Mental health and incapacity legislation

I enjoyed the article by Dawson & Szmukler (2006) because I like to keep up to date with legal and ethical issues in mental health. However, their claim for equivalence between mental and physical diseases sits uneasily with scientific papers published in the Journal. Shaw et al (2006) found that schizophrenia had a prevalence of 5% in perpetrators of homicide, compared with 1% in the general population. I would love to see comparable figures for the prevalence of hypertension, multiple sclerosis, leprosy etc., but meanwhile we have a problem. The Ritchie report on the inquiry into the care of Christopher Clunis reveals capacity’s dark side by showing how psychiatrists repeatedly brought a patient to the point at which he could make his own decisions, then left him to fend for himself (Ritchie et al, 1994). Perhaps the best way for services to reduce the stigma and discrimination associated with psychiatric illness is to reduce the 5% figure? Somehow, I cannot see capacity-based legislation playing a lead role in achieving that objective.


A. Maden Department of Forensic Psychiatry, Academic Centre, West London Mental Health NHS Trust, Southall, Middlesex UB1 3EL, UK. Email: a.maden@ic.ac.uk
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Psychiatric patients can be treated involuntarily even if they possess the mental capacity that would render the involuntary treatment of a medical patient illegal. Dawson & Szmukler (2006) describe this as a form of discrimination and propose that the relevant legislation be ‘fused’ so that, like medical patients, most psychiatric patients could be treated involuntarily only if they lacked mental capacity. I see a number of advantages to using mental capacity as a legal criterion (Buchanan, 2002, 2005). I suspect, however, that Dawson & Szmukler’s solution encourages its own form of discrimination. Under the proposals, ‘non-forensic’ patients could be treated involuntarily only if they lacked mental capacity. However, ‘forensic’ patients would be liable to a different, and easier-to-meet, set of criteria.

Underlying the distinction seems to be an assumption that the duties of doctors are different in respect of mentally disordered offenders. Some of the patients that forensic psychiatrists treat, Dawson & Szmukler write, are ‘not . . . under treatment primarily for their own benefit, but for the protection of others’ (p. 508). This seems to mistake a difference in emphasis for something more significant. First, benefiting patients and protecting others are not mutually exclusive. Second, treatment directed to both of these ends is not limited to forensic psychiatry. Third, where a tension does exist the position is straightforward. Exceptional cases notwithstanding, a doctor’s primary responsibility is his patient’s well-being. Ethical guidelines make no distinction in this regard between ‘forensic’ and other patients (Gunn & Taylor, 1993; Bloch & Green, 2006).

If capacity principles are to govern the coercion of psychiatric patients, I am not convinced that any ‘forensic exception’ is necessary. In England and Wales the important area is the hospital order under section 37 of the Mental Health Act 1983 (945 cases in 2004, 288 with restrictions). Here Dawson & Szmukler have two suggestions. The first would replace the hospital order with something like the present ‘hospital direction’ under section 45A of the Act. The second would sanction the involuntary treatment of a patient with mental capacity for a period ‘proportionate to the seriousness of the offence’ if a court thought that this would reduce reoffending. Presumably, the same treatment would be clinically indicated in many cases but the suggested criteria do not require this. Psychiatrists have complained that the hospital direction requires them to declare patients ‘fit for punishment’ (Mullen et al, 2000). The second suggestion implies the use of compulsory psychiatric treatment to achieve a legal end.

Instead, if capacity is to govern involuntary psychiatric treatment, why not make the passing of a hospital order, with or without restrictions, dependent on the patient consenting (or, if the patient lacks capacity, dependent on treatment being in their best interests)? The law could then permit re-sentencing if the convicted defendant changed their mind (or regained capacity and refused treatment), when the situation would be similar to the breaching of a probation order with a condition of treatment. The initial decision to give consent would often be difficult especially where the offence was serious and the choice lay between a substantial prison term and indeterminate detention in hospital. However, I am not clear that a competent defendant should be prevented from making it, particularly if the interim hospital order under section 38 of the Act remained available for cases where the psychiatrist was unsure whether to offer treatment or the patient was unsure whether to accept.

Because adherence is often partial there would still be cases where the doctor’s subsequent decision that a failure to participate in treatment amounted to withdrawal of consent could be seen as declaring the patient ‘fit for punishment’. Such a scheme would also have to overcome objections that section 37 of the Act already provides an efficient way of getting treatment to people who need it, resources permitting. However, by making court-ordered treatment dependent on consent, it would bring the management of those with psychiatric illness more into line with that of patients elsewhere in medicine. Moreover, it would do so without replacing one form of discrimination with another.

Authors' reply: Dr Buchanan notes that we allow an exception to ‘pure’ incapacity principles where a serious offence has been committed by a person with a mental disorder. We would allow the involuntary treatment of this narrowly defined subgroup of forensic patients under certain conditions, even if they regained capacity, to prevent harm to others. Buchanan believes this would discriminate unfairly between these patients and non-forensic patients. We are not convinced, however, that this would involve unjustified discrimination, because the commission of a serious offence constitutes a significant difference between their positions.

Nevertheless, Dr Buchanan’s suggestion that convicted offenders might be given a choice, on disposition from the court, of accepting imprisonment or consenting to treatment in hospital deserves serious consideration. However, we think a time limit should still be placed on the period during which a patient could be treated in hospital on this basis. That time would be proportionate to the seriousness of their offence. Otherwise, the patient who accepts hospitalisation and treatment initially, but later refuses treatment when they regain capacity, would face return to court for resentencing for an indeterminate period. Or, if the patient were to make a rapid recovery with treatment, would discharge very soon after a serious offence be politically acceptable?

Professor Maden, as we understand it, fears that the legislation we propose would not reduce homicides by people with mental illness, but we have little knowledge of the effect of mental health laws on rates of serious offending. What is most likely to reduce rates of violence is early access to effective treatment. Our proposal would allow involuntary treatment for the right reasons at the right time, and it may permit intervention sooner than under the 1983 Act. Some people with personality disorders who pose a risk of harm to others may not meet our incapacity test, and the transitional position of such persons who are already detained in our mental health facilities would have to be addressed. However, on balance, we think our proposals are likely to reduce violence overall, by allowing earlier access to effective treatment for persons who are incapacitated, regardless of the cause.

J. Dawson Faculty of Law, University of Otago, PO Box 56, Dunedin, New Zealand. Email: john.dawson@stonebow.otago.ac.nz

G. Szmukler Institute of Psychiatry, King’s College, London, UK
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Violence and psychiatric morbidity

Coid et al (2006) reported an important cross-sectional survey of 8397 persons in UK households and found that psychosis was independently associated with a sixfold increase in the reporting of five or more violent incidents. Given the controversy and sensitivity over the stigma associated with psychiatric illness, particularly concerning public perceptions of links between psychosis and violence, this kind of result is prone to generate misleading impressions. In a recent comprehensive review Hiday (2006) points out that surveys of this type are prone to exaggerate the contribution of mental illness and other diagnostic labels to violence as a result of several methodological weaknesses. The first is associated with the issue of comorbidity. It was not clear from the presentation of their data whether Coid et al were able to investigate the comorbidity of psychosis and other diagnostic categories and violence. It is possible that once comorbid substance misuse, personality disorder or other issues were taken into account, the unique contribution of psychosis to violence might have diminished dramatically (Hiday, 2006).

There is an even more fundamental problem that underpins violence surveys of this type: a neglect of the confounding factor that those with mental illness are more likely to reside in violent neighbourhoods and this could be the key predictive variable, not the illness itself. The term now used to describe the places where most people with severe mental illness live is ‘socially disorganised communities’, and these combine a multiplicity of factors that promote violence completely independently of psychiatric dysfunctions (Silver et al, 2001). Features of these environments include chronic disabling poverty, few employment prospects or educational opportunities, decaying buildings and few amenities. In these neighbourhoods families and similar social institutions have broken down, leaving most individuals devoid of traditional social guidance and control (Swanson et al, 2002).

Living and growing up in such environments is possibly the key variable that predicts violence, not the mental illness of the individual (Hiday, 2006). Community household surveys such as that reported by Coid et al (2006) represent a unique opportunity to explicate the contribution of ecological factors when violence appears to be linked to mental illness. It would therefore be useful in terms of advancing the debate over the link between violence and mental illness if a wider theoretical background to such analyses could be encouraged in the future.


R. Persaud Westways, The Maudsley Hospital, 49 St James Road, West Croydon CR0 2UR, UK. Email: R.Persaud@bap.kcl.ac.uk
doi: 10.1192/bjp.190.2.177

Authors' reply: We do not want our finding of a sixfold increase in reporting five or more violent incidents in persons with psychosis to give a misleading impression regarding the association of violence with mental illness. This was the only finding suggesting increased risk and means that there is a small subgroup of people with psychosis who are repeatedly violent. The real message of our paper should have
been that the true risks of violence from people with psychosis, at the population level, are exceedingly small.

Professor Persaud’s impression might be owing to the space in our paper devoted to discussing the public health impact of alcohol misuse and antisocial personality disorder on violence. In an additional paper published recently in the American Journal of Epidemiology we make the point about psychosis more strongly (Coid et al., 2006). Researchers with an interest in violence and psychosis often emphasise that relative risks of violence are greater for individuals with psychosis but they ignore the fact that illnesses such as schizophrenia are rare and that persons with psychosis account for an exceptionally small number of violent incidents at the population level. Detaining more persons with psychosis in hospital would have a very small effect in reducing violent crime (Fazel & Grann, 2006).

Misleading impressions based on relative risks are typical for homicides perpetrated by people with psychosis. These are often based on Scandinavian countries where the base rate is exceptionally low (Hodgins & Janson, 2002). In locations where the base rate is very high, for example certain areas in the USA and South American countries, people with psychosis hardly feature in criminal statistics.

Careful reading of our paper will reveal how we dealt with confounding from comorbid conditions. We agree with Professor Persaud’s point about residents in violent neighbourhoods entirely, but the sampling frame was intended to exclude bias from factors such as socioeconomic deprivation. We used two-level hierarchical models throughout the analysis to take account of clustering from these areas. We would concede, however, that our study did not adequately explore the important issue of neighbourhood effects.

M. Yang, A. Roberts, S. Ullrich Forensic Psychiatry Research Unit, Queen Mary College, University of London, London, UK

P. Moran Institute of Psychiatry, London, UK

P. Bebbington Department of Psychiatry and Behavioural Science, Royal Free and University College Medical School, London, UK

T. Brugha Department of Psychiatry, University of Leicester, Leicester, UK

R. Jenkins, M. Farrell Institute of Psychiatry, London, UK

N. Singleton Office for National Statistics, London, UK

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Assessment of manic symptoms in different cultures

Mackin et al. (2006) make a laudable attempt to evaluate cultural differences in the perception of psychiatric symptoms. Unfortunately, aspects of their methodology make it difficult to draw definitive conclusions. I will leave it for the statisticians to decide whether the sample sizes for the English and Indian groups (n = 20 and 24 respectively) are large enough to allow the findings to be generalised. Given the authors’ concerns about the influence of confounding variables on the findings, however, the disparity between the size of these groups and that of the American clinicians (n = 82) is striking. A demographic breakdown of the various groups might have been useful in allaying these concerns.

A further source of potential bias is introduced by asking the participants to complete rating scales for only two patients of a single nationality. There is a risk that cultural differences between nationalities might influence attitudes as to what can be considered ‘normal’ behaviour for people of other nationalities. Certainly, an English psychiatrist whose expectations of a ‘typical’ American have been shaped by stereotyped media images might not be expected to register certain aspects of the patients’ behaviour as pathological on the Young Mania Rating Scale. The threshold for recognition of manic symptoms might well have been different had they been asked to rate their own compatriots. More revealing conclusions could perhaps have been drawn had all participants been asked to complete rating scales for patients of a variety of nationalities, including their own.

The authors make a compelling argument about the potential consequences of cultural differences in the recognition of symptoms of mental illness, and have provided a useful starting point for future discussion and research. Unfortunately, they fall short of proving these differences exist with their preliminary data.

The study of Mackin et al. was interesting but so much highly relevant information is missing that it is hard to determine whether the findings have validity. The clinicians are effectively trial participants, yet we are not told the method of selection for doctors in each country. Training and employment structures are so different in the three countries that the clinicians are likely to have had very different degrees of experience and specialisation (the American system in particular favouring greater sub-specialisation). We are required to make the assumption that the groups are similar in all respects except the culture of the country of practice, yet there is no way to tell this without a socio-demographic profile of the participants from each country. There should be an attempt to make them representative of the total population of psychiatrists in their country in terms of ethnicity, gender and other factors which have a strong subcultural influence. There is no unifying ‘culture’ for psychiatrists in the UK, where at least one-third are trained outside the UK, and in some areas of the country the significant majority of doctors are non-UK-trained. Sampling such a small group from the UK (n = 20) would be most unlikely to give a representative picture of British psychiatry as a whole. Similarly, India and the USA are also among the most multicultural countries in the world, and the same issues of systematic sampling bias apply.

Furthermore, we do not actually know the ethnic and cultural background of the two videotaped individuals with mania. They are described only as ‘American’ – but can this be a meaningful term when describing an individual’s culture in such a varied society? The authors minimise the
implications of these difficulties by stating that ‘similar variability is likely to be present when ranking patients in routine clinical practice’. Few would debate the existence of inter-observer variability, but the core issue here is whether the authors’ data support culture as being a central factor in this phenomenon. The design of the study simply does not permit this conclusion.


R. Reed Barnet, Enfield and Haringey Mental Health NHS Trust, 58–60 Silver Street, Enfield EN1 3EP, UK. Email: ruthreed@gmail.com doi: 10.1192/bjp.190.2.178a

Author’s reply: We wholeheartedly agree with Dr Sanderson’s conclusion that this study provides ‘a useful starting point for future discussion and research’. Clearly, the number of assessed patients was small as was the number of clinician-raters. We acknowledge these points in our discussion and conclude by recommending other large studies using patients from real-life clinical settings. We also agree that perception of ‘normal’ behaviour would vary according to nationality and this might have very real significance when assessing the mental state of an individual. This warrants further research.

Drs Sanderson and Reed both comment on the lack of socio-demographic data on the rating clinicians but unfortunately these data are not available. We disagree with Dr Reed’s assertion that we are required to make the assumption that the groups are similar in all respects except culture. We state clearly that ‘we cannot exclude the possibility that other factors, in addition to cultural background, may have influenced these results’, and we go on to prescribe potential confounding influences, including age, gender, psychiatric training, years of experience, etc. Similarly, Dr Reed’s suggestion that we minimised the possibility that other factors, in addition to cultural background, may have influenced these results, is not supported by the data presented. Indeed, our research supports the notion that other factors, such as socio-economic status, may have influenced these results.

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One hundred years ago


Dr. Falciola has collected a large number of papers upon the growth of nails and the changes noted after disease, which he has tested by his own observation. He is not disposed to agree to the assertion of Parisot and Paget that the state of the nails is an index of trophic alterations in the body, although he admits that their growth is affected by a general disturbance in the economy of the organism. He found that in melancholy the growth of the nails is slower. The increase of the nails is somewhat irregular, being greater at one time than another, and differing in each finger, although there is a general equality in growth, which is more marked in the three middle fingers. The nails of one hand do not grow at exactly the same rate as in the other. He fails to find either marked acceleration or slowness of growth in states of mental depression or mania. In general he finds that the study of the growth of the nails in insane patients appears to support the views of Kraepelin on the clinical unity of all those types of mental disease which writers generally wish to treat as distinct, but which, in truth, only represent different episodes of one fundamental malady.

William W. Ireland

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