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.

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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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