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neuronal circuits utilise glutamate for neurotransmission. We are continuing to measure these metabolites in a 1.5 cm^3 volume encompassing the left anterior cingulate with MRS at 4.0 Tesla in healthy volunteers and never-treated patients with schizophrenia. The comments by Feinberg & Guazzelli regarding dreaming prompted us to examine our 4.0 Tesla MRS data with respect to the sleep/wake state of the healthy volunteer control subjects. In total, 15 healthy volunteers have been scanned to date. Of these subjects, two could not be reached. Each of the remaining subjects were asked whether they had slept during the spectroscopy study. Of these subjects, six remembered sleeping at some point during the study, six did not sleep, one person could not remember. Comparison of metabolite levels (mean (s.d.)) between subjects who remembered sleeping (n=6) and those who did not sleep (n=6) showed a significant increase in glutamine levels (9.9 (2.7) *v.* 6.1 (1.7), P=0.02) in subjects who remembered sleeping, using a two-tailed t-test. The relative glutamine increase in the subjects who reported that they were asleep was comparable to the increase previously observed in patients with schizophrenia (Bartha *et al*, 1997).

Since it is highly likely that subjects who were sleeping during data acquisition were dreaming, the increased levels of glutamine in these subjects may be due to increased glutamatergic activity in the anterior cingulate, part of the basal ganglia–thalamocortical circuit implicated by Feinberg & Guazzelli in schizophrenia. The similarity of findings between healthy subjects who were likely to be dreaming and patients with schizophrenia highlights the need for further investigation of these regions in schizophrenia.

Bartha, R., Williamson, P. C., Drost, D. J., *et al* (1997) Measurement of glutamate and glutamine in the medial prefrontal cortex of never-treated schizophrenic patients and healthy controls using proton magnetic resonance spectroscopy. *Archives of General Psychiatry*, **95**, 464–473.

Feinberg, I. & Guazzelli, M. (1999) Schizophrenia – a disorder of the corollary discharge systems that integrate the motor systems of thought with the sensory systems of consciousness. *British Journal of Psychiatry*, **174**, 196–204.

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Quick rating of depressed mood

Sir: I would like to comment on McKenzie & Marks' (1999) claims about some characteristics of their rating scale.

First, test–retest reliability was assessed by comparing self-rated single item (D1P) on its own with ratings presented in the Fear Questionnaire, one given immediately after the other. Although assessments were made at six different visits, repeated measures analysis was not used.

As the bias introduced seems substantial (e.g. lack of random order, recall, practice) the resulting distortion of estimates may be large. Additionally, the construct's time instability means that the range included the whole spectrum of values, resulting in a further inflation of Pearson's r. It is not surprising that Pearson's r was unity or near unity at each visit.

Second, they indicate that Pearson's $r=-0.54$ between D1P at discharge, and self-rated percentage improvement has predictive merit. However, 71% of the variability in self-rated percentage improvement is unexplained by D1P at discharge, and the reduction in errors of prediction is only 16% when using this information. They also conclude that pre-treatment D1P scores predict a binary category (drop-outs with low mood given as referral reason/otherwise) by referring to different means: 6.7 (drop-outs) and 3.7 (non-drop-outs). However, most pre-treatment D1P scores will not predict membership to either subset simply because of the substantial distribution overlap (s.d.=1.83 and 2.47, respectively). The positive skewness of non-drop-outs would not invalidate the latter statement as it did not seem to have seriously violated the t-test's assumptions. I conclude that D1P's predictive validity is rather low.

Third, concurrent validity was assessed by comparing D1P with D1C (same item completed by clinician), the 21-item Beck Depression Inventory (BDI), and a 'true' depression factor in the BDI (factor analysis). However, the authors used Pearson's r, a technique that does not measure agreement.

They also report a regression line with an intercept of 1.07 and a slope of 0.71, and claim interval correspondence between the BDI and D1P. Interestingly, as these values indicate no equivalence (i.e. intercept not 0, slope not 1) it could be concluded that there is lack of agreement between the scales. However, this argument is flawed because measurement error is not considered. As linear regression does not often give single values for error, it is difficult to assess comparability using this technique.

It is unfortunate that the authors have not used appropriate methods to assess agreement between measurement instruments, such as the well-known techniques proposed by Altman & Bland (1983).

Altman, D. G. & Bland, J. M. (1983) Measurement in medicine: the analysis of method comparison studies. *Statistician*, **32**, 307–317.

McKenzie, N. & Marks, I. (1999) Quick rating of depressed mood in patients with anxiety disorders. *British Journal of Psychiatry*, **174**, 266–269.

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Authors' reply: Marchevsky noted Altman & Bland's (1983) suggestions to compare two methods of measurement. These have some merit but do not change our conclusions.

Test–retest reliability was studied by comparing the D1P on its own and as one of 23 questions embedded within the Fear Questionnaire; presented at the same time were problems and targets, work/social adjustment and the Compulsion Checklist (Marks, 1986). Apart from the Compulsion Checklist all the items were rated on a nine-point scale. The order in which the two D1P modes and the other scales were rated varied (albeit non-randomly) across subjects. It thus seems unlikely that there were order effects, or that bias was introduced by practice or recall, since that would require recall of one among nearly 40 similar ratings. Not only Pearson's r but also Cohen's kappa, a chance-corrected measure of agreement, showed values close to unity.

Dr Marchevsky questions the use of Pearson's r and beta from linear regression. These equal one another (0.71 in the instance he quoted). One might debate how much an r measure of association also measures agreement. However, that r represents the degree to which variability in one

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measure is attributable to variability in the other is not disputed and we interpreted it thus in our discussion. We used the regression line to work out correspondence with recognised cut-off points on the BDI taken as a 'gold standard'. This enabled us to identify clinically important thresholds and then calculate sensitivity and specificity.

Yes, D1P does not strongly predict outcome of behaviour therapy, nor does the BDI. This does not invalidate the D1P's utility as a reasonable rough guide to mood during treatment of anxiety disorders.

Altman, D. G. & Bland, J. M. (1983) Measurement in medicine: the analysis of method comparison studies. *Statistician*, **32**, 307–317.

Marks, I. M. (1986) *Behavioural Psychotherapy: Maudsley Pocket Book of Clinical Management*. Bristol: Wright.

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The difficult patient – Schreber revisited

Sir: Hinshelwood (1999) proposes that if the care-giver concentrates on understanding his or her countertransference to the difficult patient, this may overcome obstacles in treatment.

There are theoretical difficulties with this psychodynamic approach. Hinshelwood himself has alluded to the potential for coercion in the intersubjective dialogue (Hinshelwood, 1997). This is the case when one of the two subjects involved – the care-giver – is the expert or "the one who knows" ("*le sujet supposé savoir*"; Lacan, 1964). Within mainstream psychiatry, trenchant opposition to the coercive possibility in psychoanalysis is consistently articulated (Clare, 1994). Many psychiatrists believe that there can be no collaborative starting point between psychiatrists and psychoanalysts in patients with severe mental illness.

Faced with this impasse, in re-focusing attention on Schreber, Hinshelwood indicates the way forward, but does not go far enough. Echoing Freud in his 1911 study of the Schreber case, it is my opinion that, rather than countertransference issues, it is the discourse of the person with psychosis that must be prioritised. Schreber himself was a very disturbed individual with paranoid schizophrenia. Despite the bizarre and seemingly senseless character of his delusions, they represented a mythic reconstruction giving meaning to his disastrously altered world. Although normal subjects differ crucially from those with psychosis, the notion of a psychotic mythic reconstruction is not outlandish, given that normal subjects also mediate experience through construction of a myth – the more acceptable neurotic myth (Lacan, 1953).

Schreber demonstrates that the discourse of someone with psychosis is an organised, logically driven process. This logically structured discourse often dictates its own imperatives, sometimes to the detriment of the individual with psychosis or others. This is the phenomenon of *passage-à-l'acte*, where the person with psychosis acts in a sudden, often violent and seemingly unintentional way. Such acts are symbolically determined. It is my argument that because the patient's psychotic structure remains unchanged despite neuropharmacological intervention, psychoanalysis has a place in ameliorating the expression of the myth that drives him. Equally, one must accept that few patients with psychosis would consider – or tolerate – involvement with a psychoanalyst. Despite this, we must set up the conditions that encourage this small minority to engage analytically. This requires dialogue between psychiatrists and psychoanalysts whose views on the treatment of psychosis are not irreconcilable.

Clare, A. (1994) Training in psychoanalytic psychotherapy: the psychiatric trainee's perspective. *Irish Journal of Psychological Medicine*, **12**, 57–59.

Hinshelwood, R. D. (1997) *Therapy or Coercion? Does Psychoanalysis differ from Brainwashing?* London: Karnac Books.

— (1999) The difficult patient. The role of scientific psychiatry in understanding patients with chronic schizophrenia or severe personality disorder. *British Journal of Psychiatry*, **174**, 187–190.

Lacan, J. (1953) The neurotic's individual myth. *Psychoanalytic Quarterly* (trans. M. N. Evans, 1979), **3**, 405–425.

— (1964) *The Four Fundamental Concepts of Psychoanalysis*, pp. 230–243. Reprinted 1987 (trans. A. Sheridan, 1977). Harmondsworth: Penguin Books.

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EURODEP study

Sir: The desire to join up dots on graphs must be tempered by reason. In the EURODEP study reported by Copeland *et al* (1999) figures 8 and 9, of depression prevalence by age group, are interpreted as showing "no overall tendency to rise or fall with age, except in extreme old age". Differences in prevalence of any condition between age groups cannot be regarded as functions of 'rising' or 'falling' without repeated measurement in cohorts; joining up dots in cross-sectional surveys encourages almost unconscious assumptions, also manifest in the discussion of the relationship between depression and physical illness. Cohort effects are at least mentioned in another paper by the same authors in the same issue (Prince *et al*, 1999) but, for obvious reasons, care must be used in the use of words such as 'increasing' and 'age effects' which are so easily misunderstood to mean ageing itself.

Copeland, J. R. M., Beekman, A. T. F., Dewey, M. E., et al (1999) Depression in Europe. Geographical distribution among older people. *British Journal of Psychiatry*, **174**, 312–321.

Prince, M. J., Beekman, A. T. F., Deeg, D. J. H., et al (1999) Depression symptoms in late life assessed using the EURO-D scale. Effect of age, gender and marital status in 14 European centres. *British Journal of Psychiatry*, **174**, 339–345.

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Author's reply: Professor Macdonald is right to say that tendencies over time can only be mapped on longitudinal as opposed to cohort studies. Therefore, it would have been more accurate to say that the prevalence of depression varied in the different age groups of the cohorts, older ages not being associated with higher levels of depression.

Clinicians are interested in whether they are missing depression in older patients, and the mistaken public perception still remains that older people are by nature miserable, so prevalence levels by age are not unimportant. Because large samples will be necessary for longitudinal studies of the very old because of the high death rates, we are planning to explore the longitudinal characteristics of the EURODEP samples.

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Psychosis associated with gonadorelin agonist administration

Sir: Puerperal and menstrual psychoses have been associated with hormonal fluctuations