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EPIDEMIOLOGY TODAY:
'A THOUGHT-TORMENTED WORLD'
Epidemiology Today: ‘A Thought-Tormented World’*

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Epidemiology has seen many theoretical advances over the past 20 years. Since the advances of one period often become the impediments of the next, it is timely to seek out disorders of thought that may beset our studies of disorders of health. In this undertaking, I speak as a premillennial epidemiologist who has bent effort to promoting one kind of modern epidemiology. My intention is not to decry but to look forward. For epidemiology that is still to be, therefore, I shall venture some value-laden hopes.

Premodern epidemiology was primarily an epidemiology of substance. It was epidemiology for problem solving and for the prevention or control of disease, subservient to this purpose and hence to its subject matter. At the same time, it was often epidemiology pursued intuitively, an avocation (excepting infectious disease epidemiologists) of gifted amateurs using primitive means. It engendered the excitement of the unexplored, and the frustration of technical incapacity. This kind of epidemiology was in decline by the 1950s and passe by, say, the early 1970s.

Present day epidemiology, on the other hand, is primarily an epidemiology of technique, at risk of existing for its own sake regardless of subject matter. It is epidemiology pursued as a vocation by accomplished professionals who deploy refined and complex methods. In the absence of a central concern with subject matter, the satisfactions of technical command are held within narrow bounds, in the absence of broader purpose, an arsenal of methods might not necessarily be directed to the benefit of the public health.

The transition from premodern to modern epidemiology was governed first by the historical shift in the substance of epidemiological studies from acute infectious diseases to chronic non-infectious diseases. The shift in substance elicited advances in research design and in quantitative methods. At the same time, the economic and social impetus of industrialization that brought about the changes in disease pattern was accompanied by the now familiar technical developments that made a sophisticated modern epidemiology possible. I refer of course to the combination of rapid large-scale data processing with the unimagined analytical facility conferred by digital computers.

New and powerful technology often exacts unanticipated costs in changed ideas, goals and values. To judge from present day texts and from present day programmes in the academic mainstream, concentration on technique has been accompanied by a change in the focus of epidemiology. The tendency is to operate on a single plane. Our works tend to be flat and not rounded, caricatures rather than portraits. The variables analysed are multiple, but they are often divorced both from biological substrate and from societal context. A divorce from biology sacrifices depth. A divorce from society sacrifices breadth.

Underlying the modern trend common themes can be discerned. It may not be too much to say these hang together as a sort of ideology. Precisely because I take this to be a trend in collective thought,1 I omit citations of individual works.

Styles of Thought in Epidemiology
One major component in current styles of thought flows from efforts to generalize epidemiology as an analytical task. Another flows from efforts, especially in the US, to generalize it as a philosophical task. These components occur sometimes separately and sometimes together, but both tend to narrow the limits of the discipline.

First, I turn to consider briefly the analytical task of epidemiology. Creative scholarship is in many fields analytical and also self-contained, given over to the advance of the field for its own sake. The exclusive pursuit of analytical technique, however, fosters a virtuous in which value-free means can be substituted.
for valuelined. Technique, hybridized with a thoroughgoing scepticism, seems also to have bred an ahistorical perspective. Scepticism can be a well spring for discovery, it can also dissipate. Scepticism that flows from history and counts its constraints is a stimulant. An ahistorical scepticism can lead to the dry well of solipsism and self absorption. In prototypical works, it is all too apparent that antecedents, precedents and analogies are few.

Thus, in contrast with the past, some see epidemiology as a discipline with no object other than to study relations between exposures and effects, with effects largely conceived as disease. Accordingly, distinctions between descriptive and analytical epidemiology are denied. A new definition of epidemiology in this mode—simply, the study of disease occurrence—illuminates the bypassing of precedents and the narrowed focus. This definition does in fact go to the pith of an existing definition of epidemiology, namely the study of the distribution and determinants of health states in populations. The connection between the new and the old, however, is not vouchsafed together with the new definition. Nor can one find the purpose-oriented addendum—commonly attached to the old definition to respect the origins of the discipline in public health and the corresponding values of most of its practitioners—that epidemiology aims to prevent or control disorders of health.

The new definition states the business of the discipline succinctly and at a high level of abstraction. Abstractions can enclose the essence of a problem; the intellectual effort and rewards involved are not to be deprecated. Yet to work solely in abstractions is necessarily to float above the uneven surface of the mundane and the diverse. Thus the older and wonderer definition carries no superabundant baggage if it is to convey the concrete activity of epidemiology in its task of understanding the states of health of populations.

The diverse subject matter of health alone distinguishes epidemiology from other population sciences. Within epidemiology also, subject matter leads to distinctions ignored by some who subscribe to the new definition. Not to perceive differences between the epidemiology of chronic non-infectious and acute infectious diseases, for example, can legitimately be taken as a symptom of inattention to the diversity of substance. Admittedly, the principles, the definitions, and the objectives of epidemiology remain the same for both chronic disease and acute infectious disease. Yet the tactical differences between the two fields can be overlooked only if the significance of substance is passed by. The transmission and acute effects of microorganisms on one side, and the gradual development of physiological dysfunction on the other, do not call on the same domains of biological, social and environmental knowledge.

Insofar as the new definition emphasizes disease, what it omits betrays a probably unintended bias. A perspective that always begins with disease may discover an array of causes but obscures the array of possible outcomes, a seminl insight taught by John Cassel. For public health, a perspective that begins with environmental factors (say, contaminated water or social situation) and searches out the consequences leads to discoveries less likely to be made from the opposite perspective. Further, the array of health consequences does not fit into the narrow confines of the disease concept, let alone a single disease: outcomes germane to modern epidemiology include disease, illness and sickness; impairment, disability and handicap; and individual development as well.

A second component in the collective thought of present day epidemiology, noted above, turns on the philosophy of science. To address epidemiology as a philosophical task in the manner of some modernists is to go to the nub of the discipline in its concern with causality. Thus, some authors advise us to abandon generalization by induction from particular facts, together with the supposedly fruitless task of verifying causality; the sole valid result of a test is held to be in its refutation. Along with this goes the dismissal of criteria of judgement to buttress causal inference, also a requirement of the stand against induction. From this stance, it is a small step to brush aside descriptive epidemiology altogether. Traditionally, description has been both the necessary prelude to knowledge about the variation, scale and public import of disease and the frequent progenitor of new hypotheses. We shall see that the problems engendered by this philosophical approach are in the last resort again those of high-level abstractions that must ignore the concrete diversity of the human world we study.

The various challenges thrown out by modernists deserve more critical attention than they have so far received. As to the analytical task of epidemiology, I shall do no more here than I have done above, which is to give an outline of a sample of related positions. As to the philosophical task, I shall bring some episodes in the history of discovery to bear on the new debate initiated by the followers of Karl Popper. Although neither philosophers nor historians will mistake my efforts for those of a professional, the problems of epidemiology are for epidemiologists to solve. The examination of logic in epidemiology leads on to a brief discussion of science in society and scientific values and
I conclude with some suggestions about where in epidemiology we might turn.

Failures of Induction: Swans and Achromatopia

Scepticism is a value that most epidemiologists share with Popper. Popper's scepticism, however, is of another order than the common variety: he holds that all knowledge is provisional and unverifiable. This radical scepticism was fathered by David Hume who concluded, in the eighteenth century, that induction is always fallible. Because the sun has risen every day for as long as we have had knowledge, Hume argued that there was no reason in logic to say that it will do so tomorrow. In any inductive procedure, as many particular events as one may assemble to reach a generalization may yet be contradicted. In the philosophers' pet example, the generalization that swans were always white did not rule out the possibility of black swans. Indeed, those were European swans, the discovery of Australia led in turn to the discovery of black swans.

In medicine, a more recent example of the fallibility of induction unfolded with the elucidation of acquired colour blindness (or cerebral achromatopsia), a story recounted by Oliver Sacks and Robert Waterman. During World War I, the British neurologist Gordon Holmes collected a large and seemingly definitive series of cerebral injuries with visual disorders. He found no single case of cerebral achromatopsia. As a result, the disorder was dismissed from the medical literature. Yet, around 1890, cerebral achromatopsia had been well described. Only around 1975 did experimental animal studies followed by clinical re-evaluation bring the disorder back from exile. It is turned out that Holmes' sample was skewed towards occipito-parietal lesions, whereas the disorder occurs with occipito-temporal lesions. (My guess is that few survived occipito-temporal combat wounds.)

Failures of Deduction: Witchcraft and Scurvy

As an alternative to induction, the process of inferring a particular outcome from a generalization by deduction offers little refuge from false inference. This logic opens the road to self-fulfilling prophecies, predictions or theories that when put to the test are immune from failure. Witchcraft exemplifies the difficulty. Complacent of our own civilization, the cruelties visited on so-called witches were once a staple of school texts and of colonial tales of primitive tribalism. Examination of witchcraft trials, and of the practices Evans-Pritchard described among the Azande in the 1930s in the Sudan, shows the accusations to have been founded on coherent theories. The theory attributed unusual events to the exercise of supernatural powers that inhibited certain individuals. These were theories, moreover, often subjected to hypothesis testing. The test might require the suspected witch to endure some ritual ordeal, or to refute assertions by witnesses that were not susceptible to any kind of proof; by deduction, failure proved guilt ipso facto.

In the period that preceded Francis Bacon's promotion of the era of inductive science and empiricism, a priori theories and deductions from those theories generated not only witchcraft but much of medicine. In those times, a mere observation was often insufficient to displace a good theory. The second-century theories of the great Galen, founded on the balance of the four humours, and adopted by medieval Europe from the Arabic world, could still dominate medicine into the sixteenth and seventeenth centuries.

Some of the impediments to science created by the insistence on theory and deductive logic are well illustrated by the history of scurvy and the discovery of vitamin C, as recounted by Kenneth Carpenter. James Lind, best known to us as the forebear of the modern clinical trial, published his treatise on scurvy in 1753. From his excellent review of the literature up to that time, one learns that the Dutchman Beaufour van Ronsse or Rousseus—in 1564, 200 years before Lind—had described the use by Dutch sailors of oranges as a cure for scurvy. Rousseus' comment illuminates the contrasting values placed on theory and observation. He wrote 'Although this is empirical, it seems to have something in common with reason...'

Rousseus evidently feared being dismissed as a 'mere empirick.' His observations, not deduced from any pre-existing theory of the nature of disease in general, carried little weight. Witchcraft, on the other hand, was deduced from a general theory: hence for those who held with the theory, it did carry weight (not to mention the authority of the stake).

The naval surgeon James Lind too could be classed as a 'mere empirick'. Any theory Lind might have had in mind he left unstated. First, by securing the literature, he assembled six treatments which he inferred by induction were the most likely cures. In order to evaluate all six, he assigned a pair of scurvy-ridden sailors to each. In this experiment, most are inclined to say,*

* A Popperian might prefer to say that 1 died of collar (a treatment usually sulphuric acid, vinegar, salt water, and a mixed medicinal paste) and allowed two to survive (the effects of cider) and thought, were perceptible although much less striking than those of citrus fruit). Lind himself did his findings to refute the hypothesis that antiscorbutics owed their effects to their acidity.
Lind demonstrated the efficacy of a treatment that combined oranges and lemons. The significance now attached to this first controlled trial is retrospective—a matter of hindsight as, over the last several decades, we have come to appreciate the power of controlled trials. The British Navy tried but did not firmly adopt Lind's citrus fruit treatment until another 50 years had passed. Moreover, practice was variously modified in the second half of the nineteenth century, and unwavering commitment waited upon the twentieth. In 1879, the social philosopher Herbert Spencer—in making his case against government intervention in general—ventured a sociological explanation for the delay, which he attributed to bureaucratic ineptitude. History refutes this legend, however, and returns us to the evolution of theories and deductive tests. Misapplication of Lind's findings, Carpenter shows, was a matter mainly of the problems of science, inference and lack of knowledge.

Lind was not without influence; he rose to direct the largest and newest British naval hospital of his time. In this 2000-bed hospital with hundreds of cases of scurvy, Lind himself seemed to lose confidence in the results of his first trial as he failed to repeat them. Further, in his search for a practical vehicle for the preservation and storage of the active principle of citrus fruit, he had recommended a boiled-down, insipid extract of oranges or lemons. Vitamin C being unstable, he thereby reduced the amount administered.

A century and a half after Francis Bacon had made the case for scientific empiricism and inductive inference, the intellectual climate was more favorable to empiricism. Yet in the absence of an adequate conceptual framework, Lind's work was shadowed by a succession of contemporary and later theories. Before the final to Lind's controlled experiment was reached, theories and trials of treatments deduced from them turned this way and that. As late as 1874, Jean-Antoine Villemin, who had demonstrated the transmission of tuberculosis in the 1860s well before Koch's discovery of the bacillus, advanced a theory of infection. Around 1900, the redoubtable Almroth Wright supported a metabolic theory that attributed scurvy to excessive alkalinity of the blood. Also around 1900, field research in the Arctic on a 'ptomaine' theory of bacterial contamination of tinned foods was sponsored by Lord Lister, famous for his work on antisepsis and by then President of the Royal Society. Only in the early 1920s was general agreement reached that scurvy was owed to dietary deficiency. Finally, in the 1930s, Albert Szent-Györgyi first isolated a powerful reducing agent from the tissues of both animals and plants and Glenn King recognized its identity with the postulated vitamin C.

In the light of the history of these many theories about scurvy, it is plain that a lack of deductions and tests does not explain the length of the haul to discovery any better than bureaucratic bungling. This history typifies the elimination of alternative hypotheses, a process central to science and one that is nearly always long and arduous. The history shows too that this process of translation from hypothesis to believable fact does not rest on deduction any more than it does on induction.

Popper's Alternative and the Circulation of the Blood

Popper's alternative logic is his hypothetico-deductive method, unique in that it excludes both induction and verification. Science, in Popper's creative demarcation, is what is testable. Well-framed hypotheses are most particularly testable. In the 'context of discovery,' according to Popper, we arrive at an hypothesis by an act of invention or imagination; in the 'context of justification,' we devise tests aimed at falsifying the hypotheses so arrived at. The fallacies of the self-fulfilling prophecy are avoided by allowing an hypothesis to be falsified but not to be verified. If the test does not refute the hypothesis, the hypothesis merely survives. Any affirmative element, like any so-called truth, is provisional.

Yet Popper's falsificationism scarcely conforms with reality in the biomedical sciences (as I have more than once suggested elsewhere). An exemplary case is the discovery of the circulation of the blood. William Harvey may well have arrived at his hypothesis that the blood circulates by a large leap of the imagination (so Walter Pagel has argued), a leap that would thereby exemplify Popper's requirement for hypothesis formation. But a reading of de Motu Cordis shows that Harvey's hypothesis was both preceded and followed by an extensive process of induction. Imagination need not act to the exclusion of inductive reason. By whatever process Harvey reached his hypothesis, he then subjected it to what can be described as hypothetico-deductive tests, being so far again in conformity with Popper. However, he neither refuted his hypothesis nor aimed to do so, but verified it.

The pre-Harveian belief was that blood, constantly formed from the nutrients ingested, was contained in the reservoir of a static vascular system. Having
studied intensely the action of the heart in systole and in diastole, Harvey realized, whether suddenly or slowly we do not know, that the heart pumped a volume of blood incompatible with a static reservoir incapable of expansion. 10

Harvey already knew that the blood flow was not reversible. From vivisection he had learned the structure of the heart and the arteries and veins, the direction of flow of the valves in the arterial and venous systems, and the volume blood that was being pumped. In the static system of the then current theory, the vast amount of blood being pumped out of the heart without any return flow must disrupt the arterial system. Even if the vascular system were capable of expansion, the intake of nutrients would not supply blood sufficient to maintain the large and constant flow he had observed and measured. In all this, whether before or after he arrived at his own theory, Harvey argued by induction.

Harvey then tested his idea in a brilliant series of simple experiments. With tight ligatures, he compressed blood flow in the arteries, and demonstrated the blanching that occurred below the ligature because the arterial supply was cut off. With moderate ligatures, he cut off the flow in the veins only, and demonstrated the swelling and cyanosis that then occurred below the ligature because the venous return was blocked.

Certainly, Harvey’s discovery of the circulation of the blood meets Popper’s requirement that it followed the testing of an hypothesis, and one that Harvey might well have reached by an imaginative leap. Certainly too, the previously existing hypothesis was disposed of. Contra Popper, the new and untested hypothesis did more than merely survive; it was verified and took its place in science as a basic fact.

Logic versus Rationality

The Humean criticism—which at a stretch one can say stands to the founding of modern medical science as Newton’s does to physics—shows that rationality is least as important as refutation. While refutation is salutary in clearing away detritus, creative science is positive and affirmative. To say all knowledge is provisional is to say too much. Thus Popper allows for the existence of ‘singular basic facts’ that constitute our material world. Many such facts, like the circulation of the blood, were themselves hypotheses in the past, however. How then do we pass from hypotheses to fact? This is a fundamental question that Popper considers but that for my part I have not found satisfactorily answered.

If Popper fails to account for the discoveries of William Harvey (and many others), where and why does he go wrong? Popper made a contribution by his emphasis on refutation, although he was not by any means the first to see its value. Thus Thomas Huxley (commenting on Herbert Spencer) said ironically that the great tragedy of science was the slaying of a beautiful hypothesis by an ugly fact. The tradition goes back at least to Bacon in the early seventeenth century, to John Stuart Mill in the nineteenth century, and to others. For Popper, however, refutation becomes a centrepiece for his logic of discovery, and Bacon and Mill are arch-conservativists. He reaches this position as a philosopher dedicated, not to the practice of science, but to a system of logic.

A requirement for a logical system is that it is internally consistent in all its ramifications. Popper was committed to creating such a system when he set out to describe the logic of scientific discovery. He explains that to do so he had to deal with the fallibility of induction. As noted, he dismissed the problem by declaring induction nonexistent in the logic of science. That is, the fabric of knowledge and generalization drawn from many particulars is, if not rendered valueless, without logical justification.

For Popper, then, the sole avenue remaining to the logical scientist is the falsification of an hypothesis deduced a priori. Not even Popper, though, would say that falsification was the sole avenue open to the rational scientist; not all that is rational is logical by the definitional criteria set up by a logical system. One can reason and be pragmatic and commonsensical and yet fail to meet the demands of formal logic. Thus, we have in Popper a coherent logical system but not the whole reality of science.

The gap becomes evident in the consideration of hypothesis formation. With induction banished, Popper needed to address the question of when and how the first hypothesis arises. Popper’s answer to the riddle of which comes first, the hen or the egg? is, according to Harold Hacker, an earlier egg; that is, what comes before an hypothesis is an earlier hypothesis. In trying to circumvent such infinite regress, Popper provided an original but vulnerable solution.

Forced to the wall by the demands of his logical system, Popper resorted to a theory of inborn expectations. Instead of the old idea that generalizations about the regularities that exist in the world are arrived at by induction from many observations, Popper hypothesized an innate predisposition to impose patterns selectively on observations whether or not these truly exist.

This conception of the formulation of a priori
hypotheses seems to one engaged in testing them. Admittedly, Claude Bernard, (a founder of experimental medicine who was by inclination a poet) valued both *a priori* formulations and unformed intuition. Yet in the modern condition, who among us can afford to raise undeveloped intuition or preconceptions to the status of testable hypotheses with all the justification by rigorous research that step entails?

Since Popper sees refutation as most likely to be decisive when testing the least probable hypothesis, he may not violate his own canons when he resorts to inborn propensities towards selecting or creating regularities to explain the emergence of hypotheses. Yet he might better first have refuted simpler explanations. Newton, Bernard and many others found it easier to believe that the regularities they observed existed in nature, and said as much; they testify to arriving at generalizations by induction from empirical observations as well as by seemingly unbridged imaginative leaps.

Popper's view of hypothesis formation as an innate process is damaged both by the daily practice of science and by unexpected events that leave no room for predispersion. As to practice, inductive thought is our everyday tool. Any epidemiologist faced with a disease of totally obscure origin is bound to use induction. The pre-existing descriptive biology and epidemiology of disease are bricks and mortar for the construction of hypotheses. As to the unexpected, without doubt scientific hypotheses sometimes originate in an observation untouched by anticipation or predispersion. On this point, Hinsworth cites an experience of Pierre Curie. After carrying a piece of radium in his waistcoat pocket for some time, Curie found a skin burn immediately under the pocket. From this unforeseen observation of a cellular reaction to radium, radiobiology took its origin.

The fallibility of induction, for logicians a nightmare, seldom confronts biomedical scientists with a critical problem. Most of them, I shall risk asserting, are in their practice materialists or pragmatists or naive realists. However much they must question and test received knowledge and specify their ignorance, they do not live in a provisional world. Working scientists judge by consequences and also by antecedents. By Hinsworth's measure, were the sun not to rise tomorrow, human beings would not be there to see it. Induction played its part in Harvey's revelation of the circulation of the blood, as it did in the discovery of Vitamin C.

**Perspectives and Values**

Popper's radicalism can be turned back on itself. His challenge raises the question of the role of philosophy in science in general and in epidemiology in particular. Philosophers help us to be rational and logical, and to conceptualize our world and activity. Epidemiology is not contained within the compass of Popper's philosophy of science, however. He espouses a traditional philosophical model of science as a search for universal laws in the manner of theoretical physics. This model is a poor fit with a human science like epidemiology. While molecules may be universal, cell types, tissues and individuals are not. In the relevant sciences, the generality of laws must be limited to these particularities, and no universal law can apply to them.

Where then, if not to Popperian logic and philosophy, should epidemiologists look to counter the two charges made in my introduction? how shall we regain depth and breadth to flesh out our technical accomplishments?

First, how might we add depth to the benefits of technique? For this purpose, we need to direct our minds to the range of accessible levels and dimensions of the question under scrutiny. We need, that is, to take account of the hierarchy of levels of organization in which the objects of study are embedded; we must probe or at least keep in view the ecological and the individual and, within the individual, the psychological, the physiological and the molecular levels. To this purpose, we need to adapt the instruments we devise for measurement, the designs we construct to isolate the segment of the system under study, and the methods appropriate to each level.

In studying AIDS, say, epidemiologists need to go beyond their first if signal contribution in recognizing the groups at risk and the mode of transmission. Difficult as the thought and weak as the means may be, epidemiologists must try to fill the multivariate gaps that lie between the disease manifestation in populations on the one hand and viruses, molecules and genes on the other. Equally, they must fill the gaps that lie between the disease manifestation in populations and individual and social behaviour, political structure, and economic forces. The penetration of these many strata, essential to the understanding of disease, also enhances the prospects of control and prevention.

The current revolution in biology is bound to deepen epidemiology. The application of molecular and genetic measures in the population at large seems likely to generate a productivity that could match the era of microbiology initiated by Louis Pasteur and Robert Koch and their cohorts. Just as epidemiology then prospered under the new paradigm of the germ theory, one may anticipate that epidemiology-to-be will prosper under the molecular paradigm.
Second, by what means shall we acquire breadth? If for this purpose philosophical logic profits us little, another resort is to turn to the social sciences. While particularistic in universal terms, they offer generalization at the social level. Although the social sciences too have not escaped obsessions with technique and shallows of substance, they can contribute to epidemiology in at least four ways; they bear on the appraisal of individuals and populations, on the collective thought of the discipline, on the values that permeate epidemiology as a segment of the social institution of science, and on the direct and indirect social forces to which epidemiology is subject.

Epidemiology is a human science, inevitably entangled with society. Any epidemiological variable that describes an individual, whether age or sex or occupation or family background, is social as well as a biological variable. The social sciences provide the concepts that relate individual human beings in a meaningful way to the social order.

Further, scientific thought is not insulated from the social climate of the times. Prevailing intellectual concepts guide the kinds of questions most epidemiologists are likely to ask. The significance for discovery of ‘thought collectives’ and ‘thought styles’ was recognized by Ludwik Fleck, a Polish physician and sociologist. His book, in German and under the title ‘Genese und Entwicklung einer Wissenschaftlichen Idee,’ was contemporaneous with the first appearance in German of Popper’s ‘Logic of Scientific Discovery.’ Fleck examined the evolution of ideas about syphilis from the time of the inverting epidemic around 1500 and, in doing so, anticipated by some 30 years several similar ideas of Thomas Kuhn. For scientists, and certainly for epidemiologists, it is important to be aware of the external forces acting upon us if we are to limit the ill effects and distortions they bring about. To misapply Ecclesiastes (XIII):

For now we see through a glass darkly
But then face to face
Now I know in part
But shall I know as also I am known.

Social context matters to epidemiologists not only for their practice and thought; society presses closely on the profession as an institution. An understanding of the role of epidemiology in science and in the world at large requires a grasp of the social values that permeate it. An innocent will quickly learn that ‘pure science’ is an illusion. Modern science has its origins in the fifteenth and sixteenth centuries. Science then was justified by economic utility and the glorification of God, as Robert Merton epitomized Max Weber’s twin forces of capitalism and the Protestant ethic. Merton went on to say that ‘with the unending flow of achievement... the instrumental was transformed into the terminal, the means into the end.’

In other words, science became an end unto itself, a part of the social structure, with its own set of institutions and values.

Today, the social connections of science are even more evident in the directions it takes. Modern science is to some degree obliged to follow political mandates. In the US, a national cancer institute and a heart institute and a mental health institute and groups of nuclear weaponry engage thousands, not out of individual choice but because political decisions gave these areas priority and funded them in a manner that would attract aspiring scientists.

Regardless of the social pressures on science, there is no doubt also about the relative autonomy and the margin of independence within the social structure that science and scientists maintain. We are not and can never be (as Popper too argues) fully determined. Science as an institution has a value system of its own that guides individual scientists. To this effect, Claude Bernard quotes an unnamed contemporary poet: Art is myself, science is ourselves. This scientific value system has a moral prescriptive content. Some of the content is in accord with society at large—depending on the society—and some, in discord, places science and the larger society at odds. The science of epidemiology is utilitarian in its traditional values. Its pursuit has been more than a matter of satisfying intellectual curiosity or aesthetic needs. Our discipline is inextricably linked with public health and medicine, which are themselves shot through with humanist and utilitarian values. If I can be forgiven a cliché for the truth it embodies, one may say that as a physician scientist is to a patient, so is an epidemiologist to society. Unless we pervasively subvert our history, we are forbidden to be antihuman, or antisocial, or even asocial.

By now it must be evident to all that, besides its societal antecedents, science has societal consequences both harmful and beneficial. Szent-Györgyi, himself forced to flee the Nazis in Hungary, pointed out the irony of his discovery of vitamin C. By the very fact that it prevented scurvy, vitamin C allowed Hitler’s submarines to endure at sea for months on end while wreaking devastation.

In this decade, as the ideology of privatization and private enterprise penetrates the academic world, public health as a prime value is more than ever needed to protect epidemiological expertise from misuse. The results of epidemiology are often not neutral; the interpretation of our work is subject to political and social
pressures. We know better how to deal with such pressures if we are candid with ourselves and others about our professional and personal values. An unhunking neutrality is a stance that can have even greater political implications than open commitment. A critical self-awareness is one defence against the inflation of personal and social bias into our science.

In summary, a prescription for epidemiology-to-be is, first, to go beyond the single plane of the events that befall individuals as the unit of study in populations. We need to probe in depth the hierarchy of levels of organization, from the ecological to the molecular, that characterize the populations under study. Second, we need to broaden our discipline by taking hold of research in its social context. Third, we need to recognize that epidemiologists are themselves social beings. In an epidemiology of the future, modern achievements in technique might then more easily sustain it as a foundation science of the public health.

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