

THE CONTROL OF HUMAN FERTILITY IN JAMAICA

J. MAYONE STYCOS AND KURT W. BACK

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This is an important investigation of a subject vital to the Caribbean at large—fertility control and its correlates, and it complements the studies conducted by the present authors and Reuben Hill in Puerto Rico on the same topic. The sample from which the material was collected constituted “a particular subgroup of the Jamaican population—currently mated lower-class women of childbearing age.” There are two parts to this volume. A formal analysis of mating and fertility, together with an inquiry into attitudes toward, and knowledge of, contraceptive practice, constitutes the first part. The second is devoted to an experiment designed to test the effects of educational programs on attitudes toward fertility planning. At both levels the contributions are significant; of special interest are the plans for the experiment and the analysis of the results.

Clearly the work has been well planned and the field operations, conducted under D. O. Mills, have been competently carried out. Still, at one point a question about methodology may be raised. This centers on the instructions to the interviewers. They were told to incorporate in their preliminary remarks to respondents a statement that “a group of American doctors” was anxious to find out “how we Jamaicans live, and how we would like to live and all about our family life.” One may question whether such an introduction is calculated to ensure completely spontaneous and reliable answers, especially as a hypothetical type of question is relied on so heavily in assessing attitudes. A somewhat different question—one of presentation—may also be posed. In a preliminary report, devoted essentially to discussions of the methodology adopted, it was described as a survey under “unusual” conditions (K. W. Back and

J. M. Stycos, *The Survey under Unusual Conditions*, 1959), but neither in that publication nor in the work under review is there any hint as to what made the conditions under which the survey was conducted "unusual," or what, if any, special procedures had to be resorted to as a consequence of the unusual conditions encountered by the authors.

Attitudes toward family size are examined from several angles. The authors find no support for the view sometimes expressed that Jamaican women are strongly conditioned toward bearing numerous children. Some inconsistencies in the replies of respondents to family size preference are evident, for despite the general preference for small families many women show "a tendency toward tolerance of the large family." Women in rural areas declare a larger family size preference (3.8 children) than do their urban counterparts (3.1). Also explored are opinions on what constitute small and large families in Jamaica. The size preferred by these women does not differ much from that shown by females in the United States, but it seems conservative by comparison with family size preferences of Puerto Rican females.

One of the significant findings of the survey is the small proportion of the sample with a clear knowledge of birth control. Moreover, there was a marked reluctance to reveal what was known about the subject; 40 per cent of the women in rural areas could not or would not name any method when asked, while of those with some knowledge many knew only primitive methods. Condoms, foam tablets, and abortion were the methods most frequently reported. Despite the small proportion of females with such firm knowledge, there is no widespread aversion to the practice of contraception.

In general, after a woman has had three births her attitude toward fertility control tends to be favorable, irrespective of her age. A somewhat unexpected finding is the liberal attitude of Roman Catholic women, which is in direct contrast with the conservative attitude shown by women with no religious affiliation. This may be associated with the comparatively high degree of urbanization of the Catholics.

Only one-tenth of the women in the sample have ever practiced birth control. Under the most favorable conditions revealed by the

data no more than 23 per cent have employed it, while under the least favorable conditions no more than 5 per cent have resorted to it. Since the small numbers who ever used birth control precluded any analysis of the effects of contraceptive practice on fertility, analysis of attitudes toward contraception cannot always be conclusive, even though the limited material bearing on this is skillfully utilized.

The treatment of what is termed marital status by the authors is interesting, but does not at all points prove convincing, especially when they seek to advance motives for entry into the different types of unions. It is questionable whether, in developing typologies for analyzing mating, Simey, Henriques, and Clarke were, as Stycos and Back claim, merely attempting "to distinguish between grades of stability." There is certainly much more than a consideration of stability involved in establishing a classification of this nature. Nor does it seem acceptable that "common law unions [in Europe] represent an insignificant proportion of the wedded couples." The three-fold typology adopted—married, common law, and visiting—is that used in many recent studies in the West Indies. The discussion of the incidence of the several types is interesting, although the fact that concepts such as union, dissolution, and stability are not satisfactorily defined detracts from the force of some of the arguments advanced. The authors seem to be on insecure ground when they deal with motivations for entry into the several types of unions. Many of these arguments are put forward with an assurance hardly warranted in view of the fact that the material on which they rest does not form part of the questionnaire proper, but derives from the follow-up inquiry carried out three years later, which unavoidably covered a small, select number of women. It is in any case doubtful whether material obtained exclusively from lower-class females can form the basis for a definitive assessment of motivations behind entry into all types of unions in the population as a whole. It should also be noted that the discussion on mating does not take into account recent analyses of the subject with reference to Trinidad and British Guiana, which at several points do not support the authors' interpretations.

An interesting hypothesis—that there is a tendency for children to be desired in each union—is tested by means of replies to questions seeking to find out whether the respondents want more children. The data tend to confirm this, but it is not easily reconciled with findings appearing elsewhere in the study. For instance, at another point it is urged that births in nonlegal unions tend to encourage parents to marry, while from a further line of argument it appears that the breakup of a union may follow a pregnancy. Taken together, these attempts to derive explanations and relationships seem to be pushed too far; this is virtually conceded when the authors conclude, “perhaps not only does a stable union encourage fertility, but also fertility encourages a stable union.”

Unexpectedly, knowledge of fertility control seems greatest where it is least needed: in urban areas among women with histories of unstable unions. A line of analysis which might have helped to clarify the treatment at this point is missing—the relationship between educational attainment and knowledge of birth control, which proved significant in the Puerto Rican study.

The chapter on fertility considers differentials from many aspects, and contains a notable contribution to the understanding of urban-rural differentials, by showing that the highest levels obtain among females who moved from rural to urban areas. Information on the frequency of sexual intercourse confirms that differential exposure to the risk of childbearing accounts for a well-known fertility differential by type of family union observed in the West Indies. The very small numbers of respondents who practiced fertility control made it impossible to examine the effects of contraception on levels of fertility.

The second part of this work deals with “a number of experimental techniques geared to produce positive changes of attitudes and behavior towards fertility control.” The relative efficiency of pamphlets, group meetings, and individual case visits as educational media was studied. Chosen as subjects were women most likely to have additional children, that is, those currently mated and not using any contraceptives and having had at least one pregnancy in the previous five years. Using adequate control groups, the authors

were able to assess the effects of the educational program at three stages. The first was immediately after the interview, the second nine months later, and the third three years later.

Careful plans were made for applying the educational methods. A film comparing the developing situation of a large family and one in which family planning was practiced was used in group meetings, while pamphlets constituted educational media in another context. The individual case visit is considered "the most intensive and personal method." Several advantages and disadvantages of the three techniques are discussed. Pamphlets provide the cheapest and seemingly the easiest approach, but cannot be adapted to individual problems while they depend largely on the education of the respondents for their effectiveness. In the case of group meetings, the greatest difficulty seems to be to ensure regular attendance. The case-worker approach tends to be expensive, but since it can provide appeals most suited to individual cases, it is very effective.

Stycos and Back seek to evaluate the results of their experiment in terms of several types of variables. There were "troubling questions concerning reliability of respondents' statements," but evidently these do not materially impair the conclusions reached. "A surprising degree of success in Kingston and a moderate degree in country areas" were reported, in so far as the initial effects were concerned. But few of these women electing to practice birth control continue to do so for any length of time. Group meetings appeared to be the most effective method.

The authors carry out a close analysis of the results of the experiment and attempts are made to deal with possible discrepancies in the findings. Nevertheless some obscurities remain. A few results of the experiments seem unexpected in view of the position presented earlier in the study. For instance, their analysis of attitudes toward birth control led them to assert that "a considerable amount of education would be necessary to have any decided impact on national birth rates" (p. 59). But discussions of the outcome give a much more optimistic picture of the possible effects of an educational program. Further, a consideration of the relationship between marital status and knowledge of birth control led to the conclusion

that the latter was greatest where it is least needed, and least where it is most needed. By contrast, one conclusion from the experiment is that those who want information about fertility control most are those who "are in the condition of highest exposure to fertility, but are also most likely to use birth control" (p. 275). Suggestive though the results are, they cannot give a firm indication of the actual effects, if any, the experiment had on fertility. For it is important to recall that the very small numbers practicing fertility control precluded any assessment of the possible influence which might be expected from the extension of their use, while assessments of the experimental results did not include their impact on childbearing.

In their concluding chapter the authors consider the possibility of developing a wide program for spreading fertility control. Despite the very limited knowledge of the subject in Jamaica, from the evidence of the study, they remain optimistic about the prospects for its spread. An examination of three notions which, in their opinion, oppose the spread of birth control programs is attempted. These notions—that Jamaican females favor large families, that they oppose birth control, and that educational programs will be resisted—are considered groundless in the light of the survey results. However, this conclusion need not be as conclusive as the authors seem to suggest. For the sample does not include women of the middle and upper classes, whose particular family size preferences may not only be more easily realized, because of their presumably greater access to contraceptives, but also have an important bearing on ideal family size in the country at large.

The specific suggestions for furthering a program of contraception in Jamaica do not all seem to follow from the study, but are still acceptable. The small family ideal, it is urged, has to be made normative rather than merely permissible. The small-family goal must be frequently reinforced "by tying it to peer and reference groups." There must be ready access to contraceptive services and material. With justification, the authors do not support the traditional family planning clinics, with their strong bias toward the diaphragm. On the other hand, their advocacy of sterilization (and possibly abortion) will hardly command support, while the very

recent development of the intra-uterine devices necessitates many revisions of their plans.

Doubtless much can be said for limiting the survey to lower-class females. But the consequences of this for analytical purposes, and especially for affording insight into the future course of events, cannot be ignored. Indeed, the fact that the views held by the middle and upper classes on such issues as ideal family size, the acceptability of birth control, and the relative merits and demerits of the three types of union are not provided for weakens the analysis at several points. For it is the attitudes of the lower classes, to the exclusion of all others, which must be taken as representative, indeed normative, of the society at large. For other reasons already indicated the interpretations will not at all points carry conviction, couched as they often are in an assurance which belies the remark with which the study concludes, "we have raised more questions than we have answered." Nevertheless, this remains a very important contribution to the literature on fertility control, and the steps it advocates for the control of fertility will repay careful study.

GEORGE W. ROBERTS

A FOLLOW-UP STUDY OF NON-SCHIZOPHRENIC PSYCHIATRIC PATIENTS

A. B. LEVY, C. M. SMITH, AND D. G. MCKERRACHER

Saskatoon, University of Saskatchewan, 1964.

The past few years have seen a considerable growth of knowledge about the career of the mental patient. But, as in most burgeoning areas of research, each study raises at least as many questions as it answers. The follow-up investigation reviewed here is a welcome addition to the field. In particular, it has two unique qualities: the data collection takes into account the period of hospitalization (most other studies begin at or near the point of return to the community), and the authors study a group of nonpsychotic patients (rather than focusing mostly on schizophrenics).

It is important to point out that the document was not intended to be distributed widely. It is a "working report," full of unnecessary tables, repetition, and lacking the refinements common to well-done monographs. For the most part, its usefulness is limited to persons actively engaged in research in the field, and it is not recommended as an addition to the bookshelves of the practicing psychiatrist or the behavioral scientist with a casual interest in mental health.

There also are serious problems of method. Although the authors carefully note most of the limitations, the problems of method nevertheless raise serious questions about the usefulness of the findings. One troublesome problem is the loss of patients from the study group; by the end of the study, data are unavailable on one-third of the patients, leaving only a little over 100 cases for most of the analysis. The findings are not only restricted to "co-operative" patients but the size of the sample prohibits the simultaneous "partialling" of variables. Given the vast amount of work involved in

interview-schedule design and the development of sample selection procedures, one must question the advisability of undertaking a quantitative investigation when the number of variables involved far exceeds the number of cases. Further, by restricting the sample in the way they did, the authors are stuck with the problem of reliability of diagnosis. Finally, although their list of references includes much of the other work in the field, the report itself fails to make use of other findings and their analysis proceeds from one issue to the next without adequate consideration of the implications of their material in the light of other research on the careers of mental patients.

Yet there are many provocative findings in the study, and most of them are satisfactorily consistent with other research on mental patients. Rather than attempting to provide a complete summary of their work, it is probably more useful to select some of the most provocative findings:

1. Assumptions about the relationship between the referral process and outcome require revision. Patients who initiate hospitalization for themselves are most likely to shift in the direction of being less favorable toward treatment; those who were referred to the hospital by physicians change in a favorable direction. Apparently the treatment experience polarizes attitudes in ways contrary to common assumptions about patients' attitudes to hospitalization.

2. The type of treatment a patient receives is not associated with his diagnosis or length of stay. The use of particular medications in the care of the neurotic patient is related to their "fashionableness" rather than to the purely clinical indications of the illness.

3. The most important influence on treatment and outcome is the physician responsible for the patient. The patient's physician not only is the key determinator of the treatment program for the patient but the physician's characterization of him is the major factor in the assessment of his potential for release.

4. Differences among patients on admission tend to be reduced as a result of common experience and interaction during the hospital stay, but these differences appear again after the patients return to their own homes. Institutionalization has only a fleeting impact on the over-all life experience of the patient.

5. Psychotherapy, if it has any impact, is associated with poor posthospital adjustment. Patients who received psychotherapy had poorer outcomes than those who did not have psychotherapy. The negative finding on psychotherapy cannot be explained in terms of selection criteria, and apparently this treatment modality has limited efficacy for the hospitalized neurotic.

6. Neither the patient's conception of stigma nor his attitude to treatment seems to facilitate his response to care.

It should be emphasized that the small and biased study group limits one's confidence in the findings, but certainly the relationships suggested by the investigation should provoke more definitive studies. The authors are to be commended for making the data available rather than leaving them in their files.

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INNOVATION OUT OF UNITY

ANTHONY M.-M. PAYNE

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 oversimplification—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 intellectual 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.