BJPsych The British Journal of Psychiatry (2013) 202, 472. doi: 10.1192/bjp.202.6.472

From the Editor's desk

By Peter Tyrer

DSM or DTM

By the time readers open this issue of the *Journal* they will almost certainly be aware of the arrival of the new DSM-5 classification of psychiatric illness, which will be revealed in all its many colours at the end of May at the annual meeting of the American Psychiatric Association. I continue to say to many authors submitting manuscripts to our *Journal* that we in the UK are a DSM-free zone, as virtually all aspects of our clinical practice are linked to the alternative classification, ICD-10, also soon to be revised as ICD-11. But DSM has been the lead classification in psychiatry, ever since the flame of Robert Spitzer lit up the gloomy convoluted caverns of psychiatric nosology in 1980 with the launch of DSM-III. In retrospect, this was the high point of DSM; with each succeeding revision it has lost a little of its lustre, and the latest revision has been dogged by controversy over alleged undisclosed conflicts of interest, the medicalisation of behavioural upsets that we all have from time to time, and the proliferation of diagnostic labels that are likely to be rarely used. The real problem is that the biological markers that should underpin successful diagnosis, as they do in most aspects of medicine, are conspicuously lacking for most of the conditions we describe in our classification, and we continuously struggle to find them, as Serra-Blasco *et al* (pp. 434–440) do in their paper that relates depression to grey matter volumes. When it comes to clinical validity – identifying conditions that unequivocally exist and can be measured – we are largely whistling in the dark.[1] This issue, and indeed most of our previous ones, acknowledges, illustrates and half apologises for this deficiency over and over again. Thus, Leff *et al* (pp. 428–433), in their fascinating study of a new approach to schizophrenia, do not use conventional diagnosis at all and confine their study to treating not a diagnosis, but a symptom group, 'medication-resistant auditory hallucinations'. Bebbington *et al* (pp. 419–427) go even further and widen the concept of paranoia to include the full panoply of paranoid ideation, extracted not from a diagnosis, but a rating scale; and Ani *et al* (pp. 413–418) describe all the difficulties in defining the most complex of psychiatric presentations, conversion hysteria.

One of the most serious criticisms of DSM is that it fails to deal properly with comorbidity. When conditions occur so frequently together they can hardly be separated it is rational to regard them as part of the same syndrome and so these conditions could therefore be described as showing consanguinity rather than comorbidity.[2] So many of the disorders we diagnose overlap in this way[3–5] that genuine comorbidity, multimorbidity as described by Langan *et al* (pp. 391–393), is almost forgotten. So when, for example, we look at interventions for attention-deficit hyperactivity disorder (Rajyaguru & Cooper, pp. 398–399), a condition with so many associations that it is almost the queen of comorbidity,[6] it is difficult to know what is the target of treatment. But the strongest criticism of DSM, and indeed ICD and all other attempted classifications of mental illness, is that they are based primarily on clinical agreement, decisions arrogated at meetings where charm, downright rudeness, lobbying, politics, dogma and opinion are combined in equal measure, while science whimpers outside and can hardly get its foot in the door. Possibly the most prestigious psychiatric research institute in the world, the National Institute of Mental Health in the USA, has now rejected DSM as a focus for research on mental illness. Thomas Insel, its Director, expresses this trenchantly, 'DSM diagnoses are based on a consensus about clusters of clinical symptoms, not any objective laboratory measure. In the rest of medicine, this would be equivalent to creating diagnostic systems based on the nature of chest pain or the quality of fever.'[7] But this does not mean we should abandon DSM; we should stop it being our master and make it into our servant. I have long thought it should be relabelled a Diagnostic and Training Manual (DTM) for mental disorders, as the adjective 'Statistical' always gave it specious respectability. It is important to know what we mean when we use a diagnosis, to try and make the wording unequivocal, and to ensure that we reach agreement over our descriptions, but this does not give them credence as full diagnoses. The conditions we describe are proxies, the best available at present, but should always be challenged and revised when good new data come along.

Golden vistas for psychotherapy

But enough of pessimism. Kingdon (pp. 394–395) describes a world in which excellent psychological treatments can cut through the maze of diagnostic uncertainty and reveal real benefit. He belongs to a different school in which what he describes as the 'cul-de-sac of neurobiological approaches' is by-passed and replaced by a highway of therapeutic opportunity. But we must not get too carried away. Psychological treatments are undoubtedly effective, and Kingdon is right in arguing for much better research funding, but there is still an unacceptable degree of bias in this area[8] and too many studies suggestive of efficacy are of poor quality and overstate their conclusions.[9,10] But all will agree, biologists, social scientists and psychotherapists, that we must not only be open and caring in our attitudes to our patients,[11] but also clear in what we are hoping to do, as Priebe *et al* (pp. 459–462) have demonstrated so persuasively, and it also seems that the more accurate and objective feedback we can give them, the better (Boyer *et al*, pp. 447–453). Being a little bit more honest when it comes to diagnosis will, I suggest, help as well.

1 Kendell R, Jablensky A. Distinguishing between the validity and utility of psychiatric diagnoses. *Am J Psychiatry* 2003; **160**: 4–12.

2 Tyrer P. Comorbidity or consanguinity. *Br J Psychiatry* 1996; **168**: 669–71.

3 Andreescu C, Lenze EJ. Comorbid anxiety and depression: bête noire or quick fix? *Br J Psychiatry* 2012; **200**: 179–81.

4 Owen MJ. Intellectual disability and major psychiatric disorders: a continuum of neurodevelopmental causality. *Br J Psychiatry* 2012; **200**: 268–9.

5 Weich S, McBride O, Hussey D, Exeter D, Brugha T, McManus S. Latent class analysis of co-morbidity in the Adult Psychiatric Morbidity Survey in England 2007: implications for DSM-5 and ICD-11. *Psychol Med* 2011; **41**: 2201–12.

6 Thapar A, Cooper M, Jefferies R, Stergiakouli E. What causes attention deficit hyperactivity disorder? *Arch Dis Child* 2012; **97**: 260–5.

7 Insel T. Transforming diagnosis. http://www.nimh.nih.gov/about/director/2013/transforming-diagnosis.shtml

8 Cuijpers P, Smit F, Bohlmeijer E, Hollon SD, Andersson G. Efficacy of cognitive–behavioural therapy and other psychological treatments for adult depression: meta-analytic study of publication bias. *Br J Psychiatry* 2010; **196**: 173–8.

9 Huntley AL, Araya R, Salisbury C. Group psychological therapies for depression in the community: systematic review and meta-analysis. *Br J Psychiatry* 2012; **200**: 184–90.

10 Bracken P, Thomas P, Timimi S, Asen E, Behr G, Beuster C, et al. Psychiatry beyond the current paradigm. *Br J Psychiatry* 2012; **201**: 430–4.

11 Cowen PJ. Has psychopharmacology got a future? *Br J Psychiatry* 2011; **198**: 333–5.