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CHANGING CONCEPTS OF HEALTH AND DISEASE, WITH PARTICULAR REFERENCE TO "PSYCHOSOMATIC MEDICINE"*

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I do not think anything I shall say is new; much of it has been better said before, and some of it is very old—so old indeed that there always is a chance it may be mistaken for new. But I am encouraged to hope that, even so, it may fulfil the terms of the Bradshaw Trust, which states simply that a lecture shall be given in memory of Dr. William Wood Bradshaw on a subject connected with medicine or surgery.

As a starting-point I propose to try to put into words some ideas about the nature of disease which seemed to be taken for granted in the medicine I was taught nearly twenty-five years ago. It was assumed, so far as I could see, that there were things or entities called diseases which could be studied and treated as such. Some of these arose in and affected the mind, and were the province of psychiatrists. Others arose in and affected the body, and were the province of physicians, surgeons, and various specialists.

Diseases with physical manifestations could be divided into organic and functional. Organic diseases were regarded as real, objective, and the proper object of study by scientific medicine. They had physical causes, which either had been or would be elucidated as a result of increasingly thorough study by physical, chemical, and bacteriological methods. In the investigation of a disease the object was to find "the cause" and to deal with it. There was little or no interest in functional disorder. Functional symptoms seemed usually to be regarded either as invented or imaginary or as rather mysterious and discreditable states which in some way affected inferior personalities.

I do not think, as a student, I heard the word "psychosomatic," which is not surprising, for this actual word was apparently first used in 1926. Within a few years, however, it was suggested that certain physical diseases for which no satisfactory cause could be found, and which were becoming increasingly noticeable as important sources of disability, were psychosomatic in nature. This appeared to mean that the structural changes found in the body were caused by a disordered mind. This possibility was eagerly seized on by a few, but denied or ignored by the many who believed that real organic disease had little or nothing to do with the mind and

would ultimately yield its secrets to the methods of physics, chemistry, and bacteriology.

Though stated baldly and perhaps oversimplified, I believe that is a fair summary of some of the main ideas about disease which were more or less taken for granted in the medicine I was taught. If any of my teachers should say they took nothing of the kind for granted, I can only say that this was what an average student gathered, not so much perhaps from what they said as from what they did not say. One saw, of course, that many of one's teachers did treat their patients as persons rather than as mere vehicles of disease, but I believe this was more an expression of humanity than an indication that they accepted such an approach as necessary to the understanding of disease.

Ideas Still Taken for Granted

I believe that some at least of the ideas I have outlined are still taken for granted by many persons engaged in academic medicine, by which for the purpose of this lecture I mean medicine as taught in undergraduate teaching schools. I will not waste time in defending this latter statement at length, but would refer to two recent articles by eminent teachers in undergraduate schools.

The one lists what he calls the five common causes of disease: inborn and inherited abnormalities; excess of a chemical agent in the environment; deficiency of a chemical substance; infection or infestation by viruses, bacteria, fungi, or animal parasites; and physical trauma. He thinks it possible that the important diseases of unknown aetiology, such as the rheumatic diseases, peptic ulceration, and ulcerative colitis, will turn out to be due to these same causes, acting singly or in combination, but entertains the "possibility that there exists a group or groups of diseases the causation of which we do not comprehend, or comprehend but dimly." His statement of what he calls the psychosomatic hypothesis—which is at least commendably brief—is that it "attributes disease to certain abnormal states of the mind." This hypothesis is then immediately dismissed along with that of focal sepsis as unworthy of serious consideration by scientific medicine. "The small amount of critically established fact," he says, "on

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which these two hypotheses were based can probably be accounted for on the fact that the condition of any patient suffering from a chronic disease deteriorates when there is superadded infection or disturbance of the mind" (Pickering, 1950).

The other, speaking of the place of psychotherapy in the treatment of peptic ulcer, says: "In this connexion it should be recognized that, while many neurotics invent digestive disturbances, the ranks of genuine ulcer cases contain no higher proportion of neurotics than does the population at large. Most ulcer patients, indeed, are the reverse of neurotic. They may be anxious and worried, but they never invent symptoms, generally make light of their suffering, and rarely give up work unless the pain is intolerable" (Illingworth, 1952).

I would like to assure the authors of these in many ways admirable lectures that I have picked on theirs out of many others I could have chosen, not from any personal animosity, but simply because they happen to illustrate my thesis that certain ideas which seemed implicit in what I was taught are still current in academic medicine to-day.

I believe these ideas are almost without exception open to serious objections. I shall now examine some of them further, and in doing so review in an admittedly eclectic manner some papers which seem to me to illustrate changing ideas. In particular I wish to examine a common misapprehension that there is something called the 'psychosomatic hypothesis, which holds that abnormal mental states can cause physical disease. This will involve at least some reference to the idea of diseases as entities, to the idea of cause in medicine, and to the problem of the relationship between mind and body.

Diseases

It was and often still is assumed that there are entities called diseases which can be studied and treated as such. But, if we think about it, it seems obvious that we cannot observe diseases: we can only observe diseased persons during life or the mortal remains of diseased persons after death. Nosological classification may be a necessary convenience for purposes of orderly description, but we have come to think and often to act as if the states it describes exist as entities which can be studied and dealt with apart from the person who is diseased. One might almost feel one had to apologize for mentioning something so apparently trite and obvious were it not that much current teaching and action appears to be based on the opposite assumption.

Cause in Relation to Medicine

I have already said that it seemed to be assumed, when I was a student, that organic diseases had physical causes, and by this there was often implied a single cause. The cause of lobar pneumonia was the pneumococcus. The cause of cirrhosis was alcohol, and so on. The business of diagnosis was to find the cause of the disease, and the object of rational treatment was to remove it. When the cause was unknown, only symptomatic or empirical treatment was possible. I have perhaps laboured these illustrations to show that we really did seem to think in terms of single causes, though we cannot have gone very far in considering the implications of this view.

The idea of cause is a complicated one, and many people much better qualified to do so than I have written about it in relation to medicine. Howe (1934), for instance, speaking of the kind of examples of cause and effect I have just mentioned, says: "However, not one of these examples is truly a case of cause and effect. Not only is the effect produced in each case by the interaction of at least two

antecedent events, but each of these antecedent events is produced by the interaction of another two. If we are to be accurate we must then recognize not 'a cause' but an endless multiplicity of causation, which is extremely confusing. There never is, in fact, 'a cause' but always 'a relationship.'" And a little later on he says: "It is to-day generally agreed in theory, although not always recognized in practice, that disease is essentially an interaction between seed and soil, in both of which there is a convergence of a sequence of related events, from the association of which the effect is developed."

What we can observe, he says, is a series of events connected and related through the medium of time, stretching back into the past and forward into the future. If a bus runs over my leg I may say that the impact of the bus is the cause of the fracture of my femur. More accurately, the event may be described as the intersection of two time-sequences in which the bus time-sequence crosses the leg time-sequence. "If we seek causes we find two causal time chains: what caused the bus to be where it was, and what caused the driver of the bus to do as he did; what caused the leg to be where it was, and what caused the leg eventually to heal. These are not truly to be regarded as being causes, but as a series of related time-effects."

Recently Strauss (1952) devoted the first of his Croonian Lectures to a discussion of causality in medicine. I am afraid that many of the ill-educated generation to which I belong may have found his philosophical arguments difficult to follow. But we cannot fail to appreciate his criticism of rigid causal relationships in medicine and to be convinced of the inadequacy of our search for single or "specific" causes of disease. I cannot, however, resist one or two comments.

"It is arguable," says Strauss, "that a pure paired cause-effect relationship exists at two levels only: the ethical and the mechanical." Of the pure paired cause-effect complex at the mechanical level he gives two examples: a person swallowing a large amount of arsenic by mistake—the cause—leading to acute gastritis—the effect; and a second example, a personal experience of his own, of the sudden protrusion of a lumbar intervertebral disk—the cause—leading to severe pain in the back and complete immobilization below the waist—the effect.

These examples of simple cause and effect at a mechanical level are then contrasted with an account of the possible factors—bacteriological, constitutional, sociological, and psychological—concerned in the development by a particular person of the mode of morbid behaviour which we call pulmonary tuberculosis. The contrast is instructive, but one is compelled to ask whether in the two examples cited the effect can really be attributed to a single mechanical cause. I would like, for instance, to know how the "mistake" (which Strauss agrees is an over-simplification) of swallowing the arsenic occurred, and I would suggest the effect might have been different had the subject by some odd but not impossible chance been a habitual arsenic eater with a tolerance to the drug, such as has apparently been observed in Styrian woodcutters. Should not the effect in this case again be regarded as resulting from the convergence of two sequences of events—what caused the arsenic to be where it was and to be swallowed, and what caused the person who took it to be susceptible to its effects?

A Personal Experience

The second example interests me, also on account of a personal experience. I too suffered from a pain in my back—in this instance a chronic pain over many years—severe enough to be a nuisance at times, but never incapacitating or bad enough to make me do anything about it. In the earlier years I supposed it was due to fibrositis, whatever that is, and encouraged my dentist to look for a source of focal sepsis, which he did not find. Some years later, when it was particularly troublesome in a humid

tropical climate, I supposed that constantly damp clothes and bedclothes had something to do with it. Later, after reading about psychogenic backache, I concluded this must be the whole explanation. Some months ago an x-ray film of the spine showed, I am told, conclusive evidence of an old and considerable protrusion of a lumbar intervertebral disk. Here, surely, was another example of direct mechanical cause and effect, and incidentally a vindication of the importance of searching for a single demonstrable cause for a symptom or group of symptoms, rather than indulging in the vague speculations of multiple aetiology.

But I am not so sure, for I believe that the protrusion of my disk was not entirely an accident; and, further, I have to account for the fact that I have for some time been practically free from pain, though I am assured that the actual disk lesion is unlikely to have altered.

Briefly—and if you will forgive some personal details—I believe the protrusion of my disk can be traced back to two what might be called parlour tricks. The first was to challenge my fellows to put their hands flat on the ground in front of them without bending their knees. This, the only faintly distinctive physical achievement I had, used to give me satisfaction, and was probably harmless. Unfortunately some years later, though I had never been able to jump anything, I discovered I could do what is known in gymnastic circles as a “neck roll.” This consists in diving over some object, turning a somersault on a cushion or mat, and landing on one's feet again. It was a great success at Army mess nights and similar occasions. Unfortunately it involved a sharp flexion of the lumbar spine, and after a year or two I had to abandon it on account of increasing pain and stiffness in my lower back. I have little doubt this eccentricity was an important factor in the protrusion of my disk, and, of course, the story could easily be pursued further by inquiring why a clumsy and rather obese physician should have found it necessary to perform “neck rolls” at an age when such an accomplishment is not generally considered necessary—but I do not intend to follow it further on this occasion. I have said enough to indicate that I am fairly sure the protrusion of my disk was not a simple mechanical accident.

Once it was discovered I was given some back-extension exercises and advised to wear a lumbo-sacral support. This contrivance supports nothing except any abdominal protrusion which may be present, but is effective because it makes bending so uncomfortable that one ceases to bend. I persevered with the belt and the exercises for at most a week or two. I was virtually free from pain within a few days and have now learnt that I can remain so without the aid of the support or the exercises, so long as I do not habitually slump in chairs and car seats. When I do so slump, I get a slight return of pain. I have no doubt that for me “slumping,” as I have called it, is a physical expression of certain emotional attitudes.

I have troubled you with this rather long personal anecdote because it seems to me to illustrate clearly the inadequacy of the idea of single causes. For you will see that if you were to ask me what was “the cause” of the pain in my back I should find the question difficult to answer.

What we call the cause of an illness is never strictly the whole cause, though it may be a necessary factor in causation in the sense that one would not have that particular illness without that particular factor. I would not have had the particular kind of pain in my back if I had not had a protrusion of an intervertebral disk. But the protrusion of the disk was simply one link in a chain of causation, and the pain, once it was understood, was relieved by fairly simple measures, which presumably left the disk lesion *in situ*.

It is worth noticing, perhaps, that the main difference between these two examples of the protrusion of a disk is that the first might be described as an acute illness and the second as a chronic. This is probably an example of a

general difference, to which attention has often been drawn, as for instance by Ryle (1942). Whether or no the search for single or specific causes is ever justified on philosophical grounds, it has been more productive of results in acute than in chronic diseases. Medicine's greatest successes have been in connexion with acute disease and particularly acute infections. Where partial success has been achieved in the treatment of chronic diseases, as in myxoedema, diabetes, and pernicious anaemia, one must note that in each instance the patient is not cured, and our knowledge of the aetiology of the condition is still quite incomplete. Our ability to deal successfully with most chronic diseases has advanced much more slowly than in the case of acute ones, and it is possible that this is due, at least in part, to our undue preoccupation with single specific causes and direct mechanical cause and effect.

Aetiology in Relation to Medicine

Aetiology is defined in the *Oxford Dictionary* as “the science or philosophy of causation” and in *Dorland's Medical Dictionary* as “the sum of knowledge regarding causes.” Strauss has described the substitution of the term “aetiology” for “cause” as no more than a resort to the magic of semantics in an attempt to escape from the fetters of rigid causality. But I believe that the change in words can be something more than semantic juggling, and may represent a change both in ways of thinking and in ways of acting, which is to be encouraged even though we are still far from a satisfactory concept of aetiology.

Ryle (1942), after quoting the definitions I have mentioned, continued: “It is well to insist on such definitions, for the discovery by Pasteur and the great pioneers in bacteriology of specific microbic agents of disease, and the recognition by others of specific chemical agents and specific deficiencies, have, until quite recently and contemporaneously with their immense benefits to medicine, had a peculiarly limiting effect upon the vision and the practice of many medical men, not excluding teachers. They have, in fact, compelled a neglect of the associated causal factors without which no disease can have its being. They have also fostered a belief in or search for single determining causes where none exist.”

A paper by Halliday (1943) seems to me of particular value in that, though some difficult matters are perhaps oversimplified, the author outlines a concept of aetiology which seems to avoid obvious pitfalls and to provide a reasonable system of thought on which medical action can be based. In the space available I can do scant justice to this paper, which could with profit be read, pondered, and re-read by every teacher of medicine.

Cause in medicine, says Halliday, has usually been regarded in one of two ways; and he calls them the “mechanismic” and the “biological.”

Mechanismic Cause

The word mechanism refers to a system of mutually adapted parts working together as a machine. Given the requisite preceding movement, the ensuing movement follows necessarily upon it, provided the machine is in working order. During the last three centuries, knowledge of the human organism in terms of mechanism increased progressively and the organism came to be regarded as if it were in actual fact a machine. If it failed to function properly, the cause of the breakdown was similar to that of a machine—that is, a fault in one or more of the component bits and pieces. The primary concern of medicine was to identify the fault, which might be viewed by any technique—for example, gastric ulcer, a structural fault; acidosis, a chemical fault; or hypertension, a physical fault—and to take appropriate action by interfering with the mechanism.

Some of the implications of mechanismic aetiology Halliday summarized as follows: (1) The human organism

is a machine composed of mutually adjusted parts working together. (2) Illness corresponds to breakdown in the machine. (3) The cause of illness (provided the patient has adequate food, air, and water) is something wrong—a fault, disease, lesion, imbalance, or abnormality—in one or more of the parts. (4) Medical action is confined to interfering with the mechanism by what is known as the appropriate treatment.

This mechanistic attitude has, of course, brought great advances. The criticism of it is not so much that it is wrong as that it is not enough. In particular it gives little or no guide to action in the prevention of disease, for it is concerned with how a patient is ill rather than why he is ill. One might add that while few doctors now take a purely mechanistic view, most patients do, hence the very great difficulty of giving them any reasonable explanation of functional symptoms. Say what we will, the patient believes there must be a fault in the mechanism somewhere—"the cause"—and an appropriate means of dealing with it—"the cure." One might also point out that the mechanistic idea of disease allows of no definition of health other than the absence of disease, which is plainly inadequate, for there is a great difference between "no disease" and health.

Biological Cause

Halliday explains what he calls the biological idea of cause as follows. "Illness is regarded, not as a fault in the parts, but as a *reaction*, or mode of behaviour, or vital expression of a living unit in response to those forces which he encounters as he moves and grows in time. Cause is therefore *twofold* and is to be found in the nature of the individual and the nature of his environment at a particular point in time." The environment, the totality of exterior circumstances, may be investigated by a variety of techniques—physical, chemical, bacteriological, psychological, and so on—and in this way split up for convenience into separate components which we may call factors.

That an aspect of cause is found in the individual person may not at first sight seem so obvious. It may be illustrated by pointing out that when two persons encounter the same environmental factor the behaviour of each depends on his characteristics. Suppose, for instance, that a weight of 2 st. falls equally on the legs of a man aged 30 and of his father aged 80. The son will probably develop a superficial bruise, and the father a fracture of both legs. Two men swallow water containing virulent typhoid bacilli. One who gives no previous history of typhoid fever responds ten days later by a morbid reaction whose features include fever, prostration, diarrhoea, etc. The other, who does give a history of previous enteric fever, shows no evident morbid behaviour. When an SOS is broadcast several million people may be quite unaffected, while one man falls down in a faint. In each of these imaginary but quite possible examples the environmental factor was certainly not the whole cause. An aspect of cause was present in the persons affected.

Some of the implications of biological aetiology were summarized by Halliday as follows: (1) The human organism, although composed of parts, may also be regarded as an integrated unit or living person. (2) Illness represents a vital reaction of a person to factors of the environment which he meets as he moves in time. (3) The cause of illness is therefore twofold—certain characteristics of the person and certain factors of the environment. (4) Medical action is concerned primarily with measures designed (a) to alter or prevent characteristics of the person known to be causal, and (b) to alleviate or remove factors of the environment known to be causal.

This biological attitude is, as Halliday points out, a *sine qua non* for effective prevention of disease and may be very important in treatment. One might add that it does make possible a reasonable explanation of functional symptoms, the reaction of a particular personality to factors in the environment, and it does make possible a concept of health

other than in terms of "no disease." Halliday then summarizes the ideas underlying the phrase "the cause of the illness" as follows:

"(a) As regards the *illness*, the fields of observation and discourse are the features (signs and symptoms) of a mode of behaviour of an individual.

"(b) As regards the *cause*, the fields of observation and discourse are three—namely:

"(1) *The field of the person*. Under this heading are put the observations on the characteristics of the person before he became ill. These are innumerable, and may be noted in terms of any technique. They include, for example, age, sex, weight, height, bodily habitus, history of previous illnesses, history of illness in the family, blood pressure, menopause, intelligence, etc. The problem is to distinguish which of these characteristics may be regarded as relevant and causal.

"(2) *The field of the environment*. Under this heading are included observations on the factors of the environment which the person met at, or shortly before, the time he fell ill. The observations may be made in terms of any technique—e.g., physical (trauma, heat, cold, light), chemical (diet, poisons), bacteriological, and psychological (death of a loved person, failure or promotion, etc.). Such factors are innumerable, and the problem again is to determine which of them may be regarded as relevant and causal.

"(3) *The field of mechanism*. Under this heading are put observations on the 'bits and pieces' (structural, physical, chemical, psychological, etc.) which are set into action by the encounter and which ultimately bring about the particular mode of behaviour. The problem is to distinguish which of these are primarily involved."

And the three questions which he suggests one should attempt to answer about any illness are, of course, now quite well known: (1) What kind of a person was this before he took ill?—that is, which characteristics of the person are relevant and causal? The field of the person. (2) Why did he become ill when he did?—that is, which factors of environment are relevant and causal? The field of the environment. (3) Why did he become ill in the manner he did?—that is, which part contacted the factor and which bits and pieces by preceding movement and ensuing movement finally made manifest the particular mode of behaviour? The field of the mechanism.

It seems at least, from this brief review of what is meant by cause or aetiology in medicine, that we can never strictly think in terms of a single or specific cause of a disease. On this ground alone it appears unsatisfactory to suggest that abnormal mental states can in themselves cause physical disease.

Body-Mind Relationships, with Reference to the "Psychosomatic Hypothesis"

It was assumed, and I think still is, that diseases can arise in and affect the mind on the one hand, or can arise in and affect the body on the other; and we argue then whether disturbance in the mind may cause disease in the body. My excuse for venturing, with some trepidation and in however superficial and amateur a fashion, on a subject which has puzzled some of the best minds of every generation for well over two thousand years, must be that I believe it is impossible to practise or teach medicine without making some kind of assumption or taking for granted some kind of hypothesis about the relationship between what we call mind and what we call body. Many people do not, of course, consciously make any such assumptions. They take them over ready-made from teachers and textbooks. I should put it rather, perhaps, that our attitude to disease and our manner of dealing with patients must involve some hypothesis of body-mind relationships, whether we are aware of it or not. Fortunately the problem I want to discuss is a fairly limited one. It is simply to ask, have we any right to divide diseases into physical and psychological, in the sense that they arise in the body or arise in the mind, and have we any right to talk about events in the mind causing disease in the body?

Monism

Victorian materialism regarded matter as real and objective, and mind simply as some kind of a by-product of matter. Though, so far as can be seen from their medical writings, this is the view to which many medical scientists still adhere, others adopt some kind of psycho-physical parallelism or some form of monism, which regards mind or conscious states and brain states as different aspects of the same events. As Russell Brain (1951a) explained one form of this hypothesis: "What we call events in the physical brain are happenings about which we may have indirect knowledge inferred from our perception of other people's brains and what they tell us about their experiences; but we have direct awareness of the physical events in our own brains and when we thus perceive them we find them to be thoughts, sensations, feelings, and so on."

According to this hypothesis there is thus only one kind of events, but they appear different to us because we have knowledge of our own minds in terms of perceptual symbols and knowledge of the physical world, including other people's brains, expressed in conceptual symbols.

If for the moment we accept this hypothesis, then it seems to me that the questions I asked have little meaning. Brain states and mind states are two aspects of one series of events which appear different only because we perceive them in different ways. One cannot then imagine purely physical disease or purely psychological disease, and it makes no sense to ask whether mental states can cause physical states. If one asks whether emotions like anxiety or anger can cause physical disease—what one is really asking is whether mind-brain states like anxiety or anger can cause physical disease elsewhere in the body. If, indeed, we watch someone losing his temper, can we really say that this is something in the mind—something purely mental—causing something in the body? And I do not think it makes much difference to the argument if we suppose that brain states and mind states are different events, but events moving, so far as we know, exactly in parallel.

Common-sense Dualism

I would like to ask those who talk about physical diseases arising in the body and psychological disorders arising in the mind, and who argue whether disorder in the mind can cause physical disease in the body, to explain to what kind of a hypothesis of body-mind relationships they are subscribing? So far as one can see, it must be a so-called common-sense dualism, which conceives of an independent immaterial mind in some mysterious way interacting with a material brain and body. But I suspect that this popular dualism again depends on the two kinds of knowledge to which Russell Brain (1951b) has drawn attention. We have difficulty in equating events in our own minds which we perceive directly as thoughts and feelings with events in brains of which we have only indirect knowledge by inference, and we therefore regard our thoughts and feelings as more or less independent things, without pausing to consider the implications of this view.

But whatever ultimate belief or philosophical hypothesis we may entertain, so far as our observation can go, a person is surely a body-mind unity. We have no experience of mind separate from body in ourselves, and we cannot imagine such a state of affairs in anyone else. So far as our own observations and the inferences we can make from them go, a person is a body-mind unity; body and mind are in some way inseparable aspects of a person.

It would seem, therefore, that we must regard all diseases as states or modes of behaviour of a person who is a body-mind unity, and it follows, surely, that all disease must be regarded as psychosomatic, in the sense this word is generally used. All disease must affect in some degree both body and mind. We have no right, then, to divide diseases into physical and psychological, in the sense of that which arises in or affects the mind and that which

arises in or affects the body, nor to say that something happening in an entity mind can cause something else to happen in an entity body.

But we can study diseased persons by different methods. If we study them by methods which provide information in physical terms, we obtain information in physical terms. If we study them by methods which give an answer in psychological terms, we obtain information in psychological terms.

What we can do then is to group disease states roughly into those in which most of the relevant and useful information seems at present to be obtained by physical methods; those in which most of the relevant and useful information seems at present to be obtained by psychological methods; and those in which both methods supply relevant and useful information. The last group corresponds, of course, to what are often now referred to as the psychosomatic disorders.

But even this division can be no more than a rough and temporary grouping for the sake of convenience. For I believe it is true that the more diseased persons are studied by both physical and psychological techniques, the more disease states we find in which both methods produce relevant information.

Literature of "Psychosomatic Medicine"

The literature of so-called psychosomatic medicine—I shall come back to that term later—is already considerable, and I shall not attempt to review more than a very little of it, partly because such an attempt would need to occupy at least several lectures, and partly because I believe it would be unprofitable. For I suspect that a person's attitude to what may be called the psychosomatic method in medicine depends less on an intellectual evaluation of the literature than on his attitude to neurosis. Just as an overtly neurotic patient tends to arouse irrational feelings in most of us, so does the suggestion that illness, needless to say in ourselves as well as in our patients, can be fully explained only when psychological and physical factors are taken into account. We resist this suggestion as applied to ourselves, and we tend to deny it in our patients.

Earlier I quoted an article by Pickering (1950) in which he dismissed what he called the psychosomatic hypothesis in a few lines on the ground that the small amount of critically established fact on which it was based could be explained in other ways. But in doing this he makes an assumption: he assumes, it seems to me, that a satisfactory account of the causation of disease states, or some of them, has been, or can be, given in purely physical terms; and that it therefore behoves those who are interested in the so-called psychosomatic hypothesis to prove their case by providing critically established fact.

But this is a big assumption, and I would ask those who make it to consider carefully in how many chronic diseases can they give a satisfactory account of the aetiology in purely physical terms? We come nearest to giving such an account, I suppose, in nutritional deficiencies and in certain chronic infections, where again one factor in the environment is of particular importance. But is that factor, even in these instances, the whole story? A great deal is known, for instance, about the tubercle bacillus; but can we really explain on humoral grounds why some people become ill with tuberculosis while others do not, and, of those who do become ill, why in some the process is arrested while in others it is not? I doubt if we can, and I believe that studies of personal factors which may be concerned in the onset and course of tuberculosis such as those of Wittkower (1949) and of Day (1951, 1952), even though they do not amount to "critically established fact," are still of great interest and importance in this connexion.

I would like to suggest that we may equally, and indeed with much more reason, start from the historically older and philosophically more satisfactory assumption that illness

is a state of the whole man—in the sense that it is a state of a body—mind unity, and ask those who doubt the importance of psychosomatic method to establish their case by giving a satisfactory account in purely physical terms of the aetiology of even one of the chronic conditions which are at present described as of unknown aetiology. Peptic ulceration, ulcerative colitis, hypertension with all its sequels, rheumatoid arthritis, and asthma, to name but a few, await such an explanation.

But, it may be said, since rheumatoid arthritis can now be dramatically if not completely relieved by supplying the substance cortisone, we can surely regard rheumatoid arthritis as a purely physical abnormality, curable by physical means. And doesn't this make the complicated and difficult psychosomatic approach unnecessary?

Those who use this kind of argument are confusing mechanism with aetiology. The discovery of cortisone and A.C.T.H. has brilliantly illuminated, or bids fair to illuminate, the physico-chemical mechanism of rheumatoid arthritis amongst other things, but it has brought us no nearer being able to answer the question why a given person is taken ill with rheumatoid arthritis or how this illness can be prevented—just as the discovery of insulin and vitamin B₁₂ has led to very great advances in our knowledge of the physico-chemical mechanism of diabetes and pernicious anaemia without telling us why some people develop these conditions or how they may be prevented.

Medicine in undergraduate teaching schools, as Halliday put it, has shown hitherto a distinctly mechanistic bias, in the sense that it has devoted enormous energy to the explanation of the mechanism of disease in physico-chemical terms, relatively less to the environment, and still less to the person. But there are signs that this is changing. There have been, for instance, numerous psychological studies of personality types in different illnesses, notably the very extensive contributions of Dunbar (1943), and her colleagues from the psychiatric and medical divisions of Columbia University. My own feeling is that these and other personality studies, though of great interest, are somewhat inconclusive in that they seem to depend so greatly on the subjective interpretations of the observer, and it remains to be seen whether independent observers will always find the same personality types or range of types in the same illnesses. At the same time a start has been made on the more accurate study of physical types by means of Sheldon's method of somato-typing (Tanner, 1949).

To my mind the outstanding recent contribution to the study of man, rather than mechanism, has been the work of Wolf, Wolff, and their colleagues at Cornell University Medical College, over the last 10 to 15 years.

For details of their methods and results one must read at least their two monographs (Wolf and Wolff, 1943; Grace, Wolf, and Wolff, 1951). Very briefly, one patient with a gastrostomy and four with prolapsed colonic mucosa were studied over considerable periods. The rate of blood flow, as judged by colour changes in the mucosa, and secretory and motor activity of the stomach and colon were measured in a variety of circumstances, and notes made on the effect upon them of ordinary physiological stimuli, of drugs, of the patient's particular life situation, and sometimes of emotionally coloured happenings, either occurring spontaneously or provoked deliberately during the course of the experiments.

I believe these are important studies for several reasons. They were performed on man, and intact man, except for the accident by which either gastric or colonic mucosa was visible. Instead of trying to standardize the conditions of their experiment by avoiding the complication of using conscious human subjects, or by what might be called eliminating the human factor, these observers standardized all the other conditions of their experiments so far as possible, and deliberately set out to study, among other things, the effects on the stomach and colon of human situations and emotions.

What emotion the subjects were actually experiencing in the different experimental situations had, of course, to be inferred. But, given that the authors' inferences were substantially correct, their results are of the greatest interest. I would like to mention one or two points which seem particularly relevant to my lecture. In an animal preparation the actions of a drug are usually constant and predictable. In these observations on man there are several instances where this was not so, and where the actual effect of a drug appeared to depend not on its pharmacological action but on the meaning of its administration to the particular subject. This is seen most clearly in the account of the effects of the intravenous administration of atropine on the colon in three of the subjects with colostomies. In two subjects who were thought to be in a state of relative security and relaxation the injections were accepted with apparent equanimity, and their effect was a profound decrease in motor activity of the colon and some blanching of the colour of the mucosa, as would be expected on pharmacological grounds. In a third subject, who resented the experiment, the same intravenous dose of atropine was followed by a great increase in motor activity and in the colour of the mucosa—changes which had been found previously in states of anger—and, though the atropine produced its expected effects on the salivary secretion and circulatory system, at no time was there any pallor or diminution in the motor activity of the colon. It appears that in this instance the physiological effect of the resentment aroused by the injection of the drug predominated over its expected pharmacological effect.

This observation is in fact a particular instance of something which appears throughout these studies—namely, the important and often predominant effect on the subject's gastric and colonic function of the relationship existing at the time between the observer and the subject or, in more general terms, the physician and the patient.

Another point I would like to mention is the authors' finding that the colonic changes in anger and hostility were greatest in the two of the four patients who had conspicuous difficulty in expressing their feelings (and these incidentally were the two of the four patients who suffered from ulcerative colitis). This again only confirms what has long been held by psychiatrists, but the demonstration that the actual changes in the colon were greatest in those who appeared to be suppressing their anger is new and impressive.

I believe this study is important not only for its content, but because it represents a new departure—the measurement changes in function in the organs of man, in health and disease, in relation to life situations and emotional states. It may well be the beginning of a new chapter in our understanding of illness.

Health

It seems inescapable that sooner or later we shall come, or indeed we shall be driven by the economic pressure of the cost of sickness, to regard the prevention of disease and promotion of health as more important than the curative or more often palliative medicine to which at present we devote so much more attention. One of the main difficulties seems to be that we have no clear idea of what constitutes health. So far as I can remember, health was not mentioned when I was a student. The mechanistic view of disease which was then the rule left no room for a concept of health, other than in terms of no disease. Just as a machine which was in working order ran, so the body was regarded as healthy so long as there was no breakdown or disease. But good health is something more than no disease, and we can all recognize it when we see it, even though we cannot say just what constitutes it or how to go about getting it. I believe it is our profession's lack of a concept of health, other than as a state of no disease, that leaves the way open for widespread exploitation of the average person's natural desire to enjoy as good health as he can.

My object in raising this question is not to attempt to settle the meaning of health. We are asked to believe, indeed, that this has already been done by the World Health Organization, which has laid it down in most authoritative and uncompromising terms that "health is a state of complete physical, mental, and social well-being; and not merely the absence of disease and infirmity." Here, adds its Director-General, as a kind of warning to meddlesome intruders, is one word concerning the meaning of which the dictionaries will not have to worry from now on (Brock Chisholm, 1949). I need not spend time on this definition (with its necessary implication that all of us are sick, since none of us enjoy complete physical, mental, and social well-being), because it has already been ably examined and criticized by Curran (1952). With all deference to the Director-General of the World Health Organization, I believe we are still far from a satisfactory idea of what constitutes health.

The *Oxford Dictionary* defines health first as "soundness of body; that condition in which its functions are duly discharged," and gives as a further meaning "spiritual, moral, or mental soundness." I imagine it would be generally agreed that health cannot be fully explained in physical terms, and, whatever opinion we hold about disease, health is certainly psychosomatic.

One of the most interesting discussions of health and disease I know of occurs in a small book by Crew (1949) on *Public and Personal Hygiene*. After pointing out that the individual must be capable of adjusting himself to the conditions and circumstances of his external physical and social worlds, he defines health "as that state of the individual in which harmony exists between the various component parts of himself and between the individual as a whole and the circumstances and conditions of his external world."

This definition is a distinct advance, but does not seem entirely satisfactory for at least two reasons: first, no one enjoys complete harmony either in himself or with his external world; and, secondly, health is not so much a state of harmony as the product of a process of continuous adaptation or adjustment, by which a reasonable degree of harmony is maintained. The nearest I can get to an explanation of health—I would not say a definition—is that a person's health depends first on the constitution he is born with, and then on the success he has in constantly adjusting either himself to his environment or his environment to himself, so that a reasonable degree of harmony is maintained both within himself and between himself and the social and material world in which he lives.

But my object in raising this question was not to attempt to settle the meaning of health. It was to point out the need for such a concept, and some of the difficulties in formulating it—difficulties which are, I believe, intimately connected with the questions that I have been discussing. It is no accident that academic medicine has no concept of health; for the mechanistic idea of disease leaves no room for one, other than in terms of no disease, which is clearly inadequate.

Discussion

I would now like to try to pull together some of the apparently disconnected threads of my discursive and eclectic review of some changing concepts in medicine.

Thinking in medicine seems too often to be based on unexamined or insufficiently examined assumptions. It seems often to be taken for granted, for instance, as I have tried to point out, that we can usefully study diseases rather than diseased persons; that all disease or most diseases can be explained in terms of direct mechanical cause and effect; and that diseases can be divided into physical and psychological, with the implication that so-called physical diseases can be studied and treated satisfactorily by exclusively physical methods. In general these individual assumptions add up to a main one, that disease—no one seems to have worried much about health—can be explained in mechanistic or physico-chemical terms.

The rise of so-called psychosomatic medicine, which in America appears already to have assumed very considerable proportions, can best be regarded in a social sense as a revolt against this main assumption; and it is significant that this revolt has coincided with a growing tendency among some physical scientists to question how much their methods can be expected to explain. Strictly, as I have also tried to emphasize, there can be no such thing as psychosomatic medicine or a psychosomatic disease, for these terms involve a confusion between the methods of examination and the objects of study. Health and disease are necessarily psychosomatic, so there cannot be a part of medicine or some diseases which are psychosomatic. What the psychosomatic method does insist—or rather reiterate, for its teachings are as old as medicine—is that in health and disease man must be considered as a whole, and diseased persons must be studied by both physical and psychological methods. It is perfectly true that, in some, much information may be given by physical and little by psychological methods, and in others much by psychological and little by physical methods; but we must not be tempted to conclude from this that there are physical diseases and psychological diseases.

It is often assumed that there is an antithesis of some kind between science and art in medicine. If one were to hazard a guess, a time will come when this too will be regarded as having been a mistaken assumption. When this has happened we shall recognize sciences of pathology, pharmacology, experimental medicine, and so on (for many aspects of disease are explicable in terms of mechanism); and possibly a science of psychology. But strictly clinical medicine will, I believe, be regarded neither as an art nor as a science in itself, but as a special kind of relationship between two persons, a doctor and a patient. It will be recognized that this relationship is the essential feature of clinical medicine; and that the degree of success that it has depends, as in other human affairs, on the skill with which both science and art are called to its aid.

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Australia's greatest pest, the rabbit, is rapidly being brought under control by the planned introduction by the Commonwealth Scientific and Industrial Research Organization of the virus disease myxomatosis. The disease is spread among rabbits by mosquitoes, and in the last three years has gained such ground that the Lands Department of Victoria now estimates that more than 90% of the rabbits have been destroyed in large areas of the State. Similar destruction is reported from many parts of New South Wales, South Australia, and Southern Queensland, with the result that farmers and graziers report a phenomenal increase in the carrying capacity of their pastures. The Minister for External Affairs, Mr. Casey, who is also in charge of the C.S.I.R.O., said that if the gains won at such low cost were maintained Australia's carrying capacity would increase by the equivalent of 10,000,000 sheep. However, he warned that in many districts rabbits were showing an increased resistance to the disease, and that in a very few more seasons myxomatosis might have lost most of its killing power.