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Letter to the Editor: The midlife peak in distress amongst the disadvantaged and existing ideas about mental health inequalities over the lifespan

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Letter to the Editor

The midlife peak in distress amongst the disadvantaged and existing ideas about mental health inequalities over the lifespan

Numerous large-scale studies have now shown that psychological distress rises steadily from early adulthood to middle age before declining and then levelling off in old age (e.g. Blanchflower & Oswald, 2008; Stone *et al.* 2010). Lang *et al.* (2011) examined data from over 100 000 people to provide the first substantive evidence that age-related changes in psychological distress differ as a function of income. Specifically, distress, psychiatric diagnoses and the use of psychiatric medicine were found to peak sharply in midlife primarily amongst those in the bottom 20% of the income distribution. These observations are consistent with findings showing that financial pressures and finance-related subjective stress peak in midlife (Almeida & Horn, 2004). The less well off could therefore be at particularly high risk of financial strain at this time. In addition, a lack of financial resources during middle age may generate a spiral of stress in other aspects of life (e.g. interpersonal tension, sleep and work difficulties).

If the findings of Lang *et al.* (2011) are shown to be robust (e.g. to period and cohort effects) they may shed new light on contradictions in existing

conceptualizations of mental health inequalities over the lifespan. At present, two competing hypotheses propose that: (i) mental health inequalities increase continually over the lifespan (cumulative disadvantage hypothesis), and (ii) mental health inequalities converge with age potentially as a result of selective mortality amongst the disadvantaged (age-as-leveler hypothesis). Support has been gathered for both ideas (e.g. Dupre, 2007; Yang, 2008).

Interestingly, Lang *et al.* (2011) find evidence that disparities in distress widen in the first half of life and are attenuated following middle age. It follows that those examining the trajectory of socio-economic differences in distress in samples of young adults may observe findings which are compatible with the cumulative disadvantage hypothesis, as illustrated in Fig. 1. Support for the age-as-leveler hypothesis could tend to occur in samples of older adults, as illustrated by the post-midlife section of Fig. 1. Those who test for linear (rather than a quadratic) interactions between age and socioeconomic status in predicting mental health amongst adults of all ages may observe parallel lines (as the inclines and declines in distress amongst the disadvantaged will cancel each other out) and no interaction effect.

Further research will help evaluate the merit of these predictions. However, it is clear that studies examining income-related differences in psychological distress do need to incorporate quadratic age effects. The consideration of such U-shapes may act as a catalyst for developing a deeper understanding of trajectories of mental health inequalities over the lifespan. A first step in this direction will be to establish if the U-shape in distress and well-being identified in numerous studies, rather than being normative, tends to occur centrally amongst an economically disadvantaged subset of the population.

Declaration of Interest

None.

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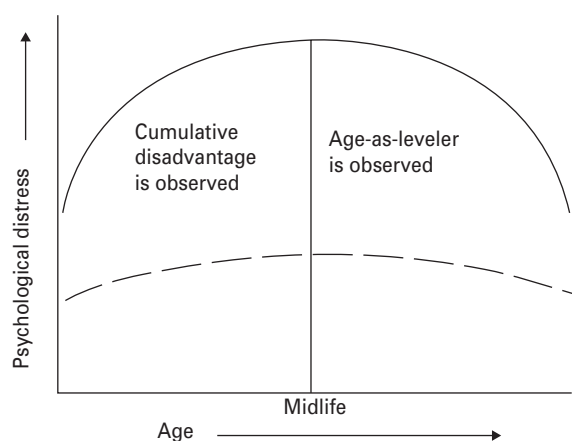


Fig. 1. Curvilinear relationships between age and psychological distress for those with low (—) and high (---) income.

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Letter to the Editor

Well controls in case-control studies

We were interested to read June's *Psychological Medicine* in which one of the methodological problems found in psychiatric case-control studies was examined (Schwartz & Susser, 2011). The authors gave a hypothetical example of the use of 'supernormal' well controls where a certain disorder was an exclusion criterion for controls but not for cases, creating a difference in the prevalence of this disorder between the groups. This disorder had its own strong relationship with the exposure and overwhelmed any which may have existed between the exposure and the intended outcome, leading to misleading results. They point out that this practice has no conceivable benefits and should be discouraged.

Schwartz & Susser cite our work quantifying the prevalence of this and other problems in the general psychiatric literature for the years 2001 and 2002 (Lee *et al.* 2007). We found 32/408 (8%) of the studies were guilty of using supernormal controls and that the problem could not be excluded in a further 145 (36%). This left 231 (57%) of the studies reporting applying the same recruitment criteria to cases and controls, in accordance with best practice.

However, the particular issue of differential recruitment criteria was only the 14th most prevalent methodological problem of 17 examined. The most prevalent problems were poor reporting of the sampling of participants and poor descriptions of the

cases, including whether incident cases were recruited. In general, non-reporting of key methodological issues was at least as great a problem as the reporting of poor methodology.

Since we collected our data, the STROBE (STrengthening the Reporting of OBServational studies in Epidemiology) Statement has been published (von Elm *et al.* 2007). It is a consensus statement documenting best practice of reporting observational epidemiology and is now officially adopted by at least 106 biomedical journals (STROBE, 2011), and four of the six journals we examined. The two journals which neither refer to the Statement in their instructions to authors nor refer to the Uniform Requirements published by the International Committee of Medical Journal Editors (which themselves refer to the Statement) are the *British Journal of Psychiatry* and *Psychological Medicine*, ironically the very journals which published these two methodological articles.

We suggest it is of particular importance for more general journals to insist on the use of guidelines of this kind because the existence of the powerful biases which may overwhelm true findings in case-control studies may not be acknowledged by all researchers in all domains of study. It is hoped that better reporting will lead to better methodology and therefore more valid results in observational epidemiology, including case-control studies in mental health research.

Declaration of Interest

None.

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