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Psychological sequelae of torture

SIR: I was grateful to read your annotation on psychological sequelae of torture by Turner & Gorst-Unsworth (*Journal*, October 1990, **157**, 475–480).

Torture has been a widespread experience in the 20th century. Most probably there has never been a time when institutionalised torture has been so widely inflicted on large masses of people in all continents. After the military coup of 1980 in Turkey, I witnessed large numbers of victims of torture in Metris Military Prison, Istanbul, where I was imprisoned for one year (1982–1983). I would agree with the authors that torture has a wide variety of psychological effects on the victims, their families and friends. But psychological sequelae of torture cannot be limited to them, but should be extended to the large group of torturers who have been especially trained to torture.

The situation where individuals first have been forced, then slowly taught, to obey and then to enjoy human suffering, and to become professionally trained systematic torturers must be considered. Having had the opportunity to observe torturers, one cannot help feeling for those who have most probably in their turn been psychologically and physically abused. In Turkey, many turturers warrant diagnoses of psychiatric syndromes which have never been diagnosed or treated. Suicide rate, deliberate self-harm, alcohol dependency and possibly other drug misuse appears to be much higher in those individuals trained to be involved in torture. Different forms of psychotic episodes are commonplace and homicide rates among torturers are much higher than in the general population.

Torture has wide implications upon the whole society where its practice takes place. The society as a whole gets enmeshed into the idea of its existence, and fear and degradation is extended to all aspects of life. Now in Turkey torture has become a major theme in short stories, poetry, films, pictures and songs. In the last ten years there have been hundreds of poetry books, short stories, paintings and films on the tortured, the torturers and their circumstances. It has become part of the language and culture and almost a way of communicating. Its existence transcends all boundaries and makes itself felt in all aspects of life.

In my out-patient clinic at the Charing Cross Hospital, Turkish immigrants who have never experienced torture come with stories of ill-treatment as their psychological complaints. Both neuroses and psychoses in these people are flavoured with stories of torture, sufferings and horror. The idea of torture, even if they know little about it, has become an expression of their persecutions, anxieties, racist and sexist experiences. It is a component of their guilt, self-pity and hopelessness. The individual and the whole society has been marred by the psychological effects of torture.

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British and Australian depression revisited

SIR: Two recent long-term outcome studies of depression, from Sydney (Kiloh *et al*, 1988; Andrews *et al*, 1990) and London (Lee & Murray, 1988; Duggan *et al*, 1990), have shown similar results. The initial diagnosis often heralded a very poor long-term outcome, and personality disturbance was one part of the explanation for this. However, there are important differences between the Sydney and London findings which we wish to highlight.

The first of these concerns the neuroticism (N) subscale of the Eysenck Personality Inventory (EPI). The EPI N now has an excellent pedigree as a predictor of outcome in depression, but Professor Andrews *et al* found its predictive power to be confined to their subgroup of 'neurotic' depressives. This may encourage readers to re-identify raised N with a diagnosis of 'neurotic' depression, which would be unwise. In both series, N scores do not differ between 'neurotic' and 'endogenous' subtypes, so that whatever separates the 'neurotic' from the 'endogenous' depressive, it is not the degree of neuroticism. In London, unlike Sydney, we found that EPI N predicted chronicity particularly in the 'endogenous' (melancholic) subgroup. We therefore propose that the relationship between high N scores, diagnostic subgroup, and outcome should remain open to further investigation.

A second difference concerns the influence of depressed mood on the assessment of personality. Andrews *et al* apologise that their patients were assessed when they still had symptoms, and argue that 'recovered' personality has the more significant

influence on outcome. We were able to compare the effect of personality estimated when ill and again on recovery and found that the former was the more important. This is in agreement with findings from cognitive psychology, where significant differences in dysfunctional attitudes between depressives and normal controls are found only in the presence of depressed mood. Thus it may be that self-ratings of personality are more valuable predictors when an individual is in a state of depression rather than on recovery, and that the changes produced by the depression are precisely those which offer most information for the future.

Our third point of difference concerns diagnosis. Professor Andrews *et al* claim to show that 'endogeneity' in depression has at most only a trivial effect on long-term outcome. However, this conclusion depends heavily on their definition of 'endogenous' depression – there are at least 18 different definitions and the degree of overlap between them is only partial. We have compared the prognostic value of several of these in our Maudsley series and have found (a) that differing definitions of 'endogenous' depression include different populations and (b) DSM–III melancholia emerges as the best predictor of poor long-term outcome (Duggan *et al*, 1991). Thus, it may be premature to dismiss the importance of the endogenous/nonendogenous distinction, as Professor Andrews *et al* have done.

Finally, the Sydney group continue to misrepresent our earlier papers. In London, 'endogenous' scores on Kendell's continuum predicted a much poorer outcome. Despite several appeals, it was the swift hare, not the slow tortoise, that lost Aesop's race.

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Urinary chromatographic profiles in psychiatric diseases

SIR: In 1980 Trygstad *et al* reported that patients with a variety of psychiatric diseases could be distinguished from each other and from normal controls by the pattern of peptides excreted in urine as studied by chromatography. Specifically, it was claimed that when urine was precipitated by ethanol saturated with benzoic acid, centrifuged at 4000 *g* for 10 minutes and the precipitate washed with ethanol, dissolved in ammonium bicarbonate and applied to a Sephadex G-25 column, the following distinctions in ultraviolet absorption profile could be made. Firstly, patients with unipolar and bipolar depression had patterns which differed from normal subjects and patients with neurotic depression, and these profiles were normalised by tricyclic antidepressant medication. Secondly, patients with hebephrenic or paranoid schizophrenia differed from normal subjects in demonstrating one or other of two chromatographic profiles characterised by an excess or deficit of peaks of absorption seen at elution volumes between 1200 ml to 1400 ml. Thirdly, patients with autism had excretion profiles which were similar to those seen in schizophrenia, but which could be distinguished from these latter on further (unspecified) fractionation steps. Fourthly, patients with hyperkinetic syndrome (minimal brain dysfunction) could be shown to have a distinct, but variable, pattern of urinary peptide excretion which returned to normal with amphetamine treatment.

These and related claims were made in a series of papers (e.g. Reichelt *et al*, 1981, 1985, 1986) by the same group of workers.

To assess these claims we have conducted an investigation of urine samples from five neuroleptic-free patients with schizophrenia (DSM–III criteria) and four age- and sex-matched normal subjects, using Sephadex G-25 and Biogel P-2 chromatography with techniques as described by the Norwegian group and a series of modifications (Gilroy *et al*, 1990). In the course of our investigation a number of difficulties with these techniques became apparent. In communication with the Norwegian workers, we have attempted to clarify the nature of the difficulties, and to further specify the precise procedures adopted. Details of these technical problems are given in our paper (Gilroy *et al*, 1990).

Our findings differ from those of the Norwegian group. We observe urinary chromatographic profiles which do not closely resemble those which they have reported. We found substantial differences in excretion profile between men and women on Sephadex G-25, but no significant differences (in our