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## Homicide rates and income inequality

There is evidence that psychosocial factors other than those discussed by Swinson *et al*[1] affect homicide rates and it is important to know whether these disproportionately affect individuals diagnosed as mentally ill. Specifically, there is evidence that income inequality strongly influences rates of violent crime, including homicide.[2] Wilkinson & Pickett have claimed that changes in inequality also influence rates of substance misuse.[3] It is thus important to know whether the increase in homicide rates described by Swinson *et al* could be caused by those with psychiatric problems being 'left further behind' in terms of income and/or social status.


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**Authors' reply:** We were looking for factors which corresponded to the overall rise in homicides in people with psychosis; factors which showed increases of a similar magnitude, over a similar timescale. This was the case for drug misuse, allowing us to infer an association. Evidence has been found linking income inequality to both violent crime[1] and rates of substance misuse,[2] although this has been disputed and there is controversy[3] over the validity of the association found between income inequality and mental illness.[4] There has been a marked increase in income inequality in recent years[5] but, from the data which we have available to us, we are unable to comment as to whether this is also the case among those with mental illness, and whether there is any causal association with homicide rates. In future research we hope to explore the data using deprivation indices which might provide further information on any association between income inequality, mental illness and homicide.


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## Observational BALANCE

We read with interest Kessing *et al*'s timely and welcome paper[1] supporting, by way of observational cohort study, the findings of BALANCE.[2] Lithium again is shown to be superior to valproate for the management of bipolar disorder. The strength in this case comes from bridging the gap between the relatively brief follow-up in randomised control trials (RCTs) and the real-life situation faced by clinicians managing a lifelong illness of unpredictable course. Although the enriched study design in BALANCE aimed to maximise the generalisability of the findings to a clinical population, limitations inevitably remained in terms of including patients who had shown a differential previous response to either lithium or valproate, diagnostic heterogeneity within the sample population, and frequency of comorbidity compared with the general population. The limitations of observational cohort studies are multiple and well documented. One key concern is confounding by indication, but more general problems exist with group biases and masking of cause and effect relationships.

Kessing *et al* used 'switch to' and 'add on' as proxy outcomes for the efficacy of mood stabilisers. It would have been interesting, if possible, to separate the 'switch to' group from the 'add on' groups. The 'add on' outcome probably represents a treatment failure; however 'switch to' is likely to be a combination of lack of efficacy and poor tolerability. Indeed, their findings suggest that the initial, very rapid increase in incidence of switch/add on is related to tolerability rather than efficacy, whereas in BALANCE this finding would have been lost by drop-out during the run-in period. This is unlikely, however, to explain the superiority of lithium that is clearly present in both outcome measures.

It was previously argued that observational studies would overestimate treatment effects and that they hold little value in assessing therapies; however, comparative studies with RCTs, across various branches of medicine have now dismissed this.[3] This sort of complementary approach, reconfirming findings from RCTs over long follow-up periods, is an important addition to the evidence base for treatment. This is especially true in areas where the disorder under investigation is chronic relapsing–remitting, and when the exclusion criteria of RCTs can often mean that external validity is low. If, as has been suggested, bipolar disorder is a heterogeneous condition with subtypes associated with preferential response to specific mood stabilisers[4] (which can be identified by symptoms, clinical course and family history), then the observational study carries even more weight when compared with the RCT as it 'allocates' patients to treatments on the basis of



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predicted response, rather than randomisation. Bias can then be minimised by propensity score matching[5] (controlling for unmeasured bias between study groups), although this method was not employed by Kessing *et al*.


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**Authors' reply:** We certainly agree on the mentioned advantages and disadvantages of observational studies and on the strengths of combining findings from randomised trials with those of observational studies.

Further, we agree on the possibility of the suggested analyses with 'switch to' and 'add on' as two separate outcomes. We chose the combined outcome measure as using two separate outcome measures (in addition to hospitalisation as an outcome measure) would decrease the statistical power to a low level in some of the analyses. In addition, one of the advantages of using the combined outcome measure is that the results may turn out to be more clear to guide clinical decisions on whether to use lithium or valproate in long-term treatment of bipolar disorder following a number of clinical situations (depression, mania, mixed episode or remission).

Propensity score matching (or other ways of introducing propensity score in the analysis[1]) is a viable alternative to the approach based on multiple Cox regression models used in our paper. However, much experience (e.g. Sturmer *et al*[2]) suggests that the results thus obtained would not tend to be substantially different. The limiting factor seems to be the available amount of covariate information.


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## Role of postcards in reducing suicidal behaviour

The article by Hassanian-Moghaddam *et al*[1] provides useful insights into the potential utility of postcard intervention in reducing suicidal behaviour. The authors by virtue of this study have found that among participants who had self-poisoned, nine postcards sent sequentially over a period of 12 months produced reduction in suicidal ideation and suicide attempts. The study deserves accolades for various reasons, including a large sample from a non-Western population and a randomised control design, ensuring an over 90% retention rate and nearly equal rates of loss to follow-up in both groups. The results of the study are illuminating but their generalisability and applicability in day-to-day clinical practice needs to be analysed against the backdrop of following limitations.

(a) The study provided for assessment of outcomes only at 12 months. It would have been better if the assessments were performed more frequently such as once in 2 or 3 months.

(b) The study at no point assessed suicidal intent among participants.

(c) Instead of employing any standard sampling technique, the participants of the study included consecutive individuals with poisoning, admitted from March to June 2006 in the Loghman-Hakim Poison Hospital.

(d) Baseline assessment did not include a comprehensive psychiatric evaluation that could have ascertained the specific psychiatric diagnosis of the participants and permitted subgrouping of the participants based on psychiatric diagnosis, thereby providing a valuable opportunity to study the differential impact of postcard intervention in reducing suicidal ideation and suicidal attempt among the participants with different psychiatric disorders.

(e) There is no mention in the article of whether the delivery of the postcards was confirmed by the recipients.

(f) The participants were masked to study outcomes but the research psychologist was not masked to allocation, and this could have inadvertently influenced responses at follow-up.

(g) Individuals may have got some clue about the study outcomes from the questions asked of them and this could have influenced the final results of the study.

(h) A small minority of participants withdrew from the postcard intervention but the specific reasons for the same were not assessed.

To make the postcard intervention more acceptable and effective, one needs to ascertain the specific reasons which made the participants withdraw from this intervention.


1 Hassanian-Moghaddam H, Sarjami S, Kolahi A, Carter GL. Postcards in Persia: randomised controlled trial to reduce suicidal behaviours 12 months after hospital-treated self-poisoning. *Br J Psychiatry* 2011; **198**: 309–16.


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**Authors' reply:** Drs Jhangee & Bhatia have mentioned a number of strengths and limitations, which were specifically addressed in the paper. The other issues that were raised are addressed below.

(a) Postcards are a minimal intervention sustained over 12 months. Optimal assessment is end of treatment and at follow-up, which allows comparison with similar studies.[1,2] Repeated contact and assessment might 'wash out' the effect



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