Correspondence

EDITED BY STANLEY ZAMMIT

In defence of the case report

During his ten years as Editor, Greg Wilkinson worked hard to produce a modern, polished journal with an impressive international reputation. In his valedictory editorial (Wilkinson, 2003) he sets out the goals he has pursued. Almost by definition, an editor cannot receive universal approbation. However, while I suspect that academic/research colleagues will have been happy with his stewardship, many clinicians are likely to have some reservations. The reason for this will be found in three lines in the middle of his final editorial: 'I hastened the demise of the case report, to exclude what I see as psychiatric trivia. I published original research . . .'.

This is a cameo of the polarity that exists between academic, research-oriented psychiatrists and those clinicians who provide the bulk of the service in the National Health Service. They confirm the contemporary ethos that the only worthwhile form of study is that of groups. The nomothetic approach takes precedence while the detailed study of an individual patient is marginalised as trivia.

Psychiatry is not unique in having been seduced by the scientific process. Unfortunately, it is doubtful how much the practice of our discipline has gained from this development (Shooter, 2003; Wilkinson, 2003). This is not surprising. Psychiatry is a discipline in which the information is 'soft' and much of it subjective. In contrast, the scientific approach insists that any parameter of illness that cannot be measured in terms of hard data is suspect.

As academic psychiatrists have become more influential within the profession and training is more university based, research and related activity are seen as the acme of psychiatric work. Working closely with patients and creating enduring therapeutic relationships is not valued and is sometimes seen as drudgery. This is a damning paradox. Is it surprising that it is hard to recruit into psychiatry – a specialty that is dismissive of the very core of its professional ethic?

Psychiatry needs to return to its core values (Simms, 2003). It needs to place the care and treatment of the individual patient centre-stage. Students, young doctors and psychiatric trainees must see at first hand the fascination and reward of working with patients, and see that the work is attractive and satisfying. A part of this process must be the rehabilitation of the detailed case report.

Shooter, M. (2003) On Pushto, principles and passion: just what is an advance in psychiatric treatment? *Advances in Psychiatric Treatment*, **9**, 239–240.

Simms, A. (2003) 'Back to basics': on not neglecting the elementary in continuing professional development. *Advances in Psychiatric Treatment*, **9**, 1–2.

Wilkinson, G. (2003) Fare thee well – the Editor's last words. *British Journal of Psychiatry*, **182**, 465–466.

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In defence of specialist mental health care trusts

Psychological medicine is an interesting way to describe an attractive field (Lloyd & Mayou, 2003). The patients are generally interesting and engaging, the work is usually consensual and professionally rewarding. However, when mental health services are attached to general hospitals, liaison psychiatry is merely one of an array of specialties competing for funds. Commissioners may find that more acute, high-profile services that are better supported by the public take priority when it comes to the annual funding round.

A failure to secure sufficient funds in this situation can lead to psychiatric wards and facilities appearing neglected and shabby compared with general medical wards in the same hospital. When coupled with a staff that is liable to feel undervalued, the quality of care can suffer and the stigma of mental illness is compounded.

The appearance of specialist trusts in many different areas of medicine should allow the strategic, systematic development of a comprehensive range of specialist services. Lloyd & Mayou should welcome the opportunity to develop their field in such a focused setting along with other psychiatrists with different interests. By seeking to 'make itself [liaison psychiatry] more acceptable to medical colleagues' they could be distancing themselves from the 'psychotic patients [historically] housed in large asylums'. These are the very patients that suffer the greatest amount of stigma and social exclusion, that form the bulk of most psychiatrists' case-loads and that are the least visible to general hospitals.

All psychiatrists should have the opportunity to develop their skills by caring for this group of patients as part of their training. It would be a pity if the views of Lloyd & Mayou were taken to their logical conclusion and 'psychological medicine' divorced itself from mainstream psychiatry and sought to become recognised as a sub-specialty with our esteemed colleagues at the Royal College of Physicians.

G. G. Lloyd & R. A. Mayou (2003) Liaison psychiatry or psychological medicine? *British Journal of Psychiatry*, **183**, 5–7.

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Mental incapacity and medical ethics

With reference to the editorial by Sarkar & Adshead (2003), we are pleased to see this area of discussion being raised. However, we wish to make a couple of additional points relating to capacity.

We appreciate that a psychiatrist's ability to override a competent refusal raises particular ethical dilemmas and it is right that this should be highlighted for attention. However, we felt that other points in the section 'Psychiatry as a special case' could, and do, apply to many non-psychiatric patients, particularly those with acute medical illness.

The authors assert that 'The most significant difference between medicine and psychiatry lies in the relative incapacity of psychiatric patients to make decisions for themselves'. Although it is true that some

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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 & Adshead'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.

Grisso, T. & Appelbaum, P. S. (1995) The MacArthur Treatment Competence Study. III: Abilities of patients to consent to psychiatric medical treatments. *Law and Human Behavior*, **19**, 149–174.

Sarkar, S. P. & Adshead, G. (2003) Protecting altruism: a call for a code of ethics in British psychiatry. *British Journal of Psychiatry*, **183**, 95–97.

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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 neuroanatomical 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.

Malhi, G. S. & Sachdev, P. (2002) Novel physical treatments for the management of neuropsychiatric disorders. *Journal of Psychosomatic Research*, **53**, 709–719.

Mayberg, H. (2001) Depression and frontal–subcortical circuits: focus on prefrontal–limbic interactions. In *Frontal–Subcortical Circuits in Psychiatric and Neurological Disorders* (eds D. G. Lichter & J. Cummings), pp. 177–206. New York: Guilford Press.

Persaud, R./Crossley, D. & Freeman, C. (2003) Should neurosurgery for mental disorder be allowed to die out? (debate). *British Journal of Psychiatry*, **183**, 195–196.

Pressman, J. D. (1998) *Last Resort: Psychosurgery and the Limits of Medicine*. Cambridge: Cambridge University Press.

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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 antipsychotic 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).

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