

cerebral ventricles in schizophrenia. As Dr Miller states, one or two studies have reported a weak correlation. By the same token, at least one study has reported an inverse correlation: that lateral ventricles somehow shrink as the illness progresses.

Overall, this pattern of findings suggests an occasional Type I statistical error, hardly surprising given the large number of studies now in existence and the many possible confounding factors. Even if Dr Miller were correct, it is still a big jump to suggest that progressive brain changes might be arrested by antipsychotic drug treatment. It would be equally reasonable to say that prolonged drug treatment were the cause. In any case, conscientious clinicians who already pay close attention to drug treatment will continue to do so, whichever view turns out to be right.

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#### 'The disease concept of alcoholism'

SIR: Professor Griffith Edwards has kindly commented on my review of Jellinek's monograph, *The Disease Concept of Alcoholism* (*Journal*, March 1991, 158, 431). Like him I have been unable to find any information that is not generally known about Dr E. M. Jellinek. As far as I am aware, after making enquiries, no biography of Jellinek has ever been written, although my understanding is that one is currently being undertaken in the USA. Mr Archer Tongue, a former Director of the International Council on Alcohol and Addictions, who has more knowledge of workers in the alcoholism field than most, was also unable to provide any further information.

It is interesting that Professor Edwards states that Jellinek's ideas really have to be seen as part of a continuing process, with which I would agree, although I myself am not familiar with the references he cites; but there is no doubt, as outlined in the excellent review by Levine (1978) that such concepts as "The disease concept of alcoholism", extended, particularly in the United States, throughout the 19th and 20th centuries, as has the continued debate as to whether alcoholism is best regarded as an illness or as deviant behaviour.

The comment that the alcohol dependence syndrome is a more socially acceptable form of the disease concept is not my original statement but the view of Heather & Robertson (1981) who state ... "therefore the alcohol dependence syndrome is a conception of abnormal drinking based primarily on

psychobiological dependence with impaired control as its leading symptom. With the substitution of loss of control for impaired control, how different is this from Jellinek's (1960) formulation?"

Despite this criticism, the alcohol dependence syndrome offers those of us working in this field a practical definition, linking alcohol dependence to drug dependence, and also giving us a model in which the dependence phenomenon is not all or none, but graded, a concept which has considerable importance, both in treatment goals and research into different populations varying in their level of dependence.

HEATHER, N. & ROBERTSON, I. (1981) *Controlled Drinking*. London: Methuen.

LEVINE, H. G. (1978) The discovery of addiction, changing conceptions of habitual drinkers in America. *Journal of Studies of Alcohol*, 39, 143-174.

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#### Double blind acceptance

SIR: I am certainly happy to accept that my position and that of Double (*Journal*, April 1991, 158, 573-574) do not now differ materially. He is right to point out that blindness is prone to be used as a buzz word emptied of its legitimate meaning, so that one should be happier about a study in which the investigators accept they cannot achieve blinding, than one in which blindness is claimed, but is illusory. (In the same way, the key concept of random allocation becomes vacuous when 'random' is taken to mean 'arbitrary' and no longer implies specific measures to minimise the possibility of bias being introduced by the admitting clinician.) The application of a method that will be as scientifically valid as possible in a real clinical situation, with the limitations which that inevitably introduces, remains the goal of study design. Scientific rigour in anywhere near an absolute sense is rarely attainable, and the editorial peer review process should regard a study report in which the limitations of the results are specified clearly as more acceptable than one in which the problems have been swept under the carpet, although inferable by a reasonable measure of experience-based lateral thinking.

The application to clinical and preventive practice, in the absence of clearcut results, is also a very important issue. Often one cannot simply ignore an issue: to do so is effectively to act as if the status quo were established. This applies whether the problem