

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Mass media representation of suicide in a high suicide state in India: an epidemiological comparison with suicide deaths in the population
<b>AUTHORS</b>	Armstrong, Gregory; Vijayakumar, Lakshmi; Pirkis, Jane; Jayaseelan, Mala; Cherian, Anish; Soerensen, Jane; Arya, Vikas; Niederkrotenthaler, Thomas

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Lydia Sequeira 1. Institute of Health Policy, Management and Evaluation, University of Toronto 2. Centre for Addiction and Mental Health Toronto, ON, Canada
<b>REVIEW RETURNED</b>	07-May-2019

<b>GENERAL COMMENTS</b>	<p>Thank you for the opportunity to review this article. This is a well written paper, reporting on an important and timely area of research, given the rise in suicide rates worldwide. Given below are a few suggestions for improving your paper:</p> <ol style="list-style-type: none"><li>1. Study objectives: Within the research objectives in your abstract, you can specify “one Indian state” or “within one state in India” instead of India to provide more clarity.</li><li>2. Methods:<ol style="list-style-type: none"><li>i. For those articles that reported on suicide briefly (i.e. &lt;50%), was the reasoning that you did not include these because they did not produce any demographic or identifiable characteristics, or was this mainly due to these articles not having a great impact on the reader?</li><li>ii. You mention the exclusion of suicide bombings or euthanasia. I am wondering if there were any physician-assisted suicide related articles that you came across, and if yes, this could be called out in the exclusions too.</li><li>iii. I am assuming that the all the 1,631 reported death by suicides within the newspapers were specifically from the state of Tamil Nadu - I think it would be helpful to add this detail to the methods to give the reader some more clarity. It was mentioned that the newspapers were highly read in Tamil Nadu, but it was also mentioned that five of these fall within the top circulated newspapers within the country, which could pose some questions about whether these 5 newspapers also report on non-Tamil-Nadu suicides (in which case, there would likely be some expected variation in demographics when comparing to the official suicide records for Tamil Nadu). So some additional clarity here could help!</li></ol></li><li>3. Transgender people have known to have a significantly higher risk of suicide in many other countries, so its interesting that this group was removed out of the analysis (although justifiably so, due to the small number of cases). This could be an added discussion point –</li></ol>
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	<p>is there still a cultural lack of acceptability and therefore under-reporting of these deaths in Tamil Nadu?          (see Virupaksha, H. G., Muralidhar, D., &amp; Ramakrishna, J. (2016). Suicide and suicidal behavior among transgender persons. Indian journal of psychological medicine, 38(6), 505.)</p> <p>4. Limitations: You have defined your limitations well, especially the 2016 - 2015 gap. Was it difficult to collect 2015 news reports? You can also comment on the low overall percentage variation of suicides within Tamil Nadu over the past few years (found in the AD&amp;S) to further strengthen your reasoning behind “unlikely substantial variation”.</p> <p>ii. You mentioned the under-reporting of poisoning deaths. Are “accidental overdoses” a problem in India? If yes, this lack of clarity of outcomes (i.e. suicide or accidental overdose) could also pose some challenges to finding the true number of cases.</p> <p>5. Figure 2: If possible, please centre align the categories (i.e. students, Persons engaged in the farming sector) so that it falls under male, female &amp; total.</p> <p>6. Page 6, line 27 :... to note that (instead of “to that that”)</p>
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<b>REVIEWER</b>	Chris Patterson University of Glasgow, UK
<b>REVIEW RETURNED</b>	10-May-2019

<b>GENERAL COMMENTS</b>	<p>Thank you for the opportunity to review this paper. This research presents compelling evidence about media misrepresentations of suicide in Tamil Nadu that may be important in informing action to improve media representations of suicide in Tamil Nadu or India as a whole.</p> <p>I think this paper is close to being worthy of publication, but I have a few minor suggestions that could improve it. These suggestions are itemised by section below:</p> <p>Title: Arguably the title over-promises by saying "India" instead of "Tamil Nadu" or "an Indian State". Consider whether the title could be modified to be more accurate while not underselling the potent national-level importance of the work.</p> <p>Abstract:</p> <ul style="list-style-type: none"> <li>- I feel that the research design (content analysis) should be named within the Design section of the abstract.</li> <li>- In the Results section, "more lethal methods" seems like an odd description of a method of suicide. Aren't all methods of successfully suicide equally lethal? I suggest rephrasing this.</li> </ul> <p>Background:</p> <p>The introduction does a good job of providing background, identifying the gap in the literature, and setting out the research aims. My only issue is, in the final paragraph, it isn't clear what is meant by "The approach of focusing on one state allows us to comprehensively track changes in media reporting in this state". Why would studying the whole country not allow comprehensive tracking of changes in media reporting? Furthermore, while I can see the logic in focusing on the state with the highest suicide rate, I would welcome some reflection on the effect that may have on the national representativeness of the study.</p> <p>Discussion: This section does a comprehensive job of summarising</p>
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	<p>findings and presenting potential explanations for those findings, as well as differences between your findings and those of research from other nations. However, I feel that this section is unusually long, particularly when compared to the relatively concise Results section, and I think readers would welcome more brevity if possible.</p> <p>Limitations (within Discussion):</p> <ul style="list-style-type: none"><li>- Is media coverage of suicide always based on police reports? Might journalists not learn about suicides through other methods? I am not an expert on suicide, journalism or police crime reporting in India so cannot say whether that is correct or not, but perhaps worth raising as potential shortcoming?</li><li>- The fact that the crime statistics sample and media sample do not cover the same time period is unfortunate. It is likely that you are correct, in that crime statistics do not change dramatically from year to year (unless there are changes in crime recording policies), but it would be reassuring if you supported that argument by establishing a lack of substantial differences between the 2015 statistics and earlier years. I don't think an in-depth comparison of year-to-year statistics is necessary, but readers may be reassured if you state that you have examined those historical statistics to establish the lack of substantial year-to-year variation.</li><li>- Regarding occupational categories, it would be useful if you could include one or two lines identifying any potentially-important occupational categories that you were not able to meaningfully record.</li><li>- The limitations section should include discussions of the positives and negatives of focusing on one state rather than the whole of India.</li></ul> <p>Conclusion: Within the context of the paper the conclusion is fine, but for the benefit of readers who skip directly to the conclusion it would be helpful if the conclusion was more clear, spelling out things like the state that the research relates to, what was measured etc.</p> <p>FIGURE 1: This figure illustrates a comparison of reported/actual female suicides, but I feel that the title of the chart could be more clear so that it can be easily interpreted by those who have not yet read the paper. Similarly, the asterisks on some of the numbers in the chart identifying significant differences aren't very clearly described. Finally, I think it is standard to present discontinuous age group data (rather than continuous age data) as a bar chart, rather than a line graph.</p> <p>Thanks again for the opportunity to read and review this important paper.</p>
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### VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Thank you for the opportunity to review this article. This is a well written paper, reporting on an important and timely area of research

Thank you.

Given below are a few suggestions for improving your paper:

1. Study objectives: Within the research objectives in your abstract, you can specify “one Indian state” or “within one state in India” instead of India to provide more clarity.

We have adjusted both the study aim and the title of the manuscript to refer to undertaking this research in a ‘high suicide state in India’.

2. For those articles that reported on suicide briefly (i.e. <50%), was the reasoning that you did not include these because they did not produce any demographic or identifiable characteristics, or was this mainly due to these articles not having a great impact on the reader?

The rationale was to exclude articles largely focused on another topic that only reported on suicide briefly or in passing. Thus, both of the statements you mention would be true, that they would not produce any demographic or identifiable characteristics and they would be less likely to have a great impact on the reader. The latter was the main reason, as the study was initially designed to assess the quality of reporting against WHO media guidelines.

3. You mention the exclusion of suicide bombings or euthanasia. I am wondering if there were any physician-assisted suicide related articles that you came across, and if yes, this could be called out in the exclusions too.

We did not identify any such articles, but we will monitor this in our upcoming work.

4. I am assuming that the all the 1,631 reported death by suicides within the newspapers were specifically from the state of Tamil Nadu - I think it would be helpful to add this detail to the methods to give the reader some more clarity. It was mentioned that the newspapers were highly read in Tamil Nadu, but it was also mentioned that five of these fall within the top circulated newspapers within the country, which could pose some questions about whether these 5 newspapers also report on non-Tamil-Nadu suicides (in which case, there would likely be some expected variation in demographics when comparing to the official suicide records for Tamil Nadu). So some additional clarity here could help!

Although some of the newspapers (e.g. Times of India) are in the top 5 most circulated in the country, each different city edition carries different local content, and the suicides are predominantly (but not always) from within Tamil Nadu. We have now added the following information to the methods.

‘the vast majority of these suicides were of people from Tamil Nadu (84.0%, n=1371), while the remainder were largely of people from elsewhere in India (14.5%, n=236) or from other countries (1.1%, n=18), with location unable to be determined in 0.4% (n=6) of the media reports. We elected to retain all suicides in our primary analyses, regardless of the location of the deceased, as these reports will still influence the public’s impressions of who is affected by suicide; secondary analyses will exclusively examine suicides of people from Tamil Nadu.’

We have also subsequently undertaken secondary analyses, only including media reports of suicides of people from Tamil Nadu. The results are absolutely consistent with our primary analyses, and we have included these as a supplementary file. We have added the following text to the results section:

‘Secondary analyses were undertaken, only including media reports of suicides of people from Tamil Nadu, to examine if our findings were confounded by media reports of suicides of people from outside Tamil Nadu. The results are consistent with the primary analyses (See Supplementary File 1).’

5. Transgender people have known to have a significantly higher risk of suicide in many other countries, so its interesting that this group was removed out of the analysis (although justifiably so, due to the small number of cases). This could be an added discussion point – is there still a cultural lack of acceptability and therefore under-reporting of these deaths in Tamil Nadu?

(see Virupaksha, H. G., Muralidhar, D., & Ramakrishna, J. (2016). Suicide and suicidal behavior among transgender persons. *Indian journal of psychological medicine*, 38(6), 505.)

This is a really interesting point and something we are very interested in, but we don't think we have enough data to be able to comment more on this. The other peer reviewer is also stating that we already have a rather lengthy discussion section and is asking for brevity. For both of these reasons we are reluctant to make more of this issue in the discussion section.

4. Limitations: You have defined your limitations well, especially the 2016 - 2015 gap. Was it difficult to collect 2015 news reports? You can also comment on the low overall percentage variation of suicides within Tamil Nadu over the past few years (found in the AD&S) to further strengthen your reasoning behind “unlikely substantial variation”.

We collected the data prospectively, handsearching printed editions of the newspapers in 2016. We didn't anticipate that the NCRB would delay releasing the 2016 suicide statistics for so long. We have waited for some time before attempting to publish this paper, waiting for the updated data. The delay is creating a sense of unease about the reliability of the more recent NCRB suicide data, and so we have proceeded with publication while accepting the inherent limitation.

We have reviewed the previous NCRB data over the preceding two years (i.e. 2013 and 2014) and we observed no substantial differences. We have added the following text to the limitations section.

'While these are two different time periods, it is the most recently available suicide statistics for India and we expect a high degree of stability between the 2015 and 2016 data. We compared the official suicide statistics for 2015 against that for 2014 and 2013 and we observed a high degree of stability in the data; for example, the suicide rate for Tamil Nadu was 22.8 in 2015 and 23.4 in 2014, and 29.7% and 30.7% of suicide deaths were people aged 18-29 in 2015 and 2013 respectively.'

5. Limitations: You mentioned the under-reporting of poisoning deaths. Are “accidental overdoses” a problem in India? If yes, this lack of clarity of outcomes (i.e. suicide or accidental overdose) could also pose some challenges to finding the true number of cases.

Yes, it is possible that it is hard to ascertain the true population rate of poisoning suicide deaths. That would be a higher-level issue about the quality of the official statistics. However, for our paper, we are comparing the official statistics against the newspaper reports, and both are largely based on the same source, police reports. Thus, we don't expect this is an issue for our analyses.

6. Figure 2: If possible, please centre align the categories (i.e. students, Persons engaged in the farming sector) so that it falls under male, female & total.

In our Word version that is what we had already done, but it seems to change when converted into PDF format through the BMJ Open submission process. We will ensure this is resolved appropriately if the paper moves to the pre-publication formatting phase.

7. Page 6, line 27 :... to note that (instead of “to that that”)

Thank you for spotting this error. We have now fixed this.

Reviewer: 2

Thank you for the opportunity to review this paper. This research presents compelling evidence about media misrepresentations of suicide in Tamil Nadu that may be important in informing action to improve media representations of suicide in Tamil Nadu or India as a whole.

Thank you

I think this paper is close to being worthy of publication, but I have a few minor suggestions that could improve it. These suggestions are itemised by section below:

1. Title: Arguably the title over-promises by saying "India" instead of "Tamil Nadu" or "an Indian State". Consider whether the title could be modified to be more accurate while not underselling the potent national-level importance of the work.

We have adjusted both the study aim and the title of the manuscript to refer to undertaking this research in a 'high suicide state in India'.

2. Abstract: I feel that the research design (content analysis) should be named within the Design section of the abstract. In the Results section, "more lethal methods" seems like an odd description of a method of suicide. Aren't all methods of successfully suicide equally lethal? I suggest rephrasing this.

We have amended the abstract to mention that this is a content analysis study. We have also changed the wording regarding 'more lethal methods', which we have replaced with 'methods with a higher case-fatality rate'.

3. Background: The introduction does a good job of providing background, identifying the gap in the literature, and setting out the research aims. My only issue is, in the final paragraph, it isn't clear what is meant by "The approach of focusing on one state allows us to comprehensively track changes in media reporting in this state". Why would studying the whole country not allow comprehensive tracking of changes in media reporting? Furthermore, while I can see the logic in focusing on the state with the highest suicide rate, I would welcome some reflection on the effect that may have on the national representativeness of the study.

The main issue in undertaking a national study was the large number of languages across India (20+ major languages and many other languages on top of that). To run a study with different teams of people speaking all the different languages would have greater resources than we had. The following text has been added to the background section.

'Due to the large number of languages across India and the prohibitive level of resources required to fund a study with a broad national spread of newspapers published in a range of languages, we elected to undertake a comprehensive study of newspaper reporting in one state.'

It is a challenge to undertake "national" studies in India. India has an enormous and diverse population, with several states home to populations large enough to make it on their own into the top 20 most populous countries in the world. We are currently undertaking further research in the north of

India, where we will produce additional data on reporting practices there. We have also now acknowledged the limitation that our study looked at reporting in only one state.

4. Discussion: This section does a comprehensive job of summarising findings and presenting potential explanations for those findings, as well as differences between your findings and those of research from other nations. However, I feel that this section is unusually long, particularly when compared to the relatively concise Results section, and I think readers would welcome more brevity if possible.

Unfortunately, because many readers will be unfamiliar with the Indian context, it has perhaps taken more words than it otherwise might to discuss the results. We have also taken the opportunity to add a section on the suicide prevention implications of the findings, which we would be reluctant to remove. We note that the manuscript is still well under the 4000 word limit set by BMJ Open, but we are certainly happy to follow any further editorial advice regarding the length of the discussion section.

Limitations (within Discussion):

5. Is media coverage of suicide always based on police reports? Might journalists not learn about suicides through other methods? I am not an expert on suicide, journalism or police crime reporting in India so cannot say whether that is correct or not, but perhaps worth raising as potential shortcoming?

Journalists may learn of a suicide from other sources, but they will almost always need to verify this first with the police as the official source of information. Typically, journalists in India are part of group text message or Whatsapp groups, with unofficial and official police information flowing to them about suicide events; we are currently preparing a paper regarding this based on 27 qualitative interviews with journalists in north and south India. Thus, we see no reason to change the current text.

6. The fact that the crime statistics sample and media sample do not cover the same time period is unfortunate. It is likely that you are correct, in that crime statistics do not change dramatically from year to year (unless there are changes in crime recording policies), but it would be reassuring if you supported that argument by establishing a lack of substantial differences between the 2015 statistics and earlier years. I don't think an in-depth comparison of year-to-year statistics is necessary, but readers may be reassured if you state that you have examined those historical statistics to establish the lack of substantial year-to-year variation.

We have reviewed the previous NCRB data over the preceding two years (i.e. 2013 and 2014) and we observed no substantial differences. We have added the following text to the limitations section.

'While these are two different time periods, it is the most recently available suicide statistics for India and we expect a high degree of stability between the 2015 and 2016 data. We compared the official suicide statistics for 2015 against that for 2014 and 2013 and we observed a high degree of stability in the data; for example, the suicide rate for Tamil Nadu was 22.8 in 2015 and 23.4 in 2014, and 29.7% and 30.7% of suicide deaths were people aged 18-29 in 2015 and 2013 respectively.'

7. Regarding occupational categories, it would be useful if you could include one or two lines identifying any potentially-important occupational categories that you were not able to meaningfully record.

We have added text to the limitations to indicate that we would have liked to assess other occupational categories, such as housewives:

'There are several additional occupational categories that would have been interesting to examine; for example, "housewives" comprised 53% of female suicide deaths in the 2015 official suicide statistics, yet this occupational information or the specific term was rarely communicated in media reports.'

8. The limitations section should include discussions of the positives and negatives of focusing on one state rather than the whole of India.

Further information was provided in the background section of the paper to explain the approach of focusing on one state, as per your earlier suggestion, including the advantages of this approach. We have also now mentioned in the limitations that the study was limited by focusing on one state.

9. 'Conclusion: Within the context of the paper the conclusion is fine, but for the benefit of readers who skip directly to the conclusion it would be helpful if the conclusion was more clear, spelling out things like the state that the research relates to, what was measured etc.'

We have now added this information to the conclusion.

10. 'FIGURE 1: This figure illustrates a comparison of reported/actual female suicides, but I feel that the title of the chart could be more clear so that it can be easily interpreted by those who have not yet read the paper. Similarly, the asterisks on some of the numbers in the chart identifying significant differences aren't very clearly described. Finally, I think it is standard to present discontinuous age group data (rather than continuous age data) as a bar chart, rather than a line graph.'

As suggested, we have now converted this Figure into a bar chart. We believe the indications of statistical significance are clear and appropriate, with the explanatory note under the table. We have workshopped the title for this Figure several times and the version we included is the most accurate description of the data that we have been able to come up with.

### VERSION 2 – REVIEW

<b>REVIEWER</b>	Lydia Sequeira Institute of Health Policy, Management and Evaluation, University of Toronto, Toronto, ON Centre for Addiction and Mental Health, Toronto, ON
<b>REVIEW RETURNED</b>	05-Jun-2019

<b>GENERAL COMMENTS</b>	Thank you for detailing all your justifications and adding in the secondary analysis of Tamil-Nadu only suicides. Given this addition, along with other edits, I don't see the need for any other revisions required to this manuscript.
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<b>REVIEWER</b>	Chris Patterson University of Glasgow, UK
<b>REVIEW RETURNED</b>	11-Jun-2019

<b>GENERAL COMMENTS</b>	Thank you for addressing my comments and questions. I am largely satisfied by the explanations given and the changes made. While not all of my original concerns about Figure 1 have been addressed, I feel that changing the format of the figure from a line graph to a bar chart has improved the figure sufficiently that I am now satisfied with it. While the discussion has not been shortened as suggested, I am happy with the rationale assuming the Editor has no objections.
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	<p>The one outstanding issue I have is that the repercussions of focusing on one state (and that state having a disproportionately high suicide rate) have been explored sufficiently in the paper. I think this is something readers are likely to question, and therefore something that should be engaged with overtly. While the relevant additions in the revised paper do help by convincingly justifying the decision from a practical standpoint, the paper does not adequately discuss the positives and negatives of doing so. The limitations section states that examining other regions would have been desirable, but does not comment on why. I suggest that either the background or limitations section should contain brief but substantive reflection on how focusing on one state (and also that state having disproportionately high incidence of suicide) may affect the generalisability of the research.</p> <p>Once the minor issue described above has been addressed, I recommend the publication of this paper.</p>
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### VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Thank you for detailing all your justifications and adding in the secondary analysis of Tamil-Nadu only suicides. Given this addition, along with other edits, I don't see the need for any other revisions required to this manuscript.

Thank you.

Reviewer: 2

Thank you for addressing my comments and questions. I am largely satisfied by the explanations given and the changes made. While not all of my original concerns about Figure 1 have been addressed, I feel that changing the format of the figure from a line graph to a bar chart has improved the figure sufficiently that I am now satisfied with it. While the discussion has not been shortened as suggested, I am happy with the rationale assuming the Editor has no objections.

Thank you

The one outstanding issue I have is that the repercussions of focusing on one state (and that state having a disproportionately high suicide rate) have been explored sufficiently in the paper. I think this is something readers are likely to question, and therefore something that should be engaged with overtly. While the relevant additions in the revised paper do help by convincingly justifying the decision from a practical standpoint, the paper does not adequately discuss the positives and negatives of doing so. The limitations section states that examining other regions would have been desirable, but does not comment on why. I suggest that either the background or limitations section should contain brief but substantive reflection on how focusing on one state (and also that state having disproportionately high incidence of suicide) may affect the generalisability of the research.

In response to this suggestion, we have:

- 1) Added this limitation to the 'strengths and limitations of the study' section of the article summary

2) Added additional text to the limitations section explaining that 'we only examined newspaper reports in one southern state of India where the suicide rate is substantially higher than the national average. It is possible that our findings do not reflect newspaper coverage of suicide in other states of India, particularly those with a lower suicide rate where mass media reports of suicides may be less common. Future research should examine reports in other states as well as reports in other forms of mass and social media.'

Once the minor issue described above has been addressed, I recommend the publication of this paper. Thank you for the opportunity to review this paper. This research presents compelling evidence about media misrepresentations of suicide in Tamil Nadu that may be important in informing action to improve media representations of suicide in Tamil Nadu or India as a whole.