

- 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 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 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. Saliency dysregulation 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 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

privileged developed societies. As we point out in our paper, the important implications of our research relate to the interpretation of the abortion laws in legislations such as those in the UK and New Zealand where the mental health risks of unwanted pregnancy are the principal grounds on which abortion is authorised. Our findings suggest that in the New Zealand context, at least, the mental health risks of abortion may outweigh the mental health risks of unwanted pregnancies that come to term but that, in any event, the mental health risks associated with either of these outcomes appear to be relatively small.

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Editor's note: When I commissioned the commentaries on Dr Fergusson's paper I was aware that the subject of abortion tended to polarise opinions. For this reason I commissioned two reviews, one from each side of the debate, but I had confidence from my choice of authors that they would focus primarily on

Dr Fergusson's paper, not the wider issues. I chose Professor Casey as someone who was a pro-life supporter and Dr Oates as a representative of the pro-choice group (with agreement for Drs Jones and Cantwell to be added later), even though I consider these terms somewhat limited and two-dimensional in the context of reviewing a scholarly paper, and believe that specific declarations of interest in this context were unnecessary. I hoped that neither commentary was viewed as tendentious by our readers and personally regard both of them as adding substance to the conclusion of Fergusson *et al* that 'the results do not support strong pro-life positions that claim that abortion has large and devastating effects on the mental health of women. Neither do the results support strong pro-choice positions that imply that abortion is without any mental health effects' (p. 450).¹

- 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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Correction

Group psychoeducation for stabilised bipolar disorders: 5-year outcome of a randomised clinical trial. *BJP*, **194**, 260–265. Table 2, p. 264: the values for Depression should read: Control group 398.55 (364.16); Psychoeducation group 93.28 (165.46). This was a typographical error only and does not affect the statistical analysis presented.

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