Balint, the doctor, and the fear of being unscientific

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The scientific method is a potentiation of common sense, exercised with specially firm determination not to persist in error if any exertion of hand or mind can deliver us from it.

Medawar and Medawar (1977)

There is no science, there is only the hope of a science.

James (1890)

SUMMARY. The current debate on the contribution of Michael Balint's work to general practice has been initiated by Sowerby's (1977) lengthy critique.

Sowerby's arguments, however, depend on one particular definition of science, simplify some complex issues, and have rigid and restrictive qualities. I give some examples to illustrate this.

Secondly, Sowerby's definition of the science of psychology leads to an intellectual separatism which Balint sought to reduce. The alternative diagnosis of 'depressive illness' is neither more helpful nor precise.

Finally, criticisms of Balint seminars which Sowerby perceives as dangerous are challenged. I argue that Balint's approach in verifying and refuting hypotheses in the face of prospective observations and evidence was truly scientific.

Introduction

THIS topic has been chosen for exploration because I am keen that doctors in their dealings with patients should not "persist in error". Human psychology is a young discipline and our knowledge of patient-centred medicine lags significantly behind illness-centred medicine.

The problem of what body of knowledge is to be called 'scientific' and what 'non-scientific' is not new. It is also not simply of abstract theoretical or semantic interest since its resolution has implications for training, research, and research funding. I embark on these stormy seas fully aware that I am *not* a hard scientist, a general practitioner, or a philosopher of science. I write as a psychoanalytically trained social scientist interested in and directly involved in Balint groups and the application of psychoanalytic method and insights. My exploration, therefore, has an additional practical basis. This is connected with problems of 'unlearning' and 'scientific overkill' in the initial training of doctors and the ensuing problems for Balint type experience (Barnett, 1978).

1. Sowerby and 'Science'

Sowerby (1977), in this *Journal*, followed Karl Popper's views on the status of psychoanalysis, and made a reasoned attempt to dissociate Balint's ideas from medical 'science' and accord them the status of 'myth'. To do this Sowerby leans wholly and heavily on Popper's concept of 'falsifiability' in which it is argued that unless hypotheses and theories can be stated in such a way as to be refutable they cannot be called 'scientific'.

I argue that Sowerby oversimplifies some quite complex issues. For example, he premises his argument by simply and tidily equating 'science' and 'logical empiricism'. He gives the impression that the concept of 'falsification' is valid, unambiguous, and accepted by all concerned, when even a cursory glance at the literature suggests that this is not so (Farrell, 1964; Martin, 1964; Seaborn Jones, 1968; Lakatos, 1970; Kelk, 1977). Such apparent simplicity and clarity then enables him to mount a fierce argument against the Balints' work on the grounds of confusion, myth-making, or acting like novelists.

According to Sowerby, psychoanalysis and its application to medicine (Balint's 'whole person' approach) is a pseudo-science replete with 'metaphysical' hypotheses which, unlike scientific hypotheses, are 'untestable' and do not obey the rules of true empiricism. Now, both eminent scientists and philosophers are by no means agreed as to what science is or what are its definitive rules, as Sowerby would have us believe

(Guntrip, 1978). It is true that Sowerby can find support in such authorities as Popper (1959, 1965), Cioffi (1970) and Eysenck (1978), who take psychoanalysis to task and relegate it in the quest for scientific respectability to the level of astrology and alchemy.

On the other hand we have only to list the various terms used as qualifying adjectives to 'science' to get an inkling of the narrow, rigid, and restrictive quality of Sowerby's usage. Besides 'pseudo-science' we can find 'pre-science', 'young science' (Hilgard, 1970), 'normal' and 'revolutionary' science (Kuhn, 1971), all of which give rise to considerable debate particularly in relation to the biological sciences and psychology.

As one enters further into these discussions it becomes increasingly clear that what is genuinely scientific for one authority is hardly 'science' for another. For example, Popper (1959) states that science is practised by a scientist putting forward a series of statements and testing them step by step. "He constructs hypotheses . . . and tests them against experience by observation and experiment." Kuhn (1970), arguing along historical and somewhat different lines, suggests that what is most frequently 'tested' are "statements of an individual's best guesses about the proper way to connect his own research problem . . . with scientific knowledge". The 'normal' scientist is a "puzzle-solving addict". The scientist's aim is to "solve a puzzle" and "only his personal conjecture is tested". If it fails the test, only his own ability, not the corpus of current science, is impugned. In short, though tests occur frequently in *normal* science, these tests are of a peculiar sort, for in the final analysis it is the individual scientist rather than the current theory which is tested. Now Kuhn (1971) has written at length about this behaviour of 'normal' scientists. He emphasizes that its nature is necessarily narrow, rigid, and well defined, that it is centred on soluble problems reducible to puzzle form and that as an activity it can quickly become an end in itself. What may then develop are communities of scientific workers each with its own 'paradigm' isolated from each other and from the wider community and its problems. 'Paradigm' is used here to mean an accepted body of scientific achievements that for a time provide model problems and solutions to a community of practitioners. A paradigm is an example of actual scientific practice which includes law, theory, application, and instrumentation together and which provides a model giving rise to a particular coherent tradition of research (Masterman, 1970). The practice of 'normal' science may, therefore, become 'a closed society of closed minds' in which falsification of theories is not in evidence, precisely the charge which Sowerby levels against the Balints and their associates.

The nature of science

Sowerby maintains that research, in order to be scientific, must follow the tenets of logical empiricism, that

is, Popperism, positivism, formalism, and pragmatism (Radnitzky, 1968; Barratt, 1976). Thus the researcher must work within the confines of 'objectivity, testability, falsifiability' and concentrate on 'behavioural events' (that is, open to *public* verification) as the only 'true' data.

There is, in fact, a substantially argued viewpoint that attempts to confine scientific exploration to a strict and narrow criterion of validity, particularly in relation to the social sciences, are both outdated and unattainable (Harré, 1972; Wollheim, 1976). It appears particularly unrealistic to prescribe a single set of detailed canons of procedure as applicable to each and all the sciences. An overemphasis on 'falsifiability' has, therefore, come to be criticized as dogmatic and naïve and at least in relation to human psychology to have little bearing on major developments in our knowledge. Even in the physical sciences examples can be given of complex phenomena making anything other than a highly sophisticated 'falsification' approach of little use. Polanyi (1964, 1966, 1968) has described the subjectivity dimension in the pursuit of 'objective' scientific knowledge. He emphasizes the essential ambiguity intrinsic to scientific 'facts', showing clearly how in the great scientific controversies the two sides did not accept the same 'facts' as facts and still less the same 'evidence' as evidence. Kuhn (1971) gives a particularly vivid example of this phenomenon in 'normal' science.

> "An investigator who hoped to learn something about what scientists took the atomic theory to be asked a distinguished physicist and an eminent chemist whether a single atom of helium was or was not a molecule. Both answered without hesitation, but their answers were not the same. For the chemist the atom of helium was a molecule because it behaved like one with respect to the kinetic theory of gases. For the physicist the helium atom was not a molecule because it displayed no molecular spectrum. Presumably both men were talking of the same particle but they were viewing it through their own research training and experience."

On the topic of falsifiability Polanyi gives a striking illustration of the complexities involved. Suppose, he says, we consider the hydrogen atom as described by quantum mechanics. It presents us with a map which assigns to every point of infinite space a number which is a function of its distance from the nucleus. This number denotes the probability of finding the electron of the hydrogen atom at this particular point and likewise at any other point having the same distance from the nucleus. Now (says Polanyi) the simple reason why this statement cannot be contradicted by any conceivable event lies in the fact that it admits that the electron may be found or not found at the designated place on the specified occasion. This, he argues, is like the story of the dog owner who prided himself on the perfect training of his pet. Whenever he called "Here, will you come or not!", the dog invariably came or not. But that is exactly how electrons behave according to probability.

Whole person medicine

How, then, are we to view Sowerby's charge that since the work of the Balints is 'irrefutable' it is 'unscientific'? It must be understood from the outset that the research objective was to investigate 'whole person medicine' in general practice. The concern was with aspects of medical thought in need of revision, that is "the pathology of the whole person". The doctor was then led to reconsider the *person* as such and not to rest content with understanding the bits and pieces, dead and alive, that were rightly or wrongly the main emphasis of his initial medical education.

Balint's 'scientific method' therefore needed to be appropriate to his pioneering investigations of the doctor/patient relationship. He makes it clear that he starts from the psychoanalytic premise that for the doctor's observations of the patient as person to be valid he must give some attention to *himself* in interactions with the patient. It seems most unlikely that he would have gone far if encumbered with the tenets of logical empiricism. Such fetters would undoubtedly have led along sterile paths and blind alleys. Instead he chose to behave fearlessly as had Freud before him, linking careful observation with creative imagination and designing and evolving a method to collect the necessary data.

According to Hilgard (1970) his work can be recognized as an early stage of a 'young' science developing through stages from naturalistic observation to detection of areas of lawfulness and towards more comprehensive theories. From such observations he derived inferred variables (for example, 'Apostolic Function') and in principle was doing what any conceptualizer of science does. He was aware of the dangers of his use of metaphor but does so as a preliminary step (Frenkel-Brunswick, 1954), awaiting better anchored concepts and further investigation (cf. his remarks on being 'novelistic' (Balint, 1964) and of the 'poetical atmosphere' (Balint and Norell, 1973) of his thinking). The complexity and novelty of whole person medicine suggests that an approach limited to testing separate propositions one at a time would at best be premature and at worst trivial or irrelevant.

Revolutionary science

The first steps of the Balints' research into the doctor/patient relationship in the early 1950s marked a new and ambitious venture. The development of the work over 25 years or so comes near to bearing some of the characteristics of what Kuhn has called 'revolutionary science'. In this kind of science a whole, well established body of thought is challenged and overthrown. It often occurs in response to a 'crisis' brought about by an incompatibility of 'theories' with evidence. In many ways what was attempted by the Balints and partially achieved resembles Kuhn's description of 'paradigm shattering research'; that is, such work differs from habit-governed, puzzle-solving activity and is a fundamentally upheaving type of research having a truly 'falsifying' quality.

2. Balint and the 'Problem of Diagnosis'

Sowerby's definitive views on 'science' may lead us to narrow and infertile territory in investigating whole person medicine. In fact, throughout his article he seems to refuse to entertain the idea of a genuine 'science' of psychology since the study of Man, he says, is the sole province of the arts. He therefore concentrates his attention from the outset on the problem apart from the patient and the patient apart from the doctor.

This separatism can be contrasted with Balint's whole objective in his work which is implied by the title of his classic book *The Doctor,* His *Patient and The Illness* (1964) (my emphasis). It may be suggested, therefore, that Sowerby's main concern is less with 'science' as such and more with 'scientific respectability'. He understands the psychoanalytic influence on medicine as a fundamental challenge to respectable medical practice and adopts similar opposition as orthodox medicine took to Freud (Eissler, 1965). The nature of the threat is well illustrated by his adopting Popper's use of the term 'myth'. The *Shorter Oxford Dictionary* defines 'myth' as a purely fictitious narrative, usually involving the supernatural—that which has no foundation in fact.

What seems to be 'feared', therefore, is that Balint's psychoanalytic ideas are dangerous 'fantasies'; that Balint has no respect for the strictures of philosophers of science and their rulebooks (that is, their vision of 'reality'), and that what may be neglected in the Balint approach are the real accomplishments in 'scientific' medicine of the last 100 years. By becoming encumbered with Balint's fantasies and 'fictions' and the paraphernalia of the 'unconscious', the doctor will become divorced from his scientific potency.

This fear of what the likely consequences are for the 'unscientific' doctor is nowhere better illustrated than in Sowerby's comments on Balint's descriptions of case histories from the general practitioner seminars. Here he takes Balint to task for faulty and unscientific diagnosis.

Sowerby's alternative diagnosis

In 18 cases Sowerby favours a 'blind' diagnosis of 'depressive illness' and he rests contentedly with it since it is refutable. He does not, however, discuss in what way such a diagnosis will lead to rational therapy for the patient (for example, case 2) and is presumably happy to rely on 'puerperal depressive illness' and 'spontaneous remission'. In other words the doctor need do very little. Once he has applied the label (as Balint says) he, the doctor, at least feels better, no matter how 'superficial' and 'incomplete' the diagnostic tag is. Balint's aim to consider the utility for doctors of a 'comprehensive' (that is, 'total') diagnosis and a 'comprehensive' treatment plan, to differentiate surface and 'deeper' levels of

clinical investigation is likely, says Sowerby, to turn medicine into literature and, by implication, doctors into poets. Instead he concludes that doctors must stay on safe and sure scientific ground by providing diagnostic tags such as 'fixed neurosis of poor prognosis'. How scientific and practically valuable is this approach?

Bearing in mind the quotation from Medawar and Medawar (1977) at the beginning of this paper, we may ask several questions of Sowerby's use of classical psychiatric diagnostic procedure. Is the label 'depressive illness' likely to be reliable? Is it not vague rather than precise? Will it lead to rational treatment plans? What will be the possible undesirable side-effects on doctor and patient labelling?

Balint (1964) himself has dealt with some of these questions in characteristic style: "The diagnosis 'neurotic' we all know can be made by anyone and it provides the doctor with hardly any indication of what his next step should be. It is a kind of magic name only, and not a diagnosis in the proper sense . . . neurosis is even less of a diagnosis than pains, constipation, etc." Acceptable diagnostic and treatment procedures always have to consider the problem of 'side-effects' and we may likewise examine Sowerby's diagnostic approach from this point of view. Rosenhan (1973) in a thoughtful and provocative article has examined the problem of labelling, and its side-effects on all concerned.

He describes an experiment in which eight 'sane' people gained secret admission to 12 different hospitals. Although this setting is in marked contrast to that of the doctor and his patient the investigation is relevant because it convincingly demonstrates the effect of 'diagnostic tagging' on those responsible for it or directly related to it. Following the use of 'schizophrenic' the label itself profoundly coloured other people's perceptions of the patient and his behaviour. In particular, says Rosenhan, "the perception of his (the patient's) circumstances was shaped entirely by the diagnosis". For example, the pseudo-patients were deliberately engaged in writing everything down. The nursing staff perceived this and interpreted it in terms of the label; that is, the writing was seen as an aspect of the pathological behaviour. Thus 'patient engages in writing behaviour' becomes a series of conjectures (writing in a ward—disturbance—compulsive behaviour—schizophrenia). This work illustrates well the tendency for the psychiatric label (scientifically derived or otherwise) to have a life and an influence of its own. Thus once a doctor forms an impression that the patient suffers from 'reactive depression' he will expect her to continue doing so and then when no depressive features occur, she will be considered 'in remission'.

In all events he can remain supportive but fairly distant from the person. It is as if, having carefully captured the 'disease process', we have lost the 'person' (Kendell, 1975). What is even more worrying is that, though such a 'psychiatric diagnosis' may in theory be falsifiable, in practice it is rarely admitted to be in error.

3. The critique of the Balint seminar

In the last part of his paper Sowerby focusses his concern on the Balint seminar as a method of training general practitioners. A method, he says, 'openly' (as if it should be kept hidden!) derived from psychoanalysis, and it is this 'frankness' or openness that he finds so worrying. The doctor who is not a good 'talker' is, says Sowerby, under fire and unfairly so. My own experience of co-leading such a seminar suggests, however, that this observation is only a half-truth, for the seminar is *par excellence* a place to think, feel, and silently deliberate on one's own practice, as well as to talk. Sowerby perceives the seminar as dangerous because a powerful leader may thus unwittingly impose some theory on the group, and because it is a place to perpetuate intellectual confusion, a place where the doctor is 'fettered' by irrefutable conjectures. I invite him to sample a seminar and its atmosphere. It seems to me that doctors are encouraged and challenged to be better 'observers' and 'listeners' rather than 'talkers'. It is true that the case presentations often contain the doctor's confusion, yet this confusion is recognized as inherent in much general practice. It is brought into the light, held, and examined and used to help understand the doctor/patient relationship and to reach a rational (rather than rationalized) decision about treatment. The doctors are certainly not asked to forsake their scientific heritage. The work of the seminar stimulates the doctor to practise meticulous observation, the careful testing of hypotheses, a continual self-correction, and a concern with empirically derived data.

In considering Balint's contribution to 'science', it is important to consider the growth and the development of his ideas and concepts over a span of 25 years. No-one who has perceptively read his last 1970 statement *Research in Psychotherapy* (Balint and Norell, 1973) could, I think, easily conclude that he works and writes like an advocate defending a 'cause' rather than a scientist searching for truth. While he remains entangled in conceptual metaphors there seems to be no doubt that he has proceeded in the research in the manner of one who is empirically anchored, in verifying and refuting hypotheses in the face of further observations and evidence. The doctor as the 'great detective' is refuted and the doctor's capacity to 'tune in' is hypothesized and further examined. His discussion of a crisis in the research in which 'everything was challenged' is not the method of the dogmatist or the spinner of myths. What seems to have been the theoretical structure here is what has become fashionable in psychology in the last 20 years, that is, the model as a replacement of the theory.

In the words of Boring (1963): "You see how well you can get your data to fit, perhaps adjusting the model to make it fit better. If the fit is good, you have a good summary of these data and then you may use the model to predict other data and test it empirically. If the

prediction is borne out, the model gains in dignity and importance."

Conclusion

It has been argued that attempts to apply narrow and rigid rules as prescriptions for scientific endeavour are counter-productive. This is particularly the case in the young, developing field of patient-centred medicine. The work of Michael and Enid Balint and their associates approximates what Kuhn has described as 'revolutionary science'. This research seeks above all not to persist in simplistic, rationalized and erroneous approaches to the doctor/patient relationship. I have tried to demonstrate that the fear of being unscientific can lead to unscientific behaviour on the part of both doctors and scientists.

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