Explanations and Ideologies of Mortality Patterns

Stephen J. Kunitz

Explanations of changing mortality rates and patterns, both historically in the developed countries and contemporaneously in the developing ones, have been concerned with apportioning credit to a variety of causes: public health measures such as sanitation and immunizations; improvements in nutrition; therapeutic interventions; educational reforms; increased standards of living; and so on. Mortality trends are of very immediate concern with respect to health care and development policy worldwide. In this article, however, I consider primarily the explanations themselves and only secondarily the phenomena they are attempting to explain. In other words, I treat mortality rather like an ink blot and explanations as projections of professional and political values. In the conclusion I address more directly the phenomena I treat here as secondary: mortality rates and patterns themselves.

The starting point for this article is the profound change in ideas about disease causation that occurred in the late nineteenth century. As a consequence, it became possible to believe that specific diseases could not occur in the absence of specific causes (causal necessity). This new paradigm did not replace the older one—that many different causes could result in the same disease (multiple sufficient causes)—and over the past century these two conceptions of causal attribution have coexisted and sometimes competed. The purpose of this article is to explore some of the ways in which each of these paradigms has been used to explain changes in mortality, to justify public policies, and to advance the claims of various groups of public health professionals.

The nineteenth century American background

For the mid-nineteenth century physician, professional knowledge was local knowledge (Warner, 1986). It was knowledge of local climate and topography, local customs, and particular people. It was dependent upon knowing how a multiplicity of causes influenced sickness in individuals and endemic and epidemic diseases in populations. As Charles Rosenberg has observed, "The model of the body, and of health and disease . . . was all inclusive . . . capable of incorporating every aspect of man's life in explaining his physical condition. Just as man's body interacted continuously with his environment, so did his mind with his body, his morals with his health. The realm of causation in medicine was not distinguishable from the realm of meaning in society generally" (1979: 10).

Public health intervention usually involved controlling local sources of pollution and was rationalized by the miasmatist, or filth theory of diseases. Although contagion was recognized as a real phenomenon, it was also argued that diseases could develop from local sources and spread through the air in the form of miasmas.

K. Codell Carter (1985) has argued convincingly that until the 1880s, when Robert Koch enunciated the postulates that now bear his name, physicians explained the etiology of disease in terms of sufficient or weakly sufficient causes: which is to say, a particular effect (a disease) might occur after exposure to one or more causal influences, of which contagion might be one. Indeed, a particular effect (disease) might be caused by several different conditions, and diseases might blend into one another. Even a well-defined disease such as smallpox, with a known course, mode of transmission, and preventive measures, was thought of more as an exception than the rule. "Physicians in the period repeatedly warned against inferring a common cause from a common effect" (Carter, 1985: 372).

The impact of the germ theory and of Koch's postulates was to introduce the notion of causal necessity in addition to that of causal sufficiency. A necessary cause is one in whose absence a particular effect cannot occur. In the case described by Koch, tuberculosis could not occur in the absence of the tubercle bacillus. The bacillus was also sufficient, or weakly sufficient, in that tuberculosis had a high probability of occurring in its presence. The result was a subtle but far-reaching change. Many diseases could now be classified etiologically as well as anatomically or symptomatically. Disease specificity became increasingly possible, and with it the possibility of disease-specific interventions that would be applicable in all places and among all people, regardless of topography, climate, and culture. Under the impact of these new ideas, whose validity was demonstrated by the rapid discovery of the causal agents of many infectious diseases—and combined with a growing awareness of interdependence among communities and of the spread of diseases from one to another—the previously dominant theory of the local origin of diseases gave

way, though not without considerable struggle (Jacobi, 1885; Marcus, 1979). In medicine—as in the social sciences (Haskell, 1977)—professional knowledge now became universal knowledge.

Charles Chapin and the decline of mortality in Providence

Faced with a rapid succession of striking discoveries, all reinforcing the concept of causal necessity of disease, many influential public health workers embraced the idea enthusiastically. It underlay the Rockefeller Foundation's campaign during 1909–14 against hookworm in the Southern United States (Ettling, 1981), and it was incorporated by Charles Chapin, the influential health officer of Providence, Rhode Island, in his campaign to professionalize public health. Indeed, Chapin was so profoundly impressed with the force of the germ theory and of the idea of causal necessity and disease specificity that he offered a sardonic funeral oration over its predecessor:

> When our honored and lamented Reed went to Havana and discovered that yellow fever was transmitted by the bite of a mosquito, and Gorgas, by the most brilliant sanitary experiment ever made, put an end to this disease in its very stronghold, they drove the last nail in the coffin of the filth theory of disease. But it is to be feared that the devotees of this theory are loath to bury it, thus violating one of their cardinal principles. It seems to me it is the duty of the health officers of this country to see that this ceremony is properly performed. (Chapin, 1934 [1902]: 20)

The germ theory, said Chapin, had made possible a more lucid, efficient, and economical attack on the causes of mortality. Specific diseases could now be directly targeted and directly defeated. Medicine, he said, had acquired a new precision. Depending on the situation, medical interventions would vary: they might require isolation of the infectious; they might require treatment with antitoxin; they might require measures to protect water and milk; and they might require health education. But, medicine could now focus directly on the fundamental cause of each disease and could conquer each with an effectiveness previously unknown.

Chapin also argued that necessary causation rendered irrelevant the activity of those he saw as misguided social reformers and politicians. Where an elegant precision in identifying the cause of disease was possible, there was no further need for the efforts at municipal cleanliness that had characterized the urban reform movement. He dismissed municipal cleanliness as uneconomical, unprofessional, and unscientific (Rosenkrantz, 1974).

The striking decline of mortality that had characterized the nineteenth century continued into the twentieth. Chapin sought to explain the phenomenon in terms of the new causal concept of disease. He analyzed the decline of mortality in his own city, Providence, between 1856 and 1905 and sought to

trace the reduction in deaths from specific diseases to specific interventions. He linked the decline in smallpox to vaccination; he linked the decline in scarlet fever to isolation; he linked the decline in diphtheria to antitoxin; and he linked the decline in typhoid to the improvement of the water supply. Political, socioeconomic, and environmental conditions had receded into the background of his analysis (Chapin, 1905).

Chapin's influence was widespread. Not only was he an active proselytizer for a new, professionalized public health system, but his was a vision of public health founded upon the notion of causal necessity. Half a century later Fred Soper of the Rockefeller Foundation hailed Chapin as "having the vision and the voice of a prophet; he recognized instinctively that the rejection of spontaneous generation [one basis for miasmatist theories] and the acceptance of the germ theory of disease implied the concept of contagious disease eradication. Were Chapin alive today . . . he would be championing the cause of world eradication programs" (Soper, 1970 [1960]: 334).

Soper had begun his career working on the Rockefeller Foundation's hookworm eradication program in the Southern United States, later moving on to programs in Latin America and elsewhere. He was the spokesman for the position that infectious diseases could and should be eradicated, a position that depended on the notion of causal necessity. Such a position carried with it the assumption that a particular disease was everywhere the same; that international boundaries should not be barriers to worldwide eradication of certain important diseases; and that a universal medicine based upon transferable technology and knowledge was possible. He went further, pointing out that "Once this principle [of eradication] is accepted, the problem becomes one of devising economically feasible administrative methods adapted to each situation. This 'economically feasible' restriction is most important; methods must be not only feasible, but economically so" (Soper, 1970 [1960]: 334; see also Worboys, 1976). Thus, like Chapin, Soper understood that the idea of the causal necessity of disease made conceivable the efficient application of professional knowledge in ways that required neither enormous expenditures nor municipal or national reforms.

The 1920s and 1930s: The return of causal sufficiency

Eradicationist ideas were not universally accepted. Soper himself observed that, "The period between the late 1920s and the early 1930s was probably this century's low point in acceptance of the eradication concept in the prevention of communicable disease" (1970: 340). There were several reasons. Soper attributed declining optimism to the fact that yellow fever had proven recalcitrant to eradication, because it existed in a jungle form as well as an urban form.

This was but one example of the discoveries being made regarding the complexity of the ecology of infectious diseases. As early as the end of the

nineteenth century the carrier state had been described. It was only some decades later that the phenomenon of nonapparent infection that conferred immunity in the case of many viral diseases began to be understood. Thus, host factors assumed increasing concern to clinical investigators and epidemiologists (e.g., Crookshank, 1920). For example, in a series of lectures in the early 1930s, the eminent microbiologist, pathologist, and public health official Theobald Smith observed that parasitism (what today is called symbiosis) evolved from predation, as the result of mutual adaptation between host and parasite. The balance or equilibrium between them could be disrupted by any number of factors, and when it was, disease would occur (Smith, 1934).

Ten years later, in 1943, already aware and fully appreciative of the efficacy of sulfa drugs, C.-E. A. Winslow, professor of epidemiology at Yale, observed: "There is today a wholesome reaction against exclusive emphasis on the germ and recognition of the importance—even in many germ diseases—of factors of constitutional resistance (diathesis) and of the influence of climate and season and nutrition upon vital resistance" (Winslow, 1943: 380–381).

Writing the history of his field, John Gordon, professor of epidemiology at Harvard, observed of the period following World War I:

> In the minds of many, realization took form that disease was no longer being studied, but rather the parts of disease; that too frequently the parts were considered the principal phenomenon; and that in pursuit of knowledge about infectious agents, the main objective was being lost. (1952: 115)

According to Gordon, these ideas crystallized in the years immediately following World War I as a result of the failure to explain and control the 1918 pandemic of influenza and outbreaks of polio, meningococcal meningitis, and encephalitis. Moreover, he said, the "changing social order" brought an increasing awareness that "cultural, economic, and social factors are important determinants of health and disease in groups of people." He continued:

> This movement did not originate precisely in 1920, nor in the years immediately following. Matters simply came to a head then. The return to a holistic interpretation of community diseases, its consideration as a unified and total process, had been under way for a number of years. Opinion solidified, to give general appreciation that there is no single cause of mass disease, that causation involves more than the agent directly giving rise to the process, that cause lies also in the characteristics of the population attacked and in the features of the environment in which both host population and agent find themselves. The result is the modern concept of epidemiology as medical ecology and of disease as an ecologic process. (1952: 115–116)

Such ideas did not emerge from infectious disease epidemiology alone. By the 1920s there was a growing awareness, in the United States and other industrialized societies, that causes of morbidity and mortality were changing, that noninfectious diseases were becoming more significant as infectious dis-

eases declined (Cohn, 1931; Henderson, 1949 [1927]), and that Western society was now facing what Peyton Rous (1929) called "The Modern Dance of Death." Judging by the topics covered in articles in the *American Journal of Hygiene* in these years, however, as well as in course offerings in schools of public health, chronic diseases were low on the list of concerns of epidemiologists (Terris, 1985: 22–23).

On the other hand, ideas of adaptation, of the importance of equilibrium, and of the embeddedness of individuals within a complex environment were becoming more widespread in these years and were to be found in biology (Worster, 1985); in the social sciences (Russett, 1966); and in philosophy in such ideas as organicism (Whitehead, 1925), emergent evolution (Morgan, 1923), and holism (Smuts, 1926). In all these fields the idea of chains of causation was being found wanting and was being supplanted by the idea of webs of causation. The idea of causal necessity was being challenged by the idea of multicausality—that is, by the reemergence of the idea of multiple weakly sufficient causes.

Many of these ideas formed the core of a remarkable analysis by Theobald Smith of the causes of mortality decline in urban America. Lecturing a decade before the first antibiotics had been discovered, Smith observed that the decline had occurred at the very time that populations of diverse origin had immigrated to America at unprecedented rates. And he asked a question of profound importance. Given the emergence of new human infectious diseases from animal hosts; given improvements in transportation that facilitated the introduction of new exotic infections into populations; and given increasing population density, which had increased the risk of exposure to old as well as new infectious diseases; how was it, then, that infectious diseases were declining? Indeed, why were there no new epidemics? He answered:

> This decline involves more than the words stand for. It means the suppression of new, as well as the blocking of the return of old, well-known diseases. Protagonists of the part played by medical science and practice in the decline of infectious diseases have in general failed to make use of their strongest argument, namely, that it has probably suppressed many diseases *in statu nascendi* and that the falling curve of certain endemic diseases is only a surface phenomenon of what is going on underneath. (Smith, 1928: 353)

Reflecting the adaptationist thought of his period, Smith postulated that host populations and infectious agents tended to adapt to each other over time, and diseases thus became endemic. Under such circumstances, a variety of host and environmental factors might upset the balance and cause clinical disease. The fact that under a complex of circumstances ripe for the production of new epidemics virtually none had occurred in preceding decades suggested to Smith that the effect of all the factors believed to have influenced disease had been that of inhibition: "In every detail of individual and communal life,

medical science has formulated protective devices to maintain health, largely by the suppression of infection'' (1928: 360).

In saying that prevention of disease and declining mortality in the twentieth century were attributable to medical science, Smith and his contemporaries meant public health measures, not the personal physician system. For example, in 1939 Murray Horwood, an engineer at the Massachusetts Institute of Technology, explained the decline of mortality in previous decades and the striking fact that mortality did not increase during the Depression of the 1930s by observing that environmental control was more significant than clinical care, and that no one was allowed to die of cold or starvation, that water and food continued to be protected, and that sewers still worked (Horwood, 1939). Similarly, C.-E. A. Winslow argued that it was not private practitioners who had been responsible for declining mortality since the turn of the century, but ''the organized forces of the community,'' by which he meant public health departments (Winslow, 1944).

Thus in the late 1930s and early 1940s the concept of necessary causes of disease was far less popular than it had been 20 years earlier. It had not led to significant preventive or therapeutic interventions and resulting declines in mortality. Indeed, for public health workers the history of declining mortality had become the record of their accomplishments, and these were primarily the control of multiple sufficient causes of infectious diseases.

The 1940s to the 1980s: The developed countries

Even as Horwood and Winslow were writing, the development of antibiotics and pesticides was helping to resurrect the importance of causal necessity. The ability to cure previously fatal diseases and to abort epidemics of typhus during wartime gave those who believed in the possibility of eradication considerable support.[1] Since, however, the infectious diseases were no longer the major causes of death in developed countries, the importance of eradication was less significant there than it was in poor countries, where the infectious diseases were still particularly important.

Nonetheless, even in the developed countries the impact of antibiotics was not insignificant. Recalling the effect years later, Walsh McDermott wrote: ''The crossing of the historic watershed could be felt at the time. One day we could not save lives, or *hardly any* lives; on the very next day we could do so across a wide spectrum of diseases. This was an awesome acquisition of power . . .'' (McDermott, 1982: 303). Physicians and researchers acquired immense cultural legitimacy and institutional power, even as the paradoxical consequences of success became evident. Those paradoxes were two. First, Americans were now dying in increasing numbers of the very diseases that the laboratory scientists had been unable to explain and cure: the chronic degenerative diseases. The most brilliant successes of the advocates of nec-

essary cause had shifted the spotlight to their greatest failure. And second, there were those who questioned whether the decline of mortality since the introduction of antibiotics had had anything to do with such drugs and other interventions at all.

With respect to the chronic diseases, it was only after World War II that epidemiologists began to devote serious attention to what Milton Terris (1983, 1985) has called "the second epidemiological revolution." Two early studies are particularly important: the studies (in England) by Doll and Hill (1952, 1954, 1956, 1964; Hill, 1953) of the association between smoking and lung cancer, and the Framingham study of heart disease (Dawber, 1980).

Doll and Hill discovered that smokers had a higher probability of developing lung cancer than nonsmokers; that heavy smokers had a higher probability of developing lung cancer than light smokers; that nonsmokers had a very low but nonetheless real probability of developing lung cancer; and that the vast majority of smokers did not develop lung cancer at all. In other words, they had observed that cigarette smoking was neither a necessary nor a "strongly" sufficient cause of lung cancer but that it was a "weakly" sufficient cause.

In the case of heart disease, the seminal study was conducted in the United States. It was based on a random sample of the population of Framingham, Massachusetts, first examined in the late 1940s and followed prospectively to this day. Its purpose has been to identify the etiological factors in heart disease. In an early publication the investigators wrote:

> Certain factors are associated with increased risk of development of coronary heart disease. Most important of these are hypercholesterolemia and hypertension. Many factors appear to be related to the development of coronary heart disease. The exact interplay of these factors and a possible common denominator important in the pathogenesis of coronary heart disease remain to be determined. No one factor has been clearly demonstrated to be essential. (Dawber et al., 1959)

This marks one of the first uses of the term "risk factors," which of course are the same thing as multiple weakly sufficient causes. Indeed, Thomas Dawber, the principal investigator of the Framingham study for many years, wrote:

> Cause, a word epidemiologists prefer to avoid, to many people implies a factor without which the disease would not occur. If it were used with this connotation, a conclusion would be reached that the tubercle bacillus was the inevitable cause of tuberculosis or that lead paint was the . . . cause of lead poisoning. In actuality, all persons exposed to the tubercle bacillus do not develop clinical tuberculosis, nor do all those exposed to lead paint develop lead poisoning. Other factors also may be causally related. . . .
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> In the absence of the sine qua non for disease development, the term *cause* becomes even more controversial. A combination of circumstances may lead

> to the increased occurrence of a disease, yet no single factor must necessarily be present. Although young male alcohol or drug addicts may contribute to a high rate of automobile accidents, clearly neither alcoholism nor drug addiction is necessarily the sole cause. Both of these conditions nevertheless are "causally" related, since youth, maleness, and the amount of alcohol or drugs used are importantly related to automobile accident rates. Thus we should speak of the *causes* of disease rather than concentrating on a single cause. The task of the epidemiologist is to root out *all* the causes, all the various factors that act to increase the incidence of the disease. (Dawber, 1980: 5)

Brian Macmahon restated these ideas in the form of a simple principle: "Effects are never dependent on single causes" (Macmahon et al., 1960: 18). The notion of "webs of causation," he said, was a more adequate description of reality than the notion of "chains of causation." What is important here is that the ideas of causation that were being enunciated by chronic disease epidemiologists were an elaboration of ideas that had been applied by people like Theobald Smith to the study of infectious diseases a generation earlier (see also Gregg, 1956).

A related issue arose concerning the true impact of antibiotics. Thomas Magill observed that, "The concept is difficult for us to accept that infection may be a matter of ecology" (Magill, 1955: 1). He claimed:

> It would seem to be a more logical conclusion that during recent years, quite regardless of our therapeutic efforts, a state of relative equilibrium has established itself between the microbes and the "every-varying [sic] state of the immunological constitution of the herd"—a relative equilibrium which will continue, perhaps, just so long as it is not disturbed, unduly, by biological events. (1955: 7)

A far better known statement of a similar position was that of René Dubos a few years later in his book, *Mirage of Health*. Dubos (1959), citing Theobald Smith (p. 77), pointed out that parasites and hosts adapt to one another; that "Disease, when it occurs, is due to a change in the conditions under which the ecological equilibrium has evolved" (p. 79); and that the "search for *the* cause [of disease] may be a hopeless pursuit because most disease states are the indirect outcome of a constellation of circumstances rather than the direct result of single determinant factors" (p. 86). On mortality decline in particular, Dubos said:

> Because the decrease in death rates appeared obvious to everyone after 1900, scientific medicine and the germ theory in particular have been given all the credit for the improvement of the general health of the people. The present generation goes still further and now believes that the control of infectious diseases dates from the widespread use of antibacterial drugs. So short are medical memories! In truth the mortality of many other infections had begun

> to recede in Western Europe and North America long before the introduction of specific methods of therapy, indeed before the demonstration of the germ theory of disease. (1959: 126)

In a later work he pointed out that in the developed countries the most prevalent microbial diseases "are completely different in their origin and manifestations from those which are so effectively dealt with by modern techniques." Dubos continued:

> The sciences concerned with microbial diseases have developed almost exclusively from the study of acute or semi-acute infectious processes caused by virulent microorganisms acquired through exposure to an exogenous source of infection. In contrast, the microbial diseases most common in our communities today arise from the activities of microorganisms that are ubiquitous in the environment, persist in the body without causing any obvious harm under ordinary circumstances, and exert pathological effects only when the infected person is under conditions of physiological stress. (1965: 164)

Thus in developed nations the hope of eradication of the most common infectious diseases using current techniques (antibiotics, pesticides, immunizations) was, according to Dubos, a chimera. Like the noninfectious diseases, they were attributable to multiple weakly sufficient causes, or risk factors, that reflected not only the characteristics of the agent, but also the susceptibility or resistance of the host, as well as a variety of environmental factors.

Dubos's statement is important because several observers, most notable among them Walsh McDermott (1981), have suggested that the notion of multicausality was resurrected in response to the cessation in mortality decline in the United States between 1954 and 1968. This does not seem to have been the case. Ideas of multicausality (multiple weakly sufficient causes) preceded any awareness that mortality rates had reached a plateau, persisted for the 14 years during which mortality failed to change significantly, and continued to be important in the thinking of epidemiologists after the decline resumed in 1968. It seems most likely that the persistence of these ideas—and conceivably their increased popularity in the 1950s and 1960s—despite changes in the course of mortality decline, resulted from the inability to discover a satisfactory alternative explanation based upon necessary and sufficient causes.

This, then, is a quick sketch of the history of causal attribution in epidemiology as it evolved through the 1960s, at the time when a series of political and ideological upheavals occurred in American life. The first, in the early 1960s, was the civil rights movement for racial equality; which was followed by the reaction against science and technology prompted by the Vietnam war; which was followed in its turn by the environmental movement in the late 1960s and early 1970s. It was widely believed that man had made the environment a dangerous place: the nuclear bomb had been dropped, obliterating two Japanese cities; children as well as adults were being set ablaze

with napalm in Vietnam; at home official sources were predicting massive epidemics among workers from exposures to carcinogenic chemicals in the workplace, and in the general population from carcinogenic pollution. In this manmade world, diseases were manmade too, and the very existence of the species was thought to be endangered.

Many concluded in the late 1960s and early 1970s that modern technology was either failing to cure the public (e.g., of heart disease), or was killing the public (e.g., with carcinogens), or that the public was killing itself with modern technology (e.g., automobiles). Under these circumstances, renewal of the debate about the course and causes of mortality was inevitable. The conflict revolved around three issues: the role in the new morbidity and mortality patterns of environment, the role of medical care, and the role of individual behavior. It was an unprecedentedly intense debate, complicated by the fact that starting in 1968 mortality rates began to decline again and have continued to decline ever since. I shall be concerned here with the debate that occurred within epidemiology and public health itself.

Epidemiologists and public health workers tended to agree that medical care did not have a measurable impact on mortality rates, though all could affirm that it played an important samaritan function. Their debate was about the relative importance of environmental and individual factors. McDermott (1981) has pointed out that only under very special circumstances can the personal physician system be expected to show an impact on mortality rates. Such circumstances occur when (1) a broadly effective intervention against a very common disease becomes available and diffuses rapidly through the medical care system; or (2) when a very effective intervention against a wide array of common causes of morbidity becomes available and is also diffused rapidly through the medical community. The first situation occurred when antibiotic treatment for tuberculosis, a common disease, became available in the late 1940s and 1950s. The second occurred when antibiotics against a broad range of infectious agents became available in the late 1930s and early 1940s. McDermott attributed what he interpreted as an increase in the rate of decline of mortality between 1937 and 1954 to these two conditions. Since then, he claimed, no comparable intervention had become available, thus making it impossible to attribute any of the decline in mortality since 1968 to the personal physician system.

In addition, however, because the evidence on the case–fatality ratios of many noninfectious diseases is not good, McDermott argued that it was impossible to say how much of a burden of illness in the population each death from a particular disease represented. In the past when the number of deaths from, say, typhoid fever declined, it could reasonably be assumed that fewer people had contracted the disease. McDermott (1981) maintained that was not a valid assumption with noninfectious diseases. Thus a decrease in deaths from coronary heart disease might reflect either a decline in the incidence of the disease with a constant case–fatality rate, or a decline in the case–fatality rate while the incidence of the disease remained constant, or declines in both

incidence and case–fatality rate. Thus it became difficult if not impossible to apportion credit for the decline to any particular factor. In a situation of such uncertainty, interpretations were highly prone to assume the characteristics of a projective test of values and assumptions.

And this is precisely what happened. The debate was, in essence, between those who attributed most importance to socioeconomic factors in disease and those who attributed most importance to "lifestyle" factors. For purposes of this discussion, these two groups will be called determinists and voluntarists. The conflict was not epistemological: all participants were multifactorialists. It was rather a conflict over the locus of causal attribution.

The determinists shared the view of those who perceived diseases as caused by society and citizens as victimized by that society (e.g., Crawford, 1979). The risk factors upon which they focused were such technological ones as toxic and carcinogenic chemical exposures from industry and agriculture; radiation from military, industrial, and medical sources; drugs and food additives; industrial and farm machinery; such implements of modern life as handguns, rifles, knives, and automobiles; such phenomena as the breakdown of social supports in the course of urbanization; the loss of autonomy by local communities; crowding, unemployment, and poverty; and the competitive tensions of modern life—in short, an immense web of causation. An explicitly Marxist group attributed these interdependent pathogenic phenomena to capitalism and claimed that the entire society was "sick" (Stark, 1977). All determinists shared the concept of society as a malevolent force, a source of victimization. Thus in the American context they were confronting every major social institution.

Perhaps the most eloquent and gifted writer in this group was René Dubos, who in *A God Within* built upon his earlier notions of adaptation and the impossibility of eradication of infectious diseases to plead that mankind abandon "Faustian Civilization" for "Arcadian Life." Humankind was not adapted to the kind of civilization it had created, and unless life were lived in closer accordance with the requirements of the ecosystem, "racial death" would occur (Dubos, 1972; Efron, 1984: 37–40; Fleming, 1972).

Given the determinists' view of man as victim of social, technological, and economic forces created by society itself, there were two groups above all that commanded their attention. In the United States the major victim group was blacks, who died at higher rates than whites from hypertension, heart disease, stroke, cancer, cirrhosis of the liver, accidents, and murder. Outside the United States the major victim group was the "Third World," by which was meant the poverty-stricken, barely industrialized nations including parts of Latin America, Asia, and particularly sub-Saharan Africa, where malnutrition, parasitism, and infant mortality caused incalculable suffering.These two groups, perceived by the determinists as the victims of both the nation and the world, were the embodiment of their epistemological and ideological assumptions: the concept of society as first cause; of man as victim; and of disease as any cause of morbidity and mortality. Moreover, the intensity of

suffering of these two groups justified a utopian fervor among the deterministes, who sought immediate and universal reform and even revolution as the only solution.

That was one side of the debate. The voluntarists held diametrically different positions on the central assumptions: society was not the first cause and thus, by implication, man was not inherently a victim. For the major causes of mortality, the locus of control was internal; it had to do with the individual's will and moral responsibility. The political orientation of this group was mixed (one of the most eminent leaders, Sir Richard Doll, was a socialist), but clearly they were not possessed of revolutionary fervor. Indeed, their medical and scientific assumptions and conclusions led to positions that were to become congenial to conservative governments. And, not irrelevantly, they had different priorities and a different definition of disease. Generally they used the traditional physiological definition of diseases, and they were concerned with the greatest good for the greatest number: the factors associated with reduction of the greatest number of deaths.

This led to a striking difference in the attitudes of the voluntarists toward US blacks and the impoverished of the Third World. Blacks in the United States did not constitute a special victim group because their most devastating diseases were the same as those of the whites, and the same kind of interventions would apply to them as well. As for the impoverished Third World, it too did not have a unique medical-political status for the voluntarists. Their constellation of views was not embodied by politically symbolic victim groups: they were not utopian reformers.

While individual voluntarists had been reaching these conclusions independently for years, they became an identifiable group with the publication of two books by one of their number. In 1976 Thomas McKeown published both *The Modern Rise of Population* (1976a) and *The Role of Medicine* (1976b), which set the voluntarists' explanations of contemporary mortality patterns in industrial countries in a broad historical context. His essential conclusions were these: (1) until the twentieth century therapeutic and preventive measures undertaken by individual physicians with individual patients had had no impact on the decline of mortality; (2) even after the development of antibiotics the contribution of the personal physician system had been minimal; and (3) until the middle of the nineteenth century protection of water supplies and disposal of sewage had had no effect on mortality either. McKeown summarized the sequence of important causes of mortality decline from the eighteenth century to the present as follows:

> The main influences, in order of time and importance, were environmental (nutrition and hygiene), behavioural (control of reproduction), and medical (immunization and therapy). To what extent has this order been affected by the modification of health problems?
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> The main change is that in developed countries behavioural influences are now relatively more important than environmental ones. (1976b: 98)

His explanation of the decline in mortality up until the development of antibiotics differed in detail but not in essence from explanations offered by many writers before him. What made McKeown's work so important was that he assessed the contribution to declining mortality of the personal physician system and biomedical research as minor, and he emphasized the significance in the present period of personal behavior at the very time that Western governments were becoming increasingly concerned with the costs of health care. And he gave the voluntarists a historically coherent explanation of the very conclusions they had reached; namely, that social and economic development had proceeded so far that individuals were now freer than they had ever been before of exogenous forces affecting mortality, and that endogenous forces under the control of individuals themselves were now the major determinants of morbidity and mortality.

From all sides voices elaborated on these conclusions. John Farquhar (1978) declared *The American Way of Life Need Not Be Hazardous to Your Health*—the title of a book in which he advised readers how to alter their behavior patterns in order to diminish their risks of heart disease. Michael Shimkin stated, "Accidents, suicide, and homicide rank above all diseases. In older age groups, many of the diseases are self-induced. . . . Our main threats to life are now based primarily on life-styles and habits" (Shimkin, 1975: 441–446). Richard Doll claimed, "Relatively few causes of mortality . . . have become more common recently, and most of those that have are due to changes in behavior and not to the introduction of new toxic substances" (Doll, 1978: 486). And Lester Breslow said that the prevention of mortality had shifted from the identification of specific disease entities "toward the identification and reduction of the factors that lead to clinical disease." The reduction of such risk factors was to be accomplished by educating the public to adhere to seven simple "health habits" (Breslow, 1978).

An unmistakable theme of moral denunciation appropriate to the importance attributed to voluntary behavior emerged in many of these statements. Ernest Wynder said, "the leading causes of death today are the result of life style. The public must realize that the persons who are responsibly protecting their health are actually paying for the health care of those who behave irresponsibly" (Wynder, 1975: 448). Gio Gori, who at the time was director of the smoking and health program of the National Cancer Institute, declared, "Evidence accumulated during the last twenty years indicates that the most important of modern diseases are caused by a variety of factors, most significantly by reckless personal and social habits. . ." (Gori and Richter, 1978: 1124). And from one of the central institutional supporters of biomedical research came the voluntarist position complete with its philosophical and political implications. John Knowles, president of the Rockefeller Foundation, declared that "Cartesian rationalism, Baconian empiricism, and the results of the Industrial Revolution" had led the medical profession to hitch its wagon to the star of high technology. This orientation, he said, was strengthened by

the development of the germ theory and the beguilingly simple notion of "one germ–one disease–one therapy." This, he continued, had resulted in very-high-cost medical care and in indifference to the moral responsibility of individuals for their own diseased state:

> Prevention of disease means forsaking bad habits which many people enjoy—overeating, too much drinking, staying up at night, engaging in promiscuous sex, driving too fast, and smoking cigarettes—or, put another way, it means doing things which require special effort—exercising regularly, going to the dentist, practicing contraception, ensuring harmonious family life, submitting to screening examinations. The idea of individual responsibility flies in the face of American history which has seen a people steadfastly sanctifying individual freedom while progressively narrowing it through the development of the beneficent state. (Knowles, 1977: 58–59)

It was this position of the voluntarists that recommended their interpretation to government policymakers in several Western nations. In Canada the Lalonde Report concluded that "equating the level of health . . . with the availability of physicians and hospitals is inadequate." "Future improvements in the level of health of Canadians lie mainly in improving the environment, moderating self-imposed risks and adding to our knowledge of human biology" (Lalonde, 1975: 18). The bulk of recommendations makes clear that "moderating self-imposed risks" was to be the major thrust of government action.

Similarly, the US Surgeon General's 1979 Report, *Healthy People,* recommended more research, environmental improvement, less dependence upon acute medical care, and diminution of risk factors for various causes of morbidity and mortality—what has come to be called "health promotion, disease prevention." And again it is clear that personal behavior is to receive the bulk of attention (DHEW, 1979; Starr, 1982: 408–411). Noteworthy was US Secretary of the Interior Donald Hodel's recommendation that President Reagan not sign an international agreement to reduce chlorofluorocarbons in the atmosphere. It is believed that these chemicals are damaging the ozone layer, and that the increased exposure to radiation will cause an increase in cancer. Instead, the Secretary recommended that people use "personal protection": suntan lotion, hats, and sunglasses (*Rochester Times Union*, 29 May 1987; *Financial Times*, 2 June 1987).

Lastly in England a Consultative Document, *Prevention and Health: Everybody's Business,* also emphasized modification of individual risk factors as the major factor in disease prevention (DHSS, 1976). The very political nature of these differing perspectives accounts for the difficulty faced by its authors in distributing widely the so-called Black Report, *Inequalities in Health* (Townsend and Davidson, 1982: 11). The voluntarist perspective also underlay the remarks of Edwina Curry, a junior Health Minister in Margaret Thatcher's government, when she explained to an audience in the North of England why

health in the North was worse than in the South: "We have problems here of high smoking and alcoholism. Some of these problems are things we can tackle by impressing on people the need to look after themselves better. That is something which is taken more seriously down South. There is no reason why it cannot be taken seriously up here" (quoted in *The Manchester Guardian Weekly*, 5 October 1986).

It must be observed that the voluntarist position does not necessarily imply that the availability of health care ought to be reduced, although the position may be used to justify such reductions. Indeed, some voluntarists assume health services are sufficiently available so that the residual problems that remain are susceptible to modifications of individual behavior. This assumption is especially prevalent in England and Canada, where subsidized primary health services have been more accessible than they have been in the United States.

Despite McDermott's (1981) observation that it was no longer possible to determine what was causing the decline in mortality since 1968, the voluntarists have clearly had the best of the debate so far. Politically their position is strongly represented in the US federal government, but beyond that the epidemics of cancer that had been predicted in the 1960s have not occurred; and mortality from heart disease, stroke, and lung cancer (among men) has declined, even among blacks. The prevalence of smoking, a significant risk factor in both heart disease and lung cancer, has decreased among men; hypertension, a significant risk factor for stroke, is more widely detected and successfully treated now than in the past; and changes in dietary habits may have reduced other risk factors for coronary heart disease. On the other hand, there is evidence that funding cuts have influenced the health of the poor adversely, and thus the determinists continue to maintain that individual lifestyle is not an adequate explanation of mortality patterns (Lurie et al., 1984; Fisher et al., 1985; Mundinger, 1985; Calkins et al., 1986). What is no longer a serious topic of debate among epidemiologists in developed countries is the notion of multicausality itself, although the criticism is sometimes made that risk factors are likely to be thought of as necessary causes (Susser, 1973; Mausner and Bahn, 1974). The debate is far from settled in developing countries, however.

The 1940s to the 1980s: The developing countries

I have said that in the industrially developed countries there is general consensus among epidemiologists and public health workers that diseases are multifactorial in origin, whether they are infectious or noninfectious. The situation is somewhat different in developing countries, where infectious diseases are still among the most common causes of death. Here the notion of causal necessity as implied by the concept of eradication is still well represented, as is the idea of multiple causal sufficiency as reflected in the so-called ecological approach. I shall consider each of them in turn.

I have already suggested that the notion of causal necessity is implicit in the idea of disease eradication, for if a necessary cause is one without which a disease cannot occur, then eradication can be accomplished if one can somehow remove a necessary cause of the disease. No matter what the other causes contributing to disease, if a necessary cause is removed, the disease cannot occur. That was the fundamental idea underlying the Rockefeller Foundation's effort to eradicate hookworm in the Southern United States; the worldwide expansion of its later efforts as the International Health Commission; and all subsequent efforts at disease eradication in the course of this century (Williams, 1969).

Perhaps the most articulate spokesman for the eradication concept was Fred Soper, who began his career working for the Sanitary Commission's hookworm campaign and who continued working on similar projects into the early 1960s. Soper pointed out that in the 1920s and 1930s the idea of eradication had been discredited as a result of the resilience of jungle yellow fever. By the end of World War II much had changed, however. In a lecture in 1959 Soper said:

> However justifiable the complacent acceptance of the persistence of preventable diseases may have appeared 30 years ago, it is no longer defensible. The success of local and national eradication efforts during the past three decades, the discovery of new methods of disease prevention, and the increasing participation of all nations in coordinated international health programs have led to rehabilitation of the eradication concept. Today the nations of the Americas are committed to the eradication of the *Aedes Aegypti* mosquito, malaria, smallpox, and yaws; the nations of the world have joined in the global eradication of malaria, and the demand for the eradication of other preventable diseases is inevitable. (Soper, 1970: 338)

Soper's was a rigorous definition of eradication, which he contrasted with disease control, to the disadvantage of the latter. In 1962 he defined them as follows: "The objective of control is to reduce the incidence of a given disease to a low level and to maintain this low level forever. The objective of eradication is completely to eliminate the possibility of the occurrence of a given disease, even in the absence of all preventive measures" (1970: 515).

If control is the objective, one may "glory in the percentage reduction of disease incidence, whereas in eradication any reduction short of the absolute leaves one preoccupied with the seeds of infection that remain." In control, interest tends to be lost just when, if eradication is the objective, difficulties become greatest. This also means that if control is the objective, the interests of the minority may be neglected because they may be the most inaccessible. In contrast, if eradication is the objective, everyone must be reached. "Eradication cannot be made available to part of the people; protection of all the population becomes the only acceptable professional public health standard" (1970: 516).

Furthermore, Soper pointed out in 1964, eradication means that special programs must be devoted to specific diseases. They cannot be diluted by being part of a general health service. By being narrowly targeted on one disease, they can be universal in coverage. Indeed, this has been an argument for what are generally called vertically organized programs.

Finally, "In control, one must count the cost as part of the continuing unending annual budgets; in eradication, one may capitalize future savings over an indefinite period against the peak costs of the years required for eradication" (Soper, 1970: 516).

This extreme view of eradication was not widely accepted, certainly not for tuberculosis, one of the diseases for which it was advocated in the 1950s. Most experts accepted the more modest goal of "eradication as a public health problem," rather than total eradication (Cockburn, 1967: 128). Total eradication has worked in one disease, however: by the early 1970s smallpox was declared to have been eliminated (Hopkins, 1983: 310). Malaria, another major disease against which war had been declared, had not been eradicated, although the number of cases had been reduced very substantially as a result of efforts at eradication (Bruce-Chwatt and de Zulueta, 1980: 179–182).

The eradication ideal seems to have been most popular during the 1950s, at a time when development theory seemed to imply that money spent on health programs was being misapplied because it would only lead to increased population, which would vitiate the effects of economic development. It was believed that funds should be invested productively and that in the long run the resulting economic development would lead to improved health for the entire population as benefits trickled down from the top to the bottom of the social hierarchy (Bryant, 1969: 96–98). The appeal of eradication programs in this context was that they were economical as well as humane, and that they did not require the expansion and restructuring of the health services of less developed countries or income redistribution and long-term commitments of aid.

By the 1960s what I have called the ecological approach was becoming more popular. As early as 1952 in his historical account of American epidemiology, John Gordon had said:

> If communicable disease is a pure ecologic process, its aim is that of all biologic processes, namely, to permit survival of both living elements. Nature is as much concerned with the welfare of the parasite as with that of the host. This has an important bearing on principles designed to guide prevention and control. The practical objective is not so much to eradicate disease, as to modify it to innocuousness. (Gordon, 1952: 118)

In his now-classic studies with Scrimshaw and Taylor of the interaction of nutritional status and infection, Gordon used this ecologic view to great effect (Scrimshaw, Taylor, and Gordon, 1968). In these studies it was shown that the nutritional status of the host had a significant bearing upon the severity of

infectious diseases, and likewise that an episode of disease—most importantly diarrhea—could cause significant deterioration in the nutritional status of the host. Thus the relationship between host and parasite was determined not simply by the virulence of the parasite but by the condition of the host. Disease, then, was multicausal.

This position became increasingly popular in the 1960s for several reasons. First, of course, the studies were well conceived and executed, and the results were impressive. Second, by the 1960s the significant causes of morbidity and mortality in many developing countries were less often the "named" disease than the "pneumonia-diarrhea complex," which was the result of any number of infectious agents and which was sensitive to the nutritional status of the host (McDermott, 1966).

And third, development philosophy had begun to change. One important variant was the "human capital" approach, in which population itself was considered a valuable resource. Thus investment in health and education was an investment in the improvement of human capital, which would ultimately lead to economic development through increased productivity (Lee and Mills, 1983). This meant that broadly based (horizontal) health programs rather than vertical disease-oriented programs were now favored by development specialists. Gordon and his colleagues wrote:

> It should not be forgotten that a high prevalence of debilitating infectious disease may also affect the nutritional status of a population by reducing the ability of many of its members to produce or earn their food supply. Populations with a great deal of endemic malaria, severe hookworm disease, schistosomiasis, and other infections common to tropical, underdeveloped areas are likely to lack the physical stamina to be efficient in agricultural and industrial labor. Heavily parasitized farm animals give a poor return for the food they consume and for the effort in caring for them. These direct effects on economic development provide major justifications for expanded health programs. (Scrimshaw, Taylor, and Gordon, 1968: 13; see also Taylor, 1965; Taylor and Hall, 1967; and Scrimshaw, 1974)

In its programmatic implications, the multicausal, ecological conception of disease was compatible with the human capital approach, for it advocated the expansion of health systems horizontally to cover a wide assortment of major causes of morbidity and mortality. The approach was adopted by the World Health Organization and the United Nations Children's Fund and ultimately came to be called primary health care. It was considered an alternative to the sophisticated health care available to the upper classes of most developing nations, as well as to the disease-eradication programs that had characterized the 1950s. Djukanovic and Mach wrote:

> The relative emphasis on programmes to control specific diseases may also have hindered the development of basic health services over the past 25 years. As

> early as 1951, when the efforts of many developing countries were centred on specialized mass campaigns for the eradication of diseases, the Director-General of WHO pointed out in his annual report that these efforts would have only temporary results if they were not followed by the establishment of permanent health services in rural areas to deal with the day-to-day work in the control and prevention of disease and the promotion of health. (Djukanovic and Mach, 1975: 7)

The WHO–UNICEF conference at Alma-Ata in 1978 formally endorsed primary health care, which "addresses the main health problems in the community" and provides promotive, preventive, curative, and rehabilitative services as one of the ways to deal with the disparities between the people of the developed and the developing countries (WHO–UNICEF, 1978). The goal was health for all by the year 2000. The conference report was as much a political as a public health document, based upon the assumption that declines in mortality and improvements in health could only be accomplished by broad changes in social and political organization, as well as massive expenditures on health and social services. Indeed, the report was placed within the context of economic and social development based upon a New International Economic Order requiring the transfer and redistribution of wealth from the developed to the developing countries.

There was criticism of this broad-gauged approach. For instance, Walsh and Warren from the Rockefeller Foundation considered the Declaration of Alma-Ata well meaning, even noble, but hopelessly unrealistic. They remarked: "The goal set at Alma-Ata is above reproach, yet its very scope makes it unattainable because of the cost and numbers of trained personnel required. Indeed, the World Bank has estimated that it would cost billions of dollars to provide minimal (not comprehensive) health services by the year 2000 to all the poor in developing countries" (Walsh and Warren, 1979: 967). The alternative they proposed, known as selective primary health care, was characterized as follows:

> On the basis of high morbidity and mortality and of feasibility of control, a circumscribed number of diseases are selected for prevention in a clearly defined population. Since few programs based on this selective model of prevention and treatment have been attempted, the following approach is proposed. The principal recipients of care would be children up to three years old and women in the childbearing years. The care provided would be measles and diphtheria-pertussis-tetanus (DPT) vaccination for children over six months old, tetanus toxoid to all women of childbearing age, encouragement of long-term breast feeding, provision of chloroquine for episodes of fever in children under three years old in areas where malaria is prevalent and, finally, oral rehydration packets and instructions. (Walsh and Warren, 1979: 972)

Critics called this a repackaging of vertical disease control programs (Gish, 1982; Berman, 1982; Unger and Killingworth, 1986). In a sense it was,

but it was more than that, for vertical disease-control programs tend to be aimed at one disease and typically have been concerned with eliminating the necessary cause of the disease. Selective primary health care was also concerned with the nutritional status of the human population and thus with reducing morbidity and mortality from a multiplicity of causes.

A more recent version of the same approach was based upon a consideration of the ways in which a number of low-income countries have managed to reduce mortality rates without substantial foreign support (Halstead, Walsh, and Warren, 1985; Caldwell, 1986). It was generally agreed that vertically organized categorical disease programs were an essential ingredient, but beyond that were a number of other important factors. Warren wrote, "One just can't wait for affluence."

> For the last decade at least there has been a model for health in the developing world which I shall call the Northern paradigm. The evolution of good health in the developed world or the North, in the terminology of the Brandt report, has been related particularly by McKeown to the process of development, i.e., the growth of a literate population living in spacious housing provided with piped water and sanitary facilities and supplied with the fruits of industry and agriculture via good roads and communication facilities. The allopathic medical system which gained ascendancy in the North had little to offer prior to the late 1930s or early 40s. Therefore, the governments of the developing world, aided and abetted by multilateral, bilateral and non-governmental aid agencies have been attempting to institute the Northern model of health. The cost of this approach is staggering . . .
>
> In the meanwhile, it appears that certain countries of the South have quietly evolved a different model, which I shall call the Southern paradigm, resulting in a remarkable reduction in infant and child mortality rates and increase in life expectancy. The basic elements of this approach as described by China, Kerala State, Sri Lanka, and Costa Rica appear to be only four:
>
> 1 Political and social will.
>
> 2 Education for all with emphasis on primary and secondary schooling.
>
> 3 Equitable distribution throughout the urban and rural populations of public health measures and primary health care.
>
> 4 Assurance of adequate caloric intake for all. (Halstead, Walsh, and Warren, 1985: 246)

There are several points to be made about this policy. First, as in the case of selective primary health care, modest vertically organized programs aimed at disease control have replaced disease eradication, suggesting that control is considered a more realistic goal than eradication even by those who favor low-cost programs. In a series of articles on infectious diseases edited by Walsh and Warren and published in *Reviews of Infectious Diseases* in 1984 and 1985, virtually none of the authors advocated eradication as a goal. Second,

multicausality has been adopted—though in a very modest form—even by those who consider cost to be an overwhelmingly important consideration. Third, the model of economic development that is said to have been responsible for mortality decline in developed countries is judged inappropriate for developing countries (which suggests that the adjective "developing" may not be appropriate any longer). But high-technology medicine is also inappropriate, for it too is expensive and does not address the needs of the population. The necessary ingredient is deemed to be "political and social will," which is the functional equivalent at the national level of responsibility for one's own lifestyle at the individual level, and which will allow poor countries to improve their mortality patterns independently of economic development and without having to depend upon large amounts of money from the industrialized countries.

W. Henry Mosley (1983) has observed that such modifications of primary health care (PHC) represent an implicit acknowledgment of the difficulties of implementing the very fundamental political commitments that are necessary to bring it to fruition. "As a result, PHC, rather than being a revolutionary force for change, is more often simply added as another appendage to the assortment of vertical programs directed to the masses" (1983: 5). "There remains [a] rather truncated definition of PHC which is essentially a top-down strategy to reach the community with some simple but theoretically effective preventive and curative technologies along with health motivation using various types of village level workers" (1983: 7).

Mosley's own alternative model of health dynamics of a population is far more complex and includes a variety of determinants—nutritional status, environmental contamination, maternal factors, personal illness control, injuries, and so on (Mosley and Chen, 1984: 29). It is based on the assumption that the "named diseases" that appear on death certificates are not necessarily the "cause" of death but simply the culmination of numerous insults that have weakened the organism. Thus, even if measles were to be eradicated, there might be no perceptible impact on mortality since some other "cause" would result in death.

> A novel aspect of this conceptual model is its definition of a specific disease state in an individual as an indicator of the operation of the proximate determinants [i.e., maternal health, etc.] rather than as a "cause" of illness and death. This is not to undervalue the usefulness of etiology-specific classification of disease and death for the development of rational therapeutic and preventive interventions. Rather, the aim is to emphasize the social as well as medical roots of the problem. This in fact is the standard approach of epidemiology, which begins with a biological problem in the host and then searches for its social determinants in order to develop rational control measures. The strategic approach of child survival research implied by this framework parallels methods

> used in the epidemiology of the chronic diseases rather than of the acute diseases. Chronic diseases such as heart disease are typically multifactorial in causality, have long latency periods between disease exposure and manifestation, and are powerfully influenced by lifestyle and socioeconomic circumstances. There is ample evidence in the medical literature that child mortality, especially in developing countries, also possesses these attributes. (Mosley and Chen, 1984: 28)

Clearly this is a far more "ecological" model of health dynamics than the one assumed by the advocates of selective primary health care; nonetheless, even the latter includes implicitly a conception of multicausality. Thus with respect to mortality and morbidity in both developing and developed countries, the notion of multiple weakly sufficient causes has emerged as dominant in epidemiology and public health, although in each instance its expression is shaped by a variety of considerations, primarily political ideology. In the concluding section I discuss these ideological issues and consider some of the objective phenomena with which all these authors are grappling.

Conclusions

It is clear that ideological considerations have much to do with the kinds of explanations of mortality patterns epidemiologists and public health workers have chosen in the past and continue to advocate. There has been much written, and some disagreement, about the attractiveness of the germ theory to philanthropists like John D. Rockefeller and his advisor, Frederick Gates, for example. Significantly, those competing explanations themselves represent ideological positions. A Marxist interpretation has it that the germ theory was so attractive because it rationalized vertical interventions in public health problems that could be scientized and depoliticized. Universally applicable knowledge administered with precision by trained experts (professionals) could remove the offending problem while leaving existing social institutions and relations intact (Berliner and Salmon, 1979; Brown, 1979).

This explanation has much to recommend it. Certainly Charles Chapin used the new theory of disease as a rationale for professionalizing public health and attempting to remove it from the arena of urban politics (Rosenkrantz, 1974). Fred Soper, too, was explicit about the need to define diseases as public health problems rather than social problems. Speaking of the requirements for tuberculosis eradication, he listed among other points: "Acceptance of tuberculosis as no longer essentially a social and economic but rather a public health and medical administrative problem" (Soper, 1970: 515).

But the issue is more complex than that, for as John Ettling has suggested with reference to Rockefeller and especially Gates (who had been trained as a Baptist minister), the germ theory was also deeply resonant with the notions

of sin they had acquired as part of their Baptist upbringing in western New York State in the second half of the nineteenth century.

> [Hookworm's] cure . . . did not seem to require a complicated or sophisticated environmental approach of the kind eschewed by evangelical Christians. Gates could regard its eradication much as Finney had foreseen the destruction of slavery seventy years earlier: that is, he could bypass politics, the law, economics, and social custom to reach out and touch the life of the individual sufferer (sinner/slave owner). (Ettling, 1981: 206–207)

Similarly, I have suggested that the variety of multicausal explanations offered in recent decades also reflects political ideologies and what seem to me to be deeply held assumptions about the nature of society, the existence of free will, and the requirements of justice. With respect to the chronic noninfectious diseases now most prevalent in the developed countries, for example, determinists extrapolate from the most deprived groups for whom options may not exist to the majority for whom the residual health problems are largely the result of personal behavior that is within their power to modify. The voluntarists, on the other hand, may have blinded themselves to the powerlessness of the poor by assuming that they are as able as the majority to control the most significant causes of morbidity and mortality. They thus express less concern than the determinists about problems of access to care, poor living conditions, unsafe work conditions, and the like.

But causes of disease and death are not simply a blank screen upon which we cast our prejudices, our cultural biases, our religious values, and our professional and political ideologies. They also exist as phenomena that can be known, however imperfectly. The importance we give to ideas of causal sufficiency and necessity also reflects the state of knowledge about the most prevalent diseases in any particular time and place—the state of knowledge often measured by our efficacy in dealing with them—as well as the characteristics of the disease process.

With respect to the changing balance between causal necessity and sufficiency, then, part of the explanation has to do with the characteristics of diseases themselves. Some infectious diseases seem not to be made more lethal by the nutritional status of the human host. Smallpox seems to be one example, malaria another. In the absence of nonhuman hosts and a complex mode of transmission, it has proven possible to eradicate smallpox from the face of the earth. Malaria has so far proven more intractable, although it has been reduced significantly.

In developing countries the recession, if not eradication, of diseases that are responsive to relatively simple interventions—those not requiring major behavioral changes on the part of individuals or major institutional reforms on the part of society—means that another set of diseases has become relatively more significant. These may be described as multifactorial because they are

not amenable to simple interventions and because host factors—notably nutrition and personal hygiene—determine the outcome. That seems to be the situation in most developing countries at present: the decline of the "named" diseases has resulted in the nonspecific "pneumonia-diarrhea complex" assuming a place of greater importance. Since this complex is by definition nonspecific in origin, the necessary cause explanation and eradication efforts based upon it are far less appropriate than horizontal interventions based on the notion of multiple weakly sufficient causes (Kunitz, 1986).

Something similar is occurring in the developed countries, where chronic noninfectious diseases are said to be the result of multiple sufficient causes and thus where horizontal interventions—either at the individual or institutional level—are said to be most appropriate.

In both developing and developed countries, therefore, public health now as in the nineteenth century is committed to an explanatory paradigm of multiple weakly sufficient causes. This is not simply because too little is known to penetrate to what a laboratory scientist might regard as the most fundamental, the most basic, and the most real causes of the most prevalent diseases. A great deal is known about the pneumonia-diarrhea complex, enough to say that the causes truly are multiple.

On the other hand, far less is known of the noninfectious diseases so prevalent in developed countries, and for some of them it is not unreasonable to think that a necessary cause may be found. If such a cause were found for some or all cancers, say, it might well lead to effective prevention and therapy. And it might create a revolution in ideas of causal attribution of the magnitude of the revolution created by the germ theory a century ago, particularly if it were part of a unified theory encompassing the major noninfectious diseases (Lower and Kanarek, 1982). It is thus unwise to assume that chronic diseases by their very nature are multicausal in origin. That may or may not turn out to be the case. In the meantime, now as in the nineteenth century, it is proving possible to reduce mortality and morbidity by reducing exposure to multiple weakly sufficient causes, that is, to risk factors.

Finally, it is important to observe that diseases, and what we define as diseases, will continue to change in the future as they have in the past. The emergence of AIDS, possibly from an animal source, would not have surprised Theobald Smith. New noninfectious diseases may also appear. And the debate over the definition of disease itself will certainly persist and perhaps grow more strident, for many of the behavioral and cognitive phenomena that result in death or social dysfunction will become relatively more important to the degree that other causes of death and disability wane. I have shown that there are differences even among epidemiologists in developed countries with respect to the boundaries they are willing to draw around what is and what is not a disease. The differences are at least as profound when other professions, not to mention entire societies, are considered (Kunitz, 1983). For as long as diseases, knowledge of diseases, and even what we define as diseases remain

unstable, there is no reason to believe there will be a permanent resolution of the debate between adherents of these two broad notions of causal attribution.

Notes

The author is grateful to Edith Efron for lengthy discussions of an early version of this article and to Stanley Engerman, Roger Schofield, and Theodore M. Brown for helpful comments. Versions of this article were presented at a seminar in the Department of Humanities and Social Studies in Medicine at McGill University in May 1987, and at a lecture at the annual meeting of the British Society for Population Studies later in the year.

1 Writing in 1955 on "A century of international mortality trends," George Stolnitz observed that postwar experience in the developing world had convinced many observers that mortality decline could occur in the absence of any significant economic development. He then commented, "It is worth noting . . . that a similar causal process may have been operative in the acceleration of Western survivorship a good deal earlier" (1955: 28–29). This represented a significant reinterpretation of the past and prompted much of the criticism of the "medical contribution" that has characterized subsequent debates.

It is also significant that in these same postwar years demographers began to revise their views of demographic transition theory and to suggest that fertility might be modified by technical interventions in the absence of major socioeconomic change (for an overview see Hodgson, 1983). This striking congruence of changing ideas of the causes of both mortality and fertility deserves further scrutiny.

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