

## Review article

## Letters, green cards, telephone calls and postcards: systematic and meta-analytic review of brief contact interventions for reducing self-harm, suicide attempts and suicide

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**Background**

There is growing interest in brief contact interventions for self-harm and suicide attempt.

**Aims**

To synthesise the evidence regarding the effectiveness of brief contact interventions for reducing self-harm, suicide attempt and suicide.

**Method**

A systematic review and random-effects meta-analyses were conducted of randomised controlled trials using brief contact interventions (telephone contacts; emergency or crisis cards; and postcard or letter contacts). Several sensitivity analyses were conducted to examine study quality and subgroup effects.

**Results**

We found 14 eligible studies overall, of which 12 were amenable to meta-analyses. For any subsequent episode of self-harm or suicide attempt, there was a non-significant

reduction in the overall pooled odds ratio (OR) of 0.87 (95% CI 0.74–1.04,  $P=0.119$ ) for intervention compared with control. The number of repetitions per person was significantly reduced in intervention v. control (incidence rate ratio IRR=0.66, 95% CI 0.54–0.80,  $P<0.001$ ). There was no significant reduction in the odds of suicide in intervention compared with control (OR=0.58, 95% CI 0.24–1.38).

**Conclusions**

A non-significant positive effect on repeated self-harm, suicide attempt and suicide and a significant effect on the number of episodes of repeated self-harm or suicide attempts per person (based on only three studies) means that brief contact interventions cannot yet be recommended for widespread clinical implementation. We recommend further assessment of possible benefits in well-designed trials in clinical populations.

**Declaration of interest**

None.

Attempted suicide and self-harm comprise significant public health burdens in many countries. The cross-national prevalence of attempted suicide across low-, medium- and high-income countries is estimated to be about 3% of the general population per year.<sup>1</sup> The prevalence of self-harm (with or without suicidal intent) is higher than this.<sup>2,3</sup> About one-third of those who engage in self-harming behaviours seek treatment for their injuries from hospital emergency departments.<sup>4</sup> The treatment and aftercare of these patients often constitutes a considerable cost burden on healthcare facilities. Efficacious treatment for self-harm and attempted suicide is an important issue given the high economic, emotional and social costs of the behaviour.<sup>5</sup> However, those who engage in self-harming behaviours are acknowledged as being a difficult to treat population,<sup>6</sup> mainly because of their low adherence to treatment over time<sup>7</sup> and their high likelihood of repeating self-harming behaviours.<sup>8</sup> Many treatments that do exist are relatively resource intensive, requiring specialist training of clinicians, and are therefore not feasible in many contexts.

In recognition of these challenges, there has been growing interest in brief interventions for this population that are focused on maintaining long-term contact and/or offering re-engagement with services when needed.<sup>6,8</sup> Brief contact interventions are distinct from other forms of outreach care and case management in that they are not required to be conducted by a mental health specialist (e.g. social worker, psychiatrist, psychologist), occur according to a structured schedule and are delivered or operational over a sustained period of time. Brief contact interventions employ direct ongoing contact, and/or offer the possibility of re-contact with clinical services if required (either implicitly or explicitly). They either do not include any formal therapy or only provide a

minimal component of supportive intent or psychoeducation. Some brief contact interventions have taken the form of supportive short letters, phone calls or postcards.<sup>8–10</sup> Another form of brief intervention is the provision of an emergency or crisis card (sometimes referred to as 'green cards') encouraging help-seeking and offering on-demand crisis admission or other help to those persons presenting to hospitals or healthcare facilities.<sup>8</sup>

Although there have been a number of reviews on treatments for self-harm or attempted suicide,<sup>6,11–18</sup> all but one<sup>17</sup> have covered the full range of interventions and have not specifically focused on brief contact treatments. There has been no meta-analyses evaluating the effectiveness of brief contact interventions for reducing self-harm, suicide attempt or suicide. The aim of this review was to synthesise the evidence regarding the effectiveness of brief contact interventions for reducing self-harm, suicide attempt and suicide. The review sought to answer three main questions: (a) do those in the intervention group (i.e. who receive brief contact interventions) have lower odds of self-harm or suicide attempt than those in the control group; (b) do those in the intervention group have a fewer number of repetitions of self-harm or attempted suicide in the period following the intervention than those in the control group; and (c) does the intervention group have fewer deaths by suicide than the control group at follow-up?

**Method**

The review protocol was based on the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA, <http://www.prisma-statement.org/>).<sup>19</sup>

## Search strategy and keywords

To search for all relevant studies, we first assessed past systematic reviews on interventions for suicidal behaviours.<sup>6,11–18</sup> If relevant, all cited articles within these reviews were considered eligible. Following this, we conducted a search using the Cochrane central register of controlled trials and library, as well as Medline and Embase. The search terms we used are shown in the online supplement. An example of a search strategy for Cochrane was: 'self-harm OR suicide AND intervention AND post-discharge AND postcard OR brief contact AND follow up AND care'. No language or additional limits were applied. A secondary search of reference lists was undertaken from within the retrieved articles. Authors were contacted to provide additional statistical details on retrieved studies, and to provide any updates or new data on published work. The initial searches and short-listing were undertaken by A.J.M. Subsequent searches and checking were undertaken by the other three authors and any disagreements about whether to include a study was resolved by consensus.

## Inclusion and exclusion criteria

We considered articles if the search terms were included in the abstract or title of the article and they were published in a peer-reviewed journal. After a review of the title and abstract, editorials and papers in languages other than English were excluded. Following this, we reviewed the abstract and text to assess whether the study utilised a brief contact intervention and also whether suicide attempt, self-harm or suicide was a measured outcome variable. We excluded articles if the contact intervention was not brief in nature or did not assess the effect of telephone calls, green cards, crisis card, letters or postcards as the intervention. We also excluded any articles in which participants were not sourced from a hospital or a healthcare setting.

## Type of studies and outcome

The studies eligible for this review were those that considered interventions that used: (a) telephone contacts following presentation to an emergency department or healthcare facility; (b) emergency crisis cards or green cards; and (c) postcard or letter interventions.<sup>8</sup> We included studies that evaluated these approaches in isolation or in combination with treatment as usual (TAU).

We considered randomised controlled trials (RCTs), cluster randomised controlled trials (cRCTs), quasi-experimental and non-randomised trials as eligible for inclusion in this study. The primary outcomes considered in this review were: (a) the occurrence of any subsequent episode of self-harm or suicide attempt; (b) the number of repeated episodes of self-harm or attempted suicide per person; and (c) the total number of suicide deaths.

## Data extraction, risk of bias and quality assessment

Two authors extracted the following information from each study for both treatment arms: (a) the number of individuals with and without subsequent presentations for self-harm or attempted suicide and the time period this information related to; (b) the total number of repeated episodes of self-harm or attempted suicide and the total person-time (or the incidence rate ratio (IRR) and its standard error that summarises this information); and (c) the number of deaths by suicide. We also extracted data on the source of information for the outcome, the type of brief intervention and intensity (e.g. how many follow-up contacts were made with participants), sample definition, authors' details and country in which the study was conducted. We assessed the risk

of bias associated with methods of randomisation, allocation concealment, masking, selective reporting, loss to follow-up, completeness of reporting outcome data, and adherence with the intention-to-treat protocol. Risk of bias was rated as low, medium or high according to the GRADE criteria.<sup>20</sup> Funnel plots were used to assess the possibility of small study bias. For each outcome we generated 'risk of bias'.

## Statistical analysis

We used random-effects meta-analysis<sup>21</sup> to assess the impact of brief contact interventions on self-harm, attempted suicide and suicide. Study-specific weights used for estimating the pooled effect size were calculated using the Mantel–Haenszel method. We undertook sensitivity analyses to assess the effect of removing studies at high risk of bias from the analysis. Studies were considered at high risk of bias if they were graded as 'high risk' in randomisation and allocation sequence, or in masking or information on loss to follow-up. We also conducted additional analyses to assess the impact of an objective (e.g. information obtained from hospital records) *v.* a subjective outcome (e.g. self-reported self-harm or suicide attempt) and assessed whether study context (i.e. high-income countries in the Organisation for Economic Co-operation and Development (OECD) and Taiwan) influenced the number of repeat episodes of self-harm or suicide attempt.

We considered the pooled evidence as inconclusive of benefit or harm if the confidence intervals of the accompanying point estimates crossed over the threshold of one. Evidence about interventions was considered conclusive if confidence intervals were above or below this threshold.

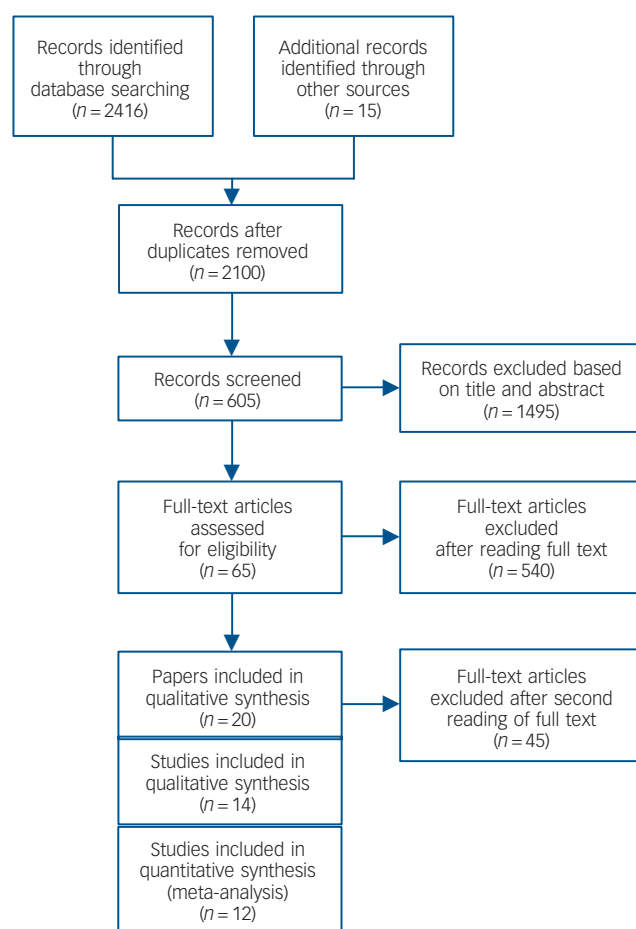
Heterogeneity was assessed through visual inspection of the forest plots and with the  $I^2$  statistic, which provides an estimate of inconsistency across studies. The benchmark of an  $I^2$  statistic of 75% or greater was considered a substantial amount of heterogeneity.<sup>22</sup> A negligible value of  $I^2$  indicates little variability in the effect size between studies. Where high levels of heterogeneity were observed, we explored this through subgroup analysis and by examining the quality of individual studies. We assessed publication bias with funnel plots. Funnel plot asymmetry was assessed using Egger and colleagues' 1997 test for small study effects in meta-analysis.<sup>23</sup> All analyses were conducted using Stata version 12 for Mac.<sup>23</sup>

## Results

We identified a total of 2416 articles from a systematic search of the databases and 15 articles from other sources (Fig. 1). After exclusions based on title and abstract, 65 articles were read in their entirety. From this, a further 45 articles were then excluded because they were based on long-term face-to-face treatment, were trial protocol papers or the study was not an RCT, cRCT or other eligible design. After exclusions 20 articles remained. This was equivalent to 14 unique studies as a number of authors had published follow-up papers<sup>24–28</sup> and there were several substudies published from larger trials (online Table DS1).<sup>29,30</sup>

## Description of studies included in the systematic review

Most studies used an RCT design (Table 1). One study used a cRCT design.<sup>31</sup> The majority of studies enrolled participants into the trial following presentation to a hospital emergency department for hospital-treated self-harm. One study recruited participants attending a mental health out-patient facility.<sup>32</sup>



**Fig. 1** Flow of information through the phases of systematic review.

Studies had been conducted in a variety of locations, including the USA,<sup>33</sup> the UK,<sup>27,28,31,34–36</sup> New Zealand,<sup>37</sup> Australia,<sup>24–26,32</sup> Taiwan,<sup>38</sup> France,<sup>39</sup> Sweden<sup>40</sup> and Iran.<sup>41</sup> The review also contained a large multicentre study carried out in a variety of locations, including India,<sup>30</sup> Iran<sup>29</sup> and several other countries.<sup>9,10</sup>

The outcomes assessed included self-poisoning,<sup>24–26</sup> self-harming behaviours<sup>27,28,31,35,37,41</sup> and attempted suicide.<sup>9,29,30,39–41</sup> There were several studies that also assessed mortality from suicide.<sup>10,26,33,39,40</sup> Six studies obtained information on outcomes from subjective reports<sup>9,10,29,30,32,41</sup> whereas the remainder used objective sources such as hospital reports or mortality records. The review included a number of telephone interventions,<sup>9,10,29,30,39,40</sup> postcard interventions,<sup>24–26,32,37,41</sup> green card or crisis card interventions,<sup>27,28,35,36,38</sup> and two letter interventions.<sup>33,34</sup>

Follow-up ranged from 6 months to 15 years and there were considerable differences in number of contacts made with those in the intervention between studies. Eight studies had five contacts or fewer,<sup>27,28,31,35,36,38–40</sup> ten studies had between 6 and 10 contacts,<sup>9,10,24–26,29,30,34,37,41</sup> and two studies had over 11 contacts.<sup>32,33</sup>

### Study quality

Three studies were rated as having high risk of bias because they had unclear or imprecise details about randomisation and allocation procedures, masking, and/or incomplete reporting.<sup>33,35,36</sup> The remaining papers were rated as having a low risk of bias as information on randomisation, allocation concealment and

masking was available and data on outcomes were ascertained from hospital or mortality records.

### Reasons for exclusion of studies from the meta-analysis

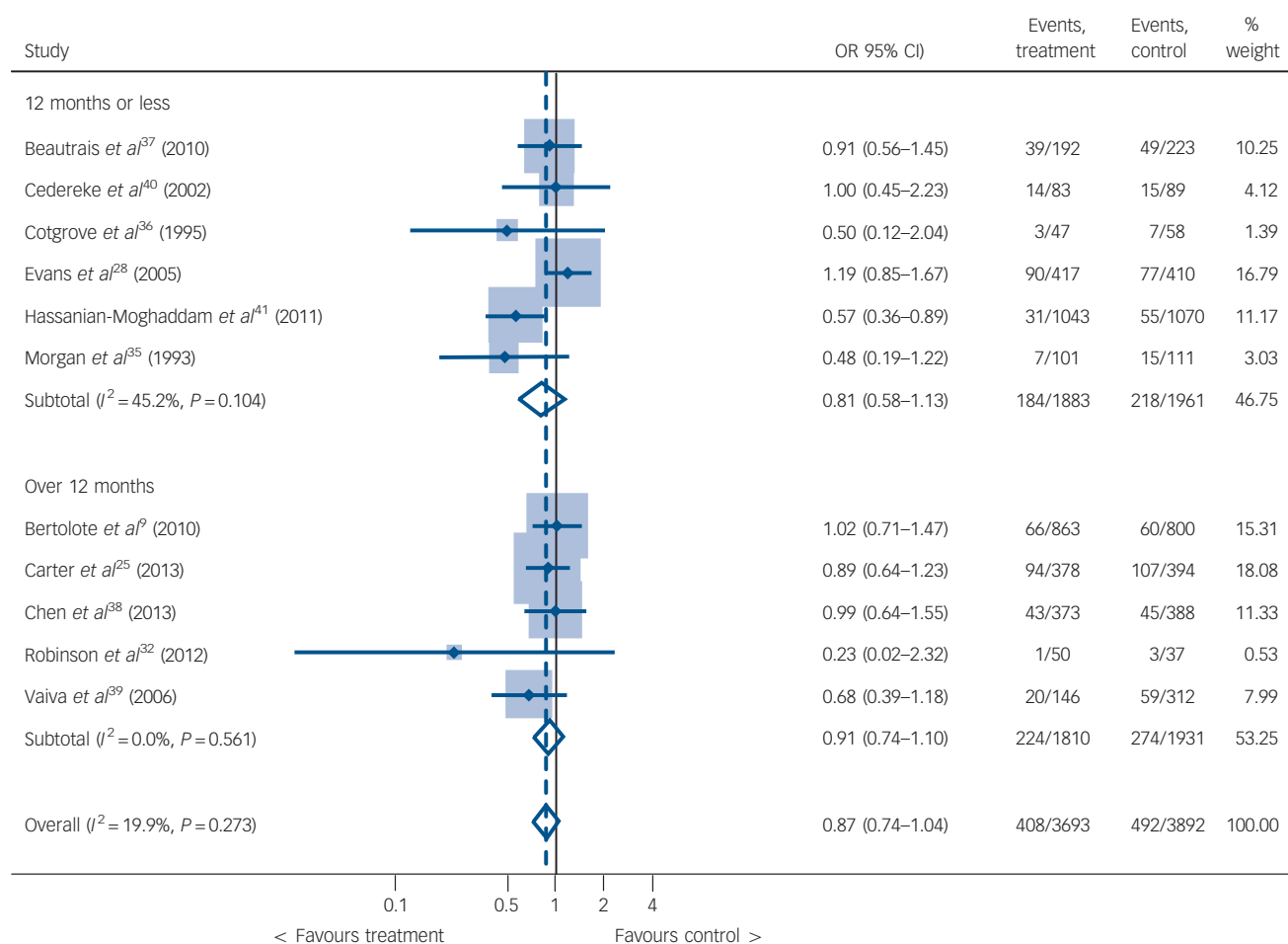
We excluded one study<sup>31</sup> because this was a cRCT testing a contact-based instructional intervention directed at primary care physicians rather than patients. We also excluded another study<sup>34</sup> as this was designed to test the utility and feasibility of the intervention methodology rather than being an efficacy trial of treatment. However, we assessed whether the inclusion of this feasibility trial had an impact on the results for our first aim (subsequent episode of self-harm or suicide attempt) in sensitivity analysis. Where multiple studies were reported using the same patients but different follow-up times,<sup>24–26</sup> we only report data from the most recent study, and substudies published from larger trials were also excluded to avoid repetition of data.<sup>29,30</sup> Similarly, when studies reported both suicide attempt and other self-harming behaviours using specific methods, we chose to assess suicide attempt as this was seen as a more general outcome (i.e. where multiple methods of harm could have been used).<sup>41</sup> It was not possible to assess differences by gender as only a small number of trials provided this information. Sensitivity analyses were conducted for our first aim (subsequent episode of self-harm or suicide attempt) as sample size restrictions hindered the ability to conduct sensitivity tests for our second and third aims (number of repeated episodes of self-harm or attempted suicide per person and the total number of deaths by suicide, respectively).

### Self-harm or suicide attempt in the intervention compared with the control group

We looked at self-harm or suicide attempt in the intervention compared with the control group by the longest reported time period for studies. The individual study estimates and the pooled odds ratio of any repeated episode of self-harm or attempted suicide for all participants are shown in Fig. 2. In total, there were 11 studies eligible for inclusion into the analysis, giving a total of 8485 participants (4101 in the treatment group and 4384 in the control group). The overall pooled odds ratio was 0.87 (95% CI 0.74–1.04,  $P=0.119$ ). Inspection of the  $I^2$  statistic indicated a moderate and non-significant amount of heterogeneity between studies ( $I^2=19.9\%$ ,  $P=0.273$ ). Figure 2 also shows results by follow-up time period ( $\leq 12$  months and  $> 12$  months). For those studies that had follow-up at 12 months or less, the overall pooled OR for self-harm or suicide attempt in the intervention compared with the control group was 0.81 (95% CI 0.58–1.13,  $P=0.215$ ). The overall  $I^2$  was 45.2%,  $P=0.104$ . For over 12 months' follow-up, the overall pooled odds ratio for self-harm or suicide attempt was 0.91 (95% CI 0.74–1.10,  $P=0.321$ ). Inspection of the  $I^2$  statistic indicated a negligible amount of heterogeneity between studies ( $I^2=0\%$ ,  $P=0.561$ ).

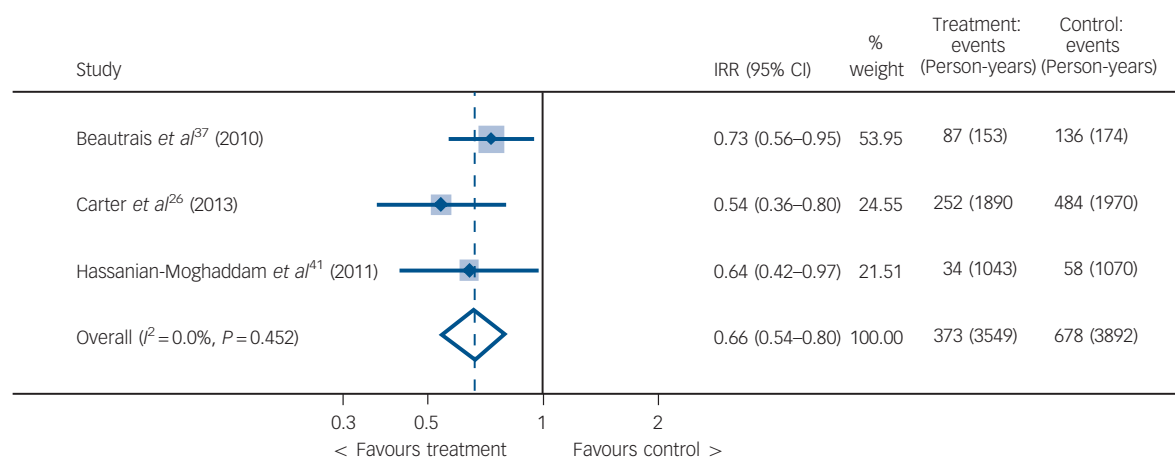
### Number of repetitions of self-harm or suicide attempt in the intervention compared with the control group

We next looked at the number of repetitions of self-harm or suicide attempt over the time of the intervention compared with the control group by the longest reported time period for studies. Figure 3 shows the individual study effect sizes and pooled incidence rate ratio evaluating the impact of brief interventions on the total number of episodes of self-harm and suicide attempt in the treatment and control groups. Across the three eligible studies, there were 373 repeats (out of 3086 person-years) in the



**Fig. 2** Pooled random-effects meta-analysis assessing the effectiveness of brief contact interventions on the odds of any repeated episode of self-harm or suicide attempt, longest reported follow-up time from 11 studies.

Weights are from random-effects analysis.



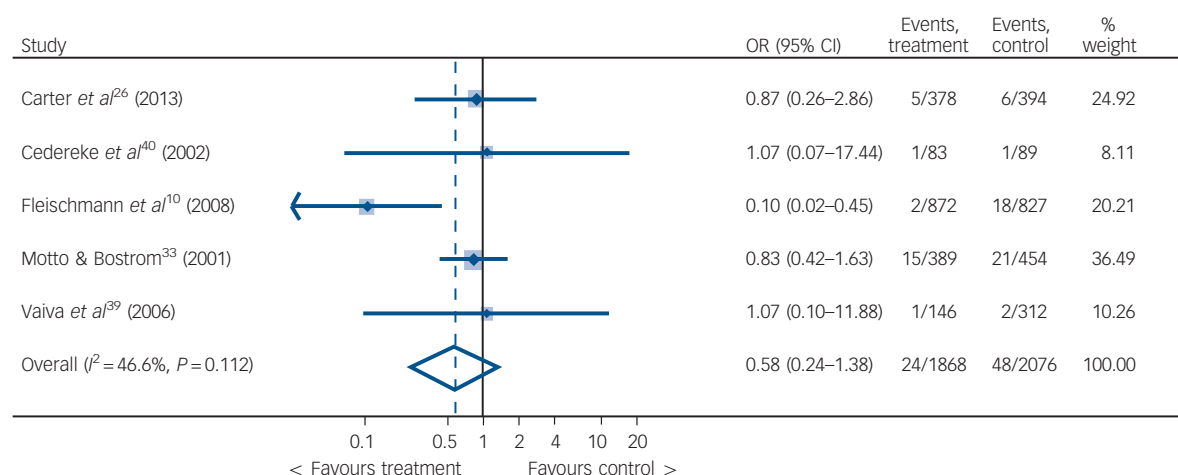
**Fig. 3** Pooled random-effects meta-analysis assessing the effectiveness of brief contact interventions on the rate of repetitions in the treatment and control groups, longest reported follow-up time from three studies.

Weights are from random-effects analysis. IRR, incidence rate ratio.

intervention condition and 678 repeats (of 3214 person-years) in the control condition, leading to a 34% reduction in the number of presentations per person-year (IRR = 0.66, 95% CI 0.54–0.80,  $P < 0.001$ ). The overall  $I^2$  indicated an absence of study heterogeneity ( $I^2 = 0\%$ ,  $P = 0.452$ ).

#### Deaths by suicide in the intervention v. the control group at follow-up

Figure 4 shows the study-specific odds ratios and pooled odds ratio for the five studies that examined suicide as an outcome. In all, data were available for 4106 individuals, of whom 72 died



**Fig. 4** Pooled random-effects meta-analysis assessing the effectiveness of brief contact interventions on the odds of suicide, longest reported follow-up time from five studies.

Weights are from random-effects analysis.

by suicide. The results suggest that the odds of suicide are lower but not significantly so for the treatment group compared with the control group (OR=0.58, 95% CI 0.24 to 1.38,  $P=0.216$ ). The heterogeneity in this analysis was moderate but non-significant ( $I^2=46.6\%$ ,  $P=0.112$ ).

### Sensitivity analysis

The analysis for our first aim (subsequent episode of self-harm or suicide attempt) was stratified by whether an objectively or subjectively measured outcome was used. The results suggest that the intervention did not reduce the odds of self-harm or attempted suicide for a subjectively reported outcome (e.g. information reported by the participant) (OR=0.80, 95% CI 0.62–1.08,  $P=0.089$ ) or for an objectively reported outcome such as hospital records (OR=0.94, 95% CI 0.82–1.08,  $P=0.362$ ). There was greater heterogeneity in those studies reporting subjective outcomes ( $I^2=61.9\%$ ,  $P=0.072$ ) than those using objective sources of data for the outcome ( $I^2=0\%$ ,  $P=0.523$ ).

For the studies at high risk of bias, the pooled odds ratio was 0.52 (95% CI 0.25–1.06,  $P=0.071$ ); the pooled odds ratio in studies at low risk of bias was 0.92 (95% CI 0.82–1.04,  $P=0.183$ ). Heterogeneity was moderate in the low-bias studies ( $I^2=19.5\%$ ,  $P=0.270$ ) and negligible in the high-bias studies ( $I^2=0\%$ ,  $P=0.969$ ). We then assessed whether including the study by Kapur and colleagues<sup>34</sup> affected our results for our first aim (subsequent episode of self-harm or suicide attempt). The overall pooled odds ratio when this was included was 0.88 (95% CI 0.73–1.08,  $P=0.228$ ). This result suggests that the inclusion of this trial did not markedly change the results of the analysis of our first aim, described above. Finally, subgroup analysis restricted to high-income countries only (USA, UK, Sweden, France, Australia, New Zealand and Taiwan) showed no evidence of effectiveness of the brief contact intervention in these settings (OR=0.95; 95% CI 0.80–1.12,  $P=0.553$ ). The  $I^2$  was 0%,  $P=0.515$ .

### Publication bias

Possible publication bias and small study effects were assessed through inspection of a funnel plots (see online Fig. DS1) and through Egger and colleagues' 1997 test for small study effects in meta-analysis.<sup>23</sup> The estimated bias coefficient was  $-0.66$  with a standard error of 0.37, giving a  $P$ -value of 0.084. This test

provides no evidence of small study effects. The funnel plots indicated asymmetric plots for all outcomes, with smaller studies showing larger effect sizes. Most studies show a null effect or a reduction in self-harming behaviours in the intervention compared with the control group. This may suggest possible publication bias, whereby studies with null or negative effects (i.e. where the intervention was associated with no difference or an increase in self-harming behaviours) have been missed.

## Discussion

### Main findings

Although the meta-analysis suggests lower odds of any episode of self-harm or attempted suicide among those receiving the intervention compared with control, this result was not significant. This finding needs to be considered in the light of methodological factors. First, it is likely that the individual studies included in this review were underpowered. Second, there were likely considerable differences in results depending on the context and time period during which the study was conducted. For example, two studies reporting the effectiveness of the intervention condition in reducing suicide attempts were conducted some decades ago and were rated as having a high risk of bias,<sup>35,36</sup> whereas recent studies find more conservative results.

Postcard interventions significantly favoured a reduction in event rates (IRRs) among the intervention group.<sup>26,37,41</sup> This suggests that brief contact interventions were more successful in reducing the frequency at which individuals re-attempted or self-harmed, rather than the overall proportion of people that engaged in self-harming behaviours or suicide attempts. However, we acknowledge that one of the studies included in this analysis was set in Iran,<sup>41</sup> a non-Western clinical population and assessed a variation on the original postcard intervention (i.e. a card containing messages of support and inspiration), whereas the other two studies were set in similar Western contexts and clinical populations (Australia<sup>26</sup> and New Zealand<sup>37</sup>). The study set in New Zealand was originally halted because of the perceived benefit of the postcard intervention resulting in activation of the *a priori* stopping rules. This early stopping of the study then resulted in an underpowered study. There was also some evidence of imbalance at randomisation between the control and the intervention group. After statistically adjusting for the imbalance, the study reported



no differences for any repetition or number of repetitions. Considering these differences, and the fact that more evidence is needed as our results were based on only three studies, more evidence from new RCTs on the possibility of reducing the frequency of self-harm is clearly needed. Notwithstanding, a possible interpretation of the meta-analysis results is that the brief contacts provided a form of social support for those at risk, who were then less likely to engage in repeated self-harming behaviours over time.<sup>24,33</sup> We were able to examine events per individual for postcard studies only, but speculate a similar result may also be apparent in other types of brief interventions. To investigate this, future studies should endeavour to include an outcome assessing the number of events per person, as well as the overall proportions of self-harm and suicide attempt in the intervention compared with the control condition.

Despite the meta-analysis suggesting an overall reduction in mortality as a result of suicide among those in the intervention compared with those in the control condition, the odds ratio was not significant. This may be because suicide is a rare event and none of the studies reported here was adequately powered to assess mortality as an outcome (hence, the large confidence intervals around the effect estimate). Even in the one study showing positive effects of a telephone intervention, there were just 20 deaths across 1699 people included in the trial. Questions must also be raised about whether brief intervention trials in clinical populations are appropriately designed for reducing mortality, given that those who are most at risk of dying by suicide show different clinical and epidemiological characteristics from those who engage in self-harm or attempted suicide.<sup>42–45</sup>

Sensitivity analysis suggests no evidence of effectiveness of the brief contact interventions in high-income countries. This may reflect less of a difference between ‘treatment as usual’ and the brief contact intervention in countries with established resources and treatment protocols for self-harm, and hence, less observable effects of the intervention.<sup>40</sup> Results also suggest more favourable effects for those studies rated as having a higher level of bias, which indicates the need for a greater number of rigorously conducted trials in the future.

## Limitations

A limitation of the meta-analysis is that it included only a relatively small number of studies. It is also possible that studies were missed or excluded, and there were several large trials on brief interventions still being undertaken at the time the review was being conducted. The review focused on a range of brief interventions (letter, postcard, telephone, green card or crisis card studies), all of which might have different efficacy. We examined both self-harm and suicide attempt in combination, but recognise that there may be notable differences between these behaviours in relation to intent.<sup>3</sup> A lack of generalisability is also a problem as only RCTs were included in this study.<sup>46</sup>

## Future research

Future studies are needed that adequately address the limitations of past trials and answer several questions arising from this review. For one, we were unable to assess participant subgroup differences (e.g. gender, age, history of self-harm) or the time between the index attempt and first contact because of sample size limitations. This is potentially important considering that some research suggests that brief interventions may be less efficacious for those with ongoing and long-term psychiatric problems<sup>29</sup> and there is an elevated risk of repetition in the first weeks following an index attempt.<sup>6,31,40,47</sup> In stating this, it is also important to consider

differences based on the context of the intervention and clinical population being studied, as a postcard study in Iran<sup>41</sup> found beneficial effects of the intervention for those with a history of self-harm. This necessitates the importance of further attention to the context, baseline characteristics of those in the control and intervention condition,<sup>37</sup> and potential subgroup effects. Treatment conditions provided to those in the control group is another important influence that needs to be further considered, as greater attention to follow-up care in control conditions has been cited as an explanation for null findings in intervention studies.<sup>40</sup> There also needs to be a closer examination of the acceptability of the intervention for participants, the possibility of a range of psychiatric and non-psychiatric adverse outcomes and also economic evaluation of brief contact studies in reducing the burden of care for hospital and healthcare facilities. Extending evaluation into cost–benefit studies would be useful as, at the very least, our evidence suggest that brief contact interventions were not related to an increase in self-harm, suicide attempt or suicide. Finally, considering our results, more studies need to focus on the likelihood that brief contacts reduce the frequency of repetitions for individuals. This is particularly important given that this is where evidence about the effectiveness of brief interventions is the strongest.

## Clinical implications

At this stage, we would not recommend widespread clinical implementation of brief contact interventions; however, given the possible benefits, low cost and unlikely adverse effects, large-scale trials in clinical populations would be worthwhile. We would suggest that randomisation be stratified by gender and past history of self-harm or suicide attempt in order to examine important potential subgroup differences.

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