

pharmacotherapy of first-break schizophrenia was significantly related to poorer work outcome, but did not report any measures of association. Dr Muijen correctly noted that a *P*-value conveys nothing about the strength of association. Certainly, one cannot quarrel with Dr Thomas's statements about the dependency of progress in the 'soft' sciences of psychiatry and clinical psychology on rigorous formulation and testing of hypotheses, as this is true in all sciences. But issue may be taken with his interpretation of the relation between the magnitude of a correlation and its utility.

Dr Thomas states that correlations less than the 0.866 he derives from the application of information theory are "not associated to any useful extent (whether for clinical decision-making purposes or for the advancement of theory)". At least with regard to practical decision making, this position is an excessively narrow view of the practical utility of small correlations.

Very small correlations may have important consequences. For example, the relative risk of depression in United States soldiers who served in Vietnam was approximately twice as high as that for soldiers who served elsewhere. The correlation coefficient associated with this difference in relative risk was 0.06. Coming closer to home, the relative risk of myocardial infarction was twice as high in physicians receiving placebo than those receiving one aspirin a day. The correlation, 0.03, was large enough and important enough to guide clinical decision making: the trial was discontinued because of the questionable ethics of maintaining a placebo condition.

The proper appreciation of strength of association, at least in clinical research, required the evaluation of effect size rather than absolute magnitude of correlation. An excellent discussion of effect size estimates may be found in Rosenthal & Rosenow's (1991) recently revised text from which the above examples were drawn. Regrettably, the practical implications of Drs Johnstone *et al*'s findings of an association between drug-treatment and disadvantaged occupational functioning in first-break schizophrenia patients remain unclear, for the reply to Dr Muijen (*Journal*, May 1991, 158, 713–714) did not include any measures of association either!

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The future of psychotherapy

SIR: I read with interest the Point of View "Psychotherapy 2000. Some predictions for the coming decade" by Holmes (*Journal*, July 1991, 159, 149–155).

With the recent surge in the field of biological psychiatry, psychotherapeutic management and research has taken a back seat. The grim prospects in the treatment of psychiatric patients by psychodynamic means have been highlighted in recent years (Mueser & Berenbaum, 1990). It seems that, in future, psychotherapy will also be at risk of occupying the initial pages of the psychiatric textbooks as a management procedure of historical importance, just as leucotomy is remembered today.

It is interesting to see the way the orientation of psychiatric practice has changed over the last 100 years. In the late part of the 19th century, neurology dominated much of psychiatry. Kraepelin, with whom a new era of modern psychiatry dawned, had other prominent neurologists of his time (Nissl, Alzheimer and Brodmann) in his department. Freud, a neurologist by orientation, borrowed heavily from neurological concepts when he attempted "The project" (although it remained buried until after his death). It was only later that he shifted from organic to psychoanalytic concepts.

By the beginning of this century, psychiatry had gradually begun to drift away from neurology, probably under the influence of the psychoanalytic school which emphasised the unconscious rather than the conscious manifestations of the mind. Some psychiatrists even began to resent the interference of neurologists in their field which they thought had nothing to do with the structure of the brain. James V. May, in his presidential address to the American Psychiatric Association in 1933, called it an invasion of the psychiatric field by the neurologists. Strecker (1934) wanted the borders of psychiatry and neurology to be sharply demarcated. Psychiatry at this time was dominated by psychoanalysts and psychotherapists.

After a period of relative success with the psychological modes of treatment, with the introduction of neuroleptics in the 1950s there was again a definite shift towards neuroscientific understanding and genesis of psychopathology. In the transitional period, both neurological and psychoanalytic concepts were being incorporated into a common hypothesis. Ostow (1966), among others, voiced one such opinion: "What one sees clinically is that after the administration of such drugs [neuroleptics], the ego seems to be depleted of drive energies and to be unable to sustain its own proper ego functions". A healthy union of the biological and psychothera-

peutic approaches was thus hypothesised. With the influx of 'newer' treatment modes, the enthusiasm had shifted away from psychotherapeutic to pharmacotherapeutic modes which were more 'mathematical' and 'objective'.

Since these modes were simpler and shorter, and their efficacy more measurable, clinicians started casting a doubt upon the usefulness of psychotherapy, not to mention its possible deleterious effects. The success of neuroleptics was, however, not the only reason for pessimism in psychodynamically orientated theories and therapies. The outcome studies in the field of psychotherapy only reinforced the fears of its demise (Stanton *et al*, 1984; Gunderson *et al*, 1984) and cautioned its readers about the inherent difficulties of undertaking research in this immensely popular research area in the early part of this century.

In the past, probably, research in this field has been overinclusive. Trying to explore and analyse every aspect of patients' psychopathology which was volunteered or exposed was neither cost effective nor productive. In contrast to psychotherapeutic research, pharmacotherapeutic research currently enjoys substantial grants since it somehow succeeds in halting the disease process, the management being shorter, simpler and target orientated. It is more acceptable although it is symptom orientated and lacks a definite aetiological hypothesis (the aetiology is suggested only because the drug works). Psychodynamic exploration and psychotherapeutic management, although seen with scepticism, happen to be assessment procedures which are actually based on a solid and stable aetiological hypothesis, and thus enjoy a distinct advantage over biological research. Abandoning such a potential area when the answers are still not available in biological psychiatry may be a serious omission.

It is encouraging to see the newer psychotherapeutic procedures becoming short term, target orientated and cost effective. If psychotherapy is to survive as a potentially useful management procedure in the future, such amendments are necessary.

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Child psychiatry in the 20th century (England & Wales)

SIR: The paper by Wardle (*Journal*, July 1991, **159**, 53-58) misleadingly refers, in its title, to the development of services for child and adolescent psychiatry in Britain. The paper, in fact, refers to the development of these services in England and Wales; Scotland is ignored. There is no mention, in Table 6, of the Kilbrandon Report of 1964 and the subsequent Social Work (Scotland) Act of 1968 which led to the setting up of Children's Hearings. Similarly, this table ignores the report *Crossing the Boundaries: New Directions in the Mental Health Services for Children and Young People in Scotland* published in 1983. Perhaps the College could ensure in future that papers purporting to describe national developments should do just that: the alternative, as displayed in Dr Wardle's paper, is at best unsatisfactory and at worst offensive.

HMSO (1983) *Crossing the Boundaries: New Directions in the Mental Health Services for Children and Young People in Scotland*. Edinburgh: HMSO.

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Evaluation of motor disorder in mentally handicapped people

SIR: We welcome Jones' discussion (*Journal*, September 1991, **159**, 441) of the evaluation of motor disorder in mentally handicapped people (MHP) and the DISCUS (Sprague *et al*, 1989).

There are numerous difficulties in any form of assessment of dyskinesia in severely and profoundly mentally handicapped people, the level of cooperation being only one of them. Others include: stress and anxieties caused by the examination, and exacerbated by the problems in verbal and non-verbal communication; the inability on the examiner's part to assess the individual's wishes and consent; the limited information of the mental state; and especially, mood on the day of rating.