

intelligence, all did badly on the Gottschaldt. In many years of work on this topic, I have never found this degree of consistency with any other test purporting to measure schizophrenic thinking.

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TREATMENT OF PHOBIC PATIENTS WITH ANTIDEPRESSANTS

DEAR SIR,

My earlier letter, in addition to earning me a magisterial rebuke from yourself ('Dr. Mawson expects too much'...), has clearly made Dr. Freeman very angry indeed. I have therefore re-read it carefully in an attempt to understand why. The results of this exercise would clearly not justify publication had Dr. Freeman contented himself with attacking me personally. Unfortunately he passes from the argument *ad hominem* to the argument *ad institutionem* and also chooses to ascribe the vices which he believes my letter to illustrate ('intellectual arrogance' and 'neglect of practical and humane considerations') to 'the development of academic psychiatry'; thereafter his targets proliferate, coming to include 'academic assessors', 'the pursuit of methodological purity', 'scientific sophistication' and even, at least by implication, the Dunlop Committee on the safety of drugs! It is possible that the prejudices thus revealed are shared by a significant proportion of psychiatrists and it would therefore seem important to identify the real issues at stake and discuss them fully and, if possible, dispassionately.

An initial step is to identify these issues which are not basic to the dispute. The principal of these is the question of whether or not MAIOs are in fact effective in the treatment of phobias. Despite Dr. Freeman's supposition to the contrary I too 'actually treat patients', and my experience of treating phobic patients with MAIOs leads me to believe that they do produce a striking response in at least a proportion of cases. Thus the difference between Dr. Kelly and his colleagues and myself,

within this narrower context, is that I know I only believe whereas they believed they knew. (I am glad to see, from their courteous and temperate reply to my previous letter, that they no longer consider that 'to carry out a trial using a placebo appears unjustifiable' and instead state that 'it now seems justified to carry out a double-blind controlled trial of phenelzine versus placebo'.)

Perhaps the real and important issues can be expressed as four questions.

(1) When should a statement in the form 'Treatment with X, has been shown to result in Y ($p = < 0.001$) be afforded more respect than one taking the form 'The authors' extensive experience has shown that treatment with X is highly effective in producing Y'—unembellished with probability values?

A statement in the first form purports to be a scientific statement, and is likely to be accepted by most readers as really meaning that there is less than a one in a thousand chance that X did not 'result in' Y. It may be helpful here to consider the following passage: 'In our general impressions far too great weight is attached to what is marvellous . . . the scientific man takes care to base his conclusions on actual numbers. General impressions are never to be trusted. Unfortunately when they are of long standing they become fixed rules of life and assume a prescriptive right not to be questioned. Consequently those who are not accustomed to original inquiry entertain a hatred and horror of statistics. They cannot endure the idea of submitting their sacred impressions to cold blooded verification.'⁽¹⁾ Francis Galton was writing nearly 100 years ago: the fact that his remarks still have some relevance is illustrated by the applicability of the last sentence of the quotation to the anguish expressed by Dr. Freeman in connection with his fluphenazine trial and those 'academic assessors'. In the main, however, the point which Galton expressed so well has been heeded, but the result has not been altogether an unmixed blessing.

On the one hand there is the loss, lamented by Dr. Sutherland in your columns two years ago, of the subjective, anecdotal or speculative type of article, putting forward hypotheses, formulations or models, dealing with 'soft' and often intrinsically unquantifiable data, and perhaps based on detailed but uncontrolled observations of small numbers of cases. To exclude such articles is also to exclude much of the subject matter of our specialty and to deny the value of the methods of, for example, Freud or Piaget. (It is ironic, in the context of the current dispute, to recall that Dr. Sutherland referred to your preference for research . . . "dominated by the

rigours of statistical and experimental method"). But there is a second danger, more relevant to the present argument.

It is all too easily assumed that because Galton's 'scientific man' 'takes care to base his conclusions on actual numbers' and employs statistics, therefore an author who includes a decent sufficiency of 'numbers' in his paper and applies statistical procedures to them must be a 'scientific man'. As such he will have employed scientific rigour in designing his experiments, in eliminating or allowing for variables other than those under investigation, and in studying the reliability of his data before applying further statistical procedures to them... 'than which nothing could be further from the truth', as I pointed out in my earlier letter. The fact is that when the altered expectations of editors (let alone 'academic assessors') lead even those who share Dr. Freeman's fundamental contempt for 'the pursuit of methodological purity' and for 'scientific sophistication' to dress up their clinical impressions (Galton's 'general impressions') with retrospective quantification and probability values, the situation merely becomes confused.

Fortunately, however, so far as the investigation of treatment methods is concerned, one does not need to be a statistician to distinguish a statement in the first form which deserves the respect accorded to science from one which does not. One needs only to appreciate the limited number of basic methodological requirements which I condensed, in my earlier letter, from Chapter II of a standard undergraduate textbook of clinical pharmacology.⁽²⁾

(2) Is it really evidence of 'intellectual arrogance' and an 'unfortunate by-product of the development of academic psychiatry' if, prompted by special interest to read an article carefully, and then recognizing that despite the distinction of its authors and the impressive probability values the basic methodological pre-requisites for the application of tests of statistical significance have not been met, one points this out? Surely it would be evidence of greater arrogance to decide that readers of the article could be assumed to fall into two groups: those who will already have perceived the same shortcomings and those who would be incapable of appreciating their importance?

(3) Is it really evidence of a 'neglect of practical and humane considerations' to draw attention to the fact that the efficacy of a drug, and especially of a group of drugs with certain known dangers, has not really been proved as convincingly as the authors claim?

Throughout the history of medicine sincere and devoted doctors have derived comfort, in the face

of the exigencies of 'day to day responsibility for large numbers of severely ill and handicapped people', from knowing that the treatments they prescribed were effective: knowledge supported (or at least not perceived as being refuted) by their extensive clinical experience. Accordingly successive generations of patients have been bled, purged, colectomized and leucotomized by therapeutic enthusiasts acting from the highest motives. We neglect the lessons of the past at our peril, or, more precisely, at our patients' peril. I would suggest that if the present and future generations of patients are to prove more fortunate it will be the result not just of the continuing proliferation of new treatment possibilities but also (perhaps even more so) of increasing acceptance of the need to subject the claims made for new treatment methods to the rigorous assessment of properly conducted controlled trials as soon as small pilot investigations suggest that they may reward the effort. That such will indeed be the case in general medicine there is little doubt. With respect to psychiatry it is hard to be so sanguine: the response with which my earlier letter has been met makes it—for me—harder still.

(4) Embracing the second and third questions and also extending beyond them is the more general question: what is the role (and what are the responsibilities) of 'academic psychiatry'? Dr. Freeman states that intellectual arrogance and a neglect of practical and humane considerations are the 'undesirable by-products' of its development, but does not tell us what in his view the desirable direct products of that development are; evidently not the 'pursuit of methodological purity' or the encouragement of 'scientific sophistication', since these, it seems, lead to investigations the results of which 'are sometimes very difficult to accept'. Perhaps we may turn for an answer to one whose opinions are afforded a respectful hearing by both sides in most psychiatric disputes. In his inaugural lecture at the Institute of Psychiatry, Professor Sir Denis Hill described and delineated the limitations of the predominantly scholarly and the predominantly scientific approach to psychiatry before concluding with the following passage: 'There is the ever-present danger that, disregarding the insights and understanding which scholarship can provide and the sense of criticism and scepticism which a scientific frame of mind can give, the clinician will degenerate into a technician, and in our present era a mere empiricist, purveying the products which our advanced pharmaceutical industries continue to develop and provide. The best hope for the avoidance of these dangers lies, in my opinion, in the intelligent and inspired partnership of university departments

of psychiatry with those whose business it is to treat patients as they actually present in our society.'(3)

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[A reply from Dr. Freeman will be published in a future issue and it is intended that this correspondence shall then be closed. Eds.]

A CASE OF ATRIAL FIBRILLATION FOLLOWING THE USE OF SUXAMETHONIUM DURING ECT

DEAR SIR,

A recent experience of the case described in detail below led to a review of the literature on cardiac irritability caused by suxamethonium. It was found that all the reported cases and animal experiments have referred to ventricular effects. The present case appears to be worthy of report, as we have been unable to find any reference to atrial effects in the literature.

The patient, a 57-year-old woman, was admitted to hospital for electroplexy for a fairly typical endogenous depressive illness. There was no previous history of cardiovascular disease, and routine physical examination at the time of admission revealed no abnormality in the cardiovascular or other systems. She had been taking Largactil 50 mg. t.d.s. for one week, but this was discontinued on admission.

Pre-medication consisted of atropine 0.4 mg. intramuscularly 45 minutes before treatment. General anaesthesia was effected by slow intravenous injection of 10 ml. of a 2.5 per cent solution of sodium thiopentone (Pentothal), and this was followed by intravenous injection of 30 mg. of suxamethonium chloride (Scoline). The first application of electroplexy passed off uneventfully. The second application was given two days after the first, and this was followed by the occurrence of unmistakable atrial fibrillation which was first detected 30 minutes after the injection of suxamethonium. Pulse rate at the wrist was 60 per minute, while the ventricular rate was 120 per minute. The patient was kept in bed and the fibrillation disappeared spontaneously after about 36 hours. Radial pulse settled at a regular rate of 64 per minute. The patient was asymptomatic through-

out, but following this episode further ECT was abandoned.

The fact that history and clinical examination failed to reveal any predisposition to the occurrence of atrial fibrillation in this patient suggests that the phenomenon represents an idiosyncratic response to suxamethonium.

I am grateful to Dr. F. A. Bleaden, Consultant Psychiatrist, St. John's Hospital, Lincoln, for permission to publish this case.

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This electrocardiogram was taken when the patient in the case described was fibrillating.

DANGERS OF FLUPHENAZINE

DEAR SIR,

A new drug is being widely used in the treatment of mental illness. It is long-acting and used by injection—its name is fluphenazine (Moditen). Is this the thalidomide of the 70's? I would like to have the opinion of other doctors. Whilst it is still new maybe we are lulled into a false sense of security, but are we justified in using a drug, which may take up to six weeks to eradicate from the tissues, without being sure of its safety? Its side effects alone are legion. A study of 13 papers gives the following:

Common side-effects reported are—lethargy, drowsiness, dizziness, muscular inco-ordination, paraesthesia, hypotension, blurring of vision, dryness of mouth, malaise, feelings of tension, confusion, nausea, vomiting and aches and pains.

Parkinsonism is extremely common. Incidence in reports varies from 100 per cent to 24 per cent with many reports around 50 per cent.

Depression is quite common and tends to be severe—5 suicides reported and two suicide attempts.

Other reported side-effects include psychotic relapse and glaucoma.

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