Balint's contribution to general practice

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Introduction

I AGREE with Dr Sowerby (1977) that the time to reappraise the contribution of Michael Balint is ripe and I would like first to examine Dr Sowerby's criticisms from a rather neutral standpoint and then to take a more personal view of Michael Balint's influence on general practice.

Dr Sowerby's criticisms

Dr Sowerby has four main criticisms which he maintains are rooted in the false assumption that human behaviour is scientifically understandable in theoretical terms. To support this he draws on Karl Popper's analysis of the theoretical validity of the ideas of Freud and Adler. In the latter, Popper proved that psychoanalytic theory, as it existed then, was interpretive and not explanatory; in short, it started with a description of what transpired and then interpreted it through a number of conceptual generalizations but it was not able to draw together these interpretations into a causal model of human behaviour except through inductive techniques. Although this was acceptable to the founding fathers of psychoanalysis—as indeed it had been to the rest of the scientific world from at least the time of Francis Bacon—it was not acceptable under Popper's stringent criteria for the definition of the Scientific Method (Popper, 1959, 1963, 1972).

For Popper, the acid test of scientific theory was its refutability, but Dr Sowerby seems to misinterpret Popper. He argues that not only must our theories be confirmed—they must also be refutable. If a theory is refutable, then perforce it must be false and what Dr Sowerby seems to be saying is that objective fact is unknowable, a rather nihilistic though traditional view of science. The alternative interpretation is that Dr Sowerby is repeating the semantic fallacy of 'the exception proving the rule'. The latter expression has through colloquial usage changed its original meaning in which the word 'prove' meant 'test'. Perhaps philosophically this is an appropriate stance but for the purposes of objective science—the study of fact knowable to humanity, and the subject of Popper's deliberations—we need to be able to differentiate between true and false theories. In order to make this distinction Popper chose another semantic technique: he said that the form in which the theory is expressed must be capable of refutation; that is, it must be capable of objective assessment inasmuch as this is within the capabilities of the human state. If such an expression—a hypothesis—cannot be refuted it can be held deductively to be true until any such refutation is made. Popper thus avoided the contradictions of induction pointed out by Hume (1739) and mentioned by Dr Sowerby. He took the view that science is concerned with the perceived world, a change in perspective which was of fundamental significance for the scientific world. It is important to recognize that the spectacles through which problems are seen change from time to time depending on the preoccupations of the scientists of the day. It is only when these spectacles are changed that the world is seen in a different light or from a different perspective, but such a change is revolutionary rather then evolutionary (Kuhn, 1970). We cannot assume that merely because we see life in one way that our ancestors should also have seen it in the same way; the change in scientific interest from final causes to mechanisms in the Renaissance, or the recent development of interest in causality (or prior causes), were as fundamental as any other changes in morality or ethics.

It is really only in the last 50 years that any real interest has been paid to prior causes as a legitimate field of study, and it should not surprise us to discover that Freud and his colleagues were not preoccupied with them. Nor should it surprise us that, within the past few decades, even academics like Balint should have chosen to ignore the procedures and limitations of a relatively new approach to scientific enquiry. Nevertheless, we should recognize that the social and psychological sciences have for some years been concentrating on the elucidation of causal models of human behaviour which would ultimately provide the theoretical basis of what Dr Sowerby holds to be impossible—a scientifically valid theoretical explanation of behaviour. To maintain that something is impossible because we cannot see how it can be possible is to fall into Hume's inductive fallacy, and those readers who wish to avoid this may care to read one of the several texts on social science methodology aimed at this particular problem (Rosenberg, 1968). In attempting to criticize Balint's work it is not legitimate to suggest that facts or perspectives which have only become available at a later date should have been utilized. The only legitimate criticisms can be of the work's justification in terms of its purposes, its contextual appropriateness, and its contemporary relevance. It is from these three standpoints that I want to examine Balint's work.

The basic purposes of Balint's work inevitably revolve round the fact that he was a psychoanalyst. We can assume that this means that he rejected the 'medical model' as an explanation of psychological disorder. Yet it is because of this rejection that Dr Sowerby criticizes him most severely. He apparently does not seem to realize that Balint was interpreting the cases in the particular way selected so as to show the relevance of methods and models other than the obvious medical ones. Dr Sowerby does not think that this justifies a total neglect of the possibility of 'depressive illness'. However, our ideas about depression in the 1950s were very different from those we hold now and the biochemical evidence for 'depressive illness' was not known. Indeed, even today there is serious doubt whether the biochemical sequelae of depression are truly the cause rather than the effect of the disturbance, and there is by no means total agreement within the psychological world that depression is a disease. Not only is Dr Sowerby's contention that Balint should have tagged his cases 'depressive illnesses' an imposition of a later perspective but it is also inappropriate to the purposes of the psychoanalyst. His comment: "I do not criticize Balint or anyone else for not agreeing that depression is an illness: I criticize them for not accepting the possibility that it might be" not only misses the point of Balint's endeavours to introduce a different, *additional* perspective to medical practice but also smacks of the same sort of tendentiousness of the 'flat-earthers' who insisted that Copernicus was wrong to proffer his heliocentric theory of the universe without admitting that he may not be correct!

Balint's philosophy

The point is that Balint did not say anything which was new. What he did was to link the various strands of his argument into his psychoanalytical model to show the relevance and potential value of his own school's approach. Let us look at the three basic strands of Balint's philosophy. The first was that psychological problems are often manifested through physical means and that even physical disease has its own psychological consequences which demand particular attention. This is hardly new; it was one of the basic principles of Hippocratic teaching. So why did he say it at all? Surely there are a number of possible answers. He might have been concerned that the medical model was inappropriate for problems with which he as a psychoanalyst was having to cope—and let us not forget that psychopharmacology in the 1950s was pretty rudimentary. He might have been concerned that the medical profession always sought biological or physical illness so that psychological problems were given low priority or had to be cloaked by physical symptoms to be presented at all. He might have been concerned that psychological diagnoses were the dustbin of medicine; in other words they were what patients had when all the examinations and investigations that could possibly be done had turned out to be negative or contradictory.

The second strand of Balint's philosophy was that the physician is not a neutral uninvolved observer; the patient is not a goldfish in a bowl. He was one of the first people to point out to the medical profession something that had been known to psychologists and sociologists for some years—that any observer biases his observations by the very process of observation. In short, the clinician is integrally involved in the patient's life; their psychological personalities interact in very much the same way as two drugs. The implications of this are obvious. Because of the special functions of the physician, and because he can ignore the usual taboos of society by questioning, examining, or manipulating the patient in the most intimate of ways, the quality of doctor-patient interaction is both unique and potentially psychoactive. This cannot be illustrated by recourse to the 'medical model' and only follows if we are willing to accept the perspective of the 'psychoanalytic model'. Since Dr Sowerby refuses this it is not surprising that he also refuses to accept the interpretation of the case histories provided by Balint as evidence.

The third strand is that the very nature of the general-practice consultation has strengths and weaknesses for the practice of psychoanalysis. Balint believed that special training was necessary to produce a "limited but considerable change in the doctor's personality" so that he could "become more sensitive to what is going on . . . in the patient's mind when doctor and patient are together" (Balint, 1957). Obviously, if the medical profession are to take notice of the then new knowledge of non-verbal communication derived from psychology this has to be communicated in some way. We should not criticize Balint for proposing a form of teaching which was to him the most logical even if there are alternative strategies available to us today. We cannot merely assume that the physician is sensitive to the patient unless we make some attempt to train him. Should we leave it to 'professional intuition', whatever that is, or could we use some explicit method of training? That, of course, depends on what is being taught; if it is merely how to listen and how to seek signals from the patient then this might be a by-product of normal medical education. But clearly Balint thought that there was an additional skill to be learnt. He felt that it should be possible for the clinician to tap the subconscious of the patient—a belief which was taken further by later workers and which culminated in the elucidation of the 'intuitive flash' (Balint and Norell, 1973). However, even this is not new. 'Intuitive flashes', by whatever name, were introduced into formal philosophy by Pythagorus though it is likely that they were part of Orphic doctrines even earlier. Since then, intuition has been fundamental to many philosophers including Bacon, Schiller, Kant and Emerson, to name but a few, and it forms the basis of much modern research into ESP. If this view is justified, and clearly Balint considered it so, then we require techniques to be provided by which students can learn about it.

When Balint first wrote his book these basic principles were largely unrecognized or unheeded by the medical profession. The justification of his work is that his ideas became the focus of a tremendous amount of later discussion and effort; they were fundamental to much of the renaissance of general practice as a discipline in its own right. They may not have been the most appropriate means of achieving Balint's purposes but at a time when ignorance was rife and they were largely experimental in nature how can we say that *contextually* they were inappropriate? Thus, the only real criticism we may legitimately offer is on the relevancy of Balint and his writings. All innovators are doomed to be surpassed by their followers in due course and no one should expect that Balint's original suggestions about technique could not be improved. It is perhaps sad that Balint's main contribution to our discipline has been through his proposed method of sensitizing doctors to their patients' problems. It is probable that this could, and possibly should, have been improved. But what of the principles themselves? As Dr Sowerby has so ably demonstrated, there are those who still refuse to see psychosocial problems outside their own medical model, so Balint is not outdated there. Too many patients are written off as functional x when the actual nature of x is obscure, so Balint's plea that psychological disorder should not be diagnosed by exclusion but by analysis is still very appropriate; and presumably clinicians still interact with their patients to identify and interpret problems.

The only fault I can find with Balint is that he did not make it perfectly clear that he was talking about issues such as those outlined in this critique. The worst thing about 'balintology' is that it appears to suggest that if a psychosocial factor can be identified then this determines the treatment whatever the presented complaint —the "ah, but why did she come with her varicose veins at that time . . . how's her sex life?" syndrome. Everyone has a psychosocial problem and the difficulty is to say whether this is the cause or the effect of the presenting complaint, or just a coincidence. Balint suggested a possible means but this was lost on most of his disciples. Gratuitous psychoanalytical interpretation of patients' symptoms is, after all, quick, easy, and irrefutable—at least by the patient. It also allows the doctor to rationalize any failure of diagnosis or therapy and to justify his neglect of patients' presenting symptoms. But who wins? Balint's aim was to improve general practice. His fundamental point was not the importance of psychological aspects of life or illness but the interactive nature of the medical consultation. His message is as relevant today as it was when he first expressed it, but the distortion of many of his points by subsequent practitioners suggests that the way in which he expressed his message was inept. Balint never suggested that medicine should not concentrate primarily on biological deviation for which the 'medical model' is supremely well suited. But he did suggest that general practice had many of the aspects of pastoral care for which that model was unsuited. In suggesting at least one alternative approach he undoubtedly helped us onto a wiser course for our discipline for, whatever Dr Sowerby may say, the eclectic nature of general practice demands a versatility in thought and in practice. Balint is dead but long live his purpose.

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Effect of saltbutamol in children with wheezy bronchitis

Using the technique of whole body plethysmography, lung mechanics were measured in a group of infants with wheezy bronchitis. Compared with a group of ten normal infants, aged 8 to 43 weeks, previously studied airway resistance and thoracic gas volume were found to be raised. Nebulized salbutamol was then administered and measurements were repeated when it was found that there was no objective improvement. It is concluded that salbutamol may not be an effective form of treatment of wheezy bronchitis in young infants and the reasons for this are discussed.

Reference

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Similar effects were found by N. Rutter and colleagues (*Archives of Diseases in Children,* **50**, 719-722), using salbutamol and isoprenaline in 16 children aged 3 to 36 months—*Ed.*