The Epidemiologic Tradition
The Wade Hampton Frost Lecture

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THE WADE HAMPTON FROST LECTURE honors the memory of a career officer in the U.S. Public Health Service who, when the Johns Hopkins School of Hygiene and Public Health was established in 1919, was detailed by the Surgeon General to be resident lecturer, then professor of epidemiology, and subsequently head of the Departments of Epidemiology and Public Health Administration. The man chosen to develop the first university department of epidemiology represented, therefore, in his background as well as his academic responsibilities, the union of theory and practice which is a major strand in the epidemiologic tradition. Indeed, as Maxcy points out in his introduction to Frost's papers (*1*), "Whatever contribution he made to the improvement of the methods of epidemiology, he was cognizant at all times of the usefulness of this tool for the improvement of public health practice. This point of view was constantly emphasized in his teaching . . ."

This aspect of the epidemiologic tradition is one of a number that I shall discuss here. I shall not attempt to cover all facets of that tradition, but rather to explore those that are of particular interest and importance at this time.

The Nature of Epidemiologic Science

Epidemiology, the study of disease in society, is an extraordinarily rich and complex science. It must draw upon and synthesize knowledge from the biological sciences of man and of his parasites, from the numerous sciences of the physical environment, and from the sciences concerned with human society.

It has become fashionable, in recent years, to use this complexity as a convenient cloak behind which to hide from difficult problems. Instead of stating openly that our knowledge is inadequate, we fall back on the cliché of "multiple causation" in the noninfectious diseases, in contrast to the alleged single causes of infec-

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tious diseases. But there are no single causes of infectious diseases; their causes are multiple and entwined in a web of causation which is often more complex than that of many noninfectious diseases. It is well known that the causes of cholera in India today go back hundreds of years in India's history, to the British invasion and destruction of once-flourishing textile industries; the maintenance of archaic systems of land ownership and tillage; the persistence of the caste system and the unbelievable poverty, hunger, and crowding; the consequent inability to afford the development of safe water supplies and sewage disposal systems; and, almost incidentally, the presence of cholera vibrios.

It can be argued that no case of cholera can occur without the presence of the cholera vibrio, whereas 10 percent of lung cancer cases occur among nonsmokers. But it is also true that no case of scurvy, for example, occurs without vitamin C deficiency, whereas there are multiple bacterial and viral causes of upper respiratory infections, bronchitis, pneumonia, gastroenteritis, meningitis, and encephalitis.

Not only is epidemiology a complex science, but its data reflect that complexity, coming partly from clinical medicine, partly from the laboratory, and largely from the community itself. Epidemiology is a field science, and any attempt to withdraw from the field into academic seclusion is self-defeating.

Since much of the data obtained in the field on possible etiological factors are rough, approximate, and open to criticism in terms of accuracy, epidemiologic studies often lack the precision and elegance of laboratory investigations. Yet, as all laboratory investigators know, their work also suffers a fair share of unreliable and inaccurate data. Furthermore, a remarkable fact about the science of epidemiology has been its ability to "make a silk purse out of a sow's ear." Taking the inevitably crude and approximate data that one must work with in the field—in investigating living human beings in society—William Budd and John Snow demonstrated the modes of transmission of typhoid fever and cholera, Kenneth F. Maxcy elucidated the epidemiology of murine typhus, Joseph Goldberger proved that pellagra is a nutritional deficiency disease, and numerous current investigators have established the relation of tobacco and alcohol to a variety of major diseases.

The Relation to Medical Science

Unlike a number of other social sciences, epidemiology has developed a scientific tradition which is almost unparalleled both in its strictness and in the universality of its influence. In large part, this tradition resulted from the close relation with vital statistics—symbolized in the 19th century in the person of William Farr. In our century, three schools of public health that have had enormous national and international influence have embodied this tradition in slightly different ways: the London School of Hygiene, where Major Greenwood, a physician who was also a pupil of Karl Pearson, was professor of epidemiology and vital statistics; the Johns Hopkins School of Hygiene, where Frost in epidemiology and Lowell Reed in biostatistics developed a very close and fruitful collaboration; and the School of Public Health of the University of Chile, where an epidemiologist, Hugo Behm, held the chair of professor and chairman of biostatistics.

The important role of statistics in the science of epidemiology is well recognized by other medical disciplines. In most medical schools, statistics is taught by departments of preventive medicine, and the National Board of Medical Examiners includes biostatistics as part of the preventive medicine examination.

This close relation of epidemiology and statistics has become even more marked in recent years, as epidemiologists have increasingly sought help from their statistician colleagues to separate the effects of multiple variables. At the risk of exposing ignorance or prejudice, or both, I am inclined to judge that the results of this collaboration to date have been somewhat disappointing. These attempts should be pursued, nevertheless, with continued vigor and, I hope, with newer approaches and greater ingenuity.

One danger in an overemphasis on statistical approaches is the tendency to neglect the fact that epidemiology is a biological science concerned with disease in human beings; indeed, it is one of the basic medical sciences. In commenting on Farr's stature as an epidemiologist, Greenwood (*2*) pointed out that:

> On the other side, the biological side, of epidemiology, Farr had insight enough to perceive the importance of such work as that of Henle and, of course, that of Pasteur, but he was not a pioneer. Biologically he did not see so far ahead as either Snow or Budd; sometimes he is almost verbalist, seems to attach too much importance to mere nomenclature. He lacked basic knowledge; he had not, like Henle or Pasteur, done any biological or chemical research work himself and lived before the time when all medical students received some laboratory training.

The lack of a biological orientation has, in our time, led a number of eminent statisticians into serious error. As Abraham Lilienfeld pointed out, Joseph Berkson, J. Yerushalmy, R. A. Fisher, and others took a negative position in the lung cancer controversy because they had a purely statistical view of the problem; they failed to recognize the important fact that the smoking hypothesis was pathobiologically reasonable and sound.

The clinical disciplines are a major component of the biological side of epidemiology. They not only help determine whether hypotheses are biologically feasible, but they also identify the specific cases that require epidemiologic investigation. Most important, the clinical disciplines provide a major source of epidemiologic hypotheses. For example, it was the chest surgeons such as Evarts Graham and Alton Ochsner who first noted the association of cigarette smoking and lung cancer in their patients. The epidemiologists came later, carrying out the numerous investigations which tested and eventually proved the truth of the surgeons' hypothesis. And it was an Australian ophthalmologist, N. McAlister Gregg, who, on the basis of observations made originally in his practice, developed and tested the hypothesis that rubella early in pregnancy causes congenital malformations.

The history of epidemiology is one of periodic enrichment with the talents of deserters from the various fields of clinical and laboratory medicine. The motive for desertion was typified by that of the outstanding French hygienist of the 19th century, Louis René Villermé, who gave up medical practice in despair in 1818 because of the therapeutic impotence of clinical medicine (*3*).

More than 125 years later, on the threshold of the second epidemiologic revolution, John Ryle signaled the beginning of a powerful movement of chronic disease epidemiology by resigning as regius professor of medicine at Cambridge to become the first professor of social medicine at Oxford. His motive was much like Villermé's (*4*):

> Thirty years of my life have been spent as a student and teacher of clinical medicine. In these thirty years I have watched disease in the ward being studied more and more thoroughly—if not always more thoughtfully—through the high power of the microscope; disease in man being investigated by more and more elaborate techniques and, on the whole, more and more mechanically. . . . The morbid 'material' of the hospital ward consists very largely—if we exclude the emergencies—of end-result conditions for which, as a rule, only a limited amount of relief repays the long stay, the patient investigation, and the anxious expectancy of the sick man or woman. With aetiology—the first essential for prevention—and with prevention itself the majority of physicians and surgeons have curiously little concern.

Now, some 30 years later, we find a similar phenomenon in a burgeoning area, the epidemiology of cardiovascular disease, in which the relatively few professional epidemiologists have been joined by large numbers of pathologists, physiologists, chemists, pharmacologists, internists, and other specialists concerned with the causes and prevention of diseases of the heart and arteries. The verve and spirit of these epidemiologists, whatever their background, is reminiscent of William Henry Welch's description of the scientific explosion that ushered in the first epidemiologic revolution (*5*):

> At the end of that wonderful decade, 1880–1890, perhaps the most wonderful decade in the history of medicine, there had been a revolution in medical thought through the discovery of the agents causing infectious diseases—such discoveries as the bacillus of tuberculosis, of Asiatic cholera, of diphtheria, of typhoid fever and other infectious diseases. Those living today can hardly realize the enthusiasm and youthful spirit which was stirred not only among medical men, but in the general public by these discoveries.

Infectious and Noninfectious Diseases

In 1927 Frost (*1*) noted the uncertainty of usage of the term "epidemiology," pointing out that:

> It seems customary also to apply the term to the mass-phenomena of such noninfectious diseases as scurvy, but not to those of the so-called constitutional diseases, such as arteriosclerosis and nephritis. Therefore, in view of the latitude which the uncertainty of usage allows, epidemiology will be considered here as referring exclusively to the diseases of man which are classed as specific infections, since this will permit of a more coherent discussion. In this sense epidemiology may be defined as the science of the mass-phenomena of infectious diseases, or as the natural history of infectious diseases.

Note that it is only "in this sense," to "permit of a more coherent discussion," that Frost limited his definition of epidemiology to the infections. Further, he went on to quote Hirsch's description of historical and geographical pathology as offering a fuller definition of epidemiology, namely:

> A science which . . . will give, firstly, a picture of the occurrence, the distribution and the types of the diseases of mankind, in distinct epochs of time and at various points of the earth's surface; and secondly, will render an account of the relations of these diseases to the external conditions surrounding the individual and determining his manner of life.

My generation of epidemiologists, who were trained in the late 1930s and early 1940s in the principles and methods of epidemiology developed for the infectious diseases, found it relatively easy to apply those principles and methods to the noninfectious diseases. In taking this road we were following a tradition created many years before by Goldberger, whose brilliant investigation of pellagra is one of the great classics of epidemiology (*6*). Before his assignment to the pellagra problem by the Surgeon General of the Public Health Service in 1914, Goldberger had been concerned entirely with infectious diseases. Early in his career he had been one of a group of Public Health Service officers, including Frost, assigned to control an epidemic of yellow fever in New Orleans. Subsequently, Goldberger investigated typhoid fever in Washington, D.C., dengue in Texas, typhus fever in Mexico, diphtheria in Detroit, as well as measles and other diseases.

Goldberger's career is a striking reminder of the essential unity of infectious and noninfectious disease epidemiology. Another is the career of the 1978 recipient of the John Snow Award, Morton Levin, who was a student and then a colleague of Frost in the studies of diphtheria in Baltimore and is now being honored for his outstanding work in the epidemiology of cancer.

There is a growing tendency, nevertheless, to separate these two areas into airtight compartments. It has been proposed, for example, that certification in epidemiology be permitted either in infectious or noninfectious diseases. This is unjustified, for not only are the principles and methods essentially similar in the two areas, but our current knowledge of the noninfectious diseases indicates that some actually do result from infections; the congenital malformations caused by rubella are a case in point. Further difficulties would occur with the newer areas of epidemiology, since it will be logical to demand separate certification for the epidemiology of trauma and the epidemiology of health, neither of which can be considered to be part of the noninfectious diseases. And what will be done with those who are studying the effects of medical care programs and using both infectious and noninfectious diseases as their markers? Will they also be considered different enough to require separate certification?

Observation and Experiment

We are fortunate that—unlike a number of the physical, biological, and social sciences—epidemiology is clearly an experimental as well as an observational science. This is attested to by many examples: the numerous vaccine trials, the work of Walter Reed and his colleagues which proved Carlos Finlay's hypothesis that yellow fever is transmitted by mosquitoes, Goldberger's brilliant experiments demonstrating that pellagra is a nutritional deficiency disease, the community experiments that showed the effectiveness of sodium fluoride in lowering the prevalence of dental caries, the Health Insurance Plan of Greater New York study of the value of screening by clinical examination and mammography in reducing breast cancer mortality, and most recently the various field experiments for the prevention of cardiovascular and cerebrovascular disease.

One of the most inspiring aspects of the epidemiologic tradition—and one that we would miss if we limited our consideration to the noninfectious diseases—is the willingness of epidemiologists to take serious risks by experimenting upon themselves. In the United States this tradition was largely associated with the Hygienic Laboratory of the Public Health Service, that remarkable organization which produced a very large proportion of America's outstanding epidemiologists, including Frost. In 1909, when the Director of the Hygienic Laboratory, Milton J. Rosenau, left to become professor of preventive medicine at Harvard Medical School, he was succeeded by John F. Anderson. In the same year, Anderson and Goldberger began their important work on typhus fever in Mexico, which included the following experiment in which Anderson was the subject (*6*):

> F.J. adult, American, nonimmune, lived at a hotel in Mexico City, but came in daily intimate contact with cases of tabardillo (typhus fever—M.T.) between November 22 and December 16, 1909. On the nights of January 5 and 6 he slept in a bed that had been occupied on January 2, 3, and 4 by a patient in the first three days of a well-marked attack of tabardillo. None of the bedding or bedclothes had been in any way disturbed in the interval prior to their use by this individual. At the end of three days the bedclothes were changed, but with this exception the bed and room remained as they had been when occupied by the patient. F.J. inhabited this room for three weeks longer. On careful search no insects other than fleas were found in the room. During a period of observation of 17 days this man continued in his usual health.

Even more startling, perhaps, were the pellagra experiments conducted by Goldberger in 1916. The volunteers consisted of Goldberger and other members of the Hygienic Laboratory, including its new director, George W. McCoy, as well as other Public Health Service officers (*6,7*). (It is of some interest that one of the Hygienic Laboratory volunteers was a 32-year-old medical officer, Warren F. Draper, who many years later became the Executive Medical Officer of the United Mine Workers Welfare and Retirement Fund.)

The 16 volunteers received materials from 17 pellagra patients. Blood was administered by intramuscular or subcutaneous injection; secretions by application to the mucosa of the nose and nasopharynx; and scales and excreta by mouth. Both urine and feces were ingested by 15 volunteers, 5 of whom also took blood, secretions, and scales. Although in four or five instances temporary gastrointestinal reactions followed the ingestion of the large doses of excreta, there was no evidence of pellagra, and Goldberger remarked, "When one considers the relatively enormous quantities of filth taken the reactions experienced were surprisingly slight" (*6*).

Epidemiologists engaged in experimental work, whether as investigators or as subjects, did not always escape serious consequences. Howard Taylor Ricketts, for example, died of typhus fever while investigating that disease in Mexico City in 1910 (*7*). T.B. McClintock of the Hygienic Laboratory contracted a fatal infection in Victor, Mont., while doing experimental studies of Rocky Mountain spotted fever (*7*). Walter Reed's colleague, James Carroll, described the results of his experimental bite by a mosquito infected with yellow fever and of Jesse Lazear's presumably accidental bite (*8,9*):

> After having slight premonitory symptoms for two days I was taken sick on August 31, and on September 1 I was carried to the yellow fever camp. My life was in the balance for three days, and my chart shows that on the fifth, sixth, and seventh days my urine contained eight-tenths and nine-tenths of moist albumin. The tests were made by Dr. Lazear. . . . Thus it happened that I was the first person to whom the mosquito was proved to convey the disease. On the eighteenth day of September, five days after I was permitted to leave my bed, Dr. Lazear was stricken and died in convulsions just one week later, after several days of delirium with black vomit. Such is yellow fever.

These accounts offer some measure of the courage and devotion to the public good which is basic to the epidemiologic tradition. That tradition was forged, it should be noted, not by private practitioners, but by salaried people with modest incomes, mostly government workers, those who are often described as timeservers and soulless cogs in the bureaucratic machine. How many of our practitioner colleagues would have been willing, in the fight against human disease and suffering, to ingest the discharges from pellagra patients that Goldberger administered to George McCoy, Edgar Sydenstricker, G.A. Wheeler, Warren Draper, and the rest of that gallant group of Public Health Service volunteers?

The Web of Causation

In discussing the question of single versus multiple causes earlier in this paper, I mentioned what MacMahon and Pugh (*10*) so aptly termed the web of causation, a concept which is not only basic to epidemiologic thinking but which also has significant bearing on the measures taken for prevention and control. It is unfortunately true that, despite general acceptance of this concept, many noninfectious disease epidemiologists are guilty of the same error that was made by many of their infectious disease predecessors who tended to focus attention primarily on the micro-organism. Now it is the particular agent, or risk factor, or vehicle that is the center of attention; this is considered to be the cause, while the whole complex of social and other environmental factors that create that cause, and bring it into effective contact with the host, tends to be ignored. The epidemiologic tradition, apparently, maintains its continuity impartially for its negative as well as its positive aspects.

Such narrowness of view, however, does not accord with the best of our tradition. Snow, in his classic investigation (*11*) that was so profoundly admired by Frost (*1*), documented the transmission of cholera not only through water supplies, but also through person-to-person contact and other means of fecal-oral spread. Nor did he neglect to analyze in detail the socioenvironmental determinants of that spread, as indicated by this example:

> Nothing has been found to favour the extension of cholera more than want of personal cleanliness, whether arising from habit or scarcity of water, although the circumstance till lately remained unexplained. The bed linen nearly always becomes wetted by the cholera evacuations, and as these are devoid of the usual colour and odour, the hands of persons waiting on the patient become soiled without their knowing it; and unless these persons are scrupulously cleanly in their habits, and wash their hands before taking food, they must accidentally swallow some of the excretion, and leave some on the food they handle or prepare, which has to be eaten by the rest of the family, who, amongst the working classes, often have to take their meals in the sick room: hence the thousands of instances in which, amongst this class of the population, a case of cholera in one member of the family is followed by other cases; whilst medical men and others, who merely visit the patients, generally escape. . . .
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> It is amongst the poor, where a whole family live, sleep, cook, eat, and work in a single room that cholera has been found to spread when once introduced, and still more in those places termed common lodging-houses, in which several families were crowded into a single room. It was amongst the vagrant class, who lived in this crowded state, that cholera was most fatal in 1832; but the Act of Parliament for the regulation of common lodging-houses, has caused the disease to be much less fatal amongst these people in the late epidemics.

Similarly, in the great classic of noninfectious disease epidemiology, Goldberger and Sydenstricker made a detailed and thorough analysis of the web of causation of pellagra in the lower Mississippi River area, including three sets of conditions: (*a*) the dietary habits of the inhabitants, (*b*) the tenant farm system of cotton production, and (*c*) the availability of supplies of various foods which, in turn, is influenced by the one-crop type of agriculture, with the consequent lack of diversification, and by the dietary habits of the people (*6*).

Parenthetically, it is of some interest that Sydenstricker, the Public Health Service economist, who with Goldberger pioneered in the development of morbidity survey methods in the pellagra studies, was then assigned during the influenza pandemic of 1918 to work with Frost in studying influenza morbidity. Maxcy commented that this began "a congenial and productive relationship which had a profound influence upon the development of both men" (*1*). Sydenstricker was later responsible for the Hagerstown morbidity studies. Thus, there is almost a direct line from Goldberger, Frost, and Sydenstricker to the later studies culminating in our present National Health Survey.

Epidemiology and Prevention

Epidemiology is not only a complex science, but one which is designed for application. The call for ameliorative measures is almost explicit in this excerpt from one of Farr's official reports (*2*):

It is found that of 10,000 children born alive in Liverpool, 5,396 live five years; a number that in the healthy districts could be provided by 6,554 annual births. This procreation of children to perish so soon—the sufferings of the little victims—the sorrows and expenses of their parents—are as deplorable as they are wasteful. In Liverpool the death of children is so frequent and dreadful, that a special system of insurance has been devised to provide them with coffins and burial ceremonies. The mother when she looks at her baby is asked to think of its death, and to provide by insurance not for its clothes but for its shroud and other cerements.

As Dorn pointed out, Farr "had no interest in the preparation of files of documents and in the compilation of statistical tables as an end in themselves; he was interested in the statistics as a means of social reform. . . . Farr devoted his entire official career to the task of using the records that flowed to the General Register Office to portray important health and social problems and to measure the effects of sanitary legislation" (*12*).

In some instances, the publication of morbidity data had almost immediate effects. In Paris in 1840, Villermé published his "Survey of the Physical and Moral Conditions of the Workers Employed in the Cotton, Wool, and Silk Factories," which revealed incredible conditions of squalor, overwork, and misery, especially among child workers. More fortunate than most epidemiologists, Villermé had the satisfaction of seeing that, due to the pressure of an alarmed public opinion, a law limiting child labor was promulgated the next year (*3*).

The great classics of epidemiology do not limit their concern to the scientific basis for action, but move logically from that basis to urge adoption of measures for prevention. Snow, for example, ends his paper "On the Mode of Communication of Cholera" with seven measures to be taken during an epidemic of cholera and five to be taken beforehand to prevent one. Among the latter are a number of serious demands on society (*11*):

To effect good and perfect drainage;

To provide an ample supply of water quite free of contamination with the contents of sewers, cesspools, and house-drains, or the refuse of people who navigate the rivers; and

To provide model lodging-houses for the vagrant class, and sufficient house room for the poor generally. The great benefit of the model lodging-house arises from the circumstance that the apartments for cooking, eating, and sleeping, are distinct, and that all the proper offices which cleanliness and decency require are provided. The very poor who choose to avail themselves of these institutions suffer a rate of mortality as low as that of the most opulent classes. The public wash-houses, which enable poor persons to wash the soiled linen of the sick or the healthy, without doing it in the midst of the plates and dishes and provisions of the family, are well calculated to prevent the spread of disease.

Goldberger and Sydenstricker similarly outlined a series of recommendations for the prevention of pellagra, including improvement and stabilization of tenant income, diversification of agriculture, increase in the milk supply through community dairies and other means, increases in cattle, swine, and poultry products, and greater development of vegetable gardens in tenant farms (*6*).

Although much alike in their concern that their findings be used for prevention, Snow and Goldberger were quite dissimilar with regard to the circumstances which led to their investigations. Snow was a clinician who acted because of his personal interest and concern; he was, in current parlance, a part-time volunteer. Goldberger was a government servant, assigned to pellagra by the Surgeon General, who wrote to him that "Within the past several weeks the importance of pellagra has been urged on me by members of Congress and other prominent people from sections in which the disease prevails." (*6*).

In the United States, the epidemiologic tradition has generally fostered a close union of theory and practice—of epidemiology and prevention—because almost all of the early epidemiologists worked for health departments whose basic mission was disease prevention. Outstanding epidemiologists were produced, not only by the Hygienic Laboratory of the Public Health Service, but also by a number of State and city health departments. One of the most important of these epidemiologists was Charles V. Chapin, Health Officer of Providence, R.I., from 1884 through 1931. Chapin's book on "Sources and Modes of Infection" (*13*), first published in 1910, revolutionized U.S. public health practice by urging that it be made consistent with the new epidemiologic knowledge.

Shift to the Academic Milieu

Although epidemiologists at the Federal, State, and local levels of government still play an important role today, epidemiology has shifted considerably to the schools of public health as a major locus. This change to the academic milieu has been mitigated by the fact that, like Frost, a number of the leading teachers come from a background of health department experience in both infectious and noninfectious disease epidemiology. Nevertheless, some of the intrinsic tendencies of academic life have become increasingly evident: a greater concern with the methodology of data manipulation than with the solution of disease problems; a withdrawal from the community, from field studies in which the investigator knows his data and their limitations, and the increased use of someone else's data regardless of their value; an orientation geared more to the goal of "publish or perish" than to the goal of preventing disease and death; and finally, an arrogant and elitist attitude toward the local health officer that is similar to the academic clinician's attitude toward the local medical practitioner.

Academic epidemiologists, however, are also servants of the government, although indirectly. The National Institutes of Health not only control the quality of epidemiologic and other research but also its direction by the allocation of funds to specific areas. These allocations are made in the first instance by the Congress and reflect public concern with the diseases that strike most heavily at the population.

More than half a century ago, the Public Health Service assigned Frost to Johns Hopkins University to develop the first university department of epidemiology. Such departments have since flourished greatly, largely as the result of Federal grants. Now, when the time has arrived to fulfill the promise of the second epidemiologic revolution, to put into practice the epidemiologic knowledge which enables us to prevent some of our most important diseases, we are handicapped by a lack of epidemiologically trained personnel to develop the control programs on a sound scientific basis. It would be most appropriate, therefore, for the schools of public health to practice a "reverse lend-lease," in honor of Frost, by assigning epidemiologists to local, State, and Federal health departments, either part time or full time, to help work out not only the strategy and tactics but the operation and evaluation of the programs for prevention. These epidemiologists could in turn be helped to enrich the academic programs of the schools of public health; they could not only train health personnel for the new programs, but they could also recast their course work to teach epidemiology and disease control as a single, indissoluble discipline.

Tradition and New Directions

My generation of epidemiologists, coming to maturity during the Great Depression, the New Deal, the civil war in Spain, and the war against fascism, could not ignore the dire need for the melioration of the human condition, the prevention of human misery. We also developed, paradoxically perhaps, a general attitude of optimism and faith in the further progress of society and humanity.

My generation, furthermore, was extraordinarily privileged. Trained in the epidemiology of infectious diseases, we did much of the basic work involved in unraveling the epidemiology of some of the major noninfectious diseases.

Our successors, the new generation, will have two major tasks. One is to extend epidemiology further—to attack the unsolved problems of infectious and noninfectious diseases, to carry out major work in the occupational diseases, to develop serious research in the epidemiology of health, and to study the effects of public health and medical care services on disease and its outcomes. The other task, which is of the utmost importance, is to carry out the second epidemiologic revolution—the control of noninfectious diseases and trauma—which my generation has hardly begun.

I hope that, in carrying out these tasks, the new generation will become thoroughly imbued with the great tradition of epidemiology, and that they will not only safeguard but enrich it. A principal aspect of that tradition is involvement with the life of the human community; as C.P. Snow stated, "There is a great dignity in being a spectator: and if you do it long enough, you are dead inside" (*14*). Even more central to the epidemiologic tradition is an admonition by Horace Mann (inscribed on his monument at Antioch College) which undoubtedly reflects the basic motivation of Farr, Villermé, Snow, Goldberger, Finlay, Reed, Lazear, Ricketts, Sydenstricker, Chapin, Frost, and a host of other epidemiologists: "Be ashamed to die until you have won some victory for humanity." I would only add: "however small."

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