a low dose. We also wonder why the authors arbitrarily decided to have a tenfold lower dose in the control group. We question why the authors did not try to compare the intervention drug with an existing drug such as olanzapine, as Hill[3] reports that the key point is how a new treatment compares with existing treatment rather than whether it is better than nothing.

1 Haas M, Eerdekens M, Kushner S, Singer J, Augustyns I, Quiroz J, et al. Efficacy, safety and tolerability of two risperidone dosing regimens in adolescent schizophrenia: double-blind study. *Br J Psychiatry* 2009; **194**: 158–64.

2 World Medical Association. *World Medical Association Declaration of Helsinki. Ethical Principles for Medical Research Involving Human Subjects*. WMA, 2008.

3 Hill AB. Medical ethics and controlled trials. *BMJ* 1963; **1**: 1043–9.

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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.

1 Aman MG, De Smedt G, Derivan A, Findling RL, Group RDBS. Double-blind, placebo-controlled study of risperidone in the treatment of disruptive behaviors in children with subaverage intelligence. *Am J Psychiatry* 2002; **159**: 1337–46.

2 Reyes M, Buitelaar J, Toren P, Augustyns I, Eerdekens M. A randomized, double-blind, placebo-controlled study of risperidone maintenance treatment in children and adolescents with disruptive behavior disorders. *Am J Psychiatry* 2006; **163**: 402–10.

3 Stroup TS, Alves WM, Hamer RM, Lieberman JA. Clinical trials for antipsychotic drugs: design conventions, dilemmas and innovations. *Nature Rev* 2006; **5**: 133–46.

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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 al*[2] 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 patients[4] 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 use aetiological concepts rather than nebulous descriptive ones.

1 van Os J. A salience dysregulation syndrome. *Br J Psychiatry* 2009; **194**: 101–3.

2 Dudley R, Siitarinen J, James I, Dodgson G. What do people with psychosis think caused their psychosis? A Q methodology study. *Behav Cogn Psychother* 2009; **371**: 11–24.

3 Kingdon DG, Turkington D. *Cognitive Therapy of Schizophrenia*. Guilford, 2005.

4 Kingdon D, Gibson A, Kinoshita Y, Turkington D, Rathod S, Morrison A. Acceptable terminology and subgroups in schizophrenia: an exploratory study. *Soc Psychiatry Psychiatr Epidemiol* 2008; **43**: 239–43.

5 Kingdon D, Vincent S, Selvaraj S, Kinoshita Y, Turkington D. Destigmatising schizophrenia: changing terminology reduces negative attitudes. *Psychiatr Bull* 2008; **32**: 419–22.

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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 George[1]). 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] their proposal 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. If anything, research suggests that there may be interacting causes that have an impact on the same final common pathway.[3,4] Although it could certainly be argued that as long as there are established risk factors (although doubts exist[5,6]) 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 of '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, in the case a 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.

1 van Os J. Salience dyregulation syndrome: a patient's view (author's reply). *Br J Psychiatry* 2009; **194**: 467.

2 Kingdon D, Gibson A, Kinoshita Y, Turkington D, Rathod S, Morrison A. Acceptable terminology and subgroups in schizophrenia: an exploratory study. *Soc Psychiatry Psychiatr Epidemiol* 2008; **43**: 239–43.

3 Cougnard A, Marcelis M, Myin-Germeys I, De Graaf R, Vollebergh W, Krabbendam L, et al. Does normal developmental expression of psychosis combine with environmental risk to cause persistence of psychosis? A psychosis proneness–persistence model. *Psychol Med* 2007; **37**: 513–27.

4 Houston JE, Murphy J, Adamson G, Stringer M, Shevlin M. Childhood sexual abuse, early cannabis use, and psychosis: testing an interaction model based on the National Comorbidity Survey. *Schizophr Bull* 2008; **34**: 580–5.

5 Macleod J, Davey Smith G, Hickman M, Egger M. Cannabis and psychosis. *Lancet* 2007; **370**: 1539.

6 Morgan C, Fisher H. Environment and schizophrenia: environmental factors in schizophrenia: childhood trauma – a critical review. *Schizophr Bull* 2007; **33**: 3–10.

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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 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 plagues 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, completely 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.

1 Fergusson DM, Horwood LJ, Boden JM. Abortion and mental health disorders: evidence from a 30-year longitudinal study. *Br J Psychiatry* 2008; **193**: 444–51.

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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