

# The doctor, his patient, and the illness: a reappraisal

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**SUMMARY.** Psychoanalytical theories seek to explain human behaviour. I believe that they are not scientific, whereas it appears that Balint thought they were. I suggest instead that they are better regarded as myths and part of an artistic discipline. Whereas most of the problems brought by patients to general practitioners can be understood in scientific terms, others can be understood only in artistic terms.

These two terms reflect fundamentally different kinds of problems, and different language must be used to discuss them. Neither the two sets of terms nor the two kinds of problem can be confused without giving rise to error. I argue that Michael Balint came to a false conclusion about the nature of the general practitioner's task, about the way the problems posed by his difficult patients may be identified, and about some of the training doctors should receive.

Balint's main contribution remains. He showed us that scientific skills alone are not enough if we are to understand our patients fully. He also showed us how a descriptive science of human behaviour in the consulting room was possible.

To these insights must be added new understanding. Popper (1963) has provided us with a clear line of demarcation between science and the rest of our knowledge. This idea suggests that general practitioners should reaffirm the importance to them of the intellectual discipline of science. If they wish their understanding and practice to be comprehensive they must also affirm the importance of the arts. What they must not do is to confuse one with the other.

### Introduction

I WISH to criticize the book that has probably had more influence than any other on the development

of general practice over the past 20 years, *The Doctor, His Patient and the Illness*, by Michael Balint. In it Balint reports the results of a study to elucidate the "psychology of general practice".

A group of general practitioners brought to a series of seminars problems they had encountered in their everyday practices. These case histories, of patients with 'illnesses' their doctors did not understand, were examined by a method derived from psychoanalysis, wherein the doctor was encouraged to express freely his feelings about the patient and the problem. These were then subjected to criticism and interpretation by the group and its leader.

From these Balint formulated some hypotheses about the nature of the general practitioner's work and his relationships with patients and colleagues. The general conclusion of the book is that "the more one learns of the problems of general practice the more impressed one becomes with the immense need for psychotherapy".

There are a number of corollaries to this. The first is that psychological illness should not be diagnosed by exclusion and thereby relegated to a low place in the ranking order of illness. Secondly, because of the continuity of relationship existing in general practice, a unique kind of psychotherapy is possible. To make the most profitable use of this the general practitioner requires training which "entails a limited, though considerable, change in the doctor's personality". The method of training suggested is essentially the same as that employed in the book, *The Balint Seminar*.

Thirdly, Balint comes to the conclusion that "on the basis of mutual satisfaction and mutual frustration . . . a unique relationship establishes itself between a general practitioner and those of his patients who stay with him". This, taken in full context, implies a unique behavioural pattern between a general practitioner and his patient.

The appearance of Michael Balint's book in 1957 was a seminal event and created for many established general practitioners a new insight into the nature of their task and tremendous promise for the future development of general practice. However, its most

significant achievement was to give to general practice a new and separate identity, with a body of knowledge and skills unique to a now defined task, worthy to stand comparison with any other defined specialty in medicine. It materially assisted in raising the morale of general practitioners and was to have a profound effect upon the development of general practice (*Journal of the Royal College of General Practitioners*, 1972). The basic assumption of this work, that a psychoanalytical approach is valid to apply to the 'problems' of general practice, has never been challenged, nor have any of its conclusions. I wish to do this now.

### Popper's contribution

Any criticism of Balint must begin with the fact that he was a psychoanalyst. He believed that it was possible to arrive at a scientific understanding of human behaviour through its interpretation according to a number of recognized theories. These theories themselves are derived from studies of human behaviour and because these were carried out objectively they are given the appearance of scientific enquiry. Indeed they are scientific, provided patterns of behaviour are only described and named.

However, when interpretations of such behaviour are sought, scientific activity ceases, for the theories on which any interpretation must rest cannot be subjected to any test of their truth. As Karl Popper (1963) points out, they are irrefutable, and this in his view bars them from any claim to be scientific.

Popper's seminal contribution to philosophy was to provide us with the means of demarcating science from the rest of the world's knowledge and at the same time solve the problem of induction. Since the time of Francis Bacon it has been assumed that the scientific method involved the collection of observations, from the collation and comparison of which it was then possible to propose theories which explained them. Further evidence was then sought to support the theory and then it was applied in other fields where it might be fruitful. This idea of science leans heavily on the theory of induction: the method of deriving a theory from a series of observed instances. David Hume (1739) described the problem of induction saying that there is no logical reason why the sun should rise tomorrow morning or why, because we have counted 999 white sheep going through a gate, the next one should not be black. Our expectation is not logical but psychological. There Hume left it, with no solution to offer.

### Refuting conjectures

Popper's solution is deceptively simple, yet really involves an intellectual revolution. He says that what demarcates scientific propositions from the rest is not the possibility that they may be confirmed but the possibility that they may be refuted; that the scientific method does not start with the making of observations upon which we base a conjecture, but with a conjecture

which we then confirm or refute by observation. If our first conjecture is successfully refuted we make a second one and subject it to the same testing procedure all over again. Our second conjecture should then be better than our first because it has taken account of our first refutation. Thus knowledge advances. It cannot advance if our theories can only be confirmed; they must be also refutable.

Popper insisted that psychoanalytical theories are not refutable and therefore cannot be regarded as scientific. On the contrary, he says all observations seem to confirm them. "Every conceivable case (history) could be interpreted in the light of Adler's theory, or equally well of Freud's. I can illustrate this with two very different examples of human behaviour: that of a man who pushes a child into the water with the intention of drowning it; and that of a man who sacrifices his life in an attempt to save the child . . . according to Freud the first man suffered from repression (say of some component of his Oedipus complex), while the second man had achieved sublimation. According to Adler the first man suffered from a feeling of inferiority (producing perhaps the need to prove to himself that he dare commit some crime), and so did the second man (whose need was to prove to himself that he dared to rescue the child)" (Popper, 1963).

To say that psychoanalytical theories are not scientific does not deny them any value at all. On the contrary, the evidence of their impact upon the world is enough to show that they have value. The point I am labouring is the kind of value. I believe their value is more akin to that of a myth. As Popper points out, myths were our earliest conjectures. They were the means by which we came to terms with an alien world which we could not understand or describe in scientific terms. They are so still, but they should not be confused with science. It is tempting here to pursue the nature of myth and psychoanalytical theories, what it is that gives them value and why it is that psychotherapy works, but my purpose now is to show only how errors can arise from the basic assumption that psychoanalytical (and other) theories of human behaviour are scientific, in particular when applied to general practice. I believe Balint did just this.

### Conjecture precedes observation

Balint used classical psychoanalytical interpretations rarely, but he plainly showed his willingness to interpret human behaviour in other ways. It is not possible to interpret any behaviour unless we do it according to some previously held theory, whether acknowledged or not. This is the same as Popper saying that conjecture always precedes observation.

When we make a simple statement like "John is tall", we interpret John's height according to the theory that most men are not as tall as John. This is a scientific theory because it is refutable. We can measure the height of a number of men, determine the mean and

compare it with John's height. If, however, we observe John to attack his brother, Daniel, we shall be all agog to know why and soon tempted to infer, perhaps, that John is angry with Daniel. This is to interpret John's behaviour according to the theory that when one man strikes another he is angry with him. This is not a scientific theory because we cannot refute it. It may be true, but on the other hand John may be playing with Daniel, he may be trying to deceive us into thinking that he is angry with Daniel, he may even be provoking Daniel, but there is no way of knowing which because we cannot lay bare John's mind to observe his reasons directly. Yet there can be no doubt that a perceptive person may confidently ascribe John's actions to anger. This is an intuitive judgement which could only become scientific if we were to become aware that, for instance, some facial expression over which we have no voluntary control invariably indicated anger.

### **Possibility of depression in Balint's case histories**

The case histories illustrate Balint's interpretations of individual behaviour. They can also be used to illustrate how other interpretations are possible, both refutable and otherwise.

In Case 16, a long interview seems to be the only indictable event to account for the dramatic change in Mrs O's behaviour. But we are not told whether or not her symptoms stopped, only that she stopped telling her doctor about them. We can never know if there were any other, perhaps more potent, indictable events in her life at the same time.

Case two is presented as a textbook illustration of "the child as the presenting symptom". "Whenever (the mother) was under some strain the child became ill". Yet the duration of symptoms, which justified the case's inclusion in the book, was only the first six months of life, after which the patient changed her doctor. Is this long enough to justify the confident assertion that the disharmony between the mother and the people downstairs was reflected in the anxiety of the mother and illnesses of the baby? The more obvious alternative, of puerperal depressive illness in the mother causing a bad relationship with the people downstairs and undue anxiety about the child, is not even mentioned.

Case nine is a quite astonishing revelation of the fixity of Balint's ideas. Not only does he not mention the most likely possibility, that this is a depressive illness with phobic features which, in the majority of cases, remits spontaneously, he does not mention the psychosomatic possibility of this being a reactive depressive illness remitting when the cause (the poverty of her emotional and sexual life) was removed. Nor can he accept a possible psychological interpretation, that although not suffering from a depressive illness her symptoms were somehow caused by a need for love which obviously improved when love was provided.

Similar comment can be applied to almost every other case history. It is always possible to invent some other plausible psychological interpretation which may readily gain acceptance because it will necessarily be irrefutable. It is significant that in no less than 18 of the 28 case histories it is possible to impute a diagnosis of depressive illness. The concept of depression as a descriptive diagnosis makes it refutable. We can argue about whether there are enough features of the illness present to warrant the diagnosis. On the other hand the concept of depression as a symptom is necessarily held together with the concept of a psychological cause. This is irrefutable.

I am concerned that the possibility of depression being an illness giving rise to symptoms is apparently completely ignored. 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.

Balint's descriptions of "the doctor's apostolic function", "the collusion of anonymity", "the perpetuation of the teacher/pupil relationship", and so on, are, however, scientific. They consist of naming repeatedly executed behaviour. This is a necessary first step in the elucidation of all disease processes as well.

In Popper's terms, from our unconscious experience of the behaviour of sick people we form a conjecture that there is a recurring pattern among some of them. Observation then confirms or refutes this. Having established a recurring pattern we are then in a position to make a conjecture about its cause so that we may hope eventually to apply a remedy. Here surely was the answer to Balint's plea for a "set of technical terms", yet, in the context of the individual patient's illness, he fails to make use of it.

### **Diagnosing psychological illness by exclusion**

As for Balint's criticism of the practice of diagnosing psychological illness by exclusion, he says that this leads to a low rank being assigned to psychological illness which is thereby neglected when, as he rightly points out, it gives rise to just as much suffering, if fewer deaths, than physical illness.

It is unfortunate that Balint is not explicit about what he means by psychological illness. One can only infer what he means from the general argument of the book and from one specific admission: "We psychiatrists cannot yet give the general practitioners the badly needed set of technical terms which they could use confidently and which would help them to understand the deeper personality problems of their patients." This is an admission of a lack of descriptive diagnoses; the lack of a descriptive science and scientific language for his discipline. Yet ironically and significantly there was, for some of his cases, a scientifically descriptive diagnostic term which he chose not to use: depressive illness. He regarded depression only as a symptom.

I think we can safely assume that what he means by

psychological illness is undifferentiated illness which presents as a disorder of behaviour. The mere fact that these illnesses are undifferentiated renders it logically impossible to diagnose them except by exclusion. This does not necessarily mean that they are ranked lower than physical illness. 'Pyrexia of unknown origin' is an undifferentiated group of physical illnesses, necessarily diagnosed by exclusion, but certainly not ranked lower than other physical illness.

We may look at this another way. Psychological diagnoses can be made only according to interpretations of behaviour which are irrefutable, whereas physical diagnoses are made according to physical observations which are refutable. The latter are therefore excludable while the former are not. Therefore, we can only make psychological diagnoses, of any kind, by exclusion.

#### *Balint's use of descriptive diagnosis*

Expanding on this theme Balint informs us that to arrive at a full understanding of neurotic symptoms we must explore completely the setting in which the symptoms arise. Judging by the way this is done in the book it is clear that we are asked to describe it in social and psychological terms. In this way, he says, we shall arrive at a "deeper level of diagnosis". From the way he rejects the possibility that some of his cases might be differentiated into 'depressive illness' or any other, such as anorexia nervosa (Case 12), I believe it is quite clear that he prefers this kind of unconstrained descriptive diagnosis and regards it as a valid end in itself.

Here again Balint reveals his philosophical misunderstanding. He is quite unaware that this idea of diagnosis is logically at odds with his plea for more scientific language for his discipline. He rejects the opportunity to demonstrate a descriptive science of behaviour and asks us to produce literature instead.

He asks us to make observations according to the theory clearly expounded in the introduction to the book that, "some of the people who, for some reason or other, find it difficult to cope with the problem of their lives resort to becoming ill". The whole book is devoted to an attempt to persuade us that 'some reason or other' is really 'some psychological reason or other'. In other words, we are asked to make observations according to an irrefutable theory; an unscientific conjecture. He diverts our minds into literary understanding when scientific understanding is possible and more appropriate.

I do not suggest that we take no note of the social and psychological relationships of illness. My plea is that doing so must not become an end in itself. The danger of ascribing any pre-eminence to this kind of descriptive task being the proper method of a different discipline is that it will beguile us into the belief that once completed the whole of our own discipline's task is also completed, whereas our primary task is to make a scientific diagnosis. To describe the social and psychological relationships of illness is indeed a necessary task. Not only does it illuminate our scientific diagnosis but it

draws our attention to very practical matters, such as the need to alleviate poverty or bad housing. However, it is not to make a diagnosis, it only complements it.

I must now account for the problem posed by those patients for whom, by the necessary process of exclusion I have described, there is none other than a psychological diagnosis. If Balint's sample is representative, and it accords with at least my experience that it is, there are fewer than Balint would have us believe. A large majority would now be regarded as depressed. Of the rest, the majority would probably be regarded as fixed neuroses of poor prognosis no matter how much help they had received. Only the remainder were undoubtedly helped by the doctor's behaviour, the doctor himself being the successful 'drug'. Why he was successful is not clear from the book. Nor is it clear from the book why the doctor was able to establish a good relationship, if not a very helpful one, with some of the fixed neurotic patients.

A group under-represented in the book, but which we know to have formed a large proportion of the seminar's caseload, may provide a clue. These are the patients who were new to their doctor. The book *Treatment or Diagnosis* (Balint *et al.*, 1970) came about because a member of the seminar commented that many of the case histories they discussed were of patients not well known by their doctor. My own experience in this respect is relevant.

#### **My own experience**

I arrived in my present single-handed rural practice in 1961 having spent eight years in a suburb of a large city. At that time I was enthusiastic about Balint's ideas, for they had served me well. My new practice was disorganized and my neighbouring general practitioner was ill. There was no time for long interviews. Seven years later an incident drew my attention to the then undoubted fact that I appeared no longer to subscribe to Balint's teaching. I had apparently forgotten that the initial presentation of illness often has nothing to do with the patient's real needs. When pressed I found myself denying that this was so for any but a small minority. I certainly would not have done this during my first two or three years here for, somewhat to my surprise, I found just as many 'neurotic' patients and 'covert' presentations as I had had in the suburb. On reflection I had no doubt that the number of patients who behaved in this way had diminished. Yet the practice was virtually unchanged. In those years turnover was almost entirely accounted for by births and deaths.

More to assure myself that this was not idle fancy rather than to provide a proper enquiry, I decided to look back through my records to see if there was any evidence for my suspicion. I compared a month's work in 1964 with a month in 1972. I looked for neurotic patients and covert presentations typical of the case histories in *The Doctor, His Patient and the Illness*.

There were 41 such patients presenting in 1964 in 70 consultations. In 1972, 21 patients presented covertly in a total of 33 consultations. Of the 41 patients presenting in 1964, five were dead in 1972, 14 had removed, six were still presenting covertly, while 16 had stopped doing so.

Of the 21 patients presenting in 1972, six, as already mentioned, were among those presenting in 1964, five were on the list in 1964 but behaving normally, while ten were new additions to the list.

Of the 16 patients who had stopped behaving covertly, two had gone onto the repeat prescription list (Balint *et al.*, 1970) and three had gone onto the repeat visit list (perhaps a rural equivalent of the repeat prescription list). One had become so intellectually damaged that she no longer had conscious control over her behaviour. Two I knew were no different but took their troubles elsewhere (to non-medical people). One seemed to have gained true insight and needed only support in crises. Six were clearly and radically changed for the better. (It is notable that in each of these there had been a significant change in circumstances: the death of a relative in four, and a change of occupation in two.)

#### *"Rituals of courtship"*

Thus it seems that some function of time, which when closely examined may not reflect well on the doctor, can also bring about a change in the way patients present their illnesses, being covert when they do not know him well and straightforward when they know him better. Perhaps much of the behaviour that stimulated Balint to write his book may now simply be described as the "rituals of courtship", or, less fancifully, as the behaviour shown by dissimilar and perhaps antipathetic personalities when they try to get to know each other. We are all familiar with this kind of behaviour in ourselves and in other people. It is not unique to the general practitioner's consulting room; it can also be seen at cocktail parties. There is, after all, nothing unique about the doctor-patient relationship.

#### **Training general practitioners by case discussion**

Integral with Balint's theory of psychological causes for the 'problems' of general practice is his concept of the training that general practitioners must undergo if they are to identify these problems and have any success in helping their patients with them. The method he said should be adopted is openly derived from the training of psychoanalysts. It has been adapted to suit the special conditions of general practice. The basis of it is the case conference and the objective to help the doctor "to become more sensitive to what is going on, consciously or unconsciously, in the patient's mind when doctor and patient are together". Before this is achieved "there must be a limited but considerable change in the doctor's personality". During the case conference the doctor reports on cases which interest or puzzle him. He

is encouraged to be frank in expressing his feelings about the patient, the problem and his colleagues, and to accept criticism and interpretations of his own behaviour and feelings without rancour.

I think this exercise may have great value for some doctors and that the goal of trying to sensitize the doctor to what is going on in the patient's mind is invaluable, but whether its value is truly educative or merely supportive is uncertain. It is equally clear that it has no value for others. Balint openly says that the reason for this is psychological: either the doctor is immature, disturbed, or rigid. In any event he is not likely to be able to help those of his patients with psychological problems in the way Balint thinks best. No other reason for the dislike of this method by some doctors is advanced by Balint or his followers.

Yet this is very obviously a training method requiring verbal skill of its participants. If a doctor is at all inarticulate about his feelings and those of others he must be at a disadvantage. There can be no doubt that every doctor needs to be emotionally articulate to some degree, but whether he requires the order of skill necessary to enable him to keep his end up in a Balint seminar is another matter.

Alternatively, some may be more skilled in other ways of communicating feeling; perhaps more skilled than those who are verbally articulate. There are some cultural differences which are worth noting. Some people, like Yorkshiremen, are notoriously monosyllabic, while others, like Italians, are just as verbose. Yet there is no evidence that the social organization of Italy is any better than that of Yorkshire. Perhaps too some doctors have ways of trading in and expressing emotional understanding other than verbally. If this is true of doctors, then perhaps it is also true of some patients.

#### **Balint seminars**

Unhappily the Balint seminar, although realized by Balint to be unsuitable for some, is now promoted as a method suitable for all, and one suspects it is thought that those for whom it is not suited are in some way deficient in the qualities necessary for general practice.

There may be two reasons for this: first, a certain degree of emotional security is required to participate in a Balint seminar and it is glibly assumed that the converse is true, and secondly, in our bones we feel that being articulate is somehow culturally superior. But here we have a confusion of language.

Let me draw attention to the difference in language used when we describe a door from that used when we describe a smell. By describing a door as made of a metal and giving its measurements we can reproduce its like exactly. But not so the smell. Here we must resort to the literary devices of simile and allusion to convey our meaning, and even then we cannot be precise. The language we use to describe the door is explicitly verbal, or scientific. The language we use to describe the smell

is 'non-verbally' verbal, or artistic. The folly of trying to use scientific language to describe a smell is plain (unless we happen to know the chemical structure of the substance that assails our nostrils). The folly of trying to explain human behaviour in scientific terms is one and the same. It is to use a pseudo language or jargon. It is the language of neither art nor science and it pretends to be both. So that to find oneself out of sympathy with the Balint seminar in this respect is not to admit cultural inferiority at all. On the contrary it implies a healthy suspicion of all things pseudo, and of jargon.

If the Balint seminar were to be rigorously literary in its approach it would be a more credible medium for sharing this kind of experience and gaining this kind of knowledge. In the hands of the most able leaders I am sure this is achieved for much of the time, but such leaders are rare. Perhaps we should think seriously about recruiting more from among the ranks of playwrights and novelists.

If we apply here the philosophical limitations I have already applied elsewhere to the interpretation of behaviour, we see that the participants would be wise if they confined their observations either to refutable concepts—to descriptions of behaviour, leaving interpretation to the private and non-verbal intuition of the individual—or to frankly artistic expressions of thought and feeling.

The danger, as already pointed out, is that someone will be unable to resist the temptation to make an explicit interpretation. As we have also seen, this must be according to some theory or other; some theory of behaviour which may or may not be articulated. A powerful leader may thus unwittingly impose some theory on the whole group; a theory he himself may be unaware of, and the group too.

Balint leaders are likely to have the same kind of basic assumptions and this theory is quite likely to be uniform among them. It is theoretically and practically possible that a future generation of general practitioners, having all been trained by this method, will have a uniform conception of the way people ought to behave according to whether they are ill or not, and unwittingly impose this on their patients. This is precisely what Balint himself was anxious to avoid when he described the apostolic function of the doctor which drew attention to the danger and the difficulty.

## Conclusion

Balint thought that a scientific understanding of human behaviour was possible, not only in descriptive terms, which is true, but in theoretical terms, which is not true. The false conclusions he came to about general practice are rooted in this basic assumption.

These were: first, that general practice is primarily concerned with psychological problems—this was inevitable given the irrefutable conjecture from which he started. As shown, irrefutable conjectures gather

supporting evidence from almost anywhere. If we seek to prove a theory with evidence that cannot help but support it we are not likely to add anything new to the stock of human knowledge. Indeed we can only restate our case in different terms. Balint's book does just this. It is possible to state its aim and achievement as: having investigated the theory that people present illnesses to their doctors when they have psychological problems, by exploring the psychology of their relationship with their doctors, we come to the conclusion that doctors have a psychological problem in their relationship with their patients because they present illnesses when they have psychological problems.

Secondly, he said that there is a unique relationship between a general practitioner and his more familiar patients. Here, not only does his stated reason for this judgement rest on circumstances held in common with many other relationships; his unstated, but obviously implied, support for this statement is his interpretation of the case histories. More often than not he opted for irrefutable ones and so was perhaps blinded to refutable ones which would not have supported his conclusion.

Thirdly, he said that psychological illness should not be diagnosed by exclusion and consequently came to believe that the diagnostic process was one of description rather than identification.

Fourthly, he said that general practitioners should be trained in behavioural skills by an explicit rather than by an intuitive method. He thus initiated a confusion of language and a proliferation of jargon which we should now reject.

The fact that this paper is almost entirely critical must have left its readers with the impression that I can find little in the book to praise. Belatedly and too briefly I must correct that impression, for I am in no doubt about its value and benefit to general practice. However, others have paid ample tribute to this and I must plead lack of space.

Despite this, I believe it has diverted general practice from its true course. It is my firm belief that if general practice is to prosper as an independent discipline it must return to a primarily scientific orientation. Not to do so is to perpetuate intellectual confusion which can only beget ever greater errors. Doing so we shall not become less conscious of the social and psychological relationships of illness, or less compassionate. Rather, being no longer fettered by irrefutable conjectures about the springs of human behaviour, shall we acquire more humility before the people we care for.

*The Doctor, His Patient and the Illness* is a book whose friends see in it nothing but virtue, and whose enemies nothing but vice. I wrote this paper in the hope of achieving two goals: first, that it might correct both its friends and its enemies and bring a more realistic appreciation of its virtues; secondly, that by showing the relevance of Popper's work to our problems it might help us find our way back to a wiser course for general practice.

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## Pharmacists and prescribing

For example, it could be argued that, for a pharmacist, the prescribing of oral contraceptives ought to be no more difficult than counter prescribing generally. Certainly the problem of diagnosis is removed, but on the other hand the side effects of oral contraceptives are so potentially hazardous that it might seem a wise precaution that their prescribing be restricted to doctors.

There is the further aspect of the matter that, up to now, prescription-only has meant prescription by doctors, dentists, or veterinary surgeons. The widening of the term to include other health professionals would make the checking of credentials of prescribers by the dispensing pharmacist much more difficult. Again, the cost of maintaining registers, with annual authorization, would be quite high, and would presumably be met by payment of a fee by would-be prescribers—in other words a further spreading of the bureaucracy with which so much disillusionment already exists in Britain.

On the other hand, to have widened supply of oral contraceptives to general sale would have been a lunatic decision. It is surprising, therefore, that the working party gives a hint that if its recommendations on prescribing prove to be less effective in extending the availability than it hopes, then it would not rule out the possibility of making oral contraceptives available on general sale at some later date when it might be that safer preparations would be available.

The working group recognizes that there may be many legal, or practical and administrative difficulties in implementing the recommendation. It may indeed be that a new act will be required before a special class of prescribers could be created. Then, again, special arrangements would have to be made for the training and registration of those who would wish to take up the prescribing of oral contraceptives.

It is not clear from the evidence that the need justifies the changes proposed. One is inclined to the belief that the proper people to prescribe oral contraceptives are general medical practitioners. Perhaps the new developments are a reflection of their failure to meet the legitimate needs of the public. Would not the solution be to improve their performance in that respect?

### Reference

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## OCCASIONAL PAPERS

The *Journal of the Royal College of General Practitioners* has introduced a new series of publications called *Occasional Papers*.

Like the *Reports from General Practice* and *Journal Supplements* these are published by the Journal office, but unlike the other two series, will not be posted to all readers of the *Journal*. Readers can obtain copies direct from either 14 Princes Gate, Hyde Park, London, SW7 1PU.

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### OCCASIONAL PAPER 2

#### An Opportunity to Learn

*Occasional Paper Number 2* is the report of Dr E. V. Kuenssberg, the Wolfson Visiting Professor, and describes his visits to many countries of the world, his assessments of general practice, its organization, development and future.

### OCCASIONAL PAPER 3

#### Trends in National Morbidity

The third *Occasional Paper* compares and contrasts the changes found on analysing the results of the two national morbidity surveys in Britain.