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correspondence

The mathematics of risk assessment for serious violence

Sir: I am pleased that Dr Harry Kennedy has picked up on the issue of prediction of rare events that I mentioned in my paper on inquiries after homicide (Szmukler, *Psychiatric Bulletin*, January 2000, **24**, 6–10; Kennedy, *Psychiatric Bulletin*, June 2001, **25**, 208–211). He makes an important point concerning an assumption in analyses like mine that clinical interventions do not substantially affect rates of serious violence. I will turn to this in a moment.

But first I want to draw attention to Dr Kennedy's use throughout his calculations of a theoretical predictive test for serious violence having a 'sensitivity' of 0.9 and 'specificity' of 0.9. In my paper I called such a test "wildly unrealistic". In the real world, a test with a 'sensitivity' of 0.52 and a 'specificity' of 0.68 is closer to the mark (Buchanan & Leese, 2001). Using these figures the 'positive predictive value' (the proportion of positive predictions that turn out correct) for base rates of violence in the patient population of 1%, 5%, 10% and 20% are 0.02, 0.08, 0.15 and 0.29, respectively. (These can be readily calculated using a probability tree method that I have described elsewhere (Szmukler, 2001).)

This means that if violence occurs in say 5% of a patient population, the predictive test will be wrong 92 times out of 100. In an inner-city community mental health team setting we found around that frequency of patients committed an act of violence against persons in a 6 month period (Shergill & Szmukler, 1998), with the vast majority of these incidents not causing serious injury. On the other hand, there is evidence that serious violence in patients with schizophrenia resulting in conviction in a higher court occurs in about 0.5% of males and 0.05% of females – over a 3 year period (Wallace *et al*, 1998). Here the 'positive predictive value', as for homicides, is quite useless; the prediction will be wrong more than 99 times out of 100.

However, Kennedy is right in pointing to a significant caveat concerning these analyses. There are no controlled trials that allow us to

evaluate the extent to which psychiatric interventions, including custodial ones, prevent incidents of serious violence. Thus we cannot know what the 'true' population base rate might be if clinicians never intervened to prevent them. But is there any reason to believe it would be much higher? Do changes in mental health services, for example, result in significant changes in the rate of serious violence in people suffering from mental illness? There is little to go on. In Victoria, Australia, despite major changes in service configuration, the relative risk of violent offending by patients with schizophrenia compared to controls did not change between 1975 and 1985 (Mullen *et al*, 2000). I know of no better evidence on the subject. Are these events rare because services are effective in making them so, or are they just rare (as they are in the non-patient population)? We can't know for sure, but the latter must be far more likely. Even if serious violence in males with schizophrenia, without clinical interventions, was 10 times greater than found by Wallace *et al*, and occurred in 5% instead of 0.5%, the 'positive predictive value' of our real world test would still only be 0.08.

Kennedy refers to 'stratification' of risk: pick a very high-risk group and focus on them. The cost of doing this is that you then miss the majority of cases who will later be violent. An excellent example concerns the prediction of in-patient suicide, also a rare but tragic event (Powell *et al*, 2000). The investigators could define a group of patients with all five identified risk factors in whom the probability of suicide was almost 40%. Unfortunately only one out of the 97 eventual suicides was at this level of risk.

If the risk of serious violence could be eliminated by a simple low-risk intervention, such as giving an aspirin, one might be able to put an argument to support the enforced treatment of say 10 or 20 patients to prevent one act of serious violence. However, the interventions we are talking about often involve compulsory treatment or detention for protracted periods of time. The implications of risk assessment are thus extremely serious. Claims for its validity need much stronger evidence than we have so far seen. To me, the mathematics of rare events indicates we are unlikely to ever see it.

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Undue emphasis on risk may increase stigma

Sir: I agree with Petch (*Psychiatric Bulletin*, June 2001, **25**, 203–205) that an undue emphasis on dangerousness may not serve to protect the public. Indeed, it may actually do more harm than good by increasing the stigma of mental illness.

For example, the recent Government White Paper (Department of Health, 2000) expresses a desire to reduce the stigma of mental illness but, in the next paragraph, it talks of the "toll of homicides" by those with mental disorder. There is no recognition of the excellent work that mental health services routinely provide. Instead, the focus is on rare, mostly unpredictable tragedies, not on the disasters that services have averted. Policies appear to be influenced by media rather than scientific evidence: the proportion of homicides by those with mental illness, for example, has fallen since the introduction of community care (Taylor & Gunn, 1999).

According to Government proposals, there will be a statutory duty to divulge patient information to non-clinical third parties, for example the police and housing associations. This will undermine



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patients' confidence and trust. The concern is that those who pose the greatest danger to others, may be the ones who become most motivated to avoid contact with services.

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In-patient adolescent services

Sir: I was interested to read the report of Worrell and O'Herlihy (*Psychiatric Bulletin*, June 2001, **25**, 219–222) summarising the views of psychiatrists on in-patient child and adolescent provision. I have completed a similar survey in Wales, with a response rate of 96% (25/26 responses).

In Wales no psychiatrist has access to an adolescent psychiatric in-patient bed for emergency admissions. Eighty per cent ($n=20$) usually use a bed 'borrowed' from adult services and 20% ($n=5$) use paediatric beds either primarily, or equally to adult psychiatric beds.

Eighty-eight per cent ($n=22$) believe appropriate in-patient care is delayed for adolescents with mental illness because of inadequate provision. All believe this is primarily because of insufficient beds. Sixty-eight per cent ($n=17$) identify the lack of specialist adolescent provision, particularly adolescent psychiatric intensive care and adolescent forensic mental health provision.

Forty per cent of psychiatrists ($n=10$) feel the regional adolescent units are frequently unable to offer a bed within an acceptable time. Those patients are managed locally in adult psychiatric (24%) or paediatric (16%) beds or referred out of area largely to beds within the independent sector.

The response rate of >95% suggests the views expressed are representative of opinion in Wales. In my study higher percentages report delayed in-patient care (88% v. 36%) and inadequate specialist provision (68% v. 17%).

Regional differences in current provision may influence the level of concern expressed in Wales. My findings indicate the themes raised by Worrell and O'Herlihy are not only representative of opinion within the specialty but that experiences in Wales may be more extreme than those in England.

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Mental health problems of asylum seekers

Sir: Derek Summerfield's editorial (*Psychiatric Bulletin*, May 2001, **25**, 161–163) shows how apparent mental health problems of asylum seekers/refugees must be seen in the context of disrupted social lives, and the importance of such practical issues as employment.

One practical issue not mentioned was housing. Those with no established right of abode experience special difficulties in securing accommodation.

For the hospital, this causes a danger of bed blocking if the patient ought not be discharged without an address to allow appropriate follow-up. Normally referrals are made to social services or to the Salvation Army. However, the social services has no duty to house an illegal immigrant, and the Salvation Army cannot help as it cannot recover costs via the benefits system. The Home Office does have a duty to house immigrants whose status is being investigated. However, faced with a detention centre, the patient may withhold consent to inform the Home Office. The stand-off can persist until a stage is reached in an asylum application when the applicant becomes eligible for benefits, and can be brought into the normal support system.

Even for an experienced doctor knowing the system, the process is difficult and extremely frustrating. How much harder must this be for someone trying to deal with a foreign language. As Derek Summerfield and others (for example, see Burnett & Peel, 2001) note, symptoms of psychological distress are common among refugees but may not signify clinical mental illness. Other cultural and social factors may contribute to psychological distress (for a recent analysis see Bhugra & Jones, 2001). Surely, the difficulties of finding accommodation must be one such contributing factor.

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Hyperglycaemia and myoclonus with clozapine

Sir: Hyperglycaemia and myoclonus have been reported as uncommon adverse events with clozapine treatment. We report two cases in which they occurred together in close temporal association.

Case 1

A 42-year-old African–Caribbean man with a history of severe, treatment-resistant schizophrenia developed myoclonic jerks while taking clozapine 700 mg/day. Eight weeks later he developed diabetic ketoacidosis with a blood glucose of 44 mmol/l, for which he required intensive care treatment. Clozapine treatment was subsequently stopped. Following recovery he was reinstated on clozapine, has not developed further myoclonus and his glucose tolerance is not impaired.

Case 2

A 58-year-old White British man with a history of clozapine-induced hyperglycaemia was restarted on clozapine 400 mg/day. After 2 weeks he developed severe myoclonus and deterioration of glycaemic control with random serum glucose as high as 21.5 mmol/l. Clozapine treatment was withdrawn. Within 7 days control of his blood sugar was re-established and myoclonus resolved.

Myoclonus has been reported in 0.2% of 24 000 clozapine treated patients on the UK Clozaril Patient Monitoring Service (CPMS) database. Results from case series suggest the incidence may be as high as 2.0% (Safferman *et al*, 1991) to 2.7% (Sajatovic & Meltzer, 1996).

Impaired glucose tolerance with clozapine treatment is also probably more common than the 0.4% quoted by the UK CPMS (Linda Annan, Advisor, CPMS, personal communication). In a case-note study of 82 patients, 36.6% were considered to have developed diabetes (Henderson *et al*, 2000). The true prevalence of these adverse events remains to be established.

The association between hyperglycaemia and movement disorders, including myoclonus, has been well documented (Moores & Dire, 1989). Hyperglycaemia is the commonest metabolic disorder to be associated with clonic activities of the extremities and other focal motor phenomena. Correction of the underlying metabolic disturbance prevents further focal seizures or movement abnormalities (Berkovic *et al*, 1982).

Since uncontrolled hyperglycaemia is potentially life threatening, the presence of myoclonus in clozapine treated patients may be of use in alerting clinicians to the presence of impaired glucose tolerance.

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