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Correspondence

Edited by Kiriakos Xenitidis and Colin Campbell

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The classification of psychosis

Lawrie *et al*'s editorial on the 'continuum of psychosis' is timely and welcome.[1] I see this debate two ways: as a doctor needing order to help ease suffering, I agree that it is better, for the time being, to keep existing diagnostic categories of psychiatric disorder, however imperfect they may be. As a patient, I of course want care, but I also want to be understood. Many psychiatrists now consider that too much of life is branded 'disorder': in this, none of us diminishes the reality of suffering, but we do look for better ways of explaining it. Certain scientists may hate this – but people's lives do have narrative. I think we underestimate humankind if we say that we cannot accept symptom-based descriptions of suffering. I hope I am not wrong to suggest that most of the treatments used today to improve mental health are not disease specific, but rather act on either mood, thought or both.

Nevertheless, I agree that the cry for a spectrum approach to psychosis is premature and it does not fit with my experience of so many troubled lives encountered. Peter Tyrer is correct to raise the potential problems, both clinical and pragmatic, of premature abandonment of current diagnostic classifications.[2] However, there remains a need to reconsider the neo-Kraepelinian model, if only to bring greater alignment with the technology that Lawrie *et al* hope will be to our greater mental good. It is my belief that, under the present classification system, neurobiological research cannot fully address complexity. My own view is that we have given too much attention to what Steven Rose[3] has termed 'neurogenetic determinism' rather than applying biological research to life (we should not risk losing the baby with the bath water, however dirty).

I would contest the presentation of the neurobiology literature as presented by Lawrie *et al* in the opening paragraph of their editorial. I would also contest the claim, attributed to a paper by Tandon *et al*,[4] that 'advances in our understanding of aetiology and pathogenesis [of psychosis are] based on highly replicable neurobiological differences'. I have read that paper several times, but found, for all the studies and indeed all the words, neither one simple biomarker of any utility nor indeed anything even approaching specificity. Perhaps we should ask why this may be? Could it be that categories, clinically practicable, and needed for now, do not match the complex epigenesis of psychosis?

In concluding, I would suggest that we do not forget history. James Clerk Maxwell was bold enough to stop looking for matter and to consider the energy fields that now govern our lives and, indeed, technology that has been to our collective good. Do we need another Maxwell moment, scientifically brilliant, religion free, willing to see matters as simple as possible, but not simpler? I have no such moment to offer. But brilliant folk like Lawrie and his colleagues have that tradition and they perhaps raise the chances that such scientific inspiration can help us once again.

1 Lawrie SM, Hall J, McIntosh AM, Owens DGC, Johnstone EC. The 'continuum of psychosis': scientifically unproven and clinically impractical. *Br J Psychiatry* 2010; **197**: 423–5.

2 Tyrer P. From the Editor's desk. *Br J Psychiatry* 2010: **197**: 508.

3 Rose SPR. The biology of the future and the future of biology. *Perspect Biol Med* **44**: 473–84.

4 Tandon R, Keshavan MS, Nasrallah HA. Schizophrenia, 'Just the facts': what we know in 2008. Part 1: Overview. *Schizophr Res* 2008; **100**: 4–19.

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Lawrie and colleagues urge us not to reject the current categorical classification system prematurely.[1] I wish to add to the argument that a categorical system is more likely to be internationally useful. More than 80% of mental illness occurs in middle- and low-income countries.[2] Much of the world's mental illness is seen in overstretched clinics, by practitioners who treat up to 100 patients a day and often have had no training in psychiatry since medical or nursing school. Administering the rating scales necessary for a dimensional system may be possible in high-income countries, but is difficult or impossible elsewhere. The categorical classification system can be used quickly by someone with relatively little training. There is also the problem of translating and validating the rating scales into hundreds of languages. Most published research currently uses the same categorical system, which means that it is useful to doctors all over the world. If the research were to refer only to a dimensional system, then it would not be useful in settings where it is impossible to administer the rating scales. The categorical system gives more people access to evidence-based treatment than any dimensional system would. A classification system that is going to be used all over the world needs to be simple and robust across healthcare systems, languages and cultures, and this is just as important as how closely it resembles the truth.

1 Lawrie SM, Hall J, McIntosh AM, Owens DGC, Johnstone EC. The 'continuum of psychosis': scientifically unproven and clinically impractical. *Br J Psychiatry* 2010; **197**: 423–5.

2 World Health Organization. Disease and injury regional estimates for 2004. WHO (http://www.who.int/healthinfo/global_burden_disease/estimates_regional/en/index.html).

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As psychologists who have long researched and argued for a dimensional view of psychosis, we would like to comment on Lawrie *et al*'s editorial.[1] We are surprised that the authors pay no attention – with one exception – to the psychological literature. If they had done so they would know that considerable evidence supporting the continuum view has accrued over many decades. The one psychologist they do cite – the late Paul Meehl – is an unfortunate choice. Quite apart from the fact that it is unclear to us how Meehl's taxonomic (categorical) approach actually helps their case, the authors ought to be aware that the theory is now on the wane. A more viable alternative is what we have termed a 'fully dimensional' theory that is capable of encompassing more of the

known facts about psychosis, including the clear dimensionality of the risk of illness and the likely form of the heritability underpinning this, coupled with the notion of discontinuity to recognise the break in behaviour and psychological state that occurs when vulnerability translates into clinical symptoms. Importantly, the model also recognises something that Lawrie *et al* entirely ignore – the fact that psychotic traits can have a healthy expression that takes the individual outside the domain of psychiatric judgement.

Of course, many questions remain, such as how to deal with the overlap between schizophrenic and affective expressions of psychosis, explain the underlying biological mechanisms of these disorders, and incorporate into our thinking how expressions of vulnerability can vary from sick to benign. However, answers to these questions will not make dimensionality go away, for it is part of the essence of human variability (of which psychosis is one form).

On the practical front, these ideas admittedly make for a messy picture that is inconvenient for clinicians seeking a neat solution to diagnostic issues. But psychiatry does itself no favours by ignoring them and retreating (yet again) behind the ramparts of its traditional mode of thinking. Fortunately, as Lawrie *et al* will be aware, their profession actually has moved forward in recent years towards an attempt to find ways of integrating both dimensional and categorical perspectives into its future diagnostic systems. Our plea is that, in doing so, it becomes an even more 'psychologically informed' psychiatry.

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Authors' reply: We thank Drs Gordon and Shoesmith for their interest in our editorial, their complimentary remarks and their considered responses to what we said. Dr Gordon repeats our call to avoid prematurely abandoning categories or dimensions, and highlights the lack of known diagnostic biomarkers for psychosis, either as a whole or for current subtypes. Tandon *et al*[1] did not really consider this, quite reasonably, as their review focuses on what is known about the aetiology and pathogenesis of schizophrenia. As we have clarified in a forthcoming review,[2] the lack of known biomarkers for psychosis (whether as categories or continua) is at least partly because the right sort of studies to find them have only rarely been done and reported in this light. The relevant populations need to be studied and then the results analysed according to the principles of clinical epidemiology (or evidence-based medicine), to extract the potential clinical significance for individuals of statistically significant abnormalities evident in groups of patients. Thus, for example, if one wished to identify specific diagnostic markers of schizophrenia that have clinical utility, a (preferably large) representative population of people in their first episode would need to be assembled, and predictive values and/or likelihood ratios calculated for the value of potential markers of schizophrenia as opposed to, say, bipolar disorder. Despite the paucity of studies, there are already a few well-replicated large differences between people with schizophrenia and healthy controls, which may also distinguish them from those with bipolar disorder.[2] Not all of these require high-tech investigations. Simple clinical measures of neurodevelopmental aberration such as neurological soft signs, and even historical measures such as early social difficulties, are common in people who go on to develop schizophrenia but may not be in those with bipolar disorder. These already influence clinical decision-making but in an informal and rather haphazard fashion. The optimal method of eliciting and using such information needs further investigation, as outlined above and in our review.[2]

Dr Shoesmith is absolutely right to remind us that any resource-intensive diagnostic procedure is going to be much less practical in less well-developed health services. This is of course an immediate and quite possibly fatal problem for any system requiring multiple ratings on continua and could be even more so if, for example, magnetic resonance imaging of the brain/mind turns out to be diagnostically valuable – as we suspect it might.[2] In the long run, whatever turns out to be the best conceptual approach to psychosis for the maximal benefit of patients, and whether or not this has to be pioneered in leading clinical research centres, the process of formalising our diagnostic and therapeutic judgements will bring a much-needed and long-overdue re-engagement of psychiatry with the rest of medicine.

We are also grateful for the opportunity to respond to the letter from Professors Claridge and Barrantes-Vidal, especially those of us who after more than four decades still remember Professor Claridge's excellent and provocative teaching on, and seminal contributions to, the field of schizotypal cognitions, beginning as they did more than 30 years before this area became fashionable. We cite Paul Meehl as he is one of the very few commentators on diagnosis in psychiatry, whether psychologists or psychiatrists, to have offered a testable hypothesis that would allow one to make an informed decision about whether a categorical or continuous approach might be more valid. We recognise that there have been several alternative proposals to handling the complexity of psychosis, but very few of these have been tested in practice. To clarify our position, we are not opposed to continuous measures, be they psychological trait or cognitive test scores or brain imaging variables, nor are we particularly in favour of the *status quo* or hybrid models. We are simply arguing that any proposals to change our diagnostic approach to psychosis, which has survived to this day for some quite good reasons, should be based on data and therefore built on evidence rather than fashion or because something looks good on paper. We would very enthusiastically support, for example, a trial that tested the efficacy of one or more treatments on one or more continua of psychosis severity. Having said that, however, even if that trial generated informative results for clinical practice, any resulting practical system would of necessity have to include thresholds for treatment and would thereby create categories. As we said, continua may or may not be more valid than categories of psychosis, but clinical decisions require choices between alternative courses of action.

1 Tandon R, Keshavan MS, Nasrallah HA. Schizophrenia, 'Just the facts': what we know in 2008. Part 1: Overview. *Schizophr Res* 2008; **100**: 4–19.

2 Lawrie SM, Olabi B, Hall J, McIntosh AM. Do we have any solid evidence of clinical utility about the pathophysiology of schizophrenia? *World Psychiatry* 2011; in press.

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An unjust review

In his review of my book *Fiction's Madness*,[1] Beveridge comments on my omission of Laurence Sterne's *Tristram Shandy* in discussing the history of the novel form.[2] On fictional development in the 1950s, Hawthorn[3] pointedly excludes *Tristram*