DISCUSSION AND CRITICISM

On the Use of Microstructural Bone for Age Determination

by DEBRA L. MARTIN, ALAN H. GOODMAN, and GEORGE J. ARMELAGOS
Department of Anthropology, University of Massachusetts, Amherst, Mass. 01003. U.S.A. 20 i 81

Pfeiffer (CA 21:793–94) has presented a comparison of age-at-death estimates of six adults from a Late Archaic burial site in Ontario. Age estimates based on macroscopic observations of the pubic symphyses are compared with those based on microstructural observations of undecalcified thin sections from femoral midshafts. By examining the “degree of correspondence” of the microstructural age estimates with estimates by the more traditional method, Pfeiffer attempts to determine the accuracy of the microscopic technique. She concludes that when macroscopic analysis is impossible “one can confidently substitute microstructural age estimates for the more traditional pubic-symphysis age estimates” (p. 793). We shall show that the data presented do not support this conclusion and attempt to explain the lack of concordance between age estimates. At issue are problems of small sample size, Pfeiffer’s interpretation of these data, problems in the use of the aging technique, and lack of concern with factors other than age which may affect the microstructure of compact bone. Our purpose is not to argue that microstructure should not be used for aging, but rather to point out that it may be affected by factors such as nutrition and disease stress. Variation in age estimates such as Pfeiffer presents for older individuals supports a tentative hypothesis that such stresses may have been active in the population under study.

Pfeiffer’s data include results of pubic-symphysis age estimation by two methods (table 1), one based on the McKern and Stewart (1957) methodology and the other on the Gilbert and McKern (1973) standards. These are compared with femoral-microstructural age estimates based on the norms of Alhqvist and Damsten (1969). Pfeiffer contends that the macrostructural ages determined from the pubic symphyses are comparable with the microstructural ages. She presents results of a t-test comparing microstructural ages with the less variant Gilbert and McKern macroscopic ages and then, at another point, cites a comparable McKern and Stewart age. She does not present the results of a comparison of microstructural ages with the McKern and Stewart macroscopic ages. This is unfortunate, since the mean presented for the McKern and Stewart ages (31.3 in table 1) is incorrect. We calculate a mean of 30.5 years, 3.7 years less than the microstructural mean age. Our comparison of the McKern and Stewart ages with the microscopic ages produced a Student’s t = 2.02 (two-tail, p ≤ .01, d.f. = 8). Such a small probability with such a small sample size gives reason for concern. The microstructural ages may be significantly overestimated.

When Pfeiffer’s data are examined more closely, one sees that the differences between microstructural and macrostructural ages are highly variable by individual; the range of differences extends from 0.2 to 14.1 years. Struck by the variability in age estimates generally found in the older individuals, we compared the microstructural ages of three older individuals (Burials 3, 10, and 19) with their macroscopic ages by the two standards. Both comparisons yielded highly significant t values (t = 3.48 for McKern and Stewart [two-tail, p ≤ .05] and t = 3.23 for Gilbert and McKern [two-tail, p ≤ .05]). On the basis of statistical reasoning, and contrary to Pfeiffer’s conclusions, the microstructural age does not seem to be a reasonable predictor of macrostructural age in this population with respect to the total sample, and especially for the subsample of older individuals.

There are several reasons, both methodological and theoretical, for the discordance between aging techniques. The original Kerley (1965) aging technique was updated by both Kerley and Ubelaker in Ubelaker (1978), with a change in size of field and new regression equations. Exact replication of the technique is difficult because of inherent differences in microscopic lenses, sizes of microscopic fields, and precise thicknesses of the sections (Stout 1978). These variables are crucial to the accuracy of the method. Indeed, Stout and Simmons (1979) point out that the accuracy of any microscopic index is compromised when different investigators perform the same measures on tissue of varying volumes. The Alhqvist and Damsten (1969) technique used by Pfeiffer, while much easier to use than Kerley’s, is less accurate and has a larger standard error. Further, Alhqvist and Damsten employed a very small sample (20), predominantly individuals over the age of 50. Accuracy is yet further reduced when only one bone is analyzed from each individual because of variability in osteonal apposition and drift in different bones (Kerley 1970).

We are not arguing that compact bone does not follow a predictable pattern of osteonal growth throughout the life cycle; rather, the question concerns the ranges of variability and the factors which can potentially affect the normal pattern. It is clear that while the techniques pioneered by Kerley do address the normal age changes in compact bone, the methods for assessment have yet to be tested, standardized, and replicated on large and homogeneous autopsy, clinical, or archaeological populations. What has been established in biomedical (Frost 1964, Albanese 1977) and paleoepidemiological (Stout and Teitelbaum 1976, Martin and Armelagos 1979) studies is that internal osteonal remodelling is greatly affected by factors such as diet, nutritional adequacy, disease, biomechanical stress, genetics, gender, and aging. In fact, the occurrence of premature or idiopathic bone loss in juveniles and young adults has been firmly established (Garn 1970, Huss-Ashmore 1978, Martin 1980). Microstructure in an individual with premature osteoporosis or bone loss (possibly for a variety of reasons) will appear older than it really is.

Factors such as these can produce wide variation within the normal and predictable process of internal remodelling throughout the lifetime. With increasing age, the likelihood of encountering these exogenous stressors also increases. This may explain why the differences in the ages were greatest in the older adults in Pfeiffer’s sample. Pubic symphyses are not as strongly affected by these factors and are therefore more stable indicators of developmental age. Microstructural analysis can be useful in the ossuary or forensic context for identification of individuals’ approximate ages. It can be more valuable, however, in the biomedical or paleopathological context for analysis of population response to environmental stress.

Reply

by SUSAN PFEIFFER
School of Human Biology, University of Guelph, Guelph, Ont., Canada N1G 2W1. 22 n 81

Martin, Goodman, and Armelagos have adopted a tone of controversy when in fact there may be very little. They note the greater disparity of age estimates in the older individuals, as well they might. My report also noted this, including the observation that for one older individual the age estimates do not overlap within the range of the 90% confidence intervals. I went on to note that the three older individuals demonstrate

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Ossified soft-tissue elements. Hence environmental or developmental factors may be relevant here as well.

I stated my results tentatively because they are obviously equivocal. Indeed, in research completed since my report was submitted to CA, age estimations based on pubic symphyses and microstructure appear to show an amplified pattern of disparity (Pfeiffer and Lynn 1980). This more recent work is based on a large ossuary sample and demonstrates statistically significant age differences among young adults and among old adults. The age estimates based on microstructure are older in both categories.

In interpreting such results I concur with most of Martin, Goodman, and Armelagos’s reservations regarding the accuracy of microstructure age estimates. However, I am not so convinced as they apparently are of where to lay the blame. We do not know unequivocally that pubic symphyses “are more stable indicators of developmental age” for prehistoric populations that may have been influenced by a wide variety of environmental stressors. Not too long ago (Ubelaker 1974, 1978) an opposite argument—that microstructure ages were likely to be more accurate—was widely accepted. If we see discrepancies today, can we be sure that the error lies in the microstructure ages? What is needed is cooperative research aimed at getting clear answers to the questions of how to age adults accurately from prehistoric samples and how to gain maximum insight into environmental stressors via the responsiveness of bone.

References Cited


More on Shovel Probes

by B. Mark Lynch

Department of Sociology and Anthropology, University of Santa Clara, Santa Clara, Calif. 95053, U.S.A. 29 i 81

Stone (CA 22:182–83), in his comment on my discussion of shovel probes and site artifact density (CA 21:516–17), raises a number of valid points. Quite obviously, sample size has an important bearing upon the results obtained from shovel probes. The major difficulty in my report, however, is not the size of the samples involved, but the unfortunate choice of the term “predict.” The intent was not to use shovel probes to predict site artifact density; as Stone quite correctly points out, shovel probes are primarily intended to discover sites. Rather, it was to illustrate the fact that site artifact density, a variable often completely ignored by archaeologists, has to be considered when designing survey strategies that rely upon shovel probes to locate sites. Indeed, as I clearly stated (p. 517), the density results “simply suggest that an archaeologist cannot arbitrarily base his decisions on interval size on considerations of area alone; relative site artifact density also warrants consideration.” The density figures presented were simply intended to illustrate this point. The model Stone proposes, which accommodates site artifact density, has a great deal of promise for evaluating discovery probability, and I hope it will be elaborated upon in future work.

More on the Day and Ancient Mexican Myth

by Michel Graulich

15, rue E. Olivier, 1170 Brussels, Belgium. 15 i 81

In his sympathetic comments, Edmonson (CA 22:179) notes that the Xiu might have left Tula in a.d. 692. Here I cannot help but quote Sahagún (1956 [1577], vol. 2:290), who in 1571 writes that 1,890 years have gone by since the fall of Tollan. Doubtless this is an error by the copyist and we should read 890 years, since elsewhere Sahagún (vol. 1:229) says that it has been almost 1,000 years since the end of Tollan. Subtracting 890 from 1571 gives us 681, a close approximation of the date suggested by Edmonson and of the one I have come to, not for the origin of the Aztec calendar, but for the period in which it coincided exactly with the seasons.

It seems that some of my hypotheses are gaining ground: in a recent article van Zantwijk (1980)—who, incidentally, was in 1979 a member of my dissertation panel—uses my ideas of the model of the day for Mexico “history” and of the cyclic Other World, with Tamoanoch as the paradise of the afternoon. He also acknowledges the importance of the false afternoon sun. However, I think that some of his modifications of my model are unfounded. In what he calls “a mythical model for Aztec historiography,” van Zantwijk locates Aztlan, the land of origin of the Aztecs, in darkness, i.e., in the underworld, whereas in my opinion Aztlan corresponds to the original