searched to the level of the bottom of the midden deposits and that little in the way of human material would have been lost, although the faunal evidence is no doubt compromised by the removal of the layers which would have overlain those in which – in general – the skeletons lay. It seems that we are justified in stating that we have something close to the totality of skeletons originally buried at Moita.

The 1880 skeletons lay at the base of the archeological deposits and this was also true of the 1950 skeletons, which were found at the level of the basal sands. The same pattern probably held for most of the skeletons excavated at Arruda in the 19th century, and for all those excavated at Arruda in the mid-20th century (Jackes and Meiklejohn, 2004). This, however, is not true of one Arruda skeleton from a recent exploratory excavation (Roksandič and Rolão pers. comm.). Nevertheless, most of the evidence from Arruda, and from Moita, is that the skeletons lay at the same general lower level rather than being randomly scattered throughout the midden deposits. At Moita, that level was at about 21 m ASL.

Furthermore, the horizontal as well as the vertical placement may have been patterned. The Moita skeletons lay in clusters arranged in a rough semi-circle to the north, east and south of the mound centre (Jackes and Alvim, 2006), and the clusters formed a horseshoe shape opening towards a series of features which Roche (e.g. 1989) interpreted as a domestic structure. This interpretation can be questioned, especially as the skeletons would have been placed at the general level of the structure and in close proximity around it. The majority of the youngest children lay in pits placed to the south west of, and adjacent to, the structure. However the "structure" is to be interpreted, there is no doubt that a complex array of post holes, pits, shaped pebble concentrations and hearths was found in association with the Moita burials (Roche, 1972).

Material from the excavations of the 1950s

In dealing with Moita, the easiest part should be assessment of the skeletons from the 1952–54 excavations. Much information has recently become available. The publication of Veiga Ferreira's notebooks and sketches, together with photographs not included in Roche's publications, has been a very important addition to our knowledge (Cardoso and Rolão, 1999/2000). Newly discovered unpublished photographs (Jackes et al., in prep. a) have added a good deal of information on the skeletons excavated in 1954. The majority of the Moita material excavated by Veiga Ferreira and Roche was deposited in Porto at the Mendes Corrêa Institute of Anthropology, but six skulls remain stored in Lisbon in the Geological Services Museum.

An inventory of the 1950s material has been drawn up for this paper, derived from photographs and sketches made during excavation and from the Roche material still in Lisbon, together with the records made by Ferembach (1974) and by Meiklejohn in 1969 (numbers 1–18 only are in the CM Porto inventory), as well as a record made by Jackes and Huet Baçelar in 1984 when a search was undertaken to locate all the Muge material and labels that had been salvaged from a 1974 fire in Porto. Further work is being undertaken by Eugénia Cunha at the University of Coimbra to which all the Porto material has now been moved. In this paper, we are deriving our numbers of individuals from the original records up until 1984 and from Jackes et al. (in prep. b). However, even this well-recorded material furnishes us with difficulties, as discussed in Appendix I.

Material from the excavations of the 1880s

In trying to gain an accurate assessment of the Moita material from the 1880s, we meet greater problems. No full study of the skeletal material was attempted until the work of Ferembach who published a cursory inventory (1974). Added to that are complexities of the labeling and identification. The material is labeled from 1 to 60, A to Z and I to XLI, but in no case is the series complete. The Roman numerals are meant to designate the skulls, but this does not seem to have been true in the 1920s, since Vallois (1930) used Arabic numerals for skulls.

The skulls are meant to be associated with burials, but there is no consistency in the pattern of numbering. Vallois (1930) noted that most skeletons were accompanied by a card specifying the associated skull. However, he stated that in every case but one there was a discordance with the card on the skull specifying the associated skeleton: the exception was Skull 16/Skeleton 8 (Vallois, 1930:365). The confusion was exacerbated by the fact that, in a number of cases, there were elements of several skeletons stored together under the same number. The problems we faced in 1969 (CM) and later in the 1980s (CM and MJ) were already patently evident by the time Vallois examined the collection in the late 1920s.

The problems are similar to those we described for Arruda (Jackes and Meiklejohn, 2004). The situation is perhaps less acute, but nevertheless includes difficulties deriving from burial practices, from curation around the time of excavation, and from the subsequent history of the collection. Naturally the situation with the material deriving from the excavations in 1880 and 1885 is more problematic than that from the early 1950s. Vallois (1930) noted that the death of Paula e Oliveira no doubt led to a loss of information on the 19th century material. In fact, Ribeiro and Roque, the original excavators of Moita in 1880, both died before the second period of excavation in 1885 was undertaken by Paula e Oliveira (Jackes and Alvim, 2006): as a result there was a double loss of continuity. Using archival material, Jackes and Alvim (2006) provide new evidence of the conditions under which the 1880 excavations were undertaken. In 1880, at least 16 skeletons were found heaped together, so that the excavator had no idea how to approach the problem. The 1880 problems were such that although Arruda was photographed, published (e.g. Ribeiro, 1880) and proudly displayed to prehistorians attending the International Congress for Prehistoric Anthropology and Archeology (CIAAP) in Lisbon that year, details of the Moita excavations have remained unknown until recently (Jackes and Alvim, 2006).

There are some published descriptions of Moita material from the 19th and earlier 20th century, mostly focusing on supposed differing Muge skull types and recording disagreements as to whether the differences are real or simply the result of post-depositional distortion and inaccurate reconstruction. Illustrations of some of the skulls under discussion were provided by Paula e Oliveira (1884), Cartailhac (1886), Vallois (1930) and Ferembach (1965) allowing us to identify most as being from Arruda (see Jackes and Meiklejohn, 2004). Of present interest in these publications is passing mention of the nature of the collections and their curation. For example, two Moita skeletons had no skulls (Paula e Oliveira, 1881:10 ftn 1; Cartailhac, 1886:314–315); 25 Moita skulls were catalogued in the Geological Services of Portugal Museum in Lisbon in the 1920s but there might have been about five more (Vallois, 1930:356–7) some of which Ferembach (1965:269) reconstructed herself. Vallois stated (1930:357) that it was actually impossible to determine the number of Moita skulls present in Lisbon. He recorded that skulls were lost, or had no numbers, or did not have the numbers by which they had been previously described in Mendes Corrêa's publications. Mendes Corrêa (1923) even doubted whether one Moita skull was Mesolithic and noted that museum employees were unable to prove to him that other material was verifiably from Muge[7].

Thus, previous publications on the 19th century Moita collection have not provided us with much assistance in trying to assess the number of individuals excavated from Moita. The most complete description we have of the 19th century material is of crania (Vallois, 1930)[8].

The majority of problems no doubt arise from the simple moving of pieces from one individual to another within the collection. Drawers containing individuals usually had a paper label lying with the bones, but most individual bones were not specifically numbered. As a simple example, a child accompanied by the label "No. 47" had two right mandibular rami in the 1980s – one appears to be the same age as the isolated left mandibular ramus of the child identified as No. 46.

Many of the problems are more complex and unfortunately involve the very youngest individuals, who appear to be under-represented within the samples from both the 1880s and the 1950s. Appendix II discusses some of these problems and the rationale for decisions we have made when trying to count the number of young children.

The question of whether there was selective burial of children in one area of the site is very important for paleodemography. There is no indication whatsoever in the 19th century accounts of Moita and Arruda that there were concentrations of juvenile burials. Indeed, Cartailhac (1886) recorded a mother and baby buried together at Arruda and an 1880 sketch map of Arruda showed that just one child lay fairly close to a general area of scattered adult burials (Alvim et al., in prep.). At Moita, children were found among adults by Roche: No. 6 was a 2 year old, according to Ferembach (1974), and a small infant was found with No. 7, while Skeleton No. 13 was accompanied by fragments of two children, 12 years and 3 years. And yet, Roche excavated an area at Moita which was obviously dedicated to child burials: the juvenile skeletons Nos. 22, 23, 24, 25, 26 and 27 all lay together in an area of the site some way removed from the main concentrations (Jackes and Alvim, 2006). Ferembach gave these ages of 2 years, 10 months, 18 months, around 12 months, around 3 years and circa 7 months. The skulls of 23 and 24 were still on open display in Lisbon until sometime in the 1990s and they were studied both by Ferembach (1969) and by Meiklejohn (although they were no longer labelled with those numbers by the 1980s – one was given another number and one had no number).

While Roche recorded an area for juvenile burial at Moita, the sites were obviously mixed to some extent. This is an important point with regard to Arruda which has been incompletely excavated: could there be an area of Arruda child burials yet to be discovered? In fact, we have found that the Arruda fertility level is higher than that of Moita (Jackes and Meiklejohn, 2004), meaning that the ratio of juveniles to adults is higher among the Arruda skeletons, but there is no evidence whatsoever that this is the result of the excavation of an area in which many children were selectively buried.

It is clear that we have too few young children and that they are the most problematic individuals in our attempt to count the Moita dead with accuracy. However, by including or excluding individuals under 5 years of age we will not be altering our conclusions, in that our method acknowledges the frequent under-representation of young children and excludes them from the analysis. However, it is important to know whether we have a full representation of older children and of adults. And a major issue here is related to burial practices and to the completeness of excavation rather than to taphonomy or post-excavation curation. Both Moita and Arruda have problems in this regard and we have attempted to describe and confront these problems (Jackes and Meiklejohn, 2004; Jackes and Alvim, 2006) in order to justify our method of the age at death counts for paleodemographic reconstruction of Mesolithic populations at Muge. However, excavation and post-excavation history both add to the problems and have led to our choice of counting the dead through detailed examination of the mandibles. It is, of course, also dictated by the necessity of undertaking *absolutely comparable analyses* of the Mesolithic sites with the material from the Neolithic ossuary cave at Casa da Moura.

The method chosen for establishing the numbers of individuals has been outlined and justified fully, most recently with regard to our complete reassessment of Arruda paleodemography (Jackes and Meiklejohn, 2004). Seriation of mandibles with particular emphasis on the cheek teeth ensures that the skeletal elements with the highest probability of preservation, careful excavation, retention and identification in museum collections are used for a count of individuals in cases where there is a chance that skeletons were mixed, disturbed, or partially discarded.

Our re-examination of the historical records on the excavation and subsequent curation of Arruda confirms that some skeletal material excavated from the 1860s to the 1880s was indeed mixed within the deposits and some

was discarded upon excavation. We can expect that the Moita skeletons underwent the same fate, especially because the burials excavated at Moita in 1880 were heaped together much more closely than were those at Arruda. By concentrating on the mandibular elements, we are using material which is least likely to be discarded and also taphonomically and diagenetically most likely to survive.

Furthermore, mandibles provide the most detailed approach to the problem of coming to the best estimate of the number of individuals in a site – an estimate higher than that provided by, for example, right distal humeri. The reason for this is that one does not have to restrict oneself to the single most frequent side, but can use detailed fragment comparisons based on attrition, pathology and metric and non-metric morphology.

TRANSLATING THE NUMBERS OF DEAD INTO DEMOGRAPHIC VALUES

What was the fertility rate in Central Portugal between 6,000 and 8,000 years ago?

Information on the age distribution of the mandibular dentitions used for the present analysis is shown in Table 1. The distributions for Arruda and Casa da Moura are the same as those published in Jackes and Meiklejohn (2004), but that for Moita has been altered based on the complete reassessment of the material, especially of the material excavated by Roche and Veiga Ferreira, because additional information on the excavations is now available, as discussed above.

Why do we use these age categories? Standard five year age categories are desirable so that comparisons can be made with model and

Table 1: Age distribution of the dead

	Moita	Arruda	Casa da Moura
age	*n.*	*n.*	*n.*
0–4	11	17	42
5–9	7	9	31
10–14	1	5	33
15–19	2	4	18
20–25	10	8	64
25–x	54	62	152
Total	85	105	340

historical data. As mentioned above, we cannot calculate infant mortality since the under-representation of infants, especially in Neolithic sites, is extreme – because of taphonomic reasons, because of differential burial and/or because of poor excavation or curatorial methods. We are likely to have an under-representation of children up to age 5, in fact, but by that age we have sufficient and well-preserved dentitions to be able to make a reasonable estimate of age (we use radiographs in most cases). It would be preferable to have complete skeletons in order to examine diaphyseal lengths of long bones and development of epiphyses, but it will be clear that this is not possible in Neolithic cave ossuaries such as Casa da Moura, and is unlikely in the Muge sites under the conditions of excavation and curation that prevailed in the 19th century.

Figure 3 shows that the Neolithic site, Casa da Moura, differs from the two Mesolithic sites in the distribution of the dead. The under-representation of the very youngest age category is obvious. Since loose teeth make up part of the sample, there is a possibility of some errors of attribution at age category margins and this may explain why the Casa da Moura juvenile mortality curve does not have the shape that we might expect. The use of summary values in making comparisons (e.g. the mean of childhood mortality values calculated from the age at death distribution and the ratio of children to adults in a cemetery sample) allows us to avoid such finer-grained but error-prone detail. More problematically, Casa da Moura older adults are likely to be under-represented in the loose dental sample (see below). Since the Casa da Moura sample size is very adequate, in archeological terms, we will assume that the Neolithic data bears some relation to reality.

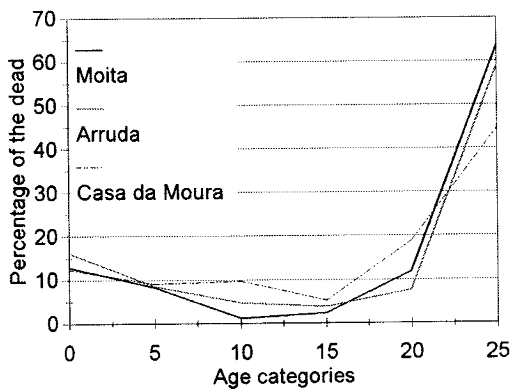

Figure 3: Age at death distributions across standard 5 year age categories, with all adults 25 and over grouped, for three Portuguese archaeological sites

However, it must be understood that there are many unknown factors regarding cemetery populations: excavation, curation, taphonomical factors, osteological methods, cultural variations deriving from the distant past, burial practices – all these introduce such uncertainty to our subject that we must beware of ascribing precision to our results. Since we have stressed in the past (e.g. Jackes and Meiklejohn, 2004), and again make clear, that our samples have many sources of error, it is worthwhile asking whether they are sufficiently robust for us to be sure that the age at death distributions are actually different from each other. The Index of Dissimilarity[9] and Kolmogorov-Smirnov two tailed exact significance in Table 2 demonstrate the similarity of the age distributions of the two Mesolithic samples and their difference from Casa da Moura (the difference is more extreme in the case of Moita and Casa da Moura). The differences among the samples are therefore robust. This is particularly important because Moita has a sample size of only 85. Subadult:adult ratios, which we will use below, have been shown to be ineffective analytical tools when sample sizes are below 100 (Paine and Harpending, 1996:153). MJ has, since 1984, developed a data base of nearly 200 archeological and historical standardized age at death distribution tables with sample sizes of 100 or more. This has given us a very clear picture of the valid range of our demographic estimators (subadult:adult ratios), of their relationship to each other, and of the nature of those very few cases which fall far beyond the normal range. Since Moita falls well within the expected range, and all the characteristics of the distribution (for example, the shape of the mortality quotient curve) appear normal for archeological material, we have chosen to use the site in our analysis.

How can we translate our rough approximation of age at death distributions, derived from dentitions, into demographic terms? We need to put it into a format that will allow us to compare among the sites and – above all – assess whether the data make sense in biological terms. We need to be able to translate this into fertility estimates and assess their validity. The technique which we will use allows us, for example, to verify that the small

Table 2: Testing differences among age at death distributions*

	Moita	Arruda	Casa da Moura
Moita	–	0.086	0.194
Arruda	0.395	–	0.182
Casa da Moura	0.004	0.019	–

*Index of Dissimilarity (above the diagonal) and Kolmogorov-Smirnov two tailed exact significance (below the diagonal) and incomplete Melides sample mentioned above, which comprises burials in two caves of apparently differing phases during the Neolithic, should be ignored. It shows us that the 95% confidence limits of the estimates for total fertility (TF) using two methods would range from 18 to 22. This would mean that the *average* Melides woman during her reproductive period would have around 20 children, clearly beyond the bounds of biological probability (as discussed in Appendix III).

The technique used has been described in detail elsewhere (e.g. Jackes and Meiklejohn, 2004 and Jackes et al., in press). Our method has been to use the index of juvenility (the juvenile adult ratio or J:A – the ratio of juveniles aged 5–14.99 years to adults aged 20 and above) first proposed by Bocquet-Appel and Masset (1977, 1982) against the mean of the apparent mortality quotients for the three age categories from 5 to 19.9 years (MCM or mean childhood mortality quotient; Jackes and Lubell, n.d.; Jackes, 1986). We have a reasonable idea of the relationship of these two values, which we use as demographic estimators, based on our archeological and historical samples discussed above. Our next step is to find their relationship to fertility. Briefly, total fertility rates (TFR) are derived from the model data of Coale and Demeny (1983: 51 tables) and the United Nations (1982: 3 tables) While the use of model tables is not ideal, there is no appropriate large body of actual data on the relationship between juvenile to adult ratios and the total fertility rate. The Coale and Demeny tables were based on empirical data strictly assessed for accuracy and our use of them is restricted to the proportion of subadults to adults. It should be reemphasized that we are not directly using any Coale and Demeny fertility estimate, but have calculated estimates from the pooled sex model life tables, adjusted for the male/female ratios. While the thrust of the argument here is the *relative* fertility among the sites, and this can be accommodated by the use of estimates derived from model data, there is some assurance that our estimates of fertility levels are plausible (see below).

The juvenile adult ratios and mean childhood mortality values for these model data are used to generate fertility estimates by prediction (fit for both TF with J:A and for TF with MCM from SPSS 12.0 curvefit quadratic) as described in Jackes et al. (2006).

Recently, Bocquet-Appel (2002) has proposed the use of P (5–19/5+), which avoids many of the problems of age assessment of juveniles and subadults. It is basically the equivalent of MCM. Why would we wish to retain values that are subject to age assessment errors? In fact, the use of the age categories around age 20 gives the anthropologist an incentive to be extremely careful in checking the data. We often assume that we can tell a 19 year old

from a 20 year old, and yet this is a very problematic age range. The need for great care lies in the fact that paleodemographic reconstructions have often had a very large number of people supposedly dying in their late teens or their early twenties. This was believed to be characteristic of archeological populations, since Weiss (1973) published model tables which he had developed from 50 sets of data, all but 14 of which were archeological. Figure 4 shows the variations of Weiss's age at death distributions from age 15 onwards (ages 0 to 14 varied according to a separate but comparable pattern). It is surprising that such age at death distributions were accepted without question. We can only assume that the real reason for the appearance of such curves in the paleodemographic literature is that there must be mis-aging at category margins. The implications of this sort of mis-aging for demographic values will be further discussed below.

Figure 5 shows the relationship of total fertility to our demographic estimators, using the limits of those estimators derived from our data base of samples of 100 and more (see above). The correlation coefficient is very slightly higher with MCM ($R^2 = .99$) than with J:A ($R^2 = .987$), but they are basically equivalent. MCM is more dispersed than J:A in our large comparative data set of archeological age at death distributions and we would choose to emphasize the TF estimations from J:A simply because MCM may be more error-prone. MCM is more dispersed than J:A in the comparative data set because it tends to fall too high in some archeological, but not in the historical, samples. In other words, MCM is too high for J:A in some of the archeological samples, and the samples in which this discrepancy occurs are those with excessively high late adolescent mortality. An extreme example

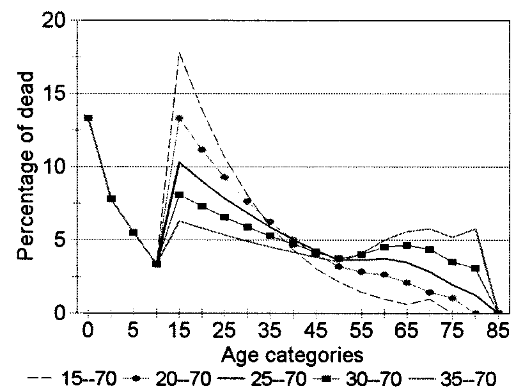

Figure 4: Range of adult age at death distributions for Weiss model tables demonstrated by the use of five selected tables (Weiss, 1973)

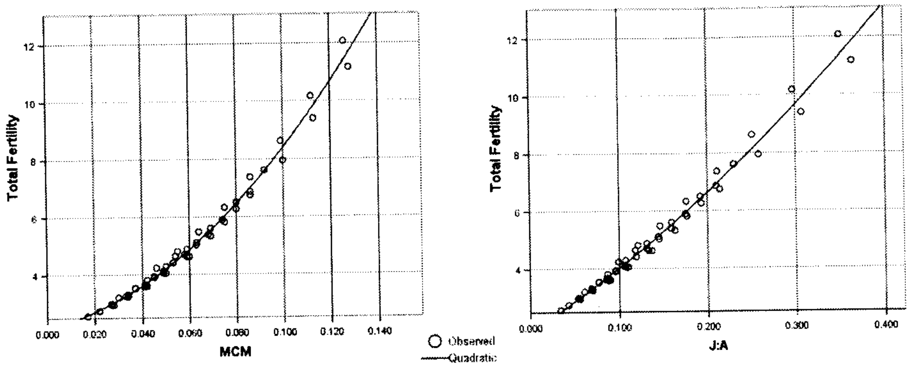

Figure 5: Total fertility derived by calculation from 51 West model tables (Coale and Demeny, 1983): total fertility and mean childhood mortality and total fertility and the juvenile adult ratio

of this would be provided by the Weiss model table 15–70 (MCM = .124; J : A = .171) (see Figure 4). At J : A = .170 one would expect the MCM to be much lower, under .100. The significance of the discrepancy can be shown by pointing out that from J:A, the Weiss 15–70 estimate is that the average woman would have many fewer children (TF = 5.4–6.4, 95% CL) compared with the estimate from MCM (TF = 10.7–11.7, 95% CL). The full life table TFR is 5.87.

Table 3 presents fertility estimates for the Portuguese archeological samples using both estimators, together with the 95% CL range of the predictions. But do the TFR range estimates provide adequate proof that there is no overlap between Mesolithic and Neolithic fertility? Greenwood's formula for the standard error of the survival function was employed to test this. The last two columns of Table 3 give TFR estimates calculated from the survival function error range derived from Greenwood's formula and

Table 3: Ranges estimated for total fertility rates from 95% CL of predicted values and using Greenwood's formula for the survival function standard error

	TFR range predicted from 51 West model tables 95% CL		Range for TFR predicted from the SE of the survival function using 51 West model tables	
Site	MCM	J:A	MCM	J:A
Moita	3.6–4.5	4.1–5.1	3.6–4.4	4.2–5.2
Arruda	5.4–6.3	6.1–7.2	5.3–6.6	6.0–7.5
Casa da Moura	8.2–9.1	9.0–10.1	8.0–9.3	8.9–10.4

indicate that the ranges provided by the 95% confidence limits of the Coale and Demeny predictions are adequate. It is possible to go further, giving the 95% confidence limits of this standard error range: once again there is no overlap between the Mesolithic and Neolithic TFR estimates, and the overlap between Moita and Arruda is minimal.

It must be understood that the figures provided in Table 3 are presented as indications of relative rather than absolute fertility. TF may overestimate fertility (Terrisse, 1986, but there is no real substitute, Bongaarts and Feeney, 1998); the true sex ratio is unknown, so we are simply assuming a 1:1 sex ratio. A very important point is that Casa da Moura, with an MNI estimated directly on loose teeth in some cases, must present an age at death distribution which has an inbuilt error, one that is likely to increase the estimate of fertility. The reason for this error, as mentioned above, is that it is certain that adults of advanced age must be under-represented in the loose dental sample.

Discussion on fertility estimates

Upon what should we base our estimates of unknown factors of the distant past, basic factors such as fertility levels? We may choose historical data, but these may represent locations in which the population structure is altered by immigration. This would be true, not only of European settlements in the New World, but of cities like London and Geneva[10]. The effect of rural-urban immigration has long been debated (see e.g. Sharlin, 1978, 1981; Finlay, 1981) but the use of data from outside major cities is obviously preferable. Historical data deriving from small rural centers in which there is no underreporting of infant deaths and no out-migration of younger adults, so that every individual born in the community is recorded by age of death, would be an ideal, but unrealistic, expectation. The historical data which we will use below is, in fact, from a region of emigration. The absence of sufficient historical data, expressing all human mortality and fertility experience accurately, is, of course, the reason for the development by demographers of model tables.

We have chosen to use Coale and Demeny West model tables to generate fertility estimates. However, this choice could be criticized. As Brass (1971:88, 90) noted of Coale and Demeny tables, "extreme schedules.... are extrapolations to a greater or lesser extent... [and] the schedules constructed for very high mortality levels extend well beyond the range of the observed life tables..."[11]. We have therefore generated relational (logit) model tables (Brass, 1971) in order to test the comparability of our fertility estimates with those derived from an alternative method (see Appendix III). Paine and Harpending have previously tested tables developed using the Brass approach against Coale and Demeny model West tables and found that there is "little or no difference" (1996:152; 1998:235), but they stated that the Crude Birth Rate may be underestimated by around 5% by Coale and Demeny tables (Paine and Harpending, 1998:238).

It is worth repeating a point made clear in Appendix III, that it has long been recognized that there may be discrepant results in relational tables with regard to child mortality, requiring the use of a very specific standard (Zaba, 1979:81). *And we have no idea what standard should be used with archeological data.* As stated in Appendix III, we are using an historical French sample, Tourouvre, as a standard from which to generate relational tables because it represents the midpoint of our historical samples.

As shown in Appendix III, Coale and Demeny West model tables will provide fertility predictions that are comparable to those which can be generated by relational tables, using an historical sample, Tourouvre, as a standard (cf. Figure 5 and Figure 6). We cannot, of course, claim pinpoint accuracy: for example, both the formula used to calculate ${}_1L_0$ and the actual sex ratio of adults 15–44.99 are problematic under the best of conditions. The general accuracy of the approach by which Coale and Demeny West model tables are used to predict the TFR in archeological samples has, however, been tested in several ways. The basis of testing is 1) to see whether TFR predictions accord with table TF values, 2) to see whether there is independent evidence of those fertility levels, or 3) to consider independent evidence of relative fertility levels derived from the predictions.

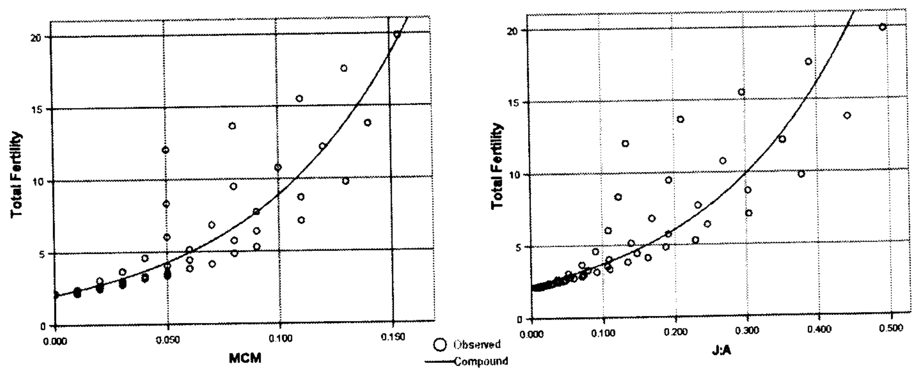

Figure 6: The relationship of TFR and MCM in 55 tables generated from the Tourouvre standard, $R^2 = .842$; the relationship of TFR and J:A in 55 tables generated from the Tourouvre standard, $R^2 = .831$

Firstly, we can use the three UN tables (General, Far East, South Asia) to test the tabled lower end of the range for the 51 Coale and Demeny tables. Secondly, we have used data from the !Dobe Kung (Howell, 1979), with a TF estimate of around four children derived from the J:A of the age at death distribution. In fact, the upper confidence limit accords with Howell's estimate of 4.3 for the period from 1963–1973. Low fertility has also been tested by use of US census data for the period 1901–1910 (the data was for white individuals resident in the original registration states; Glover, 1921:66–67, 72–73) and the results are consistent with other estimates of fertility derived from these data, especially for urbanized north-eastern whites (Haines, 1989). Thirdly, the middle range has been examined in very broad terms by considering ethnohistorical evidence on Iroquoian aborigines compared with historical records for French Canadian and northern French Europeans (for an early attempt at this, see Jackes, 1994; also Landry, 1993) with consonant results, given the many uncertainties. Finally, the upper end of the range has been checked by reference to American Hutterites of the period 1941–1950 (Eaton and Mayer, 1953:238, ages redistributed into standard age units). Summing the age specific fertility rates of all Hutterite women of reproductive age between 1936 and 1940 gives a TFR of 9.4, and for those of reproductive age between 1946 and 1950 the TF value was 8.1 (Eaton and Mayer, 1953:227). It is then very reasonable to estimate ~8.8 as the correct TF value to associate with those who died between 1941 and 1950.

Table 4 lists the TFR predictions from quadratic regression based on our 51 Coale and Demeny West model tables for the Portuguese age at death distributions. We compare these with the result predicted from our estimators by compound regression from relational tables derived from the Tourouvre historical standard, first using the full set of 55 tables and then a set of 50 tables in which the TFR is below 13. We choose the compound/growth regression methods which give equivalent results and provide the highest R^2 value. Based on the Hutterite data, the most accurately recorded historical data for high fertility, Table 4 shows that our general level of total fertility is estimated with some degree of reliability. Table 4 also demonstrates that relational tables can provide widely divergent results and must be used with caution. Thus we have chosen to emphasize results from West model tables.

Finally, Table 4 makes the point that our hypothesis of increased fertility in the late Mesolithic (an increase in the TFR of 2) and a greater level of increase in the Neolithic (perhaps 2.5–3) is proven to the degree that two quite separate methods of prediction agree on this.

Table 4: Comparison of archeological total fertility rates derived from two independent sources (model tables and relational tables), with an historical sample included as a control

	Prediction from 51 West model tables MCM	Prediction from 51 West model tables J:A	Prediction from 55 tables using Tourouvre standard MCM	Prediction from 55 tables using Tourouvre standard J:A	Prediction from 50 tables using Tourouvre standard MCM	Prediction from 50 tables using Tourouvre standard J:A	Prediction from 55 tables using Brass global standard MCM	Prediction from 55 tables using Brass global standard J:A
Moita TFR	4.0	4.6	4.1	4.2	4.0	4.1	4.4	5.4
Arruda TFR	5.9	6.6	6.0	6.0	5.7	6.0	6.7	9.6
Casa da Moura TFR	8.6	9.6	9.2	9.7	8.4	9.6	10.7	20.0
Hutterites 1941–1950 TFR	7.7	9.0	8.0	8.8	7.4	8.7	9.3	17.3

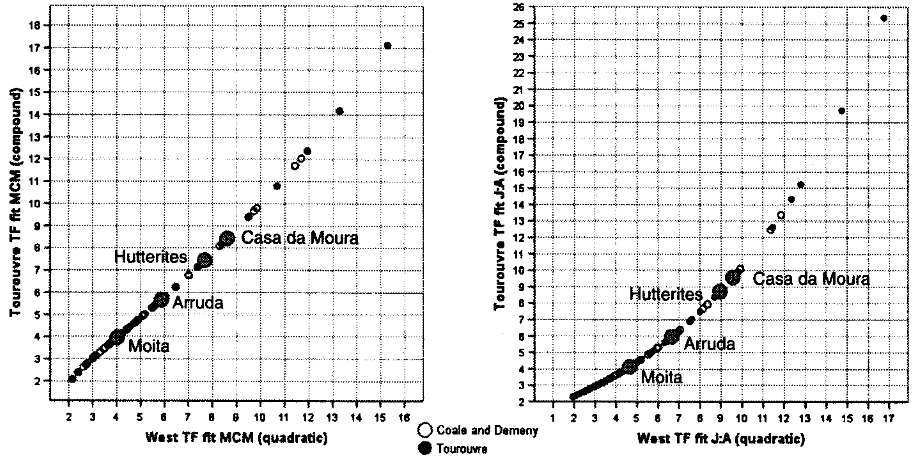

Figure 7: Comparison of fits for TFR from estimators, MCM and J:A, between West model tables ($n = 51$) and relational tables derived from the Tourouvre standard (the predictions were based on $n = 50$, $TFR < 13$, the results shown are the predictions for 55 tables)

Figure 7 gives us assurance that our general levels of fertility are likely to approximate reality since two different estimation techniques have arrived at very similar conclusions.

In fact, Figures 5, 6 and 7 indicate the likely limits of our estimators: by both methods J:A ~.300 will predict an average fertility of ten children[12]. This limit surpasses that of our historical samples and is close to the upper limit of all those archeological samples in our large data base which have sufficient sample size and appear to be unbiased.

DISCUSSION, SUMMARY AND CONCLUSIONS

Can we be completely certain we know how to interpret the Muge sites? The answer is no, in that they seem to be both special purpose and occupation sites. In fact, we have limited knowledge of them.

Moita appears to be the first of the sites in which human burials were placed. This occurred at a time when the saltwater influence on the Muge was established and increasing and while the climate was warming and becoming more humid. Pine pollen was decreasing, so the environs were becoming more open.

The burials at Moita (of which there were at least 85) all seem to occur at the same level – about 21m above the present sea level. They lay on basal sands and were mostly covered by a heavy breccia. In general, they appear to have been primary in-flesh burials, but with disturbance of earlier by later inhumations. They occurred in groups in a horse-shoe shaped formation opening onto some type of arrangement of pits and posts to the west of the burials. Burials were often accompanied by shells of one sort or another; pits of unopened *Scrobicularia plana* occurred in association with the pit and post features, and close to an area in which many very young children were buried, isolated from the other interments.

It is likely that the mass of the Moita faunal material, terrestrial and estuarine, occurred in the levels above the burials, but a great deal of this was destroyed when the deposits above the burials were removed by bulldozers in the winter of 1951–52. These deposits also included very disturbed post-Mesolithic layers.

It seems likely that many of the Arruda burials post-date those at Moita. Arruda has been only partly excavated and some of the excavated material has been lost, so the number of individuals excavated (more than 105, perhaps in excess of 150) is both estimated and incomplete. The burials so far known come from the south-east to centre section of the portion of the mound which faces the Muge. Again, the burials were generally at the level of the basal sands, though a few are known to have occurred at a slightly higher level. The burials were supine and in-flesh, although at least one child's skeleton must have been bundled. Again, 20th century excavations made it clear that earlier burials were disturbed by later burials, so that human skeletons may be found in various states of incompleteness or disarticulation.

Amoreira has also been partially excavated and a few burials were found there (Newell et al., 1979; Cunha and Cardoso, 2001). New work now underway has yielded several more (Roksandič, 2006). The site appears to have been used for inhumations in the last half of the period during which people were buried at Arruda, but further excavation may change this interpretation[13].

There were changes in the environmental conditions during the period of intensive use of the three excavated Muge middens as burial locations. After a peak of humidity, there seems to have been an increase in aridity and, at the same time, a diminishing tidal influence, probably accompanied by higher salinity levels in the marshes. We suggest that the later environment was less well tolerated by the estuarine molluscan species which formed an important dietary resource for the Muge population. Furthermore, an important source of vegetable food would have disappeared as the marshes matured into a vegetation zone more dominated by grasses and reeds. The possibility of shellfish toxicity would have increased in the hotter and drier

salt marshes. At the same time, there is an increased possibility of disease from mosquitoes breeding in the brackish standing water.

This concatenation of circumstances could explain why the Muge population apparently disappears and why evidence of the early Neolithic is found only in the adjacent uplands, around 80km to the south (Calado, 2002: Figure 2.2), predating the earliest Neolithic monuments of the Upper Alentejo (which appear ca. 6500 bp; Jorge, 2000:59)[14].

The Moita and Arruda burials provide us with some basis upon which to make estimates of the demography of the central Portuguese Mesolithic. The samples are not ideal, but they are reasonably large and they do not appear to be biased. From the demography, and from other biological anthropological evidence (as diverse as from mean stable isotope values, to dental pathology and wear, to femoral geometry), we postulate changes between Moita and Arruda, so that the Mesolithic of that time and place could not have been a static, unchanging period. Indeed, we do not see the environment as unchanging, so concomitant changes in the human population would be likely.

The paleodemographic evidence suggests that fertility rose during the late Mesolithic. But population increase could have been truncated – both by a worsening environment for the important estuarine species and by fertility-reducing fevers and higher infant mortality.

Caldwell and Caldwell introduce an unsupported contention (2003:210 – no prior discussion appears in the paper) that violence – apparently in addition to infanticide – served as a method of population control amongst hunter-gatherers. Could we be seeing an under-representation of adults resulting from deaths due to violence away from the normal burial grounds (cf. Jackes, 2004)? In fact, the under-representation of adults would serve to increase apparent fertility levels by our method of TF estimation. However, we see no osteological evidence of increasing violence in response to increasing fertility (Jackes, 2004). One Moita skull, Moita 20 from the 1950s excavations, appears to have depressed fractures (Antunes and Santinho Cunha, 1993 speculate on other Muge skeletal material), but proof of systemic violence requires a great deal more patterned evidence (Jackes, 2004) and the slight evidence available shows no increase from Moita to Arruda.

Caldwell and Caldwell (2003) argue that the upper limit of "natural" TF can be expected to be 7.5 (perhaps following Wilson and Airey, 1999), but that hunter-gatherer women raised an average of four children each, the differential apparently being explained by infanticide. We do not accept Caldwell and Caldwell's lack of emphasis on behavioral or biological restraints upon fertility (of the type discussed in Jackes, 1994). Nor is there evidence, if the population did not increase markedly, that there must have been infanticide or high childhood mortality. It is worth noting that excellent evidence on archeological fertility suggests that some horticulturalists were able to maintain a low birth rate with neither internal violence nor infanticide (Jackes, 1994; Jackes, 2004).

Since we are examining only those children over four years of age, we are not concerned with the numbers of infants and young children. There can be no doubt that there is infant and early childhood under-representation at the Muge sites. Whether this results from burial patterns, preservation factors (Bello et al., 2002), excavation or curatorial factors (cf. Jackes and Meiklejohn, 2004) cannot be determined. The representation is so low that we could not adduce low fertility as the reason for low infant mortality. On the other hand, infant mortality generally leads to a very limited increase in total fertility rates (Palloni and Rafalimanana, 1999). Infant mortality is an incomplete explanation for the relative numbers of juveniles and adults among the dead but we can avoid discussion of infant under-representation because we are examining only those children who survive beyond four years of age. What we can say is that children above four years of age are represented among the dead in such numbers (relative to adults) that the average Muge woman who completed her reproductive period would have had four to six children. Thus we propose a TF above the postulated four per hunter-gatherer woman and a slight increase in population. We see a Mesolithic population in which the diet was diversifying, the dental pathology was decreasing, and where there was perhaps a change in the level of sedentism (Lubell et al., 1994; Jackes and Lubell, 1999b). We have suggested, based on osteological evidence, that there was probably a reduction in the birth interval concomitant with increased sedentism and dietary change (Jackes et al., 1997). Supplementing the diet of nursing infants would reduce the period of lactational amenorrhea. While this would apply with most force to the Neolithic population, there could definitely have been a reduction in birth spacing during the late Mesolithic Muge occupation. We note the excellent food resources of the Muge saltmarshes, resources that were suitable as weaning foods.

It is important to emphasize here that we can discuss only the period of the Moita and Arruda burials – the situation may well have changed for the worse after 7000 calBP. In fact, we see the possibility of decreasing fertility caused by, for example, dwindling food resources and an increased likelihood of marsh fevers (the suggestion being of recurrent intermittent fevers with spring and autumn peaks) with the eventual abandonment of the

Muge valley. It is interesting that our most recent Muge radiocarbon date is for an individual from Arruda who has stable isotope values suggesting that he was, in fact, eating a marine rather than a mixed estuarine/terrestrial diet. The possibility is, then, that the later Muge people became less sedentary and ranged further across central Portugal. Migration into an empty land following a period of crisis might well have induced a rebound and some increase in fertility, an effect difficult to differentiate from that caused by increased sedentism and a change of diet (see e.g. Heuveline, 2003). But we can make no claim that Casa da Moura people had any relationship at all with the Muge – indeed, it is very unlikely, since the two groups would have been separated by a formidable geographical boundary (the Tagus River). Nevertheless, we have no alternative but to use Casa da Moura as our Neolithic exemplar – the sample size is good even though the identification of individuals from loose teeth is, of course, less than ideal. But the important fact is that the earliest date we have for Casa da Moura Neolithic skeletons overlaps the latest date from the Muge burials.

We have a generally clear differentiation into Mesolithic and Neolithic lifeways based on the stable isotopes, we have well-dated and adequate (though not ideal) samples. We can, once again, affirm that central Portugal provides us with the best evidence for an increase in fertility after the introduction of the Neolithic, but that it is important to understand that fertility could and apparently did increase within the late Mesolithic. Furthermore, the exact cause of the fertility increase in the Portuguese Neolithic is not clear: we suggest that it is accompanied by a reduction in birth spacing (Jackes et al., 1997). Many other factors could be involved as sedentism will lead to alterations in social and economic lifeways, which would affect sexual behaviour and pregnancy outcomes.

We can only guess whether our samples from central Portugal do actually provide the complete picture of the Mesolithic-Neolithic transition in Portugal. We can guess at increased fertility but we cannot prove it. We have seen that it is likely that the Mesolithic resources of central Portugal during the Mesolithic could sustain a reasonably high and probably increasing level of fertility. An increasing population and an unfavorable change in the Muge environment would suggest movement into other areas, areas previously empty and available for settlement. It is thus reasonable to assume that, once Neolithic elements were incorporated, especially the increasing sedentism, this increase would have been maintained, just as is suggested by the Casa da Moura human remains.

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APPENDIX I

A central issue concerns the consistency of the record with regard to the 1950s excavations. In this we are dependent on the information of the two excavators, the descriptive work on the collection by Ferembach, and the independent inventories we have both made. Problems are obvious from the period during and immediately after the excavation. The information provided by Roche and Veiga Ferreira is sometimes contradictory. For example, Skeleton No. 14 is sketched by Veiga Ferreira with a skull, but was photographed and described by Roche as lacking all of the cranium except part of the mandible. Inventories of Ferembach, Meiklejohn and Jackes all confirm the lack of a skull. Similarly, No. 10 had a crushed skull according to Veiga Ferreira, but no skull was ever recorded in the Mendes Corrêa Institute in Porto. However, Roche mentions a mandibular fragment, so we can at least record the presence of a mandible.

Roche (1972) says Skeleton No. 2 was an isolated skull, but that was not true at excavation according to Veiga Ferreira, and this is clear from his sketch. Ferembach's publication on Moita (1974) suggested that the material labelled No. 2 might have been mixed during transport between Muge and the Mendes Corrêa Institute in Porto. Eventually parts of four individuals were mixed under the label of No. 2. By 1984, No. 2 certainly consisted of the postcrania of several individuals, both male and female, but neither skull nor mandible. However, a mandible and an unmineralized skull, the skull oddly discordant with other Muge material, were stored with Skeleton No. 1. This skull was labelled "2". Another skull labelled "83" was also present with No. 1. Neither of these skulls was stored with either skeleton in 1969 when Meiklejohn inventoried No. 2. Ferembach, however, noted a damaged mandible that had a label saying "A2" and Meiklejohn's record of a mandible with No. 1 in 1969 is likely to be the same one. Thus we record a mandible present for No. 2.

There is also a contradiction between Veiga Ferreira and Roche with regard to No. 4, but it is clear that this individual never consisted of more than a few postcranial fragments, at best. Ferembach (1974) actually refers to it as unidentifiable fragments.

Beyond the above we have material that comprised more than one individual at excavation. Those burials recognized as containing mixed bones at the time of excavation were Nos. 1, 7, 13, 18 and 20 according to Ferembach (1974); for example, No. 13 consisted of parts of two adults and two children. Roche noted that No. 1 had parts of two individuals, and No. 7 of three individuals, that Nos. 8 and 11 could be the same person, and that

No. 13 was made up of several people. Further, Veiga Ferreira suggested that individuals Nos. 8 and 11 were in fact parts of the same person. Added to this, we have material which consisted of indeterminable bone debris (Nos. 28, 29), and were certainly nothing which even the excavators could be certain should be recorded as human burials (Jackes et al., in prep. a). Ferembach also says that No. 20 consisted of two people, but it is possible that Nos. 20 and 21 were mixed after excavation, since she did not find No. 21 in Porto. Veiga Ferreira's sketch dated 15/5/1953 of Skeleton Nos. 20 and 21, as well as the photograph of them (Roche, 1972 plate vii.1), makes it clear that this is the most likely explanation.

The excavations of 1952–54 followed upon the bulldozing of the site in the winter of 1951–52: it is worth asking whether the fragmentation and mixing of some skeletons was a result of the bulldozing. However, it is clear that this was not so. The greatest number of mixed and incomplete skeletons (i.e. Skeleton Nos 1, 2, 7, 8 and 13) lay at or near the intersection of the two test trenches dug in 1952. Veiga Ferreira described this as the "centre of the mound": at that point there lay a concentration of skeletons in a central location with regard to the mound as a whole (Jackes and Alvim, 2006) and, in fact, potentially a focal point of ritual importance (Alvim, in prep.). It seems to have been Roche's opinion (1972) that a series of burials in the same small area caused disruption and fragmentation of the earlier skeletons. Certainly, we cannot argue that the bulldozer drivers destroyed skeletons, since photographs (Cardoso and Rolão, 1999/2000, Figure 28) make it clear that the skeletons in this area – for example, Skeleton No. 2 – lay below the surface left by the bulldozers. The common loss of the knee area in some skeletons (e.g. Skeleton No. 33; Jackes et al., in prep. a) appears to gives an indication of the level reached by the bulldozers. In the central location under discussion, Skeletons No. 5, 3 and 2, although also buried with knees tightly flexed on upright thighs, sustained minimal damage, suggesting that the bulldozers barely scraped the top of the skeleton level. Because the skeletons were covered by a hard breccia, and it was this breccia that stopped the bulldozer drivers from removing the lowest archeological levels, it is logical that the only damage was to the flexed and upright knees.

Since the mixed and incomplete state of some of the skeletons cannot be ascribed to bulldozer damage, it is clear that assessment of the number of individuals would require close study even were all the material in perfect condition. Unfortunately, post-excavation events have further complicated the matter. The majority of the material was transported to the Mendes

Corrêa Institute in Porto and there suffered during a fire and subsequent flood caused by the fire hoses in 1974. We have concluded that it is best to say that the Roche excavations uncovered 33 individuals. In this, we are accepting Ferembach's assertion that a three year old and a 12 year old were included with Skeleton No. 13. There were no doubt fragments of children present. Our more recent survey of the material was rapid and followed the disruption of the fire, so although we did not fully agree with Ferembach's interpretation of the material, we cannot insist on our conclusions.

APPENDIX II

Juveniles among the skeletons from the 19th century Moita excavations present particular problems. Ferembach (1974) records the fragmentary skull of a ?newborn infant numbered No. 38 and the skull of an adult female, No. 37. In 1969, Meiklejohn did not see skull No. 38 nor was it present in 1984. However, No. 37 was made up of numerous fragments, some of them of a child, and by 1984 the fragments numbered No. 37 included a juvenile tooth, but clearly not that of a newborn.

Meiklejohn's various inventories (1969, 1984, 1985–86) indicate that both adult and juvenile material was also migrating in and out of the group of bones labelled No. 39. Ferembach describes No. 39 as a three year old represented by a mandible. This was not present in 1969, and by 1984 No. 39 was represented by the fragmentary maxilla of a small child, associated with an adult mandible. The adult mandible now labelled No. 39 must, in fact, have had another association originally. We will accept No. 39 as representing a child for whom the mandible is no longer present, since there are loose mandibular teeth recorded amongst the materials discussed. An age of three years is acceptable for this individual.

Among the material mentioned above none appears to suggest a newborn, and we might therefore question whether to include one here. A young child, under three years, now without a number, has a left mandibular horizontal ramus, and there is also a full mandible, now called No. 45. In fact, No. 45 now consists of post-cranial fragments of three children, two of them under one year. Examination of the long bones present in the collection thus becomes necessary to our attempt to establish the number of infants. The juvenile long bones in the entire Moita collection do not suggest the presence of very young children other than those indicated by the dentitions, except infant bones labelled No. 7 and those now included in No. 45. We can say, without being certain of it, that Ferembach's mention of No. 38 as a newborn and our records of Nos. 7 and 45 suggest that at least one or two children present in this collection are under one year. And we can say with more confidence that there are three older children who are under five years. But this still does not, of course, provide a full representation of the expected number of children under five. There may have been more excavated: Ferembach mentions others, for example No. 41, a skull which was not present under that number in 1969 or in the 1980s. But we have absolutely no way of knowing whether No. 41 of Ferembach is associated with No. 45 of the 1980s. Ferembach (1974:23) records that 11 children (including all the problematic cases discussed here) were originally kept together in the same drawer in the museum of the Geological Services of Portugal.

APPENDIX III

We generated a random set of relational model tables (Brass, 1971), within the values of $\alpha = -1.5$ to $+1$, $\beta = .5$ to 1.5 (Zaba, 1979), but widened to the extreme of $\beta = 1.6$ to accommodate the tables generated by Brass himself (1971: 79–81). Our first set of relational tables uses Brass's global standard as the starting point and includes Brass's own 12 tables. The second set uses Tourouvre-au-Perche (a northern French village, 1670–1719: Charbonneau, 1970:194) as seed data with the same α, β values as used with the Brass global standard. The Tourouvre age at death distribution was chosen because it occupies a central position in a cloud of 32 historical samples, directly on the line expressing the relationship of our two demographic estimators in the historical samples. There is no claim here that northwestern European historical demographic values are representative of global fertility experience (they are apparently not, though long established ideas on internal homogeneity and bimodality of western and eastern European fertility patterns in the past are being challenged e.g. Lynch, 1991; Wetherell and Plakans, 1997). Social and environmental determinants of fertility (Jackes, 1994) make it very unlikely that we can rely on one specific standard. Nevertheless, we do have some idea of *biological* constraints which will allow us to set reasonable upper limits.

Deviations in both directions from biologically feasible levels of mortality at the lowest and highest ages are immediately obvious in the relational tables. There have been a number of attempts to modify

Brass's approach, most recently that of Murray et al. (2003), especially in order to deal with aberrant results at the lowest and highest ends of the age at death distributions. This is very marked for infants, but in fact the improbability of results is much greater for the final five year age category (with extraordinary coefficients of variation ranging from 212–256 in our tests on two different sets of 55 such tables – see below). The problem of such aberrant results constitutes a major flaw, and in many of our generated tables the demographic values at the youngest and oldest age categories are markedly outside biological probability. For example, virtually the highest known infant mortality (Liberian migrant extreme mortality, ${}_1q_0 = .747$, McDaniel, 1992) is exceeded or approached in a number of the relational tables, while in others, infant mortality values are well below those of ${}_4q_1$. Since our estimates of fertility derive from the infant C values, divided by the sum of the C values for women aged 15 to 44.9, any aberrance in infant values will lead to unreliable fertility estimates.

We have excluded from analysis the standard tables and also those six Tourouvre and three Brass global standard derived tables in which ${}_1q_0$ exceeds .800. We have further removed one Tourouvre table in which the TFR would be 21, that being an unrealistic average for fertility since that would imply – for the *average* woman – one child every $9+6=15$ months for 26 years, with no fetal wastage, no illness, no food shortages, requiring 26 full years of continuous sexual activity, no spousal death, no sterility, no anovulatory cycling, no celibacy, no pregnancy complications, no age-dependent changes in fecundity, and relatively early weaning. The effect of infant mortality will not overcome the biological improbability of such a high average fertility. The fact that we have included tables in our analyses which imply that the *average* woman had over ten children might lead to criticism from medical experts (K.M. Richardson, pers. com.). The effects of great grand multiparity (ten or more children) are difficult to assess since it is rare, and studies lack access to women without modern health care. The most recent review of information (Aliyu et al., 2005) certainly points to macrosomia, medical complications and placental pathologies under the best conditions.

We are left with 39 Brass standard and 36 Tourouvre standard tables to study. Appendix III Table 1 shows us that the standard chosen is crucial in determining the fertility levels. But Appendix III Table 1 provides further information. In order to test whether the α and β values chosen for use with the standard are also likely to be crucial, we generated a further set of 54 completely different tables (values for β ranging from .5 to 1.5 in

Table 1: Descriptive statistics for life table total fertility rates and infant mortality quotient coefficient of variation, show differences among sets of relational (logit) tables

	TFR mean	sd	min.	max.	$_{1}q_{0}$CV
Brass global standard: 39 tables	3.572	1.858	2.06	8.17	85.83
Brass global standard: 55 tables	3.257	1.559	2.04	7.93	109.24
Tourouvre as standard: 36 tables	4.744	3.450	2.10	15.50	75.29
Tourouvre as standard: 55 tables	5.509	4.352	2.10	19.86	89.34
Tourouvre as standard: 50 tables	4.454	2.790	2.10	12.18	93.52

steps of .5, while each β level has α values from −1.5 to +1 in .25 steps) from the Tourouvre standard (which will be included as α = 0, β = 1 for a total of 55 tables). The question of biological unlikelihood was initially ignored, but the Tourouvre set was then reduced to 50 tables by removing all those in which TFR > 13. We also generated 55 equivalent tables using the Brass global standard to retest the effect of the standard versus the levels chosen.

The Tourouvre (n = 55) TFR standard deviation shown in Appendix III Table 1 is large. This is partly a result of the fact that the infant mortality quotient ($_{1}q_{0}$) coefficient of variation is extremely high, there being several outliers among the values (cf. West tables, footnote 11 above). In fact, the relational tables' coefficients of variation for the estimators themselves are unacceptably high and Appendix III Table 2 shows that the Brass global standard and our various sets of Tourouvre tables have demographic estimator values far more dispersed than our sample of historical life tables (world-wide, though biased towards Europe or populations of European origin).

Table 2: Coefficients of variation show differences among data sets in dispersal of estimators

	MCM CV	J:A CV
Historical data: 32 life tables	26.063	29.120
Coale and Demeny: 51 West tables	44.435	54.951
Brass global standard: 39 tables	96.828	106.165
Brass global standard: 55 tables	95.424	102.860
Tourouvre standard: 36 tables	87.713	86.636
Tourouvre standard: 55 tables	80.719	93.453
Tourouvre standard: 50 tables	79.925	89.904

The relationship between the various estimates is not simple. While the differences between the TFR predictions for West (n = 51) and Tourouvre (n = 55 or 50) do not diverge strongly at low levels of MCM and J:A, the differences plotted against the estimators inscribe curves and so it is not possible to make blanket statements about whether one method or the other over- or under-estimates TFR. However, the 95% confidence limits for the Tourouvre tables are so broad that the differences are non-significant for much of the range: extremely discordant results between the methods accumulate markedly when estimator values exceed certain levels (MCM ∼.100 and J:A ∼.300), precisely those levels at which biological feasibility becomes questionable. The Brass global standard predictions are different. The UN tables from which the global standard was derived are closer to the West model tables than to the Tourouvre standard in terms of the relationship between the estimators: thus, the major divergence of the global standard is from Tourouvre generated TF values.

This is not an appropriate place to expand on the complex relationship among the estimators, the choice of standards for relational tables, the α and β levels and the West model predictions. It is sufficient to say that the Tourouvre (n = 55/50) predicted TFR and the West (n = 51) predicted TFR generally accord unless the tables are beyond the range of biological likelihood.

It is clear that both the standard and the exact details of how those tables are generated will determine predictions of fertility levels when relational tables are used. The relational table method cannot be used without some external check on results and this we have provided.

NOTES

[1] The Melides Zambujal sample is small, about 51, and is too late in time. Melides Lagares provides a smaller sample of about 29 for which, curiously, there are no dentitions retained (see also Nogueira, 1927–1930).

[2] Hereafter called Moita, Arruda and Amoreira. The sites are sometimes referred to collectively as Muge.

[3] This is the regional mean ΔR for Portugal published at http://radiocarbon.pa.qub.ac.uk/marine/ and based on Monge Soares (1993). Correction for ΔR follows procedures recommended in http://radiocarbon.pa.qub.ac.uk/calib/manual/chapter2.html#MARINEHELP.

[4] Cunha and Cardoso (2001) give a date of 6850 ± 40 for an Amoreira burial originally stored in Porto. The date, using the mixed marine/terrestrial curve, adjusted for a 50% marine diet on the basis of a $\Delta^{13}C$ value of −16.5‰, would be 7410–7450 calBP. Meiklejohn in 1969

and Jackes in 1984 both inventoried the material held in the Mendes Corrêa Institute of Anthropology in Porto and noted very few skeletons positively identified as excavated from Amoreira.

[5] Anemia has often been associated with cribra orbitalia or porotic hyperostosis. We found no cases of the conditions in either the Moita or Arruda samples.

[6] Zilhão (2001) used a different reservoir effect value and incorrectly calculated these dates in such a way as to make them 500 years earlier.

[7] When we started working on the human remains from Portugal, the Mesolithic provenance of the material excavated in 1952–54 was not in doubt. However the earlier material was far less secure (see discussion in Newell et al., 1979). Our first radiocarbon, stable isotope and SEM analyses were designed to examine whether variations in matrix and degree of mineralization were reason enough to question the Mesolithic provenance of some Moita skeletons.

[8] Vallois described eleven skulls using Arabic numerals and those skulls seem to accord with the skulls now designated by the equivalent Roman numerals. His only Moita photograph was of Moita 19, which is demonstrably Skull xix, though between 1929 and 1984 it had sustained marked damage. The skulls he described still bear the same numbers: vii, xvi, xvii, xviii (now lacking a mandible), xix, xxiii, xxix, xxx, xxxiv (which by 1984 had a mandible not present according to Vallois), xxxv (it is possible that this skull was reshaped subsequent to Vallois' description), xxxvi (not recorded by either Meiklejohn or Ferembach in the 1960s).

[9] Expressing the sum of the absolute differences between two distributions.

[10] E.g. Dobson (1997) in passing provides much evidence on internal migration in England; Monter (1979) gives information on reasons for migration to Geneva. In data from later in Genevan history (from Perrenoud, 1978), the relationship between our demographic estimators lies slightly outside the scatter of our other historical data sets.

[11] The 129 tables from which West mortality schedules were drawn up are post-1870 and are generally from Europe or areas colonized by Europeans, apart from nine tables from Japan and Taiwan. West tables are regarded as no more than an indication of generalized demographic values. Our 51 West model tables comprise 15 decreasing, 28 increasing and 8 stationary tables. Our basic non-stationary tables cover $r = -.01, -.005, .005, .01, .015$. Included are levels 1 (8 tables up to $r = .025$), 2, 3, 4, 6 and 8 (6 tables each), 5 (10 tables up to $r = .035$), and 10 (3 tables at $r = .01$, 0 and .01). All TFR values are below 13. Infant mortality quotients are below .630, even when West 1 and 2 are extended to $r = .05$ (on 56 tables up to West level 8, ${}_1q_0CV = .33$).

[12] TFR = 10 would be predicted by the Tourouvre method from MCM $\sim$ .110, and from MCM $\sim$ .114 using the Coale and Demeny method.

[13] Carvalho (2002:239) summarizes Roche's view of the contemporaneity of Moita and Arruda and the later date of Amoreira, based on tool type percentages.

[14] This time frame occurs during a particularly broad plateau in the intcal04 calibration curve, making interpretation problematic. Nevertheless, there is a reasonable possibility of dating these structures to soon after 7300 calBP.

A new publication gives emphasis to the close association between the Mesolithic Muge sites and estuarine conditions: see van der Schriek, T., Passmore, D.G., Stevenson, A.C., Rolão, J., 2007. The palaeogeography of Mesolithic settlement-subsistence and shell midden formation in the Muge valley, Lower Tagus Basin, Portugal. The Holocene 17, 369–385.