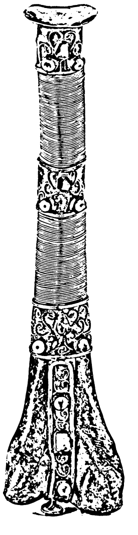

5

Representativeness and Bias in Archaeological Skeletal Samples

Mary Jackes

Introduction

In discussing the term “bioarchaeology,” Wright and Yoder (2003:44) write that “Our ability to make statements about [the past] is … dependent on the representativeness of archaeological sampling of ancient human remains”. We cannot, of course, sample ancient human remains in any meaningful way: our “population” and our “sample” are more or less the same thing. We can only work with what we are given by the chance of preservation and discovery and the multiple factors that determine whether excavations are complete and careful, whether human skeletons can be retained and curated satisfactorily and made available for study.

This raises a series of questions regarding whether or not our samples are valid representations of the populations from which they came. How can we recognize bias in the materials bioarchaeologists study, and how might we deal with any shortcomings? This chapter discusses the impact of social/cultural and demographic factors on bioarchaeological samples along with the effect of taphonomy and research techniques. Above all, it emphasizes the need to understand the context and demographic characteristics of our samples so that we can arrive at interpretations that have some relation to reality. The importance of comparison in evaluating the representativeness of samples will be evident from many of the historical and archaeological examples given.

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Bias: Can We Recognize It, Can We Compensate for It?

The human age-at-death distribution is biased, except in some exceptional circumstances in which all age groups have an equal probability of dying. Normally, the probability of dying is not uniform: deaths are not random with regard to age or sex. There are endogenous causes of perinatal death, especially in males (Reid 2000; Wells 2000), and as the nursing period ends, as breast milk ceases to shield the baby and sources of infection increase, there will be more deaths (Brown 2003; Hanson 2001; Rowland 1986; Wells, 2000). With the exception of massacres and catastrophes, deaths will often be mediated by age-specific onset diseases, by accident and conflict associated with young males, by reproductive hazards in young females, by the increasing rates of degenerative conditions among the elderly. As such, the dead are not an unbiased sample of the living population: the force of mortality, normally greatest on the young and the old, creates a U-shaped mortality curve. We will consider an unbiased cemetery sample to be one that closely approximates this "normal" U-shaped mortality by age curve, while also recognizing that the exact shape of the curve will differ according to fertility and mortality steered by varied biological and social/cultural determinants. For a preliminary idea of some of the many factors influencing our archaeological data, lacking historical control, we can examine a case where the nonbiological factors can be determined.

An historical example from Sydney, Australia

The cemetery records for St. John's Anglican Church in Ashfield, now an inner suburb of Sydney, Australia, are very informative. Connah (1993:151, figure 10.2) provides the data on the cemetery (age-at-death distribution by decade), commenting that the records demonstrate an increase in life expectancy at birth. Indeed they do – apparently. The first three decades during which the cemetery was used have a life expectancy at birth (e_0) value of about 31 years, the second three decades around 43.5 years, and the next 30 years have an e_0 value of just over 66 years. But is this a valid interpretation of the data?

The first 30 years of burials represent the beginning of growth in this area just outside the early colonial settlement of Sydney. People are moving in: the church is consecrated in the mid-1840s, and by the mid-1850s a railway station is established a few hundred meters from the church. Now begins a rush, leading into the second 30-year period, with the establishment of grand houses around the church, the richer of the new inhabitants in this English colonial society being Anglicans seeking to escape the increasingly congested center of Sydney. The third period (1900–1929) marks the start of a decline in use of the cemetery, years during which the wealthy Anglican community members, many of their sons lost in World War I, move away to the north of Sydney Harbour. Industries are beginning to come into Ashfield and the large single family houses are being replaced by denser settlement. Early in the third period a Roman Catholic church is built in Ashfield for incoming Irish working families.

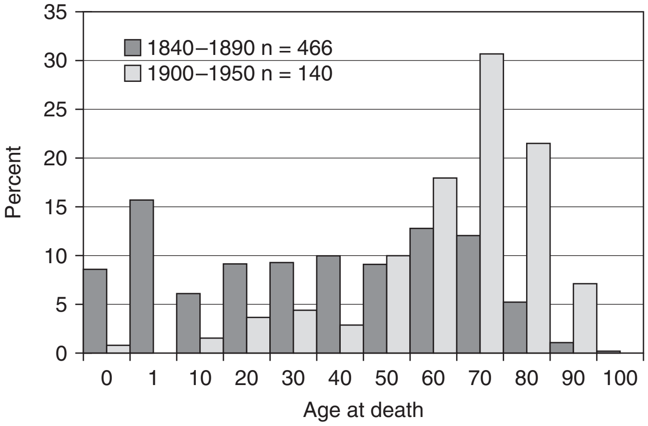

Figure 5.1 Comparison of age-at-death distributions of early and late period burials at St. John's Church, Ashfield, Sydney.
Source: Author's drawing, based on Connah, G. 1993 "Of The Hut I Builded": The Archaeology of Australia's History, figure 10.2. Cambridge: Cambridge University Press

In the first three decades young mobile people move in, establishing their families. The next 30 years show a decline in fertility as the immigrants live beyond the reproductive years. In the last three decades many members of the Anglican community leave for the newly opened up suburbs to the north: few young reproductive age people are left and a larger proportion of the population is over 50 years of age. In the first three decades, only 25 percent of the dead are over 49. In the decades 1900 to 1929 the percentage of the living over 49 has more than doubled and these older people (50 and over) make up 85 percent of the dead. St. John's cemetery represents a period of change during which people might choose not to be buried in a (now) inner city church graveyard, but rather in a new suburban cemetery (a public cemetery had been laid out further along the railway line at the end of the 1860s). Furthermore, the earlier growth of the area as a suburb of large houses is succeeded by social and economic change, with the influx of industry, multiple housing units, and people less likely to bury their dead in an Anglican cemetery.

From this one isolated example, we can see the operation of a number of factors: immigration of reproductive age individuals accompanied by few elderly people; catastrophic war-time deaths of young males, buried elsewhere; emigration of reproductive age individuals and their children, leaving the old behind; selection by religious affiliation; selection by occupation and social status; choice of a particular burial place. In this instance, changes over time in fertility and longevity cannot be assessed because the sample is biased. Figure 5.1, covering the entire period of the burial records, illustrates that examining age-based characteristics of this cemetery sample will give us information biased toward 19th-century juveniles and younger adults, while the examination of the old will be biased toward 20th-century characteristics. Cemeteries have beginnings, middle periods, and endings: in and out migration both are important, but other factors will also be in operation during the time in which the cemetery sample accumulates. The end result is a muddied picture of fertility and longevity. In the absence of chronological controls of archaeological cemetery samples, we must be very aware that our samples could be biased in the various ways illustrated by St. John's, Ashfield.

Recognition of sample composition bias through fertility estimates

There are biological limits on fertility, but we cannot be sure about the social constraints on fertility in the distant past. We assume that the populations of the past were not practicing contraception and we assume that the majority of the females were sexually active and fertile for approximately 30 years, from sometime after 15 years to sometime around 45 years. I have reviewed a number of the factors which militate against women having live born children at minimal birth intervals of, say, 15 months (9 months gestation plus 6 months lactation) and discussed the range of fertility levels that we might regard as biologically possible (Jackes 1994; 2009a; Jackes and Meiklejohn 2004; 2008; Jackes et al. 2008).

I estimate fertility levels as the total fertility rate (TFR) from cemetery populations (e.g., Jackes and Meiklejohn 2008; Jackes 2009a) based on two estimators. One is J:A, the ratio of juveniles aged 5–14 years to adults aged 20 and over (the juvenility index of Bocquet-Appel and Masset 1977). The second is MCM, or mean childhood mortality, the mean probability of death across three subadult age categories, 5–9, 10–14, and 15–19 years (Jackes 1986). I have used fertility data from external sources, that is, fertility values from the literature on historical fertility, to verify my estimates. I do not estimate fertility from historical life table values. Bocquet-Appel and Naji (2006:356, figure 6) have suggested that fertility rates derived from historical life tables are quite low. Based on records in Geneva, Switzerland, some of the most reliable historical demographic data that we have (Perrenoud 1984:55), this is true of fertility estimates from life table calculations. It is theoretically possible to estimate the TFR from ${}_{30}C_{15}$ (the proportion of the living population in the appropriate age classes) in a life table, but normally this will lead to underestimations of the true fertility rate, as seen in Figure 5.2. The life table underestimation varies with the level of population increase, approaching zero as the birth and death rates become more equal.

My estimates do not derive directly from model tables but indirectly by quadratic regression from 51 West model tables (Coale and Demeny 1983) and three United Nations model tables (United Nations 1982). In Figure 5.2 the stars illustrate that the estimated TF ranges are reasonable, especially in the earlier periods when time ranges for Perrenoud's fertility and mortality data are more equivalent (Perrenoud 1990:249, table 15.3 total progeny, adjusted age-specific fertility rates). The fertility estimates are most discrepant around 1770 following an influx of refugees and the beginning of contraception, especially among older women.

If it is granted (i) that we can estimate fertility rates with a certain accuracy from the estimators, and (ii) that we have a fairly clear idea of the biological constraints on the total fertility rate of human populations, we can at least weed out those samples in which the J:A ratio among the dead is beyond the bounds of biological possibility. I have built up a database of archaeological and historical age-at-death

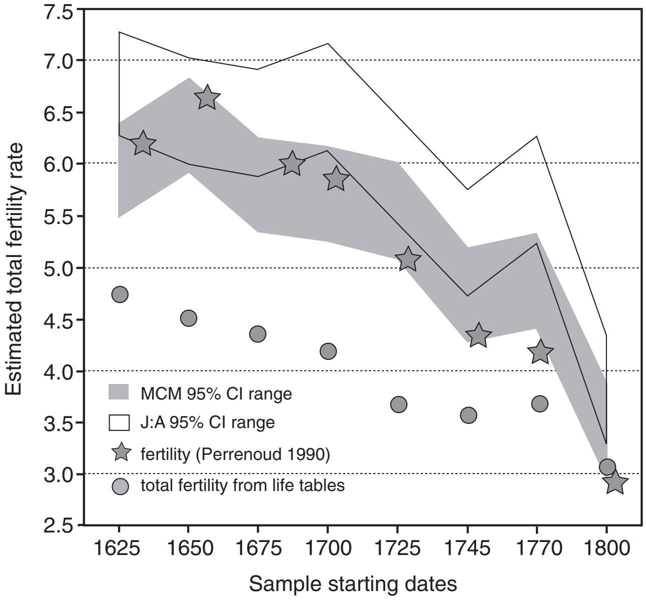

Figure 5.2 Estimates of historical fertility rates for Geneva shown as 95 percent CI ranges derived from J:A and MCM (data from Perrenoud 1984), compared with total fertility derived from life tables (circles) and fertility data (stars) given in Perrenoud (1990).
Source: Author's drawing, based on Perrenoud, A. 1990 Aspects of Fertility Decline in an Urban Setting: Rouen and Geneva. *In* A. Urbanization in History. A Process of Dynamic Interactions. J. van deWoude, J. de Vries, and A. Hayami, eds. Pp. 243–263. Oxford: Oxford University Press

distributions with sample sizes above 100 in order to test the reasonable limits of subadult age-at-death distributions. Proportions of juveniles and adults in archaeological samples are problematic because it is so often impossible to find out what percentage of the dead in a site might be represented by the excavated skeletons. If a site is not completely excavated, the dead may or may not represent a biased sample. Nonetheless, while we may never know the true nature of a sample, it may be possible to discern the presence of bias.

Over- and underrepresentation of age classes: examples from the bioarchaeological literature

We should suspect bias, and closely examine the context, in any archaeological cemetery sample in which the estimators indicate a total fertility rate of 12.5 or more.[1] In round terms, any J:A value over ~.380 and any MCM value over ~.135 needs to be examined for partial site excavation, for special-purpose burials, for incomplete life table totals, and for sample size (which should be 100 or more: Hoppa and Saunders 1998; Paine and Harpending 1996). Even if the estimates of fertility are lower than 12.5, it is necessary to consider whether figures approaching the maximum recorded or theoretical total fertility rates are reliable. The TFR is

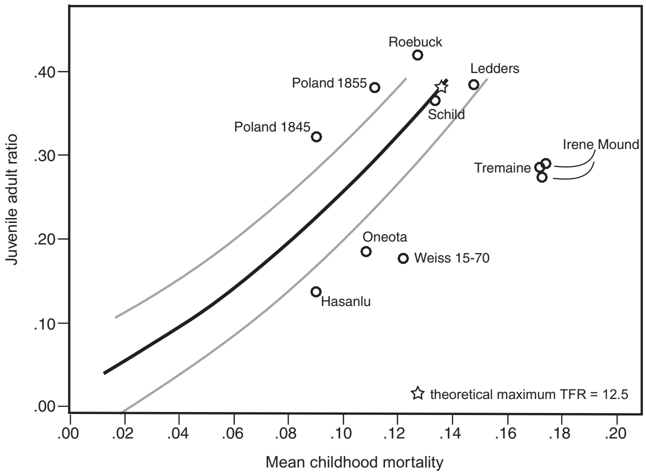

Figure 5.3 MCM and J:A, 95 percent CI for archaeological and historical data sets, showing positions of some of the samples discussed.
Source: Author's drawing

the average figure used for comparative purposes in demographic studies: it does not reflect the experience of individual women. Individual women or their husbands may be sterile and will begin and end their reproductive lives at different ages. During their reproductive lives death, disease, famine, miscarriages, stillbirths, deaths of children while still nursing, and disruption causing extended absence of their sexual partners may occur. Their fertility levels will vary over their reproductive life spans. The exact pattern of marriage, polygamy, monogamy, marriages of young women to older men, remarriage permitted or forbidden – all such factors will have an effect on fertility rates. The TFR is a "synthetic" rate, rather than a reflection of the experience of individual women. Discrepant values between the two estimators may indicate bias or error. Such discrepancy can be caused by a variety of factors and should not be ignored.

Where high MCM values cause sites to fall outside the 95 percent CI of the quadratic regression of J:A on MCM, one possibility is that the age estimates of late adolescents are wrong. An individual who is actually 20 may be estimated to be, say, 19. When this is a systematic error, causing too high a percentage of individuals in the 15–19 age category, an age distribution will fall too far to the right of the regression line (the *x*-axis is MCM, see Figure 5.3). For this reason, it is important to pay very close attention to age estimates around 19–21.

Age estimation errors during initial analysis should be considered, but when databases are derived from the literature, age redistribution can introduce error evidenced by a site falling beyond the 95 percent CI on the MCM axis. For example, the Oneota data in the Health and Nutrition in the Western Hemisphere (HNWH) database (Steckel et al. 2002, as used by McCaa 2002), is derived from Milner et al. 1989 and specifies that there were no deaths in the 10–14 age group. The Oneota data with *n* = 264 (Norris Farms 36 in Milner and Smith 1990:122–

123; see also Vradenburg 1999) recorded four deaths in that age group. The two sets of data are very different: one will fall beyond the 95 percent CI (see Figure 5.3) while the other will give a very reasonable TFR estimate range. This is a data-handling error of the type introduced into comparative studies by recasting of data from broad age categories. The Dickson Mounds site problems (Cohen et al. 1994:636; Jackes 1993) are compounded in the HNWH database use of information from Lallo et al. 1980, again with age distribution anomalies resulting from redistributing ages from broad age categories.

The HNWH database also includes Irene Mound where the TFR estimates range from 8.8 to 19.6 and individuals aged 15 to 19 constitute 27 percent of the entire sample. The discrepant nature of this is clearly illustrated by Larsen et al. (1991:194, figure 9, precontact agricultural)[2] and is more extreme than the model table Weiss 15–70 (Weiss 1973) in which the methodology led to an excessively high death rate in the 15–19 age category (Jackes and Meiklejohn 2008:231, figure 4). With sites like Tremaine (Vradenburg 1999) (Figure 5.3), we cannot automatically assume age estimation error, but might question whether the dead represent a demographically valid sample. An obvious example of a nonvalid sample is Hasanlu IV (Rathbun 1982) representing a period during which the settlement was sacked and burned (Dyson 1965:202). The skeletal sample appears to have contained "the bodies of defenders and looters alike" (Hawkes 1974:190) with 36 percent of Rathbun's total sample of 112 found in one of the several collapsed and burned buildings (BB II) (see Figure 5.3). Here we would expect an excess of young males.

While it is very rare for samples to lie to the left of the 95 percent CI range (with a *low* value for MCM relative to J:A), it can occur when late adolescents are underrepresented. This is the case for ten-year samplings from a Polish rural parish (Figure 5.3; Piontek and Henneberg 1981: corrected data; Piontek, personal communication, November 15, 1990), no doubt resulting from internal migration and political disruption.

There may be unexpectedly low numbers of children or of adults in a sample, and this type of bias could be discerned from the distribution of the fertility estimators.

Apparent low fertility when one might expect a non-contracepting population could indicate that children (not just very young children and infants) were being buried elsewhere and that there is, in fact, a relative overrepresentation of adults. It is too widely accepted to need discussion that infants will be underrepresented in skeletal samples: estimators exclude children under five years, but the possibility of differential burial of older children must be taken into account.

The reverse case, in which there seems to be an excessively high representation of children must also be considered. A very obvious case of too many juveniles is that of Nea Nikomedia in Greek Macedonia. The 95 percent CI for the two discordant fertility estimates (.168 and .683) would range from 17 to 27, far beyond acceptable demographic values. The skeletal sample is inappropriate for demographic analysis, despite having been used for comparative purposes (Angel 1971:70). A mortuary practice placing emphasis on the burial of juveniles within the walls of the village, with the archaeological excavation focusing on the settlement area, would lead to this type of result.

Table 5.1 Comparison of fertility estimates for Late Woodland mid-western sites

Site (Late Woodland)	*Source*	*TFR 95% CI*
Kuhlman Mounds	Atwell 1991	5.2 to 6.5
Joe Gay	Garner 1991	5.2 to 8.1
Koster	Droessler 1981	6.5 to 7.6
Ledders	Garner 1991	9.6 to 11.0
Schild	Droessler 1981	11.2 to 13
Ledders	Droessler 1981	13.7 to 15.9

Apparent high fertility may result from underrepresentation of adults because of exclusion of indeterminate age adults from life tables: the use of estimators allows us to avoid adult age assessment difficulties. In fact, adults may be missing for a number of reasons and much of the discussion to follow is of sites in which adults seem underrepresented as a result of the bioarchaeological analyses or because of factors in operation at the time of burial.

When there are groups of sites which do not provide clear answers on why their demographic parameters may be unrepresentative, detailed analysis by a regional specialist could clarify the situation. A series of sites in Illinois (Koster and especially Ledders and Schild; Droessler 1981, see Figure 5.3 and Table 5.1) led me to suggest several times (e.g., Jackes 1986) that the Late Woodland period was a time of exceptionally high fertility, but this is now questionable because, having a method of comparison, I can clearly identify discrepancies among sites. The Schild Late Woodland site was incompletely excavated and the Koster, Ledders, and Schild Late Woodland life tables were based on just under 75 percent of the excavated skeletons. If many of the excluded individuals were adults, then the fertility estimates would be too high. Gordon and Buikstra (1981), in studying the effect of soil pH on bone, examined 95 uncremated skeletons from the Ledders and Helton sites and noted that 16 percent would definitely or probably be excluded from age or sex assignment and a further 22 percent were cracked and fragmented to the point where detailed study was difficult or impossible. Unfortunately, we do not know the details of differential preservation of children and adults, but it is clear that full life tables would exclude many individuals. Added to this we have cremations, which were rare at Koster (Braun 1981; Tainter 1977) but more common for high-status individuals elsewhere in the area. Given the difference between the high fertility estimates for Schild and Ledders, and the Koster mid-level fertility estimate, in association with variability in diet (Buikstra and Milner 1991:324), it seems important that no definitive statements be made except in the context of focused regional studies (Jackes 2006) which include other sites, like Kuhlman Mounds.

Kuhlman Mounds Late Woodland (Table 5.1) has been studied in detail (Atwell 1991), both Mound 1 which has no cremations, and the total site, including cremations. Despite Atwell's (1991:167) opinion that Mound 1 "biases the overall shape of the site's age … distribution", the estimates from Mound 1 alone (TFR 95 percent CI 5.2–6.5) are little different from the overall site (TFR 95 percent CI

5.4–6.9). Without a comparative method, it is not always obvious whether age-at-death distributions differ in significant ways. Because we can compare the very reasonable fertility estimates for Kuhlman Mounds with those for other Late Woodland sites in the area (Table 5.1), it is possible to suggest that Late Woodland fertility might well have been around 6. The suggested total fertility rate, though high, is much more plausible than a range of 11 to 16. Such an analysis would lead to the conclusion that some Late Woodland skeletal samples, specifically Schild and Ledders, are biased by underrepresentation of adults and should not be used as components of a study of Late Woodland demography.[3] Specialist appraisal of this suggestion is necessary.

A clear-cut case would be one where some adults were excluded from normal burial – a situation documented for Iroquoian Ontario (certain categories of deaths, by freezing, drowning and violence, excluded males from normal burial, Jackes 2009a:363). The Roebuck Site (see Figure 5.3), dated 1450–1530 (Pfeiffer and Fairgrieve 1994), is a large, palisaded village (Jamieson 1990) situated in an area which was inhabited at the time of Cartier's voyages down the St. Lawrence River in the mid-1530s and early 1540s, but was completely depopulated by 1603 when the area was again visited by French explorers. The reasons for this abandonment and the fate of the St. Lawrence Iroquoians are still under discussion. The human remains were studied long ago (Knowles 1937) and the material cannot be used for comparative purposes (contra Bocquet-Appel and Naji 2006). It is an exceptional site with 11–14.5 as 95 percent CI for the two fertility estimators, only 84 individuals in the sample and a slight underrepresentation of late adolescents. There appeared to be only four males among the 43 adults excavated and it has been assumed that many of the adult inhabitants of the village were not buried within the palisades and earthworks. Besides the formally buried individuals, there were many cranial and postcranial bones recovered from "refuse deposits". The highest MNI for these fragmentary remains is 35, based on mandibular portions, all but one of them adults and generally male in appearance. The presence of burnt, cut, and gnawed human bone is not unexpected and the stray bones should not be interpreted as representing the dead adult males of the village. The exclusion of the scattered bone from consideration is reasonable: the ethnohistorically documented Iroquoian practice in war was that captives, especially males, were taken back to warriors' villages to be put to death (Jackes 2004). By the same token, we may assume that adult males were lost to Roebuck village as a consequence of Iroquois raiding.

The importance of context is very evident from our final example of the value of estimators. This is an exceptional case in which ethnohistoric resources are used to determine that a cemetery sample is heavily biased and cannot be used in comparative studies. Grimsby, a Neutral Nation Iroquoian cemetery in southern Ontario, was excavated in the 1970s as a salvage operation. The entire site was excavated, so that we know the sample represents all individuals buried in the area. The ratio of children to adults indicates an extremely high fertility rate, but such an interpretation is not possible, since there is good ethnohistoric evidence that Iroquoians of that period, and specifically the Neutral, had very small families. The reasons for low fertility relate in part to the specifics of Iroquoian life (Jackes 1994). While premarital sex was allowed, self-restraint in public was enjoined and there was no privacy in the long houses; sexual abstinence was required during ritual occasions and men were away for long periods hunting, fishing, trading, or ambushing enemies. Marriage occurred upon pregnancy but only with appropriate kin and with permission. Lactation was accompanied by abstinence and the Jesuits who lived with the Ontario Iroquoians considered that the children were nursed for a very long time.[4]

Further consideration must be given to the period covered by the cemetery, 1620 to 1650. This was a time of documented war, famine, disease, and the type of social and cultural disruption that can only lead to lowered fertility (Jackes 2009a:357). Indeed, the latest burials in the cemetery appear to date to the very period when the Neutral Nation was completely destroyed by its enemies, its people killed, dispersed, or taken captive. And yet the cemetery sample indicates high fertility.

It was possible, by examining the various burials according to the time periods suggested by the European trade beads used as grave goods, to show that the middle period of the use of the cemetery was represented by very high numbers of women and children, consistent with the location having been a place of refuge from famine, disease, and war (see Jackes 2009a for supporting arguments, both archaeological and ethnohistorical). The TFR for this period would be in the range of 25–26, biologically impossible and impossible in the historical context. The final period also had high fertility and high mortality levels, with a broad range of deaths within family groupings, including not only young and disabled people, but many elders – the leaders of this society. Again, a gathering of people into an area in which there was actually no settlement seems plausible: the refugees must have included people from other Ontario Iroquoian nations, not just the Neutral.

The value of estimators, then, is that they allow us to get an idea of fertility levels and to judge whether some age distributions at death are so extreme that they must not be used for comparative purposes. Distributions that are suspect can be identified and examined in close detail for special circumstances leading to bias.

Error and Bias: Methodological Sources Relating to Current Practices for Estimating Adult Age at Death

If we can get some idea of subadult age-at-death distributions, the proportion of subadults in the sample and whether they can be taken as accurate representations of mortality in the society to which they belonged, can we also examine adult age-at-death distributions in some way?

The most problematic area of the mortality curve for skeletal biologists is from about age 30 onwards and we must recognize the constraint that this places upon us. In writing about the "osteological paradox" Wood et al. (1992) stated that the problems of adult age assessment had been solved, a proposition that seemed very questionable at the time (Jackes 1993). A decade later, Wright and Yoder (2003:49) in summarizing recent studies on the osteological paradox, were again reassuring us that the problem of adult age has been overcome, this time "by iteration against a mortality model that is used as an informed prior distribution." They argue that ages can now be estimated from the scores for the "individual components of … traditional age indicators," referring to methods of adult age assessment such as changes in the pubic symphysis. Thus, we should be able to reach a reasonable approximation of the real adult age distribution and also be able to recognize non-representative adult age-at-death distributions, for example from preservation bias. Furthermore, our statistical manipulations should not add bias to our researches. Can we accept these propositions as true?

The Bayesian approach: taking into account the influence of the reference samples' age structure

Adult skeletons of unknown age have been given estimated ages based on comparisons with standard morphological features – for example, changes in the pubic symphysis over the life of an adult – formalized into indicator stages from the characteristics of a known-age "reference" population. For many years now it has been evident that simply applying the mean age from a reference population for a certain indicator stage will not provide useful age estimates (Jackes 1985), and that smoothing and adjustment were in the past undertaken without sufficient deliberation. An early suggestion of smoothing by use of probabilities over the 95 percent confidence interval (Jackes 1985) was designed to point out the inadequacies of the use of the mean as a point estimate, and to highlight the way in which the probability ranges for each indicator stage overlap. Accurate age estimates will not be produced by this method of smoothing (Jackes 2000: figures 15.7 and 15.8). Attention was drawn to some basic problems of age estimates and to the fact that the method controlled the age estimates: the unknown age distribution of the sample does not control the age estimates (Jackes 1985). Acceptance was growing of the forceful argument that the reference population age distribution, rather than details of the method of age estimation, is a determining factor in age estimation. Bocquet-Appel (1986) summarized this well, stating that each reference population has its own "*a priori* distribution" dependent upon the history of the collection of known-age skeletons, noting that Masset (1971) had suggested that an *a priori* uniform distribution was necessary.

While there is some truth to the idea that the underlying problem in adult age determination is statistical rather than biological as Bocquet-Appel states (1986:127), there is also a slowly strengthening undercurrent of opinion (e.g., Masset 1989; see also Hoppa 2000; Jackes 1992, 2000; various authors in Hoppa and Vaupel 2002 – for summary see Hens *et al*. 2008; Jackes 2002; Kimmerle et al. 2008 with reference to females only) that indicator stages may have different trajectories of change over different populations. New techniques are now tested over several populations and found to be appropriate for some and inappropriate for others (Schmitt et al. 2002). While it is now fully accepted that Howell (1976) was correct to emphasize the underlying pattern of human mortality as uniform over space and time, there was a tendency to expand Howell's idea to encompass not only uniformity of a basic human mortality pattern, but also a uniformity of change in indicators of skeletal age. As Müller et al. (2002:4) point out with regard to age indicators, "invariance is reminiscent of the assumption of uniformitarianism" proposed by Howell. Reminiscent, but not the same. Invariance is stated by Müller and co-authors to be the "minimum assumption" necessary for palaeodemographic research, but it is certainly an assumption which requires detailed and critical study. Frankenberg and Konigsberg (2006:253) say that it is an "untestable assumption" and, indeed, it is dismissed as something that has "needlessly" occupied our attention because "many of the perceived differences in aging between samples derive from the different age structures of the study populations" (Konigsberg et al. 2008:542). And here is the whole problem: there is bias in the original reference samples, and the age structures of our archaeological samples presumably differ, as do our study populations, thereby confounding our study of whether age indicators are invariant across populations, across differing times, places, lifestyles, nutritional regimes, activity patterns. We seem to have come full circle.

In the interim, however, we can examine the adjustment of adult age distributions, based on approaches which are not constrained by assumptions of normal distributions, in contrast to distribution over the 95 percent probability range. A number of paleodemographic researchers have proposed a Bayesian approach as a means of estimating an adult age-at-death distribution, and Chamberlain (2000) laid out the essentials for applying basic Bayesian methods to the problems of estimating the ages of adult skeletons recovered from archaeological contexts. Setting out the problem for a Bayesian approach, Chamberlain's contribution in attempting to simplify the matter has led to its being recently employed (Storey 2007).

The Bayesian approach applies to situations of uncertainty, and therefore it is intuitively an excellent approach for paleodemography, since paleodemography is an area in which there is only one certainty: the individuals with whom we are dealing *are* dead. But there are aspects of the skeleton which allow the researcher to say "it is likely" that this individual was an adult who was young, middle aged, or elderly, and these likelihoods ($p(y\,|\,\theta)$) derive from the skeletal biologist's knowledge or experience of certain age indicators. The data are the age indicators from the skeletons (y), while the unknown age values to be determined are denoted by theta (θ). The Bayesian approach lies in a formulation such as: what we want to know is the probability that an individual is of a certain age, given that his skeletal characteristics (age indicators) are at a certain stage, expressed as $p(age\,|\,indicator\ stage)$, where | means "given" – thus $p(\theta|y)$. There is an underlying difficulty, that the two attributes (here stage and age) are assumed to be independent, and naturally this assumption cannot be upheld. Indeed, if the stage were independent of age, then the stage would be of no value in indicating age. However, the correlation of age and "age indicators" is imperfect, so it has been considered worthwhile by a number of researchers to attempt a Bayesian approach to the problem of age estimation.

Besides the age indicators (y), the other element in the Bayesian formula is $p(\theta)$, in this context age-specific mortality or the probability of death at certain ages. Chamberlain points out, and this is emphasized by several contributors to Hoppa and Vaupel (2002), that the probability of death at each age is an important factor in the age | stage formulation. As noted earlier, age-specific mortality is not uniform across all adult ages. There is a lower probability of death at some adult ages, and a higher one at other adult ages. Thus the specific adult mortality pattern (the variation within the overall basic human pattern of mortality), one of the unknowns which a skeletal biologist seeks to determine for an archaeological population, is itself an important component of the probability of an unknown adult skeleton having a certain age. Chamberlain proposes the use of model data, and Boldsen et al. (2002) the use of appropriate historical data. The age-specific probability of death in the chosen data set constitutes $p(\theta)$, which is called the "prior" probability.

Thus, to our knowledge (derived from the reference population of known-age individuals) of the age distribution of age indicators ($p(y)$), we can add our knowledge of human age-specific mortality ($p(\theta)$), by using perhaps a model age-at-death distribution, and in this way we can gain some idea of the probability ($p(\theta|y)$) of a certain age (the unknown parameter θ) for a skeleton with a specific age indicator morphology. We do this by multiplying the likelihood $p(y\,|\,\theta)$ by the "prior" belief as to the age-at-death distribution ($p(\theta)$), to give a "posterior" probability $p(\theta|y)$ of the age given the indicator stage (Litton and Buck 1995; Lucy et al. 1996). We can then use this "next step" or "posterior" probability to suggest the age distribution in a sample of which nothing is known, beyond the morphological indications of age.

This is theoretically interesting, but does it produce satisfactory results? Since we are dealing with so many unknowns, paleodemographers must always be skeptical. A straightforward test should therefore be applied to allow us to see whether Chamberlain (2000:107) is correct in his suggestion that a Bayesian approach "is simple yet effective in removing the influence of the age structure of the skeletal reference population". Chamberlain works with the contingency table of the original data from the Suchey–Brooks pubic symphysis (Brooks and Suchey 1990) reference sample. The reference data are biased toward late adolescents and young adults, because the sample derives from forensic cases in a late 20th-century American city, therefore not representing normal attrition by death. The issue is then whether the Bayesian approach can remove the effect of this biased reference sample (given in Chamberlain 2000:108, table 1).

Here we will use known-age data from two sources and archaeological data from one source in order to test the approach. The known-age samples are Spitalfields, derived from a London church crypt (generally 18th century), and Coimbra, a Portuguese (generally 20th century) anatomical sample: the archaeological sample is from early mediaeval Germany (see Jackes 2000:425,430 for details). The two known-age samples are highly significantly different from each other, as are the German and Coimbra indicator stage distributions. Spitalfields and the German distributions are not statistically different. We should therefore find interesting similarities and differences in the estimated age distributions.

In Table 5.2 we have the data to be tested, and we can use not only the reference, uniform, and model prior probabilities of death, but we can also test the efficiency of the method in allowing us to estimate ages at death by using the actual known distribution of ages at death, as given in the last column in Table 5.3, expressed simply as a ratio of number in age category to sample total. In other words, the multiplication of the sample priors by the reference sample distribution across the age and stage matrix gives the values in Table 5.3.

Table 5.4 gives the posterior probabilities from which to derive the age-at-death estimates for Coimbra females. Generating an age category by stage matrix for the Coimbra females is a simple matter of multiplying the posterior probability by the appropriate Coimbra stage sample. For example (using figures from the first cells in Table 5.4 and Table 5.2) .98 × 15 gives the number of individuals estimated to

Table 5.2 Suchey–Brooks pubic symphysis stage known age and archaeological samples to be tested

Stage 1	*Stage 2*	*Stage 3*	*Stage 4*	*Stage 5*	*Stage 6*	*Total*
Coimbra female pubic symphysis stages $p(x)$						
15	2	12	27	30	14	100
Spitalfields female pubic symphysis stages $p(x)$						
3	4	9	17	14	9	56
German archaeological female pubic symphysis stages $p(x)$						
9	1	11	14	9	9	53

Table 5.3 Coimbra female pubic symphysis stages and female age distribution. The last column shows the probability of death $p(\theta)$ or sample priors and derives from the actual known distribution of ages at death

10-year age categories	*Stage 1*	*Stage 2*	*Stage 3*	*Stage 4*	*Stage 5*	*Stage 6*	*Total: Coimbra mortality $p(\theta)$*
15	0.07	0.04	0.01	0.00	0.00	0.00	0.120
25	0.00	0.03	0.05	0.03	0.01	0.00	0.120
35	0.00	0.01	0.02	0.06	0.06	0.02	0.180
45	0.00	0.00	0.01	0.02	0.05	0.06	0.140
55	0.00	0.00	0.00	0.01	0.03	0.08	0.130
65	0.00	0.00	0.00	0.02	0.01	0.11	0.140
75	0.00	0.00	0.00	0.00	0.06	0.04	0.100
85	0.00	0.00	0.00	0.00	0.02	0.05	0.070
Total	0.07	0.08	0.10	0.14	0.25	0.36	1

Source: Santos personal communication, February 11, 1999.

Table 5.4 Posterior probabilities for the Coimbra female pubic symphysis sample

10-year age categories	*Stage 1*	*Stage 2*	*Stage 3*	*Stage 4*	*Stage 5*	*Stage 6*
15	0.98	0.45	0.15	0.00	0.00	0.00
25	0.02	0.38	0.47	0.21	0.05	0.00
35	0.00	0.17	0.24	0.45	0.24	0.05
45	0.00	0.00	0.10	0.13	0.21	0.17
55	0.00	0.00	0.04	0.06	0.14	0.23
65	0.00	0.00	0.00	0.15	0.04	0.30
75	0.00	0.00	0.00	0.00	0.24	0.11
85	0.00	0.00	0.00	0.00	0.07	0.15
Total	1	1	1	1	1	1

Table 5.5 Comparison of Coimbra female known age and age estimates (n = 100), derived by several different methods. The age-at-death distribution of the pubic symphysis reference sample is shown in the last column

10-year age categories	*Known age*	*Estimated from known age posteriors*	*Estimated from reference posteriors*	*Estimated from uniform posteriors*	*Estimated from model posteriors*	*Los Angeles reference age distribution (%)*
15	12	17	19	18	17	30
25	12	14	25	16	18	25
35	18	23	21	18	21	14
45	14	13	14	12	15	11
55	13	10	12	9	13	11
65	14	10	5	9	10	5
75	10	9	3	11	5	2
85	7	4	2	7	0	1
Mean age	51.6	46.6	40.7	47.9	43.4	37.1
DI*		12.00	22.36	11.00	15.80	31.60

*Dissimilarity Index expresses the sum of the absolute differences between two distributions, here the difference from the known age: the possible range as calculated here is 1–100.

fall both within the age category 15–24 *and* the Suchey–Brooks Stage 1 age indicator phase. And summing these multiplication products across will provide the distribution of ages at death (Table 5.5, the column headed "Estimated from known-age posteriors").

Table 5.5 demonstrates that no generated adult age distribution approximates the real adult age distribution. Our tests for Table 5.5 include using: (i) a distribution exactly like the real age; (ii) a distribution exactly like the Los Angeles forensic sample, biased toward younger adults; (iii) a test assuming a uniform adult age-at-death distribution; (iv) a test based on the assumption that the West 1 model adequately reflects the age-at-death distribution. Were we to know with absolute certainty the real age distribution, we could still not produce an acceptable estimate of the mean age at death. We can see that the mean age at death estimated from the reference data is much closer to the actual Los Angeles female sample mean age at death (given in the last column in Table 5.5) than it is to the known Coimbra mean age at death. The Coimbra age at death is fairly uniformly distributed across the age samples, so the uniform estimates perform best: the Coimbra sample was selected as a test of age-assessment methods and does not reflect a normal adult death assemblage. Of course, a uniform age-at-death distribution for adults is not a reasonable choice in data manipulation (di Bacco et al. 1999).

What happens with Spitalfields, a real distribution in which deaths are concentrated within the 45–74 age range? Table 5.6 records that the Dissimilarity Index is highest when the Los Angeles reference sample distribution is used: the Spitalfields female distribution has a mean age at death which is 20 years older than the Los Angeles reference sample. The assumption among osteologists in the past was that mean adult ages at death were young so that it does not matter if we use "seed

Table 5.6 Comparison of the known age Spitalfields female pubic symphysis (n = 56) with the estimates derived from several different methods

10-year age categories	*Known age*	*Estimated from known age posteriors*	*Estimated from reference posteriors*	*Estimated from uniform posteriors*	*Estimated from model posteriors*	*95% probability distribution*
15	4	6	7	6	6	9
25	3	7	17	11	13	12
35	4	7	12	11	13	12
45	13	13	8	7	8	10
55	11	8	7	5	8	7
65	12	9	3	6	6	3
75	5	4	1	5	3	1
85	4	2	1	4	0	0
Mean age	57.4	50.8	41.1	49.0	44.6	40.8
DI* from known age		16.1	44.6	40.8	36.5	41.5

*Dissimilarity Index expresses the sum of the absolute differences between two distributions, here the difference from the known age: the possible range as calculated here is 1–100.

data" for our reconstructions reflecting this young adult mean age at death (Weiss 1973 exemplified this and the assumption influenced tests of age-assessment methods, see Jackes 1992:196). The assumption that people rarely survived past 50 and that the adult mean age at death was very low becomes a self-fulfilling prophecy when it controls methods. Table 5.6 makes it clear that using a sample with a distribution similar to that of the Los Angeles reference sample which has a mean age at death of 37 (Table 5.5, last column), will give a mean age which is barely more accurate than that calculated from the 95 percent probability ranges of the reference sample indicator stage distribution. The 95 percent probability range will provide nothing more than a slight reduction of inaccuracy over using the mean age of each stage in the reference sample (Jackes 1992:198).

The bias of the reference sample age distribution and the consequent bias of the method derived from Los Angeles pubic morphology are reduced by this Bayesian method, but the correct ages cannot be arrived at, even when we have absolutely precise and accurate knowledge of the age distribution of the sample. Of course, we do NOT know the shape of the adult age distribution in archaeological samples. The only data we have, archaeologically, is the distribution of pubes across the Suchey–Brooks pubic symphyses indicator stages. The age estimation technique obviously exemplifies the frequently cited difficulties of under-aging of older individuals, resulting in a heaping of individuals in younger age classes. The pattern has recently been confirmed in a test on a sample of 390 known-age Sardinian pubes, resulting in "a shift in both sexes from slight overestimation of age to underestimating age after age 40. "Age predictions over age 60 are drastically underestimated by 25.2 years" (Hens et al. 2008:1041). The pattern of under-aging old adults and over-aging young adults was identified by Masset in his work on

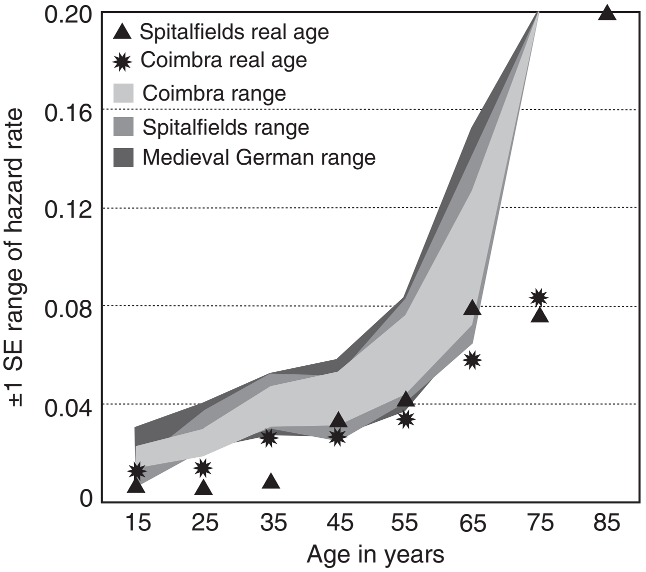

Figure 5.4 Comparison of the hazard function calculated directly from the known age distributions (Spitalfields triangles; Coimbra stars) with the Bayesian estimated standard error range of the hazard rate for Coimbra, Spitalfields, and medieval German archaeological samples. The estimated ranges are more or less identical and do not include the significantly different known-age distributions.
Source: Author's drawing

Portuguese skulls as "attraction of the middle" (see e.g., Masset 1989:82), and suggested that probability matrices would eliminate this error (Masset 1990:113), but he specified the need for standardization of age distributions.

It seems then that developing age-at-death distributions from age indicators and attempting to redistribute those distributions by choosing a model or historical example will lead to mortality patterns that reflect not the real age at death, but the methods of age assessment and, more importantly, the methods of data manipulation, specifically here the model data used. In Figure 5.4 we see that our known, very different, distributions and an unknown archaeological distribution will be rendered basically identical by the methods employed: this is illustrated by the standard error of the hazard rate. The hazard rate is derived from the probability of dying (q) and surviving within each age interval, taking the width of the age interval into account: $2q/n(1 + p)$, where n is the width of the age interval. In Figure 5.4 the actual known-age hazard functions for Spitalfields and Coimbra females are plotted and are clearly somewhat different from each other because the Coimbra sample has a fairly uniform probability distribution, while the Spitalfields distribution is weighted toward individuals aged 45 to 70. In both cases the *actual* known-age hazard rate falls outside the range estimated from model probabilities. But the further important point made by this diagram is not that the *actual* hazard functions do not lie within the SE ranges, but that the two SE ranges derived from West 1 r = 0 model adult age distribution are more or less identical, despite representing two quite different age-at-death distributions.

It is important to be clear about this. If we manipulate two completely different actual age-at-death distributions using probabilities from a specific mortality model, those two manipulated distributions will end up resembling the mortality model, not reality. If we now add archaeological data (Table 5.2) and derive the hazard rate expressed as the SE range by using the West 1 r = 0 model mortality profile, we will find the result to be the same (Figure 5.4). The mortality pattern from which we derive the probabilities will overwhelm the effect of the indicator stage data. In the past, we have been satisfied with age-assessment methods that did not indicate the presence of many elderly people within archaeological sites. Since the Bayesian priors are by definition something that we choose because they fit with preconceptions, we are in danger of forcing paleodemography into the mold of our ideas about the past, adding bias to bias.

To sum up, since the majority of methods for aging adults tend to heap individuals within certain younger age categories (well illustrated by Aykroyd et al. 1999: figure 1), it has been usual to envisage a high mortality among young adults. If the Bayesian approach of using what is considered most likely for the probabilities, and the researcher's best guess is similar to West 1 r = 0, then we would get a hazard rate consistent with that shown in Figure 5.4. Instead of age determinations controlled by the reference sample bias, we are adding methodological residue onto "the great unknown" of the actual age-at-death of an archaeological skeletal sample of adults.

As Boldsen et al. (2002:78) say, "any assumption about an informative prior can create a tautological circle". In that the estimated age distribution of the target sample will come to reflect or reproduce major features derived from exogenous data on mortality, Boldsen and his colleagues are correct. We will be imposing another layer of assumption on that already derived from the assumption of "invariance". We need a clear demonstration of the accuracy of bioarchaeological age indicator stage data manipulation before we can be certain of even an estimated adult mean age at death (Jackes 2000, 2003).

Taphonomy, macroscopic age indicators, and demography

Taphonomy refers to processes that act on organic matter after death. Taphonomical processes may lead to bias caused by differential preservation in a collection of bones. An important aspect of the biased representation of skeletal elements among sites is that age indicators may be unevenly represented, and this is particularly difficult to deal with when multifactorial age assessments are used. While not universally accepted (Saunders et al. 1992), most osteologists would probably agree that "… 'multifactorial' methods for age estimation are preferable to the use of a single ordinal categorical system" (Konigsberg et al. 2008:556). Unfortunately, what may be applicable in forensic situations, or in theoretical discussions on age assessment and paleodemography, can be very different in the situations bioarchaeologists confront.

Because preservation differs among sites, the methods used to age adults cannot be fully comparable across sites. For example, only about 50 percent of known-age individuals in the Spitalfields coffin burial collection could be examined for pubic symphysis age indicators (Jackes 2000). The lack of pubic symphyses can be even more extreme in collections made before modern excavation techniques came into common use. Excavation techniques and the value placed on different types of archaeological materials, leading to differential retention and curatorial care, no doubt explain why precisely one pubic symphysis is now to be found among the 19th-century collection from the Portuguese site of Cabeço da Arruda, despite the fact that over 70 individuals aged 15 and more were excavated (Jackes and Meiklejohn 2004). However, it may be no more dangerous to the accuracy of our interpretations to have no representation of a certain age indicator than to have partial representation.

A very clear example of the bias introduced by partial representation is provided by paleodemographic analyses of Ossossané, a fully excavated Huron ossuary from the first half of the 17th century which was studied for adult age distribution several times before the material was reburied. As originally analyzed (Katzenberg and White 1979) the demography was based on a sample size of 249. The right innominates were used, and although many more than 249 were counted, the life table was drawn up using only the innominates to which ages were given (see Jackes 1985 for summaries on methods). The *n* = 249 life table has been used in comparative studies with acknowledgment (Jackes 1986:35) or no acknowledgment (Bocquet-Appel and Naji 2006) that the study was preliminary. Jimenez and Melbye (1987) used the right mandibular P_3 socket to establish an MNI and arrived at a total of 419. Laroque (1991:241) gave the MNI based on mandibles as 447.

In order to show the effect of the differences in the three studies, we can specify the demographic estimator values (Table 5.7). As demonstrated above, this technique is of particular value in allowing comparison across data sets despite the use of different methods of analysis. Restricting the demographic analysis to those adults who could be aged by pubic indicators resulted in an extremely biased interpretation which we know from other sources was inaccurate: the Ossossané TFR estimate should probably be in the range of 4 to 5 (Jackes 1994, 2009a).

Recognition of this effect has been slow. As Wittwer-Backofen et al. (2008:385) say, "Differential preservation of skeletal elements is one of the most underrated confounders when it comes to age estimation within the bioarcheological context." Finally, we are beginning to get forceful statements such as "… when age markers are assessed independently, significant shifts in the resultant age structure can be observed. Both the number and selection of traits, and the state of preservation might impact the final paleodemographic reconstruction" (Wittwer-Backofen et al. 2008:394). This statement is, however, not forceful enough – "might" should be replaced with "will" in this quotation.

Table 5.7 Demographic estimators for Ossossané ossuary

Source for MNI and ages	*MCM*	*J:A*	*Estimated TFR*
Katzenberg and White 1979	.120	.283	9–10.7
Jimenez and Melbye 1987	.080	.118	~4.5–6.5
Larocque 1991	.053	.128	4.4–4.7

Diagenesis, microscopic aging techniques, and demography

We discussed above some of the problems relating to macroscopic techniques of adult age estimates. One aspect of taphonomical biasing is relevant to microscopic methods of age assessment. Diagenesis refers to specific changes undergone by bones and teeth after death. The changes can be chemical or they can be alteration of the microstructure. At one time there was intense interest in age assessment based on histological changes in bone cortex (Robling and Stout 2000 provide an excellent overview of publications from 1964 onwards). Since the standard deviation of the age prediction by regression is usually high (Maat et al. 2006), the method cannot give precise ages. With further testing, the evaluations of the technique have become much more critical (e.g., Lynnerup et al. 2006; Paine and Brenton 2006).

While forensic specialists see less hope of accuracy in ages at death, bioarchaeologists also see the limitations caused by bacterial destruction of cortical microstructure. Jans et al. (2004), in a large-scale study, found that 75 percent of human bone from a variety of contexts was biologically altered. Work in Portugal has shown that bone from sandy floored caves is much less likely to be altered than bone from limestone caves or from open-air sites (Jackes et al. 2001:418). Site-specific variability in the extent to which adults can be aged by examining cortical microstructure will again reduce the comparability of results across sites.

Since cortical bone diagenesis will not necessarily affect all individuals equally, there is the chance of compounding bias by differential destruction of older individuals' bones. Cortical width decreases and cortical porosity increases with age, especially in females (Cooper et al., 2007), making the elderly, and especially elderly females, more vulnerable to postmortem biological destruction of bone cortex. Add to this the under-aging of older adults by osteon counting (Walker et al., 1994) and it is unlikely that archaeological adult age-at-death distributions will be accurate or unbiased.

I have supported estimating age from microscopic examination of tooth roots using cemental annulation counts for the purpose of calibrating attrition levels in archaeological dentitions, specifically from Casa da Moura, a Neolithic Portuguese site (Jackes 1992, 2000). However, I do not believe that cross-sectioning tooth roots will provide accurate assessments of the age of all individuals in a sample. In my experience, even the roots of beautifully preserved and almost unworn large male upper canines may be unreadable because of destruction by bacteria. My experience is not unique: two dedicated laboratories were unable to estimate ages for over 50 percent of the individuals they had sampled (Geusa et al. 1998, data at section 5.2) from among people buried at the Isola Sacra necropolis, Ostia Antica (second and third centuries A.D.). Individuals assessed as under 20 years of age had the highest level of unread cemental annulations. Furthermore, the age estimates differed by up to 29 years between the two laboratories (Figure 5.5), one using phase contrast microscopy (Munich) and the other complex digital image processing (Rome). Wittwer-Backofen et al. (2008:388) have also noted "surprising" differences and "unexpectedly low [consistency]" in cemental annulation counts.

The lack of consistency in counting illustrated by Figure 5.5 is not surprising based on my experience using thin sections, 25–30 micrometers thick, generally

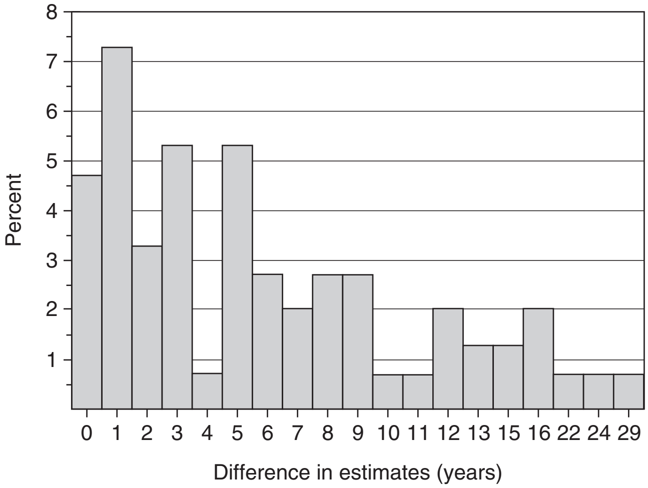

Figure 5.5 Data on cemental annulation analyses of 150 individuals from Isola Sacra necropolis, Ostia Antica, Italy. Of the 150 sectioned tooth roots, 53.3 percent were deemed unreadable by one or both laboratories. Age estimates based on range midpoints.

Source: Author's drawing, derived from Geusa, G., L. Bondioli, E. Capucci, A. Cipriano, G. Grupe, C. Savorè, and R. Macchiarelli 1998 Osteodental Biology of the People of Portus Romae (Necropolis of Isola Sacra, 2nd-3rd Cent. AD). II. Dental Cementum Annulations and Age at Death Estimates Digital Archives of Human Paleobiology. Rome: Pigorini Museum

from the juncture of the middle and lower thirds of each canine root. The problem lies not simply in visualizing the annulations. Renz and Radlanski (2006) have noted that the number of lines differs according to the portion of the root examined, along the whole of the middle third of the root, as well as according to the root face. We had multiple sections from the same region of each root, oriented so that we could specify which face of the root was being examined. There is absolutely no doubt that root faces for the one tooth can differ in the number of countable annulations. Since cementum disruption occurs with heavy attrition and cemental annulation analysis is likely to underestimate the age of old people (Meinl et al. 2008), annulation counting appears to have many problems, but a major drawback for bioarchaeologists is that many sections will be unreadable.

To summarize the argument to this point, it is important that we gain as accurate an idea as possible of the numbers of individuals in archaeological burial sites. We must base our paleodemographic studies on counts of *all* individuals because our results are not comparable across different archaeological samples if we use only those individuals who can be aged by particular techniques or combinations of techniques. Taphonomical factors on the macroscopic level are of the greatest importance in our analyses and we must acknowledge this in order to avoid biasing our findings as a result of unrecognized taphonomical factors. Furthermore, the diagenesis affecting cortical bone and dental cementum means that we cannot rely on microscopic methods in our efforts to gain a full picture of the age-at-death distribution of a cemetery sample any more than we can rely on gross morphological age assessment. In both cases we need to be aware of the ways in which postmortem changes will bias our samples.

Taphonomical bias has implications beyond questions of minimum numbers of individuals and age-at-death distributions. We need to examine how to study age-related characteristics. Dental pathology is an especially important aspect of age-related change and I will focus on the analysis of dentitions, illustrating methods and problems. Jackes and Lubell (1996) and Jackes (2009b) discuss some specific factors, including postmortem alterations, which bias caries assessments and those issues will not be touched on in this chapter.

Teeth, Attrition, and Seriation: An Experimental Approach to Inclusive and Comparable Estimates of Adult Age Distributions

How then can we avoid biasing our data by relying on particular methods of adult age assessment and by excluding those to whom we cannot give ages by these methods? My own method, which I have applied whenever possible since 1984, depends on seriation of mandibles based on examination of attrition, with attention primarily directed to attrition of the posterior teeth. Two European Mesolithic sites, Moita do Sebastião and Cabeço da Arruda in Portugal, exemplify the value of using mandibles to provide a proxy for adult age at death distributions comparable across skeletal collections. The Mesolithic material was first excavated in the 1880s and cannot be dealt with entirely as single burials for a variety of reasons: burial practices led to mixed burials; apparently not all bones were removed at excavation; retained material was curated in two (eventually three) locations, in some cases with post-cranials and skulls being located apart; some material was lost early on; material at two institutions went through fires; skeletons were curated in adjacent drawers without individual skeletal elements being marked for identification so that bones were transferred among individuals (Jackes and Meiklejohn 2004, 2008; Jackes and Alvim 2006).[5]

The minimum number of individuals at Moita and Arruda was determined on the seriation of the mandibles. This approach permits the reconstruction of fragments that are broken and separated, it makes it possible to recognize which individuals are represented by isolated fragments, and thus gives a higher MNI than if individual skeletal elements are simply counted (for example, right distal humeri or left petrous portion of temporal or even one particular tooth socket). A number of elements were recorded, but none gave as high an MNI as mandibular seriation. Seriation can also be used to develop proxy age-at-death distributions for adults who originate from different types of inhumations. The total information available is, of course, much more complete when derived from single articulated skeletons, but information from a variety of burial practices can be used when the methods are comparable. Thus, one can compare Mesolithic inhumation data with Neolithic disarticulated skeletons laid on cave floors such as occurred at Casa da Moura (a Neolithic ossuary cave in central Portugal).

If each study can be taken only in isolation and we cannot compare sets of data, then we are unable to test methods, to extrapolate from our results and to study the human past across changes of time, subsistence, geography, disease patterns, and cultures. "… heterogeneity is itself a major focus of bioarchaeological research" note Wright and Yoder (2003:46), but if we cannot ensure comparability of methods we cannot identify similarities and differences and our conclusions will have little meaning. This is especially important in regard to our study of age-related change and dental pathology is a central aspect of age-related change.

Dental pathology: a study in age-related characteristics

In asking whether our osteological samples are representative we have so far ignored a fundamental question raised at the beginning of this chapter: "are the dead an unbiased sample of the living?" In terms of age, the answer is obvious – no, the dead are dead, they did not survive beyond a certain age. Further, in cases like accident and violence the dead are dead because they were more at risk of death, or the dead may have suffered deprivation or illness, or perhaps they were frailer because they had suffered but survived such insults in the past. We will return to this question below, but in order to understand it fully we need to consider the types of evidence that might demonstrate that those who die are less healthy than those who survive. And we need to consider the contexts which would allow us to arrive at a clear interpretation of skeletal changes.

An obvious approach is to study skeletons in contexts in which we hold at least some of the variables constant – geological background or the gene pool, for example – while varying others, such as diet. In other cases, we could look at a similar diet, but in different locations, or observe change through time that is not mediated by an alteration of diet. The change in subsistence brought about by a growing dependence on horticulture is an ideal test case: Cohen and Armelagos (1984) asked contributors to their volume on the agricultural transition to examine specific features of skeletons. They concluded that the contributions allowed them to discern "clear trends … which have a significant bearing on discussions of comparative health … associated with the Neolithic Revolution" (1984:585), specifically (1984:594) that the "indicators fairly clearly suggest an overall decline in the quality … of human life associated with the adoption of agriculture".

A new volume (Cohen and Crane-Kramer 2007), 20 years on, is a reprise of this test case. The book starts with a retrospective review by Cook (2007) who, in making a strong case for the importance of local and regional studies rather than broad comparisons, questions the use of specific categories of data (e.g., stature, periosteal reactions) apart from dental caries, as contributing much to studies of subsistence change.

By focusing on caries we can also partly avoid the difficulties which stem from one undoubted fact: as Wood et al. (1992:344) said, "… the only 20-year-olds we observe in a skeletal sample are those who died at age 20." How are we to interpret what we see? Are we to interpret the presence of skeletal signs of nutritional or infectious insults as evidence of a population healthy enough to survive or of a population in poor health?

The discussion around the osteological paradox has centered on the transition to horticulture because Cohen and Armelagos (1984) argued that an increase in skeletal lesions indicates an increase in ill-health with agriculture, whereas Wood et al. (1992) would argue that people were able to survive ill-health for long enough to allow skeletal responses to infection to manifest themselves. As Cohen et al. (1994:630) ask are we seeing "relative stress … [or] … relative resilience"?

The question here is "are deaths random or selected with regard to dental pathology?" The cumulative nature of dental pathology must be emphasized. Conditions that accumulate into old age will occur at a higher rate among the dead than among the living population simply because older adults have a higher probability of dying than younger adults. So it is likely that, as samples, the dead have a higher rate of dental pathology than the living. However, it may be that dental pathology rates themselves are not entirely random. It is well known that caries is multifactorial and that heritability is quite high (Bretz et al. 2005; Slayton et al. 2005; Vieira et al. 2008). While environmental and genetic factors must be involved, an interesting outcome of the Dunedin Multidisplinary Health and Development Study is that carious lesions develop in teeth at a relatively constant rate over a person's lifetime (Broadbent et al. 2008:72). Dental caries is "chronic [and] cumulative" (Broadbent et al. 2008:69).

Figure 5.6A shows us the pathology rates for the two Portuguese Mesolithic sites of Moita do Sebastião and Cabeço da Arruda based on seriated mandibles of individuals aged 15 and older. The category (*x*) axis gives a distribution of lower molar sockets across ten groupings, each with a more or less equal representation of observable sockets, around 25 for Moita and 30 for Arruda. The rate of dental pathology is higher in Moita than in Arruda dentitions (also demonstrated by attrition grade analyses: Jackes and Lubell 1996; Meiklejohn and Zvelebil 1991). Moita has pathology throughout late adolescence and young adulthood, while Arruda has much less, and an early peak at category 5 is wholly contributed by two Arruda individuals, both apparently male. In both samples dental pathology increases over the life time of the individuals, but in Moita it is more common in very old age (when the dental crowns have been worn to the cemento-enamel junction). There are so few teeth in which to observe caries at this stage that it is not possible to prove a significant difference (two caries in four lower molars for Moita and three in 16 for Arruda), but for both abscessing ($P = .022$) and pre-mortem tooth loss ($P = .008$) dental pathology is significantly more severe at Moita in category 10.

In these two sites, we have a paradoxical situation. Dental pathology, in the form of caries, is believed to have an inverse relationship to dental attrition. In the case of Moita, however, Figure 5.6B demonstrates that caries is established very early in the late adolescent/young adult dentition of a group with a high rate of attrition. For this reason, not all of the abscessing and tooth loss can be attributed to heavy attrition – caries is obviously an important contributing factor. Meiklejohn et al. (1992) have independently discussed the fact that Moita attrition and caries do not have the expected inverse relationship. They did not, however, refer to a further paradoxical situation: Moita has a higher rate of occlusal caries than Arruda (Jackes 2009b; Jackes and Lubell 1996), whereas the expectation is that heavy attrition reduces the likelihood of occlusal caries developing. In this example of analysis, we can see that it is possible to compare across samples and to confirm differences, despite the uncertainties of age assessment in archaeological material. This study is valuable in showing that dental pathology may be a marker of dietary differences

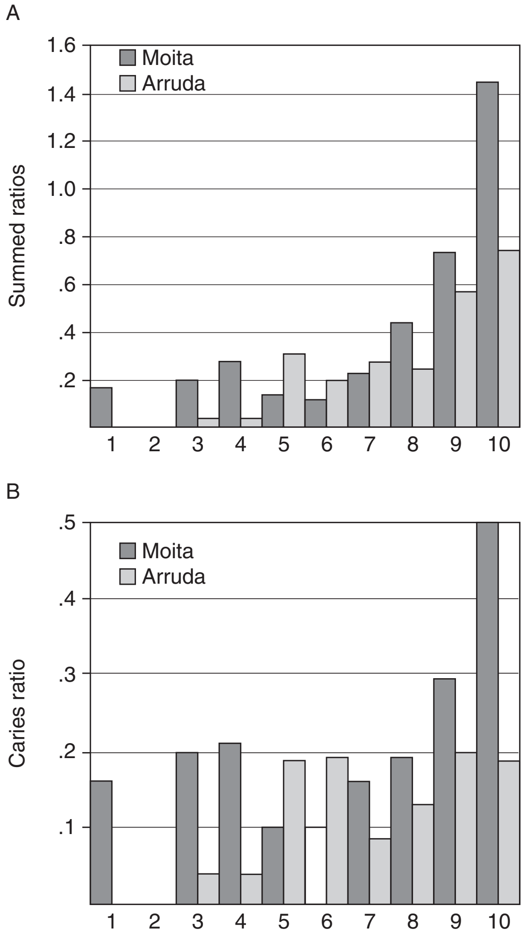

Figure 5.6 Comparison of dental pathology in two Mesolithic shell midden burial sites in central Portugal. The seriated mandibles are equally distributed across the ten categories based on representation of molar sockets. **A** The frequency of caries, of abscessing, and of pre-mortem tooth loss summed to represent pathology rate on the *y* axis. **B** The caries rate (*y* axis) – number of teeth with caries as a proportion of number of intact teeth in each category.
Source: Author's drawing

(Jackes and Meiklejohn 2008) when other variables are held relatively constant. The higher rate of pathology occurs at Moita not simply as a result of attrition and despite the possibility that a larger proportion of people survived into old age at Arruda (Jackes and Lubell 1999; Jackes 2009b).

Moita and Arruda have an overlapping time range and lie within a few kilometers of each other on either side of what was a narrow estuary in the late Mesolithic. There is no particular reason that they should differ from each other, but as in other features (Jackes and Lubell, 1999), there is evidence indicating that those living at Moita had higher rates in dental pathology than those living at Arruda and that the difference is real, not the product of bias in the cemetery samples.

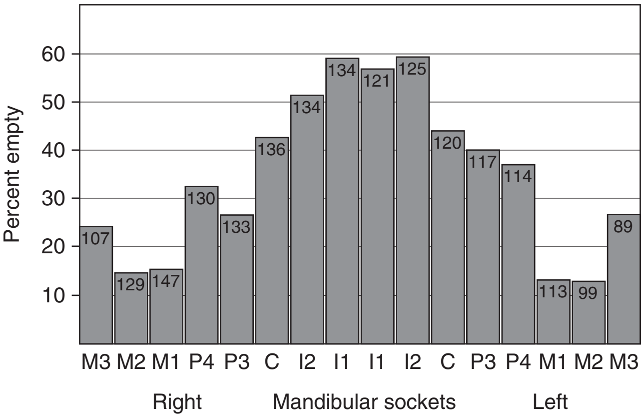

Figure 5.7 Percentage of Casa da Moura mandibular sockets empty due to postmortem tooth loss. Figures in bars refer to sample sizes for each tooth socket (highest *n* = 147).
Source: Author's drawing

The discussion above was based upon mandibles and specifically mandibular molars. There is a very simple reason for this – mandibles, mandibular sockets, and mandibular teeth are generally better represented than their maxillary counterparts (Jackes and Lubell 1996). And molars and molar sockets are the best preserved elements of mandibles. Of the preserved mandibular sockets at Casa da Moura, the Neolithic Portuguese burial cave from which disarticulated skeletons were excavated in the 1860s, 35 percent of sockets are empty, meaning that they contain no intact teeth and show no evidence of pre-mortem tooth loss. Figure 5.7 demonstrates that the incidence of postmortem tooth loss is very uneven across tooth types and that incisor sockets have the greatest chance of being empty. The simple explanation is that their small, straight, and short roots do not provide firm attachment, whereas the multiple and sometimes curved roots of lower first and second molars result in a higher retention rate after burial. The sample sizes are given for each of the sockets and it is immediately clear that they are higher on the right, the mandibular first molar socket being very much more likely to survive (the side difference is significantly different, $P = .001$). The explanation for this might be that skulls were more often laid on the cave floor on the right side, so that there would be less damage to the right mandibular rami from people moving or rearranging skeletons or from animals disturbing the bones.

The preservation of mandibular sockets and molars is general across most archaeological sites (Jackes and Lubell 1996) and a good example of this is provided by an Ontario ossuary, Kleinburg, carefully and fully excavated, curated, and analyzed. In this site, as recorded ethnohistorically for the Huron Nation (Jackes 2009a), primary burial was followed by secondary burial in such a way that one might expect full representation of every skeletal element. And yet, as is evident from Figure 5.8A, the mandibles will provide a more complete picture than the maxillae of all aspects of the dentition, of the MNI and the age distribution, of pathology and metrical and nonmetrical characteristics. Figure 5.8B, showing the

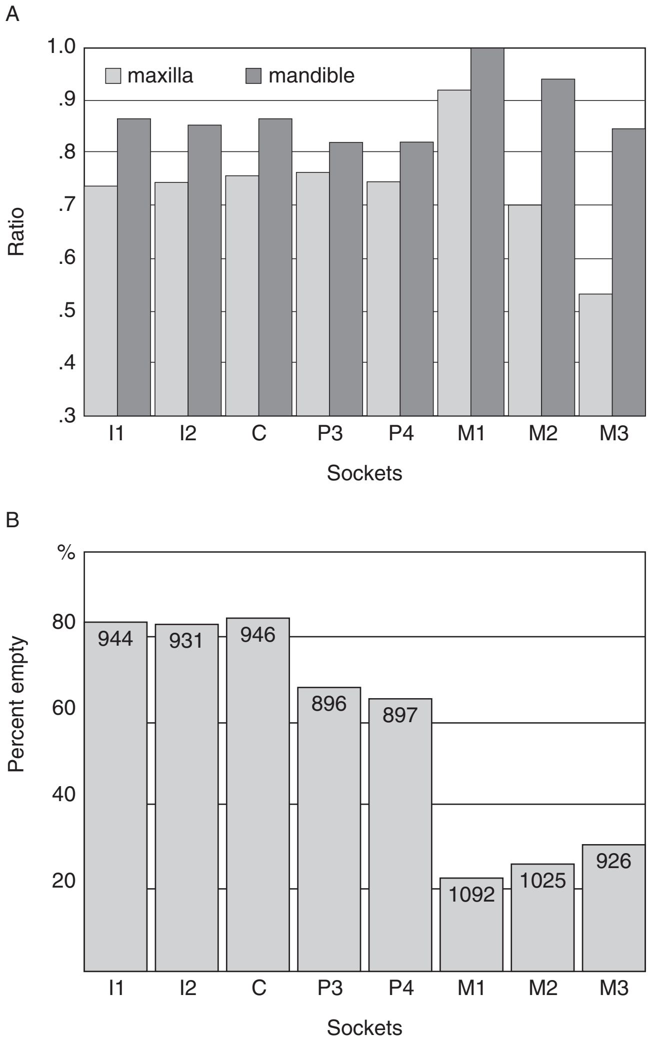

Figure 5.8 Kleinburg ossuary, Ontario. **A** Representation of permanent tooth sockets, including congenital absence, pre-mortem and postmortem tooth loss, as a ratio of the socket with highest presence (pooled sides M_1 socket, n = 1,092). **B** Percentage of permanent mandibular tooth sockets empty due to postmortem tooth loss (pooled sides mandibular sockets, n = 7,657).
Source: Author's drawing, data from Patterson, D. K. 1984 Diachronic Study of Dental Palaeopathology and Attritional Status of Prehistoric Ontario Pre-Iroquois and Iroquois Populations. Mercury Series No. 122, Archaeological Survey of Canada. Ottawa: National Museum of Man

percentage of empty mandibular sockets, demonstrates that the first and second lower molars are most likely to be retained in their sockets, as with the Portuguese sites (Jackes and Lubell 1996). Since they are also the most likely to be preserved among the loose teeth, analyses are best focused on mandibular molars for comparative studies.

Table 5.8 Summed probabilities of caries by age 16 in mandibular teeth

Mandibular teeth used in analysis														*P/n*	*n*	*P/s*	*ns*
M2	M1	P4	P3	C	I2	I1	I1	I2	C	P3	P4	M1	M2	.149	14	.033	64
M2	M1	P4	P3	C					C	P3	P4	M1	M2	.204	10	.043	48
M2	M1	P4	P3							P3	P4	M1	M2	.254	8	.051	40
M2	M1	P4									P4	M1	M2	.331	6	.066	30
M2	M1											M1	M2	.432	4	.086	20

P/n = summed probability of caries by teeth; *n* = number of teeth; *P/s* = summed probability of caries by surfaces; *ns* = number of susceptible surfaces.

Source: Calculated from Batchelor, P. A., and A. Sheiham 2004 Grouping of Tooth Surfaces by Susceptibility to Caries: A Study in 5–16-year-old Children. BMC Oral Health 4:2, www.biomedcentral.com/1472-6831/4/2.

It is important that dental pathology be studied in a careful and limited way because taphonomy biases dental samples in archaeological sites and the probabilities of caries differ across tooth classes (Batchelor and Sheiham 2004). The information given in Table 5.8 is for mandibular teeth, which have a higher probability of caries than maxillary teeth. The summed probability of caries across the five crown surfaces (four for canines and incisors which are not considered to have susceptible occlusal surfaces) is for U.S. children aged 5 to 16. Whether the probabilities for caries per tooth representation group are expressed per tooth or calculated on crown surfaces (five, and four for anterior teeth) the difference between the full set of mandibular teeth or the molars only is very clear. Caries rates are clearly shown to depend on the representation of tooth types in the sample. Therefore it is essential that caries be reported by tooth type or that tooth types included in analyses be specified.

To summarize, samples will be biased in different ways, as a result of burial practices and other factors such as excavation care and screening, conditions of curation and laboratory methods and methods of analysis, but taphonomy is of utmost importance in the reporting of dental pathology (Jackes and Lubell 1996; Jackes 2009b). Granted that mandibular sockets and teeth are most likely to be represented and that molar teeth are the most likely of those mandibular teeth to be well represented in a sample, it is best to present dental pathology for mandibular molars. Comparisons can only be made on a limited basis and in fact should be presented in association with seriation by wear. Since dental wear is specific to samples (even to individuals) this is not a perfect solution, especially since seriation becomes more difficult to apply as rates of dental pathology increase. Nevertheless, seriation of mandibles provides a means of distributing individuals by an age-dependent characteristic. It is of great importance that dental pathology be examined within the context of age and the composition of the sample in terms of age will be an important consideration for dental pathology.

A graphic illustration is provided by two Ontario sites already mentioned, the Neutral cemetery at Grimsby and the Huron ossuary at Kleinburg. It was working on these two sites – one just pre-contact and one post-contact, dated to within decades of each other and about 78 kilometers apart as the crow flies – that first introduced me to the possibility of methodological problems with adult age assess-

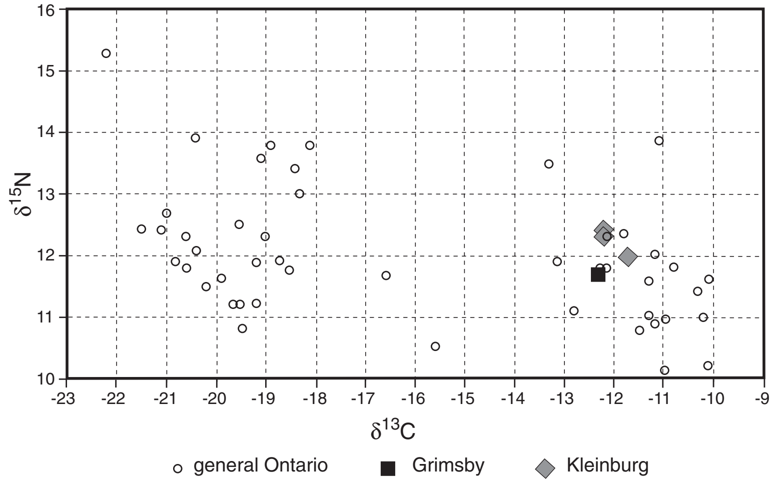

Figure 5.9 Stable isotope values for Ontario sites showing the placement of Grimsby within the data set of Harrison and Katzenberg (2003).
Source: Author's drawing, based on Harrison, R. G., and M. A. Katzenberg 2003 Paleodiet Studies using Stable Carbon Isotopes from Bone Apatite and Collagen: Examples from Southern Ontario and San Nicolas Island, California. Journal of Anthropological Archaeology 22:227–244

ment and, just as important, the possibility that samples may not be directly comparable. One problem was that there seemed to be a great deal more dental pathology at Kleinburg than at Grimsby. For example, examination of the permanent mandibular second molars for occlusal caries alone demonstrated that the difference was as extreme as 68 percent for Kleinburg and 34 percent for Grimsby. The Grimsby skeletons had been excavated as a salvage operation and had to be reburied within three months of the start of the analysis, so there seemed little chance of explaining the discrepant results by more detailed studies. One possibility was that the stable isotope values were very different: fortunately, it was possible to analyze one fragment of skull that had inadvertently escaped reburial. This analysis demonstrated that dietary differences were probably not a major factor (Figure 5.9) and it seemed likely that the explanation lay in the age-at-death distribution of the sample (Jackes 1988:125, table 116). The ratio of adults over 25 years of age to the total sample of those over 10 years of age differed remarkably between the two sites. The full explanation for this had to wait until comparative methods were developed and fully tested (Jackes 2009a). As discussed above, the Grimsby sample was highly biased in terms of age and the caries rate is not comparable with Kleinburg because of sample differences.

Do the Dead Constitute a Biased Sample? A Different Dental Perspective

We return now to the problem posed above: "Are the dead an unbiased sample of the living?" Here we are not comparing the age structure of the dead and living populations. Since "[t]he dying, on average are less healthy than the rest of the living" (DeWitte and Wood 2008:1436), we are again asking about features considered to be "skeletal indicators of health" (e.g., Steckel and Rose 2002). The "osteological paradox" introduces the concept that deaths are not random with regard to individual differences. Individual differences could relate to variations in allele frequencies relative to certain diseases, but the discussion in osteology is specifically related to the idea that some individuals are more "frail" and that these individuals can be identified in a cemetery sample by skeletal indicators of frailty. DeWitte and Wood (2008) have recently attempted to test skeletal indicators by comparing two samples of skeletons, one illustrating catastrophic mortality from the Black Death and the other, normal slow death rates from the same period. Their results indicate that linear enamel hypoplasia (LEH) of the mandibular canine most clearly indicates frailty (elevated risk of death) in normal attritional and in epidemic mortality situations (with an extremely high standard error, however, especially in regard to attritional mortality). They note that one underlying cause of the presence of the skeletal indicators and of the excess mortality associated with them is likely to be poor nutrition. We might question the assumption that a London plague pit is free of the bias introduced by the immigration of young rural people, (especially males?) and question the comparison of London with two Danish towns, still small in the 21st century. Nevertheless, we may take away the conclusion that the presence among the dead of skeletal signs of frailty indicates, at the very least, that some members of a group were subject to some type of insult, with malnutrition being one possibility.

While DeWitte and Wood (2008) obviously accept that most skeletal indicators are in fact indicators that individuals had the strength to survive past insult, they conclude that those who have survived are at increased risk of death, especially under conditions of normal attritional death. This would suggest that there is excess mortality (at younger ages) of those who show skeletal stress markers. Can we say something about the rate of stressors (in a homogeneous, unbiased sample of sufficient size), given the possibility of higher rates of mortality among survivors of stress?

We can assume from DeWitte and Wood's study that the skeletal indicator most clearly associated with increased likelihood of death is linear enamel hypoplasia (LEH). Is it possible to study LEH in older adults? In the sample of Portuguese Neolithic loose teeth from Casa da Moura, LEH might be best recorded only in younger adults because rapid wear removes crowns (Figure 5.10) and wear affects the labial and lingual surfaces as well as the occlusal surfaces (anterior teeth must have been used as tools). LEH could be worn away and unobservable unless it is very marked. However, in a first analysis of wear, 625 loose upper and lower canines were examined, and LEH that could be fitted into categories of "slight lines/pitting" or "deep lines/lines of pits" was recorded. The analysis demonstrated that although "slight" LEH increases in frequency across wear levels, "deep" LEH is also observed at higher wear levels. This was confirmed in a reanalysis of wear levels of 615 canines, in which both "slight" (31 percent) and "deep" (8 percent) LEH reached its highest frequency at wear level 4, but was very little lower at wear level 5. By wear level 6, "deep" LEH is reduced in frequency. This raises the question: does LEH in fact have any correlation with mortality at Casa da Moura? It seems more

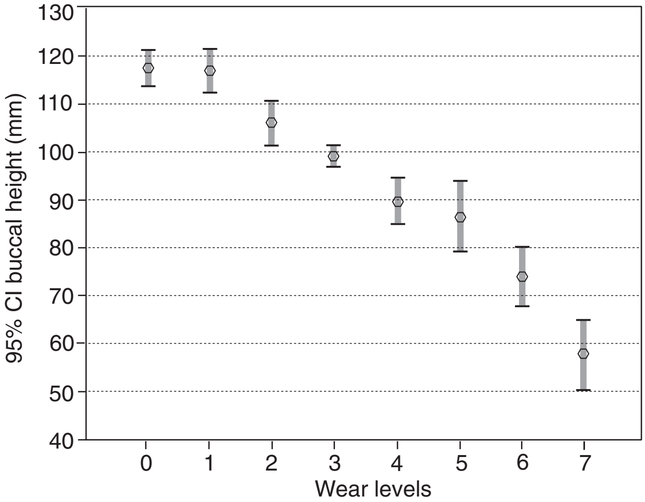

Figure 5.10 Casa da Moura mandibular canine buccal crown heights (*n* = 219, sides pooled).
Source: Author's drawing

likely that there is no selective mortality of younger individuals exhibiting LEH: mortality seems fairly neutral with regard to the presence of LEH. Across the two separate studies of Casa da Moura canine wear levels, about 30 percent display LEH; 25 percent and below for wear levels 0–2; 30 percent for 3 and 35 percent and above for 4–6; under 25 percent again for wear level 7; none in 8 (the crown being more or less completely worn away).

In summary, DeWitte and Wood's study suggests excess mortality among those who have suffered but survived past insult indicated by the presence of LEH. While we could postulate a bias in favor of LEH as observable in younger skeletons with higher rates than in older skeletons, this seems not to be true in one site with quite high wear levels. Since Casa da Moura is a loose tooth ossuary sample, we could not, of course, examine individuals, but we can study individual elements, right upper canines, for example (upper canines are used because the mandibular canine sample size is much lower). The mean percentage across sides of LEH is 30 percent in *unworn* upper canines, but lower for early wear levels. The mean percentage across sides reverts to 30 percent for upper canines in each of the second analysis wear categories 3 to 6. Thus, for Casa da Moura upper canines, we might expect about 30 percent of the population to have LEH, that slightly fewer late adolescents and young adults with LEH die, and that generally deaths are not selected for by whatever causes LEH. Whatever is the cause of lines or lines of pits appearing in developing upper canine labial enamel does not seem to predispose individuals to early death.

We have illustrated our discussion on whether the dead themselves represent a biased sample (beyond the age structure of the dead) on two different types of dental data: dental pathology, cumulative through life and likely to appear at higher rates among the oldest of the dead; LEH, established early in life and likely to have disappeared from the worn teeth of the oldest of the dead. In each case, the suggestion is that we have gained some insight into the incidences of these dental features, not only among the dead, but also among the living.

Summary: Not Just One Type of Bias …

Are bioarchaeological samples of human burials biased? Yes. They are biased by being representative of the dead, not the living. Are the dead biased? In one sense, no, because everyone dies. But when, why, how, and where each person dies is another matter. And, as we have seen, that presents us with difficult issues that really cannot be solved universally.

Bioarchaeologists face multiple problems. We must consider the burial practices, the determinants of differential preservation, the excavation history, the post-excavation curation for each of our samples. But above all, we must try to understand the context and the demographic characteristics of the population that has provided our sample. Our only hope of arriving at an understanding of all these factors is to use methods which will allow detailed and careful comparisons among samples that *are* comparable. Samples must be comparable, not only in terms of chronology and geography, but in terms of size and constituents, so that meaningful interpretations can be drawn from them.

We must also be careful not to use methods which could add bias in such a way that our results cannot have real meaning. In the 1980s it was a struggle to get anything published that pointed out that skeletal age assessment added methodological bias. Progress has been made, but we still see analysis of age-dependent factors by our methodologically biased age categories. We cannot ignore the circularity of categorizing skeletons on age-dependent factors and then analyzing those same age-dependent factors by the derived age categories. We need to choose our age-proxy characteristics carefully. In the 1980s it was almost unheard of for anyone to suggest that some skeletal samples might be quite unsuitable for demographic analysis. Even raising that question almost ensured that publication referees would be very negative in their assessments. Today published reports on age-dependent features still draw conclusions from apparent differences without any acknowledgment that sample characteristics may determine those differences. Dental pathology is still reported as crude rates without reference to the age structure of the sample or differential preservation of tooth types, let alone the myriad other factors that bear on teeth throughout the human life span. Papers are published which draw sweeping conclusions encompassing thousands of years and continent-wide geographic spreads based on flawed samples.

We have problems enough in trying to deal with the unknown age distribution of the dead, without adding methodological bias deriving from faulty techniques of age assessment or preconceptions of past populations. Interpreting differences among samples is difficult enough, without muddying our results by refusing to examine closely the nature of those samples. It is only by constant testing of methodologies and assumptions that we will improve our methods and our understanding of the past.

ACKNOWLEDGMENTS

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NOTES

1 The Hutterites studied by Eaton and Mayer (1954) practiced universal marriage and no contraception of any type in the first half of the 20th century. The study led to the proposal that the maximum TFR for human beings would be between 12 and 14. This is not the biologically possible maximum fertility for individuals but it is well above recorded total fertility rates. Hern (1977:363) records the highest known TFR of 9.94 in the Shipibo village of Paococha in the Peruvian Amazon in the 1960s resulting from contact with Western influences and declining rates of polygyny. In Figure 5.3 the star marks the theoretically high TFR estimate of 12.5 derived from J:A ~.380 and MCM ~.135.

2 Bocquet-Appel and Naji (2006) appear to have used the same data as is in the HNWH database for Irene Mound ($n = 170$). The Irene Mound data as presented by Russell et al. (1990:43, table 3–4, $n = 142$) is also problematic, with a TFR range of 8.1 to 19.8 (see Figure 5.3 for both data points). A total of 280 individuals was excavated from the site (Powell 1990). Tremaine (Vradenburg 1999), another site used by Bocquet-Appel and Naji (2006), is very like Irene Mound with a TFR range of 8.6 to 18.4: 25 percent of individuals were given ages from 15 to 19 (see Figure 5.3).

3 Note that Bocquet-Appel and Naji (2006) have an error in the figures for Schild Late Woodland 5–19 age group: P should be .352, not .253 (Droessler 1981). Kuhlman Mounds $P = .410$ appears to be an error resulting from the 15–24 age group (Garner 1991) being treated as 15–19. P should be .303.

4 Where weaning is delayed, lactational amenorrhea will be a reasonably effective deterrent to conception well beyond six months postpartum (up to 97 percent effective at 12 months – Ramos et al. 1996), even without nursing abstinence.

5 Stodder (2008) includes what happens to bones after excavation within her broad-ranging study of taphonomy.

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