

of the most severely affected patients have impaired decision-making skills, they form a minority (Grisso & Appelbaum, 1995). Most psychiatric patients (including in-patients) are perfectly capable of making decisions regarding treatment and other areas of their lives. It does not help the cause of reducing stigma for our patients to suggest that they cannot make such decisions.

Just as not all psychiatric patients lack capacity, not all medical patients have capacity. This particularly applies to in-patients in whom factors such as cognitive impairment and delirium can affect the ability to make decisions. A recent survey of medical in-patients found that mental incapacity was a very common problem, and one that was frequently overlooked by medical staff (further details available from V.R. upon request). These patients are particularly vulnerable to medical paternalism if this problem is not recognised and appropriately managed.

We agree with Sarkar & Adshear's call for a code of ethics for British psychiatry, and hope that it will address this difficult area of incapacity. Incidentally, we are also watching with interest the progress of the draft Mental Incapacity Bill. However, we suggest that this area requires careful scrutiny not because psychiatry is a 'special case' but because these issues affect all health care professionals. In this way we could help to lead the way for our non-psychiatric colleagues rather than concentrating on our differences.

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Debate on neurosurgery

The debate on the future of neurosurgery for psychiatric disorders (R. Persaud/D. Crossley & C. Freeman, 2003) is curious in many ways. Much of the criticism of neurosurgery still relies upon its historical excesses (Pressman, 1998) rather than the contemporary caution. The 'lack of evidence' argument sets up an unrealistic standard that most surgical treatments are

unable to meet. The 'progress in psychiatric treatments' argument fails to recognise that recent drug treatments are but incremental advances over drugs that have been around for some decades, and there are many patients who continue to suffer chronically from depression, obsessive-compulsive disorder and other illnesses. For those of us who practise in tertiary referral centres, encounters with their suffering are frequent and heart-wrenching. Do we wish to take away all their hope?

I am not arguing for a return to the past. Modern neuroscience is fast removing, in a practical sense, the distinction between brain and mind. It is now quite acceptable to consider neural transplants, gene therapy and neural prosthetics as neuropsychiatric treatments. Is this not the right era to revisit surgical interventions on the brain? We are already excited about developments such as vagus nerve stimulation and deep brain stimulation for psychiatric disorders (Malhi & Sachdev, 2002). We are quite comfortable with ablative surgery for epilepsy when there is functional disturbance, even in the absence of structural abnormality. The neuro-anatomical models of psychiatric disorders are becoming increasingly sophisticated (Mayberg, 2001). Should we not be working towards a new era of direct brain intervention, with surgery being an important aspect of this strategy? This surgery may or may not be ablative, or follow an initial period of brain stimulation, or be guided by sophisticated functional imaging. If deep brain stimulation, for example, is demonstrated to produce a therapeutic response without adverse effects, but only temporarily, would there not be an argument to proceed with focal ablation? The brain is, after all, not inviolable, and the evidence is convincing that focal and targeted brain lesions can spare both intellect and personality.

The answer to the question, 'should neurosurgery for mental disorder be allowed to die out?' is surely, 'Definitely not'. Let us, however, move towards a new neurosurgery that is bold but not misinformed, and that keeps abreast of the developments in our understanding of brain function.

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Cognitive-behavioural therapy for psychosis

Like a magician pulling a rabbit from his hat, Turkington draws a positive result for cognitive therapy for schizophrenia from the literature – only for McKenna to put it back in again (Turkington/McKenna, 2003). Does it exist or not? McKenna's arguments and table look convincing as, by excluding any study that does not have an active control, he reduces the number of studies he considers. But would he do the same for studies of anti-psychotic medications? Or does he assume that patients, and raters evaluating patients, can detect no difference between taking, for example, placebo and haloperidol, or even haloperidol and olanzapine? In which case why are we giving them so much of the latter?

But even focusing only on the studies that he finds acceptable, he dismisses one (SoCRATES; Lewis *et al*, 2002) for having a positive effect over active control on auditory hallucinations (oh, for a drug that had such an effect over and above those currently available!) and another (Sensky *et al*, 2000) where a differential benefit of cognitive-behavioral therapy over befriending only became apparent 9 months after therapy ended. He completely omits other widely cited studies with active placebos and positive effects (e.g. Drury *et al*, 1996). He then does an unusual meta-analytic exercise in dismissing two small pilot studies by weighing them against each other and finding them to cancel out. Other meta-analyses (e.g. Pilling *et al*, 2002) using more conventional methodology have concluded differently and, fortunately, so has the National Institute for Clinical Excellence.

The rabbit exists and is multiplying rapidly (e.g. Durham *et al*, 2003).

Declaration of interest

D.K. has published books and gives workshops on cognitive-behavioural therapy for schizophrenia.

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Author's reply: Actually, the study of Durham *et al* (2003) which was carried out under blind conditions failed to find a significant advantage for cognitive therapy over active placebo. The authors state that 'Repeated measures analyses of variance were first conducted with three levels of treatment (CBT *v.* SPT *v.* TAU) and three time points (baseline, post-treatment, follow-up). There were significant effects for time for all variables except the GAS but no significant time \times treatment interaction effects or contrasts for any of the measures'. This was for 'Changes in severity from baseline', with an essentially similar finding for 'Clinically significant improvement'.

Durham, R. C., Guthrie, M., Morton, R. V., et al (2003) Tayside–Fife clinical trial of cognitive-behavioural therapy for medication-resistant psychotic symptoms. Results to 3-month follow-up. *British Journal of Psychiatry*, **182**, 303–311.

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Personality assessment

In their description of the Standardised Assessment of Personality – Abbreviated Scale (SAPAS) Moran *et al* (2003) write that, to the best of their knowledge, only two other interviewer-administered screens for personality disorder have been published. I would like to draw attention to a third, the Personality Structure Questionnaire (PSQ) (Pollock *et al*, 2001), which consists of eight bipolar items scored 1–5 and is similarly quick to administer and to score. The scores of four clinical and four non-clinical samples are reported in the paper. Two samples of patients meeting diagnostic criteria for borderline personality disorder had mean scores of over 30, whereas the non-clinical samples scored between 19.7 and 23.3. Scores on the PSQ were shown to correlate with a number of measures of multiplicity, dissociation and identity disturbance.

Most of the items on the questionnaire describe the respondent's awareness of a discontinuous sense of self. This reflects the multiple self states model of borderline personality disorder (Ryle, 1997a), in which alternations in the operation of recognisable, discrete self states, each with a characteristic mood, sense of self and mode of relating to others, are seen to account for much of the experience and confusion of patients and of those treating them. The PSQ is similar to the SAPAS in being a screening, not a diagnostic instrument. It differs in that it focuses on the specific feature of self state instability typical of Cluster B disorders. This can be an advantage in that these patients present the greatest difficulty to clinicians. By drawing attention to this characteristic the PSQ can initiate further enquiry leading to the detailed description of an individual's self states and state switches, which can provide a basis for management and treatment directed towards personality integration (Ryle 1997b).

Moran, P., Leese, M., Lee, T., et al (2003) Standardised Assessment of Personality – Abbreviated Scale (SAPAS): preliminary validation of a brief screen for personality disorder. *British Journal of Psychiatry*, **183**, 228–232.

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Ryle, A. (1997b) *Cognitive Analytic Therapy and Borderline Personality Disorder: the Model and the Method*. Chichester: J. Wiley & Sons.

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Management of post-concussion syndrome

In his editorial King (2003) gave an excellent overview of the post-concussion syndrome, an area of neuropsychology and psychiatry that is fraught with difficulty and controversy. King pointed out that both biological and psychological factors are at play in post-concussion syndrome. Of great importance was his integration of time since injury into a model and outlining 'windows of vulnerability' for the development of symptoms. It is likely that most clinicians treating patients with post-concussion syndrome will find this model of real value for understanding and possibly preventing some of the difficulties resulting from the syndrome.

King rightly pointed out the need for studies investigating treatment and management of post-concussion syndrome. New and future research findings now need to be incorporated into King's model. For example, Ponsford *et al* (2002) in a randomised controlled trial found that the provision 1 week post-injury of an information booklet to patients who suffered a mild head injury reduced anxiety and reporting of ongoing problems at 3 months post-injury. Against a background of 'windows of vulnerability' for the development and maintenance of symptoms, providing written information to patients in addition to the early interventions reviewed by King can further improve outcome in post-concussion syndrome.

A recent example identifying a potential lack of evidence for an intervention perhaps also needs mentioning. De Kruijk *et al* (2002) investigated the effect of bed rest on outcome following mild traumatic brain injury. Bed rest has been recommended as an intervention to improve outcome following head injury; however, the effectiveness of this intervention has not been investigated. De Kruijk and colleagues did not find significant differences in outcome between their