showed significant improvements in social functioning, there were still obvious impairments within this population. Second, and very importantly, the differences between the cognitive therapy group and the control group were only apparent during the active phase of treatment – the control group continued to make modest gains during the follow-up period so that at 1 year after cognitive therapy there was no difference in social functioning between the two groups. One conclusion from this result is that individuals who receive 16 sessions of cognitive therapy for chronic or residual depressive symptoms may benefit from additional but less-frequent maintenance cognitive therapy sessions.

Lastly, Ito *et al* are right to point out that calculations of numbers needed to treat from this study are indeed indicative of substantial benefits from using cognitive therapy. For the record, using data from our study and other recent studies, only four to six additional patients need be treated with cognitive therapy to prevent one relapse.

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Long-term psychotherapeutic relationships in schizophrenia

I would like to add what I believe is an important factor not mentioned by Thornicroft & Susser (2001) in their editorial on evidence-based psychotherapies in the community care of schizophrenia. It is a factor that I think is missing from a great deal of psychiatric literature on what helps patients get better and what makes us human. People with schizophrenia have withdrawn from being able to relate to others. They need somebody who is able to provide a long-term therapeutic relationship and is not frightened off by those who say 'beware of dependency' or seduced by the culture of brief interventions; a person who can stand up to the 'package culture' and stay with the patient and family over a long period of time. This sort of work does not make headlines. I think it is the role of psychodynamic psychotherapists to champion dependency in order that the patient can find something of his or her own from the shattered fragments of self; a mature dependence, within the constraints of illness. This work is not easy, requires support, supervision, time and resources. Perhaps the paucity of evidence is because this apparently simplistic viewpoint meets great resistance and is culturally dystonic.

Thornicroft, G. & Susser, E. (2001) Evidence-based psychotherapeutic interventions in the community care of schizophrenia. *British Journal of Psychiatry*, **178**, 2–4.

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Psychodynamic thinking and the community mental health team

I read with great interest Thornicroft & Susser's (2001) editorial on evidence-based psychotherapeutic care in schizophrenia. It called for evidence-based interventions being implemented in the face of resource limitations and a remedy to the absence of implementation plans for well-established effective interventions such as family therapy. However, Thornicroft & Susser dismiss psychodynamic approaches. Although the general view is that people with schizophrenia do not benefit from intensive psychoanalytic psychotherapy, there are some heroic efforts by analysts such as Herbert Rosenfeld (1987). In particular, such approaches do address the imperfection of our models of mental disorder.

One thing the psychodynamic way of thinking can offer members of the community mental health team (CMHT) is understanding of complex mental states from the patient's perspective, and new ways of understanding those that fall outside of our management strategies. There is no doubt that the delivery of psychoanalytic psychotherapy to people with schizophrenia, on an intensive basis, will not be resourced, nor will the symptom outcomes necessarily be better. Therefore, the cost cannot be justified. However, part of the problem that faces CMHTs is the long-term nature of their work with little reward in terms of symptom improvement and recovery for those with enduring severe mental illness. This can be frustrating and de-skilling for staff, particularly if they have a limited range of therapeutic models. I have worked in an assertive treatment team for the people with severe mental illnesses and one for homeless people with mental illnesses. Staff retain curiosity and capacity to think and question their formulations about patients in a psychodynamic way. Their work continues to be fresh and motivating. This is particularly welcome in light of Wykes *et al*'s (1997) finding that CMHT staff are not uncommonly depersonalised and therefore unable to empathise with their patients. At a time when there is a movement to ensure good human relationships as well as therapeutic relationships with patients, dismissal of the relevance of psychodynamic thinking in the healthy functioning of a CMHT is premature. This is one area of CMHT functioning that warrants further research, as suggested by Thornicroft & Susser.

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What came first: dimensions or categories?

We read with much interest the paper by O'Dwyer & Marks (2000), and think that the case vignettes reported by the authors fit perfectly with Insel & Akiskal's (1986) model that considers obsessive–compulsive disorder as a disorder that can develop along a continuum of insight. Therefore, the primary problem is not the boundaries between obsessive–compulsive disorder (or anorexia, or body dysmorphic disorder) and psychosis, but rather at which point insight is lost and the disorder under consideration becomes a frankly psychotic one. If one considers insight as a dimension spanning from normality to the most severe psychiatric conditions, then it will not be difficult to posit several psychiatric disorders along it, with all possible heterogenous combinations. The model becomes even more comprehensive if we add the 'uncertainty/

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certainty' dimension, so that prevalent ideations, and thus 'normal' conditions, can also be accommodated (Marazziti *et al*, 1999). Thus, insight can be considered to be intertwined with several other dimensions, yet to be identified, and can become disturbed when these other dimensions are altered. In our opinion, therefore, insight is a phenomenon that is only apparently heterogenous, and in fact is strictly related to other variables and/or clinical core features, so that it may well respond to serotonergic drugs and behavioural therapy, as underlined by O'Dwyer & Marks. The response of insight to various drugs may depend on how close are the links with the other dimension primarily disturbed (e.g. affect; certainty/uncertainty; anxiety). Naturally, these considerations demand new operational criteria that should complete, if not replace, current diagnostic criteria.

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O'Dwyer, A.-M. & Marks, I. (2000) Obsessive–compulsive disorder and delusions revisited. *British Journal of Psychiatry*, **176**, 281–284.

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Practical application of structured risk assessment

I was disappointed by Szmukler's (2001) negative response to Dolan & Doyle's (2000) excellent review of attempts to measure the risk of violence in psychiatric patients. His pessimism about the practical application of structured risk assessment results from a misunderstanding of the way in which these instruments may be used. First, he emphasises the low baseline. Of course, we do not know the baseline, as the information has never been collected accurately in this country. The 6% in 6 months cited in the letter derives from retrospective ratings by clinicians. Studies in the USA show that rates of violence by psychiatric patients may rise threefold if self-report is supplemented by official records and by the account of a key informant. In any case, the low baseline is irrelevant. Most risk assessment tools were developed on high-risk populations, usually people who had already committed serious offences. They were not designed to be applied to all patients. If these instruments were applied to those 6% of patients with a record of recent violence, would they assist future management by identifying those who were at highest risk of a repetition? I do not know the answer, but it is a sensible and important question.

The second problem is an unrealistic expectation of what such instruments can do. They are a supplement to good clinical practice, not a substitute for it. In many parts of Canada, for example, the Violence Risk Appraisal Guide (Harris *et al*, 1993) is administered to all patients admitted to a medium secure unit. The results do not dictate future management but, like the results of any other investigation, they inform it. I have no good answer to the question of why we are not evaluating this practice in the UK. When one concentrates on small, high-risk populations, the resource arguments lose this force. With medium secure admissions in this country averaging >18 months in duration, at £100 000 per patient per annum, a few hours of a psychologist's time is neither here nor there.

It is unfortunate that British psychiatry has been slow to look at the application of these instruments, on which much basic research has already been done. The situation is even more depressing because National Health Service Trusts, fearful of lawsuits, are insisting on the introduction of standardised risk assessments. In most cases, they are not of proven value. Trusts in different areas use different instruments, making evaluation more difficult. The measures are likely to be employed indiscriminately, becoming just another form to be filled in. Would it not be better if this initiative were driven by clinicians, and underpinned by a sound methodology for evaluation?

Psychiatry must not persist in assuming that violence, an uncommon complication of mental disorder, is unimportant because of its rarity. *Reforming the Mental Health Act* (Department of Health, 2001) illustrates that concern about violence dominates the thinking of politicians in this area. It is unlikely that they are going to lose votes by overstating the level of risk associated with psychiatric patients, so the profession is going to have to come up with something better than bland reassurance. We are in a weak position, so long as we lag behind North America and parts of Europe, both in our use of existing risk-assessment technology, and in research into violence. The paper by Dolan & Doyle should stimulate us to make up some of that lost ground.

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Classic text still accessible

Professor Goldberg's (2001) excellent retrospective demonstrates the breadth of reading that used to be part of psychiatric training. His statement that *General Psychopathology* by Karl Jaspers is "out of print" is happily not the case. It was republished by Johns Hopkins University Press in 1997 in two volumes and is readily available via internet booksellers.

As Goldberg states, the work influenced a whole generation of psychiatrists and remains a very readable text, full of detail and close observation, which harks back to a time when meticulous detail and careful attention to language were essentials of good psychiatric practice. Any student of psychiatry, at any level, would do well to take a look.

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