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Letter to the Editor

Neighbourhood communalities and ultra-high risk psychosis: An emergent literature

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Letter to the Editor

The work of O'Donoghue et al. (2015) provides a welcome contribution to the emergent field exploring the association between the wider neighbourhood environment and the rate of ultra-high risk [UHR] states for psychosis.

A large literature already demonstrates that the incidence of psychotic disorders, and most strongly non-affective psychotic disorders, is raised in populations exposed to urban living (Kirkbride et al., 2012). However, epidemiological studies conducted on the basis of residency close to the onset of psychosis have not been able to exclude the possibility that reverse causation, or social drift, explains such associations, whereby people in the prodromal phase of schizophrenia are more likely to move into more urban areas, often characterised by greater levels of social disadvantage (or less likely to be able to move out of them). While findings which show strong dose-response gradients between urban birth and later psychosis risk provide some evidence against the possibility of social drift (Mortensen et al., 1999), a further way to triangulate this hypothesis is to study the neighbourhood characteristics of people who meet UHR criteria for psychosis. Measuring the association between exposure to urban environments during or before the prodromal phase of psychotic illness, potentially allows us to get a better grip on the direction of causal-ity between urban residency and later psychosis risk.

Studies such as that by O'Donoghue et al. (2015) are thus a potential boon to aetiological understanding of psychotic disorders. They also provide important information for mental health service planners, who may be interested in understanding which populations are most likely to express need for psychiatric care, which operates over a strong social and spatial gradient (Kirkbride et al., 2013). In their paper, based on a sample of 166 people who met UHR criteria living in a defined catchment area in Melbourne, Australia, O'Donoghue et al. (2015) show that the UHR incidence varies by deprivation, with a weak linear trend towards higher UHR rates in more deprived areas (RR: 1.51; 95%CI: 0.93–2.53; $p = 0.08$). However, further inspection of this data suggests that (a) absolute rates were higher in the most affluent parts of their study area compared with the most deprived, and (b) the weak linear trend smooths over apparent non-linearity in the relationship between deprivation and UHR incidence.

O'Donoghue et al. (2015) suggest that their results “[cast doubt] on recent findings of a higher distribution of UHR cases in more affluent

neighbourhoods (Kirkbride et al., 2015)”. By contrast, we suggest that their findings are, in fact, consistent with those observed in our sample in the East of England (Kirkbride et al., 2015). Both studies, for example, used identical criteria to define UHR groups. Our multivariable results, based on a population-based sample, did indeed show that the odds of receiving UHR status (versus control) were increased in more affluent (or less deprived) neighbourhoods. On one level this is consistent with O'Donoghue et al.'s (2015) observation of higher UHR rates in their most vs least affluent areas.

On another level, the data from both papers suggests that the relationship between deprivation and UHR status is probably non-linear, and such a finding is consistent with some evidence on the incidence of psychotic disorders (Croudace et al., 2000). In our paper, the negative association between deprivation per se and UHR for psychosis only appeared after adjustment for other neighbourhood-level characteristics, including the proportion of single-parent households, which showed a strong trend towards increased risk of UHR status (RR: 1.59; 95%CI: 0.99–2.57; $p = 0.056$). Given that single-parent household status was highly correlated with neighbourhood deprivation ($r = 0.66$) in our sample, we suggest that “[h]aving controlled for this, the counterintuitive negative association between multiple deprivation and psychosis proneness might indicate a nonlinear relationship.” (Kirkbride et al., 2015). Interpreted in this light, our findings also accord with O'Donoghue et al. (2015), given the presence of non-linearity between deprivation and the rate of UHR groups in their data (see their Table 1). Both papers were based on relatively small epidemiological samples of people at UHR for psychosis, making it important to replicate these findings in further, sufficiently-powered studies. Given the high levels of heterogeneity which likely exist in UHR samples (Fusar-Poli et al., 2016), careful delineation of the group(s) at risk will also be required in future studies.

We hope that our reply comment adds some further insight into the results from both O'Donoghue et al. (2015) and Kirkbride et al. (2015), which represent an emergent, important area for further research. Our conclusion remains the same: taken together, these observations support the possibility that people with increased psychosis proneness are exposed to more adverse social environments during the at-risk period. This has potentially important implications for both aetiology and public mental health.

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Conflicts of interest

None declared.

Contributors

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