a low dose. We also wonder why the authors arbitrarily decided to have a tenfold lower dose in the control group. We question whether it is better than nothing.


Authors’ reply: Several of the limitations of our study design as mentioned by Jainer & Mahood have been addressed within the publication’s discussion. The study was not designed to establish an optimal dose or evaluate efficacy v. placebo. Thus, as we noted, no conclusions can be made in this regard. The objective of this study was to determine whether there was a difference between two dose ranges; this goal was achieved. The use of an active comparator was not possible because there was no drug approved for use in children or adolescents suffering from this disorder at the time the study was conducted.

The dose ranges were chosen to compare the adult therapeutic dose, known to be effective in schizophrenia, with a low dose. This low dose was presumed subtherapeutic, but not known to be ineffective. Notably, in studies in children with disruptive behaviour disorder where the allowable flexible dose range included doses <0.6 mg/day, risperidone was shown to be efficacious.1,2 Additionally, at the time this study was designed, a low-dose comparator was preferred over placebo, although thinking on the appropriateness of using placebo control in studies of antipsychotics has evolved since then.3 A placebo effect in terms of treatment response cannot be ruled out in our study, and presumably any placebo response would have affected both dose arms similarly. Numerous safeguards were implemented to minimise risk to patients in the study from the outset. The protocol was reviewed by and received approval from an independent ethics committee and individual institutional review boards. All patients and caregivers were advised that both doses were experimental and the lower dose might be an ineffective treatment. Accordingly, all enrolled patients were initially hospitalised and only adequately stabilised patients could be discharged to continue in the trial as out-patients. Patients could discontinue treatments at any time. All patients were monitored closely throughout the duration of the trial to further ensure patient safety.

Our conclusions remain valid, as they pertain to the comparative favourable efficacy benefits achieved in this study with risperidone treatment in the 1.5–6.0 mg/day dose range compared with the lower range. Both regimens were well tolerated with low discontinuation rates due to adverse events.

Declaration of interest

The study was funded by Johnson & Johnson Pharmaceutical Research & Development, LLC. M.H. and M.E. are employees of Johnson & Johnson Research & Development, Division of Janssen Pharmaceutica, NV. S.K., J.S., I.A., J.Q., G.P. and V.K. are employees of Johnson & Johnson Pharmaceutical Research & Development, LLC.


Time to change concepts and terminology

The proposal by van Os to introduce ‘salience dysregulation syndrome’1 to describe the psychosis spectrum, replacing schizophrenia and bipolar disorder, represents an acceptance that such terms have outlived their usefulness. But by introducing three subcategories, ‘with affective expression’, ‘with developmental expression’ and not otherwise specified, he simply replaces outdated terms but retains the invalid and unreliable concepts – schizophrenia and bipolar disorder re-emerge with different names.

The evidence for a psychosis spectrum, as he describes, now seems irrefutable. At one end, manic symptoms ‘represent the greatest diagnostic value’ and this end of the continuum seems relatively recognisable and clinically relevant. Moving towards the other end takes us into Bleuler’s schizophrenias and the more recently emerged area of drug-related psychosis. We have argued the case that rather than simply continuing to try to homogenise the schizophrenias, we should listen to what patients tell us led to their first episodes. Dudley et al2 have recently used Q-sort methodology to elicit this and found similarities to concepts developed empirically from clinical practice.3 We have used these concepts of drug-related, traumatic, stress-sensitivity (early-onset) and anxiety (late-onset) psychoses successfully with patients4 and also found them to be destigmatising.5 They are derived from work which Van Os himself has been pre-eminent in developing and we suggest to him that he has the courage of his convictions and we think caused their psychosis? A Q methodology study. Behav Cogn Psychother 2009; 37: 11–24.


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Author’s reply: In an attempt to come up with new terminology, I sought to combine scientific evidence for valid contrasts with scientific evidence for a mechanism (aberrant assignment of salience) that refers to a psychological process that the general public can recognise and relate to, although a considerable amount of explanation may be necessary (see my reply to Bill George1). Kingdon et al propose a different approach: they select possible risk factors and mechanisms associated with schizophrenia and investigate whether aetiological diagnostic constructs based on these are acceptable to patients. To the degree that their method included an analysis of acceptability to patients,2 there is another opportunity to advance our understanding of the experiences of patients, which is certainly superior to mine. A weakness of the method may be that there is little evidence that, for example, trauma and drug use underlie discrete effects that can be separated diagnostically.

It certainly seems that many are of the opinion that a confusing terminology will not change. Also, the use of the term ‘psychosis’ as a synonym for schizophrenia and investigating whether aetiological diagnostic constructs based on these are acceptable to patients, does not solve the problem of the part of the stigma that is induced by the scientific evidence for valid contrasts with scientific evidence for a mechanism (aberrant assignment of salience) that refers to a psychological process that the general public cannot understand. This is a huge improvement over the term ‘psychosis’ proposed by Kingdon et al. If anything, research suggests that there may be interacting causes that have an impact on the same final common pathway,2,3 although it could certainly be argued that as long as there are established risk factors (although doubts exist4,5) and the terminology is acceptable to patients, this should not prevent their use as aetiological diagnostic constructs: a major problem would remain – acceptability to mental health professionals. How likely is it that these constructs would be accepted by the DSM and ICD committees currently revising diagnostic criteria? In my view, if we really want to abandon the stigmatising term ‘mind-split disease’, it is important to come up with an alternative that is not only acceptable to patients, but also to mental health professionals. The reason for this is that DSM and ICD terminology is by far the most influential in how the general public attempts to understand ‘madness’. Therefore, unless DSM and ICD terminology is changed, the part of the stigma that is induced by confusing and mystifying terminology will not change. Also, the continued use of the term ‘psychosis’ proposed by Kingdon et al may perpetuate the mystification of the experiences of patients, as the public cannot understand this term to make a connection to their own psychological experiences.

The most important issue, however, is how many patients, professionals and other stakeholders want the name to change. It certainly seems that many are of the opinion that a confusing and mystifying 19th-century term should not be used to diagnose patients in the 21st century. Maybe the time has come for the DSM and ICD committees to make a decision on this topic and, if the name change is favoured to develop a process through which a change that is acceptable to as many stakeholders as possible is achieved. The methodology of consulting patients developed by Kingdon et al should figure prominently in this endeavour.


Abortion and mental health disorders

The paper by Fergusson et al,1 accompanied by comments, is a valuable addition to knowledge on this topic, but I should like to mention two issues which limit the usefulness of what is presented.

First, neither Fergusson nor the commentators give sufficient emphasis to the fact that the communities of the Christchurch area of New Zealand are relatively prosperous and well organised compared with those in many parts of the rest of the world. The study findings cannot be extrapolated to communities where poverty, various degrees of malnutrition, and scarce medical and social services are common. In such communities, the modest level of what Fergusson et al call ‘mental disorders’ is likely to be present in many persons whether pregnant or not, and the significance of an unwanted pregnancy is also likely to be quite different from what it might be in more prosperous settings. How these issues interact can only be examined by direct studies in different communities.

Second, one of the commentators (Professor Patricia Casey) presents herself as ‘not a member of any campaigning organisation’ and also lists a number of her other activities to do with abortion and related issues. But there is no mention (probably due to the never-ending search for brevity that plagued us all) of the fact that she is a sincere member of the Roman Catholic Church, and that she always takes what can be called the ‘pro-life’ side in debates about abortion and related issues. Professor Casey is, of course, entitled to her opinions, and I have no doubt that she is proud of her activities in this difficult field and would never wish to hide them. But in these debates we all start from a position determined in part by personal background, and readers will not fully understand comments unless such things are known.


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Author’s reply: Professor Cooper suggests that the findings we report may not describe the linkages between abortion and mental health in communities that are more impoverished than the relatively advantaged New Zealand community that we studied. We agree that it would be rash to generalise our findings to these contexts. We are of the view that it is important that research into this topic is conducted in communities where material and economic conditions may make unwanted pregnancy a far more serious and stressful life event than is the case for relatively...