Was there a Neolithic Mortality Crisis?

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Abstract

The case that mortality rose either with the Neolithic Revolution or subsequent urbanization is made by both medical ecologists and anthropologists. The former point out that only dense farming or urban populations could sustain epidemic disease. Many anthropologists believe that Palaeolithic society either controlled population numbers or experienced low natural fertility, and that both fertility and mortality rose with denser, sedentary populations. Given that the evidence for low hunter-gatherer fertility is unsatisfactory, and that the balance of fertility and mortality was inevitably approximately maintained into the Neolithic period, it is possible that there was no Neolithic mortality crisis. This paper examines how the case was built for near consensus on such a mortality crisis, and the implications of this case being wrong.

The proposition that the beginning of agriculture (or irrigation or urbanization) was associated with a significant rise in mortality has been widely discussed and mostly supported over the last half-century. The case is found in the medical or epidemiological literature, especially that in the ecological tradition, and in works on anthropology and palaeodemography. This paper examines how these ideas developed and questions whether this near consensus could prove to be fallible.

The most certain demographic postulate for prehistoric populations is that over long periods, averaging out sudden spurts of population growth or decline and their reversal, the increase in human numbers was close to zero. Anything greater would have resulted in vastly higher human populations than now exist. Coale (1974:43) estimated population growth for the million-year period of hunting and gathering to have averaged 0.0015 per cent per annum, and for the first eight millennia of agriculture (8000 BC - AD 1) to have been 0.036 per cent or 24 times as fast. This calculation excited many in the debate as being proof that fertility had risen substantially compared with mortality, and that family building after the Neolithic Revolution must be seen in quite a different context. Actually, it implies either a drop in the crude death rate of one-third of a point per thousand (one-sixth of a year in life expectancy) or a rise in the birth rate of the same amount (one extra birth annually to one in every 150 mothers): demographic movements imperceptible to the populations concerned and most unlikely to be planned. The most common hypothesis put forward in anthropological writings cited in this paper was that both fertility and mortality had increased significantly with the advent of agriculture, with only a slight margin between the two rises. This was a necessary conclusion to maintain the widely held view that hunters and gatherers had consciously controlled population numbers and that this control was relaxed or abandoned when they became settled farmers. The complementary argument was that the more crowded post-Neolithic world must inevitably be characterized by higher mortality, because it sustained epidemic diseases

and other ailments that could not exist among more sparsely settled and mobile hunter-gatherers, and hence fertility must also have risen to prevent extinction. There has been a substantial interaction in developing these ideas between epidemiologists and anthropologists, with palaeodemographers contributing evidence used increasingly by those involved. The most extreme views were probably those of Dumond (1975:713) whose article in *Science* began: 'The demographic transition of modern times is a return to a pattern familiar to our hunting ancestors', and Black (1980:52): 'it seems as if modern advances can do no more than return us to the state of health that mankind enjoyed 10,000 years ago'.

Wood (1998:100) claimed that social scientists had abandoned the idea of Palaeolithic population control and the necessitated relatively low mortality, because of the onslaught on this position by theoretical population biologists, Maynard Smith (1976) and Wade (1978), both in the *Quarterly Review of Biology*, and Uyenoyama and Feldman (1980) in *Theoretical Population Biology*. In reality very few field social scientists have ever cited these sources and most had probably never heard of them before Wood's (1994) *Dynamics of Human Reproduction* or more likely his 1998 paper in *Current Anthropology*. Judging by current textbooks, neither the majority of epidemiologists nor most anthropologists have changed their stance. The decline in research articles on the subject by anthropologists can probably be explained by a falling off in interest in contemporary fertility control as global birth rates declined. The debate on the related topic of the 'Stone Age affluent society' (Sahlins's usage) had also run its course. The authors of the present paper were drawn to the topic by two experiences. The first was the examination of the works of the early explorers and anthropologists used as sources for Sahlins's (1972) *Stone Age Economics* where we concluded that a full assessment of their accounts would not support the thesis of low Palaeolithic mortality (Caldwell, Caldwell and Caldwell 1987:31–33). The second experience was a study of the evidence for the belief in fertility control among hunter-gatherers which we felt had not been proved (Caldwell and Caldwell 2003). These conclusions then led to the problem: if the fertility of both hunter-gatherers and early farmers was uncontrolled and equal to their mortality, was it possible, unless 'natural fertility' was lower among the hunter-gatherers, that the formidable, multidisciplinary case put forward for a rise in human mortality at the Neolithic transition was flawed? Accordingly, this paper examines how that thesis was constructed in the second half of the twentieth century and its possible weaknesses. But, first the forerunners will be noted.

The favourite whipping boys of the affluent-society adherents were those writers who had followed the seventeenth-century British savant, Thomas Hobbes, in describing the lives of hunter-gatherers as brutish and short. They included Herbert Spencer (1899, vol. I:15) who said the hunter-gatherers' condition always bordered on starvation and life was accordingly brief, and one of the fathers of modern anthropology and the coiner of the term 'Neolithic Revolution', Gordon Childe (1936:74–117) who, in his revealingly titled book, *Man Makes Himself*, maintained that the Neolithic Revolution was a forward step both generally but also with regard to diet and health. There were now readily available stored grain and root crops which were supplemented by meat, eggs and milk from the newly domesticated animals and also meat from hunting which continued alongside farming for millennia.

Others viewed this picture as simplistic, driven by a philosophy of continuing progress. Already in 1798, Malthus (1960) in his *First Essay* had noted that, although the lives of early people had been characterized by war and violent diseases (p.41), urbanization was no panacea, because 'large towns are known to be unfavourable to health, particularly the health of young children' (p.43). The biologist Carr-Saunders (1922:157) drew on both Davidson's (1892) *Geographic Pathology,* and the experience of Amerindians and the peoples of the Pacific when first infected with European diseases, to argue that non-urbanized peoples had been free of smallpox, scarlet fever, measles, cholera, yellow fever, whooping cough, diphtheria, syphilis and leprosy, most of the ailments leading to premature death. Zinsser's (1935) *Rats, Lice and History*, with its emphasis on social and time dimensions, inspired many to take a more ecological view of disease causation.

The first research advances in the field, and still the least challenged, were those made by the medical and epidemiological ecologists. Their work was encouraged by research in the 1920s and 1930s on the minimum populations of cities or islands needed to sustain epidemic diseases without their periodic disappearance. Most work had been done on measles, a disease which confers a high level of immunity, has no lasting sequelae which can prove infectious (such as shingles persisting long after chickenpox infection), and is specific to human beings. The first work was done by Panum on the Faeroe Islands in 1846, followed by Soper (1929), culminating later in such research as that of Bartlett (1957, 1960) which determined that in Britain, the United States and Canada cities had to have populations of at least 250-300 thousand for measles not to disappear after epidemics. Black's (1966) work suggested the figure could be as high as half a million on Pacific islands. Here we shall deal with epidemiologists and social scientists alternately even though after 1960 they interacted in that each group cited the other to support its case.

The epidemiological case

MacFarlane Burnet, who championed the ecological approach (which subsequently gained him a Nobel Prize), published in 1940 *Biological Aspects of Infectious Disease*, in which he indicted cities for their relatively high levels of mortality, caused by filth and crowding, and stated: 'The nomadic life is the healthy life, and the children of nomads [with whom he apparently included hunter-gatherers] survived until their numbers were too great for the steppes to support them' (1953:15). Bates (1955) cited Zinsser (1935) as an inspiration, and, drawing on reports of the devastation caused to Amerindian populations by smallpox and Pacific Islanders by measles and mumps, was led to 'wonder whether these contagious diseases may not largely be a post-Neolithic development' (Bates 1955:161). He also argued that famine decimated agricultural people in a way that was unknown to hunter-gatherers. These views were echoed by Dubos (1959) in his *Mirage of Health: Utopias, Progress and Biological Change*. Not everyone at this stage believed that mortality and fertility had risen with the coming of agriculture and urbanization. Bates suspected that epidemic-disease mortality had replaced that which had previously been caused by 'anthropoktony', which included murder, homicide, suicide and war (Bates 1955:124). J.B.S. Haldane (1956/7:328–329) argued that the epidemics replaced 'vertebrate predation'.

By the 1960s the debate about the impact of the Neolithic Revolution was centring on two matters. The first was what diseases could infect only settled populations and not

hunter-gatherers. Indeed, it sometimes seemed that so little could attack the latter that they might be almost immortal. The second was whether the greatest danger had been imposed by sedentarism with its pollution of water sources and build-up of human faeces, irrigation with its spread of water-borne disease, or urbanization with cities appearing and passing in population size various thresholds (such as 250–300,000 for measles). These issues have often been treated in a fairly slipshod way with a tendency to blur together the changes even though five thousand years (8000–3000 BC) separated the first agricultural villages from the first cities. Armelagos and Dewey (1970) were an exception.

Aidan Cockburn (1963, 1967a, b, 1971) identified the Neolithic Revolution as being identified with sedentarism, with faecal contamination multiplying hookworms and ascarids; with niches for rats, mice, ticks, fleas and mosquitoes; with the domestication of animals providing homes for ticks and lice, and the threat of cross-species leaps in infection; with dense populations that supported rubella, cholera, smallpox, mumps, measles and chickenpox (the last probably wrongly); and with irrigation to spread disease and its vectors. But he conceded that with agriculture, 'for the first time, not only was there food for all, but it was the kind that could be stored for periods of shortages' (Cockburn 1967b:86). Fenner (1970:51–56; 1980:14–15) added salmonellosis, yellow fever, typhoid and poliomyelitis, and argued, following Carl Sauer's 1952 thesis on the origins of agriculture that Malaysian fishing villages with their garden plots of yams, taro, bananas and coconuts constituted the first farming regime, that such settlements probably made mosquito-borne malaria a scourge even before crop growing began in the ancient Middle East. Black's (1975:518) list of urban diseases included influenza, colds and poliomyelitis. McNeill (1976), in his widely read *Plagues and Peoples*, emphasized the Neolithic species transfer of disease as probably having occurred from poultry, rats, mice, horses, pigs, sheep, goats, cattle and dogs, with measles (related to rinderpest and distemper) and smallpox (related to cowpox) coming from cattle, and influenza from pigs.

But what could infect hunter-gatherers? Pavlovsky (1966), in research first published in Russian in 1939, focused on *zoonoses* (infections transmitted to humans by wild animals or their parasites) and answered rabies, tuberculosis, brucellosis, plague, tularaemia, leptospirosis, Chaga's disease, yellow fever and encephalitis. Armelagos (1967:79) suggested that hunter-gatherers had probably domesticated the dog, which, together with its parasites, would have been a source of disease. Fenner (1980:14–15) listed as possible hunter-gatherer infections those from animal reservoirs such as rickettsiosis, rabies and salmonellosis, such latent or chronic infections as chickenpox, herpes, tuberculosis, leprosy and treponematosis, as well as staphylococcal and streptococcal infections. But he ruled out the following diseases that awaited farming and urbanization: human viral infections such as influenza, colds, pneumonia, enteric diseases, measles, smallpox and rubella; and human bacterial infections like shigellosis, cholera and typhoid.

Mark Nathan Cohen who had earlier addressed the origins of agriculture (Cohen 1977) and, with Armelagos, the changing Neolithic environment (Cohen and Armelagos 1984b), became from the late 1980s a major contributor to the health debate (Cohen 1989, 1994). In *Health and the Rise of Civilization* (Cohen 1989:160fn1) he acknowledged as his predecessors Haldane (1932), Polgar (1964), Fenner (1970), Burnet and White (1972), Cockburn (1967a, 1971) and Black (1975,

1980). Cohen added to the list of diseases that could kill hunter-gatherers through zoonoses haemorrhagic fever, brucellosis and anthrax; through anaerobic bacteria gangrene, botulism and tetanus; and encephalitis from mosquitoes, trypanosomiasis (sleeping sickness) from the tsetse fly and viral diseases from ticks; as well as such chronic complaints as yaws and herpes. He, like others, also based his case for health deterioration at the beginning of the Neolithic period quite heavily on the evidence from palaeodemography which became more plentiful in the 1970s, although papers estimating mortality levels from skeletons appeared as early as 1960 (Vallois 1960; Howells 1960).

Those working with an epidemiological approach first became interested in the evidence from ancient skeletons when in 1970 the Hungarian paleodemographers Acsádi and Nemeskéri published their *History of Human Life Span and Mortality*. Rather courageously they presented 13 life tables constructed from mostly Hungarian skeletal remains with life expectation at birth rising (with the exception of a few aberrant peaks) from around 21 years in the Palaeolithic to 25 years in the early Neolithic, to 28 years in the Roman Empire and 29 years in the early Middle Ages. They had no evidence of a Neolithic mortality crisis. Angel (1975) drew on Acsádi and Nemeskéri (1970) and his own observations of 2,200 skeletons and mummies from the Eastern Mediterranean to argue that mortality had risen at the end of the Palaeolithic as hunter-gatherers multiplied and large mammals declined; that it rose further with the advent of agriculture, then fell, but stabilized after 2000 BC, with health improving as swamp draining defeated malaria, and children became more resistant to urban epidemic disease.

The greatest impact on the debate was achieved by a 1982 conference in Plattsburgh, New York State, of palaeopathologists with preset agenda questions. Although, contrary to the intent of the organizers, there was a heavy concentration on American evidence in a situation where the timing and extent of the Neolithic Revolution was often blurred, the papers were subsequently published as *Paleopathology at the Origins of Agriculture* (Cohen and Armelagos 1984a). In their summary of the conference, the editors argued that the skeletal evidence showed farmers to have had higher levels of infection and a lower mean age at death than hunter-gatherers. In her summary, Roosevelt (1984) agreed, and added that, although Palaeolithic skeletons bore evidence of seasonal and periodic malnutrition, they did not show the signs of infectious disease (especially in infancy), the teeth destroyed by a carbohydrate diet, and the stunting found in Neolithic skeletons. The case seemed to be closed.

The anthropological case

Anthropologists discovered the advantages of the hunter-gatherer way of life only in the mid-1960s, influenced by Carr-Saunders (1922), the growing epidemiological and palaeodemographic literature, Boserup (1965) and the Harvard study of Dobe !Kung of Botswana in 1963–5 (see Lee 1968; Howell 1979). Cohen (1997: 243) said that all this work led him to a new perspective, namely that technological advances, and especially the Neolithic Revolution, did not necessarily lead to improved 'nutrition, health, well-being and leisure time', although he subsequently was convinced by sceptics that the !Kung were unlikely to reflect the situation in any ancient foraging society (ibid: 249–250). Boserup did not actually discuss relative mortality levels, but, in arguing that it was population pressure that forced mankind to find more intensive means for food production, she proposed that shifts from hunter-gathering to shifting

cultivation and on to sedentary cultivation had at each step meant less leisure and more hours of work. The further back in the chain of change, the greater the leisure and, in this sense, 'affluence'. A year earlier, Polgar (1964), in a revealingly entitled paper, 'Evolution and the ills of mankind', brought the health disadvantages of sedentary agriculture into the social science field by drawing on most contributions of the health ecologists up to that date (Zinsser 1935; Burnet 1940; Bates 1955; Haldane 1956; Dubos 1959; Cockburn 1963). William Allan (1965), in *The African Husbandman*, made more widely available ideas he had published first in 1949, arguing that there was for every food system a critical population density beyond which food production and, by implication, health suffered. Gluckman (1965:vii), in his introduction to Allan (1965), said that 'his theory and applications were a revelation to my social anthropological colleagues as they had been to me'.

The study of the !Kung Bushmen of Botswana, especially the Dobe !Kung of Northwest Botswana near the Namibian border, was to become a major driving force in picturing the golden age of 'Palaeolithic man'. The !Kung and most, if not all, other hunter-gatherers surviving in the world were by the 1960s by no means isolated from external influences. As early as 1961 Marshall had pictured their happy community with its meat-sharing and gift-giving, claiming that 'if there is hunger it is commonly shared' (Marshall 1961:236). The work on the !Kung provoked a much wider conference in 1966 on *Man the Hunter* and resulted in an influential book with that title (Lee and DeVore 1968). The !Kung research later also yielded a book of central importance to the debate on the hunter-gatherer mortality-fertility balance, Howell's (1979) *Demography of the Dobe !Kung.* Lee and DeVore, in the introduction to *Man the Hunter*, refer to the importance of chapters by Lee (1968) and Birdsell (1968), as well as a discussion note by Sahlins (1968a). Lee stressed leisure (but only apparently of men), with only one-third of their working hours spent in hunting, one-third in entertaining visitors from other camps, and one-third visiting other camps to be entertained, with work spread out evenly throughout the year (Lee 1968:31–34). He stressed their low mortality by estimating that 10 per cent were over 60 years of age, 'a proportion that compares favourably to industrialized countries' (p.35). Leaving to one side the question of age accuracy, 10 per cent of the society being over 60 years is likely to characterize a stationary population with a life expectancy of around 35 years, or one growing at the slow rate determined by Howell (1979:215) of 38 years (Coale and Demeny 1966, West Model). Howell's (1979:98) own estimate of Dobe !Kung life expectancy at birth was 50 years, a figure which she appreciated was partly the product of international health measures. Admittedly in the 1960s the proportion over 60 years of age in industrialized countries was only about 13 per cent, the product of annual population growth rates averaging over one per cent in the previous two decades (United Nations 2001). Birdsell's (1968) chapter on Australian Aborigines is too mired in modelling for us to be able to discern through it the real situation of hunter-gatherers or their likely mortality levels.

The 1966 conference, as recorded in Lee and DeVore (1968), saw the emergence of probably the most influential proponent of the fortunate, and, by implication, healthy and long-lived, hunter-gatherer, Marshall Sahlins. At the conference he said that it was so difficult to correct the traditional view that hunter-gatherers lived a miserable life, that 'perhaps, then, we should phrase the necessary revisions in the most shocking terms possible… the original affluent society' (Sahlins 1968a:85). He emphasized their leisure; their sufficiency in food, quoting Marshall (1961:243) on

the !Kung having 'a kind of material plenty'; and their peace of mind, citing Spencer and Gillen (1899:53) describing the Australian Aborigines as having 'not the slightest thought of, or care for, what the morrow may bring forth'. Sahlins expanded these views in late 1968 in an article published in French, 'La première société d'abondance' (Sahlins 1968b) and further in 1972 in *Stone Age Economics* where in the first chapter, 'The original affluent society', he concluded: 'The world's most primitive people have few possessions, *but they are not poor*' (Sahlins 1972:37, his italics). Although he disputed the notion that hunter-gatherers were frequently ill or starving, he said little about their levels of mortality or demographic equilibrium and nothing about causes of death.

The foregoing near-consensus was built on by the major contributor to the debate, Cohen (1977, 1984, 1989, 1994; Cohen and Armelagos 1984a,b). In *Food Crisis in Prehistory* (1977), he argued, citing Boserup (1965) as his inspiration, that for most of history hunter-gatherers had been well fed and healthy because they had a wide variety of food, both plant and animal, and because their plant food did not disappear during drought, being local varieties selected by evolution to survive in such conditions (Cohen 1977:29–30). Although they controlled fertility (pp.42–47), ultimately they filled the planet, pressing on resources with mortality probably rising, and thus necessitating in a Boserupian sense the move to agriculture which occurred over a wide area at much the same time. But agriculture was no solution to rising mortality because diets became less varied and agricultural people relaxed their control of fertility (pp.279ff.). Cohen (1984), in his introduction to *Paleopathology at the Origins of Agriculture*, deriving from the 1982 Plattsburgh conference, reported that the anthropologists were building their ideas of healthy hunter-gatherers and unhealthy farmers on Boserup (1965), Woodburn (1968), Binford (1968), Flannery (1969), Meyers (1971), Sahlins (1972), Cohen (1977) and Hassan (1981), while epidemiological arguments rested on Polgar (1964) and Fenner (1970), with contributions on nutrition from Barnicot (1969) and Yudkin (1969). Cohen admitted that skeletons of hunter-gatherers are rare and even some so identified may come from small sedentary or quasi-sedentary populations (Cohen 1984:7). In 1989 Cohen published his major work, *Health and the Rise of Civilization,* in which he placed great emphasis on the dangerous health environment after the Neolithic Revolution, citing Haldane (1932), Polgar (1964), Fenner (1970), Burnet and White (1972), Cockburn (1967a, 1971) and Black (1975, 1980). He argued that sedentarism had certain health benefits: the ability to become resistant to local parasites, to look after the sick in permanent buildings, and to get milk from domesticated animals. Nevertheless, these advantages were outweighed by denser settlement inviting epidemic disease, the growth of commerce with its spread of infection, tuberculosis from milk, cross-species infection from domesticated animals, the build-up of refuse and faeces, while permanent housing provided a habitat for vermin transmitting disease (Cohen 1989:38ff.). Furthermore, in spite of the growing of some legumes and continued hunting, people's resistance was undermined by more work and a more limited diet (pp.38ff., 55ff.). In 1994, while admitting that skeletal data were far from satisfactory (Cohen 1994:629–630) he maintained that the 1982 Plattsburgh conference had established from skeletons and mummies that (1) chronic infection was greater among Neolithic farmers, (2) they suffered more from yaws and tuberculosis, (3) they had more intestinal infections and parasites, (4) they experienced more anaemia and malnutrition, (5) they were more often stunted, and (6) dental evidence suggested they were under greater stress.

Another major contributor was Marvin Harris. His most controversial paper, and one with seemingly few followers, was 'Population, warfare and the male supremacy complex', written with William Divale (Divale and Harris 1976). It uses data from Murdock (1967) to argue that 'band and village' societies achieved low population pressures (and by implication low mortality) by emphasizing male supremacy. This male supremacy was expressed chiefly through warfare which was most easily achieved in patrilineal, patrilocal and polygynous societies, especially those with the institution of men's houses. More surprisingly, the warfare limited population not by the deaths arising directly from it, but from the resulting male *machismo* and suppression of women that made the latter unprotestingly kill many of their daughters. Two years later, Harris in *Cannibals and Kings* joined the main cultural anthropological current with its focus on the relatively low mortality of hunter-gatherers, arguing that their epidemic deaths must have been not only fewer than those of Neolithic people but also 'considerably less significant …. than they are today' (Harris 1978:13–14). He postulated that the better diet and 'general body vigour' of hunter-gatherers gave them a greater chance of recovery from illness, and that this advantage was reinforced by their living in dry areas away from the swamps, streams or clearings in wet forests needed for agriculture. In due course Neolithic man was to face the even greater danger of living in 'state-level societies', where the crowding experienced in urban life, especially among the poor in the insanitary slums, was the first theatre for the great epidemics. He drew on Angel (1975) for evidence that anthropometric measurements proved the greater healthiness of Palaeolithic man.

Hayden (1972) agreed on male supremacy, and argued from sources on Australian Aboriginals that Palaeolithic men were the only ones to enjoy the Stone Age affluent society and that women had a hard-working, debilitating life. Nevertheless, he did not maintain that this led to significant levels of infanticide.[1] Howell's (1979) final conclusion about Dobe !Kung women was that their fertility was surprisingly low, explained partly by the long duration of breastfeeding and partly by sexually transmitted disease, but also by malnutrition from eating only a limited amount of badly prepared and unappetizing food and by much movement either to new sites or while gathering food. Those Dobe !Kung women who had settled at cattle posts were fatter and more fertile. Handwerker (1983) put these two sources together to argue that hunter-gatherer women normally experienced low fecundity and generally did not need to practise infanticide. With low fertility being the usual condition, there was no need to explain the balancing low mortality. Citing Caldwell (1982) on the value of children in agricultural societies, he argued that farming families both wanted extra children and needed to have them to keep up with rising child mortality. Neither Hayden nor Handwerker resorted to the solution that seemed to be the way that Hayden was originally going, namely that pre-contact Aboriginal women, or perhaps all hunter-gatherer women, prematurely aged so rapidly that both the women and their partners terminated the women's sexual life, or very active sexual life, at an early age.

The epidemiological and anthropological cases seem almost unchallengeable, especially the former. But, if the argument that Palaeolithic population growth was controlled by infanticide is not proven, and the Dobe !Kung low fertility situation was not universal, then, given wellnigh stationary population with a mortality-fertility balance, there must be flaws. There have been increasing attempts to locate those weaknesses which we will now attempt to probe and supplement.

How good is the case for a Neolithic mortality crisis?

The term 'Neolithic Revolution' has coloured the debate by suggesting a sudden cut-off point, instead of what actually was a long, merged process (Flannery 1973; Hassan 1973:535). In better naturally endowed areas many hunter-gatherers had a kind of base camp, with many of the health dangers of small farming settlements. This is observed in some of the medical literature such as Burnet (1953) and Burnet and White (1972), but with a confusion between hunter-gatherers and nomads, the latter being a post-Neolithic phenomenon with symbiotic relations with agriculturalists. With regard to diet, farming, but not city, people's foods were supplemented by the products of hunting and gathering for millennia, as they still are quite widely in tropical Africa. And each new farming area did not necessarily have to build up new bodily resistances to epidemic diseases, if Renfrew (1987) is correct, because the farmers moved with the farming frontier.

Nor, in spite of the apparent solidity of the ecological argument, was Neolithic health necessarily worse. Acsádi and Nemeskéri's (1970:263ff) unique series of life tables derived from Palaeolithic evidence show rising life expectancies with the advent of farming, as Childe (1936) would have expected. Hassan (1973:535; 1981:211) was not surprised, because he attributed an improvement in child survival and a reduction in the age of menarche to sedentarism, more regular food, and a mixed diet provided by stored food and continuing meat, milk and eggs from domesticated animals. In addition, the pre-existing ease of hunter-gatherer life has probably been wildly overstated (Caldwell *et al*. 1987:31–33).

Fierce debate has clouded the evidence from ancient skeletons. Howells (1960:166ff) and Vallois (1960:204) both emphasized that there was marked age-selection bias in the skeletons found in ancient cemeteries, with infants the most likely to be missing. Indeed, Howells (1960:171) believed that the evidence would allow construction of life tables only from 20 years of age, or at the extreme, 15 years. This presents a problem because such life tables omit about 60 per cent of deaths in high-mortality populations (Coale and Demeny 1966, West model) and probably an even higher proportion of those caused by the new Neolithic epidemics. Cohen (1984:7), in his introduction to *Paleodemography at the Origins of Agriculture* (Cohen and Armelagos 1984a), admitted that skeletons of hunter-gatherers were rare and even many of those identified as such might well instead derive from early small sedentary or quasi-sedentary populations. This means that, even if we agree on high mortality levels of early farming or urbanized populations, we have no satisfactory Palaeolithic evidence to allow a proper comparison.

Attacks were made on the interpretation of skeletal evidence in the two major books with which Cohen was associated (Cohen and Armelagos 1984a; Cohen 1989). Harpending (1990:799–800) argued that stress marks on skeletons, rather than being indicators of the time of death, were more likely to be signs of past successful survival from such a fate. Furthermore, the age distribution of deaths was influenced by the age distribution of the population and, if fertility had risen with the Neolithic Revolution, there would therefore have been more deaths at very early ages (the qualification here with regard to a fertility rise is one which we believe is unproved). Harpending interpreted the skeletal evidence as showing that the Neolithic Revolution had resulted in declining mortality and rising fertility; this is, of course, incompatible with long-term near-equilibrium in population numbers. He was a co-author of a

subsequent major attack on Cohen's and his colleagues' work, now called 'The osteological paradox' (Wood *et al*. 1992), which maintained that there were huge problems in determining states of health from skeletons and that the evidence presented to the 1982 conference (Cohen and Armelagos 1984a) could just as easily be interpreted as showing a Neolithic fall in mortality as a rise. Wood *et al.* (1992) repeated the argument that the bone lesions could perhaps better show survival from illness than be evidence of death, and that the fall in the mean age of young children's death could be evidence of greater survival at weaning because of more satisfactory weaning foods allowing safer weaning and at an earlier time. Wood and Milner (1994:632) argued that skeletons are 'life's failures at any particular age' and can provide no evidence about the health of those continuing to live. Wood (1998) disputed evidence for any provable differences between Palaeolithic and Neolithic fertility or mortality levels and argued that 'All pre-industrial economies … are capable of generating misery and will do so given enough time' (p.121) as they reach demographic saturation. The population is regulated by 'density-dependent vital rates' (p.103), that is, they are in Malthusian equilibrium. Pennington (1996:265) pointed out that only some diseases produce skeletal lesions, and argued that, as with the !Kung, sedentarization probably usually resulted in steep drops in infant and toddler mortality because of a greater volume and more certain supply of breastmilk (apparently because of securer maternal nutrition).

Pennington (1992) had previously argued that there was no great mystery about Dobe !Kung low fertility, citing Coale (1968), Caldwell and Caldwell (1983) and Frank (1983), as showing that sexually transmitted infections (STIs) reduced fertility in some neighbouring sub-Saharan African populations to even lower levels. The adjacent Herero pastoral people had earlier very low fertility, ascribable, as subsequent effective treatment showed, to STIs, and probably had infected the !Kung to a sufficient degree to explain fully their fertility level.

What caused Palaeolithic humans' deaths? Bates (1955:112) had argued the central importance of violent deaths, war, homicide and suicide, although little evidence of the last was reported. Influenced by the contemporary Vietnam War, Fried, Harris and Murphy edited in 1968 a collection on the anthropology of war. In this book Chagnon (1968:112) said of the South American Yanomamö: 'a militant ideology and the warfare it entails function to preserve the sovereignty of independent villages', with the result that 24 per cent of adult males die in warfare (p.140). Such warfare is not related to the capture of territory but to cementing alliances and stealing women. Such a way of life means that violence also spills over into village and family disputes. Coleman (1986:29) was to put the range of prehistoric adult male deaths ascribable to violence at 14–25 per cent, noting that contemporary Papua New Guinea fell into the upper end of this range. Hayden (1972:216) used nineteenth-century sources on Australian Aborigines to argue that the hunter-gatherer way of life promoted aggressiveness and the survival of the fittest. Ember (1978:443–444) drew on studies of 50 societies from Murdock (1967) to argue that warfare characterized up to 90 per cent of these societies. It was the !Kung who were different.

Is it possible that there was no Neolithic mortality surge?

Curiously, the answer to this question centres not on mortality but on fertility. Over the long run, and until very recently, population growth rates were close to zero, except among European overseas settlers over the last half-millennium, and Europe

and parts of Asia over the last two or three centuries. There is no reason to suppose that hunter-gatherer fertility was not high. The evidence of deliberate fertility or population control is very insecure. The explanation for the contemporary Dobe !Kung is probably recent sterility arising from STIs.

All authorities agree that some societies were relatively unaggressive, and there must have been some that were peaceful, not given to practising infanticide, and not subfecund. It is clear that even these societies were ultimately constrained from indefinite growth by Malthusian bounds. Because of their peacefulness they were the least likely to swamp their neighbours. Firth (1936) claimed that this was the idyllic situation of the Tikopia in the Southwest Pacific (except that they probably had in the past practised infanticide). When ultimately faced by excessive population pressure the answer was self-inflicted violence constituted by young men setting off in boats for certain death. The evidence presented by Firth for such suicide was, however, not strong. There is also the problem of how the people felt the increasing population pressure. Was it through increasing hunger and a rising death rate? If so, what were the immediate causes of these deaths? As Malthus recognized, except occasionally in severe famines, most undernourished people die of some other immediate cause. If these causes were not the great post-Neolithic or post-urbanization epidemics, what were they?

The answer is almost certain to be found in the list of ailments and disasters that could attack Palaeolithic populations. Staphylococcal and streptococcal diseases may have been major killers. The way of life may have meant more frequent gangrene and tetanus. We do not have the background to go deeply into this question, but ecological epidemiologists should tackle it even if they are not fully convinced by the thesis of high hunter-gatherer mortality.

For many groups, violence probably played an equal or more important role. Hunting is a violent activity and is made possible only by weapons that are inherently dangerous. Hunting skills are akin to skills needed for warfare. Furthermore, the mobility of hunter-gatherers made warfare easier. Divale and Harris (1976) were almost certainly correct about ‘warfare and the male supremacist complex’. They were on less certain ground, and damaging attacks by Hirschfeld, Howe and Levin (1978) and Lancaster and Lancaster (1978) were possible, only on whether this led to their wives being bullied into infanticide. It can be argued that death from fighting and violence probably characterized the denser populations in naturally well endowed regions while death from starvation and its sequelae were more common among sparsely settled populations in hot or cold deserts or savannas.

There are other aspects of the situation that are less frequently explored. Hunters and warriors depend for their survival on being able to trust the skills and courage of their ‘brothers’ who battle beside them. In such societies men may place greater stress on relationships with other men than on those with spouses and children, who from our point of view tend to be neglected. The cult of violence can easily extend to the family and result in not only men but also women and children being killed. Women were not the major beneficiaries of the Stone-Age affluent society and may have been as burdened as they were in the picture drawn by Hayden (1972) of nineteenth-century Australian Aboriginal society. Given their circumstances and frequent moving they

might well have suffered from high levels of miscarriage, stillbirths, and infant and maternal mortality.

Sedentarization was a long, slow transition, not an overnight occurrence. So probably was the development of epidemic diseases as average settlement size grew and as species-crossover disease occurred. Deaths from violence almost certainly fell and those from emergency long-distance movement of the whole group disappeared. It is likely that fertility and death rates remained little changed and approximately equal. The vision of ancient low-birth and death-rate hunter-gatherer societies rose from the interest of medical ecologists in the impact of the rise of epidemic disease and partly from romantic visions of a Stone-Age affluent society articulated during the 1960s and 1970s when there was great stress on the need for fertility control and the hope that it had a long tradition.

The present need is for better epidemiological models showing how those diseases that probably existed among paeleolithic hunters and gatherers could, in the absence of many post-Neolithic epidemic diseases, have cut down (with the assistance of violence) populations to the point where mortality equalled uncontrolled fertility. That they did do so is certain. It is our modern knowledge of disease, and the limits on its spread imposed by scattered population, assisted by palaeodemographic findings, that should illuminate the way forward.

Note

1 Later, Hayden *et al.* (1986), using Murdock's (1967) and other material, argued that the strongest forces reducing the relative status of women among hunter-gatherers were, in descending order of importance, frequent and severe resource stress (starvation, malnutrition); the level of warfare and violence; and the importance of hunting. They felt that the feminist revolution in the West after 1960 had led anthropologists to see more gender equality in Palaeolithic society than was actually the case (Hayden *et al.* 1986:449).

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