

many complaints stem from our manner of applying it, and perhaps we should revise some of our routines. Despite the relative simplicity of the technique, it remains an ordeal for many of our patients.

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REFERENCE

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PHYSICAL ILLNESS IN PSYCHIATRIC PATIENTS

DEAR SIR,

I was most interested in the article by Drs. Maguire and Granville-Grossman (*Journal*, November, 1968, p. 1365). It highlights a problem that will probably increase if the present trend in first admissions in the over-sixties is maintained.

One of the most worrying features is the high number of cases undiagnosed prior to admission; this must result in inferior or incorrect treatment in some cases. In my own recent study of 250 consecutive admissions to a city mental hospital (Johnson, 1968). I restricted my attention solely to those cases which could be diagnosed, or highly suspected from the routine physical examination on admission. Fifty-three cases (20 per cent.) were diagnosed as having a physical illness requiring treatment. In thirty admissions (12 per cent.) it was thought that the physical state was an important aetiological factor in the presenting psychiatric symptom. Twenty-four of these cases (80 per cent.) were undiagnosed at the time of admission. In two of these admissions it is possible that earlier diagnosis of the physical illness might have saved the patient's life. One patient was moribund on admission from haematemesis, and the other suffering from broncho-pneumonia and multiple injuries. Of the thirty cases with physical illness as an important precipitating factor, eighteen (60 per cent.) were over the age of sixty.

The plea made in the article for a thorough routine physical screening of all psychiatric admissions is certainly substantiated by these figures.

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REFERENCE

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PSYCHODYNAMIC CHANGES IN UNTREATED NEUROTICS

DEAR SIR,

May I be permitted to reply to the letter from N. McConaghy (*Journal*, September, 1968, p. 1197)?

I must admit that this made me think for about forty-eight hours before seeing the solution that was clearly implied in our paper. In order to show this, it is necessary to repeat McConaghy's reasoning in summary. This was as follows:

- (1) We admit that there appears to be no detectable difference in the percentage of symptomatic improvements between series of treated and untreated patients;
- (2) We claim that symptoms are a response to identifiable stress;
- (3) We suggest that psychotherapy enables a patient to handle stress without getting symptoms; but
- (4) *Since there is no reason to suppose that treated and untreated patients differ in the degree of stress they experience, one of our propositions (1), (2), or (3) must be incorrect. Though McConaghy did not say so, the obvious candidate is proposition (3).*

The fallacy in this reasoning lies in the passage in italics in (4) above. McConaghy implies that exposure to stress is beyond the patient's control. Of course this is not so. A patient who has *not* recovered from his basic anxieties will tend to withdraw from stress; one who *has* recovered will not need to withdraw from it, and indeed should actively seek it—most of the stresses postulated in our paper are a necessary part of normal life. A series of *symptomatically and dynamically* improved patients should therefore experience a greater degree of stress than a series that is *symptomatically* improved only.

It is thus perfectly possible for the symptomatic improvement rates in treated and untreated series to be similar, and yet for psychotherapy to be effective. This would apply even if the improvement rates in the two series were known to be exactly equal, which obviously they are not; and if in dynamically unimproved patients there were always a one-to-one relation between stress and symptoms, which is obviously not so. Both of these two facts weaken further the kind of reasoning that McConaghy uses.

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