

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Dynamic pattern of suicide in Australia, 1986–2005, a descriptive-analytic study
AUTHORS	Qi, Xin; Hu, Wenbiao; Page, Andrew; Tong, Shilu

VERSION 1 - REVIEW

REVIEWER	Dr Keith Miller Flinders University, Adelaide, Australia
REVIEW RETURNED	17-Apr-2014

GENERAL COMMENTS	<ol style="list-style-type: none">1. Grammar and punctuation: some verb constructions, eg. passive verb in Abstract under Objective ("...little has been done..."); active verb under Strengths and Limitations ("...results are difficult to interpret..."; use of indefinite articles ("a") and definite articles ("the"); plurals - these are dispersed through the manuscript. It would be helpful if an editor read the manuscript again and strengthened the grammatical deficiencies.2. Use complete words, eg. under Strengths and Limitations of the Study: "cross-tabulations".3. When initials are first used, it is preferable for readers unfamiliar with these details to provide the full title as well, eg. LGA - Local Government Association, etc.4. In Introduction, a bland generalisation is made: "In Australia...remained below 10 per 100,000 in recent years." Yet during the 1990s, they were around 13 per 100,000 and peaked in 1997 at around 14.6 (ABS, 2004).5. Following sentence: "...mainly in young males..." is inaccurate. Highest numbers were amongst males 25-54.6. The concluding sentences of this Introductory paragraph make some claims that I am not sure are accurate. A number of studies have considered the long-term trends and compared suicides in different regions (metropolitan, rural and remote, farms), compared methods and age groups. Perhaps this has not been done specifically between 1986 and 2005.7. Under Design, an excellent explanation was provided of Data Source and Statistical Analysis.8. Results section portrays data findings appropriately.9. Discussion: there was no real literature review provided at the beginning of the manuscript and this has been combined with the Discussion section. This is a slightly unorthodox approach.10. There seems little acknowledgement that the numbers of suicides within different regions are actually quite small, although mention is made that there were zero suicides in some shires in a 5 year period. Due to these very low numbers in small areas, the statistical validity of conducting quantitative measures and analysis comes into question.
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	<p>11. There is a paragraph which indicates a discrepancy between QSR and ABS data in Queensland. It is really a paragraph highlighting a limitation of the data. But it seemed to provide a cursory and inadequate explanation of the discrepancy.</p> <p>12. In the Abstract, Indigenous communities were mentioned as being of high risk. While I agree with this, only a vague mention was made of this in the Discussion section rather than it being investigated appropriately as the Abstract implied.</p> <p>13. Whilst the final paragraph on Recommendations was legitimate, it was also quite vague and non-specific.</p> <p>14. I can appreciate the research which has gone into developing this manuscript. But it offers little that is new to the debate on suicide in Australia. It is a broad overview with few details and few incisive findings. For me, an interesting observation made in the Discussion was that there are less suicides by firearms but more by hangings. For me, the implication of this is that the method of suicide should only be considered a minor influence on suicides. Greater emphasis needs to be placed on the deeper causes as to why individuals consider suicide in the first place.</p> <p>15. In terms of references, you used 49 but there was little consideration given to a number of them.</p> <p>I acknowledge the amount of research which has gone into this manuscript. But there were some concerns which need to be addressed. I felt the paper was a summary and cursory overview of research which has been undertaken previously. And so nothing new or revelatory came from it. This could perhaps be addressed by focusing on certain areas and considering these in a little more detail. I think the emphasis was supposedly on the dynamic nature of suicide but, as I read the manuscript, I did not find this to be the case.</p> <p>One of the flaws in the argument was attempting to make more of a variation in LGA reports of suicide over time. My research has been that in one LGA there might be a spate of suicides over a short period of time and then none for the next 20 years. It appeared to me that you were attempting to make more of this than was evident. My thought is that an LGA is statistically too small for a measure on suicides.</p>
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REVIEWER	Nestor D. Kapusta Medical University of Vienna Department for Psychoanalysis and Psychotherapy Suicide Research Group
REVIEW RETURNED	01-May-2014

GENERAL COMMENTS	<p>The authors present a well performed study on Australian suicide mortality using novel mapping approaches. However, most of the results are not new, only the methodology improved. There are several problems:</p> <p>A major drawback is that only old outdated data were used. Another drawback is that the discussion does not follow the results but very speculative etiological assumptions are made instead. For example some discussion sections go too far, conclusions like "However, little evidence has indicated that antidepressant use has significant effectiveness on youth suicide" seem not appropriate in the context of a descriptive mortality study.</p> <p>In conclusion the value of this study is in the methodology of graphical depiction of data. In order to improve this study, actualized</p>
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	data and a toned down discussion focused on results would be necessary. Or authors might use socio-economic and other factors to justify their discussion of etiological factors.
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REVIEWER	Dr. Derek Cheung The University of Hong Kong
REVIEW RETURNED	08-May-2014

GENERAL COMMENTS	<p>The study is unique and specific in presenting the long term suicide pattern, but needs substantial revision for recommended publication.</p> <p>Abstract The sentence "Male rate was high in rural areas while female rate was high in capital cities" needs to be re-written. Indeed, female rate in capital cities is low and comparable with rural areas.</p> <p>Introduction The literature review is not thorough. Many studies about long-term trend and spatial analysis over small areas have been done (e.g. Taiwan, UK, Australia..), but the authors have not addressed them.</p> <p>Design a. It should state clearly whether the SLA and the LGA referred to the residential address or the suicide location of the suicide deceased. b. The statistical method is too brief. Authors should say how the poisson regression model is constructed for table 1 to 3. In table 1 and 2, the reference group was not specified in the whole manuscript. Are the RR adjusted by other covariates? How can the readers interpret those risk ratios? Are these variables entered into the model simultaneously or separately?</p> <p>Result a. In line 33 of page 6, author should report the proportion of suicide methods so that readers can compare them. b. In line 55 of page 6, "between 15 and 34 years" should be changed to "from 15 to 34 years" c. In line 7 of page 7, "The trends of suicide among 15 to 54 years had slight fluctuations during the study period among different sub-groups" is unclear. Please describe the pattern completely. d. In line 16-23 of page 7, authors reported that some increase/decrease happened especially in subgroups (males and rural and remote areas). Any interaction term in the regression model has been tested? e. The maps of hanging and firearm suicide over the study period are very useful to show the temporal pattern. On the contrary, the maps on page 21 aggregated all suicides in the period for each subgroup. The aggregation may hide the temporal changes, which the readers are more interested to know. f: In line 20 of page 7, "in between 1996 and 2000" should be changed to "from 1996 to 2000" g: In line 38 of page 8, change "suicide by hanging kept increasing" to "the suicide rate by hanging has increased in most areas of Australia since 1986"</p> <p>Discussion a. In line 30 of page 9, "Usually older results are less likely to seek for psychiatric support.." Delete "for". b. The association between increase in use of antidepressants and</p>
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	<p>reduction in suicide in older adults needs more justification. Did elderly suicide attribute a lot to psychiatric disorder?</p> <p>c. The first two sentences in line 8 of page 10 should be moved to result section.</p> <p>d. In line 17 of page 10, delete “and”</p> <p>e. In line 19 of page 10, “In this study, there were more decreased male suicide rate...” this finding was not mentioned in the result section.</p> <p>f. The drop of firearm suicide led by the restriction of firearm is interesting, but the authors should describe the history and phenomenon in more details. For instance, how the firearm restriction has changed over the years and how it related to the gradual decrease of suicide. Also, authors suggested the substitution effect by hanging but did not elaborate the mechanism. Authors should described more and cited more previous studies in this mechanism.</p> <p>g. In line 18-32 of page 11, please spell out the suicide rate in these area for better comparison.</p> <p>h. Some conclusions are over-interpreted and out of scope from the results. Such as “local vulnerable population (e.g. farmers and Indigenous population)” (line 55 of page 12) and “The effectiveness of antidepressant use...” (line 7 of page 13).</p> <p>i. In line 14 of page 13, “Potential impacts of socioeconomic and environmental factors...” are very vague. Authors need to discuss them in details.</p>
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VERSION 1 – AUTHOR RESPONSE

In response to Reviewer 1 (Dr Keith Miller)

1. Grammar and punctuation: some verb constructions, eg. passive verb in Abstract under Objective (“...little has been done...”); active verb under Strengths and Limitations (“...results are difficult to interpret...”; use of indefinite articles (“a”) and definite articles (“the”); plurals - these are dispersed through the manuscript. It would be helpful if an editor read the manuscript again and strengthened the grammatical deficiencies.

Reply: We have revised the errors.

2. Use complete words, eg. under Strengths and Limitations of the Study: “cross-tabulations”.

Reply: We have revised them.

3. When initials are first used, it is preferable for readers unfamiliar with these details to provide the full title as well, eg. LGA - Local Government Association, etc.

Reply: We have notified full title of SLA and LGA in part of “Strength and Limitations” (2nd paragraph, Page 3).

4. In Introduction, a bland generalisation is made: “In Australia...remained below 10 per 100,000 in recent years.” Yet during the 1990s, they were around 13 per 100,000 and peaked in 1997 at around 14.6 (ABS, 2004).

Reply: We have rewritten this part (1st paragraph, page 4).

5. Following sentence: “...mainly in young males...” is inaccurate. Highest numbers were amongst males 25-54.

Reply: We have revised this (1st paragraph, page 4).

6. The concluding sentences of this Introductory paragraph make some claims that I am not sure are accurate. A number of studies have considered the long-term trends and compared suicides in different regions (metropolitan, rural and remote, farms), compared methods and age groups.

Perhaps this has not been done specifically between 1986 and 2005.

Reply: We have revised the Introduction paragraph (pages 3 to 4).

7. Under Design, an excellent explanation was provided of Data Source and Statistical Analysis.

Reply: Thank you for your positive comment.

8. Results section portrays data findings appropriately.

Reply: Thank you for your positive comment.

9. Discussion: there was no real literature review provided at the beginning of the manuscript and this has been combined with the Discussion section. This is a slightly unorthodox approach.

Reply: We have added more on literature review in Introduction paragraph (pages 3 to 4).

10. There seems little acknowledgement that the numbers of suicides within different regions are actually quite small, although mention is made that there were zero suicides in some shires in a 5 year period. Due to these very low numbers in small areas, the statistical validity of conducting quantitative measures and analysis comes into question.

The studies by Wilkinson et al and by Burnley used Statistical Division (SD, larger than LGA) as spatial scale, which may mask the spatial variation of suicide in some rural areas (e.g., South Eastern of Western Australia) due to their large size. The study by Cheung et al selected Postal Area (smaller than LGA in general) as spatial scale, which may be too small for a measure on suicides as you mentioned above. After careful consideration, we have applied LGA as the spatial scale in our study, to keep the balance of including a certain number of suicide in each geographical unit and avoiding using too large geographical units.

11. There is a paragraph which indicates a discrepancy between QSR and ABS data in Queensland. It is really a paragraph highlighting a limitation of the data. But it seemed to provide a cursory and inadequate explanation of the discrepancy.

Reply: To avoid inadequate explanation of the discrepancy, we have revised this part in page 13.

Thanks for your suggestions.

12. In the Abstract, Indigenous communities were mentioned as being of high risk. While I agree with this, only a vague mention was made of this in the Discussion section rather than it being investigated appropriately as the Abstract implied.

Reply: We have indicated details of the Indigenous communities with high risk areas in the Abstract, Result (page 9) and Discussion (pages 12 to 13) sections.

13. Whilst the final paragraph on Recommendations was legitimate, it was also quite vague and non-specific.

Reply: We have added specific details, e.g., future suicide research (page 14).

14. I can appreciate the research which has gone into developing this manuscript. But it offers little that is new to the debate on suicide in Australia. It is a broad overview with few details and few incisive findings. For me, an interesting observation made in the Discussion was that there are less suicides by firearms but more by hangings. For me, the implication of this is that the method of suicide should only be considered a minor influence on suicides. Greater emphasis needs to be placed on the deeper causes as to why individuals consider suicide in the first place.

Reply: For some kinds of suicide (e.g., by poison, jumping and gas), there were not much changes of their patterns over time. For suicide by firearms and hanging, the pattern changed over time, which also resulted in the spatial variation over different time periods (dynamic nature of suicide). This is the new finding of this study. Studying at the individual level may provide more information in exploring causes of suicide. However, more detailed individual information were not provided in the suicide database as the Strength and Limitation section indicated. Thus the study may not explore deeper cause of suicide at the individual level. Yet we have explored the socio-economic factors which may contribute to suicide variation over time and place. More work on assessing the associations between these factors and suicide will be accomplish in another study of our research.

15. In terms of references, you used 49 but there was little consideration given to a number of them.

Reply: We have considered and discussed the references, especially in Introduction and Discussion sections. Some of them have been highlighted.

As mentioned above, I acknowledge the amount of research which has gone into this manuscript. But

there were some concerns which need to be addressed. I felt the paper was a summary and cursory overview of research which has been undertaken previously. And so nothing new or revelatory came from it. This could perhaps be addressed by focusing on certain areas and considering these in a little more detail. I think the emphasis was supposedly on the dynamic nature of suicide but, as I read the manuscript, I did not find this to be the case.

One of the flaws in the argument was attempting to make more of a variation in LGA reports of suicide over time. My research has been that in one LGA there might be a spate of suicides over a short period of time and then none for the next 20 years. It appeared to me that you were attempting to make more of this than was evident. My thought is that an LGA is statistically too small for a measure on suicides.

Reply: Thank you for your comments. We have replied as above mentioned.

Reviewer Name Nestor D. Kapusta

Most of the results are not new, only the methodology improved. There are several problems: A major drawback is that only old outdated data were used. Another drawback is that the discussion does not follow the results but very speculative etiological assumptions are made instead. For example some discussion sections go too far, conclusions like "However, little evidence has indicated that antidepressant use has significant effectiveness on youth suicide" seem not appropriate in the context of a descriptive mortality study.

In conclusion the value of this study is in the methodology of graphical depiction of data. In order to improve this study, actualized data and a toned down discussion focused on results would be necessary. Or authors might use socio-economic and other factors to justify their discussion of etiological factors.

Reply: We have deleted "However, little evidence has indicated that antidepressant use has significant effectiveness on youth suicide". In general, we have focused on the results of the study in the Discussion section, e.g., spatial and temporal variation of suicide by firearms and hanging, and suicide in Indigenous communities which is an extension of our previous studies. For socio-economic and other factors, it is inevitable in discussion (even for a descriptive study) and we have made them more concise and focusing on suicide pattern. In assessing the association between socio-environmental factors and suicide, we apply it in another paper we are drafting and revising, using some time series and spatial modelling analyses. In the Introduction and method paragraphs, we have indicated that the details of suicide data after 2005 have not acquired yet and also discussed it in the paragraphs of Strength and Limitations. This is the best we can do by now in collecting detailed suicide database as new as possible.

Reviewer Name Dr. Derek Cheung

The study needs substantial revision for recommended publication.

Abstract

The sentence "Male rate was high in rural areas while female rate was high in capital cities" needs to be re-written. Indeed, female rate in capital cities is low and comparable with rural areas.

Reply: We have revised this sentence (page 2).

Introduction

The literature review is not thorough. Many studies about long-term trend and spatial analysis over small areas have been done (e.g. Taiwan, UK, Australia..), but the authors have not addressed them.

Reply: We have added these literature (pages 3 to 4).

Design

a. It should state clearly whether the SLA and the LGA referred to the residential address or the

suicide location of the suicide deceased.

Reply: SLA/LGA code indicates the place of suicide occurrence and we have noted (page 5).

b. The statistical method is too brief. Authors should say how the poisson regression model is constructed for table 1 to 3. In table 1 and 2, the reference group was not specified in the whole manuscript. Are the RR adjusted by other covariates? How can the readers interpret those risk ratios? Are these variables entered into the model simultaneously or separately?

Reply: We have revised the part of statistical method (page 6). We use the whole population as control group to calculate the ratio for each population subgroups (e.g., by sex and age) and do not use other covariates in this study. RR should be Rate Ratio in Table 1 and we have corrected it through the manuscript. In this study, we have not added other covariates (e.g., socio-economic and environmental factors) except for suicide year. Examining these factors and suicide will be demonstrated in another study of our research.

Result

a. In line 33 of page 6, author should report the proportion of suicide methods so that readers can compare them.

Reply: We have specified the proportion of suicide methods (1st paragraph, page 7).

b. In line 55 of page 6, "between 15 and 34 years" should be changed to "from 15 to 34 years"

Reply: We have changed it (2nd paragraph, page 7).

c. In line 7 of page 7, "The trends of suicide among 15 to 54 years had slight fluctuations during the study period among different sub-groups" is unclear. Please describe the pattern completely.

Reply: We have rewritten this sentence with specification (pages 7 to 8).

d. In line 16-23 of page 7, authors reported that some increase/decrease happened especially in subgroups (males and rural and remote areas). Any interaction term in the regression model has been tested?

Reply: We use the logarithm of population (total or by subgroups) as an offset in the Poisson regression.

e. The maps of hanging and firearm suicide over the study period are very useful to show the temporal pattern. On the contrary, the maps on page 21 aggregated all suicides in the period for each subgroup. The aggregation may hide the temporal changes, which the readers are more interested to know.

Reply: Thank you for your suggestions. We have also examined spatial pattern of suicide by other methods, sex and age group in different 5-year study periods. However, the patterns of each subgroup among different 5-year study periods are similar. Thus we just applied patterns of different 5-year study periods in suicide by firearms and hanging, to highlight the distinctions and keep the concise of the manuscript.

f. In line 20 of page 7, "in between 1996 and 2000" should be changed to "from 1996 to 2000"

Reply: We have changed it (page 8).

g. In line 38 of page 8, change "suicide by hanging kept increasing" to "the suicide rate by hanging has increased in most areas of Australia since 1986"

Reply: We have changed it (page 8).

Discussion

a. In line 30 of page 9, "Usually older results are less likely to seek for psychiatric support." Delete "for".

Reply: We have revised it (3rd paragraph, page 10).

b. The association between increase in use of antidepressants and reduction in suicide in older adults needs more justification. Did elderly suicide attribute a lot to psychiatric disorder?

Reply: We have added more justifications and replied the question (1st paragraph, page 11).

c. The first two sentences in line 8 of page 10 should be moved to result section.

Reply: We have removed the two sentences to the result section in page 7.

d. In line 17 of page 10, delete "and"

Reply: We have revised it (2nd paragraph, page 11).

e. In line 19 of page 10, "In this study, there were more decreased male suicide rate..." this finding was not mentioned in the result section.

Reply: We have removed this part to result section in pages 7 and 8.

f. The drop of firearm suicide led by the restriction of firearm is interesting, but the authors should describe the history and phenomenon in more details. For instance, how the firearm restriction has changed over the years and how it related to the gradual decrease of suicide. Also, authors suggested the substitution effect by hanging but did not elaborate the mechanism. Authors should described more and cited more previous studies in this mechanism.

Reply: We have revised them in pages 11 to 12.

g. In line 18-32 of page 11, please spell out the suicide rate in these area for better comparison.

Reply: We have removed this part to Results section and indicated the suicide rate (pages 9 to 10).

h. Some conclusions are over-interpreted and out of scope from the results. Such as "local vulnerable population" (line 55 of page 12) and "The effectiveness of antidepressant use..." (line 7 of page 13).

Reply: We have revised them (pages 13 and 14).

i. In line 14 of page 13, "Potential impacts of socioeconomic and environmental factors..." are very vague. Authors need to discuss them in details.

Reply: We have specified these factors (page 14).

We have all read and approved the contents of the final version and have no conflict of interest associated with this manuscript.

VERSION 2 – REVIEW

REVIEWER	Dr Keith Miller Flinders University, Australia
REVIEW RETURNED	30-Jun-2014

GENERAL COMMENTS	<p>15. Just a few minor grammatical issues, eg. Abstract, Results, line 2, "Suicide rates were higher..." Under Strengths and Limitations, 3rd paragraph, "...ecological fallacy is inevitable... and one should be cautious about the results when interpreted..." Introduction: "...a major public health issue globally..." There are a number of occasions when either singular or plural is used inappropriately, definite or indefinite articles ('the' or 'a') should be included. It would be preferable if these were corrected for an English speaking audience.</p> <p>Under Statistical Analyses, "...95% Confidence Interval...", '...in the whole Australian population..."</p> <p>In pdf version, p9 top line: "In this study, there were more decreased male suicide rate..." - this needs to be clarified.</p> <p>p.11, line 28: "...which were resulted from both of other health problems and reluctance of psychiatry support" - this sentence needs to be clarified.</p> <p>p.11, line 44: "...cardiovascular impairment..."</p> <p>p14, line 14: "To keep the consistency of the suicide data reporting system..."</p> <p>p.15, line 48-50, under Funding: "We also thank Professor Richard ? ..."</p>
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	For the benefit of readers, it will be preferable to make the suggested changes. Once the suggested grammatical alterations have been attended to then the manuscript will be ready for publication. The Editor can check that this has occurred.
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VERSION 2 – AUTHOR RESPONSE

In response to Reviewer Dr Keith Miller

15. Just a few minor grammatical issues, eg. Abstract, Results, line 2, "Suicide rates were higher..."
Reply: We have revised it (page 1).

Under Strengths and Limitations, 3rd paragraph, "...ecological fallacy is inevitable... and one should be cautious about the results when interpreted..."

Reply: We have rewritten this sentence and deleted it (3rd paragraph, page 3).

Introduction: "...a major public health issue globally..."

Reply: We have revised it (last paragraph, page 3).

There are a number of occasions when either singular or plural is used inappropriately, definite or indefinite articles ('the' or 'a') should be included. It would be preferable if these were corrected for an English speaking audience.

Reply: We have corrected the errors.

Under Statistical Analyses, "...95% Confidence Interval...", '...in the whole Australian population..."

Reply: We have revised it (2nd paragraph, page 6).

In pdf version, p9 top line: "In this study, there were more decreased male suicide rate..." - this needs to be clarified.

Reply: We have deleted it.

p.11, line 28: "...which were resulted from both of other health problems and reluctance of psychiatry support" - this sentence needs to be clarified.

Reply: We have deleted it.

p.11, line 44: "...cardiovascular impairment..."

Reply: We have revised it (bottom, page 10).

p14, line 14: "To keep the consistency of the suicide data reporting system..."

Reply: We have revised it (2nd paragraph, page 12).

p.15, line 48-50, under Funding: "We also thank Professor Richard ? ..."

Reply: We have deleted this (page 15).

We have all read and approved the contents of the final version and have no conflict of interest associated with this manuscript.