in all cases with "unacceptable" C:N results. We felt at the time that the problem might have been due to nitrogen loss. If we accept the possibility that nitrogen loss affected the C:N values, then it is necessary to identify a mechanism for such loss. When drying our samples we used a drying oven because we did not have access to a freeze dryer. We noticed that in a few cases the samples were accidentally dried at a temperature around 120°C instead of the lower temperature (about 40–50°C) intended. A number of the samples known to have been dried at the higher temperature yielded high C:N values. For example, sample 1658 was originally dried at high temperature, yielding a C:N value of 5.0, and a second extract was later dried at low temperature, yielding a C:N value of 3.8. Sample 1669 was extracted three times and dried twice at high temperature (C:N = 6.7 and 9.3) and once at low temperature (C:N = 3.2). We rejected the first high-temperature sample, and there was insufficient of the low-temperature sample left after C:N measurement for analysis. The second high-temperature sample is the one that is erroneously listed in table 1. Since we were unaware of this problem at the time, we did not record which samples were dried at the higher temperature. We do know that the second batch of samples was dried at the higher temperature, and that batch included all of the high-C:N-value samples. However, the second batch also contained a few samples with acceptable C:N values. At the moment we do not have an explanation for this, but we speculate that the slightly acid solution (pH = 3) that was maintained during the hot-water treatment may be the culprit. As the solution evaporates it will concentrate any remaining hydrochloric acid, which may cause hydrolysis and a reaction with the protein/amino-acid nitrogen such that some nitrogen is released—perhaps as ammonia. This may be a red herring, but it does need further examination as a possible explanation.

In summary, we now feel that the sample extraction methods used produced pure collagen extract (commonly called gelatin) and that the "unacceptable" C:N values do not result from the presence of contamination in the extracted collagen. While we are not certain what they do represent, we are looking into the matter. Further samples and duplicates of some of the samples looked at here will be analyzed by us and by other researchers. If the results of this study are erroneous, this should soon become evident, and appropriate corrections can be made in any interpretations based upon them.

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On Paradox and Osteology[1]

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Wood et al. (CA 33:343–58) have recently drawn attention to the "osteological paradox"—the problems inherent in reconstructing the health of a population from a sample of skeletons of the dead. Many osteologists must worry about the interpretation of the frequencies of pathologies or other changes wrought by life on the human skeleton. There can be few skeletal biologists who do not ask themselves whether higher incidences of "stress markers" indicate an increased ability to withstand stress rather than increased stress. It is certainly time for this irritant at the back of our minds to be confronted head-on.

1. A paradox is a statement that seems absurd or contradictory but is or may be true. The original, now rare, meaning is "a statement contrary to received opinion or common belief, but nonetheless true."

My very real worry about Wood et al.'s contribution is that they do not, in fact, confront osteology's most fundamental problems. They say that most of the widely recognized problems—age and sex, inadequate sample size, bias, differential preservation—can be or have been resolved.

Wood et al. introduce their discussion with the confident statement that the concerns of Bocquet-Appel and Masset (1982) with regard to the most basic problem of all—how to determine the age distribution of the dead—have been answered. Yet Buikstra and Konigsberg (1985), although claiming to prove that adult age-at-death distributions carry meaning, in fact demonstrated that only subadult values are of significance. In their later writings Buikstra and her colleagues have moved from belief in adult age distributions to the use of a ratio of juveniles to adults over 30 years and, subsequently, of a ratio of juveniles to adults over 20 years, a definite though unacknowledged acceptance of the position of those who question the accuracy of adult age assessments and the use of life tables (e.g., Angel, Masset, Bocquet-Appel, and Jackes: for discussion and references see Jackes 1992*a*).

Nor have Van Gerven and Armelagos (1983) answered Bocquet-Appel and Masset (1982). Their initial argument hangs on the methods of age assessment at two sites: one was aged by changes in the os pubis, while in the other (Batn et Hajar) the pubic stages were smoothed by references to attrition and degeneration (Van Gerven, Sandford, and Hummert 1981:339). I have shown that when two sites are aged by the same techniques, the age distributions are almost identical despite the underlying differences in the actual age-at-death distributions (Jackes 1985, 1992*a*). The second argument propounded by Van Gerven and Armelagos is that since age-dependent differences can be seen in age-dependent characteristics, age-assessment error must be minimal. In fact we have here an unavoidable circularity. Skeletal biologists seriate skeletons using age-dependent characteristics to estimate age. Those same characteristics are then analyzed for age-dependency, and when concomitant changes in other age-related characteristics such as cortical width and trabecular density are recognized, age assessments seem to be validated. In fact, the general interrelationship of age-dependent changes makes it inevitable that trends will be observed once skeletons are seriated on one or more age-dependent characters. However, since the correlations among age-dependent characters are relatively low (Jackes 1992*a*), the recognition of trends is a very different matter from accurate age assessment.

Contra Wood et al., there is as yet no clear understanding of bias and differential preservation. Though it is fairly certain that there is a bias against the preservation of older individuals and that the incidences of age-dependent characters are altered by this (Jackes 1992*a*, *b*), we are barely beginning to understand the agents of diagenesis and their activities (Baud 1987, Jackes 1990, Jackes, Barker, and Wayman 1992, Jackes and Sherburne n.d.). Sample bias as a whole, preservational or cultural, merits closer attention before broad interpretations of, for example, Sudanese Nubian biological history are made (Jackes 1992*a*:216; Crubezy 1993:76).

Wood et al. are overly optimistic in saying (p. 344) that we have moved to "rigorous analytical investigations of demographic processes." On the contrary, close examination of the techniques of age assessment and the assumptions underlying research on age-dependent factors might lead us to despair. But the sceptical osteologist may be able to identify the problems, thus allowing a start to be made on finding methods to circumvent them (Jackes 1992*a*, *b*). Despair should arise only if osteologists are not intellectually honest and able to withstand a reappraisal of their methods.

Wood et al.'s paper is an example of an increasing scepticism, a healthy successor to the many studies by Masset and Bocquet-Appel on questions of preservation, age, sex, and bias. It highlights an approach that is commonsense: osteological assumptions can best be tested by examination of periods of great change in human history such as the transition to agriculture and the arrival of Europeans in the Americas (Jackes 1985, 1986, 1988*a*, *b*; Lubell and Jackes 1988; Lubell et al. 1993). Of course, periods of rapid population decline present problems for demographic analysis, as do those of population increase, and part of the fascination of palaeodemography lies in trying to answer basic questions such as whether population increase preceded or succeeded the introduction of agriculture. The apparent high mortality in the Late Woodland populations from Ledders, Schild, and Libben may confirm Cook's suggestion that mortality was higher during the Late Woodland transition to agriculture than during the Middle Woodland (summarized in Droessler 1981:15). Alternatively, these samples may provide us with evidence of population increase (Jackes 1992*a*). It is commonly considered, however, often on the basis of research on Dickson Mounds skeletons (e.g., Cohen 1989:121), that higher mortality (or population increase) followed the introduction of agriculture. Our work on the transition to agriculture in Western Europe (see references in Lubell et al. 1993) accords with this scenario of population increase and accompanying high subadult mortality, but we cannot uncritically embrace all supporting data. Wood et al. are sceptical that the Dickson Mounds data accurately reflect skeletal changes at the transition to maize agriculture in North America. I agree.

The basis of all the papers on Dickson Mounds osteology is the attribution of skeletons to three periods and the age-at-death assessments deriving from Lallo (1973). Lallo (1973) and Lallo, Rose, and Armelagos (1980) designate 114 skeletons as Late Woodland (LW), 224 skeletons as Mississippian Acculturated Late Woodland (MALW), and 219 skeletons as Middle Mississippian (MM). For the first two periods the mean ages of death are published as 25.8 and 25.7, while the MM mean age of death is published as 18.4. The Kolmogorov-Smirnov test is utilized (although it is not quite appropriate for large unequal samples) to show that the LW and MALW samples are not significantly different. Arithmetical errors and errors in the life-table calculation (specifically of L_x) deriving from the methods used in Swedlund and Armelagos (1969) result in published figures that are misleading. For example, the correct figures for mean age at death or life expectancy at birth are around 27 years for the LW, 21.4 for the MALW, and 20.4 for the MM, not 26, 26, and 18, respectively. Correct calculation of the Kolmogorov-Smirnov test (Siegel 1956:249) shows that the only significant difference is between the LW and the MM distribution, with the greatest alteration in mortality occurring at the end of the LW. Log likelihood ratio tests of the age-at-death distributions (with children under 5 excluded) do not support the existence of significant differences among the three samples.

Nevertheless, the Dickson Mounds skeletal material was subsequently published as two samples, LW + MALW and MM (Goodman et al. 1984, Goodman and Armelagos 1989). The LW + MALW sample here has increased from 338 to 351. The age-categories have been altered, and the age distribution of the dead has been reworked so that the mean age of death is 26 for the earlier (combined) sample and 18.5 for the MM sample. The chances are high that the original figures for the MM age-at-death distribution made more sense biologically (cf. points 4 and 8 of fig. 1).

Figure 1 illustrates a technique for comparing samples and assessing bias in age-at-death distributions (discussed in Jackes 1986, 1988*a*, 1992*a*), based on detailed examination of model, historical, and archaeological data. It excludes consideration of children under 5 years of age because of cultural or preservational bias against young children in cemetery populations. The values plotted are highly correlated with all demographic estimators; the juvenile:adult ratio is the best predictor of general fertility in stationary populations, while mean childhood mortality (based on q values) provides a correlation of .9987 with general fertility and is the best predictor in increasing populations (Jackes 1992*a*). The regression line in figure 1 derives from age-at-death distributions of 52 archaeological samples that are (relatively speaking) large, complete, and unbiased. Sample sizes for Dickson Mounds age-categories have been redistributed equally within two age-categories for these analyses. Lallo (1973) uses the age-category 15–25, Goodman et al. (1984) the age-category 20–30. Such age data must be brought into line with the standard 5-year age-categories, and equal distribution provides the middle values of the possible range of figures here.

Figure 1 indicates that the Lallo 1973 LW and MM samples (points 2 and 4) may provide reasonable demographic data but do not differ markedly from each other. The reworked (1984) MM data (point 8) fall far from the line. The reworked data from the other periods cannot be plotted separately; the combined LW and MALW fall at point 1 (in its original form the pooled sample would fall at point 6). The original (1973) MALW age-at-death distribution (point 7), while similar to the other samples (points 2 and 4), is perhaps far enough off the line to raise questions as to whether it is a true exemplar of mortality for that period.

What, in fact, is the "true" mortality (or fertility)? The other points on figure 1 are based on the research of a second osteologist, Blakely, who, like Lallo, completed a Ph.D. in 1973 on Dickson Mounds material. Lallo was especially concerned with correct age estimation. This was less important to Blakely, although he did discuss his techniques very fully (Blakely 1973:67–69). Blakely (1971) was well aware of the pitfalls of age assessment and had done detailed reassessments of the Indian Knoll material. His assessment of subadult age for Indian Knoll was quite comparable with those of Howells (1960) and Johnston and Snow (1961). It is noteworthy that the major differences between Lallo and Blakely in terms of age assessment span the years from 10 to 25. These years should not present intractable problems, but by age 20 there is a difference of some 8% in the cumulative percentages—a difference that is only exacerbated if Lallo's LW sample is removed from consideration. The modified ages published in Goodman et al. (1984) make Lallo's subadult ages fit Blakely's better, the major difference in cumulative percentages (still at age 20) being reduced to 4.8%. The curves from age 35 on have become identical. Blakely used a wider variety of techniques for subadult age assessment (diaphyseal length, for example) and an additional reference on dental calcification and eruption (cf. Blakely 1973:67 and Lallo 1973:35). Hoppa and Saunders (1992) have recently shown that the reference samples underlying some standards for estimating subadult dental age skew the results and advocate both adjustment based on long-bone diaphyseal lengths to increase age-assessment accuracy and increased scepticism regarding juvenile growth curves based on archaeological material.

The effect of the differences between Lallo's and Blakely's age assessments can be seen by comparing points 5 and B. Point B represents the 479 skeletons published by Blakely in 1971 as at least 80% Middle Mississippian (the total Mississippian A + B sample from Blakely 1973 provides almost identical values and overlaps point B). The 557 skeletons aged by Lallo (1973) fall at point 5. Whereas Lallo's analysis would lead to the conclusion that childhood mortality and population fertility increased very slightly with the introduction of agriculture, Blakely's evidence indicates a marked decline in mortality or fertility. Indeed, Blakely's data would suggest that by A.D. 1200–1300 the population was stationary (point A).

Lallo assigns all the material from each burial mound to a single period (e.g., Mounds A, B, C, D, E, and H to the "Mississipian Acculturated Late Woodland," dated A.D. 1150–1250). Blakely, after detailed examination of associated grave goods, subdivides the material differently and accepts the cultural association of fewer individuals—94 only from the Late Woodland and 167 from the Mississippian. He divides the Mississippian into Mississippian B (Eveland phase, A.D. 1000–1200; n = 81) and Mississippian A (Larson and Dickson phases, A.D. 1200–1300; n = 86). He says that both Late Woodland and Eveland-phase specimens are found in Mounds A, B, D, and E, while both Late Woodland and Mississippian A specimens are found in Mound G. Lallo (1973:29)

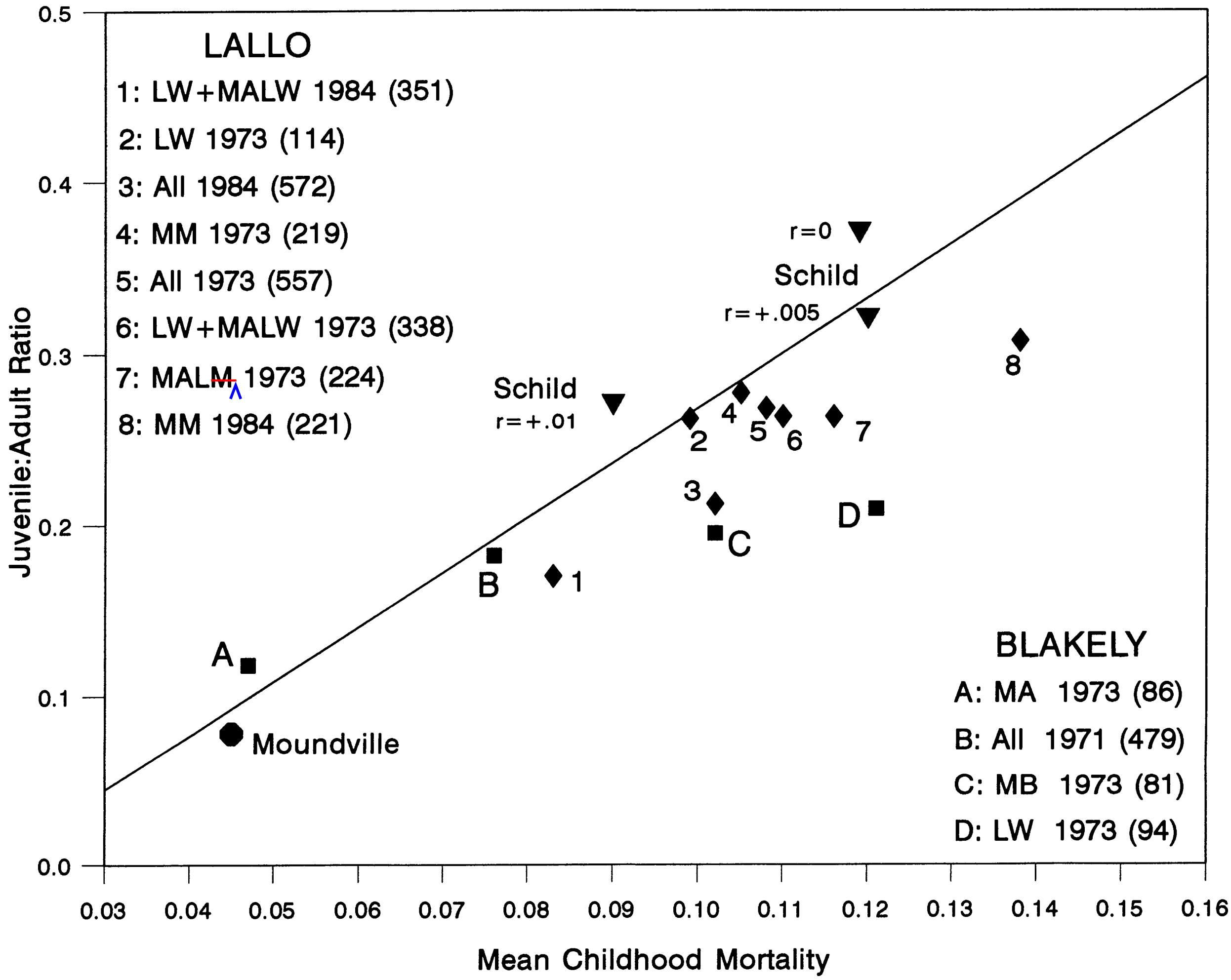

FIG. 1. *Regression of mean childhood mortality (MCM, the mean of* $_5q_5$ *to* $_5q_{15}$*) on the juvenile: adult ratio (5–14.99 yrs./20+ yrs.) for 52 archaeological samples* (y = −.04 + 3.12 × MCM, r = .97). *Age-at-death distributions are compared for samples from Dickson Mounds, a Late Woodland to Middle Mississippian site in Illinois, as formulated by Blakely* (squares) *and Lallo* (diamonds), *and for two other Middle Mississippian sites, Moundville and Schild (with adjustments for* r, *rate of population increase).*

would put all of the Mound G material into the Late Woodland.

On the basis of Blakely's attribution of skeletons to period at Dickson Mounds, it appears that childhood mortality decreased as maize agriculture became established (from point D to C to A). However, Blakely (1973:170) provides no detailed information for use in palaeodemographic studies, noting only general trends (Blakely [personal communication, August 8, 1984] has provided basic figures from his thesis research). His data have never been used in the discussions of the transition to agriculture, as is fitting: he studied 479 skeletons from an estimated total of 3,000 burials. A sample composed of a mere 16% would provide no assurance whatsoever that it was representative or random with regard to age, sex, developmental norms, or health status, especially because fewer than 9% of the possible 3,000 total could be confidently ascribed to a period. Figure 1 demonstrates that the samples are probably not biologically meaningful, for Blakely's earlier samples fall far from the line (point D is the pre-A.D. 1000 sample; point C is dated at A.D. 1000–1200) compared with his sample dated at A.D. 1200–1300 (point A). Lallo used a sample of 557 individuals from the total of 595 excavated in 1966 and 1967 (or 572 based on the modified life-table figures of Goodman et al. 1984). Yet this sample of less than 20% of the estimated total burials has been accepted as definitive of the transition to agriculture in numerous studies (e.g., Moore, Swedlund, and Armelagos 1975, Lallo et al. 1980, Johansson and Horowitz 1986, Cohen 1989).

It is clearly the right time for Wood et al. to suggest that we look to some site other than Dickson Mounds for answers to such basic questions as "Did the introduction of maize agriculture result *from* or result *in* an increase in population?" Unfortunately, the information available does not even permit us to characterize Middle Mississippian mortality or fertility. The Schild Middle Mississippian data (Goldstein 1980, Droessler 1981) suggest such high childhood mortality that it is justifiable to consider adjustments for a nonstationary population. In figure 1 the triangles labeled "Schild $r = 0$," "Schild $r = +.005$," and "Schild $r = +.01$" record the original position and the results of the application of two adjustments (see Jackes 1986 for details), suggesting that a moderate population increase brings Schild into line with other archaeological data. The position of the Schild age-at-death distribution suggests high mortality for the Middle Mississippian and population increase of just below .005. On the other hand, the Moundville age-at-death distribution (Powell 1988) provides evidence of archaeologically low childhood mortality, indicating that the health status and population stability achieved by at least some Middle Mississippian groups was very like that of the precontact agricultural populations of Ontario (Jackes 1986).

We must be grateful to Wood et al. for initiating a long-overdue discussion of preconceptions. It is obvious that we need to clear away common beliefs based on inadequate evidence before osteology can make any advances.

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On Subsistence and Ethnicity in Precolonial South Africa

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II V 93

In his review (CA 34:108) of Barnard's *The Kalahari Debate,* Kuper recognises the importance of Elphick's seminal work *Kraal and Castle (*1977), reprinted in 1985 with minor changes to the first chapter. Elphick's interpretation of the historical record is that the distinction between hunters ("soaqua") and herders (Khoikhoi) was one of fortune at the time of observation. When a herding family lost its herds through theft, disease, or drought it had to rely on hunting for its livelihood. What Kuper does not indicate is how Elphick's thesis has been used by revisionists of South African history to deconstruct the orthodox vision of separate San ("soaqua," Bushmen) and Khoikhoi (Hottentot) societies in the precolonial past (Schrire 1980, Schrire and Deacon 1989). Schrire (1987, 1990) chose the Dutch redoubt of Oudepost I for excavation because there were records of Khoi/Dutch interaction which she wanted to identify archaeologically. She did indeed find indigenous artifacts (Schrire and Deacon 1984) which were described as no different from those found on Later Stone Age sites of hunters in the Cape. Since it was the "Hottentots" who were described as being the indigenous contacts with the Dutch, this indicated that the material culture of hunters was indistinguishable from that of herders, thus reinforcing Elphick's cyclical model.

In contrast, work by Smith et al. (1991) and Sadr and Smith (1991) has identified two quite different archaeological signatures in coeval precolonial sites at the Cape within the past 2,000 years, including sites within 3.5 km of Oudepost I. Since one of these has high proportions of sheep, pottery, and large ostrich-eggshell beads but very few formally retouched stone tools, we conclude that the people who occupied the site were herders. The other signature, with a low incidence of sheep (but many small bovids) and pottery, many small ostrich-eggshell beads, and a high percentage of formally retouched lithics, we link with hunters. Elphick's thesis is based on historical documents, and there is every reason to accept that disruption of the aboriginal people of the Cape by colonial society in the 17th and 18th centuries meant that herders would have joined hunting groups as refugees. Our interpretation of the archaeological record is that distinct social and economic groups existed side by side in the precolonial period and may well have continued to do so up to the end of the 17th century.

In response to our critique (Smith et al. 1991:89–90), Schrire infers that we are trapped in a Verwoerdian paradigm of cultural separation (1992*a*:64) and that we "might be guided more by ideology than any other frame or reference" (1992*b*:132). We in turn (Yates and Smith n.d.), while recognising the ideological orientation of any researcher, would suggest that there is no reason that ethnic separation should not have continued up to the colonial period, since, in spite of Verwoerd's fears, whereas individuals may cross an ethnic and economic boundary it is unusual for whole groups to do so. Equally, herders would have been reluctant to have competition for pastoral resources in an area of low nutrient status (Smith 1984).

Thus there are wider issues of ethnicity not considered by Elphick: how hunters become food producers by overcoming economic restrictions (creating viable breeding herds while being denied access to the means of pastoral production), ideological restrictions (food sharing and immediate use of product compared with nurturing a breeding herd), social restrictions (exclusion from entry into food-producing societies), or potential marginalisation as low-class members of dominant societies (Smith 1990).

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