A Theory of Preindustrial Population Dynamics: Demography, Economy, and Well-Being in Malthusian Systems [and Comments and Reply]

James W. Wood; George L. Cowgill; Robert E. Dewar; Nancy Howell; Lyle W. Konigsberg; J. H. Littleton; R. D. Attenborough; Alan C. Swedlund

Current Anthropology, Vol. 39, No. 1. (Feb., 1998), pp. 99-135.

CURRENT ANTHROPOLOGY Volume 39, Number 1, February 1998

A Theory of Preindustrial Population Dynamics

Demography, Economy, and Well-Being in Malthusian Systems[1]

by James W. Wood

This paper presents a simple model of preindustrial population dynamics, one that brings together the theoretical insights of Thomas Robert Malthus and Ester Boserup. Central to the model is the concept of *well-being,* which refers to those aspects of physical condition that influence an individual's capacity to survive and reproduce. Changes in the mean and variance in well-being are modeled, first, under a fixed system of food production and, second, in the face of subsistence change. Among other things, the model suggests that the long-term effects of economic change on the distribution of well-being are negligible, although both the mean and variance are likely to increase temporarily in the short run. The model is used to explore several issues of enduring importance to demographic anthropology, including the nature of population regulation, the relationship between population pressure and economic change, and the demographic consequences of the transition from hunting and gathering to settled agriculture.

JAMES W. WOOD is Professor of Anthropology and Senior Research Scientist in the Population Research Institute at Pennsylvania State University (University Park, Pa. 16802, U.S.A.). Born in 1949, he was educated at Columbia University (B.A., 1971) and the University of Michigan (M.A., 1975; Ph.D., 1980). He has taught at Michigan and the University of Wisconsin and has done fieldwork on demography, reproductive biology, and population genetics in Papua New Guinea. He is currently involved in a major project that is attempting to reconstruct population dynamics in medieval Denmark using both documentary evidence and skeletal samples. His publications include *Dynamics of Human Reproduction: Biology, Biometry, Demography* (Hawthorne, N.Y.: Aldine de Gruyter), (with G. R. Milner, H. C. Harpending, and K. M. Weiss) "The Osteological Paradox: Problems of Inferring Prehistoric Health from Skeletal Samples" (CURRENT ANTHROPOLOGY 33:343–70), (with D. J. Holman, K. M. Weiss, A. V. Buchanan, and B. LeFor) "Hazards Models for Human Population Biology" (*Yearbook of Physical Anthropology* 35:43–87), and (with D. J. Holman, A. I. Yashin, R. J. Peterson, M. Weinstein, and M.-C. Chang) "A Multistate Model of Fecundability and Sterility" (*Demography* 31:403–26). The present paper was submitted 2 I 97 and accepted 8 IV 97; the final version reached the Editor's office 26 V 97.

One of the principal goals of demographic anthropology is to formulate a coherent theory of preindustrial population dynamics. How do the size and composition of preindustrial populations change over time? How do ecological and economic factors influence population size and growth? Is population growth in any sense regulated, and if so what are the mechanisms by which such regulation is achieved? What is the reciprocal interaction between population growth and economic change? And, perhaps most fundamental, what is the relationship between population size or growth and the health and well-being of the individuals making up the population? Most of these questions date back to Thomas Robert Malthus (1766–1834) and before. They are, moreover, precisely the issues that preoccupied the immediate intellectual forerunners of demographic anthropology (e.g., Carr-Saunders 1922, Krzywicki 1934, Pearl 1939, Birdsell 1957) and, in an important sense, led to the creation of demographic anthropology as an independent field of research. But despite the long attention paid to these questions, they have yet to be answered to everyone's satisfaction.

One reason these questions remain unanswered is that research on the demography of preindustrial populations has been almost exclusively empirical in nature, and the empirical findings generated thus far have been clouded by problems of interpretation. For example, a number of demographic anthropologists have managed to conduct detailed studies of formerly isolated communities that have recently come into sustained contact with the outside world (for a review, see Howell 1986). Although much of what we know about the demography of traditional, non-Western societies comes from such studies, this approach has certain built-in limitations: longitudinal records of population change are, in the nature of things, unavailable for these populations, ages are generally unknown, and demography, health, and economy often begin to change almost immediately upon contact. Thus, it is difficult enough to infer the current demographic structure of such populations and virtually impossible to learn anything of their long-term dynamics. Historical demographic studies, based mainly on family reconstitutions using parish records of baptisms, burials, and marriages, have greatly expanded our knowledge of population processes in early modern Europe (see, especially, Wrigley and Schofield 1981, Knodel 1988). But these studies have their own inherent limitations, reflecting the uncertainties of record linkage, an inability to deal with migration, and the impossibility of calculating base populations for demographic rates in many instances. And, of course, such studies are impossible wherever the necessary docu-

---

[1] This paper has benefited from comments and suggestions by Don Attwood, Jesper Boldsen, Anne Buchanan, Susan Evans, Tim Gage, Henry Harpending, Ken Hirth, Darryl Holman, Pat Johnson, Kevin Johnston, Lyle Konigsberg, George Milner, Kathy O'Connor, Sissell Schroeder, Dean Snow, Bethany Usher, David Webster, and Ken Weiss. To all these colleagues, my heartfelt thanks. This work was partially supported by a grant from the Research and Graduate Studies Office, Pennsylvania State University, and by Penn State's Population Research Institute, which has core support from the National Institutes of Health (NICHD grant no. 2 P30 HD28263-05).

ments do not exist. In principle, the fields of paleodemography and paleopathology should have fundamental contributions to make to our understanding of preindustrial population dynamics—especially when coupled with archaeological, paleoecological, and documentary research into environmental, social, and economic contexts. However, methodological difficulties having to do, for example, with determining the age of skeletons, the confounding effects of population growth, and the complex sampling processes involved in the formation of skeletal collections still make it difficult to interpret the findings of these fields (Sattenspiel and Harpending 1983, Johansson and Horowitz 1986, Wood, Milner et al. 1992, Konigsberg and Frankenberg 1994).

Even if all these methodological problems were to be solved overnight, the approaches adopted to date would still suffer insofar as they remained exclusively empirical. A few demographic anthropologists, historical demographers, and paleodemographers have attempted to draw broader conclusions from the empirical research done thus far (e.g., Flinn 1981, Campbell and Wood 1988, Cohen 1989, Hewlett 1991, Bentley, Goldberg, and Jasienska 1993), but as useful as these efforts have been, they still add up to empirical generalizations, not deeper theoretical understanding. As numerous critics have pointed out (most eloquently Keyfitz 1975), theoretical insight does not flow unaided from data alone but requires the creative interplay of data and formal models.

In this paper, I begin sketching out a theoretical framework for analyzing the dynamics of preindustrial populations. The framework is not intended to be complete—indeed, I write at some length about what it leaves out. It is a starting point, not a final theory, and it is intended to be interpretive rather than statistical—that is, it is intended not for formal estimation or hypothesis testing on specific data sets but to shed light on general patterns. Elsewhere I will use the model to interpret empirical evidence from preindustrial rural England (Wood n.d.).

The theoretical framework is a version of what Bois (1978) has contemptuously dismissed as "the neo-Malthusian orthodoxy" with a few new heterodoxies thrown in. Like most modern demographers, I don't believe that Malthus can be accepted wholesale without modification. My own pet complaint is that he had a very incomplete view of the factors limiting fertility, causing him to overemphasize late marriage as the only effective preventive check on population growth. Nonetheless, I firmly believe that Malthus had important insights into the dynamics of preindustrial populations, insights whose implications are still being explored (see Coale 1979, Watkins and van de Walle 1983, Wrigley 1983, Coleman and Schofield 1986, Rogers 1992, Lee 1993, Cohen 1995*a*). I am happy, therefore, to label my model a theory of "Malthusian" systems. However, I also draw upon more recent ideas from economic demography. These include the insights of the Danish economist Ester Boserup (1965, 1981, 1990), whose writings on the positive effect of population growth on economic innovation have had an important influence on anthropological thought. Boserup is widely regarded (above all by herself) as being anti-Malthusian, but the economic demographer Ronald Lee (1986*a*) has shown that Malthus and Boserup can be combined quite comfortably in a more general theoretical framework.[2] Although other writers have formulated mathematical models that wed Malthus and Boserup (most notably Pryor and Maurer 1982, Cohen 1995*a*), Lee (1977, 1978, 1986*a*, *b*, 1987, 1988, 1993, 1994) has been more successful in this endeavor than anyone else, and my work builds directly upon his. In fact, the material below can be regarded as a generalization of Lee's models for application to nonmonetized economies.

Some readers will probably fault me for stinting cultural, institutional, and political factors in population dynamics. My lack of attention to such things may be a serious mistake, but it is not inadvertent. I agree that such factors are important, always and everywhere, and presumably become even more important as political complexity increases. At this early stage of theory development, however, it seems prudent to focus on a small set of fundamental demographic and economic relationships—relationships that must always exist no matter what the institutional setting. Of course the effects of those relationships will inevitably be modified by the specific cultural and political backgrounds against which they are played out, but by highlighting the welter of potential modifying influences from the beginning we would miss out on all the advantages to be gained by building simple but general models. My goal in this paper is to identify common patterns, not to glory in the particularities of any specific case.

Some Fundamental Questions

The theoretical model presented in this paper is intended to address a series of questions that I regard as fundamental for understanding preindustrial population dynamics. All these questions have a long history of treatment by demographic anthropologists and other population scientists—which is not to say that any of them has been answered. Whatever the final answers may turn out to be, there can be little doubt that they will shed great light on the demographic behavior of preindustrial populations.

1. Is the growth of preindustrial populations "regulated" in any meaningful sense of the word? During the golden age of cultural ecology (roughly 1965 to 1975), the concept of population regulation played a central role in anthropological thought. A wide variety of cultural practices and institutional arrangements were regarded as operating to keep the size of preindustrial populations at levels that could easily be sustained by the

---

[2] Indeed, I will argue that Malthus actually anticipated Boserup's argument. Thus, even Malthus was anti-Malthusian if by "Malthusian" we mean the crude caricature of his thought that has grown up since his vilification by Marx and Engels (for a recent anthropological example, see Harris and Ross 1987:148–53).

local environment and the prevailing system of production. For the most part, the cultural ecologists who supported this view—and there were many who did not (see, for example, Baker and Sanders 1972)—were not overly concerned about how such practices and institutions originated or how they were maintained over time; however, when pressed to comment on these issues, they would make vague arguments that all animal species evolve mechanisms of population control through some form of group selection. These arguments appealed to the work of V. C. Wynne-Edwards (1962), who believed that the differential extinction of populations that grow beyond environmental carrying capacity was a major factor driving the evolution of demographic restraint.

Unfortunately for cultural ecology, by the early 1970s most theoretical population biologists had rejected Wynne-Edwards's model of group selection as a credible force of evolution (Maynard Smith 1976, Wade 1978, Uyenoyama and Feldman 1980). When this fact became common knowledge, it pulled the theoretical carpet out from under many old-style cultural ecologists (Bates and Lees 1979). This development has been salutary in ending some of the worst excesses of cultural ecology, but I would argue that, in reaction, demographic anthropologists have gone too far in the opposite direction: with few exceptions they have abandoned basic research into the mechanisms that limit population growth. Granted, if group selection is unimportant, then it is very unlikely that special behavioral and institutional mechanisms have evolved *in order to* restrain population growth or regulate population size. But that does not mean that factors do not exist that have that effect, even if it is not the reason for their existence. Preindustrial populations plainly do not grow without limit, and some runs of data show that such populations can experience long periods when their size is effectively constant except for stochastic variation (Lee 1987; Wachter 1987; Poos 1991:95–109). It is important to try to understand why this should be, as well as to understand why such populations occasionally grow quite rapidly. Is it possible to formulate a meaningful concept of population regulation—one that does not rely on a theory of group selection and that allows for periods of both constancy and growth?

2. *Is there an optimal population size, and do preindustrial populations tend to equilibrate at the optimum?* The concept of optimal population size is an old one in economics and demography (Sauvy 1969:36–64; Dasgupta 1974), and it is implicit in many older writings on population regulation in cultural ecology. Thus, population size was once thought not only to be regulated but to be regulated at a level that would preserve local resources and provide everyone with the highest possible standard of living (see Hassan 1981:167–75 for a review). We know that Malthus considered this idea preposterous. Was he right? To answer this question we will need, first, to think carefully about what we mean by an "optimal" population size and, second, to examine where the optimum fits in with the larger dynamic behavior of preindustrial populations.

3. *What is the relationship between population growth and economic change?* During the 1970s, a number of anthropologists (e.g., Spooner 1972, Cohen 1977) were powerfully drawn to the idea that population growth is a major stimulus to economic change, as argued by Boserup in her widely read book of 1965. However, as Cowgill (1975) has emphasized, population growth was exogenous to Boserup's original model—it was a given—and the model was therefore incomplete. A failure to deal with this problem turned population growth into a kind of prime mover, something to be taken for granted rather than explained. But if population growth is a constant "tendency" (whatever that may mean), then why do empirical growth rates vary so dramatically from place to place and year to year? Under what conditions can populations reasonably be expected to increase in size? In her more recent work, Boserup (1981, 1990) has confronted these questions head on. Unfortunately, by the time this newer work appeared, the apparently intractable problem of explaining population growth had made most demographic anthropologists skeptical of her model and thus disinclined to read more about it. I suggest that it is time to revive this question as a central concern of demographic anthropology, though in slightly different form.

Boserup's model (or at least her discussion of it) tends to confuse three quite distinct variables: population growth, population density (or size), and population "pressure" on resources. Her theory is really about the latter: how, under a given system of production, an unfavorable ratio between consumers and productive output can create favorable conditions for economic intensification. But most treatments of Boserup's model, including her own, talk as if population growth or density per se were the important predictor variable. Now, in some situations, population growth or density may be useful *measures* of population pressure—that is, they may be sufficiently correlated with pressure to act as statistical proxies for it—but they need not be and certainly are not synonymous with it. And, indeed, in many empirical studies population growth rates and densities are only weakly predictive of economic intensification (for a recent review, see Netting 1993:261–69). Of course, growth rates, size, and density do have one analytical advantage: they are fairly easy to estimate. Population pressure is a much vaguer concept, one with considerable intuitive appeal but not something easy to define, let alone measure. Can we come up with a theoretically compelling definition of population pressure that can help revive Boserup's model?

4. *What are the implications of population growth and economic change for individual health and well-being?* Within anthropology, one of the most influential positions on the long-term relationship between economic change and health is that of Mark Nathan Cohen and George Armelagos (Cohen and Armelagos 1984, Cohen 1989). These writers and their colleagues argue forcefully that economic change under Boserupian population pressure has led to a net deterioration in health for the great bulk of humanity—or at least did so before the modern industrial revolution. In particular, they be-

lieve that the development of settled agriculture and, later, of urban centers was a serious blow to human health in comparison with the rosier conditions of hunting and gathering. Elsewhere my colleagues and I have questioned the evidentiary basis of this claim in some detail (Wood, Milner et al. 1992, Wood and Milner 1994). Here I wish to make a separate point: whatever empirical support this claim may have, it is based entirely on plausibility arguments and has no basis whatsoever in formal theory (see also Pennington 1996). What do theoretical models predict about the relationship between economic change and the average health of a population? And, to raise an issue that has received far less attention, what is the predicted relationship between economic change and the *variance* in health among individual members of the population?

5. *What is the role of crisis mortality in preindustrial population dynamics?* One of the most fascinating discoveries of historical demography is that mortality patterns in late medieval and early modern Europe (roughly 1390–1780) were dominated by frequent but unpredictable mortality crises, brief periods during which the number of burials might increase by 50–100% over the normal run of years (Flinn 1981, Wrightson and Levine 1989, Duncan, Scott, and Duncan 1993, Harvey 1993). In some instances the crises can be linked to documented famine conditions, in others to known outbreaks of infectious diseases such as smallpox or plague—but mostly we do not know what caused them. This discovery immediately raises two important questions. First, can the pattern of frequent mortality crises be generalized to other preindustrial settings? If it can, then what role did these crises play in the overall dynamics of preindustrial populations, especially in population regulation?

Theoretical Background

Data alone, especially the incomplete and equivocal demographic data available on most preindustrial populations, cannot answer the kinds of questions posed here. We need formal theory as a guide to interpreting the data. Before we can do any formal modeling, however, there are several basic issues concerning the relationship between population and resources that need to be addressed. None of these issues is profound or original: discussions of them can be found in any decent introductory textbook on demography, population ecology, or macroeconomics. But the issues still need to be spelled out.

POPULATION REGULATION AND DENSITY-DEPENDENT VITAL RATES

The first and most fundamental issue is what we mean by "population regulation." I offer the following working definition: Population regulation can be said to occur *whenever there exists a locally stable equilibrium for total population size.* The existence of an equilibrium means that there is a population size such that, should it be attained, the population will remain there unless perturbed away from it by exogenous forces. The equilibrium is said to be stable if the population tends to return to it once perturbed. And local stability (as opposed to global stability) means that the population returns to the equilibrium in response to small perturbations, although there may be perturbations large enough to move the population away from the equilibrium permanently (see Gutierrez 1996:237–54 for more technical detail). Since the equilibrium must be subject to random fluctuations in births and deaths, we cannot expect it to be absolutely constant. But as long as environmental conditions stay the same, we will expect to find a stationary probability distribution of population sizes with a more or less narrow range of variation (Turchin 1995).

As I use it, the concept of population regulation is purely dynamic in nature: its existence can be inferred from the observable behavior of the population, without any reference to specially evolved social or behavioral mechanisms designed to enforce the equilibrium. (Like most population biologists, I am deeply skeptical about the existence of such mechanisms.) I will argue below that population regulation in this very general sense is common in preindustrial societies. It does not, of course, exhaust the range of dynamic behaviors observable in such societies. In the short run, major exogenous "shocks" such as famines, epidemics, or wars can temporarily dislodge the population from its equilibrium point. In the longer run, population regulation can give way to secular changes in population size as the demographic equilibrium point shifts in response to changes in climate, improvements in productive technology, or the introduction of new diseases (Hassan 1981, Dewar 1984). Indeed, I will argue that such differences in population movements at differing time scales are likely to be a common feature of preindustrial population dynamics.

One advantage to thinking of population regulation in purely dynamic terms is that we can identify the necessary and sufficient conditions for it to exist. Let N be the size of our population and r be its instantaneous per capita growth rate. Then the conditions for regulation are (1) that a nonzero value of N exists at which $r = 0$ and (2) that $\partial r/\partial N < 0$ in the vicinity of that value (see Charlesworth 1994:51–53 for a mathematical proof). The first condition merely says that a nontrivial equilibrium exists—that is, there is at least one positive value of N at which the population does not grow or decline in size. The second condition says that the population growth rate declines as population size increases, at least when the population is near its equilibrium. It is this second condition, known in population ecology as *negative density-dependence,* that guarantees that the equilibrium is locally stable.[3] If N is perturbed be-

---

[3] "Density-dependence" is something of a misnomer, since we are treating r as a function of the absolute size of the population rather than its relative density. However, the phrase is too well established in the vernacular of population biology to tinker with it now.

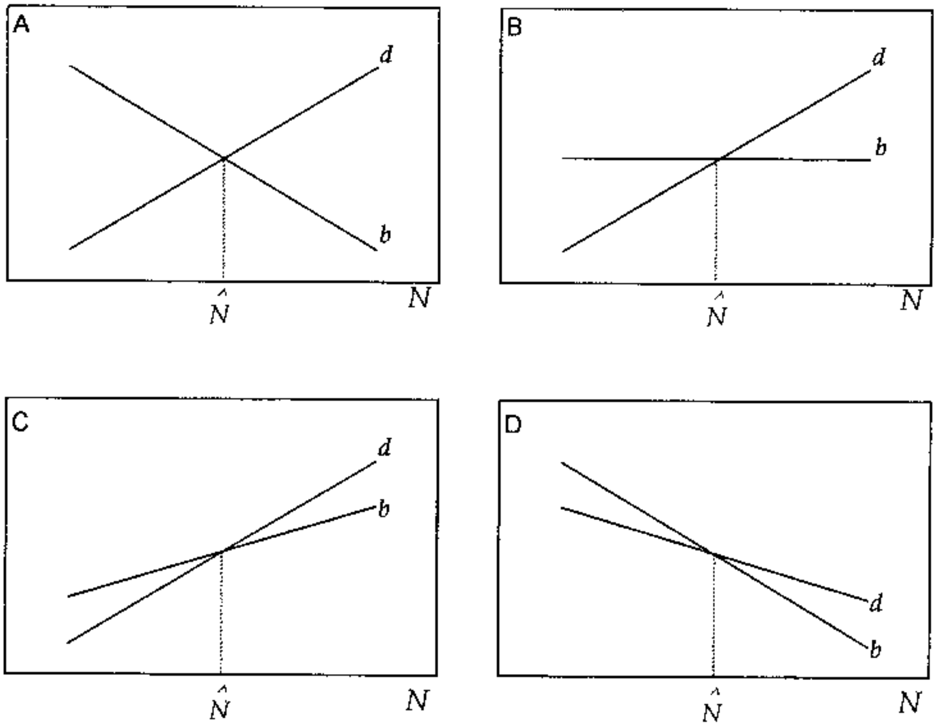

FIG. 1. *Density-dependent vital rates in a population closed to migration. In all the cases shown, the crude birth rate* (b) *or the crude death rate* (d) *changes as a function of total population size* (N). *(The relationships are plotted as linear for simplicity's sake.) The point at which the two curves intersect corresponds to the equilibrium population size* ($\hat{N}$), *where* b = d. *So long as the slope of the death curve is higher than that of the birth curve in the vicinity of* $\hat{N}$, *as is true for all the cases shown, the equilibrium is locally stable and population regulation can be said to exist.*

low its equilibrium level, r increases and the population grows back up to the equilibrium. Conversely, if N is kicked up to some value above the equilibrium, r becomes negative and the population shrinks back down again.

It is often convenient, when modeling population dynamics, to ignore movement of people into and out of the population.[4] In a population closed to migration, the current value r is determined entirely by the prevailing crude birth rate (b) and crude death rate (d). To be precise, $r = b - d$. In a closed population, then, the conditions for population regulation become (1) a nonzero value of N exists at which $b = d$ and (2) $\partial d/\partial N > \partial b/\partial N$ in the vicinity of that value. Figure 1 illustrates various scenarios that meet these conditions. What all these scenarios have in common is that they exhibit a point at which the fertility and mortality curves intersect, corresponding to the equilibrium population size, and a mortality curve with a slope whose value is higher than that of the fertility curve, at least near the equilibrium. Below the equilibrium, fertility is greater than mortality and so the population grows; above the equilibrium, mortality exceeds fertility and the population declines—the essence of population regulation.

But are any of these scenarios especially plausible? A theoretical argument from the field of physiological ecology (Sibly and Calow 1986:10–27; Ulijaszek 1996) suggests that something like scenario A may often prevail under preindustrial conditions. The logic of that argument is summarized in figure 2. In very general terms, an organism can allocate the food it consumes in

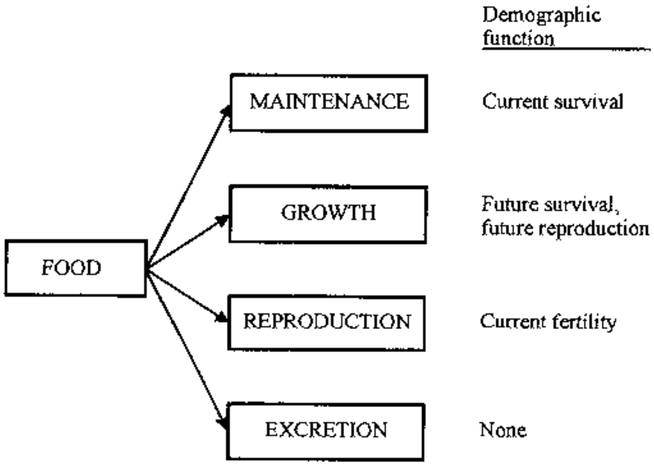

FIG. 2. *The allocation problem in physiological ecology.*

[4] There is no logical reason that immigration and emigration cannot contribute to density-dependent changes in population growth (see, for example, Wood, Smouse, and Long 1985).

four ways: it can put it into *maintenance*, that is, it can support basic metabolic processes while maintaining tissue integrity against trauma or infection; it can put it into *somatic growth* or the construction of new tissue; it can put it into *reproduction*; or it can simply lose it through *excretion*, including heat loss, the unavoidable energetic costs of digestion, and the elimination of indigestible materials and toxic by-products of metabolism. Since a given quantum of food cannot be committed to more than one of these functions, all four compete with each other when food is available in limited amounts. From a biological perspective, the individual life course represents the organism's attempt to resolve this competition in a way that maximizes its genetic fitness. For example, humans, like other primates, reduce the competition by separating the periods of growth and reproduction almost completely from each other. But the competition can never be eliminated entirely. From a demographic perspective, the resolution of this competition has important implications for population growth. The ability to support adequate maintenance is directly related to an individual's current risk of death. Somatic growth affects its future chances of both survival and reproduction. And, obviously, reproductive function is an essential component of current fertility. Moreover, reproduction now may influence the organism's immediate and future risk of death.

Malthus assumed that the per capita supply of food decreases as the population grows. If this assumption is correct (and I examine it in detail below), then the allocation problem diagrammed in figure 2 should worsen as population size increases. It is precisely this theoretical relationship that, if true, would lead to density-dependent changes in birth and death rates and hence to population regulation.

Although he did not use the term, density-dependent changes in birth and death rates are implicit throughout Malthus's writings. He classified the factors limiting population growth as either *positive checks* (those operating via mortality) or *preventive checks* (those operating via fertility).[5] Malthus's view of the positive checks is, in its essence, fully compatible with our current understanding of the determinants of mortality in preindustrial settings. Food limitation does indeed cause mortality rates to increase, especially in young children, among whom the competition between maintenance and growth is at its fiercest (Martorell and Ho 1984). We now know that one of the main reasons for this relationship is that undernutrition damages the ability of the growing child to mount an effective immune response to infection, a clear example of compromised maintenance (Shell-Duncan 1993, Shell-Duncan and Wood 1997). Moreover, we know that the synergism between undernutrition and infection is the leading cause of childhood mortality worldwide (Gage and O'Connor 1994). Thus, Malthus's assumption that there is a direct link between food availability and the positive checks has been amply vindicated.

In contrast, Malthus's view of the preventive checks now seems not so much wrong as too narrow. He argues from the premise (and he treats it as no more than that) that marital fertility is effectively constant everywhere: "It is probable that the natural prolificness of women is nearly the same in most parts of the world, but the prolificness of marriages is liable to be affected by a variety of circumstances peculiar to each country; and particularly by the number of late marriages" (1803, reprinted in James 1989, vol. 2:4). In modern demographic jargon, he is saying that age-specific marital fertility rates are everywhere more or less the same, but the age-specific proportion of women who are married at a given time varies widely; thus, variation in age patterns of marriage ("the number of late marriages") is the only *important* cause of variation in total fertility ("the prolificness of marriages"). We now know that this premise is incorrect: age-specific marital fertility rates vary widely by region and period, even under strictly preindustrial conditions (Wood 1994*a*:37–46, 54). While Malthus was right in arguing that variation in marriage patterns can be *one* important check on fertility and is sometimes the dominant check (Hajnal 1965, Wrigley 1986), other factors can also come into play: breastfeeding patterns, rates of sexual maturation, coital frequency, various aspects of male and female reproductive physiology that influence how long it takes a couple to conceive, the incidence of pregnancy loss and pathological sterility, and so forth (Wood 1994*a*:67–73). At least some of these factors are likely to be as responsive to the availability of food and other critical resources as are the prospects for marriage (Wood 1994*b*).

The important point, however, is that Malthus believed that both fertility and mortality would respond to food availability in density-dependent fashion, thus causing populations to grow when food is abundant and contract when food is scarce. Thus, for Malthus, the propensity for populations to grow under favorable conditions is attributable not to some mysterious inner tendency but to the effects of food resources acting on fertility and mortality through specifiable mechanisms, or "checks," as he called them.

WELL-BEING AND SUBSISTENCE

If we find that birth rates decline as population size increases or that death rates increase (or both), then we have good reason to suspect that population regulation may be operating. Throughout this paper, I will use the term *well-being* to refer to any aspect of individual health or physical condition that is either positively associated with the probability of childbirth or negatively associated with the risk of death. Thus, the criterion for population regulation is that average well-being declines as population size increases, at least in the neighborhood of equilibrium. Phrasing it in terms of the logic in figure 2, well-being represents an individual's ability

---

[5] Malthus plainly means "positive" in the sense of "active." Thus, the positive checks actively trim back some portion of the population, whereas the preventive checks keep it from being produced in the first place.

to allocate food to maintenance, growth, and reproduction while minimizing unnecessary loss through excretion. Since food limitation is expected to increase the competition among maintenance, growth, and reproduction, thereby compromising one or the other of these functions, the idea that well-being declines as the per capita supply of food shrinks seems altogether reasonable.

A person's well-being can be thought of as determining (within a particular social context) a set of probabilities of surviving and reproducing at each age. Writing $l(a)$ for the probability of surviving from birth to age a and $m(a)$ for the rate at which like-sex offspring are produced at age a, there must be a set of those quantities for which

$$\int_0^\infty l(a)m(a)da = 1. \tag{1}$$

The individual whose well-being provides just this combination of values exactly replaces himself or herself demographically over a lifetime. A population in which the *mean* well-being yields the same combination in the aggregate is one that is neither growing nor declining: b equals d under this average state of well-being, and the population is at an equilibrium. The mean value of well-being that meets the condition in equation 1 will be called the *subsistence* level of well-being, for it just allows a population to remain in play in the existential game of life.

The concept of a subsistence level of well-being recurs throughout Malthus's work on population and economy. It was given formal expression in the writings of Malthus's good friend and intellectual sparring partner David Ricardo, who wrote: "The natural price of labour is that price which is necessary to enable the labourers, one with another, to subsist and to perpetuate their race, without increase or diminution" (1819:85). Both Malthus and Ricardo (who disagreed about almost everything else) believed that populations tended to stabilize at levels where the "natural price of labour" prevailed, a principle that Ricardo called *the iron law of wages*. This concept can be regarded as an early version of density-dependent population regulation.

POPULATION SIZE AND THE SIZE OF THE LABOR FORCE

We now need to ask whether population regulation, in the sense defined above, is likely to be a common feature of preindustrial economies. In subsistence economies, the linkage between population size and food availability arises from the close relationship between population size and the size of the labor force. For simplicity, we will assume that the relationship is proportional: the larger the population, the larger the (potential) labor force, and by a constant factor. In reality things are not so simple. The *composition* of the population has important consequences for the size and quality of the labor force, over and above the effects of sheer population size. The most obvious compositional effect (at least to a demographer) is captured by the *dependency ratio*, the ratio of nonproductive consumers to producers. It is usually assumed in economic demography that everyone under age 15 and over age 65 is nonproductive and that everyone else is a producer; thus, the dependency ratio is determined by the age structure of the whole population. Interestingly, the dependency ratio thus defined is fairly constant across a wide range of age structures, with individuals ages 15–65 usually making up about 60–65% of the whole (Cipolla 1993:54–58). (The most conspicuous difference between preindustrial and industrialized populations in this regard is in the proportion of dependents who are elderly rather than juvenile.) So, without too much loss of significant detail, the dependency ratio can be treated as constant at the population level—though certainly not at the household level, where it changes in the course of the life cycle of the family. In the aggregate, though, an assumption that the size of the labor force is always proportional to total population size is probably not a bad one.

Labor (and hence total population) can be viewed as just another input into a system of economic production. But, as Malthus emphasized, it is an odd input, for each new pair of hands to help is another mouth to feed. In a sense, it is the dual role of individuals as producers and consumers that leads to Malthusian checks on population growth.

PRODUCTION FUNCTIONS AND THE DECLINING MARGINAL PRODUCTIVITY OF LABOR

As a general rule, gains in productivity resulting from increasing inputs into an otherwise fixed system of production are not linear but decline as the level of input increases. (Here we are assuming that technology and the organization of production are not changing; the issue of change will be taken up later.) For example, we can increase the total agricultural output of a plot of land by adding more fertilizer to it, but we cannot do so without limit. At some point the increase in production starts to diminish because other limiting factors come into play or, indeed, because we are poisoning the land with too much fertilizer. If we push things too far, we may even begin to see decreases in total production because of the increasing load of fertilizer. This nonlinear relationship between total production and the individual factors of production is what John Stuart Mill called "The Law of Diminishing Returns" (Mill 1848:book 1, chap. 13). More prosaically, it is often referred to as *declining marginal productivity*. (Marginal productivity is the increase in total production resulting from a unit increase in a particular input into the system of production when all other inputs are held constant.) The general relationship is summarized by the shape of the *production function*. As illustrated in figure 3, the relationship can take several forms. At best, it can increase linearly as in A—which is why Malthus argued that the food supply can never increase as more than an arithmetic (linear) function of labor input. But A is truly the

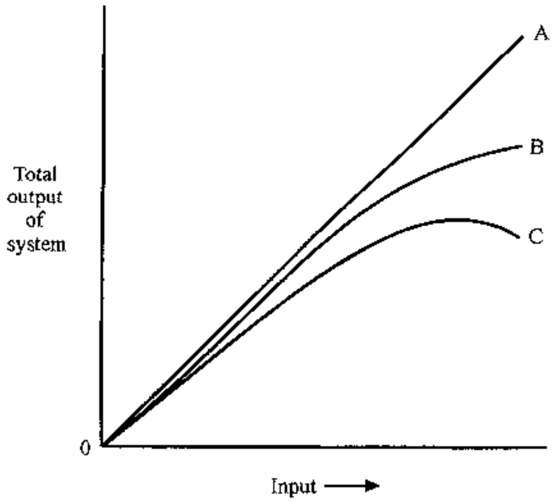

FIG. 3. *Three theoretical production functions.*

best-case scenario: both theoretical arguments and empirical evidence suggest that B and C are far more likely, especially under preindustrial conditions (Clark and Haswell 1967:85–106; Ellis 1993:17–44). Whenever the production function departs from linearity in the direction of B and C, we can speak of the declining marginal productivity of the particular input being examined.[6]

Malthus is often misunderstood on this point. The caricature of his view that still prevails in the literature is something like "Food resources tend toward an arithmetic increase." In fact, Malthus claimed no such thing. For one thing, he never argued that there is any inherent tendency whatsoever for food resources to increase; on the contrary, he felt that such increases had to be wrested from economic systems by deliberate action—by increased inputs of labor or capital. Nor did he ever argue seriously that food could undergo anything like a sustained linear increase. Instead, he called such a trend "the very utmost that we can conceive" (1798:22), "probably a greater increase than could with reason be expected" (1806:11), and even "certainly far beyond the truth" (1798:22). In his *Summary View* of 1830, written just four years before his death, Malthus stated his position clearly: "The main peculiarity which distinguishes man from other animals, in the means of his support, is the power which he possesses of very greatly increasing these means. But this power is obviously limited by the scarcity of land . . . and by the *decreasing proportion of produce* which must *necessarily* be obtained from the continual additions of capital applied to land already in cultivation" (Malthus 1830, reprinted in Flew 1970:225, emphasis added). And from the same document: "the rate of increase of food [under the most favorable conditions] would certainly have a greater resemblance to a decreasing geometrical ratio than an increasing one. The yearly increment of food would, at any rate, have a constant tendency to diminish, and the amount of the increase of each successive ten years would probably be less than that of the preceding" (Malthus 1830, reprinted in Flew 1970:239). Plainly, Malthus was a firm believer in declining marginal productivity.

Indeed, one of Malthus's most fundamental insights was that labor inputs into food production show declining marginal productivity and that *as a result* there is a curvilinear relationship between population size and total productivity (fig. 4, *top*). The relationship shown in the upper panel of figure 4 immediately implies a declining per capita productivity as population size increases (fig. 4, *bottom*). On average, then, the food resources available to each member of the population are expected to decline as the population expands, at least as long as the overall system of production is otherwise unchanged. When labor is the input under consideration, declining marginal productivity implies diminishing per capita consumption, at least on average.

The precise shape of the production function is important from several points of view. Most obviously, the overall height of the curve, the steepness of its ascending segment, and the position of the plateau where total production starts to level off have important implications for the system's ability to absorb a rising population. When comparing the demographic dynamics of populations with differing systems of production, therefore, it is necessary to take account of any differences in their respective production functions. In addition, there may be other details of the production function that can prove important in certain circumstances. For example, the curve may have a lower inflection point (fig. 5). Such an inflection could occur because there is a population level below which the labor force is too small to run the system of production efficiently. (One immediately thinks of a complex irrigation system that needs many workers to keep its channels open and flowing.) Under these circumstances, total productivity may increase very slowly with increasing population until the lower limit is surpassed. As we will see later, such details can have interesting effects on population dynamics.

---

[6] There is a simple but compelling reason to expect that marginal productivity will eventually decline: if more than one input limits the size of the total physical product, then increasing a single input while holding all the others constant cannot possibly result in an unlimited expansion of production. More complex theoretical models (summarized by Doll and Orazem 1978:11–42) always build upon this simple idea, though they may bring in additional considerations such as interactions among the inputs, the substitutability of inputs, and the possibility that increases in certain inputs may, if taken far enough, actually impair production. Unlike most economic "laws," declining marginal productivity has been subjected to (and has consistently passed) repeated experimental tests, going back to Johann von Thünen's pioneering studies in the early 19th century and continuing with the work of numerous 20th-century agronomists (Heady and Dillon 1961). And where the data permit it, the model of diminishing returns has been tested and confirmed statistically for preindustrial economies, both agricultural and pastoral (see, for example, the analysis of Domesday Book tax assessments by McDonald and Snooks 1986:97–117). There can be few principles in economics that are more firmly established than the law of diminishing returns.

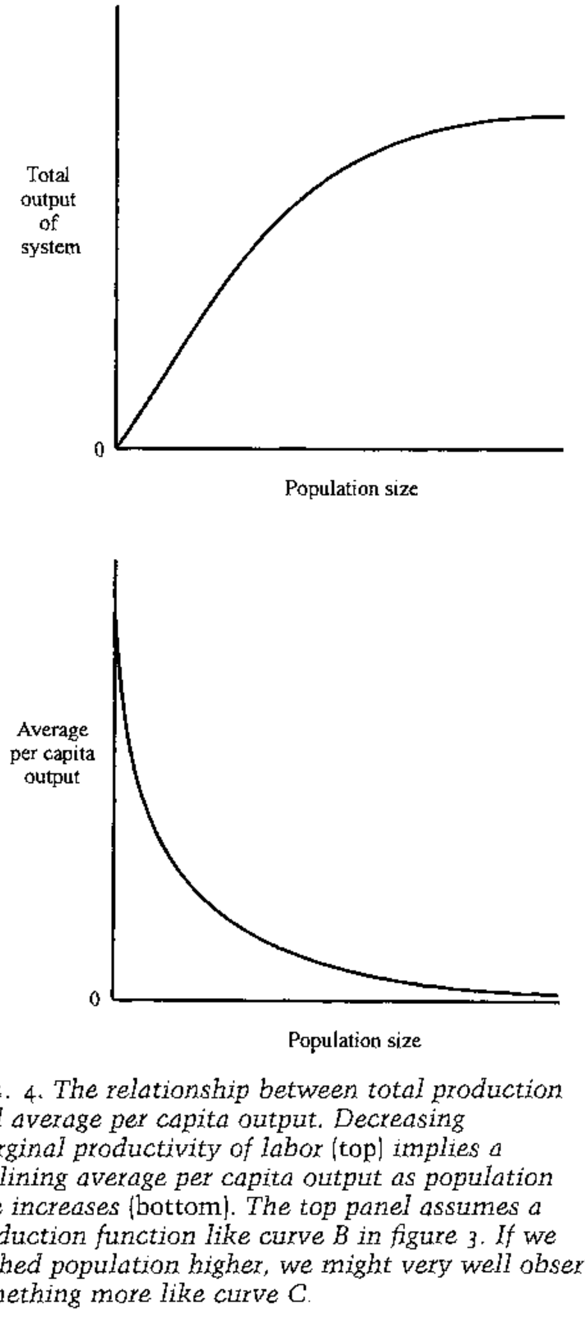

FIG. 4. *The relationship between total production and average per capita output. Decreasing marginal productivity of labor* (top) *implies a declining average per capita output as population size increases* (bottom). *The top panel assumes a production function like curve B in figure 3. If we pushed population higher, we might very well observe something more like curve C.*

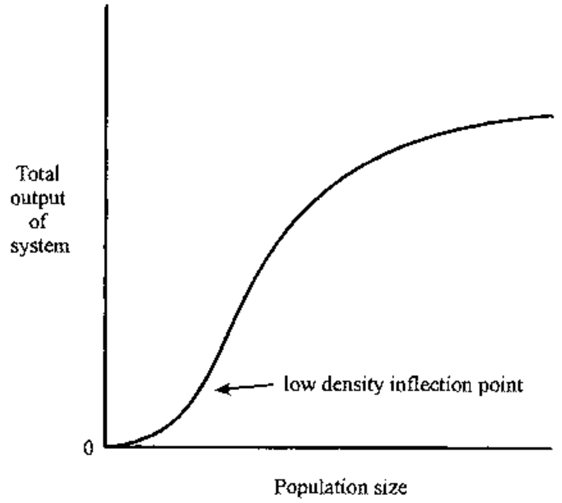

FIG. 5. *An inflected production function. At low population sizes or densities, the production function may rise slowly at first and then accelerate as enough workers become available to run the system efficiently.*

DEMOGRAPHIC SATURATION

The declining marginal productivity of labor implies that a point exists at which the increase in total production achieved by adding one more individual to the labor force is just enough to support one more individual at the subsistence level of well-being. I will refer to that point as the *demographic saturation* point of the system under study (fig. 6). More precisely, demographic saturation occurs when the increment in marginal productivity is just enough to allow one more individual to survive and reproduce at levels just sufficient to replace himself or herself exactly. To quote Malthus again, "if the capacity of the soil were at all times put properly into action [i.e. if all available arable land were cultivated suitably], the additions to the produce would, after a short time, and independently of new inventions, be constantly decreasing, till, in no very long period, the exertions of an additional labourer would not produce his own subsistence" (1830, reprinted in Flew 1970: 243). In other words, for a *fixed system of production* (or "independently of new inventions," as Malthus puts it), there is a threshold population size, which we will call demographic saturation. Below the threshold, average per capita output is high enough that individuals can thrive, survive, and procreate (conditions that Malthus called "plenty"); above the threshold, thriving, surviving, and procreation are too compromised to support further population growth (Malthus's conditions of "misery"). All other things being equal, population will tend to grow under conditions of plenty and decline under conditions of misery. The level of per capita output at the dividing line between plenty and misery is equivalent to Ricardo's natural price of labor.

What we are assuming, in effect, is that there is a direct relationship between average per capita output and average well-being in the sense defined above: as the former goes down, the latter can be expected to go down as well. This relationship leads automatically to density-dependent changes in fertility and mortality and, hence, to population regulation. For preindustrial economies in which most laborers are directly involved in food production for themselves and their families, the hypothesized linkage between average per capita output and average well-being seems incontrovertible (although, obviously, social and political differentials

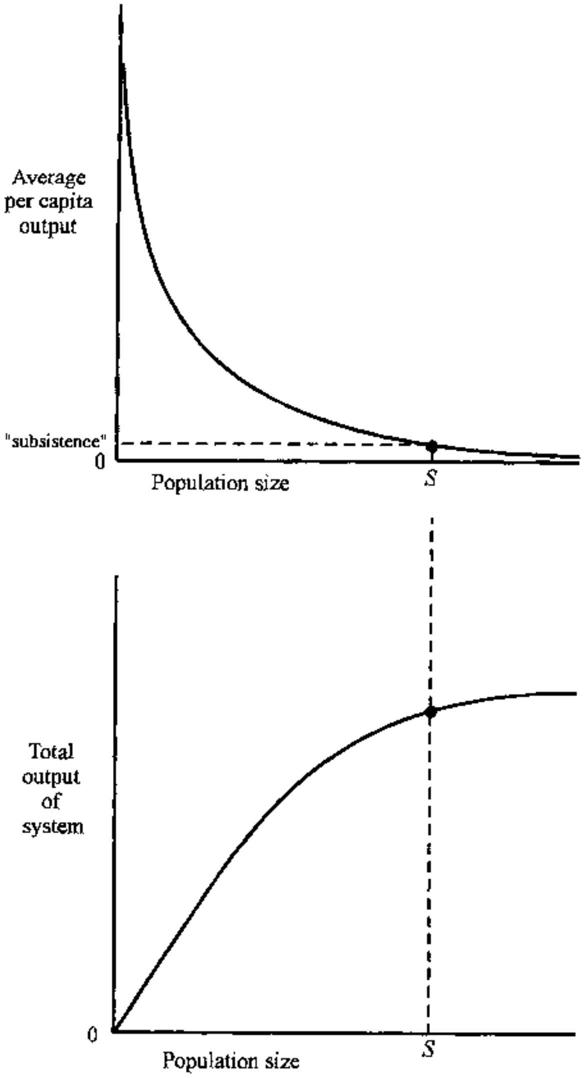

FIG. 6. *The concept of demographic saturation. If per capita output falls with increasing population size* (top), *then a point will be reached at which the per capita output achieved by adding one new worker is just enough to support that worker at the subsistence level. The corresponding size of the population, labeled* S, *is the demographic saturation point of the system of production under consideration* (bottom).

may complicate the relationship in all but the most egalitarian societies).

Demographic saturation plays much the same role in our model as "carrying capacity" does in traditional ecological theory. There are, however, some differences of emphasis that I think are important. First, demographic saturation is a point along a particular production function, and the saturation point can be expected to change whenever the system of production changes. Thus, it is meaningless to speak of the saturation point of a given environment, habitat, or set of resources, as is sometimes done in the case of carrying capacity (see Dewar 1984 for a cogent discussion of this point). At the risk of introducing a spurious precision, we might write $S_t = \mathit{func}(L_t, R_t, T_t, O_t)$, where S_t is the demographic saturation point of a given system of production, *func* is a monotonically increasing function, and L_t = the amount of land available at time t, R_t = usable resources per unit land at t, T_t = productive technology available at t, and O_t = the organization of production at t. It is this cluster of variables that we have in mind when we speak of a particular system of production. All these variables are indexed by t: they can all change over time. Land can be cleared or lost from cultivation, climatic deterioration can depress the effective resources available on the land or remove marginal land from production, new tools can be brought to bear, new ways of organizing labor and capital can be devised. It is meaningful to speak of a single demographic saturation point only insofar as L_t, R_t, T_t, and O_t are fixed.

A second advantage to thinking in terms of demographic saturation instead of carrying capacity is that the former encourages us to contemplate the entire production function. S_t is but one point along a continuous curve of output, and the shape of that curve—the form of the production function—has important implications for demographic dynamics. We will explore some of those implications in a later section.

THE CONDITIONS FOR ECONOMIC CHANGE

To this point, we have been speaking of a fixed system of production. It is perfectly obvious, however, that subsistence techniques and the organization of production changed profoundly over the course of the preindustrial era. Hunter-gatherers became agriculturalists, and agriculturalists intensified their cultivation in myriad ways. How should we think about these changes?

The most fundamental point in this connection is one emphasized by Boserup (1965:65–69): innovation is rarely if ever cost-free. For example, irrigated rice paddies can do marvelous things for productivity, but they represent an investment of labor and capital that would have been burdensome for the typical preindustrial household. Similarly, the switch from a two- to a three-field system of crop rotation that occurred in early medieval Europe (White 1962) would have required a major reorganization of tenurial rights for an ultimate outcome whose benefits would have been hard to predict. The "conservatism" and "risk-aversion" so often said to typify peasants (Ellis 1993) make a lot of economic sense when one is already living on the margins of misery. Someone who is not rich to begin with will need some persuading before deciding to make any capital improvement. Boserup referred to this economically rational resistance to change as "technical inertia" (1965: 68).

In the face of technical inertia, the economic system can be expected to change only if it is somehow forced to. Malthus and Boserup agree that the decline in per capita output associated with increasing population size can, if pushed far enough, be a major inducement to economic innovation (see, for example, Malthus 1798:29–35). And the two agree that, given the opportunity, people will usually opt for innovations that, on average, increase their fertility and/or reduce their mortality—in our terminology, they will prefer changes that entail a net increase in their well-being.

Thus, there are likely to be understandable relationships among population size, production, well-being, and the pressure to innovate. The pace of economic change partly reflects those relationships, though it may be slowed by the sporadic nature of invention and the sluggish spread of new techniques under preindustrial conditions of transport and communication. And there may be ultimate limits to how far innovation can carry us. It is at least plausible to suppose that, when any system of production grows elaborate and specialized, it may become resistant to further change by incremental technical improvements—one either has to jettison the entire system and start over again or learn to live within the limits of the old system, even when those limits are shrinking. (These ideas will seem plausible to anyone who was living in the Rust Belt of the United States during the 1970s.) Innovations falling early in the trajectory of technical elaboration can make a big difference, whereas innovations late in that same trajectory may make only modest improvements and may even damage an already well-adjusted system of production. It is tempting, then, to posit a law of declining marginal productivity of innovation: it is possible for an economic system to become "too good" to allow for further improvement.

A Model of Preindustrial Population Dynamics

Building on these theoretical concepts, we can construct a simple model of the relationships among individual well-being, population growth, and the system of production in preindustrial economies. It should be appreciated that a model of this kind is not intended to be comprehensive or realistic: it leaves out too many important things to conform to any specific empirical case. I am only trying to capture general relationships, not details, and the model may be of special use precisely because it tells us plainly what it leaves out as well as what it includes.[7]

THE MALTHUSIAN COMPONENT

How would Malthus have parameterized the relationship between population size and physical well-being? For the moment, we will think strictly in terms of the *average* well-being of the population at time t, denoted w_t. (The variance in well-being will be discussed in a later section.) By its nature, well-being influences the prospects of surviving and reproducing. We assume that there is some level of average well-being, call it θ, at which population replacement is just possible: that is, if w_t exceeds θ the population tends to grow; if w_t is less than θ it tends to decline. As a first approximation, θ can be thought of as fixed by biology and thus constant for all systems of production. Finally, we assume that w_t is determined by how close population size is to the demographic saturation point, where, by definition, $w_t = \theta$. These assumptions would seem to capture the basic way in which Malthus conceptualized the interactions of demography and economy under a fixed system of production.

Our assumptions can be parameterized as

$$w_t = \theta(S_t/N_t)^{\kappa}, \tag{2}$$

where N_t is the size of the population at time t and S_t is the current value of demographic saturation. The shape parameter κ is determined by the details of the curve of declining marginal productivity of labor. Like all the parameters in this model, θ and κ are assumed to be positive.

Writing b_t and d_t for the population's crude birth and death rates, respectively, at time t, we can specify the basic demographic relationships as

$$b_t = \beta_0 + \beta_1 \ln w_t + \beta_2 d_t \tag{3}$$

and

$$d_t = \delta_0 - \delta_1 \ln w_t + \delta_2 b_t. \tag{4}$$

These equations are written in terms of the logarithm of w_t purely for mathematical convenience. The parameter β_1 represents Malthus's preventive check on fertility, whereas δ_1 is his positive check on survival. The parameters β_2 and δ_2 capture second-order interactions between fertility and mortality, which are almost certain to occur under preindustrial conditions. For example, mortality may increase fertility by disrupting breastfeeding or by making land available for couples who wish to marry and establish new households; fertility may increase mortality through nursing competition (see Wood 1994*a*:536–37 for a discussion of these interactions).

Assuming that the population is closed to migration,

$$\frac{1}{N_t}\frac{\partial N_t}{\partial t} = b_t - d_t = r_t, \tag{5}$$

say, where r_t is the population's per capita growth rate at time t.

To complete the system, we need to constrain equations 2–5 so that $b_t = d_t$ whenever $N_t = S_t$. This constraint is implicit in the definition of demographic saturation.

In the dynamical system defined by these equations, the only exogenous variable is S_t—its behavior affects the system but is not determined by it. As already noted, S_t is a function of the amount of land exploited

---

[7] A partial list of the things left out includes the age structure of the population, time-lagged effects, spatial heterogeneity and migration, interlocal exchange, nonfood sectors of production, and the whole range of relationships and interactions that are normally grouped under the heading of "political economy."

by the population, the resources available per unit of land, prevailing technology, and the current organization of production. For the moment, we will assume that these (nonlabor) factors of production are fixed at any given point in time. Of course, any or all of them may fluctuate over time, which is why S_t is indexed by t.

As we will see, the behavior of S_t can play a leading role in governing the overall dynamics of our system. That behavior can be summarized in terms of a variable that I shall call $\dot{S}/S_t$, defined as

$$\dot{S}/S_t = \frac{1}{S_t}\frac{\partial S_t}{\partial t} = \frac{\partial \ln S_t}{\partial t}. \tag{6}$$

When $\dot{S}/S_t = 0$, the demographic saturation point—and, by implication, the entire system of production—is fixed. When $\dot{S}/S_t > 0$ the system is expanding, and when $\dot{S}/S_t < 0$ it is contracting. It is important to bear in mind, however, that the value of $\dot{S}/S_t$ is exogenous to the model as thus far defined—which does not mean we are not allowed to think about it in interesting ways.

Given the equations specified above, the demographic system goes to an equilibrium with the following vital rates (using a carat to denote equilibrium values):

$$\hat{b} = \hat{d} + (\dot{S}/S_t), \tag{7}$$

$$\hat{d} = \hat{b} - (\dot{S}/S_t), \tag{8}$$

and

$$\hat{r} = \dot{S}/S_t. \tag{9}$$

When $\dot{S}/S_t = 0$, $\hat{b} = \hat{d}$ and $\hat{r} = 0$, so the population is not changing in size. At that equilibrium for population size,

$$\hat{N} = S_t \text{ when } S_t \text{ is fixed.} \tag{10}$$

It is easy to verify that $\hat{N}$ is locally stable so long as S_t is fixed. Thus, *for a given system of production* (S_t fixed, $\dot{S}/S_t = 0$), this is a model of population regulation. The population is at a stable equilibrium corresponding to the demographic saturation point of the system and will remain there (or return there in the face of small exogenous perturbations) as long as the system of production remains unchanged. Any longer-term changes in population size must be led by changes in S_t.

It follows immediately from equation 2 that

$$\hat{w} = \theta \tag{11}$$

whenever $N_t = S_t$. Thus, average well-being always equilibrates at the level just sufficient for demographic replacement. This equilibrium is completely independent of either N_t or S_t; no matter what population level the system equilibrates at, whether high-density or low-, people will be just barely scraping by. If we can assume that θ is set by physiological requirements that are unlikely to change much from place to place or time to time, it follows that the equilibrium well-being is the same for all systems of production and that it is always at the margins of misery. This, of course, was Malthus's view. It was also Ricardo's, encapsulated in his concept of the natural price of labor. In recognition of this parallel with Ricardian thought, we might refer to equation 11 as *the iron law of well-being:* left to itself, a preindustrial system of production will tend toward a state in which the average individual is in just good enough condition to replace himself or herself demographically.

This model has several important implications for our understanding of preindustrial population dynamics. First, it suggests that population regulation may be as widespread a phenomenon as the declining marginal productivity of labor—the latter implies the former. Consequently, population regulation requires no special pleading about institutional or behavioral mechanisms of population control; in other words, population regulation can exist in the absence of group selection. A second implication is that "regulated" populations equilibrate only at the demographic saturation point. Thus, the view that preindustrial populations routinely equilibrate well below the level supportable by the prevailing system of production is inconsistent with the theory developed here.

A third implication of the model is that "overshoots" of population—in which population size temporarily surpasses the saturation level, thus sparking a demographic crisis—are not a necessary feature of Malthusian systems. This conclusion is contrary to the belief of many economic historians (e.g., Le Roy Ladurie 1974: passim; Postan and Hatcher 1985:69) though not to anything that Malthus himself ever wrote (see Malthus 1806:60–61). Overshoots *can* occur if the interactions of fertility and mortality are strong enough, and they are rather more likely to occur if we add age structure and time lags to our model (Rogers 1992), but they are by no means inevitable.[8] Thus, demographic saturation does not necessarily entail a "Malthusian crisis" followed by an inexorable reduction in population size. There is no intrinsic reason a population at demographic saturation cannot persist in that state indefinitely if conditions remain unchanged, no matter how unhappy that state may be for most of the population's members.

A final implication of the model is that demographic saturation *may* involve Malthus's positive check (an increase in mortality), but it may also involve the preventive check on fertility. Nothing in the model requires that mortality increases near saturation, so long as fertility declines. Whether fertility or mortality is more important in Malthusian population regulation is an empirical question, not a theoretical one. On empirical grounds, I suspect that mortality responses to popula-

[8] Dynamically, population overshoots result in a series of oscillations around the saturation point. Under reasonable parameter values, these oscillations are normally damped, so that population size eventually settles down at the saturation point. More extreme (and, I would argue, less plausible) parameter values can result in stable oscillations, diverging oscillations, or even chaotic fluctuations in population size. Although such odd demographic behaviors are mathematically possible, I know of no compelling evidence that human populations ever exhibit them.

tion growth are in fact more common than fertility responses, but they usually take the form of modest increases in death rates, especially among the very young (see, for example, Wood and Smouse 1982). In other words, the positive check does not necessarily involve catastrophic mortality of the sort associated with famine, war, or pestilence. This is simply another way of saying that Malthusian saturation need not induce a Malthusian crisis.

Our system will tend to display somewhat different dynamics depending upon the time scale at which it is observed. The short-term dynamics (over a period of a few years) will be dominated by homeostasis, in that the population will usually be close to and tending toward the equilibrium determined by the current value of S_t. The system is thus Malthusian in the short run. The middle-term dynamics (over the course of a few generations) will be dominated by low-frequency, low-amplitude oscillations caused by the second-order interactions of b_t and d_t, unless the system is already locked into equilibrium. One might say (rather infelicitously) that the system is "Easterlinian" in the middle term (see Easterlin 1968, Lee 1974). The long-term dynamics (observed over a few centuries) may well display secular trends in population size led by changes in S_t. Since those changes may arise from technological innovations, advantageous reorganizations of production, or the introduction of new crops, it is tempting to say that the system is Boserupian in the long run. However, changes in S_t may just as easily reflect climatic deterioration or the destruction of food resources by blights, rusts, and swarming pests. Suffice it to say that the long-term dynamics of the system will often be non-Malthusian, but the long-run trends may be better or worse than Malthus's marginal immiseration. And if there is any truth to the idea of the declining marginal productivity of innovation, a system that is truly Boserupian in the long run may yet encounter Malthusian limits in the *longer* run.

THE BOSERUPIAN COMPONENT

It is one thing to say that the system *can* be Boserupian in the long run even if Malthusian in the short, but it is an entirely different matter to show how Boserupian change can be made endogenous to the system itself. Under what circumstances might we expect S_t to increase? The heart of Boserup's argument is that the conditions for economic expansion can arise as a result of population growth itself. As the population approaches demographic saturation from below, the marginal productivity of labor declines; as a consequence mean per capita output shrinks, causing a reduction in the average individual's standard of living. In Boserup's view, such adverse effects of population growth on the standard of living can provide strong inducements for the adoption of new means of production—her own particular interpretation of the concept of population pressure. Following this logic, the pressure to adopt new means of production (technological or organizational) can be parameterized under our model as

$$P_t = \rho\,[(\theta/w_t)^{\lambda} - 1], \qquad (12)$$

where ρ and λ are positive constants. According to this definition, the pressure is positive whenever $w_t < \theta$ and negative whenever $w_t > \theta$, where, as before, w_t is the current mean level of well-being and θ the subsistence level of well-being. Population pressure does not involve population size or density per se but rather reflects the current average well-being relative to the subsistence level—which in turn depends, according to equation 2, on the ratio of population size to the demographic saturation point of the current system of production. Population size, population density, or population growth rates, considered in isolation, do not capture this full set of relationships.

One of Boserup's most important insights is that there may exist some minimally *acceptable* standard of living (call it θ^*) that is higher than the bare subsistence level associated with θ and that pressure to innovate may begin whenever w_t drops below θ^* even though it may still be well above θ. This insight can be accommodated by substituting θ^* for θ in equation 12. But there is an interesting implication of this change, for at the short-term Malthusian equilibrium we would have

$$\hat{P} = \rho[(\theta^*/\theta)^{\lambda} - 1], \qquad (13)$$

which *must* be positive whenever θ^* is greater than θ—when people are unwilling to settle for bare subsistence. Thus, if left to itself, our system tends toward a state in which the conditions for economic expansion are met. This specification removes the principal objection to Boserup's theory, namely, that she treats population growth as exogenous. The model also tells us that Malthus and Boserup, so often portrayed as irreconcilably different, are in fact perfectly compatible with each other.[9]

Note that we have two rather different scenarios that will induce positive pressure for innovation, both consistent with our model system. On the one hand, if $\theta^* = \theta$ (no difference between subsistence and the minimum acceptable standard of living), then P_t will be positive whenever exogenous shocks drive w_t below θ, as might occur, for example, if fluctuations in weather were to

---

[9] Actually, Malthus would have had no trouble accepting Boserup's argument. Indeed, he advanced it himself in the first edition of his *Essay on the Principle of Population* (1798). I suspect that most modern readers miss this point because it is buried in a long theological discussion in the penultimate chapter that seems distinctly quaint nowadays—though it must have seemed downright heretical in Malthus's own time, which may be why the good reverend dropped it from later editions of his book. Briefly stated, Malthus argues that population pressure is a gift from a benevolent God to spur mankind's industry and inventiveness; without it, we would lapse into sloth, apathy, and brutishness. "Had population and food increased in the same ratio," he writes, "it is probable that man might never have emerged from the savage state" (1798:356). This conclusion sounds especially Boserupian when we realize that Malthus used the expression "savage state" as a synonym for hunting and gathering (see, for example, book 1, chap. 3 of Malthus 1803).

depress S_t or the excessive demands of greedy landlords were to lower per capita consumption. If, on the other hand, $\theta^* > \theta$, then the system will evolve spontaneously toward a state in which P_t is positive. In the real world, of course, both kinds of inducement may be operating simultaneously.

How does a positive value of P_t translate into increasing S_t? The relationship is not necessarily straightforward, except in an implausibly simple case. If the environment (land and resources) is fixed and the availability of innovations unlimited, we would expect $\dot{S}/S_t$ to be a monotonically increasing function of P_t. But in reality those preconditions for sustained Boserupian expansion are very unlikely to have held under preindustrial conditions. The ability of the system to display long-term Boserupian behavior is constrained by the sporadic appearance of technical innovations, by the slow rate of diffusion of such innovations, and by the variability of the environment. At best, Boserupian expansion under preindustrial conditions must have been episodic, unpredictable, and of limited duration.

FROM MALTHUS TO BOSERUP AND BACK

The relationship of the Malthusian present to the Boserupian future can be illustrated by borrowing a clever graphical device from Lee (1986*a*). The relationship between total production and population size can be considered entirely in the abstract, without reference to any particular system of production (fig. 7). We can imagine a set of points in the abstract space defined by those two variables, points that represent different combinations of production and population size that are all equally consistent with exact population replacement. To be precise, these are all the points at which average per capita output is just high enough that $w_t = \theta$ and hence $\partial N_t/\partial t = 0$. This set of points defines an isocline, the diagonal line in figure 7, marking the boundary between Malthusian plenty ($w_t > \theta$) and misery ($w_t < \theta$). Demographically, it also defines the boundary between conditions under which population size will increase (the entire space above the isocline) or decline (the space below the isocline).

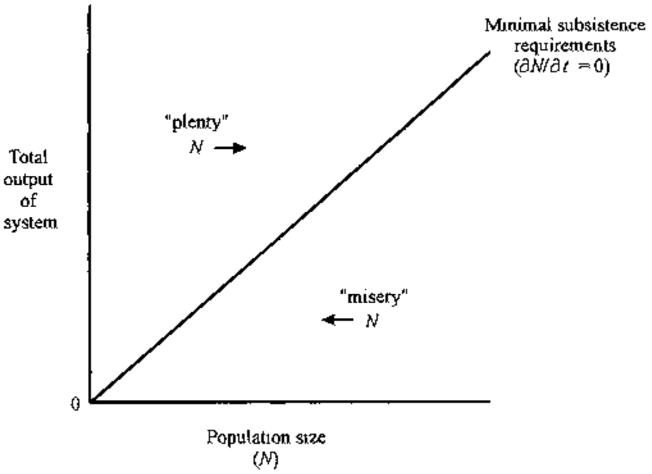

FIG. 7. *The state space of misery and plenty. The isocline* (diagonal line) *represents all possible combinations of total output and population size that yield a per capita output consistent with bare subsistence (i.e.,* $w_t = \theta$). *At bare subsistence, population size* (N) *does not change. All points above the isocline represent conditions that Malthus would describe as "plenty"—conditions under which the population is able to increase. All points below the isocline represent Malthusian "misery," under which population size is expected to decrease.*

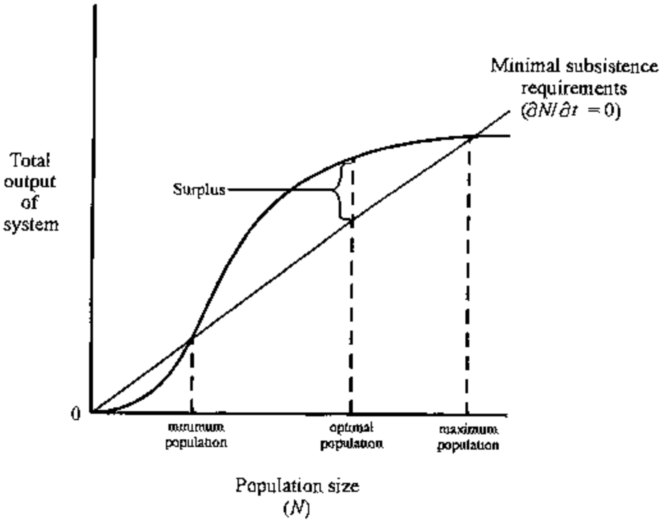

FIG. 8. *The preindustrial demographic system (after Lee 1986a). A specific production function has been imposed on the abstract state space of figure 7. Points at which the production function intersects the diagonal line (representing the subsistence isocline) are equilibria for population size. The equilibria at zero and at the maximum population are locally stable and that at the minimum population is unstable. The optimal population corresponds to the point at which the vertical distance between the production function and the subsistence isocline is at its maximum.*

Now let us impose a particular production function on this abstract space (fig. 8). To make things more interesting, we will use a function with a low-density inflection point. All segments of the curve above the isocline of demographic replacement represent combinations of production and population that will support further population growth under our system of production. Whenever the curve falls below the isocline, in contrast, population size must decrease. Every point at which the production function intersects the isocline represents an equilibrium state at which population growth is zero and w_t equals θ. Obviously, there are three such equilibrium population sizes: a trivial equilibrium at $N_t = 0$, a nonzero equilibrium associated with the low-density inflection point (the "minimum population"), and a higher equilibrium lying along the upper reaches of the production function (the "maximum population"). Are any of these equilibria stable? The answer is given by the angle of the production function as it crosses each equilibrium point (or, more precisely, by the second derivative of the production function at each equilibrium). The trivial equilibrium at zero is locally stable because the production function falls below the replacement isocline for positive values of N_t near the equilibrium. If a few people are added to the system—too few to reach the minimum population size—the system will just go extinct again. In real-world terms, there are not enough workers to maintain the system of production. By the same sort of reasoning, the minimum population size represents an unstable equilibrium: the production function is lower than the replacement isocline for values of N_t just below the minimum population point but higher than the isocline for N_t values above that point. Thus, if a population initially at rest at its minimum size is perturbed away from that point, it will rush off either toward extinction (for negative perturbations) or toward higher population sizes (for positive perturbations).

The so-called maximum population size in figure 8 is our one nontrivial stable equilibrium. If N_t is perturbed downward from that point, the system enters the region of "plenty" and the population grows back up toward the equilibrium. Similarly, if N_t is perturbed upwards, the system is forced into "misery" and the population drops back to the equilibrium. This, then, is our demographic saturation point, and at this point the population can be said to be regulated.

If we inspect the segment of the production function that falls between the minimum and maximum population sizes, we see a region in which the system is above the bare subsistence level—it is generating a surplus. In principle, there must be at least one point lying along this segment of the curve where the vertical distance between the production function and subsistence is at a maximum—that is, where the system is generating as large a surplus as it is capable of. It seems to me that this is the one point in our system that is worth calling an optimal population size, for things are as good there as they are ever going to get under this particular system of production. Sadly, the optimal population size is not an equilibrium (it does not lie along the isocline), and the population is expected to grow right past it on its way to the higher stable equilibrium. Just as Malthus predicted, population growth will consume any surplus generated by the economy and drive the system back down to the boundary between misery and plenty.

But is there a Boserupian escape from this Malthusian trap? There is, and it is diagrammed in figure 9. At demographic saturation for a given system of production, population pressure will encourage the members of the community to adopt any new system of production whose production function falls above the isocline at the current level of saturation. Such an innovation will have the immediate virtue of raising production into the realm of plenty—which is precisely why people will elect to adopt it. And, of course, because the new system is above the isocline, the population will immediately start to grow toward a new, higher demographic saturation point—whereupon average well-being will be driven right back down to its previous level. The Boserupian escape from the Malthusian trap is, by its nature, self-limiting. The only way to improve well-being further is to innovate yet again—a "solution" that can only be transitory.

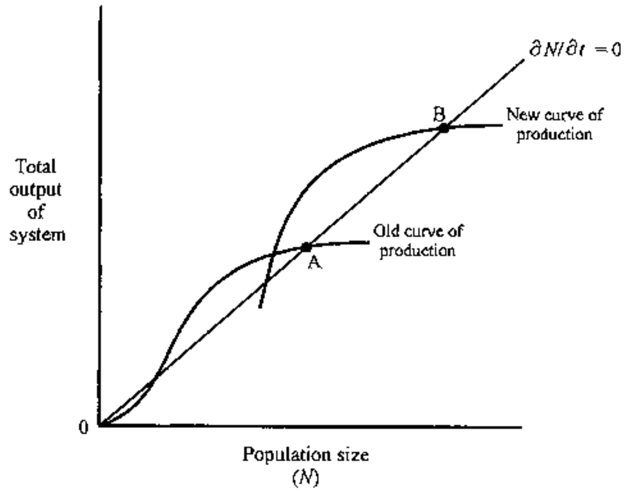

FIG. 9. *Changes in productivity associated with a shift in production techniques. At the original demographic saturation point* (A), *workers should be motivated to adopt any new technique that immediately raises total productivity* (new curve of production). *In time, however, the new system will reach a higher demographic saturation point* (B) *where per capita output and average well-being are exactly the same as they were at the previous level of saturation.*

Thus, we have a system in which economic change can win a temporary respite from marginal immiseration but which always returns (given enough time) to the same dubious state of well-being. And yet that state, wretched though it may be, itself creates the conditions for further economic growth. Thus, over long stretches of time, population and food production may leapfrog over each other, generating ever larger population sizes and more intensive systems of production but never gaining any permanent improvement in well-being.

THE MAB RATCHET

Le Roy Ladurie (1974) has used the phrase "Malthusian scissors" to describe the closing gap between growing population size and declining marginal productivity, and we have just spoken of a Boserupian escape from the Malthusian trap. At the risk of mixing metaphors, I would like to call the model of episodic population growth diagrammed in figure 9 the *MaB (Malthus-and-*

Boserup) ratchet. The MaB ratchet can, over time, jack up population size by a considerable amount. But like all ratchets, it cannot function indefinitely, and it tends to operate at widely varying speeds depending upon how often the handle is turned. The rate of MaB ratcheting is set by whatever factors limit the pace of economic innovation—the intensity of Boserupian pressure, the level of capital investment in production, the frequency of invention, and the spatial diffusion of new techniques. Each of these factors is influenced in turn by its ecological, economic, and cultural setting. During much of human history, the ratchet turned slowly when it turned at all. During the early stages of the modern industrial revolution, in contrast, it turned very rapidly indeed, at least until commercialization, capital accumulation, and contraception were far enough advanced to decouple economic innovation and population growth. A model of the MaB ratchet by itself may provide a mechanism for understanding certain forms of population growth, but it does not account for differences in the overall rate of such growth. The real virtue of the model is that it may help us to identify the additional factors that speed up, slow down, or even reverse the turning of the ratchet.

In fairness, I must confess that Malthus himself anticipated the MaB ratchet, as he did so much else. In all editions of his *Essay on the Principle of Population* he discusses an alternating process of equilibrium and growth similar to what I have described here, using terms like "oscillation" and "vibration" to refer to it. To quote one passage at length (1798:29–31),

> We will suppose the means of subsistence in any country just equal to the easy support of its inhabitants. The constant effort towards population, which is found to act even in the most vicious societies, increases the number of people before the means of subsistence are increased. The food therefore which before supported seven millions must now be divided among seven millions and a half or eight millions. The poor consequently must live much worse, and many of them be reduced to severe distress. The number of labourers also being above the proportion of the work in the market, the price of labour must tend toward a decrease, while the price of provisions would at the same time tend to rise. The labourer therefore must work harder to earn the same as he did before. During this season of distress, the discouragements to marriage, and the difficulty of rearing a family are so great that population is at a stand. In the mean time the cheapness of labour, the plenty of labourers, and the necessity of an increased industry amongst them, encourage cultivators to employ more labour upon their land, to turn up fresh soil, and to manure and improve more completely what is already in tillage, till ultimately the means of subsistence become in the same proportion to the population as at the period from which we set out. The situation of the labourer being then again tolerably comfortable, the restraints to population are in some degree loosened, and the same retrograde and progressive movements with respect to happiness are repeated.

If stripped of its references to markets and prices, this would be a pretty general description of the MaB ratchet.

One interesting implication of the MaB ratchet, again anticipated by Malthus, is that neither absolute population size nor population density is a very informative measure of population pressure. A low-density population with a nonintensive form of food production may be far more pressed for food than a high-density one with a complex system of production. As Malthus (1807:546) put it, "This unfavorable proportion [between population and food] has no necessary connection with the quantity of population which a country may contain. On the contrary, it is more frequently found in countries which are very thinly peopled than in those which are populous." The capacity to support a large population is built up gradually. Most low-density populations are at low density precisely because they have not (yet?) created that capacity. Consequently, population size or density is a poor measure of population pressure. This fact doubtless explains why cross-population comparisons so rarely find any associations between population density and interesting social phenomena. In a recent example, Keeley (1996:117–21) found no significant correlation between population density and the frequency of warfare. This negative result surprised him not at all (pp. 118–19):

> the type of population pressure that Malthus envisioned cannot be measured by simple density. . . . The quantity of food produced from a given piece of ground by farmers who possess the technology to deep plow, fertilize with chemicals or manure, and irrigate exceeds that produced by dibble-stick, long-fallow, dry farming. Primitive farmers experienced land shortages and famines at far lower population densities than do their modern counterparts. Because so many factors—latitude, rainfall, soils, forest cover, biodiversity, energy input, and general technology—must be considered, making comparisons on the basis of "equivalent" population densities is extremely difficult.

This was precisely Malthus's view.

The Variance in Well-Being in Malthusian Systems

From a health perspective, the principal conclusion of the previous section is that average physical well-being is expected to improve (though perhaps only slightly) during periods of Boserupian expansion, but given enough time it will always return to the same level of marginal immiseration, no matter what the system of production. Therefore we would expect to see occasional periods of ameliorating health, always followed by a return to marginal health. There is nothing in our model to justify the belief of Cohen and Armelagos (1984; Cohen 1989) that economic "progress" usually entails a deterioration in average well-being. According to our theory, there is no general reason to believe that agriculturalists were worse off than hunter-gatherers (to pick the comparison of most interest to Cohen and Armelagos). Indeed, during the early stages of agriculture, they were probably better off.

But these are all statements about average well-being, and they tell us nothing about changes in the *variance* of well-being. There are two compelling reasons to be concerned with variances. First, Goodman (1993) has suggested that, even if average health changes little with economic and social evolution, the variance in health tends to increase, so that a progressively larger fraction of the population falls into a state of abject misery. Material inequality, in this view, is an endogenous outcome of economic "progress" in the preindustrial world. This is an interesting idea, and it may even be correct. What does our model tell us about this possibility?

A second reason to be concerned with the variance in well-being is that it is closely related to the variance in what demographers call *frailty* (Vaupel, Manton, and Stallard 1979, Weiss 1990, Wood, Holman et al. 1992). Frailty is defined as an individual's relative risk of death compared with a standardized cohort risk; it is thus the obverse of the survival component of well-being. If that component of well-being is heterogeneous, then so is frailty. A large literature now exists in mathematical demography showing that heterogeneous frailty can have important and often unexpected effects on population dynamics (see, for example, Vaupel and Yashin 1985, Wood, Milner et al. 1992). Should we expect there to be much heterogeneity in frailty within most preindustrial populations?

To address these questions we need to model the *distribution* of well-being, including the way in which that distribution changes over time. The mean of the distribution ought to act just like w_t in the model sketched out above, but how will the variance behave?

There are several aspects of this problem that are deep and difficult. For example, while stochastic variation may tend to increase the variance in well-being over time, truncation selection is simultaneously acting on the lower tail of the distribution—if one's well-being is too low, one doesn't survive—and that selection will tend to lower the variance. What is the balance between these countervailing forces? An even thornier difficulty has to do with possible intergenerational correlations in well-being. Is the well-being of offspring correlated with that of their parents? Almost certainly, and not just for genetic reasons. (Children born to poor parents tend to grow up poor, even when the causes of poverty are entirely social.) Such intergenerational influences, combined with the effect of well-being on fertility and mortality, make for an extremely complicated dynamic over long stretches of time (see Rogers 1992).

In my opinion, the basic starting point for any attack on these problems is the realization that production, consumption, and reproduction in preindustrial societies are mostly segregated into households. Of course, households often exchange goods and pool labor and capital, but households are the primary units of landholding and production, and births and deaths happen within households. Variation in well-being among households must account for an overwhelmingly large fraction of the total variance in well-being in all preindustrial communities (which is not to say that there are not important differentials *within* households, for example, by age and sex). And intergenerational economic processes mainly play themselves out either within households or across closely related households, especially those of parents and their adult children.

The Russian economist Chayanov (1986) developed an entire theory of peasant economics based on households. While he has been justly criticized (e.g., by Smith 1984) for overemphasizing the autonomy of the peasant household, many of his ideas are still useful. One of his key insights was that changes in household size and dependency ratios arising from the inherent life cycle of the family could be a source of wealth differentials. Thus, even in the unlikely event that all households had identical access to land, they could still differ in well-being because of the demographic stochasticity that is always associated with the family-building process, a phenomenon that Chayanov called "demographic differentiation" (1986:254). Because our model does not incorporate age structure, it cannot tell us anything about the effects of changing household dependency ratios, but it can provide some insight into the more general effects of demographic stochasticity.

Imagine a village made up of a number of distinct households. Insofar as each household represents an autonomous unit of production, it can be regarded as a tiny population with its own economy, its own value of S_t (reflecting the amount and quality of the land it farms and its access to productive capital), its own value of N_t (which will change over time according to the household's birth and death rates), and its own average well-being. The variance in well-being across households can be approximated by expanding equation 2 as a Taylor series and discarding higher-order terms:

$$\mathrm{var}(w_t) = [\kappa \mathrm{E}(w_t)]^2 \times \left[\frac{\mathrm{var}(S_t)}{\mathrm{E}(S_t)^2} + \frac{\mathrm{var}(N_t)}{\mathrm{E}(N_t)^2} + 2\,\frac{\mathrm{cov}(S_t, N_t)}{\mathrm{E}(S_t)\mathrm{E}(N_t)}\right], \tag{14}$$

where the E's, var's, and cov's refer to household-level means, variances, and covariances, respectively. Ignoring demographic stochasticity for the moment and assuming that the population is at equilibrium so that N_t is locked on S_t, we can reduce this expression to

$$\mathrm{var}(w_t) = 2[\kappa\theta/\mathrm{E}(S_t)]^2\,[\mathrm{var}(S_t) + 1]. \tag{15}$$

Thus, at equilibrium, and in the absence of random fluctuations in births and deaths, the variance in well-being is proportional to a term reflecting the degree to which households differ in their productive systems (that is, in the factors exogenous to our model). At the same time, the variance is *inversely* related to the square of the mean value of demographic saturation. Perhaps, then, economic progress is not as evil as Goodman (1993) feared: it may not improve average well-being in the long run, but at least it tends to reduce wealth differentials among households. A final implication of equation 15 is that the variance in well-being shows no inherent tendency to change over time, as long as the system of production remains unchanged and demographic stochasticity is absent.

How, then, does demographic stochasticity alter this picture? One way to answer this question is to rewrite equations 3 and 4 as

$$b_t = \beta_0 + \beta_1 \ln w_t + \beta_2 d_t + \epsilon_{bt} \tag{16}$$

and

$$d_t = \delta_0 - \delta_1 \ln w_t + \delta_2 b_t + \epsilon_{dt}, \tag{17}$$

where the ϵ's are now random disturbance terms with mean zero and variance $\mathrm{var}(\epsilon_{bt})$ and $\mathrm{var}(\epsilon_{dt})$ respectively. Some thoroughly tedious calculus yields

$$\mathrm{var}(w_t) = \frac{[\kappa \mathrm{E}(w_t)]^2 t}{\mathrm{E}(N_t)} [\lambda_b \mathrm{var}(\epsilon_{bt}) + \lambda_d \mathrm{var}(\epsilon_{dt})] + \phi(S_t, N_t), \tag{18}$$

where

$$\lambda_b = \beta_2^2 - 2\delta_2 + \beta_2\delta_2 + 1, \tag{19}$$

$$\lambda_d = \delta_2^2 - 2\beta_2 + \beta_2\delta_2 + 1, \tag{20}$$

and

$$\phi(S_t, N_t) = [\kappa \mathrm{E}(w_t)]^2 \times \left[\frac{\mathrm{var}(S_t)}{\mathrm{E}(S_t)^2} + 2 \frac{\mathrm{cov}(S_t, N_t)}{\mathrm{E}(S_t)\mathrm{E}(N_t)} \right]. \tag{21}$$

To carry this exercise any farther would require us to write down exact specifications for $\mathrm{var}(\epsilon_{bt})$ and $\mathrm{var}(\epsilon_{dt})$, which seems premature, but even in its present form equation 18 has some interesting things to say. First, the term $\phi(S_t, N_t)$ captures variation in well-being among households attributable to any real differences in productive capacity. Thus, variation in the amount and quality of land, ownership of capital equipment, and other productive resources is lumped under this seemingly bland term. Such variation is always with us, but its magnitude varies according to local conditions. The rest of equation 18 is perhaps more informative about general patterns. On the righthand side of the equation, t appears in the numerator of the first term. This result tells us that the variance in well-being creeps upward over time: random disturbances tend to accumulate. In addition, the variance in well-being at any given moment is proportional to $\mathrm{var}(\epsilon_{bt})$ and $\mathrm{var}(\epsilon_{dt})$, which determine the magnitude of random fluctuations in the numbers of births and deaths, respectively, across households. Somewhat surprisingly, however, the constants of proportionality in this relationship are partly determined by the interactions of b_t and d_t, as reflected in the values of β_2 and δ_2. The interpretation of this finding would seem to be that stochastic variation in births and deaths causes fluctuations in household size and thus in well-being, but the linkages of fertility and mortality act to limit the propagation of those fluctuations. The system is thus buffered against random demographic shocks, though imperfectly so.

What else can we learn from equation 18? One thing we know about interfamily variation in births in preindustrial societies is that its magnitude tends to be positively correlated with the mean level of fertility (Wood 1994*a*:33–36). In a system of Malthusian population regulation involving preventive checks, then, $\mathrm{var}(\epsilon_{bt})$ may often increase during Boserupian expansion. In addition, the variance in w_t is always proportional to the square of the mean well-being across households, which in turn is positively associated with the population growth rate. For these reasons, periods of demographic growth will usually be characterized by widening variation in well-being, and demographic contraction will often have the opposite effect. There is, however, an inverse relationship between $\mathrm{var}(w_t)$ and $\mathrm{E}(N_t)$, which will moderate the effects of expansion and contraction.

What, then, can we conclude about variation in well-being in preindustrial populations? *If* households are independent units of production, then the bare fact of such variation should cause no surprise: it arises both from real differences in household productive capacity and from random fluctuations in births and deaths among households. The random component of the variation will tend to increase with time, limited principally by the extent to which birth and death rates interact. Periods of population growth—including those associated with economic expansion—will witness increasing economic differentials among households, although eventually the increase in population size will bring the variance in well-being back in line as density-dependent checks on birth and death come into play. Thus, it is by no means certain that economic change per se will lead inexorably to net increases in wealth differentials in the long run, although it will often do so in the short term.

Perhaps needless to say, households are never truly independent in their economic activities under preindustrial conditions. Informal markets in land and grain may be present, and land-poor peasants may sell their labor to wealthier neighbors. Moreover, complex systems of irrigation and crop rotation may require extensive coordination of production across households. These institutional arrangements must have varied effects, both positive and negative, on economic differentials within the preindustrial community. It is likely that the actual distribution of well-being in preindustrial societies is influenced *at least* as much by these institutional factors as by stochastic variation in household demography. Nonetheless, the household-level processes modeled here provide the inherent variability—the raw material, as it were—upon which these institutions operate.

The Most Important Exogenous Variables

I have already said that our model may be most useful in making plain what it leaves out. But even if our interests were confined to the model itself, there would still be an important reason to pay some attention to the variables exogenous to the model: it needs them if it is to display any kind of interesting long-term change. In the scary language of social statistics, our theory can be represented as a fully nonrecursive structural equation model (Berry 1984). To express it in a way that may be less opaque, the model contains feedback loops linking economic change, well-being, and population pressure such that no single factor is either wholly prior as a cause or wholly posterior as an effect. The problem with such a system is that it will eventually go to an equilibrium and stay there—unless perturbed by exogenous factors. As Konigsberg, Buikstra, and Bullington (1989: 634) put it, "Without any external input, a loop (or fully nonrecursive model) is the structural analog of a thermostat placed in a constant environment."

There are three major classes of exogenous variables that can potentially jump-start long-term demographic change in our model: (1) those that influence the production function and thus set the demographic saturation point (chiefly land, climate, and crop resources), (2) those that limit the pace of economic change (the timing and diffusion of innovations, including new crop resources and reorganizations of production), and (3) those that cause shocks directly to the demographic system (e.g., famines and epidemics). No doubt most of these variables *could* be made endogenous, if only our model were complicated enough. For example, the rate of innovation diffusion is probably influenced by population size and density. And we know that certain epidemic diseases, including smallpox and measles, are maintained endemically only if the population exceeds a critical threshold size (Anderson and May 1991:81–86). Similarly, climate change may sometimes be sparked by the economic activity of the human population. But it is not clear that the insights to be gained by formally incorporating such phenomena into our theory would offset the loss of clarity resulting from too complex a model. And we would still need other exogenous variables to keep the system moving. Let us keep these important factors exogenous, then, but grace them with a few passing remarks, if only to emphasize how much more work needs to be done on them.

Land and climate seem reasonably straightforward. The amount of available land plays the dominant role in population change during early stages of colonization, and it is a key factor in setting the ultimate demographic saturation point. Of course, land varies in quality as well as quantity—a point emphasized repeatedly by Malthus. As several economic historians have suggested, the extent to which marginal, low-quality land is brought under or released from cultivation may itself be an index of Malthusian population pressure (Kershaw 1973, Dyer 1989).

Thanks to recent work in paleoclimatology, we now know that climate is much more variable than we used to think (Crowley and North 1991, Lamb 1995, O'Brien et al. 1995). Temperature and rainfall undergo long-term changes over the course of centuries and short-term fluctuations from year to year. The long-term changes are arguably a leading cause of secular trends in S_t, while the short-term fluctuations partly determine the frequency and severity of harvest failures and famines (on famines in medieval and early modern Europe and their relationship to fluctuations in warmth and wetness, see Walter and Schofield 1989, Jordan 1996). Most demographers and economists dislike explanations involving climate change because it is the ultimate exogenous variable or, even worse, because it smacks of "environmental determinism." As a result, climate change is probably the most consistently underrated factor in demographic history.

Crop resources are also fairly straightforward as determinants of S_t—and here I mean "crops" in its broadest possible sense to cover both plant and animal, as well as wild and domesticated, food items. At any given time, a population has a characteristic mixture of foodstuffs, ranked by preference (luxury foods, everyday foods, poverty foods, foods fit to be eaten only in times of dire scarcity). The items on this menu partly reflect local ecology but also partly historical accident (Europeans could not have grown the potato before 1492). The precise balance of items, their ordering by preference, the labor and capital invested in their production, and the storage facilities dedicated to them can all vary widely over time and space. It is profoundly important to understand why this should be. To generalize the results of optimal foraging theory, which is beginning to provide important insights into the subsistence behavior of hunter-gatherers (Kelly 1995), we need an "optimal cropping theory" applicable to all preindustrial economies.

As Boserup (1981:129–32) has emphasized, the timing and diffusion of technical innovations were of great importance in determining long-term trends in S_t in the preindustrial world (see also Clark and Haswell 1967: 179–99). In fact, the primitive transport and communication systems typical of preindustrial times should be regarded as among the most important factors limiting the tempo and spatial extent of economic change over the course of human history. Innovation plays much the same role in our theory as mutation does in population genetics—though presumably innovations are more positively directed toward particular functional goals than are mutations, which occur whether they are useful or not. An interesting question is whether we can treat innovations, like mutations, as essentially random in the time and place of their occurrence, at least conditional on population pressure.

One generalization that I think can be made about economic innovations in the preindustrial world is that, taken one at a time, they usually entail only modest modifications in existing methods of production and thus cause small and short-lived improvements in well-being. Some of the changes may look dramatic in retrospect, but they are the accumulated outcome of many small, incremental improvements in technology, crops, and the organization of production. Preindustrial folk do not maintain Departments of Research and Development, and they understandably view large changes with suspicion. Thus, Boserupian expansion is slow, when it occurs at all, and must proceed by fits and starts.

What about exogenous demographic shocks such as famines and epidemics? Ideally the two types of shock should be considered together, since nutritional status is an important determinant of an individual's susceptibility to infection, but in these brief comments I shall keep them separate for simplicity's sake. It should be borne in mind, however, that the particular pattern of crisis mortality that we observe, say, in 17th-century England (Wrightson and Levine 1989) reflects both the main effects and the interactions of these two shocks.

Famine is known to be a recurrent threat in many developing countries (Curry and Hugo 1984, Arnold 1988). Is it likely to play an important role in the dynamics of preindustrial populations in general? In particular, is recurrent famine likely to be a major mechanism of population regulation? Like many other writers, Watkins and van de Walle (1983) have interpreted Malthus as believing that it is—that famine is always the ultimate positive check on population growth. But according to the simulation models of Watkins and Menken (1985), recurrent famines may slow population growth but are very unlikely to obliterate it altogether. This is so because famines are often followed by a compensatory period of rapid population growth, during which fertility reaches unusually high levels and mortality drops to some degree, owing to the loss of the most vulnerable segments of the population (Bongaarts and Cain 1982). As a result of this "catch-up" population growth, the long-term effect of famine on population size is usually very minor. Therefore, famine is unlikely to be a major mechanism of population regulation in its own right. But Malthus never really thought that it was. On the contrary, he believed that famine was the *result* of population's growing up to demographic saturation, not a *cause* of demographic equilibrium. After discussing epidemics, Malthus (1806:60–61) remarked,

> Of the other great scourge of mankind, famine, it may be observed, that it is not in the nature of things that the increase of population should absolutely produce one. This increase, though rapid, is necessarily gradual; and as the human frame cannot be supported, even for a very short time without food, it is evident that no more human beings can grow up than there is provision to maintain. But though the principle of population cannot absolutely produce a famine, it prepares the way for one in the most complete manner; and by obliging all the lower classes of people to subsist nearly on the smallest quantity of food that will support life, turns even a slight deficiency from the failure of the seasons into a severe dearth; and may be fairly said therefore, to be one of the principal causes of famine.

On the basis of the modeling done here, I think this statement is exactly correct. There is nothing intrinsic to the model that generates famine conditions, but the model does predict that populations should equilibrate at the edge of misery, leaving them famine-prone in the face of exogenous environmental fluctuations. Whether famine in fact occurs depends upon the variability of the local climate and the likelihood of attack by crop-destroying pests. Thus, not all preindustrial populations have equal experience of famine, but most of them can be considered more or less equally famine-prone whenever external conditions are unfavorable.

This leaves pestilence as an important exogenous shock. Infectious disease dynamics under preindustrial conditions are likely to be extraordinarily complex because of their dependence on population size and degree of subdivision and on rates of migration among subdivisions (see Mielke et al. 1984, Sattenspiel 1987, Duncan, Scott, and Duncan 1993). New diseases can be introduced, lost, and reintroduced, virtually at random. Many of the unpredictable mortality crises in early modern Europe must have reflected this complex relationship between disease transmission and population structure. And the complexity only increases when we allow for the synergism between susceptibility to infection and nutritional status. I regard the dynamics of infectious disease as one of the most difficult and fascinating aspects of preindustrial population dynamics.

One mainstay of Cohen's argument (1989) that hunter-gatherers had lower mortality than agriculturalists is his recognition, quite correct in itself, that hunter-gatherer populations were too small to maintain several important infectious diseases in an endemic state. At least some early agricultural settlements and nascent urban centers may have exceeded the critical population size needed to sustain transmission of such infections as smallpox, influenza, and measles. Thus, these settled communities presumably had causes of death that were altogether absent or only intermittently present among hunter-gatherers. How did these diseases affect the overall health and mortality of the agriculturalists? The obvious answer is that each new disease imposed an additional burden of sickness and thus increased the overall death rate, which is precisely what Cohen argues. But the obvious answer is not necessarily the correct one, especially under a Malthusian regime of population regulation. Various alternative possibilities are illustrated in figure 10. At demographic saturation, a new, independent competing cause of death can increase the equilibrium death rate by either a large amount or a small amount, have no effect on it, or even reduce it. Oddly enough, which of these alternatives is the most plausible depends on the relationship between

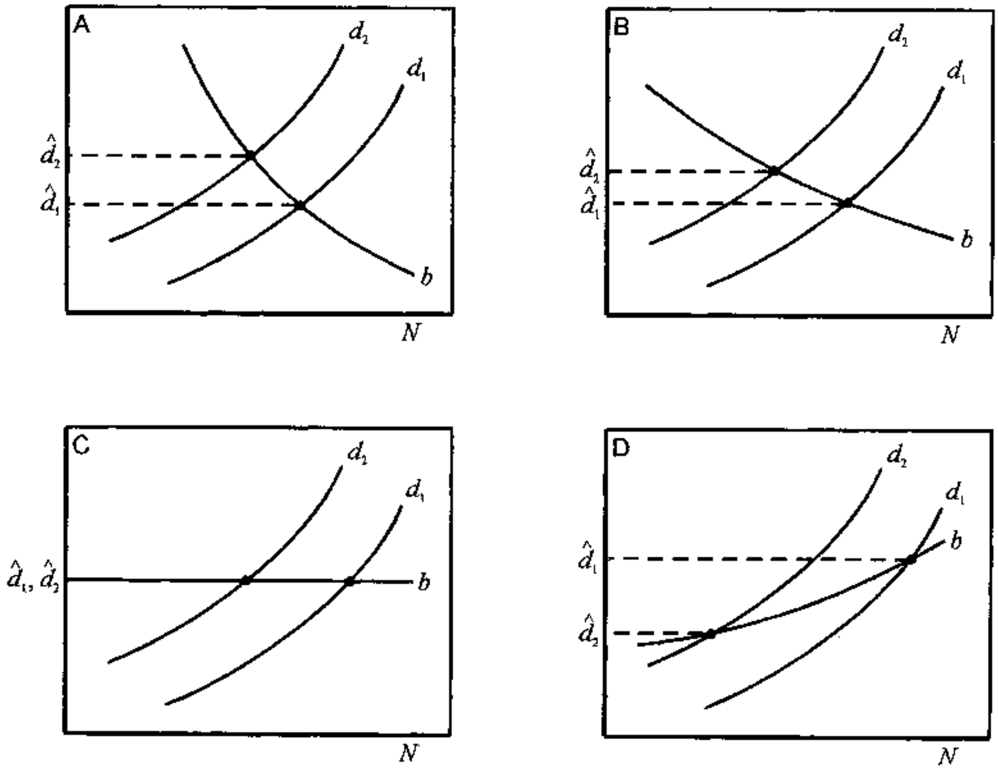

FIG. 10. *The epidemiological paradox in a Malthusian system of population regulation. Birth rates* (b) *and death rates* (d) *are plotted as functions of population size* (N). *At demographic saturation, the* b *and* d *curves intersect, and the death rate is at its stable equilibrium* ($\hat{d}$). *If a new, independent competing cause of death is introduced into the population, the death rate curve is shifted upward from* d_1 *to* d_2*. The effect of this change at equilibrium is, paradoxically, dependent upon the birth rate curve. The stronger the negative density-dependence of* b (A *versus* B), *the greater the shift in the level of mortality at equilibrium* ($\hat{d}_2$ *versus* $\hat{d}_1$). *If* b *is independent of population size* (C), *the additional cause of death has no impact whatsoever on the equilibrium death rate. Finally, if* b *increases with* N *but at a rate low enough to allow population regulation, then adding one more cause of death will actually* decrease *the equilibrium death rate* (D).

population size and *fertility*, not just mortality. This result is so counterintuitive that it amounts to a kind of "epidemiological paradox." Because of this paradox, our expectations about the effect of any new disease on mortality patterns must be conditioned by what we think is happening to fertility at the same time and by whether we believe the population is at demographic saturation. Thus, the universal truth of Cohen's argument about the effects of agriculture on mortality rates may not be as incontestable as it first appears.

Discussion

Robert Owen, the 19th-century utopian political philosopher, was the Julian Simon of his day, firm in his Whiggish belief that a proper economic system should be capable of infinite expansion to meet the needs of a rising human population (cf. Simon 1990). He was appalled by Malthus's conclusions, as he no doubt would have been by mine. But for Owen it was not so much that Malthus was wrong as that he was no longer relevant. In a letter to the *Morning Chronicle* of October 8, 1827, Owen sought to identify the source of his disagreement with Malthus:

> The cause of this difference of opinion between individuals conscientiously desirous of discovering the truth, I have endeavoured to ascertain. It seems to me that Mr. Malthus views man in his former state, unaided by science and knowledge of himself; while I draw my conclusions from man as he now is, overwhelmed with artificial means of production, derived from scientific improvements, still capable of unlimited increase, and also in possession of a knowledge of his nature, derived from an accurate attention to facts, by which he has become competent to train his progeny to attain a very superior character, so as indeed to become beings of a higher order.

Even if few modern demographers subscribe to Owen's overheated vision of the human prospect, many seem to share his opinion that Malthus is more relevant to the preindustrial past than to the industrial present (e.g., Petersen 1979; Wrigley 1983, 1986).

Current anthropological thought seems to run mostly in the opposite direction: while the modern economic system generates more than its fair share of Malthusian misery, simple economies, especially those of hunter-gatherers, are "the original affluent society" (Sahlins 1972:39; Cohen 1989:3). If Robert Owen was Whiggish, most present-day anthropologists seem downright Rousseauian.

I leave it to others to decide whether Owen was right in regarding the Malthusian model as passé as far as industrial society is concerned.[10] But what of preindustrial societies? What conclusions can be drawn about them from my own version of the Malthusian model? The best way to answer this question is to return to the five issues raised near the beginning of the paper and see what the model has to say about them. To recapitulate:

1. Is the growth of preindustrial populations "regulated" in any meaningful sense of the word? I have defined regulation in terms of a population's tendency to move toward a stable equilibrium size. This definition immediately implies the necessary and sufficient condition for regulation to occur: vital rates must be density-dependent near the equilibrium size. A major argument of this paper is that certain fundamental features of the preindustrial economy, especially the declining marginal productivity of labor and the physiological allocation problem, should cause this condition to be met whenever the nonlabor factors of production are unchanging. Thus, population regulation may indeed be common in the preindustrial world, just as many cultural ecologists have argued. However, I cannot emphasize too strongly that the view of population regulation developed in this paper is quite different from that of the old-school cultural ecologists. Population regulation as I define it is not *self*-regulation—it does not involve special adaptive mechanisms whose function is to restrain population growth. On the contrary, it rests upon the ultimate inability of individuals and economies to overcome the inherent finitude of resources. Population regulation is something imposed on the population, not an evolved feature of it.

2. Is there an optimal population size, and do preindustrial populations tend to equilibrate at the optimum? The definition of optimal population size adopted here is an economic one: it is the size of the population that produces the largest possible surplus for a given production function. This size is optimal because the per capita output of that particular system of production is as large as it ever can be. Alas, our model tells us that the optimum so defined has no special place in preindustrial population dynamics: if the population attains it, it will grow right past it until per capita output is driven back down to the subsistence level. Of course, it may be possible to design a political system to *coerce* a surplus, but the important point is that it has to be coerced—the system of production will not spontaneously produce one, at least not when it is at equilibrium.

There is another meaning to optimal population size that is often encountered in the literature on cultural ecology. This is the maximum size consistent with the long-term maintenance of the population's resource base in a nondegraded condition—the size that just permits the population to continue living indefinitely off its "interest," as it were, without dipping into its "principal." Although our model has little to say that is of direct relevance to this particular definition of optimal population size, I am far from convinced that the concept is of much use in understanding preindustrial population dynamics. It is perhaps imaginable that economic strategies for coping with environmental risk and uncertainty act to protect resources over the short run. But it is difficult to believe that any population deals effectively with what happens over the longer run. People, especially people without written records, tend to be short-termers both retrospectively and prospectively. Retrospectively, knowledge of past economic and environmental trends is likely to extend back no more than two or three generations in the absence of written records, and even this knowledge will probably be confounded by the many tricks that human memory can play. Prospectively, all is uncertainty. Even today, demographers and economists armed with sophisticated forecasting models and long time-series of data are unable to project current trends reliably over more than a decade or so (Casti 1990). Do we really believe that preindustrial folk could do significantly better?

Besides, to quote a famous remark about human economic behavior attributed to John Maynard Keynes, "In the long run we're all dead." Why should any sensible preindustrial person particularly *care* about the long-run availability of resources? A sociobiologist keen on kin selection might reply, "Because the future is where his or her descendants will live." But after only a few generations, that person's degree of biological relatedness to those descendants will be virtually nil.[11] Preindustrial people, like the rest of us, care passionately about today and tomorrow and next year—and perhaps what will become of them in their old age—but few of

---

[10] My own view is that Malthus is still relevant. If industrial societies have achieved affluence, it is only because they have followed his recommendation to restrain fertility, allowing what he called the tortoise of food production to catch up with the hare of population growth. Never mind that Malthus would be scandalized to learn that fertility had been reined in by contraception rather than "moral restraint"—by which he meant late marriage and complete abstinence outside of marriage. And never mind that the tortoise of food production has been turbo-charged with new agricultural methods that Malthus could never have dreamt of. The fact remains that the modern world, whether it realizes it or not, has grown rich by following Malthus's advice.

[11] Ignoring inbreeding, an individual's relatedness to any direct lineal descendant, to whom he or she is more closely related than to anyone else in the future, decreases as 0.5^k over k generations. Thus, after only five generations (or about 125 years), one will share no more than about 3% of one's genes with any single living individual. The propagation of genes is so powerfully dissipative that there is no evolutionary advantage to worrying about the long run.

them care much about the next century (which is too bad, since many demographic, economic, and ecological trends play themselves out over a century or more). It is hard to believe, then, that preindustrial populations routinely adopt behaviors to ensure the long-term sustainability of resources.

3. What is the relationship between population growth and economic change? It is often claimed that Malthus and Boserup have diametrically opposed views of the relationship between population growth and economic change, Malthus claiming that population growth leads to economic stagnation and a decline in the standard of living and Boserup that population growth induces economic advancement and higher standards of living. Following Pryor and Maurer (1982) and Lee (1986*a*), I have tried to show that Malthus and Boserup can be combined in a single model built upon a few simple and perhaps not unreasonable premises. And, in fact, a careful reading of Malthus reveals that he himself had something very close to my Malthus-and-Boserup ratchet in mind in his discussions of agricultural intensification. Boserup, then, is more of a Malthusian than the anti-Malthusian she has often claimed to be. What the MaB ratchet tells us is that population growth can indeed lead to economic stagnation and its attendant population pressure but also that that pressure itself can induce further economic change and a temporary amelioration of conditions for the average individual. One implication of the model is that, when environmental conditions allow, human populations can undergo long-term growth that is nonconstant (subexponential) and episodic in nature but over long periods can add up to a substantial increase in population size. The MaB ratchet thus removes a major criticism of Boserup's model, namely, that she treats population growth as an exogenous variable. Malthus *plus* Boserup gives us a more complete theory than either standing alone.

4. What are the implications of population growth and economic change for individual health and well-being? The model tells us that both the mean and the variance of well-being tend to increase temporarily as the MaB ratchet increases the size of the population and the intensity of production but also that the population then tends toward a state in which the mean is at the margin of misery and the variance is held within strict bounds by the interaction of fertility and mortality. In other words, the long-run effect of population growth and economic change on the distribution of well-being is effectively zero. Thus, the model supports neither the Rousseauian vision that health used to be much better when technology and political forms were simpler nor the Whiggish view that the human condition is always and everywhere improving thanks to economic progress. All preindustrial economies, no matter how simple or complex, are capable of generating misery and will do so if given enough time. Innovations in production are expected to ameliorate that misery in the short run—else why would they be adopted?—but can gain no permanent improvement in well-being. (Note, however, that the model tells us nothing about how long the "short run" is likely to be.) On balance, most hunter-gatherers were probably just as miserable as most agriculturalists. In this light, recent attempts to find systematic differences in vital rates between hunter-gatherers and other preindustrial societies (e.g., Hewlett 1991, Bentley, Goldberg, and Jasienska 1993) would seem to have little grounding in theory.

5. What is the role of crisis mortality in preindustrial population dynamics? The model has important implications for understanding how mortality crises work under preindustrial conditions—or at least how famines are likely to contribute to such crises. (In its present form, the model speaks much less clearly to the role of epidemics.) On the basis of the model, I have argued that preindustrial populations are likely to be famine-prone so long as they are at or near demographic saturation. What I mean by that is that a large segment of the population will be near the margin of misery, so that any fluctuation in productive output will force them, however temporarily, to fall below the subsistence level. Whether or not a famine actually occurs in any period or place depends, of course, on the variability of the environment. But if enough environmental variability does exist, famine is likely to occur. To put it another way, preindustrial populations are not expected to evolve spontaneously to a state in which they are well buffered against environmental perturbations. If such buffering exists, we need to look for special mechanisms designed to provide it. In this view, famine is likely to be a common feature of preindustrial economies, at least where environmental factors predispose to it. Thus, the mortality crises we are able to observe in late medieval and early modern Europe may not be confined to those periods.

What we have, then, is a model that is the very opposite of the old "original affluent society" vision of simple economies. Of course, the model is just a model and is only as good as its assumptions. But at least it has the virtue of parading those assumptions for all to see and following them wherever they may lead, even if we end up with a view of preindustrial society that is less than idyllic. Now, a model can be deemed successful if it meets two out of three basic criteria: simplicity, generality, and realism. Unfortunately, these are competing demands, and no model can satisfy all three criteria at once. I have deliberately sought simplicity and generality at the expense of detailed fidelity to any particular case. As a result, the model has a strong air of "all things being equal" about it—but, then, so do all general models. I do not doubt that innumerable factors, social, cultural, and political, intervene in many real-world situations to obscure the tendency toward marginal immiseration and proneness to calamity predicted by my model. But that should not be counted against it. If I have managed to identify a basic tendency of all, or even most, preindustrial populations, I will be satisfied, no matter how attenuated that tendency may sometimes be in what Malthus called the world of experience.

Comments

GEORGE L. COWGILL
Department of Anthropology, Arizona State University, Tempe, Ariz. 85287-2402, U.S.A. (cowgill@asu.edu). 5 IX 97

Wood's article has many merits, including an unusually clear and well-informed exposition of key ideas of T. R. Malthus and Ester Boserup. I hope it will do much to dispel misconceptions that are far too prevalent. Yet this clarity itself makes it easier to see critical conceptual shortcomings in Malthus and Boserup, shortcomings that Wood largely shares.

The article has significance far beyond demography, for it proposes a general theory of sociocultural change. The math may daunt some readers, but the conclusions are pretty straightforward consequences of the initial assumptions, and what matters is the adequacy and relevance of these assumptions. Readers also may be put off by the flagrantly nonpostmodern style, but we shouldn't judge an article too much by its language; to do so is to participate in the ongoing tragicomedy in which anthropology (as if we didn't face enough external hazards) energetically and repeatedly shoots itself in the foot.

All that said, my *fundamental* disagreement is that Wood regards the political economies of societies as complicating variables that can eventually be incorporated in his model but for the present are best set aside in favor of a simpler model that focuses on supposedly more basic variables. For me, nothing is more basic than political-economic aspects of societies, and models that neglect them are as fatally flawed as would be models of climate that left out precipitation. Wood says that his lack of attention to cultural, institutional, and political factors may be a serious mistake. I agree.

More specifically, Wood simply does not envision differently situated social actors perceiving themselves as having different (and often conflicting) interests and behaving differently in a given overall social situation. He speaks of variance around average values but fails to see variance as both cause and consequence of different interests. He has a good discussion of "carrying capacity," replacing it with the concept of "demographic saturation," but his concept of "well-being" is radically impoverished and is an obstacle rather than an aid to productive thought. There seems to be an assumption that most people will have as high fertility as their "well-being" permits. This assumption is increasingly belied by the kind of anthropological insight that is absent in the thought of most economic demographers and many sociological demographers but is being supplied by a few anthropologists such as the contributors to Greenhalgh (1995) and to Kertzer and Fricke (1997). An outstanding example is Bledsoe and Banja's discussion (n.d.) of a case in which women tend to regulate their fertility in order to maintain their health and their potential future fecundity as a key asset in their economically important relations with male partners. I hope that Wood will begin to take much more account of such knowledge, for it is indispensable to an adequate understanding of human population dynamics.

Although Wood's neglect of nonbiological factors is latent throughout, it becomes blatant in n. 10, where he asserts that "if industrial societies have achieved affluence, it is only because they have followed [Malthus's] recommendation to restrain fertility." Even if imperialism and colonialism have perhaps not been as entirely bad as they are sometimes represented, Wood's implication that they are not relevant at all is shocking and strains my recommendation that we not be so put off as to dismiss his article altogether. It shows how obtuse his neglect of political economy can be. It is also a nice example of how he sees "societies" globally following or not following recommendations rather than individuals or segments within societies as the entities that act.

Wood emphasizes *production* but tends to neglect *distribution,* and this is where political-economic institutions are critical. He often lumps all "preindustrial" societies together and moves back and forth confusingly between hunter-gatherers and agrarian societies. Other categories, such as "complex" hunter-gatherers, are not mentioned. This is critical because individual families (or households) in less "complex" hunter-gatherer societies are probably significantly less important units of production and allocation than Wood assumes; there seems to be a great deal of sharing among households. In more complex hunter-gatherer societies, and especially in agrarian societies, social surpluses tend to be extracted from households that have little power, and these changes in allocation practices have much to do with variance in well-being and misery. These differences, in turn, are likely to have a significant effect on fertility (Cowgill 1996).

Finally, Wood finesses the delicate point of *actually testing* his model by scarcely raising the possibility and putting emphasis on his model's implied answers to five broad questions. It is fair enough to propose a model without providing much evidence that is convincing except to the already convinced, but it is also fair for the rest of us to think about how his model might be tested. His dismissal of attempts to find systematic differences in vital rates between hunter-gatherers and other preindustrial societies because they "have little grounding in theory" is hard to understand. Perhaps he means that these efforts seem more empirical than theory-driven. Even if so, finding such differences might cause problems for aspects of his model, and that, in itself, is justification enough for advocates as well as skeptics to pursue this line of inquiry. Another obvious line is further testing of the Cohen and Armelagos thesis that average well-being tends to be lower in agrarian societies than among hunter-gatherers. Doubtless other testable implications can be derived from Wood's model, and this seems the way to proceed. In spite of my doubts about his model, it will prove valuable if it leads to productive research.

ROBERT E. DEWAR
Department of Anthropology, University of Connecticut, U-176, Storrs, Conn. 06269, U.S.A. (dewar@uconnvm.uconn.edu). 8 IX 97

Wood's model, simple though it may be, captures the important aspects of the relationship between population regulation and environmental productivity. Yet I think that it may be less general than he hopes, for it is based upon an implicit view of populations that seems implausible, particularly insofar as the models developed here avoid "the messy effects of migration."

In interpreting Malthus, Wood seems to read "population" where Malthus wrote "country." Now, Malthus almost certainly had in mind a nation, with (1) more or less fixed and definite borders, enclosing and delimiting both its natural resources and its human population, and (2) a continuing, historically based identity. In such a context it is reasonable to define an instantaneous demographic saturation point based upon a particular country's land area, resource density, technology, and organization of production, as Wood does. In the short run these factors can be taken as relatively fixed, to the extent that the national borders are fixed. It is equally reasonable to model population change within a country as solely the product of birth and death rates, since national borders sharply limited the movement of people across them. If all migration were internal to a country, the gains and losses of the various regions would simply cancel each other out, and for simplicity's sake migration could be disregarded. But do most anthropological "populations" have these characteristics? I think not; in the medium term, migration is likely to be sufficiently important to play a very significant role in balancing food supply and population density.

There is a problem in defining "population." Most anthropologists take ethnic groups as defining preindustrial populations, but recent work in ethnogenesis (see Moore 1994 for a review) has shown that frequently, perhaps usually, ethnic group identity is capable of rapid change and is very much historically situated. Ethnic groups merge and divide, and from century to century the number and identity of groups can change quite dramatically. Thus, it will frequently be the case that before a "population" can achieve a stable age distribution and a growth rate of zero, it has disappeared as an entity. Although the causes of ethnogenesis are certainly complex and poorly understood, it seems likely that one major factor is simply a feature of population size—below a certain number, a human population will be incapable of reproducing itself continuously and independently of its neighbors. Populations near the lower limit will persist only insofar as they can attract mates from elsewhere who choose to remain as immigrants.

Cohen (1977) called attention to the importance of interpopulation migration (see also Dewar 1984), and the argument can be rephrased in terms of Wood's model. Imagine a bounded region divided into a series of populations where population boundaries are not impermeable to mating and marriage. At any given point in time, each population has a level of well-being (w) determined by the ratio of its population to its instantaneous demographic saturation point. Whenever there is a marriage of members of different populations, the new spouses must choose a place of residence. It seems plausible that they will usually choose to join the population with the highest average level of well-being. Once the choice is made, the average level of well-being in the population they choose to join will decline and that of the population left by the departing spouse will increase. Just as important, to the extent that births usually quickly follow marriage, there is a shift of future births from the less to the more well-off. Over time, this kind of interpopulation flow will tend to average out differences in the well-being of neighboring populations. If entire families can shift from one population to another, this process will be accelerated. The extent to which average well-being predicts future birth and death rates is, thus, dependent upon interpopulation migration rates. An isolated region, for example, a continent, may have a population regulated largely by the balance of births and deaths, but in less closed populations the effects of migration may be equally important in establishing population levels.

Ecologists have recently become interested in situations in which more or less independent populations, each regulated in a density-dependent fashion, exchange migrants, usually labeled metapopulation ecology (see Hanski 1996). While such models are often extremely difficult to handle mathematically, some interesting features of spatial heterogeneity have been explored (see Pulliam 1996). When migration rates are relatively high, some populations may equilibrate with an excess of births to deaths by continually sending forth emigrants. Other populations may maintain an equilibrial size in the face of a permanent deficit of births to deaths because of a constant inward flow. In these latter cases, w will be consistently below 0, implying, following Wood's analysis, a positive pressure towards innovation. This recalls Binford's (1968) model for agricultural origins in peripheral zones, in which immigration produced population pressure, leading to intensification.

It is, as Wood notes, a real strength of his model that it invites attention to other important aspects of human population regulation, and he deserves our thanks for a very productive reformulation of an important and difficult problem.

NANCY HOWELL
Department of Sociology, University of Toronto, Toronto, Ont., Canada M5R 1P9 (howell@chass.utoronto.ca). 12 VII 97

I applaud Wood's thoughtful attempt to formulate a theory of preindustrial population dynamics. He incorporates the two most widely accepted theoretical frameworks (by Malthus and Boserup) and tries to integrate into that framework the formulations of optimum population size, life-history analysis, and economic development theory. Building upon the stimulating analyses of the economist-demographer Ronald Demos Lee, who has developed an integration of Malthus and Boserup with some unexpected payoffs, Wood brings new clarity and new integration of concepts to the debates of anthropological demography.

I agree with Wood when he says that the debates of anthropological demography have focused too much upon empirical work. There is nothing inherently difficult about carrying out censuses and reproductive interviews in remote and exotic populations. The difficult part is to interpret the data collected. Is the demographic behavior seen in a small population at some particular point in time a meaningful indication of the adaptive strategy of those people in their particular ecological niche? Or is it a product of recent contact and stress from the outside world, or anomalous "noise" generated by relatively rare events in a small population, or even a product of the process of study itself, which may bring antibiotics and new foods along with the laptop computers and census forms? These kinds of questions are strikingly illustrated for me by the recently published, brilliant work on the population of the Ache by Hill and Hurtado (1996). The facts that they report are often startling, their analysis is detailed and rigorous, and their grounding in life-history theory leads to some wonderful questions and answers in their highly readable book. But what can we make of these indicators of the demographic functioning of hunter-gatherer/foragers in their native habitat over the long term?

At the same time, Wood may not sufficiently stress that theory constructed without a close grounding in empirical study of particular populations is likely to be sterile and untestable. Some of the earlier ambitious attempts to formulate a theory of the evolution of population regulation have fallen because of lack of grounding in empirically observed populations. Wood's own work with the Gainj of highland New Guinea is clearly evident in the potential for operationalization of the concepts that he elaborates, even though he does not talk about that population in detail here.

Wood points out that anthropology is particularly focused on the regulation of population and its consequences for economic development in what he calls "preindustrial" societies (and operationalizes as nonmonetized economies). The building of general theory in demography has been complicated by the many ways in which industrialized populations are different from the nonmonetized populations he is focusing on. Clearly some linkages in the Malthusian diagrams of cause and effect have been disconnected in modern industrial societies, but the relationship of variables seems to be operative in hunter-gatherer, horticultural, fishing, herding, and peasant-agricultural societies, and it is here that the theory that Wood is propounding is likely to be most helpful.

Similarly, the linkages between population dynamics and other biological responses to the conditions of life by the population are likely to be invisible in industrialized countries while being highly illuminating if studied in a range of preindustrialized populations. For example, Wood points out that a given amount of food can be allocated to (1) physiological maintenance, (2) somatic growth, (3) reproduction, or (4) excretion. In industrialized populations, even the poorest of the poor are likely to find that food is not their limiting resource, so we observe tall, strapping, indeed sometimes obese poor people, whose fertility rate is likely to be higher than that of the prosperous classes in the same society (but whose mortality rate is likely to be far higher than that of the wealthy, even if it is far, far lower than everyone's mortality rate in preindustrialized societies). In preindustrialized societies, in which access to food for self and offspring is likely to be highly correlated with the amount of energy-consuming work performed by adults, body size can be meaningfully conceived of as an alternative to reproduction, and variables such as age at menarche, the duration of adolescent subfecundity, and the length of birth intervals can be seen as functions of the balance between food intake and energy output. Similarly, the caloric costs of infection, parasitic disease, and other forms of suboptimal health of parents and children can be seen as having a determinant cost in reproductive ability forgone. Darwin (based on Malthus) provides the framework within which to evaluate the differential reproductive success of individuals and families in preindustrial population conditions. Wood has reopened the debates on anthropological demography in a promising way, leading us back to a new and more sympathetic rereading of Malthus and to an empirically based evaluation of Ron Lee's formulation of the field.

The concept of "well-being" is proposed by Wood as a means of clarifying the consequences of interactions of demographic variables as they affect populations. It remains to be seen whether the concept is entirely successful in organizing the complexities of demographic growth, economic development, and social change that are implied in the model that Wood is proposing. At least the concept and the model are helpful in clarifying the differences and integration of the Malthus insights with the Boserup observations, and this article may be helpful in teaching demographic anthropology for a long time to come.

LYLE W. KONIGSBERG
Department of Anthropology, University of Tennessee, Knoxville, Tenn. 37996, U.S.A. 26 VIII 97

A few weeks ago I had a phone call from a journalist writing an article for the *Smithsonian.* The subject was something along the lines of the effects of the agricultural revolution on health, and I was very uncomfortable (in a state of "low well-being?") during the conversation, as I pictured myself caught in quotes that could only reveal my ignorance and naiveté on the subject. My conversation was peppered with suggestions that the journalist "talk to Jim Wood," with the unmuttered subtext "He is the one responsible for this mess." But when I got off the phone I was struck by two more charitable points: first, that I finally had a lead-in for this comment on Wood's insightful article and, second, that instead of being responsible for getting us into this mess he was in part responsible for pointing it out (Wood et al. 1992*b*), and has now begun to extricate us from it.

This leads me to ask how it was that we found ourselves in such ill-defined and murky methodological waters by the late 1980s. I think the answer is that we have fallen prey to slipshod use of definitions in describing the past. Discussions of paleodemography and paleopathology are rife with concepts such as "stress," "health," "disease," and "quality of life" but very short on useful operational definitions for many of them. What I find particularly elegant about Wood's current contribution is the way he replaces our old semantic baggage with concise, useful, and precise terms and definitions. In place of "quality of life" (a term that rankles more politically correct tendencies from the 1980s and 1990s) we have "well-being." Wood's "well-being" is simply the set of conditions that determines an individual's net reproductive rate; he replaces the vague "quality of life" with what might be called "quantity of life." He shows similar clarity in dealing with such difficult issues as "population regulation," and so I will not further catalog his contributions to the lexicon here.

From Wood's definition of well-being his argument flows to the logical conclusion that we are neither better off nor worse off (from a demographic point of view) for having "suffered" through the agricultural revolution. He shows that there is no end gain to cultural or technological revolutions, only the search for what might be called ever higher ruts. There are many parallels here to similar concepts from evolutionary theory, but it is to Wood's credit that he does not obfuscate his message with analogies. I will not show the same restraint. Evolution by natural selection cannot lead to lower average relative fitness, and similarly it has always been difficult for me to see how agriculture (if it truly leads to "poor quality of life" relative to hunter-gatherer subsistence) could flourish in so many parts of the world. Further, if evolution leads to optimization given the current set of environmental and genetic conditions, then we would expect that during periods of equilibrium genetic variance would be low. This is also the case for Wood's modeling of well-being, where the variance of well-being is at a minimum most of the time.

As I am critical of traditional paleopathological and paleodemographic approaches to the issue of the agricultural revolution, I think it important to close with a brief contrast between Wood's theoretical developments and previous praxis. Much of the paleopathological literature on the health implications of agriculture focuses on the concept of "stress." There is a succinct definition for this term in Selye (1976), but we should note that this book is dedicated to a discussion of stress and its effects (and treatment) within the individual. Because Wood's focus is on both the individual and the aggregate, he is able to sketch a much more complete model for the interrelationships between agriculture, population size, and "health" (well, "well-being" at least). This is a substantial improvement over the simplistic notions used in paleopathology and paleodemography, where most theoretical statements can be encapsulated in the phrase "Sick people don't tend to live as long."

Finally, I did find one apparent slight logical inconsistency in Wood's discussion of his figures 1 and 10. In an e-mail exchange he clarified this point for me. As I am approaching the word limit for this comment, I leave to him the task of clarifying the discrepancy in his reply.

J. H. LITTLETON AND R. D. ATTENBOROUGH
Department of Archaeology and Anthropology, Australian National University, Canberra, A.C.T. 2681, Australia. 30 IX 97

We are delighted to see a return to problems of preindustrial population dynamics, which have too often seemed either solved or insoluble; and we welcome many aspects of Wood's stimulating treatment of them, including its marriage of Malthusian with Boserupian elements and the attention it pays to, for example, household and group composition, physiological ecology, and palaeoclimate, amongst the relevant factors. Wood works on an extremely broad canvas, but graciously acknowledges the imprecision and omission that this involves. We make no comment here on mathematical or historical aspects but concentrate on what seem to us debatable points, amongst many others which we applaud, concerning population processes.

Like Humpty Dumpty, Wood is entitled to set up his definitions and stick to them; but when a key term is already in use with a different meaning this may invite misreadings. This is surely true of "well-being," whose ordinary usage, built on by theorists such as Boyden (e.g. 1980), is notable for freeing the user from any narrowly Darwinian implication that the only significant dimensions of health are those that affect fertility or mortality. Wood reimprisons the user in precisely this implication. The difference matters for questions of population health, even if not for demography *sensu stricto*. At one point, at least, it matters for Wood's own argument; when he argues against Cohen and Armelagos on the impact of "progress" on well-being he surely conflates his sense of the term, based only on vital rates, with one closer to the standard meaning (cf. Goodman 1993).

Wood adopts an engaging style of argument, seemingly axiomatic at the start but by the end deriving conclusions which are non-obvious (e.g. the instability of optimum population) and in some cases open to challenge. At what points do the difficulties enter? Wood does not hide the fact that he makes assumptions (e.g., the discussion he develops from fig. 8), but an issue for future development of his model will surely be how robust his general conclusions prove under variations of those assumptions. Further analytical modelling and empirical testing might both contribute to tackling this. The utility of the model will depend on, inter alia, the feasibility of operationalizing the concepts for empirical use; one might, for example, estimate "demographic saturation point" independently of actual population size and examine their relationship. If population sizes are observed to lie well below saturation point or to vary without systematic relation to it, as regional specialists have sometimes thought when discussing other measures of the population-environment relationship (e.g., Lizot 1977, Harris 1978), then one might question the abstract and seemingly fertility-control-free automatism by which Malthus, Boserup, and Wood apparently expect people to increase their "well-being." Wood himself draws attention to potential challenges to his theory; the resolution must presumably be evidential.

Preindustrial (or, surely better, nonindustrial) populations constitute a large, negatively defined, and heterogeneous category, covering a wide diversity of cultural milieux, ways of life, and environments. Doubtless it would have been hard to venture more specificity than Wood does without narrowing the range of populations covered. Furthermore, Howell's warning still applies to demographic research in societies classically studied by anthropologists: "the numbers are small, the information is scarce, and one doesn't know what to expect to find" (1973:249). Making due allowance for these difficulties, we might nonetheless draw attention to a few points, some of which arise from thinking about specific populations or subsets of the set Wood's model is to cover. First, the discussion of crisis mortality is interesting, but some of the shocks seem implausible for hunter-gatherer populations, for example, because of the protective effect of small scale from many acute epidemic infections (McKeown 1988). Second, the Malthusian emphasis on food needs a counterbalance; in much of inland Australia, for example, the sticking point is not food but water, which is less amenable to innovation. And the extent of buffering against random shocks would be interesting to explore for societies of different kinds and sizes (cf. Kelly 1968). More generally, in discussion of density-dependent variations in fertility and mortality, the different time scales on which these might react to "saturation" would surely warrant more detailed attention in future modelling. Finally, we believe a more socioculturally grounded discussion is required before a conclusion can be reached whether "marginal immiseration" leads to conservatism or innovation or, indeed, whether innovation is implicitly contingent on demographic saturation.

ALAN C. SWEDLUND
Department of Anthropology, University of Massachusetts, Amherst, Mass. 01003, U.S.A.
11 IX 97

Wood is to be congratulated on a lucid and fresh airing of the old debates on Malthus and Boserup. I find it reassuring to see some analytical support here for arguments I made some 20 years ago on the relationship between population and resources (Swedlund 1978)—the now mundane observation that population pressure can be both cause and consequence, that Malthus and Boserup are not incompatible, and that "population pressure is not necessarily synonymous with carrying capacity" (p. 153).

When different methodological domains intersect and consider similar subject matter, it is interesting to take note of alternative interpretations of similar phenomena, and my reading of past debates on population growth and pressure has something to do with this nexus. At least two genres seem apparent to me in the demographic anthropology of past human populations. One is narratives which *tend* to be *naturist*, foreground genetic, physiological, and biodemographic variables, and tend to be more deductive, reductive, and formal-mathematical. The other is narratives which *tend* to be *nurturist*, foreground cultural, economic, and sociodemographic variables, and are more inductive, expansive, and expository-theoretical. Wood captures some of this when he opposes the theoretical and the empirical, but the differences I detect are larger and yet less strictly oppositional.

One purpose of Wood's paper is to redress long-standing debates about the impact of the rise of agriculture and increased sedentism on morbidity and mortality. Wood states that Cohen and Armelagos (1984) "argue forcefully that economic change under Boserupian population pressure has led to a net deterioration in health for the great bulk of humanity . . . before the modern industrial revolution." Wood and his colleagues (e.g., Wood et al. 1992) "have questioned the evidentiary basis of this claim," and in this paper he suggests that "whatever empirical support this claim may have, it is based entirely on plausibility arguments and has no basis whatsoever in formal theory."

Do Wood and Cohen reflect some aspects of these two genres in their respective approaches? To some extent I think the answer is yes. Yet, both tend to claim fairly simple and widely generalizable retrodictions (Cohen is more unilineal in his evolutionary perspective, while Wood et al. are much more circumspect about temporal and global regularities). Each tends to claim that the other's variables are quite important but essentially (and sometimes explicitly) leave key variables out of their analyses.

For example, Wood states that he "regard(s) the dynamics of infectious diseases as one of the most difficult and fascinating aspects of preindustrial population dynamics," but in his somewhat anthropocentric model he leaves pathogens out because "it is not clear that the insights to be gained by formally incorporating such phenomena into our theory would offset the loss of clarity resulting from too complex a model." The host-pathogen relationship under differing population aggregations is central to Cohen's thesis. Cohen considers increases in fertility to be fundamental to apparent population growth under agriculture but sidesteps the analytical complexities and the implications these may entail for his notions of "well-being" (e.g., Cohen 1989: 139). Wood has been centrally concerned with the effects of fertility. In addition, their respective definitions of health and well-being are somewhat discordant.

We tend to make judgments based on two bodies of work that take glancing blows at one another but rarely seem to meet head on. There are, however, many components of Wood's models and of Cohen's that will probably be regarded as axiomatic. Some readers will add as axiomatic that social systems tend to have inequalities by age, sex, family lineage, class, ethnicity, and other social institutions, that these inequalities are likely to lead to differential control of and access to resources, and that variance in the control of resources in any preindustrial society (regardless of its subsistence strategies) will likely lead to high loads and variances in the distribution of morbidity and mortality. But elites can control resources and cause misery for those who are dependent; they can control resources and cause misery for themselves (see Levy 1992). Since Wood and Cohen apparently agree that death rates can be density-dependent and that populations can have very high levels of morbidity and mortality and still be capable of positive population growth (see also Pennington 1996, Swedlund 1994), there is a basis for dialogue and an opportunity for refining what meaningful results might look like.[1]

In my readings of historical demography I am frequently jarred by the complex and distinctive ways in which epidemiological regimes can be played out in various social and environmental settings. Historical contingency is not a trivial, residual explanation; it figures large in many important episodes of our demographic past. Likewise, however, as a biological anthropologist I am constantly reminded of the fact that birth, aging, reproduction, and death have significant biological regularities built into them and certain patterns repeat themselves (often). The historical demographer/epidemiologist is wise to heed both messages.

Wood makes explicit a number of insights that I see as attempts at bridging the gap between naturist and nurturist debates. He cites cultural, institutional, and political factors as "important, always and everywhere." He acknowledges that his one exogenous variable (S_t) is a formidable one (incorporating amount of land, usable resources, productive technologies, and organization of production). And he and his colleagues have been unambiguous in their appreciation of the significance of historical contingency (1992:367).

I also like Wood's admissions that "demographers and economists dislike explanations involving climate change" and that the language of social statistics is "scary." While these are obviously meant to inject a little welcome humor into an otherwise very serious paper, like all good humor Wood's candor in this regard has the ring of truth and reflects real sentiments (along with many others) that a number of researchers share. These point the reader to areas of potential preference or bias that both producers and consumers of anthropological demography have and that may impede more nuanced analyses.

Newtonian experiments still have a central place in a post-Einsteinian world; they are "good enough" for many real-world questions. The models that Wood presents here are "good enough" for an understanding of some fundamental relationships central to questions in demographic anthropology. They give us a reasonable foundation from which to build more complex theories of real human demographic experience. Wood piques our interest in promising that there is more to come (Wood n.d.). Susan Greenhalgh (1994:12–15), expressing concern over how much is left out of conventional demographic modeling of fertility, has recently called for the creation of "whole demographies," ones that more adequately capture the range of biological, environmental, social, and political-economic complexities that we observe in most empirical settings. To do this it helps to understand first what is plausible under very limited conditions and narrow assumptions—we are born, we grow up, some of us reproduce, and we die along the way, but oh, the myriad ways we play the game! "Whole demographies" with some generality thrown in? Could we be so lucky? I am looking forward to the sequel.

Reply

JAMES W. WOOD
University Park, Pa. 16802, U.S.A. 8 X 97

I want to thank all the authors for their thought-provoking comments. I enjoyed them and learned from them, and I am grateful for the opportunity they present to discuss some points touched upon too briefly in the paper. Before turning to the individual comments, I would like to make two general points. First, several of the comments concern important factors omitted from my model (for Dewar it's migration, for Swedlund infectious disease dynamics and class, for Cowgill political economy in general, for Littleton and Attenborough water). They are right on both counts: I did leave them out, and they are important. My "failure" to discuss these things in more depth stems not (I hope) from obtuseness, as Cowgill suggests, but from my convictions

---

[1] One exercise I have enjoyed while reading this article is trying to imagine how Malthus himself might react to the positions of Wood and Cohen. My introduction to Malthus was through economic history as a college undergraduate, and his class politics were as familiar to me as his contributions to theories of population growth and regulation. Malthus, after all, was most concerned about the naturally "indolent masses" and the ignorance and lack of restraint among the "labouring classes." Private property and the rights of the landed were not only just but sacrosanct. Indeed, Wood even has apparent difficulty in finding suitable quotes without Malthus's classist arguments' creeping in. I believe Malthus would find the lack of attention to issues of class by both researchers highly deficient.

about the most effective way to approach complex phenomena. Swedlund quotes Greenhalgh's (1995) call for a "whole demography," one that encompasses all the many variables that can affect population processes. And surely we can all agree that that is our ultimate goal; we all yearn for a theory of everything. But do we really gain anything by incorporating the whole world into our analyses from the outset, or do we, as I believe, make better progress by constructing and validating simple models and only bringing in the complexities later? Swedlund speaks of the difference between *naturist* and *nurturist* approaches to population problems. While I dislike the labels—"naturist" implies that people who like simple models also tend to be genetic determinists, which strikes me as unfair—I agree that the distinction is valid. Elsewhere Swedlund (1994:40) has written that, in trying to understand population dynamics "we must tread very cautiously between the Grand Canyon of overgeneralization and the arroyo of particularism." The Hellenistic poet Archilochus used a different metaphor: "The fox knows many things, but the hedgehog knows one big thing" (Berlin 1953). I suppose that I'm just a hedgehog who happens to be attracted to the Grand Canyon (when I fall into a hole, I *really* fall), but I acknowledge that the vast ecosystem of population science also needs its foxes and arroyos.

For me the proper balance between these poles is a combination of ontological holism ("All is one") and methodological reductionism ("'All is one' is a lousy basis for a research program"). Simple models are essential for understanding, but in the end they are in fact simple models and need considerable elaboration and extension (what we often call "operationalization") before they can be applied to particular historical or cultural situations. In reading most of the comments on my paper, I gain considerable satisfaction from seeing my poor, simple model used as a springboard for other writers' thoughts about some of the complexities that exist in the real world. I could not ask for more.

The second general point has to do with the paper's lack of any specific cultural content, something pointed out in various ways by Cowgill, Littleton and Attenborough, and Swedlund. My only defense is that the model, as a *general* model, could not possibly be based on any specific culture. All I could do in the paper was to make the occasional hand-waving sign that, yes, of course the predictions of the model may be modified by local cultural conditions—which I dutifully did. But this defense raises another problem. If I think I can ignore culture, even for a while, then I must have some underlying view of human nature that cuts across specific cultures. Indeed I do, and I ought to make it explicit. Here it is: Given the choice between *better* material conditions for themselves and their families and *worse* material conditions for themselves and their families, people mostly opt for the former. (Optimum foraging theory assumes that the same applies to birds, bats, hedgehogs, and foxes.) Of course, people are often *not* given the choice, and even when they are they may often choose on the basis of incomplete and inaccurate information (a major source of technical inertia). But I believe that, on average and over the long run, a kind of generalized economic rationality prevails, one that in most societies is played out in a familial/household environment. This assumption of "bourgeois rationality" is terribly unchic in certain anthropological circles, but I prefer it to the fashionable belief that the Other is an addlepated dolt in helpless thrall to Culture, which comes from nowhere and answers to no logic but its own. I give people more credit than that.

I turn now to the individual comments. Cowgill plainly does not care much for the paper—though I take some comfort from the fact that he got all the way to footnote 10 before being tempted to dismiss the whole thing out of hand. As near as I can tell, he objects to two things I *didn't* do: I didn't deal with political economy, and I didn't provide any empirical tests (with the clear implication that such tests may be impossible). As it happens, I repeatedly said that it was not my intention to do either of those things and that as a result my model was "a starting point, not a final theory." I am actively working on both "deficiencies," but I felt that it was time to consolidate what I had in hand and present it for public criticism.

In Cowgill's view, my concept of well-being is "radically impoverished." Presumably what he means is that the variable does not explicitly contain all the complexities and nuances of health and wealth and resource differentials among households. Of course it doesn't: it's a variable in a mathematical model, not a detailed portrait of reality. Joel Cohen, who is one of our finest population modelers, calls mathematical models "cartoons" of real-world processes (1995*b*:429). Their greatest virtue is precisely the fact that they *do* simplify (radically impoverish?) things and thus give us an intelligible mental armature on which to hang our more detailed examinations of reality. Only if Cowgill is prepared to say that we should never model anything does his criticism make sense.

Cowgill may be heartened to learn that I am currently exploring the political-economic implications of my model. The mathematics of this new work is complicated, but the gist of it is simple. As the variance in well-being increases during phases of Boserupian expansion, the better-off households invest some of their excess resources into methods for consolidating their position in the upper tail of the w_t distribution. (There's that view of human nature again.) This happens, of necessity, at the expense of households lower down in the w_t distribution, which makes this a model of what Cowgill describes as "differently situated social actors perceiving themselves as having different (and often conflicting) interests and behaving differently in a given overall social situation." The competition among households has the effect of "crystallizing" the wide variance in well-being, which would otherwise decline as the population approaches a new demographic equilibrium. The idea of investing resources to consolidate one's position in a w_t distribution is, no doubt, radically impoverished, but I have in mind such political techniques as formal inheritance systems, pompous displays of high-status goods, military and ideological weapons to coerce the lower orders, and so forth.

Contrary to what Cowgill claims, I think that the model—both as described here and in its newer incarnations—offers a wealth of empirical predictions. The strongest (i.e., most readily falsifiable) predictions have to do with changes over time in the variance of well-being. Currently my colleagues and I are operationalizing these predictions for application to large skeletal samples from medieval Denmark, where dramatic and well-documented changes in demography, health, economy, and political organization took place over a 500-year period. To carry off these empirical analyses, we will need to do a tremendous amount of osteological and statistical work. I would ask Cowgill to wait for the results before dismissing the model as untestable.

Cowgill offers the books edited by Greenhalgh (1995) and by Kertzer and Fricke (1997), as well as the research of Bledsoe and Banja (n.d.), as examples of the sort of work I really ought to be doing. I know all this work well and have a lot of respect for it, but these writers are mostly dealing with different questions from the ones I'm addressing. There is much that is fine and useful in the Greenhalgh and Kertzer-Fricke books (and a few things that are rather less than useful), but only one paper in those two volumes deals with the kind of centuries-long historical changes in population and economy that I'm concerned with (Hammel 1995). As it happens, that paper reaches conclusions that closely parallel my own. Similarly, Bledsoe and her colleagues have done some important work in Ghana, but it bears only distantly on any of the questions I am asking. Moreover, modern methods of contraception have recently increased in prevalence in their study area (Hill 1997), which has almost certainly altered what Ansley Coale (1973) would call the "conscious calculus" of fertility control. Thus, it is difficult to project their results backward into the preindustrial past. But even if it turns out, as Cowgill claims, that women in Ghana have always used fertility control "to maintain their health and their potential future fecundity as a key asset in their economically important relations with male partners," I don't see how that contradicts anything in my model. It only fills in some of the significant detail about the complicated ways in which women play out the relationship between their net reproduction and their well-being—something that is profoundly important but scarcely surprising.[1]

Turning to Dewar's comments, he is right that eventually we will need to incorporate intergroup migration into the model as a basic force of population change, and I think that he is on the right track toward achieving that goal. I like his ideas about how migration may modify the model's predictions, especially the suggestion that migration may result in local demographic disequilibria even when the metapopulation is at equilibrium, thus generating local "hot spots" of innovation. Of course, we will need to model the processes formally before we can assess the prima facie plausibility of this suggestion. Nonetheless, this is precisely the kind of creative elaboration that I had hoped my model might inspire. Incidentally, in an earlier paper Peter Smouse, Jeff Long, and I attempted to estimate a model of density-dependent intergroup migration using data from Papua New Guinea (Wood, Smouse, and Long 1985). In retrospect I now see that we made the mistake of confusing population density with population pressure, an error I discuss at some length in the present paper.

Howell emphasizes that theory, no matter how general, should grow out of empirical research (a point also made by Littleton and Attenborough and by Swedlund). She then graciously suggests that perhaps my model is more grounded in empirical research than its presentation may reveal, since my colleagues and I have done considerable demographic fieldwork among the Gainj of Papua New Guinea (for an overview, see Wood 1992). She is quite right that my work with the Gainj has had a profound effect on my general view of population in preindustrial societies, although it would require a book-length manuscript of its own to elucidate that effect. My views have also been influenced by more recent work using data from Bangladesh and medieval Northern Europe, as well as 25 years of reading the literature on historical demography and demographic anthropology, in which Howell's own writings loom so large. In fact most of my research to date has been empirical and statistical in nature. This is one of my first real forays into general theory.

I would like to correct one misinterpretation made by Howell when she says that I operationalize "preindustrial" as "nonmonetized." That statement is an understandable misreading of a sentence in my introduction, in which I say that I want to generalize Ron Lee's models so that they can be applied to nonmonetized economies. I should have said "to nonmonetized economies as well as to preindustrial economies that are monetized." Certainly my current research on medieval Northern Europe concerns populations that had both markets and money from a very early date.

Konigsberg is correct in his account of the paper's origins. In 1992 my colleagues George Milner, Henry Harpending, Ken Weiss, and I published a paper in this journal on what we called the "osteological paradox" and how it resulted in various interpretational messes (to use Konigsberg's apt term) in paleodemography and paleopathology. Central to our argument were the intertwined concepts of heterogeneous frailty and selective mortality, the joint effect of which was to make any

---

[1] With respect to this point, Cowgill says that I assume that "most people will have as high fertility as their 'well-being' permits." In a general way I do assume this. But the work by Bledsoe and her colleagues in Ghana provides wonderful examples of the reasons this assumption need not lead to high fertility per se. By its nature, well-being involves trade-offs between reproduction and survival, especially the survival of women and their already-born children. As Konigsberg points out, well-being translates into net reproduction, not gross reproduction. Well-being is a matter of striking the proper *balance* between survival and reproduction. Thus, there is nothing in my model that would necessarily predict high levels of fertility under preindustrial conditions.

sample of skeletons unrepresentative of the living population that produced it. We suggested that, if only we could model the heterogeneity in frailty, we would be able to correct for selective mortality, a suggestion echoed by Goodman (1993). As I point out in the present paper, frailty is closely related to the survival component of well-being: the higher your frailty, all other things being equal, the lower your well-being. The need to model the distribution of frailty for paleopathological/paleodemographic purposes led directly to the present paper. Since I started work on this paper, my colleagues and I have developed new parametric hazards models that, in adjusting for selectivity, actually allow us to estimate the frailty distribution from skeletal samples (Holman et al. 1997, O'Connor et al. 1997, Usher et al. 1997). This new work has, in effect, transformed the osteological paradox from an obstacle into an opportunity. Proper attention to selective mortality now allows us to estimate an important piece of the distribution of well-being, which the present model highlights as a central theoretical concern.

Konigsberg alludes to an e-mail exchange we had about my figures 1 and 10, I had used the physiological allocation problem to argue that "something like scenario A [in figure 1] may often prevail under preindustrial conditions." But, as Konigsberg pointed out, if scenario A is the right one in figure 1, then scenario A or B ought to be the right one in figure 10. And this, in turn, suggests that Cohen (1989) may be right in arguing that the emergence of new causes of death following the origins of agriculture would result in higher mortality rates among agriculturalists than among hunter-gatherers. But something is missing here, for if that were all true, then shifting from d_1 to d_2 in panel A or B of figure 10 would lead to a *reduction* in total population size at demographic equilibrium. And if we know anything about the demographic effects of agriculture, it is that population size increased.

There are, I think, two things wrong (or at least incomplete) in this line of thinking. First, although I said that scenario A in figure 1 may often prevail, my lurking suspicion is that something more like scenario B may be more realistic. That is, I suspect that mortality is usually more strongly density-dependent than is fertility under preindustrial conditions. This view (which I am reluctant to make too much of on the basis of current evidence) stems, in part, from studies we have done among the Gainj (Wood and Smouse 1982) suggesting that the mortality of the very young is quite sensitive to changes in population size or density but fertility is comparatively insensitive. Bettina Shell-Duncan's work with the Turkana of Kenya (Shell-Duncan 1993, Shell-Duncan and Wood 1997) reveals that even small differences in nutritional status can affect the risk of morbidity and mortality in young children by influencing their ability to mount an effective immune response to infection. (Immunocompetence, I now believe, is a major component of well-being.) In contrast, recent work by Judy Cameron and her colleagues at the University of Pittsburgh (Cameron et al. 1991, Parfitt, Church, and Cameron 1991, Schreihofer, Amico, and Cameron 1993) strongly suggests that food limitation will compromise reproductive function only if feeding patterns get as bad as might be expected under extreme famine conditions. Thus, I suspect that most preindustrial populations are regulated by mortality rather than fertility, as diagrammed in panel B of figure 1 or panel C of figure 10. If this is so, then new causes of death, such as the infectious diseases that followed upon agriculture, should have no impact whatsoever on the equilibrium mortality rate. Note, incidentally, that population regulation by mortality but not fertility is perfectly consistent with the physiological allocation problem; it just means that factors related to maintenance are compromised by food limitation sooner than those related to reproduction.

But there is a deeper issue. Since one can only die once, different causes of death compete with each other. As a consequence, the addition of new causes does not usually result in proportionate increases in mortality. Moreover, individuals of low well-being will die in disproportionate numbers no matter what set of causes is currently operating on them. Thus, the distribution of well-being is *at least* as important in determining mortality patterns as is the particular bundle of diseases to which the population is exposed. During periods of Boserupian expansion—for example, the one following the adoption of agriculture—well-being was, on average, comparatively high. Thus, mortality temporarily declined even as new diseases were introduced, and population grew as a consequence. Once the expansion was over and a new demographic equilibrium had been reached at higher population levels, average well-being dropped back down again and the level of mortality returned to what it had previously been. In my opinion, temporary shifts in well-being can account for observed patterns of population growth in early agricultural populations, even if the vital rates of agriculturalists and hunter-gatherers are indistinguishable at equilibrium and even if the absolute number of causes of death increases from one epoch to the next.

Like Cowgill, Littleton and Attenborough are unhappy with my use of the term "well-being," but unlike Cowgill they tell us why. They suggest that the concept of well-being has usefully been given a broad definition in the recent literature and that I have confined it in a definitional straitjacket to its great harm. In fact, "well-being" is defined in a variety of ways in the sociological, epidemiological, and economic literature, sometimes even more narrowly than I have done (Paim 1995). But I understand their point and sympathize with it. I struggled over what label to give this concept. I knew that "well-being" would have connotations that I did not intend—emotional well-being, a *subjective* sense of physical well-being, even spiritual well-being. But "physical condition" seemed too cumbersome, and "fitness" carried a suggestion of genetic determination that I did not wish to imply. (In this sense, I disagree with Littleton and Attenborough that my concept of well-being is *necessarily* Darwinian.) At one point, following Konigsberg, I toyed with "net reproduction rate" but rejected it for two reasons. First, since the net reproduction rate is inherently unmeasurable in individuals, the term fails to convey the fundamental significance of individual-level heterogeneity in well-being. Second, by its very abstractness the net reproduction rate risks drawing attention away from the tangible aspects of health and physical condition that I wanted to emphasize. Reluctantly I settled on "well-being," fully realizing that some readers might object to it. I thought that if I defined it carefully and used it consistently I might be able to stay out of trouble. Not, alas, so.

Littleton and Attenborough's objection to my use of "well-being" does raise an interesting substantive issue. They suggest that Cohen and Armelagos (1984) may be right in arguing that the origin of agriculture compromised well-being in the broad sense, even if it didn't do so in my particular narrow sense of the term. That may well be true, but I fail to see the *demographic* significance of such a broad definition. It reminds me of Cohen's (1997) recent claim that the skeletal lesions studied by paleopathologists are unaffected by selective mortality because they had no influence on the living individual's risk of death. Why in the world should we, as population scientists, waste our time on purported "health" characteristics so trivial that they have no effect whatsoever on the risk of death, even under premodern conditions? It is well-being, in my narrow technical sense, that is the linchpin linking economic and demographic change. If that linkage is what we're interested in, there seems little point in worrying about possible alternative definitions of well-being, no matter how useful they may be in other contexts. Still, if anyone can come up with a better term, I'd be eternally grateful.

Littleton and Attenborough suggest that hunter-gatherers may have been less prone to crisis mortality than I have argued because they were not exposed to several infectious diseases that were endemic to later agricultural populations. Now, it is true, as I have noted repeatedly, that there is a small set of diseases, all of them highly host-specific and immunogenic, spread by direct transmission, and lacking long latency periods, that cannot be maintained endemically in populations of less than several tens of thousands of people. Those diseases (influenza, measles, smallpox, and a few others) were indeed unlikely to have been common in hunter-gatherer populations, a point I made in the very first paper I ever published (Wood 1975). And we know that some of those diseases, especially smallpox, were important causes of mortality crises in early modern Europe (Duncan, Scott, and Duncan 1993). But those diseases do not account for all mortality crises. In my opinion, hunter-gatherers were likely to have been just as susceptible to crises associated with food shortages as agriculturalists—perhaps even more so because of their general lack of food storage facilities. I would argue yet again that the distribution of well-being, which at equilibrium is expected to be similar in hunter-gatherers and agriculturalists, is just as important as the specific set of diseases present in a population in determining its proneness to crisis.

Swedlund's thoughtful ruminations stand on their own. I do not disagree with him at all that we need to add nuance to the model, especially when applying it to the real world. Nor do I disagree that there are large areas of overlap between my work and that of Mark Nathan Cohen. Indeed, I appreciate the chance to say that Cohen's research is of great importance and has deeply influenced my own thinking about population processes, despite the criticisms I have made of some portions of it. Swedlund suggests that one source of my disagreement with Cohen is that I leave infectious diseases exogenous to my model whereas Cohen makes them endogenous. I agree with both Swedlund and Cohen that ultimately infectious disease dynamics will need to be folded into the model. But my approach to the problem would differ from Cohen's. I would argue, as I did above, that the distribution of well-being or frailty is at least as important in determining levels of mortality as is the particular bundle of infectious agents present at any given time. To translate it into epidemiological jargon, my treatment would emphasize host factors in disease and death at least as much as pathogenic factors.

Finally, I cannot leave unchallenged a passing slander on poor old Malthus contained in Swedlund's footnote: the claim that, for Malthus, "Private property and the rights of the landed were not only just but sacrosanct." Granted, Malthus was a creature of his time and class, and we should not expect an anachronistic political correctness from him. But his work as a whole is consistently motivated by a desire, perhaps paternalistic but nonetheless real, to ameliorate the plight of the poor. He did indeed believe in private property, but only because he considered it the most reliable way to ensure that workers benefited from the fruits of their own labor. (His logic in this connection anticipates 20th-century arguments about the tragedy of the commons.) Moreover, he frequently criticized the "propertied classes" and especially wealthy landowners in the strongest possible language for exploiting the poor. In a graduate seminar, we recently had our students read Malthus's 1830 *Summary View of the Principle of Population.* Their most frequent reaction was something along the lines of "Gee, I was always taught that Malthus was evil, but he's not so bad after all!" In this, the bicentennial year of the first edition of *The Principle of Population,* I suggest that the surest way to secure the "new and more sympathetic rereading of Malthus" that Howell calls for is to go back and read what he himself had to say rather than rely on what others have said about him.

References Cited

ANDERSON, R. M., AND R. M. MAY. 1991. *Infectious diseases of humans: Dynamics and control.* Oxford: Oxford University Press.

ARNOLD, D. 1988. *Famine: Social crisis and historical change.* Oxford: Basil Blackwell.

BAKER, P. T., AND W. T. SANDERS. 1972. Demographic studies in anthropology. *Annual Review of Anthropology* 1:151–78.

BATES, D. G., AND S. H. LEES. 1979. "The myth of population regulation," in *Evolutionary biology and human social behavior.* Edited by N. A. Chagnon and W. Irons, pp. 273–89. North Scituate, Mass.: Duxbury.

BENTLEY, G. R., T. GOLDBERG, AND G. JASIENSKA. 1993. The fertility of agricultural and non-agricultural traditional societies. *Population Studies* 47:269–81.

BERLIN, I. 1953. *The hedgehog and the fox: An essay on Tolstoy's view of history.* London: Weidenfeld and Nicholson.

BERRY, W. D. 1984. *Nonrecursive causal models.* Newbury Park, Calif.: Sage.

BINFORD, L. R. 1968. "Post-Pleistocene adaptations," in *New perspectives in archaeology.* Edited by S. R. Binford and L. R. Binford, pp. 313–41. Chicago: Aldine. [RED]

BIRDSELL, J. B. 1957. Some population problems involving Pleistocene man. *Cold Spring Harbor Symposia in Quantitative Biology* 22:47–69.

BLEDSOE, CAROLINE, WITH FATOU BANJA. n.d. "Numerators and denominators in the study of high-fertility populations: Past and potential contributions from cultural anthropology," in *The continuing demographic transition.* Edited by G. W. Jones, R. M. Douglas, J. C. Caldwell, and R. M. D'Souza. New York: Oxford University Press. [GLC]

BOIS, G. 1978. Against the neo-Malthusian orthodoxy. *Past and Present* 79:60–69.

BONGAARTS, J., AND M. CAIN. 1982. "Demographic responses to famine," in *Famine.* Edited by K. M. Cahill, pp. 44–59. Maryknoll, N.Y.: Orbis Books.

BOSERUP, E. 1965. *The conditions for agricultural growth: The economics of agrarian change under population pressure.* Chicago: Aldine.

———. 1981. *Population and technological change: A study of long-term trends.* Chicago: University of Chicago Press.

———. 1990. *Economic and demographic relationships in development.* Baltimore: Johns Hopkins University Press.

CAMPBELL, K. L., AND J. W. WOOD. 1988. "Fertility in traditional societies," in *Natural human fertility: Social and biological determinants.* Edited by P. Diggory, M. Potts, and S. Teper, pp. 39–69. London: Macmillan.

CAMERON, J. L., T. E. WELTZIN, C. MC CONOHA, D. L. HELMREICH, AND W. H. KAYE. 1991. Slowing of pulsatile luteinizing hormone secretion in men after forty-eight hours of fasting. *Journal of Clinical Endocrinology and Metabolism* 73:35–41.

CARR-SAUNDERS, A. M. 1922. *The population problem: A study in human evolution.* Oxford: Oxford University Press.

CASTI, J. L. 1990. *Searching for certainty: What scientists can know about the future.* New York: W. Morrow.

CHARLESWORTH, B. 1994. 2d edition. *Evolution in age-structured populations.* Cambridge: Cambridge University Press.

CHAYANOV, A. V. 1986. *The theory of peasant economy.* Madison: University of Wisconsin Press.

CIPOLLA, C. M. 1993. *Before the industrial revolution: European society and economy 1000–1700.* New York: W. W. Norton.

CLARK, C., AND M. HASWELL. 1967. 3d edition. *The economics of subsistence agriculture.* London: Macmillan.

COALE, A. J. 1973. "The demographic transition reconsidered." *Proceedings of the International Union for the Scientific Study of Population International Population Conference* 1:53–72.

———. 1979. The use of modern analytical demography by T. R. Malthus. *Population Studies* 33:329–32.

COHEN, J. E. 1995*a.* Population growth and the Earth's human carrying capacity. *Science* 269:341–46.

———. 1995. *How many people can the Earth support?* New York: W. W. Norton.

COHEN, M. N. 1977. *The food crisis in prehistory: Overpopulation and the origin of agriculture.* New Haven: Yale University Press.

———. 1989. *Health and the rise of civilization.* New Haven: Yale University Press.

———. 1997. "Does paleopathology measure community health? A rebuttal of 'The Osteological Paradox' and its implications for world history," in *Integrating archaeological demography: Multidisciplinary approaches to prehistoric population.* Edited by R. R. Paine, pp. 242–60. Southern Illinois University Center for Archaeological Investigations Occasional Paper 24.

COHEN, M. N., AND G. J. ARMELAGOS. 1984. "Paleopathology at the origins of agriculture: Editors' summation," in *Paleopathology at the origins of agriculture.* Edited by M. N. Cohen and G. J. Armelagos, pp. 585–601. Orlando: Academic Press.

COLEMAN, D., AND R. SCHOFIELD. Editors. 1986. *The state of population theory: Forward from Malthus.* Oxford: Basil Blackwell.

COWGILL, G. L. 1975. On causes and consequences of ancient and modern population changes. *American Anthropologist* 77:505–25.

———. 1996. "Population, human nature, knowing actors, and explaining the onset of complexity," in *Debating complexity: Proceedings of the 26th annual Chacmool Conference.* Edited by Daniel A. Meyer, Peter C. Dawson, and Donald T. Hanna, pp. 16–22. Calgary: Archaeological Association of the University of Calgary. [GLC]

CROWLEY, T. J., AND G. R. NORTH. 1991. *Paleoclimatology.* Oxford: Oxford University Press.

CURRY, B., AND G. HUGO. 1984. *Famine as a geographical phenomenon.* Dordrecht: Reidel.

DASGUPTA, P. 1974. "On optimum population size," in *Economic theory and planning.* Edited by A. Mitra, pp. 109–33. New Delhi: Oxford University Press.

DEWAR, R. E. 1984. Environmental productivity, population regulation, and carrying capacity. *American Anthropologist* 86:601–14.

DOLL, J. P., AND F. ORAZEM. 1978. *Production economics: Theory with applications.* New York: John Wiley.

DUNCAN, S. R, S. SCOTT, AND C. J. DUNCAN. 1993. The dynamics of smallpox epidemics in Britain, 1550–1800. *Demography* 30:405–23.

DYER, C. 1989. "'The retreat from marginal land': The growth and decline of medieval rural settlements," in *The rural settlements of medieval England.* Edited by M. Aston, D. Austin, and C. Dyer, pp. 45–57. Oxford: Blackwell.

EASTERLIN, R. 1968. *Population, labor force, and long swings in economic growth.* New York: National Bureau for Economic Research.

ELLIS, F. 1993. 2d edition. *Peasant economics: Farm households and agrarian development.* Cambridge: Cambridge University Press.

FLEW, A. Editor. 1970. *Thomas Robert Malthus's* An essay on the principle of population *and* A summary view of the principle of population. Harmondsworth: Penguin.

FLINN, M. W. 1981. *The European demographic system 1500–1820.* Baltimore: Johns Hopkins University Press.

GAGE, T. B., AND K. O'CONNOR. 1994. Nutrition and variation in level and age pattern of mortality. *Human Biology* 66:77–103.

GOODMAN, A. H. 1993. On the interpretation of health from skeletal remains. CURRENT ANTHROPOLOGY 34:281–88.

GREENHALGH, SUSAN. Editor. 1995. *Situating fertility: Anthropology and demographic inquiry.* Cambridge: Cambridge University Press. [GLC, ACS]

GUTIERREZ, A. P. 1996. *Applied population ecology: A supply-demand approach.* New York: John Wiley.

HAJNAL, J. 1965. "European marriage patterns in perspective," in *Population in history.* Edited by D. V. Glass and D. E. C. Eversley, pp. 101–43. London: Edward Arnold.

HAMMEL, E. A. 1995. "Economics 1, culture 0: Fertility change and differences in the northwest Balkans, 1700–1900," in *Situating fertility: Anthropology and demographic inquiry.* Edited by S. Greenhalgh, pp. 225–58. Cambridge: Cambridge University Press.

HANSKI, I. 1996. "Metapopulation ecology," in *Population dynamics in ecological time and space.* Edited by O. E. Rhodes, R. K. Chesser, and M. H. Smith, pp. 13–43. Chicago: University of Chicago Press. [RED]

HARRIS, M., AND E. B. ROSS. 1987. *Death, sex, and fertility:*

Population regulation in preindustrial and developing societies. New York: Columbia University Press.

HARVEY, B. 1993. *Living and dying in England 1100–1540: The monastic experience.* Oxford: Oxford University Press.

HASSAN, F. A. 1981. *Demographic archaeology.* New York: Academic Press.

HEADY, E. O., AND J. L. DILLON. 1961. *Agricultural production functions.* Ames: Iowa State University Press.

HEWLETT, B. S. 1991. Demography and childcare in preindustrial societies. *Journal of Anthropological Research* 47:1–39.

HILL, A. G. 1997. " 'Truth lies in the eye of the beholder': The nature of evidence in demography and anthropology," in *Anthropological demography: Toward a new synthesis.* Edited by D. I. Kertzer and T. Fricke, pp. 223–47. Chicago: University of Chicago Press.

HOLMAN, D. J., K. A. O'CONNOR, J. W. WOOD, AND J. L. BOLDSEN. 1997. Correcting for nonstationarity in paleodemographic mortality models [abstract]. *American Journal of Physical Anthropology,* suppl. 24:132.

HOWELL, N. 1986. Demographic anthropology. *Annual Review of Anthropology* 15:219–46.

JAMES, P. Editor. 1989. Variorum edition. *Thomas Robert Malthus's An essay on the principle of population.* 2 vols. Cambridge: Cambridge University Press.

JOHANSSON, S. R., AND S. HOROWITZ. 1986. Estimating mortality in skeletal populations: Influence of the growth rate on the interpretation of levels and trends during the transition to agriculture. *American Journal of Physical Anthropology* 71: 233–50.

JORDAN, W. C. 1996. *The great famine: Northern Europe in the early fourteenth century.* Princeton: Princeton University Press.

KEELEY, L. H. 1996. *War before civilization.* New York: Oxford University Press.

KELLY, R. L. 1995. *The foraging spectrum: Diversity in hunter-gatherer lifeways.* Washington, D.C.: Smithsonian Institution Press.

KERSHAW, I. 1973. The Great Famine and agrarian crisis in England 1315–22. *Past and Present* 59:3–50.

KERTZER, DAVID I., AND THOMAS FRICKE. Editors. 1997. *Anthropological demography: Toward a new synthesis.* Chicago: University of Chicago Press. [GLC]

KEYFITZ, N. 1975. How do we know the facts of demography? *Population and Development Review* 1:267–88.

KNODEL, J. E. 1988. *Demographic behavior in the past: A study of fourteen German village populations in the eighteenth and nineteenth centuries.* Cambridge: Cambridge University Press.

KONIGSBERG, L. W., J. E. BUIKSTRA, AND J. BULLINGTON. 1989. Paleodemographic correlates of fertility: A reply to Corrucini, Brandon, and Handler and to Holland. *American Antiquity* 54:626–36.

KONIGSBERG, L. W., AND S. R. FRANKENBERG. 1994. Paleodemography: "Not quite dead." *Evolutionary Anthropology* 3:92–105.

KRZYWICKI, L. 1934. *Primitive society and its vital statistics.* London: Macmillan.

LAMB, H. H. 1995. 2d edition. *Climate, history, and the modern world.* London: Routledge.

LEE, R. D. 1974. The formal dynamics of controlled populations and the echo, the boom, and the bust. *Demography* 11:563–85.

———. 1977. "Methods and models for analyzing historical series of births, deaths, and marriages," in *Population patterns in the past.* Edited by R. D. Lee, pp. 337–70. New York: Academic Press.

———. 1978. "Models of preindustrial dynamics with applications to England," in *Historical studies of changing fertility.* Edited by C. Tilley, pp. 155–207. Princeton: Princeton University Press.

———. 1986*a.* "Malthus and Boserup: A dynamic synthesis," in *The state of population theory: Forward from Malthus.* Edited by D. Coleman and R. S. Schofield, pp. 96–103. Oxford: Basil Blackwell.

———. 1986*b.* "Population homeostasis and English demographic history," in *Population and economy: Population history from the traditional to the modern world.* Edited by R. I. Rotberg and T. K. Rabb, pp. 75–100. Cambridge: Cambridge University Press.

———. 1987. Population dynamics of humans and other animals. *Demography* 24:443–65.

———. 1988. Induced population growth and induced technological progress: Their interaction in the accelerating stage. *Mathematical Population Studies* 1:265–88.

———. 1993. Accidental and systematic change in population history: Homeostasis in a stochastic setting. *Explorations in Economic History* 30:1–30

———. 1994. Human fertility and population equilibrium. *Annals of the New York Academy of Sciences* 709:396–407

LE ROY LADURIE, E. 1974. *The peasants of Languedoc.* Urbana: University of Illinois Press.

LEVY, JERROLD. 1992 *Orayvi revisited: Social stratification in an "egalitarian society."* Santa Fe: School of American Research. [ACS]

MC DONALD, J., AND G. D. SNOOKS. 1986. *Domesday economy: A new approach to Anglo-Norman history.* Oxford: Oxford University Press.

MALTHUS, T. R. 1798. *An essay on the principle of population, as it affects the future improvement of society; with remarks on the speculations of Mr. Godwin, M. Condorcet, and other writers.* London: J. Johnson.

———. 1803. 2d edition. *An essay on the principle of population; or a view of its past and present effects on human happiness; with an inquiry into our prospects respecting the future removal or mitigation of the evils which it occasions.* London: J. Johnson.

———. 1806. 3d edition. *An essay on the principle of population; or a view of its past and present effects on human happiness; with an inquiry into our prospects respecting the future removal or mitigation of the evils which it occasions.* London: J. Johnson.

———. 1807. 4th edition. *An essay on the principle of population; or a view of its past and present effects on human happiness; with an inquiry into our prospects respecting the future removal or mitigation of the evils which it occasions.* London: John Murray.

———. 1830. *A summary view of the principle of population.* London: John Murray.

MARTORELL, R., AND T. J. HO. 1984. "Malnutrition, morbidity, and mortality," in *Child survival: Strategies for research.* Edited by W. H. Mosley and L. C. Chen, pp. 49–68. Cambridge: Cambridge University Press.

MAYNARD SMITH, J. 1976. Group selection. *Quarterly Review of Biology* 51:277–83.

MIELKE, J. H., L. B. JORDE, P. G. TRAPP, D. L. ANDERTON, K. PITKÄNEN, AND A. W. ERIKSSON. 1984. Historical epidemiology of smallpox in Åland, Finland: 1751–1890. *Demography* 21:271–95.

MILL, J. S. 1848. *Principles of political economy, with some of their applications to social philosophy.* London: Routledge.

MOORE, J. H. 1994. Putting anthropology back together again: The ethnogenetic critique of cladistic theory. *American Anthropologist* 96:925–48. [RED]

NETTING, R. M. 1993. *Smallholders, householders: Farm families and the ecology of intensive, sustainable agriculture.* Stanford: Stanford University Press.

O'BRIEN, S. R., P. A. MAYEWSKI, L. D. MEEKER, D. A. MEESE, M. S. TWICKLER, AND S.I. WHITLOW. 1995. Complexity of Holocene climate as reconstructed from a Greenland ice core. *Science* 270:1962–64.

O'CONNOR, K. A., D. J. HOLMAN, J. W. WOOD, J. L. BOLDSEN, AND T. B. GAGE. 1997. Competing and mixed hazards models of mortality for paleodemographic analysis [abstract]. *American Journal of Physical Anthropology,* suppl. 24: 180.

PAIM, L. 1995. Definition and measurement of well-being: A review of literature. *Journal of Economic and Social Measurement* 21:297–309.

PARFITT, D. B., K. R. CHURCH, AND J. L. CAMERON. 1991. Restoration of pulsatile luteinizing hormone secretion after fasting in rhesus monkeys (*Macaca mulatta*): Dependence on size of the refeed meal. *Endocrinology* 129:749–56.

PEARL, R. 1939. *The natural history of population.* Oxford: Oxford University Press.

PENNINGTON, R. L. 1996. Causes of early human population growth. *American Journal of Physical Anthropology* 99:259–74.

PETERSEN, W. 1979. *Malthus.* Cambridge: Harvard University Press.

POOS, L. R. 1991. *A rural society after the Black Death: Essex 1350–1525.* Cambridge: Cambridge University Press.

POSTAN, M. M., AND J. HATCHER. 1985. "Population and class relations in feudal society," in *The Brenner debate: Agrarian class structure and economic development in preindustrial Europe.* Edited by T. H. Aston and C. H. E. Philpin, pp. 65–78. Cambridge: Cambridge University Press.

PRYOR, F. L., AND S. B. MAURER. 1982. On induced economic change in precapitalist economies. *Journal of Development Economics* 10:325–53.

PULLIAM, H. R. 1996. "Sources and sinks: Empirical evidence and population consequences," in *Population dynamics in ecological time and space.* Edited by O. E. Rhodes, R. K. Chesser, and M. H. Smith, pp. 45–69. Chicago: University of Chicago Press. [RED]

RICARDO, D. 1819. 2d edition. *The principles of political economy and taxation.* London: John Murray.

ROGERS, A. R. 1992. "Resources and population dynamics," in *Evolutionary biology and human behavior.* Edited by E. A. Smith and B. Winterhalder, pp. 375–402. Hawthorne, N.Y.: Aldine de Gruyter.

SAHLINS, M. 1972. *Stone Age economics.* Chicago: Aldine.

SATTENSPIEL, L. 1987. Population structure and the spread of disease. *Human Biology* 59:411–38.

SATTENSPIEL, L., AND H. C. HARPENDING. 1983. Stable populations and skeletal age. *American Antiquity* 48:489–98.

SAUVY, A. 1969. *General theory of population.* London: Weidenfeld and Nicolson.

SCHREIHOFER, D. A., J. A. AMICO, AND J. L. CAMERON. 1993. Reversal of fasting-induced suppression of luteinizing hormone (LH) secretion in male rhesus monkeys by intragastric nutrient infusion: Evidence for rapid stimulation of LH by nutritional signals. *Endocrinology* 132:1890–97.

SELYE, HANS. 1976. *Stress in health and disease.* London: Butterworths. [LWK]

SHELL-DUNCAN, B. 1993. Cell-mediated immunocompetence among nomadic Turkana children. *American Journal of Human Biology* 5:225–35.

SHELL-DUNCAN, B., AND J. W. WOOD. 1997. The evaluation of delayed-type hypersensitivity responsiveness and nutritional status as predictors of gastrointestinal and acute respiratory infection: A prospective field study among traditional nomadic Kenyan children. *Journal of Tropical Pediatrics* 43:25–32.

SIBLY, R. M., AND P. CALOW. 1986. *Physiological ecology of animals: An evolutionary approach.* Oxford: Blackwell Scientific.

SIMON, J. L. 1990. *Population matters: People, resources, environment, and immigration.* London: Transaction.

SMITH, R. M. 1984. "Families and their land in an area of partible inheritance: Redgrave, Suffolk 1260–1320," in *Land, kinship, and life-cycle.* Edited by R. M. Smith, pp. 135–95. Cambridge: Cambridge University Press.

SPOONER, B. Editor. 1972. *Population growth: Anthropological implications.* Cambridge: MIT Press.

SWEDLUND, ALAN. 1978. Historical demography as population ecology. *Annual Review of Anthropology* 7:137–73. [ACS]

———. 1994. "Issues in demography and health," in *Understanding complexity in the prehistoric Southwest.* Edited by G. Gumerman and M. Gell-Mann, pp. 39–58. Reading, Mass.: Addison-Wesley. [ACS]

TURCHIN, P. 1995. "Population regulation: Old arguments and a new synthesis," in *Population dynamics: New approaches and synthesis.* Edited by N. Cappuccino and P. W. Price, pp. 19–40. San Diego: Academic Press.

ULIJASZEK, S. F. 1996. Energetics, adaptation, and adaptability. *American Journal of Human Biology* 8:169–82.

USHER, B. M., J. L. BOLDSEN, J. W. WOOD, AND G. R. MILNER. 1997. A general approach to the detection of heterogeneity and selective mortality from skeletal lesions (abstract). *American Journal of Physical Anthropology,* suppl. 24:230–31.

UYENOYAMA, M. K., AND M. W. FELDMAN. 1980. Theories of kin selection and group selection: A population genetics perspective. *Theoretical Population Biology* 17:380–414.

VAUPEL, J. W., K. G. MANTON, AND E. STALLARD. 1979. The impact of heterogeneity in individual frailty on the dynamics of mortality. *Demography* 16:439–54.

VAUPEL, J. W., AND A. I. YASHIN. 1985. Heterogeneity's ruses: Some surprising effects of selection on population dynamics. *American Statistician* 39:176–85.

WACHTER, K. W. 1987. Mathematical requirements for homeostasis in prehistory. *Sloan-Berkeley Working Papers in Population Studies* 11:1–26.

WADE, M. J. 1978. A critical review of the models of group selection. *Quarterly Review of Biology* 53:101–14.

WALTER, J., AND R. S. SCHOFIELD. Editors. 1989. *Famine, disease, and the social order in early modern society.* Cambridge: Cambridge University Press.

WATKINS, S., AND J. A. MENKEN. 1985. Famines in historical perspective. *Population and Development Review* 11:647–75.

WATKINS, S., AND E. VAN DE WALLE. 1983. "Nutrition, mortality, and population size: Malthus' court of last resort," in *Hunger and history: The impact of changing food production and consumption patterns on society.* Edited by R. I. Rotberg and T. K. Rabb, pp. 7–28. Cambridge: Cambridge University Press.

WEISS, K. M. 1990. The biodemography of variation in human frailty. *Demography* 27:185–206.

WHITE, L. 1962. *Medieval technology and social change.* Oxford: Oxford University Press.

WOOD, J. W. 1975. The evolutionary implications of depopulation. *Michigan Discussions in Anthropology* 1:45–70.

———. 1992. "Fertility and reproductive biology in Papua New Guinea," in *Human biology in Papua New Guinea: The small cosmos.* Edited by R. D. Attenborough and M. P. Alpers, pp. 93–118. Oxford: Oxford University Press.

———. 1994*a. Dynamics of human reproduction: Biology, biometry, demography.* Hawthorne, N. Y.: Aldine de Gruyter.

———. 1994*b.* Maternal nutrition and reproduction: Why demographers and physiologists disagree about a fundamental relationship. *Annals of the New York Academy of Sciences* 709:101–16.

———. n.d. Malthus in the Middle Ages: Population, economy, and well-being in rural England A.D. 1000–1500. MS.

WOOD, J. W., D. J. HOLMAN, K. M. WEISS, A. V. BUCHANAN, AND B. LEFOR. 1992. Hazards models for human population biology. *Yearbook of Physical Anthropology* 35:43–87.

WOOD, J. W., AND G. R. MILNER. 1994. The osteological paradox reconsidered: Reply. CURRENT ANTHROPOLOGY 35:631–37.

WOOD, J. W., G. R. MILNER, H. C. HARPENDING, AND K. M. WEISS. 1992. The osteological paradox: Problems of inferring prehistoric health from skeletal samples. CURRENT ANTHROPOLOGY 33:343–70.

WOOD, J. W., AND P. E. SMOUSE. 1982. A method of analyzing density-dependent vital rates, with an application to the Gainj of Papua New Guinea. *American Journal of Physical Anthropology* 58:403–11.

WOOD, J. W., P. E. SMOUSE, AND J. C. LONG. 1985. Sex-specific dispersal patterns in two human populations of highland New Guinea. *American Naturalist* 125:747–68.

WRIGHTSON, K., AND D. LEVINE. 1989. "Death in Whickham," in *Famine, disease, and the social order in early mod-*

ern society. Edited by J. Walter and R. S. Schofield, pp. 129–65. Cambridge: Cambridge University Press.

WRIGLEY, E. A. 1983. "Malthus's model of a pre-industrial economy," in *Malthus past and present.* Edited by J. Dupâquier, pp. 111–24. London: Academic Press.

———. 1986. "Elegance and experience: Malthus at the bar of history," in *The state of population theory: Forward from Malthus.* Edited by D. Coleman and R. Schofield, pp. 46–64. Oxford: Basil Blackwell.

WRIGLEY, E. A., AND R. S. SCHOFIELD. 1981. *The population history of England 1541–1871: A reconstruction.* Cambridge: Cambridge University Press.

WYNNE-EDWARDS, V. C. 1962. *Animal dispersion in relation to social behaviour.* Edinburgh: Oliver and Boyd.

The University of Chicago Press

Journals and Annual Series

Available from Chicago in 1998
The Astronomical Journal
Publications of the Astronomical Society of the Pacific

American Journal of Education
The American Journal of Human Genetics
American Journal of Sociology
The American Naturalist
The Astrophysical Journal
The Astrophysical Journal Supplement Series
Child Development
Child Development Abstracts and Bibliography
Classical Philology
Clinical Infectious Diseases
Comparative Education Review
Crime and Justice
Critical Inquiry
Current Anthropology
Economic Development and Cultural Change
The Elementary School Journal
Ethics: An International Journal of Social, Political, and Legal Philosophy
History of Religions
International Journal of American Linguistics
International Journal of Plant Sciences
Isis
Journal of the American Musicological Society
Journal of British Studies
The Journal of Business
Journal of Consumer Research: An Interdisciplinary Quarterly
The Journal of Geology
Journal of the History of Sexuality
The Journal of Infectious Diseases
Journal of Labor Economics
The Journal of Law & Economics
The Journal of Legal Studies
The Journal of Modern History
Journal of Near Eastern Studies
Journal of Political Economy
The Journal of Religion
Law & Social Inquiry
The Library Quarterly
Modern Philology
Monographs of the Society for Research in Child Development
Ocean Yearbook
Osiris
Perspectives in Biology and Medicine
Perspectives on Science: Historical, Philosophical, Social
Philosophy of Science
Physiological Zoology
Public Opinion Quarterly
The Quarterly Review of Biology
Signs: Journal of Women in Culture and Society
Social Service Review
Supreme Court Economic Review
The Supreme Court Review
Technology and Culture
Winterthur Portfolio: A Journal of American Material Culture

Distributed by the University of Chicago Press
Metropolitan Museum Journal

The University of Chicago Press
Journals Division, P.O. Box 37005, Chicago, Illinois 60637 USA
Phone (773) 753-3347 • Facsimile (773) 753-0811
E-mail subscriptions@journals.uchicago.edu
Visit our website at http://www.journals.uchicago.edu