Research and Thinking Processes

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I am indeed honoured to speak on this very great occasion when a vast drive of energy towards physiological and medical research has just been enshrined in a beautiful and functional new building—The Charles H. Best Institute. Largely I know why I have been asked to speak, a diabetic myself rescued from death by the early researches of Banting and Best and the discovery of insulin, and partly as a representative of the British Diabetic Association and now of the International Diabetes Federation. In their name I offer our homage to this new achievement in Toronto. But what else, I ask myself, have I to say worthy of this occasion. Certainly no important new discoveries in laboratory medicine; no novel clinical facts. But I should like to lead your attention to certain considerations of thinking processes in research which interest me. I have had no systematic education in philosophy and its nomenclature and shall have to try to describe my ideas in the simplest of words.

The first question that interests me is whether there is any difference between common sense and scientific processes of thought and if so what is the difference? I am simple enough to believe there is a common mental process behind all our thinking and I shall try to analyse what this is. I hope you will not find this so elementary as to be unworthy of this great occasion.

When our attention is occupied by a thing or a happening which presents itself to our senses, our minds put it into place, or in other words assess its significance. In this "thought" really asks what are its relationships, what is behind it in the past and may ask what it will lead to in the future. We are observers in the present of an event or happening in time, which commenced in the past before our present awareness of it and leads onwards to a future event which again we may observe later. When we think, our minds ask into what sequence of *cause and effect* does the happening fall, cause being the earlier phase of the effect we observe and this is the essence of thinking processes. If the fact is nothing new and falls into previous experience, it does not occupy our thought for long and is accepted as a readily explained and commonplace sequence of events. Most people receive and accept the fact relationships that constitute common sense thinking not only from their own experience but from the heritage of accepted experience of their parents, their teachers or the social group in which they live. So there is a ready explanation and acceptance of the ordinary facts of life giving rise to little thought. It is only the unusual, the unexplained happening that gives rise to questioning thought and perhaps most minds shun such an enquiring effort into cause and effect sequences. Let us take a simple example of the above processes. Our door opens, we look for the cause of this happening and expect from experience someone to enter. No one does and so we look further for an explanation and find the cause to be a broken latch. This effect in the present will be a cause of draughts in the future. Thus the sequence of cause and effect goes on and we think in no other way than in these time-related sequences—"the way things run." I am not concerned here with the emotional and instinctive feelings which rule our motives, often subconsciously.

The process of learning cause and effect relationships or associations starts early and indeed is the basis of all experience and of education. A child burns its finger in a bright heat and, connecting the two for the first time, learns that flame is the cause and pain the resulting effect. By 5 or 6 years the questioning process is rampant and every new experience in many children leads to an irritating why, why? The puzzle of a lock to a boy, a complicated mechanism to an engineer, an abstruse problem to a mathematician or philosopher, the problems of how nature works to a physiologist or a new drug to a medical scientist, all give rise to the same cause and effect search for a logical explanatory answer. *Post hoc, ergo propter hoc* is the natural trend of all thinking, though this may have its dangers in science from too easily accepting the seemingly obvious. Whether this inevitable process is imposed by the anatomical and physical nature of the human brain I shall not discuss here. So true thinking results in an apprecia-

Presented at the opening of The Charles H. Best Institute in Toronto, September 17, 1953.

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tion of causal relations from a sequence of observed facts and the drawing of a correct inference or conclu-- sion therefrom—"putting two and two together." A true conclusion can be drawn quite correctly from the quite simple observations and experience of everyday life and this is the essence of common sense. And the more a conclusion is checked by non-contradictory observations, the more likely it is to be true. The scientific process of thought does not differ in nature from common sense thinking but science sets itself more complex problems than the simpler ones of everyday life. Experimental physiology and medicine probes into the hidden secrets of nature by increasingly complex techniques and must demand a multiplicity of agreeing measurements and controlled observations before a sequence of cause and effect is accepted as true; before a general law of relationship is established. The difference lies in the rigor of proof required.

A consideration of weather problems may usefully illustrate the above ideas on simple and complex relationships. A Londoner, simple-minded in matters of weather at least, gets out of bed on Bank Holiday, sees clouds in the sky, concludes that it will rain, cancels his outing to the seaside and turns to sleep again—a too simple inference that with clouds it will rain. This contains an element of truth but is founded on only one observation and too little experience of such a complicated matter as the changeable English weather. The shepherd, more experienced in weather, asks himself, is the air cold or warm, is the wind north or south, and from these added observations can usually decide whether his morning clouds mean rain—ordinary common sense thinking. Nowadays the more knowledgeable ask for more facts, the height of the barometer, its rise or its fall, before they draw conclusions. And the weather expert, be he amateur or professional, wants to know the barographs and other indications such as are now published in the newspapers before he forms his opinion. His conclusions are founded on a wide assemblage of factual observations from weather stations and ships widespread over thousands of miles, far outspanning his local observation of clouds and winds, but controlling them. And from years of observation of such facts and analysis of their cause and effect relations, patterns and general laws of weather sequences have been built up by what is called induction into the science of meteorology. Its main laws and generalisations have become relatively simple, the patterns of low and high pressure systems, cyclones and anticyclones, and can be learned in an hour or two by a senior schoolboy. Knowing and applying these general laws, by a process known as *deduction* to our own local observations of the barometer and the direction of the wind, we can make a far more accurate assessment of the weather that immediately concerns us than were we ignorant of these *general* laws. So far the science of meteorology can go: and the above is a general illustration of the methods and purposes of science, to build up from observed relations of facts, general laws and principles. But when we ask for absolute explanatory knowledge of why a low pressure system starts in the West Indies and travels to sweep rain over England from the west or why high pressure systems pile up in Siberia and push a different type of weather over Europe we get no clear answer. There is as yet no *absolute* or fundamental knowledge in the science of meteorology. And this is the point where research steps in to probe into the unknown and to explain it. The same applies to all growing points in every science, in physiology and in medicine. There are some absolutes and ultimates in scientific knowledge but usually they are limited, such as Boyle's Law. More often when we have solved one problem, and this is very true in biology, the answer only leads to other questions. The temporary ultimates of science rapidly become penultimates.

MENTAL QUALITIES IN RELATION TO RESEARCH CAPACITY

Probably an enquiring mind is the special quality needed for research, by which I mean original research and not merely the hack work of the technician. Let me first try to analyse the mental processes on which this depends.

There seem to be two main attributes involved in human cerebration, the power to memorize facts and the power to correlate them. Whether the widely differing capacity of different individuals in these two directions depend on the number and quality of brain cells to store facts and on inter-connecting fibres to correlate facts and experience is not pertinent to this paper although it is a temptingly simple illustrative view. Certainly these two qualities can be widely divorced. We all know people with wonderful memories who are intensely stupid. We all know students who can write wonderful examination papers from memory but who are lost when they have to apply this to practical problems involving reasoning and correlation of thought. This is all too obvious in clinical medicine where the mere memorized store of text-book knowledge cannot be brought to bear on varying problems of human disease which seldom fit into an exact academic picture. The pure memorizer is lost. Then there are the "intelligent" and the "intellectual" types. When dictionaries are consulted, we find that the two words are similarly derived and defined, but to me there is an enormous difference of meaning, pertinent to our enquiry into the special quality of the research type of mind. After all we can say that a dog is intelligent, but never that it is intellectual.

To me intelligence connotes, apart from a good or poor memory, a quick, immediate and right mental response in thought and action to presenting problems and circumstances, based mostly on rapidly applying previous experience and knowledge to the problem in hand. The intellectual person, and he is usually intelligent too when occasion demands but not necessarily quick, has special qualities of deeper and more probing thought. He is always enquiring more profoundly into accepted conventions and explanations. He has wider co-ordinations, delves deeper into accepted explanations, sees new relationships in disconnected facts, has a constant 'why' in his adult mind, a questioning of accepted convention of thought and ideas which do not seem to fit into facts and their relations as he sees them. He is a constant questioner and often a rebel both in society and in science. This is the essence of the enquiring mind, the intellectual mind. It criticises, freshly relates and reco-ordinates relationships which are accepted by merely intelligent minds: it makes a relating generalisation between two previously unco-ordinated phenomena. Often such inspirations, shall I call them, come easily like spontaneous sparks and such I think must have been Banting's about his insulin research. But when this type of mind sets itself deliberately to such a new critical synthesis, this is the hardest scientific task a brain can impose on itself in my opinion.

THE REQUIREMENTS FOR GOOD RESEARCH

Granted the enquiring mind whose qualities I have tried to analyse, how does a worth-while new idea in research arise and what is needed to bring it to fruition? The immediate stimulus usually arises in one of two ways: either from a fresh critical analysis of accepted theory, often arising from a new discovery in a related field, or from a new observation, perhaps a chance one, suggesting that the accepted explanation of a phenomenon cannot stand. A worth-while new idea can hardly arise in a mind not fully versed in the subject. If it does—and some have arisen in the minds of critical young students, fresh minds on an old problem—the next step is a thorough search of all published work which may be pertinent and often found in several languages. This may take months and may result in the discovery that the idea has been thought of and tried out before. I remember in 1924, when trying to produce food tables for diabetics, spending months in finding out what figures existed for carbohydrate foods in various tables and discovering that they varied by as much as 300 per cent. This realization was the starting point which led to a re-analysis, first of carbohydrate by McCance and myself and then of all foodstuffs by McCance and Widdowson for several years.

After this comes inevitably to the enquiring mind a fresh explanatory hypothesis, an informed guess on a new possibility or probability, which is as often wrong as right, at least in my personal guesses at how natural processes should work. The ways of nature are seldom such as my logic would expect. Next comes the planning of observations or experiments, often complex and technically difficult, to prove or disprove the idea. In this the investigator must be fair and open minded and guard himself against a bias in favour of the darling child of his own imagination. It is all too easy to plan experiments to prove a point and all too natural to gloss over a few discordant results or to weigh the evidence too much in favour of one's hypothesis. This is the lawyer's point of view, I often feel, to prove a point whether it is right or wrong in favour of the client and an idea. The scientist, and especially the young with too vast an enthusiasm, should be aware of this danger. A false finding, which appears right by his own proving, may perpetuate error for years and has to be laboriously disproved. He should therefore be made aware and keep himself aware of the necessity of the strictly controlled experiment to curb the bias of his enthusiasm. And he should know enough about mathematics to apply, or have applied, the science of statistics, first in the planning and later in the criticism of the validity of his findings.

Then there are other more material things needed. A pair of good hands with delicate touch is as necessary as clear mental qualities for performing the many fine techniques needed; the delicacy of touch required for surgery on smaller animals is infinitely more exacting than in human surgery. Good technical facilities in a well equipped laboratory are equally essential nowadays, for the simpler apparatus of the past has become inadequate for most research projects. These now involve the use of all kinds of electrical and other mechanical apparatus of extreme complexity—or so they seem to me who can hardly name, and much less understand, them.

Another requisite, for the young at least, is critical guidance and supervision by a wise and kindly director. This is usually necessary from early criticism of the idea behind the research, right through the difficulties that commonly arise at all stages in an investigation, to the final preparing of the results in a logical and well written paper—perhaps the hardest part of all in which superior guidance and strict criticism is necessary. Brief, clear and good writing is a stumbling block to many. One practical point of value which I learned from bitter experience myself is the wisdom, when results are beginning to shape and long before the end of the investigation, of writing up the results and criticising the tentative conclusions. One usually finds that some unforeseen point has been omitted which can easily be included in the investigation at an early stage but is difficult to incorporate later when the experiments are nearly concluded with some uncertain point omitted. It is depressing and sometimes impossible to restart at the final stages. Another point in writing a highly specialized paper or giving a communication is to start with a brief and simple explanation of the background and aims of the investigation to those readers or listeners who are unversed in the subject. I have often sat bemused and ignorant of the meaning of a specialized communication when a few general explanatory sentences would have removed muddledness and boredom.

Furthermore the investigator should be able to work in a state of financial security. Rich he will never be unless through some of the paths of industry, but none should be overburdened by the financial strain in meeting reasonable family responsibilities. This remark in no way belittles the vast importance of enlightened industry in supporting fundamental research in so many fields. But institutes require large resources to fill their establishments with active and satisfied workers. Many a beautiful library has been built and no money left to buy books!

ERRORS AND WASTED ENERGY IN RESEARCH

In reading or dipping into the thousands of articles published each year on my own and other subjects in biology and medicine, I am depressingly struck by the triviality and pettiness of all too many. Perhaps a major cause of this is the importance of having some research publications to lead to promotion and success in academic posts, often mainly teaching ones. Even in appointments for clinical posts in Teaching Hospitals, the background of publications often outweighs more important teaching qualities: "I must do research and publish a thesis to make my B.Sc. into a D.Sc., and so get on in life."On the other hand, early activities and discipline in research projects are of great value whether the future leads mostly to teaching or more to research. I, at least, find it impossible to read all papers even on my own subject. How does one judge whether an article is worthy of serious study? Partly from the critical calibre of the journal in which it is published; partly from the centre from which the publication comes and the acknowledgement to a director who is known to be critically concerned; partly to the opening paragraph and largely to the summary. Many papers fall by the wayside in satisfying these criteria. I think it is essential that ideas for a piece of research should be rigorously criticized from its inception to its end. This should not be so harsh as to exclude any new idea with a possibility of eliciting a new truth from a different angle. I have often wondered what would have happened to Banting's original idea about insulin if Macleod had refused his support. However, I feel certain Banting would have got on with it, somewhere, somehow. But would he have found another Best needed for the duet of success?

Poor technique and poor facilities lead to much error and poor work and need not be mentioned again. I should like to repeat my previous warning against emotional bias in favour of one's own hypothesis which leads so many investigators to false conclusions.

Finally, from what we have seen of this noble new institute and from what we know of its ideals and directorship we are sure that this new centre has all the qualities, material and spiritual, to make Toronto even more inspiring in physiology in the next than in the last half century.