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Economic Change, Mortality and Malthusian Theory

DAVID J. LOSCHKY

Since the time of Malthus, population theory and economic theory have been linked closely. Not the entire range of population theory was based on economic considerations. After all, fertility, according to Malthus, was largely a matter of convenience and moral restraint. But changes in mortality were linked closely to the economy's performance. And since mortality determined a population's growth rate, population growth was determined by output growth.

Recent work has tended to link fertility more firmly with economic activity.¹ But at the same time, it has also discredited the older tie between economic performance and mortality. In this sense, population theory is moving away from economic theory. This growing estrangement is unfortunate for various reasons. Not the least of these is that the separation is based upon objectionable arguments. In many cases the conclusions which broke the Malthusian links were based either upon inadequate grounds or else upon tests of propositions which did not entirely express the traditional Malthusian theses. It may well be that mortality is mainly unrelated to economic factors. But a reasonable formulation of Malthus's contentions is quite consistent with published research into mortality.

We begin our argument with a discussion of Malthusian mortality principles as formulated by economists. Needless to say, demographers, following the lead of economists who have rejected Malthusianism, see mortality as being influenced largely by non-economic factors.² We proceed to discuss research results which test these principles. Here we establish the current conception of Malthusian mortality theory. And we outline objections to this theory: Section II begins by evaluating one class of such objections. We then move to a reconstruction of Malthusian views on mortality. The reconstruction is interpreted in terms of modern economic concepts. And we then compare this interpretation with various critical studies. We close with a few comments on the relationship between this reformulation and transition theory.

I. THE EVIDENCE

Why was the original Malthusian connection rejected? Because the theory was tested and found wanting. If we are to reformulate Malthusian theory in an acceptable manner we must at least meet a number of these objections.

Malthusian theory was tested in diverse ways. Its assumptions were tested for plausibility. If there were a divergence between reality and the assumptions, the theory would be rejected. Alternatively, the theory could be tested by comparing its consequences with observation. If its predictions are inaccurate, the theory is rejected. Needless to say, the consequences generally are not confirmed. These tests are of particular importance to us. We acknowledge this by first noting the implications which economists have derived and then describe how tests were made and what conclusions were derived from them. The theory's logical consistency and the validity of implications drawn from it could also be tested. To some extent this was done. Lastly, and perhaps most importantly, the theory was compared with alternative hypotheses. This, of course, is also a test. And Malthusianism was rejected on the grounds that population fluctua-

¹ Work along the lines of Gary S. Becker's 'An Economic Analysis of Fertility', *Demographic and Economic Change in Developed Countries* (National Bureau of Economic Research Conference Series, New York, 1960), pp. 209–241, has tied fertility to the theory of consumer demand.

² Peter R. Cox, *Demography* (Cambridge: Cambridge University Press, 1970, 4th edn).

tions are explained better by using other factors, such as climate or the rise and decline of epidemics. These tests are some distance apart from our major arguments which are concerned with a reformulation of Malthusian theory. But demographers' views have been influenced by such studies. Consequently, we deal with the logical character of these studies to clear the way for our work. On a more formal level, Malthusian theory has been replaced by transition theory. The arguments that we apply to theories relating population to climatic studies apply *mutatis mutandis* to those based on transition theory. But as our arguments do not deal with the comparative performance of these theories there are features of these positions upon which we do not comment.

These are the major grounds for rejecting Malthusian theory. We shall not try to meet all the objections. To test for consistency, validity and relative performance would be well beyond this paper's bounds. We seek only to interpret the basic assumptions so that they become plausible and so that some implications can find empirical support. Moreover, our tests do not exhaust the available data. We consider some findings from English economic history. But, since they should reflect a diversity of mortality experience, this limited examination should meet those critical positions which rest upon empirical grounds.

What are the traditional Malthusian assumptions? And how has reality diverged from Malthusian implications sufficiently to goad critics to vigorous action?

We are not particularly concerned with determining the attainable rate of output growth either by observation or by making assumptions. Moreover, output may or may not be produced with decreasing returns to labour. Whether it is or not may be important for population growth, but, again, does not concern us at present.

On the other hand, we are interested in the relationship between output and mortality. On this point, as on others, Malthus was quite explicit. He held that mortality and output were related. When output rose above the subsistence level, mortality fell. And when output fell below the subsistence level, mortality rose. Moreover, output increases have different effects upon a family's mortality depending upon their standard of living. If they are in the higher-income classes, then income has to fall substantially before their mortality rises. Wealthy families, after all, live well above the subsistence level. Poorer families, who live constantly on the edge of subsistence, would be more immediately affected by income changes. Small declines threaten them, while slight increases reduce their risk of death.

It is a matter of taste whether we treat the entire burden of this mortality-output connection as an assumption or as an inference from Malthus's assumptions. In some respects it matters little, since we treat both assumptions and inferences similarly. Both are compared with reality. But because we discuss both tests of assumptions and tests of inferences as bases for rejecting Malthusianism, we distinguish between assumptions and implications. We regard the assertion that there is a close connection between consumption and mortality as an assumption. We treat explicit statements concerning the way mortality declines, following changes in output, as implications.

Economists generally formulate implications from the basic Malthusian supposition in aggregate terms. Mortality is a function of income per head, or depends either on national income or on the level of development. More developed countries exhibit lower mortality than less developed countries. Income growth leads to mortality decline. A more rapid rise in income produces a more dramatic fall in mortality. In this way population growth is determined in part by economic growth. In some countries the relationship is more open to autonomous influences, thus weakening the connection between economics and mortality. But even when weakened they still see mortality as inversely related to income per head. Implications such as these are the basis for many tests of Malthusian theory. Are tests of these implications tests of economists' conceptions of proper Malthusian theory? Indeed they are.

Implications such as these can be found in many economists' writings. Peacock, for

instance, in 1952, explored Malthusian theory.³ He intended to deduce implications from commonly held Malthusian assumptions using the formal methods of reasoning familiar to economists. It was Peacock's contention, that he worked with assumptions commonly held in the 1950s. Malthus's assumptions connecting mortality with consumption also found currency in Boulding's analysis of Malthusianism as a general system.⁴ Assumptions connecting mortality with output or income, in the manner noted above, are repeated in the work of Moes and Minami.⁵ Most of these analyses aimed to explore implications from commonly accepted Malthusian assumptions by the use of geometry, the calculus or some other formal method. Each sees mortality inversely related to income per head.

Somewhat different nuances are given to the theory by Leibenstein. In 1962, he contended: Compared to the other components of population change, the factors that determine mortality trends are reasonably well understood. For our purposes we can divide such factors into two sets: those that are autonomous of the economic aspects of life and those that are functions of income. The autonomous factors, apart from 'Acts of God,' such as floods and storms, are mostly scientific discoveries that reduce the incidence of certain diseases. The often-mentioned effects of DDT and the clearing of the swamps to combat malaria are of this nature. But medical and chemical discoveries require economic resources to put them into effect. Therefore, on the whole, mortality rates seem to depend on the 'standard of life,' that is, on the level of consumption. Hence, it is quite reasonable to posit that mortality rates are a monotonically decreasing function of per capita income, *given the state of public health measures in existence*. But in view of the simultaneous operation of autonomous factors, we have to keep in mind the fact that there are forces, apart from income changes, that may lead to a secular decline in mortality rates.⁶

E. A. Wrigley, in 1969, advocated an analysis of falling death rates essentially similar to Leibenstein's.⁷ Again, there is a connection between income per head and mortality. Again, the relationship is rather loose inasmuch as various other factors also influence mortality.

Malthusian theory as currently formulated leads to the implication that there is a direct relationship between income per head and mortality. Some writers would modify the relationship slightly to allow for the influence of autonomous factors. But the relationship between income and mortality remains.

Tests of these implications generally show the implications to be untenable. Other tests also do not support Malthusian theory.

Strangely enough, few if any authors deny the basic Malthusian assumption connecting mortality with consumption. Perhaps this is because the assumption is all too obviously correct. After all, if one does not eat one will die. The same reasoning holds on the aggregate level. If there is not enough food to go round, then some will die of starvation. Moreover, consumption of fuel, clothing, housing and other goods, too, can reduce mortality. This relationship, while difficult to detect for some specific commodities, is so compelling that it may be held to be an obvious truth. Critics accept Malthus's assumption that there is close connection between consumption and mortality.

³ Alan T. Peacock, 'Theory of Population and Modern Economic Analysis', *Population Studies*, 6, 2 (November 1952), pp. 114–122. See also his 'Population Theory and Modern Economic Analysis, II', *Population Studies*, 7, 3 (March 1954), pp. 227–234.

⁴ Kenneth Boulding, 'The Malthusian Model as a General System', *Social and Economic Studies*, 4, 3 (September 1955), pp. 195–204.

⁵ John E. Moes, 'A Dynamic Interpretation of Malthus's Principle of Population', *Kyklos*, 11 (1958), pp. 58–83; and Ryoshin Minami, 'An Analysis of Malthus's Population Theory', *The Journal of Economic Behavior*, 1, 1 (April 1961), pp. 53–63, along with the comments by Minoru Tachi and Tadao Yoshida which immediately follow the article.

⁶ Harvey Leibenstein, *Economic Backwardness and Economic Growth* (New York: John Wiley & Sons, 1962), p. 159.

⁷ E. A. Wrigley, *Population and History* (London: World University Library, 1969).

Nevertheless, while the basic assumption is universally or nearly universally accepted, reasons continue to be given for rejecting some or all of Malthusian mortality theory. Some writers assert that simpler or more compelling alternatives are ready to hand. Others examine the relationship between short-term fluctuations in output and mortality. In other words, they examine implications from accepted premisses. Ordinarily such studies relate harvest failures to death rates. Yet others compare income per head in various countries with their crude death rates. These tests are similar to harvest fluctuation studies in that both test implications. The difference lies in the use of cross-sectional data rather than of time series. These seem to be the major bases for rejecting otherwise unassailable assumptions. We examine each type of study in turn.

Among studies which reject Malthusianism because more attractive alternatives are available are some papers by Utterström.⁸ Utterström takes various positions. One frequently finds the claim that economic changes need not, or perhaps even cannot, explain Scandinavian mortality. Rather, Utterström holds that climatic fluctuations produced the pre-industrial population changes in these countries. An amelioration in climate produced population increase. Worsening climate led to population decline. Largely on the grounds that (1) climatic change is antecedent to some economic change, and (2) climate explains some mortality fluctuations, Utterström rejects Malthusian mortality theory.

Arguments against Malthusianism on the grounds that European and English mortality fell because of a change in the frequency and/or virulence of epidemics are of a similar nature. Various studies have come to this conclusion. J. D. Chambers, for instance, believes that population in Nottingham and Nottinghamshire changed because the plague no longer struck with such frequency or ferocity as previously.⁹ Reference to the plague as an explanation for England's or Europe's population growth can also be found in the work of Helleiner, Langer, Henry and Tucker.¹⁰ H. J. Habakkuk, apparently relying upon secondary sources, reaches the same conclusion.¹¹ Razzell constructs a somewhat different explanation. Rather than citing the end of the plague or the possibility of a rolling or moving epidemic (as used by Tucker), Razzell calls upon inoculation and vaccination to explain England's population growth.¹² Not all authors conclude explicitly from these studies that Malthusian or 'economic' explanations for population growth are untenable, though some, such as Chambers, are not reticent on that point. But members of this school of thinking use their theories as arguments against Malthus.

Those who rely on climatic or plague changes as explanations of population growth often claim that some explanation, not contradictory with an economic argument, accounts for

⁸ Gustaf Utterström, 'Climatic Fluctuations and Population Problems in Early Modern History', *Scandinavian Economic History Review*, 3, 1 (1955), pp. 3–47; and 'Population and Agriculture in Sweden, circa 1700–1830', *Scandinavian Economic History Review*, 9, 2 (1961), pp. 176–194. In two other articles Utterström repeats the same type of argument but, additionally, relies upon the assertion that death rates did not fluctuate with harvest fluctuations. See 'Some Population Problems in Pre-Industrial Sweden', *Scandinavian Economic History Review*, 2, 2 (1954), pp. 103–165; and 'Two Essays on Population in Eighteenth-century Scandinavia', in D. V. Glass and D. E. C. Eversley (eds), *Population in History* (London: Aldine Press, 1956), pp. 523–548.

⁹ J. D. Chambers, 'The Vale of Trent', *Economic History Review*, suppl. 3 (1957); and 'Population Change in a Provincial Town: Nottingham 1700–1800', in L. S. Presnell (ed.), *Studies in the Industrial Revolution* (London: Athlone Press, 1960).

¹⁰ K. F. Helleiner, 'The Vital Revolution Reconsidered', *Canadian Journal of Economics and Political Science*, 23, 1 (1957), pp. 1–9. Louis Henry, 'The Population of France in the Eighteenth Century', *Population in History*, *op. cit.*, in footnote 8, pp. 434–456. William L. Langer, 'Europe's Initial Population Explosion', *American Historical Review*, 64, 1 (October 1963), pp. 1–11. G. S. L. Tucker, 'English Pre-Industrial Population Trends', *Economic History Review*, ss, 16, 2 (December 1963), pp. 206–218.

¹¹ H. J. Habakkuk, 'The Economic History of Modern Britain', *Journal of Economic History*, 18, 4 (1958), pp. 486–501. Habakkuk here asserts: 'And for the reasons for this [the falling death rate] I think we must look to the historians of climate and disease' (p. 499).

¹² P. E. Razzell, 'Population Change in Eighteenth-Century England', *Economic History Review*, ss, 18, 2 (August 1965), pp. 312–332.

population change. On these grounds they feel Malthusian theory can be rejected. We consider their arguments in some detail later to see whether in fact they do provide satisfactory grounds for rejecting Malthusian theory.

Harvest fluctuations (playing the role of output changes) and their connection with mortality have been studied in some detail. These studies directly test implications drawn from Malthusian theory. Utterström, not relying solely upon climate, also rejects Malthusianism on the grounds that harvest fluctuations are not closely connected with mortality changes.¹³ Drake, in an elementary exercise in parish register demography, arrives at similar conclusions.¹⁴ Further study of other parishes and periods led Edwards to similar conclusions. Edwards comments that 'after 1750, although agricultural deaths occurred with increased frequency, the link between wheat prices and demographic change weakened'.¹⁵ Prior to 1750, however, he did find that mortality fluctuated with harvest failures. This, of course, contradicts Utterström's and Drake's findings cited above. Furthermore, Edwards does not agree that the plague or other epidemics played the role in Norwich's mortality that other authors, such as Chambers, found. Edwards asserts, 'it must be concluded that the Bills offer no clear sign of any "age of massacre by epidemics" in the years they cover'.¹⁶

International comparisons, as opposed to the inter-temporal comparisons made above, also display a low correlation between income per head and mortality. Stolnitz, in 1954, studied 250 life tables.¹⁷ Relating age-specific mortality to development levels, he found evidence which both supported and failed to support the inference that income per head was closely related to mortality. Dividing nations into three categories, he found a reasonably close relationship between income per head and age-specific mortality within each category. But there was little connection overall. In fact, he found that the later a nation developed, the more rapidly its mortality fell relative to income. He ascribed this to the ease with which presently developing nations could import advanced medical technology. He cited the campaign against malaria conducted by Ceylon, a campaign which materially lowered the nation's mortality with little or no attendant income growth. One year later Davis came to the same conclusion. Basing his argument upon somewhat different data, he contended there was at best a very weak connection between a nation's state of development or its income per head and mortality.¹⁸ Demeny, in 1965, claimed that 'any attempt to relate mortality trends to the latter (national income) proves to be largely unsuccessful'.¹⁹ And he continues, 'it can be concluded then, that our failure to introduce any relationship between economic factors and population growth is entirely justifiable'.²⁰

Somewhat different considerations led T. H. Marshall to reject the notion that mortality was related to economic change. Writing in 1929 he did not have available to him the empirical studies cited above. Nevertheless, he concluded that 'we find . . . in England at the end of the eighteenth century, an unprecedented fall in the death rate, caused for the most part, by non-economic forces'.²¹ In 1935 he argued the same position, elaborating upon the causes for mortality decline. Defining economic causes, he noted

¹³ Gustaf Utterström, 'Some Population Problems', and 'Two Essays', *loc. cit.* in footnote 8.

¹⁴ Michael Drake, 'An Elementary Exercise in Parish Register Demography', *Economic History Review*, ss, 14, 3 (April 1962), pp. 427-445.

¹⁵ J. K. Edwards, 'Norwich Bills of Mortality - 1701-1803', *Yorkshire Bulletin of Economic and Social Research*, 21, 2 (November 1969), pp. 94-113.

¹⁶ *Ibid.*, p. 100.

¹⁷ George J. Stolnitz, 'A Century of International Mortality Trends', *Population Studies*, 9, 1 (1955), pp. 24-55.

¹⁸ Kingsley Davis, 'The Amazing Decline of Mortality in Underdeveloped Areas', *American Economic Review*, 46, 2 (May 1956), pp. 305-318.

¹⁹ Paul Demeny, 'Investment Allocation and Population Growth', *Demography*, 2 (1965), p. 210.

²⁰ *Ibid.*, p. 211.

²¹ T. H. Marshall, 'The Population Problem During the Industrial Revolution, A Note on the Present State of the Controversy', *Economic History*, 1 (January 1929), p. 434.

the close correlation between death rates and poverty breaks down when you bring in the agricultural labourer. Mortality in some occupations was extravagantly high, but the facts show clearly that it was the home, and not the job, that controlled the rates. The differences were greater between infant than between adult death rates, and Manchester enjoyed, throughout the period, an unenviable pre-eminence over the more purely factory towns and over the seats of the most unhealthy occupations. The causes [of the death rate] were, therefore, in a sense economic.²²

And this is amplified later when he asserts 'in the nineteenth century, . . . the death rate, responding in the main to the efforts of the doctors, was, on the economic side, governed not merely by the gross production of wealth'.²³

Needless to say, other results contradict those outlined above. The medical historians, McKeown, Brown and Record, fail to find evidence justifying the conclusion that epidemics, for purely internal reasons, became less virulent.²⁴ This, of course, is contrary to the conclusion of such authors as Chambers. Obviously, McKeown, Brown and Record find an ebb and flow of epidemics. But they feel these changes were due to variations in the social and economic environment. In this sense, they hold that the decline of epidemics was connected with economic change. The conclusion is compelling because of their stature as medical historians.

Harvest failures sometimes also apparently led to changes in mortality, other findings to the contrary notwithstanding. Relying upon an abundance of material from the Baltic region, Gille concludes that

all things considered, infectious diseases seem to have accounted on the average for at least 25–30% of the deaths during the last half of the eighteenth century. In one sense, therefore, there were always epidemics, and in years with bad food conditions they were simply intensified.²⁵

Eli Heckscher, using some of the same data as Gille, also finds harvest fluctuations influencing the death rate.²⁶ And H. J. Habakkuk, following the path laid out by Heckscher, finds in some writings the impact of harvest fluctuations. He notes that there is

a marked rise in [the] death rate from 1730–39 and a second slight rise – very slight in Brownlee's figures – between 1760 and 1780. Thus, we have two periods of high mortality, the first commonly associated with an increase in gin-drinking but probably more reasonably attributed to bad harvests of the period, the second and slighter coinciding with a period of bad harvests.²⁷

At least one international comparison also contradicts the conclusions noted above. Ruprecht, concerned with other matters, commented in passing that there is a discernible relationship between death rates and income on the international level.²⁸

²² T. H. Marshall, 'The Population of England and Wales from the Industrial Revolution to the World War', *Economic History Review*, 5 (1935), p. 72.

²³ *Ibid.*, p. 76.

²⁴ T. McKeown and R. G. Brown, 'Medical Evidence Related to English Population Changes in the Eighteenth Century', *Population Studies*, 9, 2 (1955), pp. 119–141, and T. McKeown and R. G. Record, 'Reasons for the Decline in Mortality in England and Wales During the Nineteenth Century', *Population Studies*, 16, 2 (November 1962), pp. 94–122.

²⁵ H. Gille, 'The Demographic History of the Northern European Countries in the Eighteenth Century', *Population Studies*, 3, 1 (June 1949), p. 48.

²⁶ Eli F. Heckscher, 'Swedish Population Trends Before the Industrial Revolution', *Economic History Review*, ss, 2, 3 (1950), pp. 266–277. Heckscher develops an interesting variant of Malthusian theory in this article. One major emendation is the incorporation of stable population theory within the Malthusian framework. This strand of thought appears strongly to influence subsequent work by H. J. Habakkuk. See, for instance, his 'Family Structure and Economic Change in Nineteenth-Century Europe', *Journal of Economic History*, 15, 1 (1955), pp. 1–12.

²⁷ H. J. Habakkuk, 'English Population in the Eighteenth Century', *Economic History Review*, ss, 6, 2 (1953), p. 128.

²⁸ Theodore K. Ruprecht, 'A Comment on John Buttrick's "A Note on Growth Theory"', *Economic Development and Cultural Change*, 10, 3 (1962), pp. 317–319.

Furthermore, the Malthusian implication relating family income to family mortality is sometimes upheld. Henry, using eighteenth-century French parish registers, found that poorer families had higher death rates than wealthier ones.²⁹ Ursula Cowgill, studying parish registers from sixteenth-century York, also found that poorer families had higher death rates than wealthier families.³⁰ Postan and Titow, investigating thirteenth- and fourteenth-century Winchester Estate records, found that harvest fluctuations were not associated with deaths taken on the whole. But when deaths were separated into those for the poor and those for the wealthy they found:

the evidence, in spite of its indefinite lines of demarcation, shows money heriots [paid by the poor] responding much more immediately [deaths rose] and sensitively to harvest failures than the animal heriots [paid by the wealthier parties]. Indeed, our figures suggest that over the greater part of our period animal heriots show no correlation with prices [their index of harvest failures] and that it is only in the first quarter of the fourteenth century – the time of the great famines – that animal heriots respond at all clearly to variations in crops.³¹

Studies drawn from more recent times support these conclusions. Evaluating Donald Cowgill's formulation of transition theory, Satin concludes there are, indeed, mortality differences between rich and poor, and the wealthy appear to fare better.³² This, he holds, is because they can avail themselves of the modern (and some not so modern) medical developments whereas poor families find themselves less able to do so.

Lastly, some authors disagree with Marshall's assessment. He contended that changes in medical practices, the effect of urban as opposed to rural living conditions, and the impact of the working environment, each of which he found was related to mortality, were non-economic in character. G. Talbot Griffith, whose interpretation of England's declining death rate was accepted as standard for more than 25 years, argued that changes in sanitation, medical practices, the availability of hospital and dispensary facilities, and living conditions accounted for the decline in mortality.³³ Contrary to Marshall, Griffith considered these factors to be economic in character. Similar conclusions were pressed by Buer.³⁴ Lockridge, commenting upon mortality in seventeenth-century Dedham, Massachusetts, and its relationship to mortality in England at the same time, concluded that life was 'easier' here than in England and this accounted for the lower mortality.³⁵ And Eversley, surveying the findings of numerous authors, concluded that the causes of mortality were complex, but among them were distinct economic elements.³⁶

Evidence and opinions concerning that evidence are divided on the issue whether economic explanations for mortality are or are not tenable. But the weight of opinion, judging

²⁹ Louis Henry, 'The Population of France', *loc. cit.* in footnote 10.

³⁰ Ursula M. Cowgill, 'Life and Death in the Sixteenth Century in the City of York', *Population Studies*, 21, 1 (July 1967), pp. 53–62.

³¹ M. M. Postan and J. Titow, 'Heriots and Prices on Winchester Manors', *Economic History Review*, ss, 11, 3 (April 1959), p. 410.

³² Maurice S. Satin, 'An Empirical Test of the Descriptive Validity of Demographic Transition on a Fifty-Three Nation Sample', *The Sociological Quarterly*, 10, 2 (Spring 1969), pp. 190–203. Satin tested the version of the transition set forth by Donald O. Cowgill in 'Transition Theory as General Population Theory', *Social Forces*, 14, 3 (March 1963), pp. 270–274. One of Cowgill's contentions was that according to transition theory there would be significant mortality differentials between classes during the early stages of the transition. Satin claims that each of Cowgill's theses tested was 'an accurate prediction for our sample' (p. 203).

³³ G. Talbot Griffith, *Population Problems in the Age of Malthus* (Cambridge: Cambridge University Press, 1925).

³⁴ M. C. Buer, *Health, Wealth and Population in the Early Days of the Industrial Revolution* (London: George Routledge & Sons, 1926).

³⁵ Kenneth A. Lockridge, 'The Population of Dedham, Massachusetts, 1636–1736', *Economic History Review*, ss, 19, 2 (August 1966), pp. 318–344.

³⁶ D. E. C. Eversley, 'Population, Economy and Society', *Population in History*, *idem.* in footnote 8, pp. 23–69.

by the way in which mortality is explained in general population theories (e.g. transition theory) has recently moved towards the view that economic change does not explain mortality change.

II. THE EVIDENCE RECONSIDERED

It may be argued that economic explanations for mortality do not hold water. But the evidence supporting this conclusion is peculiar in at least two very important respects. First, one is struck by the fact that the plausibility of the basic Malthusian conception, that the conditions of life are closely related to mortality, has not been questioned. Starving people died in Malthus's time and they die to-day just as readily. Secondly, implications derived from these perfectly acceptable premisses are often, but certainly not always, challenged as untenable. Sometimes, for instance, harvest fluctuations influence the death rate, and sometimes not. Sometimes income differentials produce mortality differentials, and sometimes they do not. Some international comparisons show an association between income and mortality, and others do not. This peculiar combination of perfectly acceptable premisses and sometimes unacceptable inferences suggests that a simple reformulation of the Malthusian position may be satisfactory. We present such a reformulation below. But first we must deal with one critical position outlined above.

Malthusianism has been rejected on the grounds that other more suitable explanations for mortality are ready to hand. Utterström, for instance, believes climate rather than economic activity determines mortality.³⁷ But Utterström's grounds are insubstantial on several counts. To begin with, he bases part of his argument on the position that climate is antecedent to agriculture (economic growth and change) and, therefore, climate has preference as an explanatory factor. This, of course, is the familiar regression to first causes, a regression which has little if anything to recommend itself. Secondly, climatic explanations can be transformed quite easily into economic ones. Climatic changes certainly were clearly reflected in economic variables such as agricultural output, firewood prices and the like. If such a simple transformation is indeed possible, then to show that climate can explain mortality change does not amount to a demonstration that economic change fails to explain mortality change. To force the argument through that crucial second step, i.e. to show that because climate explains mortality, economic change cannot do so, it is necessary to demonstrate that climate and economic performance are either contraries or contradictory terms. Needless to say, neither Utterström nor any other author has been able to do this. Probably it is impossible since there is the intuitively obvious relationship between weather and agriculture. And lastly, man cannot manipulate climate as he can manipulate economic variables such as unemployment. If economic change can account for even a small part of annual mortality, then an economic theory offers us a vehicle for reducing deaths that a climatic theory does not. Similar remarks can be made, *mutatis mutandis*, concerning each alternative theory covered above.

Different views concerning technical change may explain some divergent analyses noted above. Marshall and Griffith generally agree upon the reasons for England's declining death rate in the eighteenth and nineteenth centuries. But they disagree on whether the factors responsible are economic or non-economic.³⁸ One calls medical advance an economic factor, while the other does not. This dispute should not be settled by looking at a dictionary. Rather we should ask whether we can discuss the consumption of medical goods and services in economic terms. If we cannot formulate a reasonable supply-demand explanation for the consumption of medical goods and services, then these matters are not in any sense economic. To determine whether the consumption of drugs, physicians' time and hospital services can be analysed in terms of supply and demand we should perhaps turn to medical economists. True, they might

³⁷ Gustaf Utterström, 'Climatic Fluctuation' and 'Population and Agriculture', *loc. cit.* in footnote 8.

³⁸ T. H. Marshall, *loc. cit.* in footnotes 21 and 22. G. Talbot Griffith, *op. cit.* in footnote 33.

not give us an unbiased answer. But their conclusions will provide some basis for discussion.

Of course, Marshall would probably have conceded that consumption could be analysed by using supply and demand. No doubt his argument was more concerned with technical change, particularly in medical services. Again, recent work, not available to Marshall in the 1920s and 1930s, suggests that technological advance can be analysed, at least in part, by using expenditures on research and development and profit maximization. If, as some writers suggest, technical change can, indeed, be explained in economic terms, then what was non-economic (perhaps fortuitous) for Marshall has become for us economic in character.

Davis and Stolnitz argue along Marshall's lines.³⁹ Arguments which apply to the Marshall-Griffith difference, therefore, apply *mutatis mutandis* to Davis's and Stolnitz's position regarding such medical events as Ceylon's malaria eradication programme. This, it should be recalled, they consider non-economic on the grounds that the technology had been imported from countries which had recently developed it.

George Stolnitz's study, and its fraternal twin, the analysis by Davis, raise another issue. They correlated income per head with crude death rates as suggested by the deductions made by the economists. Not surprisingly, they found that countries which had developed more recently lowered their death rates faster (as a function of rising income per head) than countries which developed earlier. In effect, they looked for a connection between total consumption on one hand and mortality on the other. Not finding one, they rejected Malthusian theory.

Even if Malthus had suggested such a comparison, it would have been unsound. Malthus stressed the availability of food. Agricultural output was, for Malthus, the important determinant of mortality. But we may take Malthus's use of agriculture as paradigmatic. Increased consumption of *some* goods could reduce mortality. Among these was food. But Malthus, of all demographers, certainly knew that man does not live by bread alone. And it would only be fair to Malthus to contend that he also knew that increased consumption of *some* products led to increased mortality. As a good churchman he no doubt felt that undue consumption of alcohol, say gin, would be detrimental to the public health. And he may have agreed with King James's *Counter-Blast to Tobacco*. Yet other goods can be consumed in greater or smaller quantities and no discernible impact upon mortality should be expected. The consumption of *objets d'art* might fall into this category.

This basic apprehension prevents people from rejecting the Malthusian assumption connecting consumption with mortality. Increased consumption of some goods reduces mortality. In other cases, increased consumption either has no effect upon mortality or else increases it. Furthermore, we know to-day that each such relationship is not always or possibly ever monotonic. More food will improve a starving person's health. But more food will raise the risk of death for the person already overweight. Doctors say a small amount of alcohol may prove beneficial to the person under stress. But the alcoholic should derive little comfort from this opinion. Similarly, the relationship between consumption and mortality may change over time independently of quantities consumed. For McKeown and Brown increased patronage of physicians had a questionable effect upon mortality. And a visit to an eighteenth-century hospital increased one's chances of death (due to the increased chances of secondary infection in an age with no secure knowledge of how diseases were transmitted).⁴⁰ Behind each illustration lies the strong and unassailable assertion that consumption of various goods and services is, indeed, intimately connected with mortality. This surely is a reasonable interpretation of Malthus's assumption and of what each of us knows full well.

But is it an 'economic' analysis of mortality? It certainly can be fitted within the confines of economic theory. We postulate that there are three general classes of goods. Each, when con-

³⁹ Kingsley Davis, *loc. cit.* in footnote 18; George Stolnitz, *loc. cit.* in footnote 17; and T. H. Marshall *loc. cit.* in footnote 21 and 22.

⁴⁰ T. McKeown and R. G. Brown, 'Medical Evidence', *loc. cit.* in footnote 24.

sumed, influences mortality differently. We may call them (1) largely beneficial, (2) indifferent, and (3) deleterious. Increased consumption of beneficial goods can reduce mortality. But pharmacologists tell us too much of anything is bad for one's health. Food is a case in point. Hence, the relationship between mortality and consumption is U-shaped, perhaps a U with a long flat bottom. Whether increased consumption leads to reduced mortality, to no change in mortality or to increased mortality then depends upon the initial consumption level. Changes in the consumption of class-two goods have little or no influence upon mortality. And there is a monotonically increasing relationship between mortality and the consumption of deleterious goods. These relationships are explained using biological theories. This aspect of mortality, therefore, is essentially biological.

Mortality is a weighted average. It is derived from the mortality figures given by actual consumption of each commodity class. As consumption within each class changes, the weighted average either changes or remains constant, i.e. mortality stays stable or changes. Herein lies one economic aspect of mortality. For consumption within each commodity class can be explained by using supply and demand theories.

Demand, of course, is influenced by income. To this extent the Stolnitz–Davis argument is sound. For they argue from national income per head to mortality. But we find in demand theory reasons for believing their analysis to be far from sound. For one thing, there may be changes in the distribution of national income. In that case there will be changes in consumption levels for some families, and hence changes in their level of mortality. This means there may be changes in national mortality figures while there is no change in national income per head. Moreover, other factors influence consumption. Demand theory tells us other prices are important, that social custom is important and there are other considerations as well.

The influence of social custom is cited by Oddy, for instance. When examining nineteenth-century English diets, Oddy noted that the average low-income Englishman could have had a much more nutritious diet at a much lower price if he had been prepared to change his pattern of consumption.⁴¹ According to Oddy, the people did not understand dietary requirements and even if they had, it *is still* not certain that better diets would have resulted. To-day's cigarette consumption is a case in point. Lethargy, social customs and the like often intervened. Consumption, after all, is a social activity in many of its facets. He asserts:

the investigation of these working-class families in the 1890s contributes to the paradox between the rise in real wages and the low standards of nutrition and health which observers noted at the end of the nineteenth century. It may suggest that we should be less certain that the rise in real wages or the fall in food prices led to increased food consumption, an argument which ignores environmental, physiological, and psychological factors in working-class life.⁴²

Economists recognize that these influences exist. They do not seek to explain them. Very probably they constitute the sociological aspects of mortality. But more about the economic ones.

Consumption and hence mortality can change because of shifts in supply schedules as well as demand schedules. There again are various reasons for such supply shifts. Changes in labour, land and capital prices influence supply schedules and hence consumption. Changes in an industry's structure, say from competitive to either a cartel or a monopoly, ordinarily will reduce consumption. A glance at the United States drug industry or at the American Medical Association's limitation on the number of physicians will reveal the effects of such monopolistic or cartel practices. We need only look at drug prices and at the number of doctors per 1,000 population to see this. These are all economic considerations for they can be analysed within economic theory, probably far better than within some other theory.

⁴¹ D. J. Oddy, 'Working-Class Diets in Late Nineteenth-Century Britain', *Economic History Review*, ss, 23, 2 (August 1970), pp. 314–323.

⁴² *Ibid.*, p. 332.

Technical change, of course, also enters supply theory, and hence this interpretation of Malthus. We find it occurring in two distinct roles. First, of course, technical changes shift supply schedules. Again, Stolnitz and Davis propose a reasonable explanation for some mortality changes. But they err when they assert that this conclusion shows Malthusian theory to be unacceptable. Moreover, there is some loss in analytic or explanatory or predictive power when the divorce between technical change and economic theory is consummated, as divorced it must be according to the Stolnitz–Davis argument. For to the extent that technical change can be explained in economic terms, and we noted this connection above, to eschew economic analysis amounts to throwing away some insight into mortality change. Furthermore, economists should be able to explain how supply schedules will shift given identifiable technical change. And when these shifts are coupled with demand schedules and the now explained consumption is connected with mortality, we can predict mortality changes resulting from technical change.

There is another dimension to technical change. It is both economic and biological. That is the relationship between consumption of different commodities and mortality. When technical change introduces a new commodity, such as improved medical care based upon novel and more powerful theories of human health, then we would expect to identify a new consumption–mortality relationship falling into either class one, two or three. As the relationship would relate consumption and mortality, there are these two sides to this technical change, both biological and economic.

In summary, this interpretation of Malthus has its economic aspects, its biological sides and its sociological characteristics. All are necessary to this mortality theory. The biological parts explain the relationship between consumption and mortality, the sociological elements explain the economists' preferences or consumption patterns, and the economic side would explain changes in relative consumption levels through supply and demand as well as explanations for technical change.

What implications arise from this reinterpretation? There can be three relationships between income and mortality. If consumption of largely beneficial goods is so small that some people could reduce their mortality by increasing consumption, then income growth for those people will reduce mortality. If all find their mortality reduced to its minimum, then increased income either will not change mortality or will increase it. Hence, income growth can cause reduced mortality, no change in mortality or an increase in mortality. The outcome can be determined given knowledge of (1) consumption levels, (2) postulated income growth and (3) the relationship between consumption and mortality. That relationship probably is of a simultaneous-equation nature, an equation which takes into account a society's complete consumption pattern. But we may calculate changes in a society's death rate, given small changes in the consumption of any single commodity group knowing only (1) original consumption, (2) changes in consumption, and (3) the schedule relating consumption of that commodity to mortality changes. We sum these changes over individuals. Similarly, we can deduce mortality differentials between income classes knowing consumption levels of a specific good for both rich and poor, and given the relevant schedules. Yet, if consumption patterns differ widely for a variety of goods, the interaction between commodities may invalidate the changes in death rates computed by using a single commodity group.

Two classes of studies which have provided bases for rejecting Malthusian mortality theory have been discussed. Utterström's arguments have been rejected. The Davis and Stolnitz studies have been reconciled with our reformulation of Malthusian theory. What can we say about harvest studies and research which investigated the relationship between mortality and income between wealthy and poor families? It seems fair to say that the mortality explanation advocated here contains sufficient flexibility to account for the findings discussed above. Furthermore, the contrary conclusions derived by various studies should not be unexpected. Different regions of England were investigated, and as England was not a homogeneous country,

variations in the sensitivity of mortality to income change should be expected. For instance, some regions were surely wealthier than others. Moreover, these studies related to different periods, and with the passage of time we would expect the observed relationship between income and mortality to change. But rather than investigate the properties of each study individually, we will present a sketch of English demographic history into which the various studies may be fitted.

Postan and Titow found, for the fourteenth and fifteenth centuries, a discernible relationship between harvest fluctuations and mortality response.⁴³ When harvests were poor, mortality rose almost immediately for the poorer section of the population. On the other hand, there appeared to be no converse response when a good harvest had been collected. Poor English families appear to have lived on the edge of subsistence, at least in so far as that term refers to rising death rates when incomes fell. But the relationship was not linear, for income increases led, to our knowledge, to no reductions in mortality beyond that achieved with a subsistence income.

Matters seem to have been somewhat different for wealthier families. They did not experience increased mortality when harvests were poor. Instead, they appear to have lived much as usual. Yet, when bad harvest followed bad harvest, their mortality rose. Hard times affected only the poor, but bad times were felt by all. Keeping in mind Heckscher's data on caloric intake (sixteenth-century Swedes consumed about 5,000 to 7,000 calories per day), it seems likely there was some fat the wealthy could trim. We conjecture that we observe in their mortality response the ability to make normal grain purchases during one or two bad harvests, because of their greater buying power. But stocks became exhausted if bad times continued for too long. Similarly, of course, they did not experience any continuous decline in mortality when good harvest followed good harvest.

While Postan and Titow's data do not permit any hard and fast assertions on similarity of death rates, it would appear the poor and the wealthier classes experienced similar death rates during good times, different rates during moderately bad times and possibly somewhat more similar rates again during very bad times. Increases in national income per head, therefore, should have led to little reduction in mortality except in so far as they would enable all members of society to survive bad harvests. Reductions in mortality on a year-to-year basis would have to await technical changes, changes which would reduce mortality further.

Evidently such changes did indeed occur. For Cowgill found that wealthier families in sixteenth-century York did have lower mortality than the poorer.⁴⁴ What these changes were remains to be seen. But unless these differences in mortality were associated with such factors as urban-rural differences, which they may have been, income growth should have led to reductions in mortality. Possibly changes in house design and construction along with the spread of knitted clothing may account for some of this revision of the fourteenth- and fifteenth-century prognostication. In all likelihood, income did rise during the sixteenth century. It was, after all, the time of what has become known as the First Industrial Revolution. That population did rise we can be relatively certain. What the nation's crude birth and death rates were we do not know. But by 1700, according to available information, the crude death rate was near the mid-20s. Griffith would have it be 26 per thousand for the year 1700.⁴⁵ For a pre-industrial society this is, indeed, an impressively low rate. It is difficult if not impossible to

⁴³ M. M. Postan and J. Titow, *loc. cit.* in footnote 31.

⁴⁴ Ursula Cowgill, *loc. cit.* in footnote 30.

⁴⁵ G. T. Griffith, *op. cit.* in footnote 33, p. 34. John Brownlee in 'The History of the Birth and Death Rates in England and Wales taken as a whole, from 1570 to the Present Time', *Public Health* (June 1916), pp. 211-222, and (July 1916), pp. 228-238, puts the rate at 28.6. The agreement is striking, considering the possibilities. Even taking the urban-rural rate differentials into account, the urban rate is quite reasonable while the rural rate is very low. See David J. Loschky, 'Urbanization and England's Eighteenth-Century Crude Birth and Death Rates', *The Journal of European Economic History*, 1, 3 (Winter 1972), pp. 697-712. Probable changes in the population's age structure result, according to private computations, in little change in the adjusted rates.

believe that fourteenth-century England enjoyed such a low crude death rate. And if it did not, then income growth and technical change during the intervening centuries must have been associated with a reduction in mortality.

But there is little evidence that mortality continued to fall *pari passu* with increased income during the eighteenth century. Estimates of eighteenth-century crude death rates as well as income per head are subject to an unknown but possibly considerable degree of error. Consequently, conclusions must be guarded. Nevertheless, crude death rates do not seem to have begun their well-known decline until mid-century if not later.⁴⁶ And yet Deane and Cole estimate that real income in 1750 was about 20 per cent above that in 1700 while it had risen to 30 per cent above by 1760.⁴⁷ These certainly are not inconsiderable increases. These data suggest there was a plateau in mortality similar to that experienced during the fourteenth and fifteenth centuries, but probably noticeably lower. Then, with continued growth in income, and, of causal significance, coincidentally with improvements in technology, mortality began to fall and continued to fall during the nineteenth century. This view of the eighteenth century is particularly appealing because it is consistent with the views of one informed and concerned contemporary, the Reverend Malthus. After all, he claimed that income growth above the subsistence level would *not* progressively lower the crude death rate. This conclusion was undoubtedly based upon an historical argument in part and such an argument would include the early eighteenth century as revealed in records and folk wisdom. Lastly, this view is compatible with the computations of Yule who claimed the country began a period of dynamic population change probably in the latter eighteenth century.⁴⁸

Depending, therefore, upon whether the records were examined during a period of death rate stability, slow decline or rapid decline (rapid or slow with respect to income growth), we would expect to find mortality highly sensitive, sensitive or insensitive to harvest variations. We would also expect to find the wealthy with similar or lower mortality, again depending upon the period chosen. To repeat, this chronology would have income growth associated with mortality decline between the fifteenth and sixteenth centuries (perhaps into the seventeenth as well) and in the latter portion of the eighteenth century. Meanwhile the fourteenth and fifteenth centuries as well as the late seventeenth and early eighteenth centuries should have seen little of such a relationship. This changing relationship between income growth and mortality decline, of course, would be reflected in varying population growth rates. Whether these obviously tentative conclusions will be supported by further research remains to be seen. At present the evidence which has been sifted is inadequate to identify all the relationships involved but further work can be expected to overcome this difficulty.

III. CONCLUSIONS

Three types of studies were introduced to explain observed changes in demographers' views on the relationship between economic change and mortality. We have argued against the grounds advanced by such authors as Utterström who would replace economic explanations with climatic ones. Using a reinterpretation of the Malthusian apprehensions, we contend that studies such as those by Stolnitz and Davis are entirely consistent with an economic interpretation of mortality change. Their data in no way give adequate grounds for rejecting an economic explanation formulated in the terms outlined above. And, lastly, studies of harvest variations and differences in mortality experienced by various income classes are also perfectly consistent with this explication of Malthusian theory.

Unfortunately, this explanation is not so easily reconciled with currently accepted demo-

⁴⁶ See David J. Loschky, *loc. cit.* in footnote 45.

⁴⁷ Phyllis Deane and W. A. Cole, *British Economic Growth: 1688-1959* (Cambridge: Cambridge University Press, 1967), p. 78.

⁴⁸ G. U. Yule, 'The Growth of Population and the Factors Which Control It', *Journal of the Royal Statistical Society*, **88**, 1 (June 1925), p. 9.

graphic theories. Unquestionably, transition theory is inconsistent with this explanation for mortality decline. There is in this interpretation no necessity for a single fall in mortality. In fact, it is much more likely that there will be a series of falls in mortality. And between these periods of decline there will be plateaux. During these times of unchanging mortality, income growth will have no influence upon mortality. This being the case it is not possible to incorporate this 'economic' explanation for mortality within transition theory as it now stands. To say this is to argue against this mortality theory. But the argument rests solely upon the acceptability of transition theory. For if transition theory were unacceptable, then the fact that it cannot be integrated with our economic theory of mortality will not count against our work. And transition theory might not be entirely unassailable. Other work suggests that that theory is not nearly as useful as is often supposed, and, therefore, not nearly as acceptable as may be supposed. For it has been shown that transition theory contains so few forcing principles that explanations for any population's growth or decline are relatively tautological at best, and downright deceptive at worst.⁴⁹

On the other hand, this view of mortality is completely consistent with logistic theory. Whether this recommends what we have done or constitutes grounds for rejecting this interpretation of Malthus is a matter of some debate.

⁴⁹ David J. Loschky and William C. Wilcox, 'Transition Theory: A Forcing Model?', *Demography*, 11, 2 (May 1974), pp. 215–226, 'A Reply to Kammeyer and Skidmore', *Demography*, 12, 2 (May 1975), pp. 351–360.