INNOVATION OUT OF UNITY

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On the occasion of the Sixtieth Anniversary Conference of the Milbank Memorial Fund,[1] we have three subjects before us—population change, the effectiveness of mental health services, and behavioral science in medical education. As I see them, they all have an important relationship with one subject: social behavior. They are related to it in somewhat different ways so that I think it would not be quite correct to identify social behavior as a common thread tying them together. Rather I picture it as a central mass or crystal with facets to which are attached the three main areas of our concern. The interaction is taking place at these interfaces but they are different for each. I would like to examine this model a little further in order to see whether it can contribute to our understanding of human health and disease in general, including the subjects before us today.

Bronowski, one of the leading scientific philosophers of our time, wrote: "Innovation occurs when the mind perceives in disorder a great new unity." At first glance this might carry with it something of the idea of the great scientist springing naked out of his bath with the cry of "Eureka," and the solution of one of the major problems of the universe on the tip of his tongue. Bronowski, of course, meant nothing of the sort. He was well aware of the long and painful process of conception, development, and parturition of innovation in science.

The essence of what he wrote was that perception of unity in disorder is an essential basis for innovation. However, innovation does not necessarily follow when a new unity is perceived. Harbin has pointed out that it requires a complete theoretical structure to stand up against a factual complex. A unity perceived against an incomplete theoretical structure may seem to be proved and then disproved from time to time as science develops and theory is altered to account for new facts. This may happen even if the unity is actually an essential part of the complete theory. The difficulty arises in part from misuse of the verb *to prove*. In its origin, and still in many languages (i.e., the French *éprouver*), it has the meaning *to test,* and testing is carried out either against existing knowledge or, better, against predictions based on the hypothesis in question. When either of these is deficient, the test may fail, even though the unity is actually correct.

This is the disorder with which we are faced in medicine today, within which we must seek to perceive a new unity. Let me give you an example of what I mean.

Thirty years ago—and even today in many places—the concept of the specific etiology of disease was unquestioned. It originated from the discoveries during the era of Koch and Pasteur—although, as far as I know, Pasteur himself never proposed it. According to this hypothesis, the cause of an infectious disease is the agent; all else is of secondary importance. On its basis, immense advances were made which, in the light of available knowledge, seemed to confirm it. The same hypothesis was, therefore, extended to a wide variety of other diseases, and medical scientists engaged in a search for *the* cause of cancer, heart disease, nutritional disorders, mental diseases, metabolic disorders, etc. They made many important and valuable discoveries, but if these are examined closely, they are concerned more with the *mechanisms* of disease processes rather than their *causation*. I shall return to this point.

Today we know that the concept of the specific etiology even of infectious disease is incorrect except in the semantic sense. By definition, one cannot have tuberculosis without the tubercle bacillus nor, for that matter, can one have an automobile accident without an automobile, though the automobile itself is seldom the cause of

the accident. This arises from the present method of classifying infectious disease on the basis of the concept of specific etiology. With increasing knowledge of the variety and behavior of infectious agents and of the response of man under a variety of circumstances, it is apparent that it is a gross oversimplication—not that it was completely wrong, but the factual complex on which it was based was seriously incomplete. It ignored the influence of other factors, many of which had indeed been recognized for 2,000 years, since the time of Hippocrates. These include many environmental and behavioral elements which, however, were much more vague and, therefore, less "scientific" than the discoveries of the natural scientists.

The scientific mind has a natural love of order. It tends to ignore fields in which order is not readily apparent. Emphasis has, therefore, been on subjects that can be studied in the laboratory, and the trend has been to stress the study of smaller and smaller fragments of the whole organism. Even when the whole organism is being studied, it is carefully isolated from the influence of the natural environment. Although this approach has greatly increased our understanding of fundamental mechanisms, it tells us little about the external factors which may trigger them. To cite an extreme example, one can even question the validity of many of the findings of bacteriology. Here one of the first moves is to establish a pure culture. This represents a very highly selected sample derived from perhaps one or a very few out of the original population of billions of organisms of all kinds. It is a little like making pronouncements about the people of New York from a study of, say, two persons; if we did that we would not even be sure of discovering that there were two sexes. An epidemiologist would not keep his job long if he did that. Yet data are coming to light which show that organisms grown in pure culture behave differently from mixed cultures—which is what is always found in nature. This is so even when the mixture is just of different strains of the same organism. For instance, virulence seems to be profoundly influenced by the degree of admixture of virulent and nonvirulent strains, and this is not merely a matter of the proportion of each, but of differences in certain biochemical processes in virulent and avirulent strains which

interact with each other. The same kind of interaction occurs in populations of whole animals, and especially of man with the immensely greater complexity of his mental processes and the social and environmental conditions under which he lives. Only here the interaction is primarily psychological and social, not biochemical. Of course, there are also important physical, chemical, and biological interactions; man infects his neighbor, covers his cities with smog, and pollutes his water supplies. These must be and are being studied.

But to return to the present obsession of biological scientists with the smallest particles, there is little doubt that elucidation of the DNA code and more knowledge of the internal mechanisms of cells will extend our ability to cure disease and to delay the inevitable arrival of death. I am not belittling this. But these discoveries can at best only contribute one part of the knowledge we need to meet the major challenges involving the populations of today and tomorrow. Many of these we can see, and perhaps the most chastening thought is that few of them are really new. They have been recognized for decades or even for centuries. A patch here, a little palliation there, a blind eye there, have so far enabled us to avoid facing the real challenge. But the sands are running out, and the speed and magnitude of technological advances are at last forcing us to face the issues squarely. The ostrich with its head in the sand was not in a very secure defensive position even in the bow and arrow days, though it might be able to detect footsteps of the approaching hunter. But dangers are no longer creeping up; the speed of their approach is alarming.

Today we are facing three eruptions: the population, the rising tide of expectations, and, behind them, the bomb. We may try to duck responsibility for the last, but I believe that if it does explode it will be because we have failed to control the first two. I am not suggesting that the health professions have the primary responsibility even in these, but it is certainly a major role and one which the health professions as a whole have only faced piecemeal. It is in a unity of approach between the health professions and the social and political sciences that we must seek innovation.

These problems cannot be tackled in the laboratory, though the laboratory can help. They must be studied in the context of real

life with its multitude of variables, some obviously relevant, some apparently remote yet highly significant, some extraneous. Our studies must encompass this remoteness. Twenty years ago the implications of such a suggestion would have seemed so impossible that it would have been given only the most cursory examination. Today technological advances in communications science and data processing justify their serious consideration. For the first time we are able to handle large numbers of variables and masses of data and to ask questions of the data in a way never before possible. But we must learn how to collect the right kind of data, because, to paraphrase a well-known entertainer, "a computer is like a sewer—what you get out of it depends on what you put into it."

The use of these technological advances has been most rapid when the information to be analyzed consisted of figures with a known degree of precision. In business and the fiscal aspects of government they soon became essential, and they were soon introduced into the more precise physical and chemical sciences. In the biological sciences much progress has been made. However, in the social and behavioral sciences progress has been slower.

A major difficulty has been the reduction of observations in these sciences into a form which is both meaningful and capable of being handled by these methods. For instance, we might attempt to grade sanity from 1 to 10, from sane to insane, but that would be scientifically quite unsound. In order to justify placing any group of observations or diseases into a category, we must show that all of them have some essential feature or features in common. All that the various departures from sanity have in common is behavioral abnormality which may have its origins in several of a wide variety of influences—bacterial (general paralysis of the insane), toxicological, organic, psychological, social and environmental, etc. To class all these together is rather like putting together peas, pebbles, and rabbit pellets just because they are approximately round. Obviously the quality of roundness would not be sufficient to justify categorizing them together. Nor is a behavioral element alone sufficient to justify placing all behavioral abnormalities in one category. It would be helpful if we could define rather more clearly the nature of the qualities which would result in more useful classifications.

I have already mentioned that infectious diseases are classified together on the basis of a concept which has proved inadequate. The same is true of many other classes of disease. I think that the inadequacy arises from the fact that a single quality is never enough for useful classification when that quality is not of primary, overwhelming importance. In infectious diseases, for instance, our interest is now not limited to answering the question *how* did the patient become ill—because of an infection—but also inquires *why* he became ill. This immediately introduces the circumstances surrounding his infection, his response to it and why that differed from others who were infected and did not become ill. The "specific" infection thus becomes just one of the multiple causes of his disease. We can take this a step further and study other members of the population affected by these multiple causes without the specific infection in order to see what diseases they suffer from. We find that certain groups of diseases can be associated with certain types of behavior, with certain socio-economic conditions, and with social and environmental influences. This is, of course, not a new thought. We have long talked of the diseases of poverty, and so on. But what has not been attempted, as far as I know, is the use of factors of this kind as a basis for the classification of disease.

Let me illustrate what I mean. Endemic infantile paralysis, as it used to be called, is an infection which rarely results in disease and is caused by the polio virus. Epidemic paralytic poliomyelitis, on the other hand, is a social disease, resulting from delay in primary virus infection until an age when paralysis follows infection much more frequently than when it occurs in infancy. This delay is caused by the social organization and application of sanitary measures designed to prevent the spread of intestinal infections. As countries have improved their hygiene and sanitation in the course of socio-economic development, poliomyelitis has passed into the epidemic phase in a predictable way. There have, of course, been a few exceptions to these predictions as would be expected in any such complex biological-social system. Some of these we can explain within the present theory; others can be explained on the basis of assumptions which we cannot test with the tools presently avail-

able. If these should be disproved, we may have to revise our theory to take into account the new data. In the meantime, the concept has proved to be of considerable value.

We may, therefore, turn this around and say that one of the diseases which will result from socio-economic development, if no steps are taken, is epidemic paralytic poliomyelitis and we can predict on the basis of indices of socio-economic development roughly at what stage it is likely to appear. Using the same indices, we can predict that certain other diseases will *decrease* in importance, the intestinal disorders of infancy for instance. You will note that I call these intestinal disorders, not infections. Actually, we can identify pathogenic organisms in only about one-quarter of the cases. We do not really understand the relative roles of infection, nutrition, electrolyte balance, and other physiological and psychological disturbances in the other three-quarters. But essentially these are all social diseases which can be corrected by social measures without recourse to any specific antibacterial measures.

The same social changes have another consequence. The proportion of the population in the childhood and school-age groups will increase, straining further the already strained resources for the provision of adequate nutrition during the critical developmental years, and adequate education on which rests the achievement of the rising expectations for the next generation. One consequence of malnutrition, which is only now being recognized, may adversely affect this, that is that malnutrition at a certain stage in development may result in a degree of permanent impairment of intellecual capacity. Just how common or severe this may be, we do not yet know.

There are other results of the same social changes, but I hope that these examples will suffice to illustrate my hypothesis, which is that *by examining social factors, especially changing factors, quantifying them as best we can, and relating them to the accompanying changes, both favorable and unfavorable, in human well-being, we may be able to classify the latter in terms of the social factors which brought them about, rather than in terms of a so-called specific agent, infectious or otherwise.* If by this approach we

can identify more clearly the factors involved, we should be able to revise the priorities for allocation of the resources available so that they will have their maximum effect.

Tuberculosis is another of the poverty diseases, and it also illustrates further some of the potential advantages of this approach. Under conditions of poverty, tuberculosis is most serious as a disease of infancy and childhood, associated with malnutrition, overcrowding, and all the other social deficiencies with which you are familiar. This is the age group among which active spread is occurring. Young parents are an important source of the infection for the next generation, but middle-aged adults are relatively unimportant in the epidemiology of the disease in such situations, partly because a large proportion of those unable to overcome the disease through their natural defences will have died.

In highly developed countries the picture is totally different. Disease in the young has been reduced to very low levels, the results of primary infection—meningitis and miliary tuberculosis—are rarely seen. The most important reservoir of the disease arises from the breakdown of so-called healed lesions, especially in the middle-aged male. Epidemiologically the only thing that these two diseases have in common is a specific organism, which as I have already suggested is not a sufficient reason for placing them in the same category.

Let us, therefore, examine tuberculosis in highly developed countries in order to see whether we can identify factors which are responsible for these differences.

The first finding is that whereas in underdeveloped areas roughly 30 per cent of those infected with the organism develop disease, in the United States only about 3 per cent do so. How do these 3 per cent differ from the 97 per cent who are infected but healthy? A series of recent studies has revealed a number of factors which have been summarized by Cassel as follows: They (the tuberculosis cases) frequently come from a broken family; they live in an area in which they are a distinct minority not accepted by the dominant majority; they have had an excessive number of residential and occupational changes; they are more likely to be single, divorced, or widowed than is the rest of the population; and they have been

subjected to mounting life stress without any period of remittance. Another difference from the juvenile form of the disease is that neither nutrition nor crowding seems to have an important effect on the liability to develop the late adult form. The difference between these factors and those responsible for the juvenile form is so great that, in my opinion, the childhood and late adult forms should be regarded as different diseases.

However, important though these observations are, even more significant findings came out of these studies. It was found that the social experiences of those who develop schizophrenia or who commit suicide had been remarkably similar to those of the tuberculosis cases. In contrast, there was no such similarity in those persons who develop manic-depressive psychosis.

According to my thesis—and Cassel also suggested this—adult tuberculosis, schizophrenia, and suicide should be classified together as a category of disease related to identified social factors. Manic-depressive psychosis should be placed in another category, rather than being classified with schizophrenia as it is at present.

The usefulness of this altered classification is immediately apparent. We already know that the problem of late adult tuberculosis cannot be approached in the same way as the juvenile disease. We need to include in our approach to the former an attack on the social factors identified, and if we do so, we will at the same time be contributing to the solution of two other important diseases looked on as utterly different according to the present classification. Furthermore, we would be aware of the need to incorporate in our program the additional expertise needed for the assessment of mental abnormalities.

I am well aware that these concepts are totally at variance with those used in conventional medical education, and will meet with criticism and skepticism, even though they are not new. It is really a matter of rearrangement of ideas, and sometimes that may be all that is necessary to perceive a new unity.

In the hope of clarifying this, I want to return briefly to a point which I have already mentioned. There seems to me to be some confusion among biological scientists between *causation* and *mechanisms* which lead to the *effect* we call disease. There are, of course,

philosophical difficulties as to what is a cause, and there are all the problems of direct and indirect causation. But this is not the place for a discussion of them. At the risk of oversimplification, I will define causation as the sum of those factors, extrinsic or intrinsic, which combine to *initiate* the intrinsic mechanisms, which in turn lead to certain effects which may be defined as disease. Thus the cause of diabetes, for instance, is not deficiency of insulin; that is part of the mechanism. The cause lies further back, in genetically determined forms perhaps generations back; in acquired forms, behavioral and environmental influences are concerned. The cause of coronary thrombosis is not the formation of a thrombus which blocks the artery, nor is it the atheromatous process or changed coagulability of the blood. These are mechanisms. The causes lie further back in behavioral and environmental factors, including diet, exercise, occupation, stress, etc.

My thesis, therefore, is in essence that we should attempt to reclassify diseases according to this concept of *causation,* and not according to *mechanisms* as is the present practice for many groups of diseases, nor according to descriptive criteria related to the effect of these mechanisms, that is, the clinical disease as is the practice in others.

Frankly, I do not know whether this can be done successfully. I have presented these speculations because it seemed to me that they might serve to bring out the interrelationships between the three main subjects of this conference.

The population problem involves change in numbers, distribution, and age structure. The first cannot be controlled by the "pill" alone. It will involve social and cultural changes to ensure the acceptance of the need for limitation of its rate of growth. Population growth is not just an over-all national increase. Indeed, it is far more acute in certain localities owing to changes in distribution, particularly through rural-urban migration. These movements are predominantly determined by social and economic factors, and they bring with them changes in disease patterns which could, I believe, be predictable, with a better understanding of the interrelationship of social and cultural change and disease patterns. As already noted, the change in age structure of a population brings changes in disease

patterns which again should be predictable and could be related more precisely to identifiable social factors.

Mental health services also have to be related to the social and cultural structure of the population, and the etiological factors in that structure need to be identified. Unless they can be, efforts to deal with mental health problems in the context of that structure are likely to be difficult, if not futile. It is, for instance, unlikely that a mental health problem can be resolved in the continued presence of the factors which contributed to its genesis. As long as the shoe continues to pinch, the corn will recur. Furthermore, it seems to me that one cannot evaluate the open or domiciliary treatment of mental health problems without taking into account its effect on the population, especially on the family as well as on the patients. Here is the second subject of our conference.

The third subject involves a thread which has been running all through this presentation. The health professions, especially the medical profession, have to concern themselves with behavioral and social factors if they are to deal successfully with the complex problems of the well-being of man in society. This is not a new thought. In 1946 in his book, *The University at the Crossroads,* Sigerist wrote: "That medicine is a social science sounds like a truism, yet it cannot be repeated often enough because in medical education we still act as if medicine were a natural science and nothing else. There can be no doubt that the target of medicine is to keep individuals adjusted to their environment as useful members of society or readjust them when they have dropped out as a result of illness. It is a social goal. Every medical action moreover presupposes a relationship between at least two individuals, the patient and the physician, or between two groups, society, on the one hand, and the medical corps, in the broadest sense of the word, on the other."

I have endeavored to add another reason by showing that social and behavioral factors are equally or more important in the causation of disease and the determination of disease patterns in populations as are biological factors which are the main preoccupation of medicine today.

Nearly five years of experience in a medical school have taught me that we are facing an uphill struggle in gaining acceptance of

these ideas. Yet already I am confident that it can be done. We have actually succeeded better than I had hoped when we began, though we have far to go. One of our experiences may help in the coming discussions. I have not had too much difficulty in gaining acceptance of our ideas among the top echelon of the faculty. They are all highly intelligent men who can understand and accept new concepts if they are well presented and if they can withstand scientific challenge. Few of them are influenced by emotional judgments in their professional work. Equally I have experienced no difficulty with the medical students if one can present the concepts early in their training—in the first year, before they become obsessed with the "Body in the Bed," as Edgar Allan Poe might have put it. The greatest problems arise with residents and junior faculty who have not been introduced to these concepts in their medical education. They have an immense adverse influence in this respect on students during their clinical years. They have much more intimate contact with them than have the senior faculty, so that it is difficult to counteract it. One problem is that many of the residents and junior faculty come from other schools which do not introduce these concepts to their students. It would be much easier if all schools recognized their importance and incorporated them into their curricula at an early stage. I hope that this will be possible in Latin America. If you succeed, you will soon be far ahead of this country in what I personally believe holds the most promising opportunities for medicine in the future, exceeding those presented by advances in molecular biology.

I foresee that a marriage between the medical and behavioral sciences will result in the conception of a great new unity, and out of this union will be born the innovation we so urgently need.

REFERENCE

[1] This address was presented at the Sixtieth Anniversary Conference of the Milbank Memorial Fund, held in New York City, April 5–7, 1965. It will be published in a booklet under the same title.