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*.

1 Kessing LV, Hellmund G, Geddes JR, Goodwin GM, Andersen PK. Valproate *v.* lithium in the treatment of bipolar disorder in clinical practice: observational nationwide register-based cohort study. *Br J Psychiatry* 2011; **199**: 57–63.

2 Geddes JR, Goodwin GM, Rendell J, Azorin JM, Cipriani A, Ostacher MJ, et al. Lithium plus valproate combination therapy versus monotherapy for relapse prevention in bipolar I disorder (BALANCE): a randomised open-label trial. *Lancet* 2010: **375**: 385–95.

3 Black N. Why we need observational studies to evaluate the effectiveness of health care. *BMJ* 1996; **312**: 1215–8.

4 Alda M, O'Donovan C. A much needed BALANCE. *Bipolar Disord* 2010; **12**: 678–80.

5 Rosenbaum PR, Rubin DB. The central role of the propensity score in observational studies for causal effects. *Biometrika* 1983; **70**: 41–55.

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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.

1 D'Agostino Jr RB. Tutorial in biostatistics. Propensity score methods for bias reduction in the comparison of a treatment to a non-randomized control group. *Stat Med* 1998; **17**: 2265–81.

2 Sturmer T, Joshi M, Glynn RJ, Avorn J, Rothman KJ, Schneeweiss S. A review of the application of propensity score methods yielded increasing use, advantages in specific settings, but not substantially different estimates compared with conventional multivariable methods. *J Clin Epidemiol* 2006; **59**: 437–47.

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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

of intervention and telephone contacts might specifically influence suicide attempts.[3] The costs for three assessments for over 2000 participants would have been considerable and the additional benefits of end-points measured before treatment completion are unlikely to offset the additional costs.

(b) Instruments assessing suicidal intention (rather than ideation) are contextualised to an episode of self-harm, suicide attempt or ideation. These were relatively uncommon and so intention would only have been measureable in a minority, if there was an instrument for the relevant languages and shown to be valid in the study population. Had there been such an instrument it might have been considered for baseline assessment.

(c) Using consecutive admissions is superior to any alternate sampling strategy. We acknowledged the limitations of restriction to a 4-month period.

(d) Psychiatric diagnostic assessments were done for all in-patients. We were mindful of the dangers of subgroup analyses in general. Initially we analysed for gender based on benefit only for women[1,2] and a differential gender repetition rate of self-harm or poisoning in Western populations. We accepted the editorial suggestion of a second analysis based on previous suicide attempt at baseline, since this might be the highest risk factor for subsequent suicidal behaviour. Postcards in Persia and Postcards from the EDge intended to develop interventions available to almost all emergency departments with patients who had self-harmed, even emergency departments without psychiatric services required for diagnosis; so analysis based on psychiatric diagnosis was of low importance. We have tested alternate approaches to psychiatric diagnosis, which had low agreement with clinical diagnosis.[4]

(e) There were several *post hoc* analyses based on recall of the number of postcards received. Since this was an efficacy trial, we conducted the main analyses based on randomisation, not exposure or dosage of the intervention.

(f) The research psychologist was not masked to allocation and may have inadvertently influenced responses at follow-up. Participants may have guessed the study end-points from questions asked of them, but their reports of the hospital-treated suicide attempts were found to be accurate.

(g) There were two points in the paper that suggested that a substantial response bias was unlikely. The report of hospital treated episodes was accurate. Although ideation and attempt were significantly different, self-cutting was not, which would require a differential response bias in favour of two outcomes but against another.

(h) It would be useful to know the reasons for withdrawal. However, less than 2.3% of the treatment group withdrew, suggesting acceptability was rather good and improved retention in treatment would be small. The most innovative analysis addressed the issue of the possible impact caused by individuals withdrawn or lost to follow-up. We expect that sensitivity analyses[5] that account for all possible outcomes might become a future standard for reporting randomised controlled trials that cannot guarantee an intention-to-treat analysis based on all participants or which rely on imputed values for non-ignorable missing binary end points.

1 Carter GL, Clover K, Whyte IM, Dawson AH, D'Este C. Postcards from the EDge project: randomised controlled trial of an intervention using postcards to reduce repetition of hospital treated deliberate self poisoning. *BMJ* 2005; **331**: 805–7.

2 Carter GL, Clover K, Whyte IM, Dawson AH, D'Este C. Postcards from the EDge: 24-month outcomes of a randomised controlled trial for hospital-treated self-poisoning. *Br J Psychiatry* 2007; **191**: 548–53.

3 Vaiva G, Ducrocq F, Meyer P, Mathieu D, Philippe A, Libersa C, et al. Effect of telephone contact on further suicide attempts in patients discharged from an emergency department: randomised controlled study. *BMJ* 2006; **332**: 1241–5.

4 Jayasekera H, Carter G, Clover K. Comparison of the Composite International Diagnostic Interview (CIDI-Auto) with clinical diagnosis in a suicidal population. *Arch Suicide Res* 2011; **15**: 43–55.

5 Hollis S. A graphical sensitivity analysis for clinical trials with non-ignorable missing binary outcome. *Stat Med* 2002; **21**: 3823–34.

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Corrections

Valproate *v*. Lithium in the treatment of bipolar disorder in clinical practice: observational nationwide register-based cohort study. *BJP*, **199**, 57–63. Table 1 (p. 59), final column, row 7: the hazard ratio (95% CI) for Index episode: mixed, with mania/mixed episode as the outcome is 1.59 (1.16–2.18). This typographical error does not affect the findings of the paper.

Psychiatric history and subthreshold symptoms as predictors of the occurrence of depressive or anxiety disorder within 2 years. *BJP*, **194**, 206–212. Table 3, p. 209: The values for Social phobia, *n* (%) should read: No subthreshold anxiety disorder at baseline 31 (3.3), History of social phobia 14 (15.4), History of panic disorder 2 (3.1), History of agoraphobia 5 (9.3), History of GAD 8 (9.2), No history of anxiety 12 (1.7), Subthreshold anxiety at baseline 25 (11.0), History of social phobia 6 (18.2), History of panic disorder 4 (12.1), History of agoraphobia 6 (20.0), History of GAD 4 (9.3), No history of anxiety 12 (9.4), Total 56 (4.8). The values for Generalized anxiety disorder, n (%) should read: No subthreshold anxiety disorder at baseline 22 (2.3), History of social phobia 5 (5.5), History of panic disorder 1 (1.5), History of agophobia 2 (3.7), History of GAD 6 (6.9), No history of anxiety 11 (1.5), Subthreshold anxiety at baseline 16 (7.0), History of social phobia 1 (3.0), History of panic disorder 4 (12.1), History of agoraphobia 3 (10.0), History of GAD 2 (4.7), No history of anxiety 8 (6.3), Total 38 (3.3). The erroneous values in the table do not affect other values, including the ones listed in the column Any disorder, *n* (%), or any of the statistical analyses or conclusions presented in the paper.

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