hypofunction produced by dopaminergic blockers, clinically PS (Goetz *et al*, 1982). A decrease of striatal dopaminergic neurotransmission by neuroleptics increases sensitivity for the posterior appearance of TD determining the later localization since it affects the same substrate and implies a topographical correlation of both disorders (Garcia Ribera *et al*, 1985). Crane (1972) showed that 'tardive dyskinesia is more likely to develop in patients with pseudo-Parkinsonian symptoms than in patients not exhibiting such manifestations'.

In similar cases, such as L-dopa induced dyskinesias in patients suffering from idiopathic Parkinson's disease, the severity of both disorders is frequently not related without calling into question the coexistence of dopaminergic hyperfunction and hypofunction. In spite of the apparently opposite theoretical support, coexistence of both disorders could be the expression of two different moments in the same physiopathological process. Prospective studies are needed to clarify how and where both drug-induced disorders appear, disappear or shift.

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References

CARLSSON, A. (1970) Biochemical implications of Dopa-induced actions on the central nervous system, with particular reference to abnormal movements. In *L-Dopa and Parkinsonism* (eds Barbeau & McDowell). Philadelphia: F. A. Davis.

CRANE, G. E. (1972) Pseudoparkinsonism and tardive dyskinesia. *Archives of Neurology*, **27**, 426–430.

GARCIA RIBERA, C., RUIZ, I., GUILLAMAT, R., HORTAL, M., UDINA, C., ALVAREZ, E., CASAS, M. A therapeutic alternative in tardive dyskinesia. *Proceedings of the National Meeting of Biological Psychiatry* Oviedo-Asturias, 17–19 November 1985 (in press).

GOETZ, C. G., WEINER, W. J., NAUSIEDA, P. A., KLAWANS, H. L. (1982) Tardive dyskinesia: pharmacology and clinical implications. *Clinical Neuropharmacology*, **5**, 3–22.

Psychotherapy and Placebo

Sir: Professor Eysenck (*Journal*, May 1986, **148**, 610) says that I failed completely to understand his point. I feel that, on the contrary, it is he who has not understood mine.

I share Professor Eysenck's scepticism as to the 'specificity' of the various types of analytic psychotherapy. I have long thought that any positive or negative effects of the various types of psychotherapy have more to do with the 'non-specific' personal relationship between therapist and patient than with 'specific' factors inherent in the particular therapeutic technique employed — Professor Frank's 'shared therapeutic functions' in fact. That such factors are, to some extent, also operative in behavioural as well as analytic psychotherapy was suggested by the study of Sloane *et al* to which I referred. However, if it is the case, as Professor Eysenck states, that comparative studies show a clear superiority for behaviour therapy over placebos in general and the other psychotherapies in particular, then this is, of course, an important finding, which I never questioned.

The actual words of Professor Eysenck to which I referred and to which I objected were — 'Do we have the right to... get the State to pay us for treatment that is no better than a placebo?' My objections to this were threefold. Firstly, I wished to draw attention to the confusion that can occur in psychotherapy research because both the 'specific' treatment in question (psychotherapy) and the 'non-specific' placebo with which it is compared are both presumed to be psychological in their mode of action. Attention to the conceptual confusion surrounding the terms 'specific' and 'non-specific' in placebo theory has recently been drawn by Professor Grunbaum. His article supports my contention that the allotment of the various therapeutic factors to the categories 'specific' and 'non-specific' (or in his preferred terminology 'characteristic' and 'incidental') is purely arbitrary and relative to the theoretical standpoint of the investigator concerned. Unless we are clear about this Eysenck's statement that 'psychotherapy is no better than a placebo' could be reduced to the vacuous proposition 'psychotherapy is no better than psychotherapy.' I objected secondly because a great deal of psychotherapy is in fact done with little extra cost to the State, and thirdly because the phrase 'no better than a placebo' appears derogatory — especially insofar as people tend to associate placebos with inert pills. In his letter he says that 'placebo treatment is as successful as psychoanalysis and psychotherapy in general...' This at least implies acceptance of the fact that general psychotherapy does have some degree of success even if this is not quantitatively greater than that of placebos in general. Furthermore, the fact that the effects of psychotherapy may not be *quantitatively* greater than those of placebos in general does not, of course, imply that they are *qualitatively* identical. Few, for example, would exchange a therapeutic relationship (whether in a personal or a professional setting) for an inert pill even though both could be regarded as placebos insofar as they are 'non-specific' and psychological in their effects.

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References

FRANK, J. D. (1986) What is Psychotherapy? In *An Introduction to the Psychotherapies* (ed. Sidney Bloch) Oxford University Press.

GRÜNBAUM, A. (1986) The placebo concept in medicine and psychiatry *Psychological Medicine*, **16**, 19–38.

The Prevalence of Mental Illness Among People with Mental Handicaps

Sir: A recent publication of the Royal College of Psychiatrists (Wilkinson & Freeman, 1986) contains the following statements:

"Nearly one half of mentally handicapped children and adults suffer from associated psychiatric disorder. Among mental handicap hospital populations, the prevalence is between 30% and 60%" (p. 117).

'Several surveys indicate the prevalence of severe mental illness (psychosis) in mentally handicapped people is between 11% and 13% of hospital residents" (p 122).

The discussion which followed suggested a weight of opinion in favour of the first quotation.

Two years ago I summarised all the evidence I could find (over a dozen studies) on the subject (Ineichen, 1984). Most of those which gave a figure based on diagnosed mental illness, rather than vaguer measures such as 'disordered behaviour' clustered around the 10 to 14% mark. The highest figure of all was 58.8%, including only 15.8% severe cases which warranted 'continuous and perhaps intensive in-patient care from psychiatrists' while the rest require 'at the most occasional psychiatric specialist attention (Williams, 1971), the figure of 'up to 60%' has been in circulation for a decade or more. I am still waiting to find a single study which justifies its use.

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References

WILKINSON, G. & FREEMAN, H. F. (eds) (1986) *The Provision of Mental Health Services in Britain: The Way Ahead.* London: Gaskell (The Royal College of Psychiatrists).

INEICHEN, B. (1984) Prevalence of mental illness among mentally handicapped people: Discussion paper. *Journal of the Royal Society of Medicine*, 77, 761–765.

WILLIAMS, C. E. (1971) Study of the patients in a group of mental subnormality, 17, 29–41.

Violent Behaviour in Psychiatric Hospitals

Sir: I read with interest the paper by Pearson *et al* (*Journal*, August 1986, **149**, 232–235) and felt that the report of the high incidence of violent acts at mealtimes was worth further discussion. Phillips & Nasr (1983) noted a peak of violent incidents resulting in seclusion or restraint between noon and 2.00 p.m. and in a study of my own, incidents resulting in seclusion were about twice as common at mealtimes than at any other time. There was a tendancy for non-psychotic rather than psychotic patients to be involved in such incidents, but the type of incidents were no different from those occurring at other times.

Kinzel (1970) suggests that schizophrenic patients are prone to disturbed behaviour at times when they perceive their "body-buffer zone" being encroached upon, particularly if from behind, and Bigelow (1972) states that "the crowding of strangers, especially near such valued resources as food" may result in aggression.

I believe that study of the facilities used in psychiatric units for the serving and eating of food by patients may allow modifications to be made, so reducing the incidence of violence.

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References

BIGELOW, R. (1972) The evolution of cooperation, aggression and self-control. In *Nebraska Symposium on Motivation* (eds J. E. Cole & D. D. Jones) Lincoln: University of Nebraska Press.

KINZEL, A. F. (1970) Body-buffer zone in violent prisoners. *American Journal of Psychiatry*, **127**, 59–64.

PHILLIPS, P. & NASR, S. J. (1983) Seclusion and restraint and prediction of violence. *American Journal of Psychiatry*, **140**, 229–232.

Schizophrenia and Ethnicity

Sir: One of the most intriguing facts to emerge from the World Health Organization's International Pilot Study on Schizophrenia (World Health Organization, 1974) is that two-year outcome was the higher percentage of good outcome schizophrenics in Third World ountries as opposed to First World countries.

The CATEGO Class S Schizophrenia Study (*Journal*, December 1985, **147**, 683–687) is presently being analysed regarding outcome at two years. Preliminary findings indicate that the Xhosa schizophrenics in the sample have a better outcome at two years than the White schizophrenics, but the reason for this is not known at this stage. However, there are distinct differences in terms of cultural factors, attitudes to mental illness, compliance with maintenance medication, expressed emotion and living in extended families, which are of great importance in