Correspondence

EDITED BY STANLEY ZAMMIT

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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 much of it subjective. In contrast, the scientific approach insists that any parameter of mental illness is compounded.

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 it self [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.

Mental incapacity and medical ethics

With reference to the editorial by Sarkar & Adshed (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...
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 & Adshede’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 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 be not 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.


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 x 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’.

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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).


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 Kruij 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 Kruij and colleagues did not find significant differences in outcome between their
bed rest and no bed rest groups at 3 months post injury. However, they concluded that bed rest might have some palliative effect during the initial weeks following injury.

New and future research findings on management, integrated into King’s model, can potentially enhance the prevention of chronic symptomatology developing in post-concussion syndrome. This might also inform our understanding of cases where post-concussion symptoms persist beyond 1 year following injury.


One hundred years ago

Review of Hypnotism, its History, Practice, and Theory.


“The author” of this book, we are told by the publisher, “has devoted the last 12 years to hypnotic practice and research and his personal observations of the practical work done in France, Germany, Sweden, Holland, Switzerland, and Belgium should also make the volume a valuable addition to the science of a subject which is exciting much interest at the present time.” In the introductory chapter the author tells us that in the course of the volume he proposes “to refer not only to his own hypnotic 12 years’ practice and research but also to give such a general account of the subject as can be brought within reasonable compass.” Examining Dr. Bramwell’s book in the light of his own account of his intentions we doubt if he has fulfilled the promises made. One chapter is devoted to the early history of hypnotism; it is short and very incomplete; for example, the account given of Mesmer is fragmentary. The work of Elliotson, of Esdaile, and of Braid is more fully treated. The section on the later History of Hypnotism contains an account of the practice of Dr. A. A. Liebeault of Nancy. A section follows entitled “History of My Own Practice.” The remaining part of the book is devoted to dissertations on the methods of inducing and terminating hypnosis, susceptibility to hypnosis and the causes which influence it, the experimental phenomena of hypnosis, and the management of hypnotic experiments and an account of the “different stages of hypnosis.” The therapeutic use of hypnotism is dealt with in chapters on hypnotism in medicine and surgery and in one on the management of medical and surgical cases. This is followed by a disquisition on hypnotic theories which occupies 150 pages, or nearly a third of the whole work. A chapter is devoted to “the so-called dangers of hypnotism” and this is followed by a summary, conclusion, and list of references. Of the two appendices one is the late Dr. Williamson’s account of the origin of Braid’s work and the other is a note on spiritualism, clairvoyance, and telepathy.

The general effect produced by this book is a feeling of wonder that such a subject, with all its suggestions of mystery and occultism bred of our imperfect knowledge, can have led to the production of so unstimulating a book. Doubtless Dr. Bramwell was particularly right to treat his subject very seriously, but he might have taken more steps to arouse and to hold our interest. The work would gain much if it were rearranged and rewritten so that the history of the subject and the theories of its various exponents were placed together. For example, the life of Esdaile is given in one place, while his theories will be found in another. The author’s views naturally pervade the book, but while lessons from personal experience are of the utmost value in all medical treatises, an author should beware lest the actors of the past should appear merely as puppets in his play. Dr. Bramwell is, we are sure, unaware how strongly the impression is produced that views contrary to his own are erroneous and misleading.

REFERENCE


Corrigendum

Disintegration of the components of language as the path to a revision of Bleuler’s and Schneider’s concepts of schizophrenia. Linguistic disturbances compared with first-rank symptoms in acute psychosis. B.J.P., 182, 233–240. Summary (p.233), Results should read: Strong positive correlations were found between the CLANG factor ‘poverty’ and first-rank delusions of control and between semantic/phonemic paraphasias and verbal auditory hallucinations. Language disturbances were superior to nuclear symptoms in discriminating ICD–10 schizophrenia from other psychoses.
In defence of specialist mental health care trusts
A. Sharma
BJP 2004, 184:84.
Access the most recent version at DOI: 10.1192/bjp.184.1.84-a

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