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# Correspondence

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## Letter to the Editor

### Prevention is better than cure: a reply to McKenzie, March *et al.* and Selten & Cantor-Graae

We are grateful to the commentators for their constructive observations on our review. We agree with Kwame McKenzie (2009) that consensus needs to be built; the key point we attempted to make is that, to gain such a consensus, the problem of high rates of psychosis in migrant and minority ethnic populations needs to be de-coupled from the no less important issue of service provision for minority ethnic patients. In the same way that improving customer services for insurance claimants following an accident is irrelevant to reducing the rate at which such accidents occur, so reforming mental health services (important as this no doubt is) will have no impact on population rates of disorder.

There are a number of points raised in the commentaries that we would like to address further. We were careful not to go beyond the available evidence in making the case that cumulative 'social adversity' across the life course is important in explaining the high rates. In noting these 'adversities' we were as specific (and vague) as the evidence allows. Here, we agree with March *et al*. (2009) and Selten & Cantor-Graae (2009) that future research needs to be significantly more sophisticated in how it captures the social structures and lived experiences that impact, over the course of development, on risk of psychosis. In this, socioeconomic disadvantage, usually based on some measure of 'social class', is no more of a conceptual advance than social adversity. What the evidence currently suggests is that a range of contexts and experiences are likely to be relevant (from hostile neighbourhood environments to childhood trauma to experiences of discrimination, and so on). What March *et al*. rightly allude to is the complexities involved and the inordinate methodological challenges that we face in disentangling these.

From this, much as 'social defeat' is a succinct and appealing hypothesis, we think it doubtful that the range of adversities that appear relevant over the life course can be so readily collapsed into a single exposure. In humans, the kinds of experiences that might comprise social defeat (entrapment, loss, humiliation) have been more consistently linked with depression (Harris, 2001) and indeed the 'social defeat' paradigm was originally developed as a model of depression (Bjorkqvist, 2001). In contrast, exposure to intrusive and anxiety-provoking events and contexts may be particularly relevant to psychosis, i.e. not a resulting state of defeat and learned helplessness, but a state of heightened sensitivity to the external environment and its perceived hazards. On this, there are indeed studies in humans that suggest the dopaminergic system is altered in response to adverse and stressful early environments (De Bellis, 1994; Pruessner, 2004). That this is the case further supports the proposition that a much broader range of experiences are potentially relevant – it is not only 'social defeat' that has the potential to sensitize the dopaminergic system. This reminds us that, while animal research can provide clues, we need to be cautious in borrowing terminology and applying findings to humans. There is much that can be lost in translation.

All of this said, as much as greater sophistication in research will further our understanding, we need not sit back and wait for this to accrue before advocating change at policy and service delivery levels. As Brian Cooper (Cooper, 1992) commented nearly 20 years ago in relation to the same issue: 'the history of public-health epidemiology, from cholera to bronchial carcinoma, has repeatedly demonstrated that effective preventive measures can precede the full causal elucidation of a disease' (p. 597).

Of course, achieving significant policy change that may ameliorate the impact of social disadvantage, particularly in high-risk groups, is extremely difficult. But we do not agree, as Selten & Cantor-Graae suggest, that this is primarily a job for politicians. Psychiatry has a role – perhaps even a moral responsibility – in advocating for, and contributing where possible to, the implementation of change. On a daily basis, psychiatry encounters the consequences of social inequalities, trauma, and negative life experiences in the private miseries for which patients and their relatives seek help. As we have seen in the UK recently, left to themselves politicians are as likely to tend their homes and gardens at the public's expense as they are to take seriously the needs of those they purport to represent. It is a failure for us all that, despite over 40 years of evidence, the high rates of psychosis in the Black Caribbean and other migrant populations remain of no concern to our governments.

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## Letter to the Editor

### A comment on Lynch *et al.* (2009)

Meta-analysis (MA) is an essential tool for summarizing evidence for a specific intervention, but is prone to bias and not objective *per se*. Because many MAs have failed to report procedures in a transparent way that enables readers to assess strengths and weaknesses, a group of researchers developed the QUORUM guidelines (Moher *et al*. 1999; update: Moher *et al*. 2009). These list 19 major criteria which are deemed essential for transparent reporting of the method and results in a systematic review (overall there are 27 guidelines, referring to title, abstract, introduction, methods, results, discussion and funding). Lynch *et al*. (2009) only comply with five of these. For example, they do not present the full electronic search strategy including search terms or describe the process of study selection (e.g. screening, determining eligibility) or the process of data extraction (e.g. were different raters involved in the data extraction and how did they agree?), they do not list and define all variables for which data was sought and, although they emphasize the risk of over-interpreting results from methodologically weak studies, they do not describe methods for assessing risk of bias in the included studies, such as quality of randomization and blinding or drop-out rates. Moreover, they do not transparently describe the synthesis of results. The results section contains no flow diagram of the study selection or numbers of studies screened and there is no description of the included studies with regard to relevant study characteristics.

This lack of reporting makes it extremely difficult to understand their selection of studies. For example, one study that used an active control design (Levine *et al*. 1998) and was included in other meta-analyses (Lincoln *et al*. 2008; Wykes *et al*. 2008) were not even listed in the list of excluded studies (see supplementary online Appendix in Lynch *et al*. 2009). Whereas a study by Hogarty *et al*. (1997) that used an intervention that was not considered as CBT by the author of that study or the authors of other meta-analyses, was included. Some studies were excluded because of using additional elements in the intervention, such as motivational interviewing or family inclusion whereas others were included although they also used motivational interviewing (Haddock *et al*. 2009) or involved family members (Drury *et al*. 1996). Other exclusion criteria are listed more explicitly but lack a strong rationale. For example, why was the label 'pilot study' an exclusion criteria, given all other criteria were fulfilled? This resulted in the exclusion of two relevant studies. Further, why was relapse restricted to defined symptom changes whereas studies focusing on rehospitalization or follow-up symptom scores – for which beneficial effects of CBT have been demonstrated (Lincoln *et al*. 2008) – were excluded? Despite other disadvantages, rehospitalization rates or days would have been the least prone to observer bias, which is what the authors were aiming at. Alone, the exclusion of studies that focused on rehospitalization reduced the pool of relevant studies by another five. Finally, a number of not previously defined exclusion criteria were added in the results section or appeared in the list of excluded studies, such as co-morbid substance abuse, the use of cognitive remediation as a control intervention, exceeding a certain percentage of affective psychoses or the use of 5-year follow-up periods. These criteria reduced the number of included studies by a further five. As the authors do not, in fact, restrict their analyses to blind or