

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Are hospital services for self-harm getting better? An observational study examining management, service provision and temporal trends in England
AUTHORS	Cooper, Jayne; Steeg, Sarah; Bennewith, Olive; Lowe, Matthew; Gunnell, DJ; House, AO; Hawton, Keith; Kapur, Navneet

VERSION 1 - REVIEW

REVIEWER	<p>Oivind Ekeberg Professor, Head of Research Unit</p> <p>Department of Acute Medicine Oslo University Hospital Ullevål 0424 Oslo</p> <p>and</p> <p>Department of Behavioral Sciences in Medicine University of Oslo Norway</p> <p>I have no competing interest</p>
REVIEW RETURNED	16-Jul-2013

GENERAL COMMENTS	<p>The aims of the study were to: (i) describe the characteristics and hospital management of self-harm across the same sample of 32 hospitals that took part in an earlier study and ii)– compare the findings with the results from the earlier study conducted in 2001 in order to explore whether the service variability had decreased and service quality had improved over a 10-year period. A total of 6442 individuals presented with 7689 episodes of self-harm at the 32 hospitals during the three-month period were studied.</p> <p>Overall, four out of ten individuals left hospital without having had an assessment with a mental health specialist. Compared to 2001, there was little difference in the proportion of episodes receiving specialist assessment, a significant increase in general hospital admission, and a decrease in referrals for specialist mental health follow up but limited evidence for progress in markers of overall service quality</p> <p>This is an important study. The manuscript, however, needs some revision before being published. Major comments: 1. This paper forms part of the study 'Variations in self-harm service</p>
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delivery: an observational study examining outcomes and temporal trends' with the main research question: Does the variability in service provision for self-harm have any impact on patient outcomes? (ii) Additional research questions: Has the variability in service provision decreased over recent years? Has the quality of self-harm services improved over recent years?

The research questions differ on one main point, i.e. whether the variability in service provision has any impact on patient outcomes. Adding these findings would be of great interest. On the other hand, it is not clear whether the data makes it possible to answer this research question. What is the model?

2. It would be of great interest to look closer at subgroups of patients regarding psychosocial assessment and follow-up measures, e.g. suicide intent, admission to hospital, method of self-harm. The need for psychiatric assessment is greater for a patient treated in an intensive care unit after strangulation than for a patient with minor laceration treated in the ED. Even though all patients were admitted because of self-harm, a distinction between those with serious suicide attempts vs. not is clinically relevant. If it is valid that 40% of patients are discharged without mental health assessment after a serious suicide attempt in England, it is of concern.

3. There is far too much information about the values from the 32 hospitals. This is hardly of interest for the average reader. Accordingly, table 1 and figure 1 may be deleted. A short table showing the means and ranges for the different variables in table 1 would be sufficient. One sentence explaining how many percent of the hospitals had a positive and how many a negative change in total Service Scale would also give the most important results. One sentence may also give the correlation between Service Score and proportion offered specialist mental health follow-up. Accordingly, figure 2 may also be deleted.

4. More information about the Service Scale is needed. Has the factor structure been studied? Is the sum score the optimal measure?

Minor comments:

5. The authors may consider to add "England" in the title.

6. It would be of interest to discuss a) that only 20% of the psychosocial assessments were made by a psychiatrist and b) more about the reduced rate of referral to specialist mental health follow-up. The authors suggest that this may be due to greater involvement of primary care in follow up arrangements. The rate of referral to GPs, non-statutory mental health/voluntary/other services did not increase. What was the proportion of patients without any referral?

7. A short description of the EDs in England would be informative. Are all of them part of the hospital? How long can a patient stay in the ED before discharge/transfer to a hospital bed?

8. Are there no data on the main groups of toxic agents?

9. The statement that patients who self-cut have been shown to be of greater risk of future suicide needs modification. Even though one study showed this (ref 16, Cooper et al.), there are also other findings (Runeson B, et al. Method of attempted suicide as predictor of subsequent successful suicide: national long term cohort study.

	BMJ. 2010 Jul 13;341:c3222. doi: 10.1136/bmj.c3222). 10. A conclusion is lacking
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REVIEWER	Dr Eleni Karasouli Research Fellow Warwick Medical School, University of Warwick, UK No competing interests.
REVIEW RETURNED	22-Jul-2013

THE STUDY	Reference 17 is used to potentially explain the decrease in specialist follow up referrals. However it is dated 1995 and it is hence older than both of the authors' studies used to compare data. So the authors' assumption that the decrease in levels of referrals for specialist follow up may be due to greater primary care involvement cannot be inferred based on the paper (even though there is recent movement towards primary care involvement in mental health). Is there any recent evidence that could perhaps provide a better explanation for this variation?
REPORTING & ETHICS	Patient consent was not obtained, however it was not required as data were collected by clinical staff and anonymised prior to the research team's involvement. All appropriate local and ethical approvals have been gained. For clarity, perhaps the authors would like to consider including in the "Local and ethical approval" section a clear statement of why patient consent was not necessary and hence not attained.
GENERAL COMMENTS	This is a very interesting paper, it is clearly written and well-presented. I have provided some minor suggestions that could improve the impact of the paper. I am including below some additional comments for consideration: 1. In the second part of the results in the abstract the authors present findings based on comparison with data from 2001. As stated in the method section of the main paper the 2001 data were collected in an earlier study. This is not clear in the abstract and may confuse some readers. 2. Even though the earlier study has been referenced, it would be interesting to see in the current paper a very brief description of the earlier study, including differences/ similarities with current study on methodology. 3. I am impressed with the pilot data collection exercise employed by the researchers as it adds rigour to the study. 4. The authors acknowledge the paper's strengths and limitations. It is particularly important that they mention the lack of interventions when considering referral to services. 5. Table 1 is interesting as it presents individual hospital characteristics. As the authors mention, there is variation in the service scale score. I am wondering if this variation reflects the number of SH presentations per hospital. For example, are the Clinical Guidelines for management of SH followed more closely in hospitals where they care more frequently for patients that present because of SH? It does not seem to be the case when interpreting the table, however it would be interesting to explore further in future studies.

REVIEWER	David Christmas Consultant Psychiatrist
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	NHS Tayside United Kingdom
REVIEW RETURNED	27-Jul-2013

THE STUDY	<p>1) Methods</p> <p>a) Setting and Sample</p> <p>i) The authors refer to replacing one hospital with another "...from within the appropriate stratum." However, it isn't clear how these strata are constructed and it might be helpful if there could be some explanation of the hospitals were stratified so that the reader can be reassured that the substitution is valid.</p> <p>2) Data Collection</p> <p>a) Descriptive Study</p> <p>i) Although the authors refer to "relevant search terms", the terms used aren't listed anywhere. Given that the identification of valid episodes of self-harm is a crucial part of this study, I think it would be helpful if there could be either: a) a description of the search terms; or, b) an online appendix listing the terms used. The original paper (Bennewith et al, 2004) doesn't list these terms either.</p> <p>ii) Later in this section the authors refer to adjusting the search terms to "maximise case ascertainment". Some description of this, along with access to the original search terms, is necessary in order to demonstrate that the search process was valid and that almost all cases were identified.</p>
RESULTS & CONCLUSIONS	<p>3) Analysis</p> <p>i) Correlations between total service score and "levels of hospital key management" were performed. Could the authors describe how the 'hospital key management' was assessed, perhaps by provide an online appendix similar to Appendix 2 ('21 items of service quality')?</p> <p>4) Results</p> <p>a) Characteristics of Individuals</p> <p>i) It is interesting to note that 2/3 of patients who self-harmed were not in contact with psychiatric services. This is a broadly similar finding to Scotland, where only 1-in-5 suicides had had contact with mental health services in the year before death (Information Services Division, 2012).</p> <p>b) Specialist Assessment</p> <p>i) The authors report that the median no. of hours between presentation and assessment was 11. Could the authors clarify whether this indicated delays in assessment, or whether it was considered appropriate in order to permit assessment of a less-intoxicated patient, for example?</p> <p>ii) Why was the assessment rate lower in those who self-cut instead of self-poisoned? Could the authors provide some further explanation (such as higher rates of self-cutting patients self-discharging before they could be seen, perhaps)?</p> <p>c) Variation in management of episodes between hospitals</p> <p>i) Given that the proportion admitted to a medical ward varied between 22% and 85%, could the authors comment on whether this was related to service structures, availability of short-stay wards, and other factors? Given that this isn't clearly recorded in the questionnaire, the availability of temporary assessment (or 'sobering-up') facilities may have led to some assessments having to make a dichotomous choice between allow home or admit.</p> <p>d) Comparison between 2001-2002 and 2010-2011</p> <p>i) The authors report that there were more episodes in the 2010/11 compared to 2001/02. It isn't clear what the denominator was in this comparison; specifically, was there any correction for population size. For example, if the population increased by 20% in this 10</p>

	<p>years (perhaps due to mergers of health authority areas), then an increase in self-harm episodes of 24% is not exceptional.</p> <p>ii) Could the authors please discuss why a higher proportion of episodes in the present study resulted in admission to hospital? How does this relate to changes in service provision (such as availability of short-stay wards in A&E)? Was there any relationship between who assessed the patient (doctor/nurse/social worker) and likelihood of admission to a medical ward?</p> <p>iii) Similarly, there is a mention of patients being admitted to a Medical Assessment Unit. As these are often temporary bays, did this count as a hospital admission for the purposes of this study? A lower threshold (or greater availability) of such units may explain why the admission rates went up.</p> <p>5) Discussion</p> <p>a) Main findings</p> <p>i) Since one-fifth of patients had follow-up from non-statutory services, could the authors elaborate on whether there was a relationship between proper assessment and type of follow-up? One might imagine that patients who had a full psychosocial assessment might have different follow-up arrangement than patients who had not had a detailed assessment.</p>
<p>GENERAL COMMENTS</p>	<ul style="list-style-type: none"> • I thought that this was a useful addition to the understanding of self-harm services in the UK and BMJ Open would seem like a good fit with the content. • It was generally well-written and concise.

VERSION 1 – AUTHOR RESPONSE

Comments by reviewers

This is an important study.

The manuscript, however, needs some revision before being published.

Major comments:

1. This paper forms part of the study ‘Variations in self-harm service delivery: an observational study examining outcomes and temporal trends’ with the main research question: Does the variability in service provision for self-harm have any impact on patient outcomes? (ii) Additional research questions: Has the variability in service provision decreased over recent years? Has the quality of self-harm services improved over recent years?

The research questions differ on one main point, i.e. whether the variability in service provision has any impact on patient outcomes. Adding these findings would be of great interest. On the other hand, it is not clear whether the data makes it possible to answer this research question. What is the model?

We are able to look at the variability of service provision and its impact on patient outcomes as we have collected data on subsequent visits to the emergency department with self-harm in this cohort of patients. However, it is beyond the scope of the current report to present these findings. These data are currently being examined in detail and we will publish them in a subsequent paper. We now comment in the ‘Discussion’ section, ‘Implications for research and practice’:

“Of course linking findings on variability of services to outcomes is of interest but this would require substantial further analysis that goes beyond the scope of the current report.”

2. It would be of great interest to look closer at subgroups of patients regarding psychosocial assessment and follow-up measures, e.g. suicide intent, admission to hospital, method of self-harm. The need for psychiatric assessment is greater for a patient treated in an intensive care unit after strangulation than for a patient with minor laceration treated in the ED. Even though all patients were admitted because of self-harm, a distinction between those with serious suicide attempts vs. not is clinically relevant. If it is valid that 40% of patients are discharged without mental health assessment

after a serious suicide attempt in England, it is of concern.

We agree this would be an interesting area of study and we have conducted some further analysis along these lines ['Results', 'Specialist assessment (all episodes)' section]. Ascertaining high suicidal intent consistently across 32 centres is problematic in a study of this sort and we did not include a direct measure of suicide intent. Instead we use proxy markers of intent/seriousness: hanging/strangulation (a method of harm associated with an increased risk of subsequent suicide 1) and admission to an intensive care unit, an indication of medical seriousness of the attempt. We now examine the association between these markers and subsequent psychiatric assessment.

3. There is far too much information about the values from the 32 hospitals. This is hardly of interest for the average reader. Accordingly, table 1 and figure 1 may be deleted. A short table showing the means and ranges for the different variables in table 1 would be sufficient. One sentence explaining how many percent of the hospitals had a positive and how many a negative change in total Service Scale would also give the most important results. One sentence may also give the correlation between Service Score and proportion offered specialist mental health follow-up. Accordingly, figure 2 may also be deleted.

We agree with this reviewer that the information in Table 1 is detailed and now present summary data only. However Reviewer 2 found this table informative so we suggest it is available as an appendix (we will leave this to the editor's discretion). Figure 1 has now been removed and we describe these findings in the text ['Results', 'Comparison of service provision between 2001-2002 and 2010-2011 (service interviews)' section].

Figure 2 has now been removed as suggested by this reviewer; we have already described the correlation values in the text ('Results', 'Service Score and management' section).

4. More information about the Service Scale is needed. Has the factor structure been studied? Is the sum score the optimal measure?

We now provide further information about the Service Scale under 'Methods', 'Data collection', 'Service Interviews'.

The Service Scale measure was developed as part of our previous study and was based on key elements of national guidance. Its use in the current study was principally to allow comparison with data from 2001 rather than as a standalone measure of service quality. We did not conduct a factor analysis on its items. We agree that this would have been potentially helpful but it is beyond the scope of this paper. We acknowledge these potential issues in our strengths and limitations section. It was essential that we presented data from the raw score to enable us to compare findings with our previous analysis from 2001. The sum score is one way of summarising the overall service quality and allowing a direct comparison with the earlier study.

Minor comments:

5. The authors may consider to add "England" in the title.

We have now added this to the title for clarity

6. It would be of interest to discuss a) that only 20% of the psychosocial assessments were made by a psychiatrist and b) more about the reduced rate of referral to specialist mental health follow-up. The authors suggest that this may be due to greater involvement of primary care in follow up arrangements. The rate of referral to GPs, non-statutory mental health/voluntary/other services did not increase. What was the proportion of patients without any referral?

In line with previous research in the UK, most patients are assessed by mental health nurses. We have now commented in the Discussion section about the different type of assessors and the potential influence this may have on the rate of in-patient admission. We have also now included in the text the proportion of patients assessed who received no further referral ('Results', 'Comparison between 2001-2002 and 2010-2011') but have deleted the explanation for the decrease in specialist follow up referrals being due to greater involvement of primary care in follow up arrangements

('Discussion') as this reviewer correctly points out, this was not borne out by our data. We do discuss other potential explanations for this finding.

7. A short description of the EDs in England would be informative. Are all of them part of the hospital? How long can a patient stay in the ED before discharge/transfer to a hospital bed?

We now describe our sample in more detail ('Methods', 'Setting and sample'). The sample was selected only from hospitals in England with an Emergency Department on site and we describe in the 'Discussion' section how the use of short stay beds has increase. We have now added information relating to the Government recommended target for patients to be discharged or transferred from the ED within 4 hours – a breach of this target comes with a financial penalty.

8. Are there no data on the main groups of toxic agents?

This was not the focus of this paper but we will be presenting detailed examination of the medical agents and non-ingestible substances used in self-poisoning in a paper on the epidemiology of self-harm. In the 'Results section, Characteristics of individuals' we now add: "More detailed consideration of the methods used is beyond the scope of this paper and will be reported later."

9. The statement that patients who self-cut have been shown to be of greater risk of future suicide needs modification. Even though one study showed this (ref 16, Cooper at al.), there are also other findings (Runeson B, et al. Method of attempted suicide as predictor of subsequent successful suicide: national long term cohort study. BMJ. 2010 Jul 13;341:c3222. doi: 10.1136/bmj.c3222).

We have modified this statement by adding "in the UK", and acknowledge that self-cutting is not universally recognized as a group of increased risk of suicide.

10. A conclusion is lacking

We have added this subtitle in the Discussion section and summarized the main findings.

Reviewer: Dr Eleni Karasouli
Research Fellow
Warwick Medical School, University of Warwick, UK

No competing interests.

Reference 17 is used to potentially explain the decrease in specialist follow up referrals. However it is dated 1995 and it is hence older than both of the authors' studies used to compare data. So the authors' assumption that the decrease in levels of referrals for specialist follow up may be due to greater primary care involvement cannot be inferred based on the paper (even though there is recent movement towards primary care involvement in mental health). Is there any recent evidence that could perhaps provide a better explanation for this variation?

We have now deleted this reference and explanation for the decrease in specialist follow up referrals ('Discussion', page 15) as this was not borne out by our data (see also point 6, reviewer 1). Other explanations for this finding are discussed.

Patient consent was not obtained, however it was not required as data were collected by clinical staff and anonymised prior to the research team's involvement. All appropriate local and ethical approvals have been gained. For clarity, perhaps the authors would like to consider including in the "Local and ethical approval" section a clear statement of why patient consent was not necessary and hence not attained.

We thank the reviewer for her suggestion and have added to 'Methods', 'Local and ethical approval': "The data collection process at each site was classified as a local audit and therefore individual patient consent was not required."

This is a very interesting paper; it is clearly written and well-presented. I have provided some minor suggestions that could improve the impact of the paper. I am including below some additional comments for consideration:

1. In the second part of the results in the abstract the authors present findings based on comparison with data from 2001. As stated in the method section of the main paper the 2001 data were collected in an earlier study. This is not clear in the abstract and may confuse some readers.

We have now clarified this point and added to the abstract “since an earlier study in 2001”.

2. Even though the earlier study has been referenced, it would be interesting to see in the current paper a very brief description of the earlier study, including differences/ similarities with current study on methodology.

As in the previous reviewers comments (point 4) we have now described in more detail how consistency between the studies was achieved on Service Scores (‘Data collection’, ‘Service interviews’): “Consistency of coding was achieved by meetings and regular correspondence between key researchers from the earlier and current study.”. Whilst we describe differences and similarities between the studies in the methods and results sections, for example we achieved data collection on 31 out of the original 32 hospitals, the earlier study collected information on self-harm patients over a shorter time period and did not identify individuals but episodes only. We agree a summary of the two studies in terms of the similarities and differences would be useful and have now included a table as an appendix (as referred to at the end of the ‘Methods’ section: “A summary of the methodology used in the present study and differences and similarities with our previous study is shown in Appendix 3.”).

3. I am impressed with the pilot data collection exercise employed by the researchers as it adds rigor to the study.

We thank the reviewer for this comment.

4. The authors acknowledge the paper’s strengths and limitations. It is particularly important that they mention the lack of interventions when considering referral to services.

In the ‘Strengths and Limitations’ section, we acknowledge that we do not measure the actual receipt of interventions.

Although evidence of treatment efficacy for this group of patients remains unresolved, we do not feel this is a ‘strength and limitation’ of this particular study.

5. Table 1 is interesting as it presents individual hospital characteristics. As the authors mention, there is variation in the service scale score. I am wondering if this variation reflects the number of SH presentations per hospital. For example, are the Clinical Guidelines for management of SH followed more closely in hospitals where they care more frequently for patients that present because of SH? It does not seem to be the case when interpreting the table, however it would be interesting to explore further in future studies.

In the interests of brevity and as recommended by Reviewer 1, we now present this table as a summary only and we feel that the variation in hospital characteristics will still be apparent from these summary statistics. We have suggested however that the table be available as an appendix. We agree it would be interesting to explore further in future studies the correlation between numbers of self-harm patients and adherence to clinical guidelines.

Reviewer: David Christmas
Consultant Psychiatrist
NHS Tayside
United Kingdom

- I thought that this was a useful addition to the understanding of self-harm services in the UK and BMJ Open would seem like a good fit with the content.
- It was generally well-written and concise.

1) Methods

a) Setting and Sample

i) The authors refer to replacing one hospital with another "...from within the appropriate stratum." However, it isn't clear how these strata are constructed and it might be helpful if there could be some explanation of the hospitals were stratified so that the reader can be reassured that the substitution is valid.

We now make it clear that the original sample was stratified so that four hospitals were selected within each of the eight former Health Regions in England and clarified how we selected the substitute hospital ('Methods', 'Setting and Sample').

2) Data Collection

a) Descriptive Study

i) Although the authors refer to "relevant search terms", the terms used aren't listed anywhere. Given that the identification of valid episodes of self-harm is a crucial part of this study, I think it would be helpful if there could be either: a) a description of the search terms; or, b) an online appendix listing the terms used. The original paper (Bennewith et al, 2004) doesn't list these terms either.

We have now included information about the search terms used and present these in Appendix 3.

ii) Later in this section the authors refer to adjusting the search terms to "maximise case ascertainment". Some description of this, along with access to the original search terms, is necessary in order to demonstrate that the search process was valid and that almost all cases were identified.

Not all hospital systems are the same. One way maximizing case ascertainment was to cross check mental health lists with ED lists as specified in the text; other approaches were more specific to each hospital but include cross-checking the generated list of cases against all ED attendances to ascertain if any cases had not been captured. We have added information about this, and provided examples of search terms used, in Appendix 3.

3) Analysis

i) Correlations between total service score and "levels of hospital key management" were performed. Could the authors describe how the 'hospital key management' was assessed, perhaps by provide an online appendix similar to Appendix 2 ('21 items of service quality')?

We apologise for the lack of clarity - categories of key aspects of hospital management were simply the ones listed in Table 2. This is now referred to in the Analysis section page 10, 'using categories as set out in Table 2' for further clarity.

4) Results

a) Characteristics of Individuals

i) It is interesting to note that 2/3 of patients who self-harmed were not in contact with psychiatric services. This is a broadly similar finding to Scotland, where only 1-in-5 suicides had had contact with mental health services in the year before death (Information Services Division, 2012).

This is an interesting observation and adds validity to the generalisability of our results.

b) Specialist Assessment

i) The authors report that the median no. of hours between presentation and assessment was 11. Could the authors clarify whether this indicated delays in assessment, or whether it was considered appropriate in order to permit assessment of a less-intoxicated patient, for example?

We are unable to examine the reasons for the length of time between presentation and assessment from our data but we might infer that the delay in assessment of those admitted was due to waiting until the patient was medically fit to be assessed. We now report on the median number of hours wait for patients presenting with and without alcohol, provide the median wait for episodes admitted to a medical ward (we had previously presented the median for those not admitted) and have inserted these results to the section 'Results', Specialist assessment (all episodes): "compared to 14 hours

(IQR 8 to 25) for those admitted. Episodes where alcohol had been taken within 6 hours of the self-harm act were assessed after a median wait of 12 hours (IQR, 6 to 20) compared to nine hours (IQR, 5 to 19) where no alcohol was involved". We now comment in the discussion that "differences between time of presentation and assessment may be explained by medical fitness and/or intoxication of the patient".

ii) Why was the assessment rate lower in those who self-cut instead of self-poisoned? Could the authors provide some further explanation (such as higher rates of self-cutting patients self-discharging before they could be seen, perhaps)?

We have conducted further analysis on this group and report in 'Results', Specialist assessment (all episodes): "In 15% of episodes the individuals did not wait or refused assessment, 14% amongst episodes involving self-poisoning and 18% for self-cutting episodes ($p = 0.005$)."

We have now commented on this finding in the 'Discussion' section to further explain the low assessment rate in this group. "Consistent with other large scale surveys 19 levels of assessment in those who had cut themselves were reduced - they were less likely to complete treatment and more likely to specifically refuse assessment."

c) Variation in management of episodes between hospitals

i) Given that the proportion admitted to a medical ward varied between 22% and 85%, could the authors comment on whether this was related to service structures, availability of short-stay wards, and other factors? Given that this isn't clearly recorded in the questionnaire, the availability of temporary assessment (or 'sobering-up') facilities may have led to some assessments having to make a dichotomous choice between allow home or admit.

We have looked at this in more detail and included further analysis in the 'Results', 'Variation in management of episodes between hospitals' section: "Each of the 32 hospitals had some form of short-stay ward or observation/assessment unit and medical admission here included referrals to these beds. There was no significant correlation between the proportion of episodes involving poisoning with drugs and the proportion admitted to a medical bed (Spearman's $r = 0.249$, $P = 0.17$)".

We now comment in 'Discussion': "Seventeen hospitals reported that the use of such beds had been introduced or had increased since the earlier study." and "As all hospitals in the current study had short-stay wards or medical observation/assessment units, the variation in proportion of medical admissions between hospitals cannot be attributed to availability of short stay wards."

d) Comparison between 2001-2002 and 2010-2011

i) The authors report that there were more episodes in the 2010/11 compared to 2001/02. It isn't clear what the denominator was in this comparison; specifically, was there any correction for population size. For example, if the population increased by 20% in this 10 years (perhaps due to mergers of health authority areas), then an increase in self-harm episodes of 24% is not exceptional.

We didn't correct for population size and we now acknowledge this in the 'Discussion'.

ii) Could the authors please discuss why a higher proportion of episodes in the present study resulted in admission to hospital? How does this relate to changes in service provision (such as availability of short-stay wards in A&E)? Was there any relationship between who assessed the patient (doctor/nurse/social worker) and likelihood of admission to a medical ward?

We already comment on the general availability of short-stay wards and avoidance of breach waiting times in ED in the 'Discussion' section in relation to the higher proportion of episodes resulting in a medical admission, and in addition, now comment on the increased availability of short stay wards in the current study. The discipline of the ED clinician making the initial medical assessment was not recorded. We feel that it would be of limited use investigating the relationship between the type of psychiatric assessor and the likelihood of medical admission as in many instances the specialist assessment would have taken place after medical admission.

iii) Similarly, there is a mention of patients being admitted to a Medical Assessment Unit. As these are often temporary bays, did this count as a hospital admission for the purposes of this study? A lower threshold (or greater availability) of such units may explain why the admission rates went up.

These were counted as medical admissions and we do mention the use of observation and assessment wards as a possible reason for the increase in medical admission since 2001.

5) Discussion

a) Main findings

i) Since one-fifth of patients had follow-up from non-statutory services, could the authors elaborate on whether there was a relationship between proper assessment and type of follow-up? One might imagine that patients who had a full psychosocial assessment might have different follow-up arrangement than patients who had not had a detailed assessment.

We only have data on whether or not a psychosocial assessment by a mental health specialist took place and not its quality. However, our definition for the minimum criteria for assessment (as in the previous study) was as defined in 'Data collection', 'Descriptive study': "an interview carried out by a member of mental health staff who has been trained in the process, is usually of about 30 minutes duration, and covers the assessment of factors such as the causes and degree of suicidal intent, current mental state and level of social support, psychiatric history, personal and social problems, future risk and need for follow-up".

We have now included in the 'Results', 'Specialist assessment (all episodes)' section some more detail about the proportions of referrals for follow-up care by assessment status: "Episodes receiving specialist mental health assessment were more likely to result in arrangement for follow-up care to specialist mental health outpatient services (45% vs. 13% for non-assessed episodes, $p = <0.001$) or to non-statutory services (21% vs. 12%, $p = <0.001$)".

References

Information Services Division (2012) The Scottish Suicide Information Database Report 2012. Edinburgh: NHS Scotland. <http://www.isdscotland.org/Health-Topics/Public-Health/Publications/2012-12-18/2012-12-18-ScotSID-2012-Report.pdf>

References

1. Runeson B, Tidemalm D, Dahlin M, Lichtenstein P, Langstrom N. Method of attempted suicide as predictor of subsequent successful suicide: national long term cohort study. *BMJ* 2010; 340: c3222. doi:10.1136/bmj.c3222

VERSION 2 – REVIEW

REVIEWER	Oivind Ekeberg Professor, Head of Research Unit Department of Acute Medicine Oslo University Hospital Ullevål 0424 Oslo and Department of Behavioral Sciences in Medicine University of Oslo Norway
REVIEW RETURNED	01-Oct-2013

GENERAL COMMENTS	I am pleased with the amendments that have been made, and have no further comments.
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REVIEWER	David Christmas NHS Tayside and University of Dundee Scotland, UK
REVIEW RETURNED	08-Oct-2013

- The reviewer completed the checklist but made no further comments.