

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

This paper was submitted to a another journal from BMJ but declined for publication following peer review. The authors addressed the reviewers' comments and submitted the revised paper to BMJ Open. The paper was subsequently accepted for publication at BMJ Open.

(This paper received three reviews from its previous journal but only two reviewers agreed to published their review.)

ARTICLE DETAILS

TITLE (PROVISIONAL)	Meta-analysis of suicide rates in the first week and first month after psychiatric hospitalisation
AUTHORS	Chung, Daniel; Hadzi-Pavlovic, Dusan; Wang, Maggie; Swaraj, Sascha; Olfson, Mark; Large, Matthew

VERSION 1 – REVIEW

REVIEWER	Ron Kessler Harvard Medical School
REVIEW RETURNED	03-Jun-2018

GENERAL COMMENTS	A useful contribution
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REVIEWER	Raffaella Calati University of Montpellier INSERM U 1061: Neuropsychiatry: Epidemiological and Clinical Research Department of Emergency Psychiatry and Post Acute Care, Lapeyronie Hospital, Centre Hospitalier Universitaire (CHU) Montpellier, France
REVIEW RETURNED	13-Jun-2018

GENERAL COMMENTS	<p>The present meta-analysis investigated the magnitude of suicide rates in the first week and first month post-discharge from psychiatric hospitalisation. The aim is extremely valuable. However, a consistent critical issue lays in the fact that methods are not sufficiently described to replicate the study.</p> <p>To improve the manuscript, I suggest the following modifications.</p> <p>Abstract Authors wrote: "Systematic meta-analysis". I would only write "Meta-analysis". I would mention sensitivity analyses.</p> <p>Methods Search Strategy and Selection Criteria Inclusion and exclusion criteria should be better specified: what about the study design? Patients' age? Among exclusion criteria you mentioned "emergency departments". I think this is misleading. MESH should be reported.</p>
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	<p>You wrote: "Whenever possible the authors of studies reporting [...] were contacted by email". Some specifications should be added: how many authors? How many emails to each? How many replied? How many sent you the data?</p> <p>Data extraction The list of moderators should be complete. Funnel plot is not sufficient to estimate publication bias. Please add a test. You wrote: "Pre-specified subgroup analyses according to the period of follow up, source of the data (published or obtained by personal communication), country of publication, sex, and strength of reporting". I think a verb is missing. Moreover, I think that further analyses, such as diagnosis (mood disorders, psychosis), should be added.</p> <p>Results This paragraph is difficult to read. It should be reduced, also with the help of a further table. The strength of reporting is not well described. A single paragraph should be dedicated to it.</p> <p>Figures are of very low quality.</p> <p>eSupplement 1. Strength of reporting items of included studies: in my opinion De Leo 2007 has a wrong Strength of Reporting Score. Acronyms of tables should be explained (PY, LL, UL).</p> <p>Discussion Authors should better describe and compare their results with the ones of Chung's meta-analysis (e.g., how many studies have been included in Chung's analysis on 0-3 months? Were they able to include the same studies as you in relation to 0-1 month?).</p>
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REVIEWER	Maria Vazquez-Montes University of Oxford United Kingdom
REVIEW RETURNED	13-Aug-2018

GENERAL COMMENTS	<p>This is a very interesting and relevant piece of work that complements previously published work by the authors (JAMA Psychiatry. 2017 Jul 1;74(7):694-702. doi: 10.1001/jamapsychiatry.2017.1044) by now estimating the one- and four-week suicide rates post-psychiatric care discharge in adult patients. The manuscript is well structured and the overall design is sound. Unfortunately there are details missing in the text that would require further work before this could be ready for publication. Please see my comments and suggestions below.</p> <ol style="list-style-type: none"> 1. Abstract: More details needed. <ol style="list-style-type: none"> a. It is not clear from the objective whether suicide rates will be estimated for a particular population (e.g. adults with suicidal behaviour or a particular diagnosis) or setting (i.e. is it only for patients in psychiatric hospitals?) b. Design: <ol style="list-style-type: none"> i. Search strategy not mentioned, add a brief statement. ii. Search period not provided, mention it. iii. Pg1, Line 53 is missing the noun that is "relevant English-language, peer-reviewed". iv. Pg1, Line 56 refers to several analyses, but it is not clear why more than one analysis has been done for a single rate estimation. Clarify this.
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v. Pg2, ln 4 – please rephrase or explain this idea of generalised mixed models (glm) including “random effect models”. Without further reading of the manuscript, one could think that this might mean that study is an explanatory variable, not mentioned in the previous sentence, and its coefficient will be random? Or that the glm will be a two-level model grouping by study – but unlikely unless the study is using individual data? Other? In any case, the sentence is not common.

vi. Pg 2, ln 33, should it be “from” instead of “form”?

c. Results:

i. Please review. Counts reported do not coincide with Table 1. Table has data for only 28 and 23 studies reporting four- and one-week data, respectively.

2. Introduction:

a. Aim could be more specific as suggested in 1a.

3. Inclusion/exclusion criteria:

a. From the introduction and methods sections, we can conclude that the inclusion criteria is
P-adult patients discharged from psychiatric care (any?)
I-psychiatric hospitalisation, which provides care and protection against suicide
C-descriptive study, no comparator
O-suicide rates per 100,000 person years at 1 and 4 weeks post-discharge
However, there are several studies that at first glance do not seem to satisfy the population inclusion criteria, e.g. Luxton et al (2013) (US Military treatment facilities); Qurashi et al (2006) (unspecified hospital in Manchester); Seemuller et al (2014) (twelve centres across Germany).

Suggestions:

a) Be more specific about the inclusion criteria in the text
b) Justify the inclusion of seemingly non-psychiatric care discharges

4. Search strategy:

a. Again, the search strategy and period not mentioned
b. Also, please clarify if the search was carried out anew or within the identified articles for the authors’ previous research.

5. Data extraction:

a. It would be useful to know if a particular extraction form was used and/or have a list of the variables/items extracted in a table. This could also be used to present a summary of the available information.

6. Data analysis:

a. Please see comment 1b. I would suggest highlighting what the main analysis is and explaining what all the other analyses are for.
b. Please complete the last sentence describing what happened to the pre-specified subgroup analyses.

7. Results:

a. Please see comment 1c. This applies to all analyses. It is not clear how the number of studies included in each analysis were calculated. More detail is needed. For instance, 17 studies report data on the first week after discharge but only 15 are mentioned in the corresponding results paragraph (Pg 9, paragraph 1).
b. Related to the above, I would suggest to add at the top of Table 1 the number of studies with information on the last two columns; and indicate somehow which studies have information on both columns.
c. Does Table 1 show the data retrieved by personal communications? It would be helpful to highlight them.
d. Pg 7, ln 27: Disagreements about “effect size” are mentioned.

	<p>However, the included studies are not necessarily intervention ones. There is not a comparator in the inclusion criteria. Thus it is not immediately clear which effect size this refers to. Please explain.</p> <p>e. It is not clear how missing data is managed, i.e. studies with no information on suicides are included in the analysis, are these data treated as missing or particular value given? Please specify.</p> <p>f. Subgroup analysis based on reporting strength is done but was not mentioned what the cut-off is to define low and high strength and how this was chosen.</p> <p>8. Conclusion:</p> <p>a. Figures might need to change after reviewing all the above.</p> <p>9. Supplements:</p> <p>i. eSupplement 1: There is a typo on the strength of reporting score for Castelein 2015</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Ron Kessler

Institution and Country: Harvard Medical School

Please state any competing interests or state 'None declared': None

Please leave your comments for the authors below

A useful contribution

Reviewer: 2

Reviewer Name: Raffaella Calati

Institution and Country: University of Montpellier, INSERM U 1061: Neuropsychiatry: Epidemiological and Clinical Research, Department of Emergency Psychiatry and Post Acute Care, Lapeyronie Hospital, Centre Hospitalier Universitaire (CHU), Montpellier, France

Please state any competing interests or state 'None declared': None declared.

Please leave your comments for the authors below

The present meta-analysis investigated the magnitude of suicide rates in the first week and first month post-discharge from psychiatric hospitalisation. The aim is extremely valuable.

However, a consistent critical issue lays in the fact that methods are not sufficiently described to replicate the study.

To improve the manuscript, I suggest the following modifications.

Abstract

Authors wrote: "Systematic meta-analysis". I would only write "Meta-analysis". [Done]

I would mention sensitivity analyses.[Done]

Methods

Search Strategy and Selection Criteria

Inclusion and exclusion criteria should be better specified: what about the study design? Patients' age? [we have made this clearer in the inclusion criteria]

Among exclusion criteria you mentioned "emergency departments". I think this is misleading.[We excluded discharge from non psychiatric settings, we think this is clearer now]

You wrote: "Whenever possible the authors of studies reporting [...] were contacted by email". Some specifications should be added: how many authors? How many emails to each? How many replied? How many sent you the data? [these numbers have been clarified and are also in a new table 2]

Data extraction

The list of moderators should be complete. [the moderators are now listed]

Funnel plot is not sufficient to estimate publication bias. Please add a test. [We have included an Eggers test, this and the submitted version had unpublished data that has is a more direct test of

publication bias]

You wrote: "Pre-specified subgroup analyses according to the period of follow up, source of the data (published or obtained by personal communication), country of publication, sex, and strength of reporting". I think a verb is missing. Moreover, I think that further analyses, such as diagnosis (mood disorders, psychosis), should be added. [We have added new analysis by diagnosis and by whether the patients all had suicidal thought or behaviours]

Results

This paragraph is difficult to read. It should be reduced, also with the help of a further table.

[this has been rewritten and we have included new tables]

The strength of reporting is not well described. A single paragraph should be dedicated to it.

[this has been included]

Figures are of very low quality.

[the figures have been improved]

eSupplement 1. Strength of reporting items of included studies: in my opinion De Leo 2007 has a wrong Strength of Reporting Score [corrected]

Acronyms of tables should be explained (PY, LL, UL).[done]

Discussion

Authors should better describe and compare their results with the ones of Chung's meta-analysis (e.g., how many studies have been included in Chung's analysis on 0-3 months? Were they able to include the same studies as you in relation to 0-1 month?).

[this has been done and all the numbers of paper included]

Reviewer: 3

Reviewer Name: Maria Vazquez-Montes

Institution and Country: University of Oxford, United Kingdom

Please state any competing interests or state 'None declared': No competing interests

Please leave your comments for the authors below

This is a very interesting and relevant piece of work that complements previously published work by the authors (JAMA Psychiatry. 2017 Jul 1;74(7):694-702. doi: 10.1001/jamapsychiatry.2017.1044) by now estimating the one- and four-week suicide rates post-psychiatric care discharge in adult patients. The manuscript is well structured and the overall design is sound. Unfortunately there are details missing in the text that would require further work before this could be ready for publication. Please see my comments and suggestions below.

1. Abstract: More details needed.[done]

a. It is not clear from the objective whether suicide rates will be estimated for a particular population (e.g. adults with suicidal behaviour or a particular diagnosis) or setting (i.e. is it only for patients in psychiatric hospitals?)[this has been made much clearer]

b. Design:

i. Search strategy not mentioned, add a brief statement. [this has been done]

ii. Search period not provided, mention it.[now in the text and figure]

iii. Pg1, Line 53 is missing the noun that is "relevant English-language, peer-reviewed".

[corrected]

iv. Pg1, Line 56 refers to several analyses, but it is not clear why more than one analysis has been done for a single rate estimation. Clarify this.[we think this is clearer, it is the case that we had two estimates for 1 week data, one as subgroup of the one month and a larger stand alone group]

v. Pg2, ln 4 – please rephrase or explain this idea of generalised mixed models (glm) including "random effect models". Without further reading of the manuscript, one could think that this might mean that study is an explanatory variable, not mentioned in the previous sentence, and its coefficient will be random? Or that the glm will be a two-level model grouping by study – but unlikely unless the study is using individual data? Other? In any case, the sentence is not common. [this is clearer and it is clear we used a random effects model]

vi. Pg 2, ln 33, should it be "from" instead of "form"? [corrected]

c. Results:

i. Please review. Counts reported do not coincide with Table 1. Table has data for only 28 and 23 studies reporting four- and one-week data, respectively.[the counts are in new tables 2,3]

2. Introduction:
 - a. Aim could be more specific as suggested in 1a. [done]
3. Inclusion/exclusion criteria:[done]
 - a. From the introduction and methods sections, we can conclude that the inclusion criteria is P-adult patients discharged from psychiatric care (any?)
I-psychiatric hospitalisation, which provides care and protection against suicide
C-descriptive study, no comparator
O-suicide rates per 100,000 person years at 1 and 4 weeks post-discharge
However, there are several studies that at first glance do not seem to satisfy the population inclusion criteria, e.g. Luxton et al (2013) (US Military treatment facilities); Qurashi et al (2006) (unspecified hospital in Manchester); Seemuller et al (2014) (twelve centres across Germany). [we have corresponded with all of these authors confirming that their studies meet inclusion criteria]
Suggestions:
 - a) Be more specific about the inclusion criteria in the text [done]
 - b) Justify the inclusion of seemingly non-psychiatric care discharges [we have not included any non-psychiatric discharges]
4. Search strategy:
 - a. Again, the search strategy and period not mentioned
 - b. Also, please clarify if the search was carried out anew or within the identified articles for the authors' previous research. [done]
5. Data extraction:
 - a. It would be useful to know if a particular extraction form was used and/or have a list of the variables/items extracted in a table. This could also be used to present a summary of the available information.[done]
6. Data analysis:
 - a. Please see comment 1b. I would suggest highlighting what the main analysis is and explaining what all the other analyses are for [done].
 - b. Please complete the last sentence describing what happened to the pre-specified subgroup analyses [done].
7. Results:
 - a. Please see comment 1c. This applies to all analyses. It is not clear how the number of studies included in each analysis were calculated. More detail is needed. For instance, 17 studies report data on the first week after discharge but only 15 are mentioned in the corresponding results paragraph (Pg 9, paragraph 1). [this is now clear in tables 2,3]
 - b. Related to the above, I would suggest to add at the top of Table 1 the number of studies with information on the last two columns; and indicate somehow which studies have information on both columns. [this data is evident in the table 1]
 - c. Does Table 1 show the data retrieved by personal communications? It would be helpful to highlight them. [we did not highlight this in the table – it seemed distracting and is in the acknowledgments]
 - d. Pg 7, Ln 27: Disagreements about “effect size” are mentioned. However, the included studies are not necessarily intervention ones. There is not a comparator in the inclusion criteria. Thus it is not immediately clear which effect size this refers to. Please explain.[we explained have explained this]
 - e. It is not clear how missing data is managed, i.e. studies with no information on suicides are included in the analysis, are these data treated as missing or particular value given? Please specify.[we actually went to great lengths around the zero value issue – this is why the data was fitted – both raw and unfitted data are in the supplements]
 - f. Subgroup analysis based on reporting strength is done but was not mentioned what the cut-off is to define low and high strength and how this was chosen.
8. Conclusion:
 - a. Figures might need to change after reviewing all the above.[done]
9. Supplements:
 - i. eSupplement 1: There is a typo on the strength of reporting score for Castelein 2015 [Corrected]

VERSION 2 – REVIEW

REVIEWER	Raffaella Calati Department of Psychiatry, Mount Sinai Beth Israel
REVIEW RETURNED	30-Nov-2018

GENERAL COMMENTS	<p>Authors have extensively revised the paper. However I am not able to find their reply to the points I raised. This would be useful to better understand their revisions.</p> <p>Minor remark :</p> <p>Methods, Data extraction Authors wrote: “v) whether the primary study of was people admitted for suicidal thoughts and behaviours”. To me this is not clear. Primary outcome? Moreover, I think there is a typo.</p>
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REVIEWER	Maria Vazquez-Montes University of Oxford United Kingdom
REVIEW RETURNED	17-Dec-2018

GENERAL COMMENTS	<p>Thank you to the editors and authors for considering my suggestions. The manuscript was greatly improved making the study more reproducible. All of my comments were satisfactorily addressed. However, there are still a few minor points to consider:</p> <p>Results:</p> <p>i. Quality of study was assessed using “a 0-4 point scale derived from the Newcastle-Ottawa Scale” and then subgroup analysis carried out grouping the data into low and high quality studies. Please clarify what the cut-point for this two-group split is.</p> <p>Supplements:</p> <p>ii. eSupplement 2: There is a typo on the strength of reporting score for De Leo 2007. The total should be 0.</p> <p>iii. The forest plots could benefit with a larger font size.</p>
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VERSION 2 – AUTHOR RESPONSE

1. The data extracted in relation to whether or not the primary research only included studies of people with suicidal thoughts and behaviors have been clarified on page 7. (Reviewer 2)
2. The threshold test for a higher quality study has been stated on page 7. (Reviewer 3)
3. A correction has been made in the e-supplement regarding the total quality score for the de Leo paper (Reviewer 3)
4. the fonts on the two Forest plots have been increased. (Reviewer 3)

Reviewer two also wanted some clarification of the changes made in the previous draft - these are included below

List of changes in response to reviewer 2 at the last round of reviews¹. We have simplified the title to and described the papers simply as a “Meta-analysis”

2. We have mentioned the sensitivity analysis relating to unpublished data in the abstract results

Methods

3. The inclusion and exclusion criteria are more clearly stated on page 6.
4. We have reported the number of many authors contacted by email (page 7) and the replies with data (page 9)

Data extraction

5. We have listed the moderatos on (page 7)

6. We report an eggert test for the two follow up periods (page 11)

7. We reported two further analyses regarding samples defined by diagnosis and by whether the patients all had suicidal thought or behaviours (Table 2)

Results

8. We have elected to include two tables rather report all the results in the text.

9. We have more clearly described the strength of reporting (page 7)

10. The resolution of the Figures has been improved.

11. We have made the requested changes to the esupplement.

Discussion

12. We have more completely compared their study and results with the Chung 2017 meta-analysis and have mentioned the number of studies included in Chung's analysis on 0-3 months? (There were only two studies overlap as mentioned in the introduction on page 5).