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THE NATURE AND PLACE OF THINKING IN MEDICINE*

BY

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1. The Nature of Thinking

At a first encounter there may seem to be something peculiar about the title of this lecture. Surely it could be said the nature of thinking is the same under all circumstances, and in no way dependent upon its subject matter. This view has, indeed, been held by a great many people, and among them are some excellent psychologists. But it is not to be taken for granted. Even a swift survey, whether of contemporary medical activities or of the history of medicine, is enough to suggest strongly that the fact of the very great diversity of the persons to whom medical discovery and practice offer an overwhelming attraction cannot possibly be accounted for simply in terms of difference of interest in particular medical topics. It would seem that there must be some radical differences between strongly preferred methods of investigation, and that these, in their turn, may be traced to equally radical differences in the process of thinking. Doubtless the differences which I am going to try to discuss can be illustrated in other branches of knowledge as well. But their wide range, as well as the rapidity of change of fashion, of method, and of emphasis to which they have been subjected throughout the whole of their history, makes medical science and practice an outstandingly interesting and fruitful field within which to study the nature and the place of thinking.

Both in ordinary conversation and in technical definition "thinking" is a term which is almost always given an inconveniently wide application. When a man says "I didn't think of that," all that he means is that he failed to give attention to something which was not, in fact, there for him to see or hear, or otherwise to perceive. And most formal definitions which have been framed amount also precisely to this: that there is thinking whenever sensory or perceptual signs or stimuli are supplemented in the course of their observation or discussion in any way whatsoever. Now, there is no known human observation or discussion in which supplementation of this kind does not take place. And so the common view means not only that thinking is present in perceiving—which nobody need doubt—but also that remembering is thinking, and the whole sweep of imagination and fancy is thinking, and believing is thinking. Thinking, in fact, becomes so multiform that it is quite impossible to say anything definite about it at all,

except the one formal statement that it is the use of information not immediately sensorially or perceptually present.

If we are to get anywhere effectively we must have more definite starting-points than this, although in a single lecture these will have to be stated in a rather dogmatic manner, and without that experimental justification which is available.

Everybody agrees that thinking is one of the ways that animals have developed for dealing with evidence. Broadly, all human evidence is of two sources—either it comes from direct observation or else it is in some form of record, sometimes descriptive, but more often in the mode of an achieved generalization presented for acceptance or criticism. The first is the only original source, and it requires sensorial perception. When evidence of this sort is fairly examined the striking thing about it is its partial and fragmentary character. At no stage is animal observation a literal and complete reduplication of the external environment upon which it works. The character of the sensory and perceptual mechanisms through which it is made renders such reduplication impossible. Moreover, every great step in advance of animal observation has been less in the service of the accurate and complete reduplication of the external signs and stimuli and more in the service of extending the range and increasing the amount of available evidence, and especially of the building up of information on evidence. It is exceedingly likely, for example, that human observation is less accurate and less complete, relative to its possible range, than that of more primitive and restricted forms, where the sensory mechanisms are fewer and much less specialized. However this may be, all the evidence that is directly accessible comes to us fragmentary and scrappy to a degree. It is full of gaps, and we must try to fill up the gaps. Sometimes—there are innumerable cases in clinical diagnosis and in all kinds of experiments—we have evidence, and then a gap, and then more evidence. We must interpolate. Sometimes, as in clinical prognosis, there is evidence and the evidence stops short and we must extrapolate. We can interpolate or extrapolate, if we like, in the interests of what is often called aesthetic satisfaction, or we can do either or both under the dominance of objectivity—that is, in such a way that the issue we reach can be identified by observation, or proved in accordance with the nature of the system within which we are working. Thinking chooses the second, the objectivity-dominated way.

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Thinking also usually, though not invariably, requires a number of steps or links, and when it does each step must be such that it reduces the number of possible next steps, until eventually, sometimes quickly, sometimes after a protracted process, a stage is reached beyond which all thinking persons will proceed uniformly.

That the kind of process which I have just described briefly does take place when people are given fragmentary evidence and required to bring it to an objective issue can be experimentally demonstrated. That it is the kind of process which all people who have used the term "thinking" rigorously have intended to indicate seems at least to be extremely likely.

This, however, is a lecture on thinking in medical science, and so without any more discussion I propose to try to see what direction this somewhat general lead can give to a study of *the Nature and Place of Thinking in Medicine*.

2. Everyday Thinking in Medicine

First I want to consider the psychological processes involved in certain relatively simple cases of thinking, very common and necessary as I understand them, in medical practice. I shall call this "everyday thinking."

When a "man in the street" is confronted with fragmentary evidence, say about current events, and is required to bring it to an issue, more often than not he will do it by accepting or adopting some generalization, because somebody that he trusts has said so or because it has prestige within his social group, and immediately identifying the evidence as an instance. The issue is then little or nothing more than a restatement of the generalization in terms of the particular evidence.

There seems no good reason to refuse to call this "thinking." At any rate, it certainly is what passes for thinking in most of us most of the time. It does consist in filling a gap between the generalization which is accepted and some concrete evidence which has to be directly examined. It is, basically, the process of at least some of those doctrines of "physical signs" which have received much support in medical practice from time to time.

Or perhaps an instance is the recorded statement of Ambroise Paré when he was asked if the wound of the King of Navarre was mortal. "Yes," he said, "because all wounds of great joints, and especially contused wounds, are mortal according to all those who have written about them."

From our present point of view we need not be much concerned with the generalization or formulated principle which is involved. It is sufficient to notice that this is simply accepted and may be believed. The critical part of the process lies in the treatment of the concrete evidence. For in a great many instances this evidence will lack some of the characters that are required to put it into a general class, and in practically all instances it will possess other characters. The whole process thus depends on observing or, in many cases, assuming points of similarity and ignoring differences. The interesting thing is that for anybody who relies solely on direct observation differences stick out much more prominently than similarities, and are much easier to observe. But once we have accepted a principle the situation is radically changed, and it becomes increasingly easy to slur over differences. Consequently the integrity of thinking in this simple kind of case depends almost entirely upon a determined, and usually conscious, effort to observe and weigh differences, and it is this quality in observation, to give due weight to differences, which needs above all else to be stressed in relatively routine diagnosis. No doubt extreme instances of what can happen when it is ignored are the eighteenth-century theories of "large and small" doses associated with the names of John Brown and Samuel Hahnemann.

Or, again, though maybe it is unfair to treat this as an instance of medical diagnosis, there was the driver of an ambulance wagon in the American Civil War. He was asked if he knew how to treat wounded men. "Oh, yes," he said; "if they are hit here"—pointing to his abdomen—"knock 'em on the head; they can't get well." Any pre-occupied medical practitioner who is forced to make rapid routine diagnosis is always up against this difficulty, the psychological source of which is the power of principle to obscure differences, and there seems to be only one real remedy, the development of an experimental approach. We must therefore go on to consider and attempt some analysis of

3. Experimental Thinking in Medicine

Experiment is still often regarded as a kind of abrogation of thinking. Every medical student must have been told of John Hunter's advice to Jenner, "Don't think, but try; be patient, be accurate," or, in an alternative version: "Why think? Why not try the experiment?" But Hunter's own practice, and Jenner's, too, show clearly that the warning was simply against the pitfalls of the kind of thinking that we have just been considering. I believe it can be shown that experimental thinking is different in important respects from the kind which I have called "everyday."

Perhaps there are two sorts of scientific experiment. One, usually the earliest kind to occur, is when some large topic or field of knowledge is well known to be inadequately mapped out, and somebody decides to try to improve ordinary unassisted processes of observation by special methods of recording which will make the detection and interpretation of likenesses and differences in arranged cases that come within this topic more exact and certain. The other, more limited and directed, usually begins when somebody decides to take seriously certain differences revealed by direct observation between specific instances and accepted principle. The kind of thinking required is pretty well the same in both, but the first tends to grow into the second, which becomes both more common and more fruitful as specialization advances. I propose, therefore, to take a fairly simple case of the second kind—probably simplifying it more than is right for the purposes of rapid discussion—and to try to bring out its leading characters as an illustration of experimental thinking.

When Sir James Mackenzie was a young medical practitioner he wrote, "I set myself the task of finding out the nature of the symptoms and signs I met with in the course of my practice. This placed before me two definite objects at which to aim—(1) the understanding of the mechanism of symptoms, and (2) the understanding of their prognostic significance." In this general statement there are two points of importance: the first a resolve to take direct observation seriously, and the second that observations of medical significance must inevitably fall into time-related sequences with gaps that must be filled in. The general resolve, however, had to become more specific. A few years later the death of a patient from heart failure directed Mackenzie's attention to the disorders of circulation, and especially to irregularities of the pulse. At that time any form of pulse irregularity tended to be regarded as serious.

The story of Mackenzie's investigations is already well known. First came the improvement of direct observation by the systematic use of instrument tracings. Then came the first critical step: the comparison of two sources of direct information, sphygmographic and jugular tracings which so far had not been combined for practical purposes, leading to the separation into a special group of those forms of irregularity alleged to be due to a premature beat of the ventricle. There followed the recognition that these also must not be lightly placed all into one class, leading eventually, after about ten years of observation and thinking, to the separation of that form of arrhythmia in which the absence of the auricular wave in jugular tracings was associated with the disappearance of the presystolic murmur, to the reinstatement of a sensible form of digitalis therapy,

and finally to a radical revision of the significance of heart murmurs which had already been studied and described over and over again, and to the view that in the end the only thing that matters is the reserve power of the heart muscle.

It is already an old story, and of course much of it has been rewritten since, but the course of Mackenzie's investigations does bring out very well some of the leading characteristics of experimental thinking in medicine.

First comes the firm resolve not to be swept away by the slumping power of established generalizations, but to give the direct evidence a fair chance. This means almost always that unaided observation must be supplemented by instrumental records. As soon as this is done differences between cases will at once, and, so to speak, by their own inherent capacity to attract attention, spring into prominence. This is only a first step. Differences of themselves lead nowhere, but hidden within them may be some significant similarities which have to be searched for, for they are inherently as difficult to identify as differences are easy. Almost always, perhaps always, they are found only when two or more sources of evidence or of information which so far have never been effectively combined are brought together. This identification of significant likeness or agreement, coupled with an appreciation of differences but a determination not to be controlled by them, is really the crux of the whole thinking activity in the case of the experimental investigation. There is, however, a third characteristic, important in all biological experimentation, but probably more vital in medicine than anywhere else, and this is that the direct sources of information which are used must all be treated as, related in a sequence in time and proceeding towards some point which can be treated as terminal. This consideration leads us at once to the third kind of thinking, different in its nature from the other two, far more difficult to understand, and more fascinating to study: namely, to

4. Prognostic Thinking

Perhaps it is not too much to say that all really characteristic or specific medical thinking started in a recognition of the importance of prognosis. "I hold," said Hippocrates, "that it is an excellent thing for the physician to practise forecasting." All the great clinicians have said much the same, though it is less fully recognized that prognosis is equally a part of medical experiment, and that when general biological experiments turn out to have a medical significance it is because their conclusions are viewed in the light of some prognostic approach.

On the whole there seems to have been very little direct and sustained attempt to discover and understand the processes which are internal to medical prognosis. They are perhaps most frequently supposed to depend upon continuous faithful observation and accumulated experience. "No doctor," said James Mackenzie, "lives long enough to write a reliable book on prognosis." He was himself no doubt a fine clinician, but the idea behind this assertion seems wrong. Prognosis is often, at the least, a swift process, though its confirmation may be slow enough. On the whole I think that the great prognostic physicians have shown their special facility early, just as they have also been far more successful with the particular case than with their generalizations from many cases.

There seems to be something about medical prognosis not unlike expertness in games. At some stage the games expert seems pretty well sure to attempt an analysis of his expertness, and then he will write things like the tortuous books on golf, or cricket, or lawn tennis. The elaborate post-mortem examination gets cluttered up with detail which has minor importance, if any at all, at the living moment of performance. And this may be more than an analogy, for there is something about prognosis identical with the technique of the surgeon. Sir James Paget, speaking of William Fergusson, referred to his hands, "which were so strong and yet so sensitive, so swift and light," so apt to "clear and prompt invention." There is something execu-

tive about prognosis that seizes, so to speak, the chance of the moment. It is observation of direction, and what that is still remains a psychological puzzle as yet unravelled.

There are great differences between extrapolative thinking in mathematics and the physical sciences and extrapolative thinking, of which prognosis is obviously an example, in biology and medicine. In the former the elements which have to be used are known; their properties are known, too, and the relations of those properties can be defined at the beginning and do not change. But in the latter the elements which are to be used must be picked out of a lot of others, all of which may at first seem to have an equal status. We cannot enumerate the properties of the elements completely, and especially we do not know at the beginning what are their relations. We have to inspect instances, and, basing ourselves always upon agreements and differences, reach an issue through an appropriate use of these.

However, prolonged extrapolative thinking does, of course, take place in mathematics, the main difference perhaps being that in the mathematical case the issue is more or less defined to begin with, whereas in medical prognosis it has to be extracted from the direct evidence just as much as the items of evidence which are going to lead up to it. As is well known, Poincaré and Jacques Hadamard, in particular, have made penetrating studies of mathematical extrapolation. There is one suggestion of Hadamard's which seems to me to be particularly interesting. He is discussing Barnard's statement that "those who have an excessive faith in their own ideas are not well fitted to make discoveries." He says that this is not very important in mathematics, because good mathematicians when they make errors soon perceive and correct them. "As for me, I make many more of them than my students do; only I always correct them so that no trace remains in the final result. The reason for this is that, whenever an error has been made, insight—that scientific sensibility . . . —warns me that my calculations do not look as they ought to."

Can this suggestion be taken over directly into the biological field of medical prognosis? If we do this we have to suppose that the directional character which is always present in vital evidence can be known, or perceived, only, or mainly, through a greatly enhanced sensitivity to wrongly assigned directions. I see no reason to doubt that this does happen, but that it is the whole story, or even a main part of it, seems exceedingly unlikely. I know of no outstanding recorded instance of medical prognosis in which the final verdict was reached through the progressive rejection of all possible, or even of a large number of possible, errors. What, then? Are we reduced to the view that the directional character of evidence gathered from the examination of a patient is immediately perceived, or known, by some process of immediate insight or intuition about which nothing more can be said?

I believe that the whole of this matter could be brought to the test of reasonably controlled psychological experiment. But the experiments have not been carried out, and all that I can do now is to make certain suggestions in an extremely tentative manner.

No medical prognosis is achieved on the basis of a single examination. When this appears to be done it is only because the physician has already a fund of information, based upon other examinations, about the signs and symptoms which his direct observation has identified. Moreover, the symptoms and signs are never seen in isolation, never merely enumerated; they are seen as carrying trails which run off into the contemporary life setting, and in most cases also into the course of individual history. This being so, it seems to me very likely that prognostic thinking has fundamentally the same character as the thinking involved in scientific discovery. This is a refusal to accept apparent differences at their face value, and a capacity to probe beneath them in order to find out whether there are significant likenesses. In the case of the prognosis, the observation and information made or used are sections of

an individual life stream ; in the case of scientific discovery they are more commonly fields of fact or of knowledge that are to a more superficial examination separate. In the first the resemblances are essentially of changing phenomena in temporal process. In the second this is less often the case. The essence of both is a persistent search for uniformities and likenesses where differences abound. Success in both is difficult and somewhat rare, principally because our human apparatus for building up factual observation and knowledge makes the appreciation of differences among similarities relatively easy, but of similarities among differences relatively hard.

If this is even broadly along the right lines the main reason for one thing which will impress itself upon everybody who studies the history of medicine will become more clear. All the great doctors, especially the clinical physicians, have been men of wide and varied interests and have used them all. They have put their need for this in different ways according to the different times in which they have lived, but it is a theme that crops up again and again. "The doctor," said Paracelsus, "must be a traveller because he must inquire of the world." Osler was for the peripatetic professorship, the quinquennial rejuvenation by return to hospital and laboratory, the bedside library by no means to be restricted to medical writing. Only the man of many and related interests is likely to find those uniformities in nature and in human life which he must use to fill up the gaps of observed and recorded evidence, in ways that pass unnoticed by the mass. This process of bringing together fields or items of knowledge and observation which hitherto have been unrelated is almost always the first step of discovery, especially in biology. Since in a sense every exercise in medical practice is a voyage of discovery, the vital value of varied interests is never to be overlooked.

5. The Conclusion

I have said that thinking is the expansion of evidence in accord with the evidence, to fill up the inevitable gaps in evidence. The gaps are so filled that either at once, or through a series of steps, a stage is reached beyond which all persons who accept and remain loyal to the evidence must reach the same objective issue. Since all medical science and all medical practice claim a direct evidential basis, and since our human mechanisms for dealing with evidence cannot avoid fragmentariness, thinking has its essential place in absolutely every medical exercise, whatever its level, and whatever its generality or individuality. There are some activities required in everyday life, especially probably in much current industrial practice, where all that is required is simply to treat each particular case as it comes along as nothing but a fresh instance of an already defined class of cases. Then the decision, and the resulting action, if there is any, can fairly be called automatic or mechanical. If there is any genuinely routine diagnosis it has this same character, but it is doubtful whether there ever can be, and so the physician can never get away from the challenge to think.

But the *nature* of medical thinking does differ in different instances. In so far as routine diagnosis is approached the success of the whole thing depends upon the identification of similarities combined with a resolute refusal to neglect differences. And it is the latter that is apt to be difficult.

Once the function of the observed differences becomes clear, either for the purpose of individual diagnosis or for the general development of medical science, the first step is usually to supplement the deficiencies of direct unaided observation by the invention and use of instrumental recording. Experiment comes upon the scene, and must play a greater and greater part as time goes on. But in first-hand observation the difficult thing is the identification of likenesses. So inherently difficult is this that once experiment starts it inevitably develops in the direction of limited range and specializations which separate one branch of knowledge

or topic of exploration from another. Practically all biological discovery, however, whether it concerns the individual instance or the establishment of general principles of treatment and explanation, rests upon the combination of different specialized fields of evidence. Thinking then is apt to be effective only when the thinker has many interests and is able to combine them. Always the watchword or slogan is "Search for likenesses, especially in unexpected places."

All medical phenomena have a future as well as a past, and their significance at any stage is as much determined by what lies ahead as by what has already happened. When this becomes dominant we have prognostic thinking, the mechanism of which is still hardly at all understood. Perhaps it depends upon some unusual sensitivity to those lines of exploration which are not likely to be very fruitful, or perhaps there is something more positive about it : the direct identification of lines of interpretation which will reach an issue. We call this insight. But it may be a capacity to use memory and stored experience, and at the same time to stay loyal to direct observation ; so that likenesses which are embedded in a mass of differences and very likely to escape attention are given fair weight. If so, insight, intuition, wisdom, good judgment, are as analytical in their nature as any other thinking processes.

The doctor, like any other thinker, is all the time using "signs." The items, and the established principles, of evidence are used to carry him into the gaps in evidence. If we could know what these signs are we should be in a better position to understand the internal mechanism of thinking. And if it turned out that there are signs which the doctor must use, and which are not used much or at all anywhere else, we could perhaps say whether his thinking is peculiar in its internal mechanism. This seems very unlikely. It is certain that the signs need not be words at any level of medical thinking. It seems likely that the medical scientist and the physician can use effectively any, or all, of the signs open to any other person : words, or images, or perhaps immediately apprehended "directions." Until we know a lot more about this the attempt, not uncommon nowadays, to be dogmatic about the internal, particularly the central nervous, mechanisms of thinking, whether for medicine or for any other field, seems to be premature.

A recent account published by the National Joint Industrial Council for the Rubber Manufacturing Industry summarizes the accident experience of a considerable part of the British rubber industry from January, 1950, to June, 1952. In this period more than 26,000 reports of accidents were received ; they varied from those causing trivial injuries to those with disastrous results. The authors of this enterprising study have not only shown how accidents arise in the rubber industry but indicate how wide and useful may be the application of similar methods of investigation elsewhere. They emphasize need for accuracy in classification : far too many accidents, many of them severe, come under the general heading "Other Cause." A high proportion of the accidents were found to be not specific to the conditions of the industry and its machinery but to be in two groups—"Persons Falling" and "Falling Material" ; these account for 40% of the serious accidents. Some of the figures, however, are of questionable value. For instance, the fact that more men than women were injured in certain types of accident is of no interest unless the numbers of each at risk are known. One practical result of the survey has been a substantial drop in the frequency of accidents in the industry. The important role of the foreman in keeping workers alert and careful is emphasized. The authors state that "goggles can give positive protection, and the men who need their protection really must accept them." Unfortunately, they do not. Perhaps only legal sanctions and fear of dismissal can make rules observed, though much can be achieved by education, competition, and good morale.