

## Correspondence

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## Income inequality and mental health problems

Pickett & Wilkinson's paper is the latest in a series of persuasive publications on income inequality and health stretching back to the 1990s.<sup>1</sup> However, in relation to what they variously call 'mental illness', 'mental health problems' and 'distress', I wonder whether they are taking at face value the highly inflated prevalence findings they cite. They write that 'one million British children – one in ten between the ages of 5 and 16 – are mentally ill' and that 'one in four adults have been mentally ill in the past year' in both the USA and the UK. These mostly represent the so-called common mental disorders. These figures are preposterous – as much to the citizen as to the psychiatrist in me – and make an urgent case for the profession to go back to the drawing board to revisit the core question: just what exactly do we mean by a 'mental disorder'? Quantitative surveys tend to recast the epiphenomenal features of situational distress as free-standing disorder, so cannot but recruit false positives on a systematic scale.<sup>2</sup>

The authors have plotted 12 countries onto a graph of 'Percentage with any mental illness' v. 'Income inequality', and it seems noteworthy that the strongest correlations are, successively, USA, UK, Australia, New Zealand and Canada. Below is France and, much lower down, The Netherlands, Belgium, Spain, Germany, Japan and Italy. I wonder why there is this apparent split between English-speaking countries and the rest, and whether this reflects particularities in the Anglo-American world, both in psychiatric culture and in trends towards the psychiatrisation of everyday life, which may be less advanced elsewhere. Taking account of possible skews of this kind would be likely to make the graph line rather less steep. Could the authors comment?

The authors acknowledge that a possible confounder in comparing rates of mental illness between one society and another lies in differing recognition and interpretation of survey questions, but go on to make the point that 'at least the same diagnostic interviews are used in each country'. I am afraid this is to restate the problem, which is one of validity, rather than to resolve it. Valid research methods must reflect the 'nature of reality' for participants and a standard questionnaire used across heterogeneous societies cannot do this.

Pickett & Wilkinson conclude that if the UK is to reverse the massive rise in inequality experienced during the 1980s, 'we need to encourage all mechanisms that help to reduce income differences'. But are we not all stuck with an intractable feature of late capitalism, its structural tendency to stratify incomes rather than to level them out?

- 1 Pickett KE, Wilkinson RG. Inequality: an underacknowledged source of mental illness and distress. *Br J Psychiatry* 2010; **197**: 426–8.
- 2 Summerfield D. Cross-cultural perspectives on the medicalization of human suffering. In *Posttraumatic Stress Disorder: Issues and Controversies* (ed G Rosen). John Wiley, 2004.

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**Authors' reply:** Summerfield suggests that the World Health Organization<sup>1</sup> and other survey data that we use seem 'preposterous' to him as a doctor and a citizen. His quarrel, then, is not with us but with the psychometric testing of the diagnostic interviews used by the WHO and other epidemiological surveys of mental illness. But even in terms of our own personal experience, we are not at all surprised at the 23% annual period prevalence of any mental illness in the UK. Many of us have felt incapacitated by depression or anxiety, and among our acquaintances we can count episodes of self-harm, eating disorders, addictions, behaviour problems and autism-spectrum disorders. As we mentioned in our paper, episodes of severe mental illness are also strongly correlated with income inequality.<sup>2</sup> Both sets of data suggest that inequality is related to mental health, however we choose to label the symptoms.

It is wrong to suggest that the correlations reflect only the high measured prevalence of mental illness in the English-speaking countries. Although these countries do indeed have higher prevalence of mental illness, and higher levels of income inequality, they are not outliers and do not appear to represent a distinct group: they are simply the countries at one end of the distribution. Indeed, if we look only at the subsample of the English-speaking countries, income inequality remains significantly correlated, and is an important explanatory factor for mental illness just among them ( $r=0.95$ ,  $P=0.01$ ).

It would be odd if the relationships we showed with mental illness existed in a vacuum but of course they do not. Our research focuses on problems with social gradients, and we find that more unequal societies also have lower levels of trust and social capital, poorer physical health, higher rates of obesity and teenage pregnancies and births, low child well-being, educational achievement and social mobility, and higher levels of violence and imprisonment.<sup>3</sup> Against that background it would be surprising if mental health were not also affected by wider income differences.

Until the rise of neoliberal economic policy in the 1980s, the UK was a much more equal society and it could be so again. We are optimistic that societies can change. There are numerous mechanisms through which governments and institutions can promote greater equality, and a wider recognition of the harm caused by inequality is an essential prerequisite.

The reality is that inequality causes real suffering – regardless of labels. Those of us concerned with the mental health of the public need to address its structural, as well as its individual, context.

- 1 Demyttenaere K, Bruffaerts R, Posada-Villa J, Gasquet I, Kovess V, Lepine JP, et al. Prevalence, severity, and unmet need for treatment of mental disorders in the World Health Organization World Mental Health Surveys. *JAMA* 2004; **291**: 2581–90.
- 2 Pickett KE, Wilkinson RG. Inequality: an underacknowledged source of mental illness and distress. *Br J Psychiatry* 2010; **197**: 426–8.
- 3 Wilkinson R, Pickett K. *The Spirit Level: Why More Equal Societies Almost Always Do Better*. Penguin, 2009.

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## Omission of evidence about 5-year outcomes

Bird *et al*'s review of early psychosis intervention provides a useful meta-analysis of methodologically sound studies.<sup>1</sup> However, there are major problems with it. The authors have stated that their

review focuses on the first 3–5 years following onset of illness. Yet they have omitted the 5-year results from the Danish OPUS study<sup>2</sup> and the UK Lambeth Early Onset (LEO) study.<sup>3</sup> Both of these follow-up studies found that, despite promising early results,<sup>4,5</sup> which were included in Bird *et al*'s review, positive effects were not sustained at 5 years.

Bertelsen *et al* concluded that intensive early intervention improved clinical outcomes in OPUS after 2 years, but the effects were 'not sustainable up to five years later'.<sup>2</sup> This finding was not reported by Bird *et al*. In fact, the Bertelsen *et al* study was not cited at all. According to a personal communication from the authors, it was included in their review. However, they used primary references (in this case Petersen *et al*'s analysis of 1- and 2-year outcomes<sup>4</sup>) to refer to all papers for all the trials included in the review. This seems idiosyncratic to say the least. More important, all they reported about the Bertelsen *et al* study is that 'Only one trial of an early intervention service provided long-term data (up to 5 years post-randomisation)'.

Gafoor *et al* similarly found that specialist early intervention did not markedly improve outcomes at 5 years in LEO,<sup>3</sup> in accord with the 5-year findings from OPUS. Again this was not reported and the study was not cited. Bird *et al*'s review was initially submitted in January 2009, long before the publication of Gafoor *et al*'s study in this journal, but the final revision occurred after the latter was published. Although it would not have been practical to include Gafoor *et al* in the meta-analysis, publication of the review could have been delayed, if necessary, to allow a brief discussion of Gafoor *et al*'s findings to be added. They significantly strengthened the evidence that promising early benefits are not sustainable, a very significant finding for a review of the effectiveness of early intervention in psychosis.

Bird *et al* have concluded that 'it remains to be determined whether the effects of early intervention services are sustained', yet they have omitted the best evidence of exactly that.

- 1 Bird V, Premkumar P, Kendall T, Whittington C, Mitchell J, Kuipers E. Early intervention services, cognitive-behavioural therapy and family intervention in early psychosis: systematic review. *Br J Psychiatry* 2010; **197**: 350–6.
- 2 Bertelsen M, Jeppesen P, Petersen L, Thorup A, Øhlenschlaeger J, le Quach P, et al. Five-year follow-up of a randomized multicenter trial of intensive early intervention vs standard treatment for patients with a first episode of psychotic illness: the OPUS trial. *Arch Gen Psychiatry* 2008; **65**: 762–71.
- 3 Gafoor R, Nitsch D, McCrone P, Craig TKJ, Garety PA, Power P, et al. Effect of early intervention on 5-year outcome in non-affective psychosis. *Br J Psychiatry* 2010; **196**: 372–6.
- 4 Petersen L, Jeppesen P, Thorup A, Abel MB, Øhlenschlaeger J, Christensen TØ, et al. A randomised multicentre trial of integrated versus standard treatment for patients with a first episode of psychotic illness. *BMJ* 2005; **331**: 602.
- 5 Craig TK, Garety P, Power P, Rahaman N, Colbert S, Fornells-Ambrojo M, et al. The Lambeth Early Onset (LEO) Team: randomised controlled trial of the effectiveness of specialised care for early psychosis. *BMJ* 2004; **329**: 1067.

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**Authors' reply:** We acknowledge Dr Raven's point about not having considered the 5-year follow-up data fully. Although we noted in our discussion that the evidence for long-term follow-up was limited,<sup>1</sup> we thank Dr Raven for bringing these recent studies to our attention. At the time of submitting the review, the Bertelsen *et al* paper<sup>2</sup> was the only one to have examined the effects of early intervention services at 5-years following randomisation. The study showed no beneficial effect

of such services over standard care in terms of positive, negative and general functioning symptoms, making its unique finding tentative. Furthermore, as nearly 50% of participants were not included in the analysis,<sup>2</sup> we felt it would be best to include the lack of evidence as a limitation in the Discussion.

As highlighted by Dr Raven, the more recent paper by Gafoor and colleagues<sup>3</sup> was published after our review had been submitted. This paper also suggests that the beneficial effects of early intervention services at 5-year follow-up are not sustained in terms of number of readmissions, giving more certainty to the view that the beneficial effects of these services may not be sustained once the treatment is ended. It is worth noting that in both studies, the intensive early intervention services were phased out after the end-point data collection period. In our review we concluded that the available evidence 'raises the possibility that comprehensive services comparable to those described here as early intervention services, which include a full range of evidence-based psychological interventions, should be considered for people with established psychosis'.<sup>1</sup> The fact that the effects of early intervention services were not sustained once individuals were referred back to standard care, as demonstrated in the two studies, we think supports this idea. We did not think that it appropriate to delay the paper, as we feel that our conclusion is consistent with that reported by Gafoor and colleagues, who note: 'Aside from limited statistical power, the absence of a difference in outcome between the two groups at 5 year follow up may reflect the withdrawal of the specialised intervention after 18 months (when there was a significant group difference), further investigation of this issue will require trials involving longer duration of specialised treatment'.<sup>3</sup>

It is useful that Dr Raven has brought these papers into the discussion and we feel that, on balance, the evidence from our review is still supported. Although there is now some evidence that the long-term effects of early intervention services in their present format may not be sustained once treatment is removed, a meta-analysis of long-term outcomes would still not be possible, as the papers do not share any common measures of outcome. Therefore, we still believe, as do Gafoor and colleagues,<sup>3</sup> that further research examining all these outcomes is warranted. Furthermore, research is needed to assess the effectiveness of services akin to the early intervention services that we studied, namely ones that provide a high level of support and a full range of interventions for all individuals at any stage of psychosis.

- 1 Bird V, Premkumar P, Kendall T, Whittington C, Mitchell J, Kuipers E. Early intervention services, cognitive-behavioural therapy and family intervention in early psychosis: systematic review. *Br J Psychiatry* 2010; **197**: 350–6.
- 2 Bertelsen M, Jeppesen P, Petersen L, Thorup A, Øhlenschlaeger J, le Quach P, et al. Five-year follow-up of a randomized multicenter trial of intensive early intervention vs standard treatment for patients with a first episode of psychotic illness: the OPUS trial. *Arch Gen Psychiatry* 2008; **65**: 762–71.
- 3 Gafoor R, Nitsch D, McCrone P, Craig TKJ, Garety PA, Power P, et al. Effect of early intervention on 5-year outcome in non-affective psychosis. *Br J Psychiatry* 2010; **196**: 372–6.

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