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Correspondence

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PSYCHIATRY AND THE CONCEPT OF DISEASE

DEAR SIR,

Professor Kendell (*Journal*, October 1975, **127**, pp 305–15) has given us a lucid synopsis of the development of the concept of disease and the criticisms made of the use of this concept in psychiatry. However, the argument of the entire piece appears to contain several flaws.

He seems to believe that to define disease is in some way to describe the proper area for medical concern. Yet medicine is not restricted to dealing with diseases; as he himself points out; no-one would deny the right, indeed the duty, of medicine to be involved in childbirth, yet equally no one would wish to consider it a disease. Similarly the physician who refused to treat post-herpetic neuralgia or psoriasis on the grounds that they do not fulfil Scadding's (1967) criterion of disease would rightly be considered heartless if not negligent.

Does then this definition indicate a minimum area within which medicine must operate? This may at first appear so, but applied strictly—as it must be to serve any purpose—the definition is equally valueless for this purpose. As Kendell points out, family planning is an important part of contemporary medicine. Yet sterilization fulfils his criterion of disease; it is a deviation from the norm placing the individual at a biological disadvantage (at least in Kendell's terms), the inability to reproduce. Should then sterilization be considered an iatrogenic disease? Similarly, it is not only the behaviour of schizophrenics or homosexuals that reduces fertility, but also that of all who voluntarily refrain from procreation. Should we therefore consider a vocation to the religious life as a mental illness?

The attempt to define a biological disadvantage independently of social factors is also doomed to failure, for the selection which determines whether a statistical abnormality is advantageous or disadvantageous depends upon the environment, and this surely must include the social milieu.

Further, because a condition confers a 'biological disadvantage' does it necessarily follow that a medical practitioner is 'better equipped to understand and treat it' than anyone else? The removal of biological disadvantage is not the primary concern of medicine, at least not as Kendell defines biological disadvantage. It is rather to prevent and relieve suffering and to prevent premature, avoidable death. Why, then, is a biological disadvantage necessarily a medical responsibility?

Surely what medical practitioners are particularly equipped to do is to approach a problem in a particular way; to bring their biologically-oriented training to bear on it; in fact to apply the 'medical model'. In certain situations this may be the only conceivable approach, as in a case of bronchopneumonia, broken leg or childbirth. In others it may be one of several possibilities; for example anxiety may be treated by drugs or psychotherapy. There may be other cases where an alternative approach may clearly be more suitable—perhaps a spider-phobia.

There will, no doubt, be areas of disagreement between those who think like Eysenck or Laing and those who favour the medical model. Is this not analogous, however, to the differences of opinion between proponents of medical and of surgical treatment for certain conditions, and to the often even more vexed question of 'to treat or not to treat'? Certainly it is more fruitful to concentrate on this more practical question than to chase the wild goose of a simple definition of disease, whether it is designed to include or to exclude mental illness.

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DEAR SIR,

The interesting article by Professor Kendell (*Journal*, October 1975) gives a thoughtful and realistic account of the difficulties which attend the various solutions suggested by different writers. His solution, which essentially entails acceptance of Scadding's 'biological disadvantage' criterion, does not in the least conflict with my own position, although Kendell maintains that the 'presence of a lesion' criterion is implicit in my reasoning. I was not concerned so much to try to solve this ancient

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mystery as rather to point out that there are difficulties attending the psychiatric practice of treating different types of psychiatric problems as all equally 'medical'. Kendell, in fact, lends strong support to my arguments, as expressed most recently in my Methuen pamphlet on *The Future of Psychiatry*, by concluding that the functional psychoses would, on his criterion, be 'diseases', while neuroses and 'the ill-defined territory of personality disorder' cannot (at least as yet) be so regarded. That, of course, was the basis of my argument in that pamphlet (and earlier in my *Handbook of Abnormal Psychology*); that psychiatry was in fact split in two parts, one medical, dealing with what might justifiably be called 'diseases', and the other behavioural, dealing with behavioural maladjustments and constituting a psychological-educational rather than a medical problem. The argument about 'lesions', although important, probably distracts attention from the major difference. Kendell's arguments would seem to support my position, although only implicitly.

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SUBJECTIVE AGE IN CHRONIC SCHIZOPHRENIA

DEAR SIR,

We read with interest the recent article by Crow and Mitchell (*Journal*, April 1975, **126,** p 360), and as we were in the process of testing a random sample of psychiatric in-patients in connection with another project we decided to obtain subjective ages on these patients as well. As a result, subjective ages were obtained from 144 patients in the various wards of Harlem Valley Psychiatric Center, Wingdale, New York. Responses from four of these patients were eliminated because of vagueness or extremeness (e.g. '30 to 40 years old', '1,000 years old').

The subjective age-distribution obtained is quite similar to that obtained by Crow and Mitchell (Table).

Crow and Mitchell conducted additional analyses on those patients who reported themselves to be five or more years younger than they really were. They found that 27 per cent of their sample fell in this category, with a mean true age for these patients of 59 years and a mean duration of stay of 26 years. Corresponding figures in our series were 28 per cent of the entire sample, with a mean true age of 53 years and a mean duration of stay of 18 years.

Among their patients whose subjective age was five or more years younger than their real age Crow and Mitchell attached particular diagnostic importance to those whose subjective age was within five years of their age upon admission. They found 26 patients (12 per cent) in this category, while we found 12 (9 per cent).

Our results are very similar to those of Crow and Mitchell, despite the fact that the composition of the two samples differed. Their sample consisted of only chronic, male schizophrenics, while ours consisted of males and females, schizophrenics and a few non-schizophrenics, and short-term and long-term patients. Interestingly, although our sample was more varied, of the 12 subjects in the critical group (i.e. those who reported their ages to be five or more years less than their actual age and within five years of their age at admission) 11 were diagnosed as schizophrenics and 10 of these were males.

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TABLE

Comparison of two subjective age studies

	Crow-Mitchell		Smith-Oswald	
Males	220		71	
Females	0		69	
Mean age (years) ..	54		49	
Mean length of stay (yrs)	19		15	
Distribution of subjective ages	N	%	N	%
I. Correctly reported age	80	36*	51	36
II. Subjective age within 5 years of actual age ..	74	34	39	28
III. Subjective age ≥ 5 years below actual age	60	27	39	28
IV. Subjective age ≥ 5 years above actual age	6	3	11	8

* Based on a sample of 220.

SCHIZOPHRENICS' FAMILIES

DEAR SIR,

The use of a controlled family study (Stephens *et al*, *Journal*, August 1975, **127,** pp 97–108) to investigate the schizophrenic 'spectrum' appears to have potential for clarifying the diagnostic boundaries of

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