

## Forum: *Fatal Years*

This issue of Forum is devoted to a discussion of Samuel Preston and Michael Haines's *Fatal Years: Child Mortality in Late Nineteenth-Century America* which was published earlier this year by Princeton University Press.



## Major new evidence on health transition and its interpretation

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Recent interest in the social determinants of health has been driven to a marked extent by the discovery that, in the Third World, there are wide differentials in child survival by mother's education and that these differentials remain after controlling for a range of economic factors and distance from health facilities (Orubuloye and Caldwell 1975; Caldwell 1979; Cochrane, O'Hara and Leslie 1980; Caldwell and McDonald 1981; Hobcraft, McDonald and Rutstein 1984; United Nations 1985; Cleland and van Ginneken 1988; Cleland 1990). This is the chief focus of nearly half the papers in *Selected Readings in the Cultural, Social and Behavioural Determinants of Health* (Caldwell and Santow 1989) and it formed a major section of the proceedings of the first health transition workshop (Caldwell et al. 1990).

Many assumed that similar differentials probably existed during the past in the West. We had until recently no way of testing this assumption. This situation has now changed because Samuel Preston and his colleagues have located and sampled the United States 1900 and 1910 censuses which included questions on child survival, and have analysed data on the survival of previous children from the US Birth Registration Area (BRA) for 1923-1929. They have reported their findings in three papers and one book which were written in the following order: Preston (1985), Haines (1985) and Preston and Haines (1991), all written when only the 1900 census data were available; and Ewbank and Preston (1990) which draws upon both the 1900 and 1910 censuses and the 1923-1929 birth registration data. Because the data are retrospective each source describes the mortality situation approximately five years earlier. The American data of almost a century ago record only literacy or illiteracy but plausible assumptions have been made that this equates with either 4+ or 7+ years of schooling with reassurance provided by the fact that the two different assumptions produce rather similar differentials.

To our surprise - although with the benefit of hindsight I feel we should have foreseen it - we have learnt that the differentials in child survival by mother's literacy in the West a century ago were smaller than those in the contemporary Third World at the present day. One could split straws about measures and definitions, but it is unlikely that one's conclusion would differ significantly from the following summary statement by Preston and Haines (1991:201-2):

Multivariate results show the contrast even more vividly, although the absence of identical variables in the different countries means that exact comparability in specification cannot be achieved. Nevertheless, such standard factors as educational attainment (or literacy) of mother and father, occupation of father, and urban/rural residence are available in all the developing-country data sets as well as in the U.S. A well-known result is that mother's education or literacy tends to retain a powerful effect in developing countries even after all other socioeconomic variables are controlled (e.g., Cochrane 1980; Cleland and van Ginneken

1988). Such a result is also observed in the developing countries examined here. The United Nations study (1985:56) [written by Barbara Mensch, Harold Lentzner and Samuel Preston], which also uses weighted least squares regression of a woman's child mortality index, finds that the average coefficient for mother's years of schooling in these countries, once all other variables are controlled, is about 0.34. If the illiterate have an average of one year of schooling and the literate an average of eight - reasonable figures in the United States in 1990 - such a coefficient would imply that the literate should have about 25 percent lower mortality. But the coefficient on mothers' illiteracy in the United States ... ranges between .06 and .14, only a quarter to a half of the expected effect for developing nations in the later twentieth century.

Preston and his colleagues sought other late nineteenth and early twentieth century Western data sets and found that most of the few located supported their finding that mothers' education had little impact on their children's chances of survival. This was the case in Baltimore in 1911-15 (Rochester 1923), although admittedly only infant mortality was examined, and, if one really can draw the same conclusion from the fact that the impact of maternal literacy on infant survival was not reported, in the other seven American cities that were studied at the same time (Woodbury 1926). If social class could be approximately equated with mothers' education, then it was true also of Copenhagen in the 1870s (Matthiesson 1972); but not of England and Wales during the first decade of the present century, where the children of urban unskilled workers, textile workers and miners had twice as great a chance of dying during the five years following birth as the children of the professional and higher white collar classes (Haines 1985; Preston and Haines 1991: 177-98). Preston (1985:383) at first thought this might be partly a reflection of the passage of a decade from the situation recorded by the US 1900 census at a time when child mortality was declining steeply and the new health knowledge was spreading rapidly, but the US 1910 census failed to show occupational differentials opening up to English levels (Ewbank and Preston 1990). Such differentials were apparently attained in America by the end of the First World War, but the really low child mortality was largely the preserve of the professional classes: physicians, dentists, veterinarians, judges and lawyers (Ewbank and Preston 1990). Furthermore this change can be demonstrated only by comparison with a new data source, the BRA, and it is by no means clear that the differentials which widened so dramatically between the 1910 census and the 1923 BRA data, child mortality 1905 to 1918, continued to widen once the sole source is the BRA presenting child mortality over the admittedly short period, 1918 to 1924.

The major concerns and conclusions of Preston and colleagues seem to be the following. First, the child mortality levels in the United States in the 1890s and the 1900s were higher than were warranted by the discoveries already made with regard to the role of germs in transmitting disease. This showed that the general populace was not displaying the kind of awareness of the dangers of infection that available information should have provided; it also showed that the medical profession and governmental institutions were failing to implement policies and programs indicated by the new knowledge. Second, the lack of substantial differentials in child mortality by mother's literacy or father's occupation showed that not even the better educated upper classes could protect themselves or provide community leadership in the health area in the way that educational differentials in the contemporary Third World show that elites there are now doing. There was in America at the turn of the century a failure either of education or of community outlook that no longer characterizes the community, or at least its elites, in the Third World and no longer characterized the British professional classes in the first decade of the present century. The United States moved from this situation in the second and third decades of the present century as the children of the professional classes experienced a steep mortality decline, evidence that their parents were becoming more aware of the dangers of

infection and other causes of death and changing their behaviour in the area of child care accordingly. This situation now exists in the Third World, certainly in its more educated population. Third, by far the greater part of mortality decline then and since has been occasioned by improvements in standards of living and nutrition, more comprehensive public health measures such as better sewerage systems and water supplies together with increasing vaccination coverage and better child delivery services; also by behavioural change which has led to parents providing children with better health care governed by greater foresight. The growth of biomedical scientific knowledge in the curative area and the interaction of medical services with sick children and their parents have played only a minor role. The latter point is never put as explicitly as this. The curative role of medicine is largely ignored. The following probably represents Preston and Haines's (1991: 208-9) most comprehensive summary statement and it appears to cover the whole period up to the 1990s:

We believe that McKeown [e.g. 1976] is correct that the mortality decline since the middle of the nineteenth century owes little to specific drugs and medicines. In fact, McKinlay and McKinlay (1977) have replicated McKeown's demonstrations in the United States during the twentieth century. But the new understanding of infectious disease processes led to many other forms of innovation besides medicines. Public-health officials had new and vastly improved criteria to use in cleaning up water and milk supplies, and a much stronger rationale for their work. And individual parents had access to many new, or newly justified, methods for reducing death risks in the home: boiling milk and sterilizing bottles, methods first introduced in the 1890s; washing hands before preparing meals; protecting food from flies and other sources of contamination; isolating sick family members; and so on. They also had access to physicians who were better equipped to deal with the hazards of the birth process and to render sensible advice on health maintenance.

Apart from obstetrics, the physicians' main role seems to have been to reinforce preventive behaviour. The position was stated more broadly, however, in an earlier summary which also contrasted the situation with that in contemporary developing countries (Preston and Haines 1991: 206-7). It provides the clearest reference in all these writings to specific techniques.

So the failure to achieve satisfactory mortality conditions in the United States at this time [1890s] was widespread. It extended to the literate, to professionals, and especially to urban residents. In our view, it principally reflected the shortage of specific techniques that could be used to reduce the incidence and severity of infectious diseases as well as a failure to implement some of the techniques that had recently become available. In developing countries today, the mother's education or literacy appears to retain such a high degree of explanatory power because it is associated with such health behaviors as vaccination of children, maintenance of hygienic conditions in the home, and receipt of professional health care for maternity and for child illness (Cleland and van Ginneken 1988; Rutstein et al. 1988). But vaccines existed only against smallpox in 1900, and the care that women and children received from physicians or midwives was often misguided and uninformed. Perhaps most important, there was still only a dim appreciation that the transmission of the major childhood diseases could be blunted by simple preventative public and private measures involving milk, water, food, handwashing, isolation, and the like. A growing recognition of the value of these measures is likely to be a major source of rapid declines in American child mortality in the first three decades of the twentieth century (Ewbank and Preston 1989).

This last citation is closer to my position than most of the other interpretative text in the various writings, and I may be overestimating the gap between us and its implications for the interpretation of

health change and for policies in the contemporary Third World. In order to provide Preston and colleagues with a chance to affirm or deny this and to restate their position, I will summarize my position here.

I do not consider it at all surprising that Americans at the end of the nineteenth century had not more quickly seized the opportunity to convert the implications of the germ theory into lower child mortality than they did, or even that their professional classes failed to do so. It probably could hardly have been otherwise. Because the modern world, its economy and its professional and other institutional structures were still evolving, there was little of the present health institutional structure for quickly and adequately testing new findings and following up their implications, let alone ensuring that this tested knowledge became part of governmental and public thinking and part of the health curricula of medical schools. The reforms of those schools had to await the second and third decades of this century. There was probably good reason for the public and health professionals to exhibit some caution and some scepticism about the new discoveries. The earlier miasmatic theories had also been supported by the leading professionals of the time and had received experimental support to the extent that the public health measures based upon them were effective. Boiling milk was likely to kill germs but no one knew what additional adverse effects it might have; such doubts led the highly professional Royal New Zealand Society for the Health of Women and Children (cf. the praise of Woodbury 1926:179 cited by Ewbank and Preston 1990:128) to advocate a laborious home pasteurizing system well into the 1920s.

Far from the American society of that time being less intellectually receptive to the new science-based germ theory and its consequences than the contemporary Third World, the opposite was probably the case (at least in areas that I know well, sub-Saharan Africa and mainland South Asia). Both the new medical discoveries and the scientific philosophies and attitudes that produced them were products of the long evolution of Western society as it grew richer before and during the Industrial Revolution. There may have been poorer and more ignorant people who knew less about science, but they had no deep suspicion of scientific medicine and public health and no alternative theories of disease causation. This was certainly the case in Australia in the second half of the nineteenth century, a society probably with more parallels to the United States than England had: an immigrant population; schooling levels somewhat lower than America's; fairly egalitarian; and, with per capita incomes higher than in the United States until around 1890, no submerged lower class. There was almost universal respect and trust for the public health services while there was probably excessive confidence in medical science, part of a broader respect for all science (P. Caldwell 1966). Only the better-off could afford to take their children frequently to a physician, but they might not have gained much by doing so, and in real emergencies there were mechanisms for getting treatment for the children of the poor. Parents did their best within the limits of the society's scientific knowledge, to which they had reasonable access, to ensure their children's survival, a situation which may have existed much earlier in the West (van de Walle and van de Walle 1990).

The main reason for the small differentials at the turn of the century in American child survival rates by mother's literacy or father's occupation was probably not any inexplicable level of ignorance or wrong-headedness on the part of the literate or the professional classes, but the fact that even the illiterate and the working class believed that disease was caused by earthly forces that might be warded off by commonsense and protective care (even if the existing level of knowledge meant that this was often misdirected), and that they were encompassed within the public health and medical systems. Parents of all types believed strongly that they had a personal responsibility for the survival of their own children. They were characterized, as Simons (1989) puts it, by an *internal locus of control*. In these circumstances it was probably not surprising that the addition of literacy in the path analysis of factors

affecting child survival in the 1900 census data explained little additional variance (Preston and Haines 1991:174). It was probably not an appropriate behavioural care marker in that society; the appropriate ones - perhaps psychological or measures of intelligence or outlook - would not have been collected. Nor is it surprising that present-day margins are small (Caldwell and Caldwell 1991).

The situation in much of the Third World is very different (Caldwell, Reddy and Caldwell 1983). Large numbers of people have alternative explanations for many illnesses, involving not pathogens or infection but punishment for sins or machinations by others. Admittedly, many people can operate simultaneously in both modern and traditional health systems, but they are more likely to favour the former first, and unreservedly if they are educated. They are certainly more likely to follow its precepts at home if they are educated. In addition, the educated are more likely to employ the modern health system because school has identified them in their own minds and in those of others with that system. Large numbers of mothers, especially the less-educated, also feel or are made to feel that the responsibility for taking action about a child's sickness is either a matter for others or in the domain of non-human forces. In rural areas of India many of the ill are treated by doctors with minimal or no training in modern medicine - even by government medical officers with a single year in a modern medical school. It would be hard to argue that many of these doctors are better trained than American ones even at the beginning of the century, but they do have access to modern drugs. It is probably no accident that some of the most educated and socially transformed contemporary Third World countries, such as Sri Lanka, Korea, Malaysia and Jamaica, most resemble the United States of a century ago in that child survival differentials by mother's education or father's occupation are small (cf. the data in Hobcraft et al. 1984:198-201).

It is very difficult to conclude what was meant by the wider differentials than in turn-of-the-century America found in child survival by father's occupation in the 1911 census of England and Wales and the 1923-29 US Birth Registration Area data. It certainly did not mean a long-term trend toward the situation in the contemporary Third World. It was probably the product of some intensification of behavioural and care factors as a result of an even greater belief in science and personal responsibility as a result of the growth of a more scientific and educated society, the activities of infant welfare movements, and the decline in family size. These hypotheses are supported by the exceptional steep decline shown by Ewbank and Preston (1990) among the children of the higher professions rather than the rich in general. But it probably also meant that by 1906 in Britain, and certainly by 1918-23 in the United States, modern medicine had more to offer, and, in spite of the fact that Ewbank and Preston (1990:141-2) could not show income to be significantly related to the amount of the decline in relative child mortality in the US during the first quarter of the present century, that the professional classes and other upper socio-economic groups could buy more of it. In contrast, in most of the Third World countries employed for comparison, health care was cheap or free during the 1970s.

One other point that does not come through very clearly from these works is that the health and medical systems evolving in America, Britain and other industrialized countries at the beginning of this century were a single system which has increasingly developed and become available around the globe. This contributes to sanitary, vaccination and obstetric services. It would be hard to argue that it does not also make a major contribution to saving contemporary Third World children's lives through curative services, especially where it is egalitarian and offers easily accessible services to the urban and rural poor and illiterate. One type of evidence is provided by contrasts of districts in the Third World with and without medical facilities (Orubuloye and Caldwell 1975). Another is the fact that in those countries where comprehensive curative services were spread in a short period through whole populations, mortality, including child mortality, declined steeply, even in circumstances where public health programs did not take a quantum leap at the same time (Halstead et al. 1985; Caldwell 1986).

This happened, for instance, in Sri Lanka in 1946-53, not only in the Dry Zone where the DDT campaign against malaria was important, but also in the densely settled south-west where malaria had not been a significant cause of death. I doubt if the vast rise in life expectancy this century in industrialized countries from around 50 to 75 years can be explained except by allocating a major role to biomedical curative medicine. I believe also there can be no other explanation for the jump in life expectancy in Sri Lanka and Kerala, both societies having long demonstrated sensitivity to child sickness and with strong feelings of maternal responsibility for child survival (Caldwell et al. 1989; Sushama 1990), from the upper and lower thirties respectively in the early 1930s to around 70 years now. In this regard, I am a follower of the earlier Preston (1975).

Let me summarize my misgivings with the emphases, as I read them, in the interpretations provided by Preston and his colleagues. I believe that the extraordinarily broad differentials found in developing countries during the 1970s in child survival by maternal (and usually parental) education can be shown to be the product of two processes: (1) the importation of a global health system, developed first in the West and based on biomedical discoveries, which explained ill-health quite differently from indigenous explanations, and which underlay the provision of both the new preventive and curative health services; (2) the importation of a modern educational system, developed in the West and laden with assumptions about the truth of science, personal responsibility for ensuring health, and the need to employ and co-operate with other modern institutions such as health systems (cf. Caldwell 1980 on the hidden agenda in Western-derived education). The greatest gains made in Third World health, and the greatest potential gains yet to be realized, are from the product of these two forces. Thus, policy-makers would be well advised to make considerable and balanced investments in both health services, democratic curative ones as well as preventive ones, and education, especially girls' education. I do not think we should anticipate finding equally large differentials in child survival by mother's literacy or education or father's occupation in the earlier West and we should not expect to find the West developing far in this direction over the last hundred years. Although parental behaviour is undoubtedly an important factor in child survival, I do not think we should underestimate the importance of biomedical advances during the present century or the extent to which parental behavioural or care factors in America during the twentieth century or in the contemporary Third World achieve their impact on child survival by interrelating with curative services.

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## McKeown's mistake

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A recent publication on child mortality in late nineteenth-century America by Preston and Haines (1991) concluded: 'We believe that McKeown is correct that the mortality decline since the middle of the nineteenth century owes little to specific drugs and medicines'. In 1976 Thomas McKeown, Professor of Social Medicine in Birmingham, England, published an important monograph, *The Modern Rise of Population*. It was based on changes in mortality in England and Wales from 1838 to 1970, and it had a major effect on thinking about health, not only historically and parochially, but currently and globally (McKeown 1976). While his data from the nineteenth century and the early twentieth century provided support for his conclusions, McKeown's mistake was that he extrapolated his conclusions, for example, 'Medical measures of immunization and treatment were relatively ineffective; they were also unnecessary', to the developing world of today.

The components of McKeown's thesis (1976:152,153) were:

- The increase of population is attributable not to an increase in fertility but to a decline of mortality.
- The decline of mortality was due essentially to a reduction of deaths from infectious diseases.
- Exclusion of a fortuitous change in the character of infectious diseases and of immunization and therapy leaves one other explanation for the reduction of mortality, namely, improvements in the environment.
- The most acceptable explanation of the large reduction of mortality and growth of population which preceded advances in hygiene is an improvement in nutrition due to greater food supplies.

McKeown's belief that 'improvement in nutrition was the predominant influence on the fall of mortality in the eighteenth and nineteenth centuries' was in keeping with the widely held belief that malnutrition is the basic underlying cause of death from infectious diseases. The studies of Mata over the last two decades, however, have clearly described the converse, that infection is a major cause of malnutrition, particularly in infants and children (Mata 1978). For the latter half of the nineteenth century, McKeown found that 'hygienic measures such as purification of water, efficient sewage disposal and improved food hygiene had an effect, especially on intestinal infections'. He saw little or no effect of medical measures until 1935 when 'the first powerful chemotherapeutic agents came into use ... supplemented by improved vaccines'.

In 1986 the World Health Organization published a report of a committee chaired by McKeown on health-research strategy for Health for All by the Year 2000. It emphasized 'control of disease origins' rather than 'intervention in disease mechanisms' (World Health Organization 1986). This led McKeown to conclude that 'what is needed for achievement of an acceptable minimum standard of health throughout the world is not so much basic knowledge as effective managerial procedures and, above all, political will'. Two major aspects of his arguments and beliefs have led me to the conclusion that his extrapolation of historical data to the present global health situation was a grave mistake. The

first was that he set up a straw man with his vehement claims that medical measures were relatively ineffective and unnecessary. The second was the polar, neo-Luddite position he assumed in the continuing biomedicine-public-health debate.

With respect to the former, many outstanding biomedical researchers and clinicians have been in fulsome agreement with McKeown. Kass (1987) observed that the decline in mortality due to infectious diseases in the 19th century was independent of the introduction of specific preventive and therapeutic measures. Thomas's (1983) *The Youngest Science* graphically described the state of medicine in the early twentieth century. In the chapter entitled '1911 medicine', Thomas observed that his father's medical training was influenced by 'the school of therapeutic nihilism' of Sir William Osler, in reaction to medicine as previously practised.

The medical literature of those years makes horrifying reading today: paper after learned paper recounts the benefits of bleeding, cupping, violent purging, the raising of blisters by vesicant ointments, the immersion of the body in either ice water or intolerably hot water, [and provides] endless lists of botanical extracts cooked up and mixed together under the influence of nothing more than pure whim (Thomas 1983:19).

He then described the revolution following Osler's assertion that 'most of the medicines in common use were more likely to do harm than good'. In '1937 internship' he observed that hospitals were 'simply custodial'. 'Whether you survived or not depended on the natural history of the disease itself. Medicine made little or no difference'. The only genuine and effective treatments at the time were the use of specific antiserum for lobar pneumonia, insulin and intravenous fluids for diabetic coma, bleeding, digitalis leaf and oxygen for acute heart failure and months and years of treatment with arsenicals, mercury and bismuth for syphilis.

In his latest book, *The Fragile Species*, Thomas describes 'the unequivocal successes' since the late 1930s (Thomas, in press). These include

miliary tuberculosis, tertiary syphilis of the brain and heart, poliomyelitis, the childhood contagions, septicemias, typhoid, rheumatic fever and valvular heart disease, and most of the other great infectious diseases, now largely under control or already conquered. This was the result of the second big transformation in medicine, starting about 50 years ago with the introduction of the sulfonamides, penicillin, and the other antibiotics, gifts straight from science. The revolution continues in full force, thanks to what is now called the biological revolution, ... with new technologies of fantastic power, such as recombinant DNA and monoclonal antibodies.

In addition to the above, one could mention the spate of effective vaccines developed since 1935 for tetanus, cholera, pertussis, influenza, yellow fever, polio (Salk), polio (Sabin), measles, mumps, rubella, meningococcus, pneumococcus, hepatitis B and H. influenzae b. Other accomplishments of note in recent years are the global eradication of smallpox and the achievement of 'Universal Childhood Immunization', that is, 80 per cent of the world's children fully immunized. Furthermore, the discovery and application of oral rehydration therapy has drastically ameliorated the principal cause of mortality among infants and children in the developing world, the infectious diarrhoeas. It is clear, therefore, that the greatest scientific success has been, and still is, the medical treatment and prophylaxis of the infectious diseases, which remain the greatest killers in the developing world, particularly of infants and children.

Perhaps the most striking study of the role of medicine in the Third World was the superbly controlled investigation of two Nigerian villages reported in 1975 by Orubuloye and Caldwell (1975). These villages, one with good medical facilities (Ido) and the other with no facilities other than

traditional ones (Isinbode) were matched culturally, geographically, socially and economically; the only difference between them was the availability of medical services in Ido. Deaths under one year of age and from 1 to 4 years in Ido (medical facilities) were respectively 34 and 57 per cent of those in Isinbode. Life expectancy at birth in Ido was 52 as compared with 24 in Isinbode.

The nineteenth century, which was largely the period under discussion both in Preston and Haines (1991) and McKeown (1976), was also the period in which the great explorers from the northern temperate regions opened up the tropics, now called the developing world or South. It is also the period of colonialism in which the northern powers appropriated large chunks of the southern territories bringing in their armies and colonists. These individuals coming into an alien environment suffered terrible losses from the myriad infectious diseases of the tropics. Thus began the field of tropical medicine approximately 100 years ago, many of the great practitioners of which were also deeply concerned with the health of the indigenous peoples of the South (Warren 1990).

Almost from the beginning a major conflict developed between the supporters of science-based tropical medicine and the exponents of environment-based tropical health, as reviewed by Warren (1990). Sir Patrick Manson, the greatest exponent of the former, stated in 1898

I now firmly believe in the possibility of tropical colonization by the white races. Heat and moisture are not in themselves the direct causes of any important tropical disease. The direct causes of 99% of these diseases are germs. To kill them is simply a matter of knowledge (Warren 1990:143).

His antagonist was Sir Ronald Ross who, after discovering the mosquito transmission of malaria (with the aid of Manson who had previously determined the mosquito transmission of filariasis) began to work in Sierra Leone to improve sanitary conditions, attack mosquitoes, remove garbage, provide piped water, develop sewerage systems, drain ponds, and clear undergrowth. Ross's point was

Do not think that when you have made your discovery you have finished the matter ... the discovery is only half way up the mountain, and beyond it extends the arduous summit of the practical application (Warren 1990:148).

Manson's reply was

Circumspection has been forced on governments by the mistakes of sanitary advisers ... if authorities now believed that the most ambitious schemes did not always mean the most rapid progress, they had abundant precedent to justify them (Warren 1990:148).

The polarity of these views has continued to the present day. Rifkin and Walt (1986:565) described one group that 'see health improvements as a result of programmes based on medical and technological interventions', and another that 'see health as a process dependent on individual knowledge and choice, of which medical intervention is only one, and often not the most important, input'. McKeown, of course, has been a powerful exponent of the latter point of view. But the answer to me appears to be absurdly simple: 'It is abundantly clear that we must have both tropical medicine and tropical health' (Warren 1990). Dr. V. Ramalingaswami (1984:252) has put it this way:

While development is essential for health promotion and vice versa, today's technology and the exciting prospect of further biomedical advances add a new dimension, providing mankind with an opportunity to bring about rapid improvements in health conditions, even where economic growth and development are limited.

My own conclusion was

Thus, we can now see that the concatenation of Manson's interest in the power of biomedical research and Ross's concern with the application of its results is essential for the well being of

both the developing world of the south and the developed world of the north. As the north/south health and disease problems merge in the period of the health transition and beyond, all of the peoples of the world will then face the fundamental problem of their common mortality together (Warren 1990:154).

In summary, there is no question of the importance of the historical data gathered by investigators such as McKeown (1976) and Preston and Haines (1991) in illustrating the remarkable effects of socio-economic development on health. Few would object to their assertions that little or nothing was owed to medical measures during the nineteenth and a significant proportion of the twentieth century. A major mistake, however, lies in extrapolating those results beyond the late 1930s, and another in giving little credence to the power of modern biomedical science. There is no question that the latter, its outcomes and their 'arduous' application have been a major factor in improving health in the world today. Does this assertion mean that I don't believe in nutrition, water supplies, sanitation, housing and general socio-economic development? Are you kidding!

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## Fatal Years\*

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\* Stanley Engerman made useful comments on these comments.

This is a valuable contribution to the literature on the history of mortality, for it makes imaginative use of a unique data set, a sample drawn from the 1900 Census of the United States population, to grapple with a series of important questions having to do with the causes of declining mortality. I suspect, however, that most interest for an international audience will centre on Chapter 5, which presents comparative material from England and Wales in 1911 and from a composite of several less developed countries in 1975-80. I will in these brief remarks focus on the association between social inequality and mortality raised by that chapter.

The first part of Chapter 5 uses the mortality index to describe the differences in child survival among members of different occupational classes in the United States and England and Wales. 'The index is readily interpretable', the authors say. 'A value of unity means that a woman or a group of women was experiencing child mortality at about the national average, while a value above unity means that child mortality exceeded the national average' (p. 90). It is thus analogous to a standardized mortality ratio since it describes how much above or below a national (or some other) average any particular group is, but by definition it does not tell what the actual death rate is.

Using this index, the authors show convincingly that there were much greater class differences in England and Wales than in the United States. One of their most striking results is that the professional and higher white collar class in the United States 'was considerably less favored with respect to child mortality...than in England and Wales' (p. 186). They argue that in part this was simply the result of higher incomes in England and Wales than in the United States, but they go on to show that neither income nor urbanization can entirely explain the differences. They write:

Evidently, social class in England connoted a constellation of factors related to mortality: earnings, education, style of life, housing, security, residential amenities, privilege, empowerment, and so forth. English society was apparently far more stratified and differentiated along these lines than was American society at the time (p. 196).

This makes a good deal of sense and suggests that social classes in England were far more homogeneous in respect of culture and ways of life than they were in the United States. Whether this was in fact the case, and – if it was – why it was the case, remain unasked and unexamined questions. The authors do observe that 'The contrast between the more rigid and hierarchical British class system and the more fluid, permeable, egalitarian American class system has become almost a cliché' (p. 198), but the data beg for more analysis than that.

One of the reasons more analysis of the stratification systems of the two countries would have been valuable is because such analyses would, I think, speak to some of the questions raised by the very interesting data presented in the second part of the chapter. In this part of the chapter, the authors provide data on both relative mortality and on the estimated probability of dying before the age of 5 in the United States in 1900 and in a composite of several developing countries in 1975-80. They show that the variability of mortality across occupational classes was less, and that in virtually every occupational category the expectation of dying was greater, in the U.S. than in the developing countries. They write that social class (and literacy) made so little difference in the United States because of 'the shortage of specific techniques that could be used to reduce the incidence and severity of infectious diseases as well as a failure to implement some of the techniques that had recently become available'. The problem is that the same lack of knowledge existed in England and Wales, and social class made an enormous difference there (data on literacy seem not to be available).

I have made rough estimates of the variability of mortality in the developing countries in 1975-80, in the U.S. in 1900, and in England and Wales in 1911 by dividing the highest  $q(5)$  by the lowest in each country (I could only estimate very roughly the  $q(5)$ s from England and Wales). The ratio of highest

to lowest was 1.38 in the U.S., 1.85 in the composite of developing countries, and 1.96 in England and Wales. The latter two were much more nearly alike than the first was like either of them.

The comparison of the United States and the composite of the developing countries is illuminating for some purposes, as I said above. It makes clear that relatively well-to-do people in a relatively well-to-do country a century ago had lower child survival than the poor in poor countries today, and that literacy made less of a difference then than now. But precisely these observations make the contrast with England and Wales more pressing; for it was a place where social class (and literacy?) seems to have made the same sort of difference then as it does in the developing countries at present. Preston and Haines argue – correctly, I believe – that in many respects England was an exceptional case. I suspect that the same may be said of the United States: that in many respects it, too, was (and perhaps still is) exceptional. If the comparison between England and Wales and the composite developing countries were to have been made, I suspect that – with the exception of agricultural workers – q5s of occupational classes in the developing countries would have been found to be lower than they were in England and Wales, but that the index of mortality would have been almost as great.

The authors are surely correct in showing that most poor people now have lower mortality than even well-to-do people a century ago. They are also correct, I think, when they argue that the diffusion of science and of a variety of health-related technologies must be given a large share of the credit. They give short shrift, however, to the continuing significant impact on mortality of the stratification systems of many countries. For by controlling for occupational class and discussing the effects of literacy, they raise the important issue of the association between literacy and child survival. They do not, in my view, deal in sufficient depth with the problem, but more to the point, social stratification as an important independent variable in the developing countries gets lost from view and never re-emerges.

It is often said that a reviewer should not criticize an author for not writing the book the reviewer thinks should have been written but should attend to the book that was written. That is good advice, if difficult to follow. One of the great virtues of this book – and there are many – is that it provokes questions of the sort I have raised. It will be a springboard for much fruitful research in the future.

## **Mortality differentials before the health transition**

**Daniel Scott Smith**

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The first phases of the health transition appear to be associated with a widening of differentials in child mortality by such characteristics as mother's education and father's occupation. One obvious reason for such an increase in historical data is the absence or modest size of such individual-level differentials before the transition. Why should this be so?

In an earlier article (Smith, 1983), I speculated that the shift from a regime of high and highly variable short-run fluctuations in mortality to one of low and temporally stable mortality included two main patterns with respect to differences within populations: (1) a continuous reduction of spatial differentials – between countryside and city, among regions, and among neighbourhoods within cities; (2) an increase, and then a decrease of mortality differentials related to most social and economic characteristics of individuals. Supporting this hypothesis was some historical evidence and the

assumption that the incidence of death by cause was the proximate intermediate variable that lay behind the differentials.

In a mortality regime dominated by variation in the incidence of infectious diseases, it was location in a spatially-structured disease environment (or a given time period) that mattered most for mortality variation, not the advantages or disadvantages consequent upon the status of individuals.

The most striking illustrative evidence for this emphasis comes from one of the most extreme situations in the documented history of human mortality: the African slave trade. On the voyages of some 92 British ships in the period between 1792 and 1796, the annual death rate for slaves was 116 per 1,000. For the members of the crews, however, the death rate was nearly twice as high, 207 per 1,000. On these voyages, death rates attributed to gastro-intestinal diseases were about the same for the crews and the slaves; both groups shared contaminated water and other factors in the environment. On the other hand, fevers were the source of the largest absolute difference in the advantage of the slaves over the European crews on these deadly journeys. At this time in the slave trade, differences in immunity favoured those of African origin. Diseases such as measles and smallpox, for which the European crews had a comparative advantage over the African slaves in terms of immunity, were virtually absent on these voyages (Steckel and Jensen, 1986).

Of course, the United States in 1900 had a much lower level of mortality than that extant on ships in the slave trade. Yet the most pronounced variations in mortality were related to living in different environments. Preston and Haines (1991: 196–97) note that the larger occupational differences in childhood mortality in England than in America may be attributable in part to the fact that ethnicity, not class, was the principal basis of residential segregation in the United States. Race, the strongest predictor of mortality variation, combines both association/segregation effects and a variety of unmeasured individual circumstances and behaviours.

Differences in residential distribution are probable for several of most important individual-level socio-economic predictors of variation in childhood mortality uncovered by Preston and Haines for the U.S. in 1900. Put another way, how some of these indicators capture differences in health-related attributes is puzzling (Smith, 1992). One therefore conjectures that renters and owners, households with and without boarders, and husbands with and without periods of unemployment were not uniformly distributed among places or within American cities. Thus, some of these differentials should be attributed to the environment of the neighbourhood (for example, effective sewerage, treated water) rather than to the health-related behaviour of individual households.

Even if Preston and Haines had included indicators of location within neighbourhoods in their multivariate analyses, a precise separation of the ecological and individual effects of these indicators would still be difficult. Households whose members had the attributes that favoured more healthful practices that were known at the end of the nineteenth century would have chosen their place of residence on that basis. Alternatively, for example, neighbourhoods with concentrations of these individuals would have used political pressure to get connected first to the modern sewer system.

A final unrelated point concerning the interpretation of measured differentials over time: widening percentage differentials in mortality are compatible with narrowing of differentials in survivorship.

Consider hypothetically, for example, that 200 of the children of 1,000 illiterate mothers die before age 5 compared to 100 per 1,000 literate mothers in one period. Then, after the health transition, the rates are 50 and 25 respectively for the two groups. Differential mortality remains constant; in both periods the rate for the literate is half that of the illiterate. On the other hand, calculated in terms of the proportions surviving to age 5, the 12 per cent advantage for the literate before the transition shrinks to only 3 per cent afterwards.

As the base rate moves toward zero, it would be easier for the rate for a group to be higher in percentage terms relative to the mean. Translating this difference into person-years lost as a result of the differential would make the impact seem much smaller. From a welfare perspective, each method of computation captures part of the truth of the situation.

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## Is there major new evidence on health transition and its interpretation?

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*Fatal Years* is an elegant and optimistic account of historical events. Preston and Haines conclude that 'a satisfactory escape [from the 'natural forces' that cause 'excess child mortality'] would await the technical and social triumphs of the twentieth century' (p.210). Their tone suggests that they have considerable faith that the human condition can be improved by scientific knowledge applied to medicine and public health, by education and by economic growth.

In his contribution above, Caldwell shares the enthusiasm of the authors of *Fatal Years* for the good news that these relatively new man-made forces can improve the conditions of life for infants and, by extrapolation, for the rest of us. The 'greatest potential gains' in 'Third World Health' occur, he says, as a result of the 'importation' of health and education services.

The source of the triumphalism that Preston, Haines and Caldwell share is their assumption that subsequent knowledge can be used to evaluate events in the past. As Preston and Haines write, the 'failure to achieve satisfactory mortality conditions in the United States [in 1900]...principally reflected the shortage of specific techniques that could be used to reduce the severity of infectious diseases' and the failure to 'implement some of the techniques that had recently become available'. As the 'shortage' of techniques diminished and more people recognized the 'value of available techniques', child mortality declined in the United States (p.206).

Many professional historians regard as problematic or dubious the assumption that human experience in the past can be judged by the standards and knowledge of the present. To their discredit, however, historians have not successfully communicated their doubts about this assumption to colleagues in adjacent social sciences who use historical evidence.

The core of Preston and Haines's argument is their claim that, although very little could be done to lower rates of infant mortality in the United States in the 1890s, not enough was done because many people could have behaved differently from the way they did. Their evidence for this claim is that a few

far-sighted people advocated what later turned out to be correct behaviour to shield infants from infection.

Then they move forward several decades. By the 1920s, what had been a minority opinion became a social consensus. Between Time 1 and Time 2 infant mortality declined. Therefore, Preston and Haines conclude, the cause of declining infant mortality was mainly the ascendancy of correct views about controlling infection. They make no effort to use primary sources to understand how people in the past explained the world around them and made choices among available courses of action.

In recent articles that address some of the same issues as *Fatal Years*, Tomes (1990) and Hareven (1991) offer more sophisticated analyses of, respectively, domestic hygiene and family structure in the late nineteenth and early twentieth centuries. These articles are more hesitant and elusive than *Fatal Years*. They make it more difficult to use historical arguments as justifications for policy. Tomes, for example, describes ways in which, in the nineteenth century, families were the agents of sanitary and bacteriological science rather than the laggards described by Preston and Haines.

To make clearer my concern about Preston and Haines's assumption that the present can supply evaluative criteria for the past, I compress it into a single question: Would infant mortality in the United States in 1900 have been lower if parents had behaved precisely the way they did twenty or thirty years later? Preston and Haines would answer 'of course'. Cautious historians would say: 'We cannot know and besides the question is ahistorical and thus irrelevant'.

Professional historians often exasperate their colleagues in adjacent disciplines with their love of complexity. Such pedantic habits, however, inhibit rushing to judgement: in this instance to the judgement that medical care and education, within and outside families, make the world a better place. Medical care and educational advance surely make it a different place; but different is not necessarily better for purposes of judging one's own life or that of others or for policy making.

Triumphalist historical analysis makes the concept of a 'health transition' overtly normative. Like the optimistic concepts of modernization and the stages of economic growth before it, a normative model of a health transition can lead to exaggerated claims on behalf of what Preston and Haines, along with many other authors, call the 'dramatic successes' of medical science and public health.

Exchanging one epidemiological or economic or educational situation for another is not necessarily judged to be a benefit by the people whose experiences provide the sources for historical social science. If infant mortality, past or present, outrages our sensibilities, so should the plight of the frail elderly and the frustrations of chronically ill and severely injured adults of working age, or of children and adolescents with severely disabling conditions.

Students of the health transition know that while infant mortality was declining in industrial countries, mortality from chronic disease was rising. According to U.S. Census Bureau data for 1900, for example, 24.4 per cent of the crude death rate in the registration states could be attributed to chronic disease. This rate rose to 61.2 per cent by 1940. The age-adjusted rates were 28.1 per cent and 56.9 per cent respectively (Birne and Fox 1991).

But neither social science nor policy analysis benefit from using these data to decide whether, or how much, progress occurred. Data merely help us to understand the human experience. Better understanding of the human experience by policy makers may in some way contribute to better policy. The relationship between any scientific knowledge, and especially historically grounded social science and policy, is never as straightforward as intellectuals want it to be (Fox 1991).

The best 'lesson' of history to date is humbling rather than triumphalist. It is that disease has, so far, outrun the findings of science, the instruments of technology and policies based on them. Diseases are made; whatever position one takes in the epistemological debate about whether biology is 'socially constructed', for practical purposes pathogens and their hosts (both individual and societal) are

inseparable. This point has recently been made compellingly about both infectious viral disease (Morse 1991) and social behavioural pathologies (Sugar, Kleinman and Heggenhougen 1990).

And this is just pestilence. Malthus's other Horsemen of the Apocalypse (war, famine and the absence of moral restraint) still account for a great deal of human misery despite the advances of science and technology, the apparent advantages of market over command economies in production and distribution, the spread of literacy and the contemporary ascendancy of libertarian ideologies.

Nevertheless, I share the implicit belief of Preston, Haines and Caldwell that it is not useful to despair about the human condition. I have spent my career making and writing about policy. I continue to insist that the application of good science, of all varieties (even historical), can help to make better policy. I do not, however, share the optimism of the authors of *Fatal Years* and other proponents of a normative historical model of a health transition because I believe that interpreting the past and extrapolating its data to policy are extraordinarily difficult. 'Politics', Max Weber wrote in 1919, 'is a matter of boring down strongly and slowly through hard boards with passion and judgment together' (Weber 1919, 1991). That is still good advice.

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## Response to comments on *Fatal Years*

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We are pleased to have the opportunity to respond to the commentary about *Fatal Years* and related research that we have conducted. There is no need to restate the main theme of this work because Caldwell has provided a faithful summary. It is useful, however, to reiterate that *Fatal Years* is addressed to cross-sectional data for the United States in the late nineteenth century. We describe

social, economic, and residential differentials in mortality and try to understand why they look as they do. Two subsequent papers (Ewbank and Preston 1990; Condran and Preston 1992) attempt to deal explicitly with factors responsible for declines in American child mortality between 1900 and 1930.

Our areas of agreement with Caldwell are far more extensive than our areas of disagreement. The prominent position with which both of us take issue is that child mortality declines in the twentieth century are primarily attributable to increases in living standards as measured by per capita income or dietary intake. Economists are drawn to this position, perhaps because it places explanatory power in their domain, and Thomas McKeown (especially in his earlier writings) was an influential advocate. We took pains to demonstrate that the United States in 1900 was, even by current world standards, a rich and well-fed country. Yet even its highest socio-economic groups suffered very high child mortality levels. Our explanation of this situation, buttressed by a review of medical theory and practice at the end of the nineteenth century, was that there were very few effective means at the disposal of either physicians or parents to enhance children's survival prospects. That statement would be more accurate still for 1880, before the germ theory had begun its ascendancy, and quite inaccurate for 1930.

Our disagreement with Caldwell is about the relative weight to be assigned to preventative measures compared to curative measures and, to a lesser extent, to public health activities relative to activities of physicians and hospitals. Our subsequent research suggests that improved preventative measures were the principal forces of change in the United States between 1900 and 1930. The most important of these were probably measures taken by parents in the home, taking advantage of the new knowledge that disease was spread interpersonally by invisible micro-organisms (Ewbank and Preston 1990). Sanitary environments were also improved through public-health activities. But the distinction between public health and private health for children was blurred (much as it is today for chronic diseases of adulthood) because many of the principal public-health efforts were aimed at improving private practices – at producing ‘better motherhood’. Physicians undoubtedly played a role in inducing these behavioural changes, and would have been particularly important agents of change for the upper classes. Our review of the many successive editions of *The Care and Feeding of Children* by Luther Emmett Holt between 1894 and 1933, and other texts, showed increasingly sound advice about protecting children from the common diseases of childhood (Condran and Preston 1992). Some of the advice was about how to treat an ill child but most of the improvements appear to be in the area of prevention. Until the advent of sulphonamides in the late 1930s and antibiotic in the 1940s, physicians simply had few medicines in their arsenals that would arrest the progress of an infectious disease once contracted.

The situation is surely different in developing countries today, and we see no reason to disagree with Caldwell's assertion that curative measures carried by physicians are playing a bigger role both in reducing mortality and in expanding social class differences therein. It is simply necessary to recognize that forces operating in one historical setting need not be identical to those operating in another. His more synthetic and far-reaching argument is that receipt of western-style education, with its endorsement of western science and medicine, will differentiate groups' mortality levels to an extent that depends upon the competition posed by alternative medical systems. This is an imaginative and important hypothesis that deserves continued attention. He has found support for a corollary of it - that better educated people will take advantage of better medical facilities to stretch their mortality advantage - in Nigeria, as well as in our data. A review by Cleland and van Ginneken (1988) of these interactions between one's educational attainment and the availability of modern medical facilities is more guarded. In some cases, the facilities seem to substitute for, rather than complement, higher levels of education. In other words, schooling is not always necessary in order to take advantage of the fruits of western medicine.

As implied above, we are in complete agreement with Warren's position that medical and health issues in contemporary developing countries can and should be addressed differently than they were addressed in the Western world before 1930. The technology of disease control is far more advanced, and it would be foolish to try to replicate the slow and arduous decline in the United States from 1880 to 1930 using only the weapons then available. Indeed, one of us has been a principal proponent of the view that health programs are mainly responsible for the great gains in longevity experienced by developing countries between 1940 and 1970 (Preston 1975, 1980). Research of the kind that we have conducted into social history may or may not contribute to current policy debates, but it will make no contribution whatsoever unless the historical specificity of the claims are recognized.

While we believe that McKeown (1976) was essentially correct about the minor role of specific medicines in the Western mortality decline before 1930 (with the principal exceptions of smallpox vaccination and diphtheria antitoxin), we are not happy that Warren then tucked us into a socio-economic bed with McKeown. Our research, including *Fatal Years*, has consistently suggested that economic development has not been the prime force in the mortality declines that we have studied, and many other studies reach the same conclusion. McKeown grossly underestimated the role of public-health programs, as Szreter (1988) has shown clearly in England. In the United States from 1900 to 1930 as well, public-health programs were vigorous and effective, not simply in public works but also in personal health promotion. Methods of combating diseases in the home were also promoted by popular newspapers and magazines, physicians, the Children's Bureau, mother's clubs and other agencies. They were successful in part because the population was literate, highly motivated and believed in the power of science. In the area of disease prevention, that power was increasingly demonstrable through epidemiologic studies. We believe that the population as a whole participated actively in the advances in child survival; it wasn't simply 'delivered' to them through something akin to an insecticide-spraying campaign. Does this mean that insecticides should not be sprayed where malaria-bearing mosquitoes are abundant? Of course not!

Kunitz and Smith address themselves primarily to the magnitude of social class differences in mortality that are revealed by the 1900 U.S. Census. Smith correctly points out that where one lived made a considerable difference in mortality levels at the time because public-health initiatives had only begun to attenuate differences induced by variation in population density and in other locational features. It is possible that these features are reflected in socio-economic differentials. While our basic comparisons controlled such geographic variables as density and state or region of residence, these undoubtedly do not capture all sources of geographic variation in mortality. We consider it unlikely that more fine-grained geographic variables would have substantially altered social-class differences in mortality, but agree that the issue is worth exploring. The relation seems likely to be especially important in Britain, where social classes were more segregated residentially.

Kunitz puts his finger on an important puzzle that we fail to solve. Mortality differences by father's occupation in turn-of-the-century Britain were much larger than in the United States, and so are contemporary differences in developing countries. We invoke one explanation for the latter contrast - the greater access of upper classes to effective medical techniques in developing countries today - but such an explanation is less plausible for the British/American contrast. It is clear by any measure that the economic circumstances of the social classes were far more differentiated in Britain than in the United States at the time, but our efforts to trace greater dispersion in mortality to greater dispersion in income are only partially successful. We speculate that greater residential segregation by class in Britain may have led to regionally differentiated public-health programs that reinforced the private advantages of the upper classes.

The class structures in Britain and the United States were probably near opposite extremes on a rigidity/fluidity scale. Mortality differentials in Copenhagen at the time looked more like those in the United States than like those in Britain, but we have little information on the rest of the now developed world at the time. As a crude approximation, the British income distribution was probably more similar to that in contemporary Latin America, which typically shows the largest social-class differences in child mortality (United Nations, 1985). Asia and Africa may look more like the United States did. In all these areas, however, mother's education or literacy exerts a far more important impact on child mortality than it did in the United States, even when measures of the household's economic circumstances are controlled. It is this contrast between literacy effects in less developed countries and the United States rather than one based on husband's occupational class, that we find most striking. And here, unfortunately, there are no turn-of-the-century British comparisons since no mortality data were tabulated by mother's education or literacy. Our prediction would be that, as in the United States, literacy differences in child mortality in Britain were small once other variables were controlled because it is in literacy differentials that one is most likely to find evidence of differences in access to modern medical techniques.

We turn finally to the perplexing remarks by Fox. He caricatures our methods, applies a belittling label, and attributes to us policy implications that we never discussed or intended. Most of his remarks appear to be addressed not to *Fatal Years* but to the published analysis of factors responsible for declining child mortality in the United States between 1900 and 1930, which he clearly hasn't read. True, this work is summarized in several sentences in *Fatal Years* but that is hardly sufficient to characterize our methods, our sources, our perspective, or our degree of ambiguity. We will not attempt to respond to these characterizations; suffice it to say that the three paragraphs in Fox's review beginning with 'The core of Preston's and Haines' argument...' are inaccurate and misleading summaries both of *Fatal Years* and the later work.

Our disagreements about methods appear fundamental. To us, it is obvious that mortality has improved. The probability that an American child will die before age five has declined from 0.180 in 1895 to 0.012 today. As revealed by primary sources (e.g., Pollock, 1987), most parents, then and now, are happier if their child survived than if it died. In this limited but salient sense, child health conditions improved. Fox does not like the use of such terms as 'improved' because it imposes the perspective of the present on the past. But there are few societies of the past one hundred years which would not consider a decline in child mortality to be an improvement, and most are consciously seeking to induce such declines. Meckel (1990) documents the history of collective efforts in the United States.

Fox does not believe that data should be used 'to decide whether, or how much, progress occurred'. In his data-less world, 'the best "lesson" of history to date is humbling rather than triumphalist. It is that disease has, so far, outrun the findings of science, the instruments of technology and policies based on them'. What could this claim possibly mean? It is inconsistent with data on the incidence or fatality of virtually any important disease of a century ago or a half century ago, including diseases concentrated at older ages. But, of course, if data can't be used to refute the statement, then it must stand unchallenged. In the garden of the 'professional historian' that Fox has constructed, ambiguity is nurtured and data are pests.

Some clue about what the author might intend by his declaration of victory for disease is indicated by his reference to evidence that the percentage of deaths attributable to chronic diseases has risen during the twentieth century. This information is used to support the claim that 'mortality from chronic disease was rising'. But this inference is incorrect; a rising percentage of deaths from chronic diseases may simply reflect the fact that death rates from infectious diseases have fallen faster than those from

chronic diseases. That has been the typical experience over the course of the twentieth century (Preston 1976).

Preston and Haines do not know whether the 'human condition' has improved over the past century, but we do know that mortality conditions have improved at all ages. We are led to that conclusion by data: those who study mortality, diet, income, height, and other markers of well-being cannot afford to ignore the data and measures pertaining to these characteristics. Nor are we inherently optimistic; some of the data on processes that we study, such as child poverty and psychological health, have made us publicly pessimistic. Our disagreements with Fox are less about optimism and pessimism, triumphalism and millennialism, than about the role of evidence in the social sciences.

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