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The Palaeodemography of Central Portugal and the Mesolithic-Neolithic transition

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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 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 palaedemographic data and methods that we think are crucial. We then provide an introduction to 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. Finally, we place this into the general context of the transition to agriculture in Portugal.

Palaeodemographic Reconstruction

The potential uses of demographic data in bioarchaeological interpretation have undergone a revolution over the past thirty years. When Peterson wrote his critical review of palaeodemography (1975) his concerns were with the accuracy of sex and age determination, and with sample bias. His conclusion was that palaeodemography 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 & 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 palaeodemographic age at death distributions are to be interpreted accurately. The first is that there must be accurate age

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assessment of the osteological sample. This has been extensively reviewed (e.g. Jackes 1992). Very careful assessment of the sample is 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.

Palaeodemographic 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 palaeodemographic 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 initial transition in the Near East 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 & 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 Epipalaeolithic, 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 Epipalaeolithic pattern continues.

Our work on the Near East (Jackes *et al.* in prep. a, contra Eshed *et al.*) 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).

Introduction to the Palaeodemography of the Shift to Food Production in Europe

We began studying palaeodemographic 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 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 centred on samples from Northern Europe, the second on collections from Djerdap (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 archaeology and archaeological 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.

Introduction to the Palaeodemography of the Shift to Food Production in Portugal

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), and since then our attention has been focused on providing 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 archaeological record makes available to us, so there can never be the rigour 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 archaeological exploration so that we can at least approximate a reasonable research methodology. This is not to gainsay that further archaeological research in central Portugal may provide evidence of a "fuzzy" transition, and we very briefly touch upon this below. Archaeologists 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

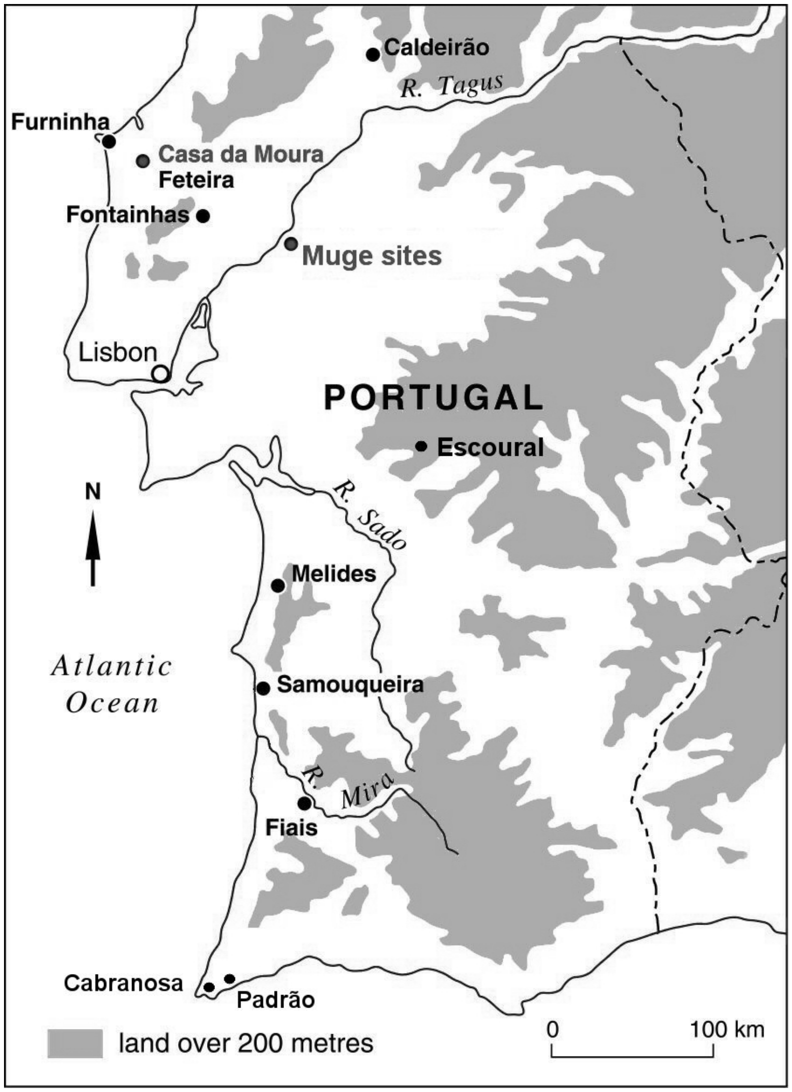

Figure 1: map showing locations of sites mentioned in text

Lagares is of the right time period[2] . 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.

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 in press, Alvim *et al* in

[2] 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).

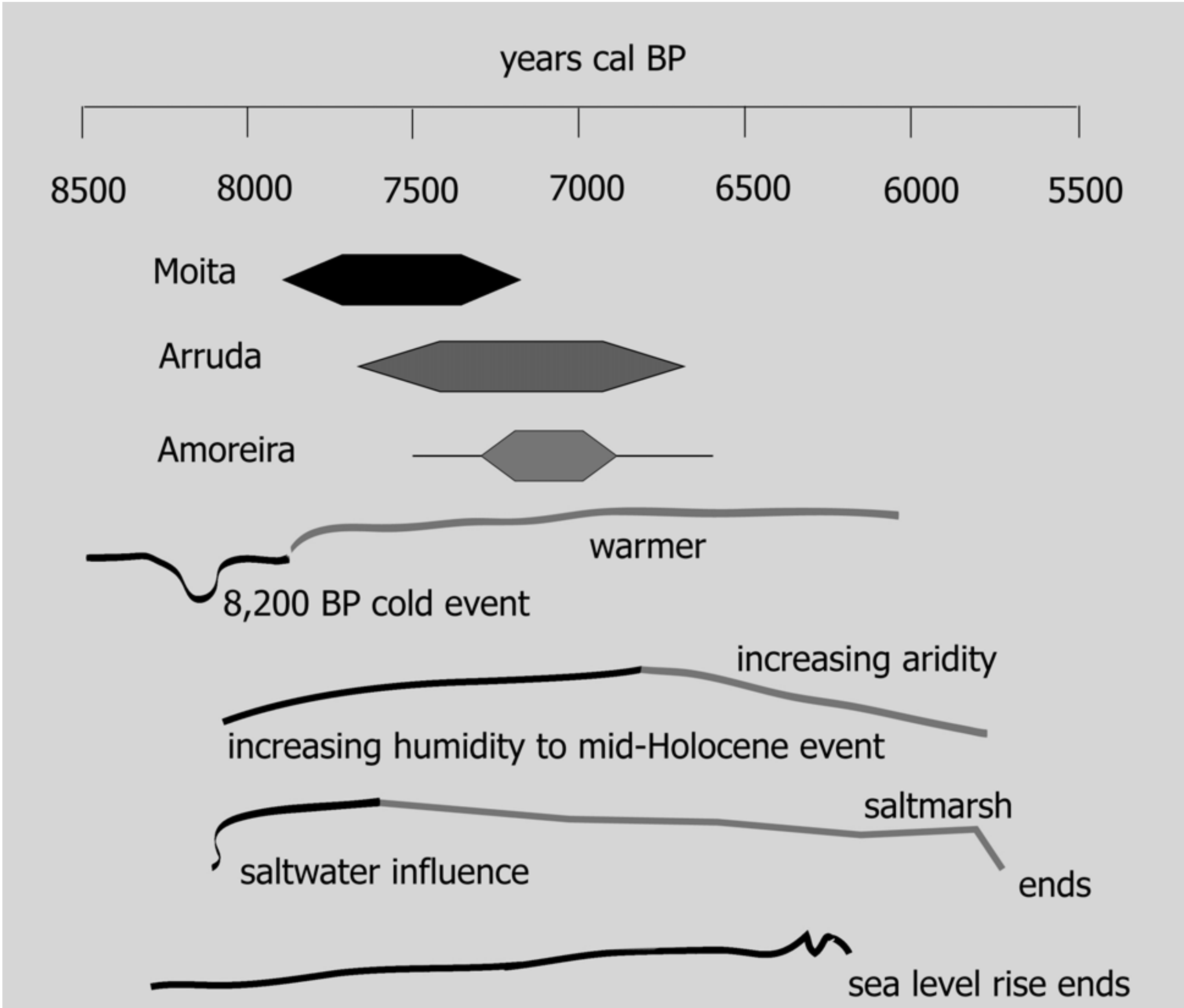

Figure 2: diagrammatic summary of changes in the Muge valley at the Mesolithic-Neolithic transition

prep.; Jackes *et al*. in prep. b), 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 palaeodemography. 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 new evidence on the skeletons excavated in 1954 (Jackes *et al.* in prep b). 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 palaeodemography

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[3]. 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[4]. 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[5]. The "whiskers" show the maximum range for the use of this site, derived from recent dates on faunal material (Roksandić , pers. com.). Charcoal dates on basal levels at Moita and Amoreira (see Jackes and Meiklejohn, 2004: Fig. 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 after 6000 calBP.

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 (i.e., the completion of the isostatic rebound effect) 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).

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

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

[5] 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.

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 archaeological 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 (Pizzola 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. Chenopodiaciae pollens are dominant in the area of Moita and Arruda during the period of occupation (van der Schriek 2003, Fig 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 on **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; Haworth 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 pickup 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 diarrhoea. *P. vivax* is milder than *P. falciparum* malaria (though still capable of causing anaemia[6]), 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 crude shelters and used fire to warm themselves during the winters, 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.

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

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 neighbouring 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 tributary 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 us 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.* in press), 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[7], 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

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

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 (in press) have discussed the documentary evidence on the 19th century excavations. The 1950s excavations by the Abbé Roche and O. da Veiga Ferreira came after bulldozing and construction had revealed additional human skeletons.

Jackes and Alvim (in press) 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 in press) 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 in press), 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 archaeological 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 (Roksandić 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 in press), 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. b) 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 his 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 labelling and identification. The material is labelled 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 & MKJ) 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 in press): as a result there was a double loss of continuity. Using archival material, Jackes and Alvim (in press) 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 the European prehistorians attending an important meeting in Lisbon that year – the International Congress for Prehistoric Anthropology and Archaeology (CIAAP) – details of the Moita excavations have remained unknown until recently (Jackes and Alvim in press).

There are some published descriptions of Moita material from the 19th and earlier 20th century, mostly focussing 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 Oliviera (1884), Cartailhac (1886), Vallois (1930) and Ferembach (1965) allowing us to identify most as being from Arruda (see Jackes and Lubell 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 Oliviera 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[8] .

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 cranial material comes from Vallois (1930).

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[8] 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.

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).

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 palaeodemography. 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 yrs 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 in press). Ferembach gave these the 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 & 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 & Meiklejohn 2004; Jackes and Alvim in press) in order to justify our method of the age at death counts for palaeodemographic 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 palaeodemography (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.

Table 1	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**

Why do we use these age categories? Standard five year age categories are desirable so that comparisons can be made with model and 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 the age distribution of the dead across the categories of analysis and allows us to see that the Neolithic site, Casa da Moura, differs from the two Mesolithic sites in the distribution of the dead. It is clear that there are some anomalies, especially for Casa da Moura. The under-representation of the very youngest age category is obvious. There is a possibility of some errors of attribution at age category margins (not unexpected when loose teeth make up part of the sample). In other words, the Casa da Moura curve does not have the shape that we might expect: the use of a mean mortality quotient across the three subadult age categories is designed to smooth out such problems. Nevertheless, the basic difference between the Mesolithic and Neolithic curves is clear. There is likely to be an under-representation of Casa da Moura older adults, and adjusting for this would bring the Neolithic curve into greater alignment with those of the Mesolithic sites, but since the Casa da Moura sample size is very adequate, it is likely that the Neolithic curve bears some relation to reality.

How can we translate this rough approximation 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

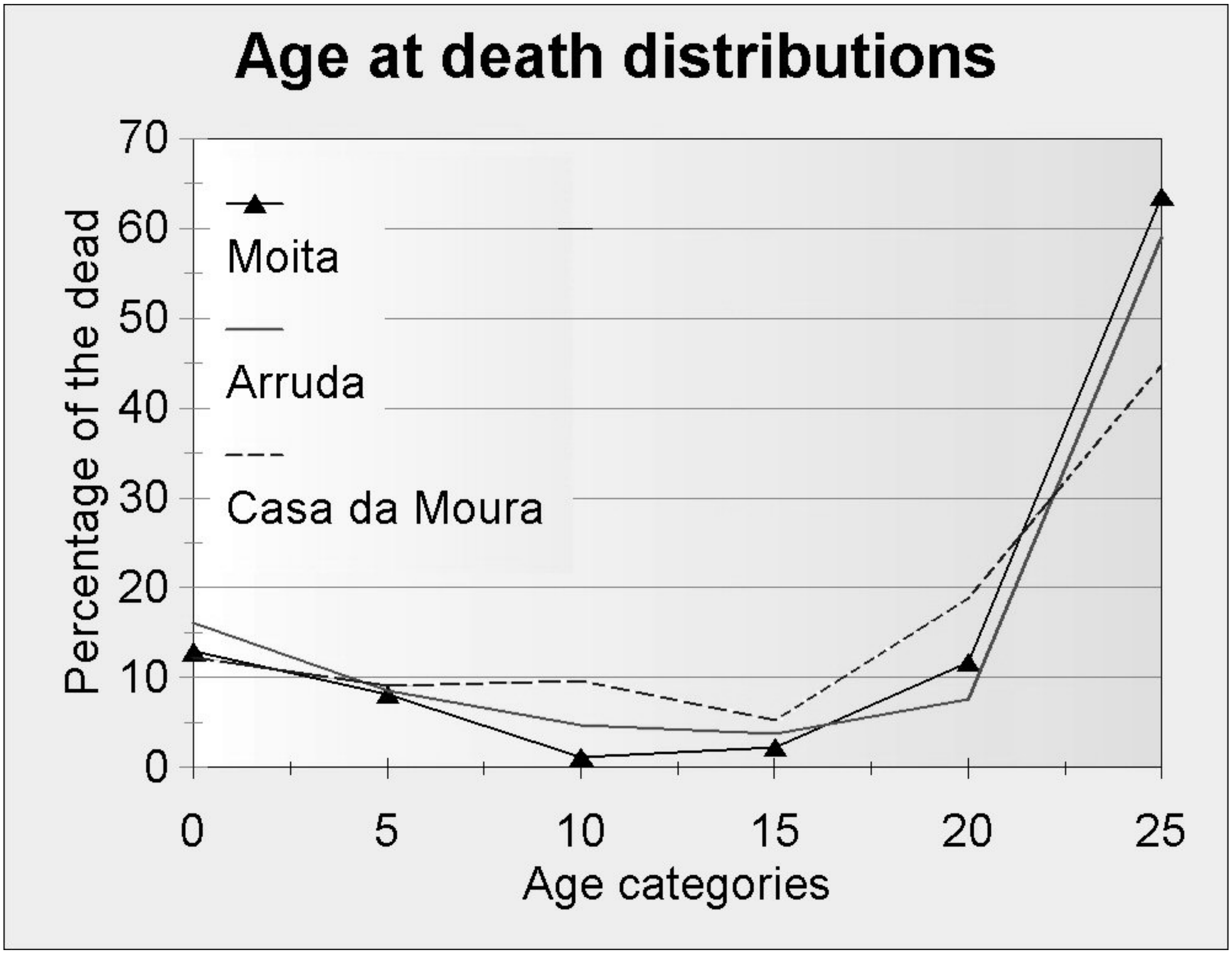

Figure 3: age at death distributions across standard 5 year age categories, with all adults 25 and over grouped, for three Portuguese archaeological sites

fertility estimates and assess their validity. The technique which we will use allows us, for example, to verify that the small and incomplete Melides sample, 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.

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 mortality quotients for the three age categories from 5 to 19.9 years (MCM or mean childhood mortality quotient; Jackes & Lubell n.d.; Jackes 1986). Briefly, total fertility (TF) rates are derived from the model data of Coale and Demeny (1983: 51 tables) and United Nations (1982: 3 tables). The juvenile adult ratios and mean childhood mortality values for these model data are used to generate fertility estimates by prediction (fit for TF with J:A from SPSS 12.0 curvefit quadratic and fit for

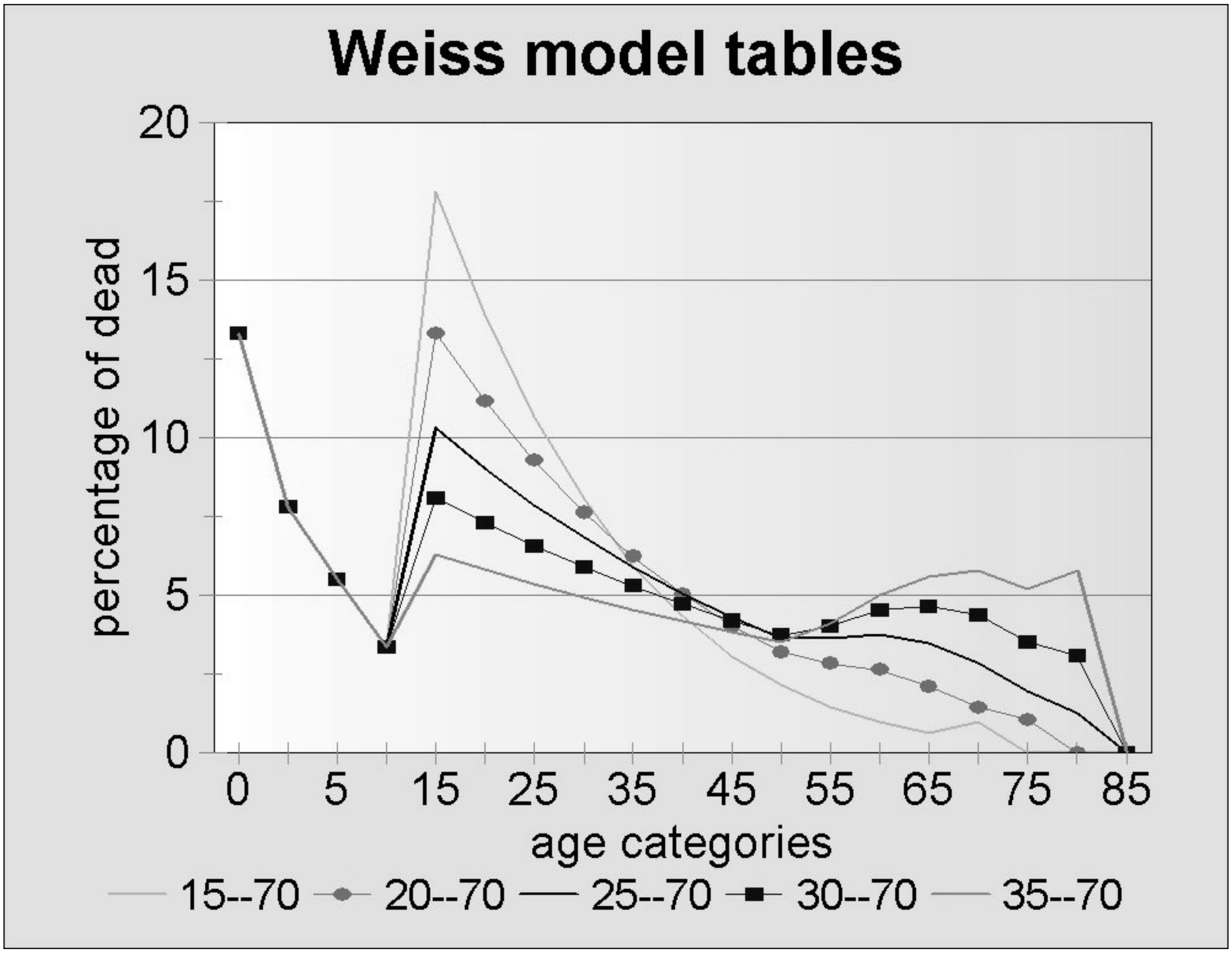

Figure 4: range of adult age at death distributions for Weiss model tables demonstrated by the use of five selected tables (Weiss 1973)

TF with MCM from SPSS 12.0 curvefit quadratic) as described in Jackes *et al*. (in press).

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 palaeodemography has 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 archaeological populations, since Weiss (1973) published model tables which he had developed from 50 sets of data, all but 14 of which were archaeological. **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 reasons for the appearance of such curves in the palaeodemographic literature is that there must be mis-aging at category margins. The

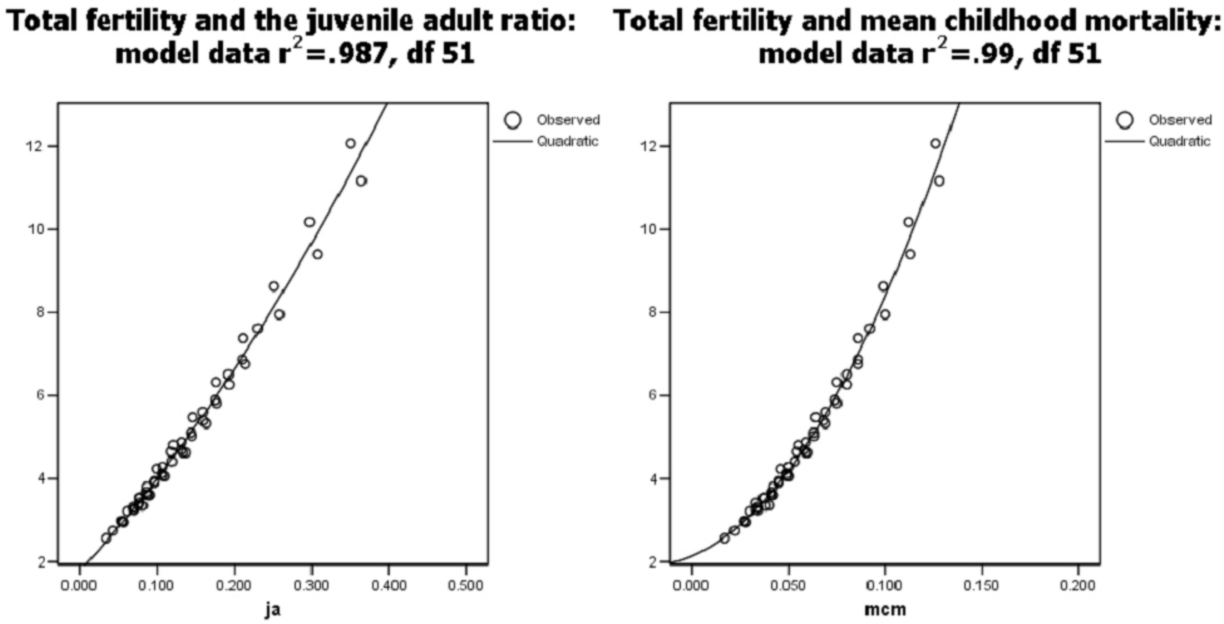

Figure 5: Total fertility derived by calculation from model tables

implications of this sort of mis-aging for demographic values will be further discussed below.

Figure 5 indicates that – at the highest levels of fertility – MCM is a better predictor of TF based on model data. In fact, however, TF is best predicted for just under 50% of the model data sets by MCM calculated with the constant (the intercept is the constant) within high, mid range and low fertility. The correlation coefficient is very slightly higher with MCM ($r^2 = .99$) than with J:A ($r^2 = .987$), but they are basically equivalent (r = .995 for each). J:A calculated with the constant provides the best TF estimates for two known end points (shown in black in **Figure 6**), comparing historical evidence with the estimates derived from model data by quadratic regression. For this reason, and because MCM is much more dispersed than J:A in our large comparative data set of historical and archaeological age at death distributions, we would choose to emphasize the TF estimations from J:A.

MCM is more dispersed than J:A in our comparative data set because it tends to fall too high in some archaeological but not in the historical samples. In other words, MCM is too high for J:A in some of the archaeological samples, and the samples in which this discrepancy occurs are those with excessively high late adolescent mortality. An extreme example 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.

The two points shown in black on **Figure 6** are two sets of known data, and – to repeat – the fit for these two is better with the estimates derived from model J:A 95% confidence limits. The

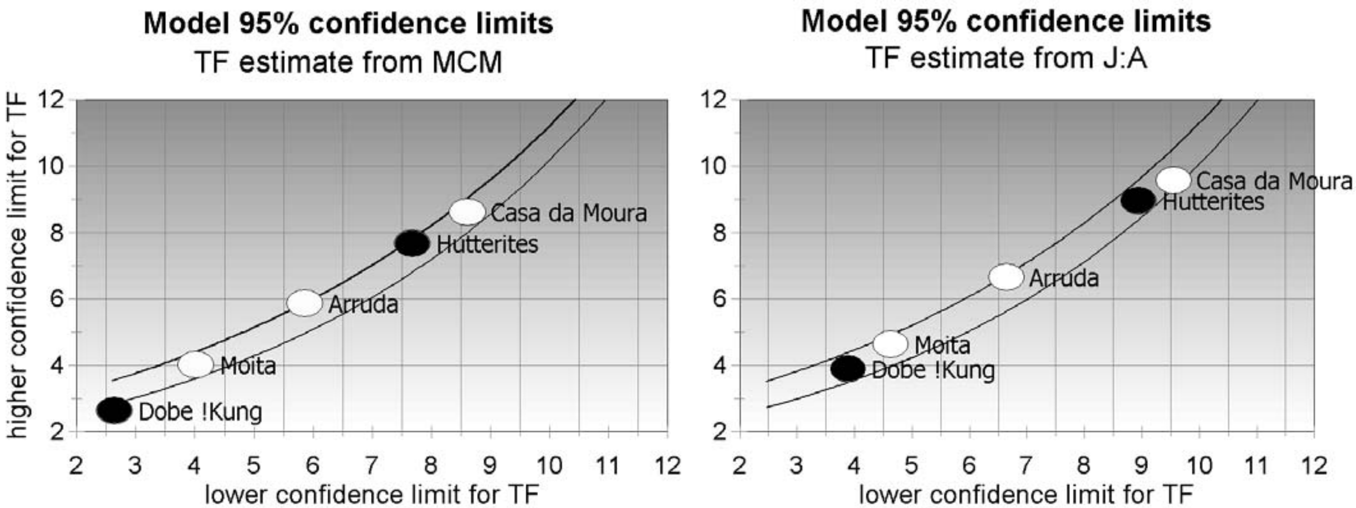

Figure 6: total fertility estimates for the archaeological data and for two historical age at death distributions predicted by regression from model table calculated TFR: exponential curves of predicted TFR derived from Coale and Demeny (1983) West tables provide the context (x and y axes)

estimates derived from MCM do not fit these two sets of data so well. The black point to the lower left represents 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 own estimate of 4.3 for the period from 1963-1973. The black point on the upper right of **Figure 6** represents the Hutterite TF (8.4 to 9.5 estimated by regression from the age at death distribution J:A based on the 95% confidence limits for the estimate) for 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 9 as the correct TF value to associate with those who died between 1941 and 1950.

Table 2	**TF estimated from**	
Site	**J:A**	**MCM**
Moita	4.63 (4.1-5.1)	4.02 (3.6-4.5)
Arruda	6.65 (6.1-7.2)	5.87 (5.4-6.3)
Casa da Moura	9.56 (9.0-10.1)	8.61 (8.2-9.1)

Table 2 presents fertility estimates for the Portuguese archaeological samples derived by both methods, together with the 95% CL range. It must be understood that the figures provided in **Table 2** are presented as indications of relative rather than absolute fertility. TF may overestimate fertility (Terrisse 1986); 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 is that it is certain that adults of advanced age must be under-represented in the loose dental sample.

TF for the three archaeological sites may be reasonably estimated at 4.5 for Moita, 6 to 6.5 for Arruda and, at most, 8 or 9 for Casa da Moura[9], as long as all caveats are noted.

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 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 21 m above the present sea level. They lay on basal sands and were mostly covered by a heavy breccia. In general, they appear to be 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 of the 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 and post-Mesolithic layers.

It seems likely that many of the Arruda burials post-date those at Moita. The site has been only partly excavated and some of the excavated material has been lost, so the number of individuals excavated from there (more than 105, perhaps in excess of 150) is both estimated and incomplete. The Arruda 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 are 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

[9] New work on Casa da Moura mandibles in 2004 may lead to re-evaluation of the demography. The suggested TFR is based on estimating that ~12 old adults were not represented among the loose teeth because of premortem tooth loss (derived from tooth loss rates in *in situ* lower molars).

(Roksandić in press). 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.[10]

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 80 km to the south (Calado 2002: Fig 2.2), predating the earliest Neolithic monuments of the Upper Alentejo (which appear ca. 6500 bp; Jorge, 2000:59)[11].

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 palaeodemographic 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. Furthermore, we see no osteological evidence of

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

[11] 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.

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 behavioural 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 archaeological 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 4 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 4 years of age. What we can say is that children above 4 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 live born 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, where there was perhaps a change in the level of sedentism (Lubell *et al.* 1994; Jackes & Lubell 1999b). We have argued for a reduction in the birth interval as the cause of higher fertility (Jackes *et al.* 1997). 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, though the basis appears to be the reduction in birth spacing (Jackes *et al*. 1997).

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 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 unfavourable 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 Veiga Ferreira and Roche 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 4 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, was 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 himself 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. b). 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 1951-2: 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 in press) 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, Fig. 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. b) appears to gives an indication of the level reached by the bulldozers. In the central location under discussion, Skeletons No. 5, 3 and 12, 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 archaeological 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 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 3 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 nineteenth 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, 1985-6) 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 3 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 3 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 younger child, under 3 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 1 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 1 year. And we can say with more confidence that there are three older children who are under 5 years. But this still does not, of course, provide a full representation of the expected number of children under 5. 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.

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