

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	OVERALL, GENDER AND SOCIAL INEQUALITIES IN SUICIDE MORTALITY IN IRAN, 2006-2010: A TIME TREND PROVINCE-LEVEL STUDY
AUTHORS	Kiadaliri, Aliasghar; Saadat, Soheil; Shahnavaizi, Hossein; Haghparast-Bidgoli, Hassan

VERSION 1 - REVIEW

REVIEWER	Yeon-pyo Hong Dept of Preventive Medicine Chung-Ang University, College of Medicine Republic of Korea
REVIEW RETURNED	01-Jun-2014

GENERAL COMMENTS	Method to describe the inequality is nice, but rationale of Iran's suicide to publish in the BMJ need to reinforce.
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REVIEWER	Caine, Eric Injury Control Research Center for Suicide Prevention Dept of Psychiatry University of Rochester Medical Center Rochester, NY, USA
REVIEW RETURNED	16-Jun-2014

GENERAL COMMENTS	<p>This paper offers new and useful data. It needs some work, as noted below. It does add to what is known about regional and socio-economic variations, and suicide.</p> <p>Careful review by someone with greater statistical sophistication than me would be worthwhile!</p> <p>This is an interesting paper but is jargon filled; e.g., the use of "pure," and the econometric modeling. It will stump the general reader, and therefore will not have the potential impact that it otherwise might.</p> <p>As well, it would be best served by using a map showing the variation of economic variation along with suicide variation. This would make the findings more apparent and interesting.</p>
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REVIEWER	Francis Mitrou Telethon Kids Institute, The University of Western Australia
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GENERAL COMMENTS

This paper is generally sound on all levels, with a clear discussion of methods, logical conclusions, and a pleasingly frank limitations section. I have only two minor points to make.

One concerns a possible typographical error in the final sentence of the first paragraph in Methods. The sentence should state "HDI" (for Human Development Index) rather than just "HD". The "I" appears to be missing.

My second point relates to the statement in the Discussion (pg8) that a recent study has reported divorce as one of the risk factors for completed suicides [1]. The authors make this point in support of their assertion that divorce rates are higher in Iranian provinces with low socio-economic status, as these provinces also have higher than average suicide rates. It may indeed be true that divorce is a risk factor for both attempted and completed suicide, but the paper referenced in support of this relationship appears to offer a weak source of evidence for this notion, with apparently unadjusted odds ratios and a high proportion of missing data making clear conclusions around the relationship between divorce and suicide difficult to draw [1].

The paper in question showed just 71 total cases of attempted (54 cases) and completed (17 cases) suicide by divorced persons, compared with 16,499 total cases for single persons (15,216 attempted vs 1,283 completed) and 12,165 cases for married persons (10,793 attempted vs 1,372 completed). Of greatest note is that 24,365 total cases (23,154 attempted vs 1,211 completed) have missing data on marital status.

Given the enormous level of missing data and low recorded cases for divorced persons, it is quite possible that the conclusion drawn in that paper about suicide fatality rates amongst divorced persons is wrong. Using that evidence to support a statement about suicide rates amongst divorced persons in this current paper under review is therefore problematic. I'm reluctant to see the existing reference [1] used in support of the notion of a relationship between divorce and suicide in Iran. I suggest the authors re-think their approach here. Perhaps they could suggest that divorce may be a contributing factor, but that there is limited empirical data available to confirm this.

[1]. Saberi-Zafaghbandi MB, Hajebi A, Eskandarieh S, Ahmadzad-Asl M: Epidemiology of suicide and attempted suicide derived from the health system database in the Islamic Republic of Iran: 2001-2007. Eastern Mediterranean health journal 2012, 18:836-841.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1

1. Method to describe the inequality is nice, but rationale of Iran's suicide to publish in the BMJ need to reinforce.

Reply: Although this study conducted in the context of Iran, however, the findings may also be applicable to other middle-income countries, in particular countries in the Middle East region, which share similar culture.

Moreover, we believe that, in terms of methodology, our analysis present a good example for employing a triangulation of different methods for evaluating inequalities in suicide mortalities.

2. What is the author's specific object to treat gender inequality?

Reply: In line with the reviewer's comment we have now added few sentences in the discussion section regarding implication of gender inequality or gender analysis.

Reviewer 2

1. This is an interesting paper but is jargon filled; e.g., the use of "pure," and the econometric modeling. It will stump the general reader, and therefore will not have the potential impact that it otherwise might.

Reply: We agree with reviewer that the use of "pure inequality" is less common among non-economists. Therefore, we replaced it with "overall inequality" which is more common and gave a reference for readers more interested [Wagstaff A, van Doorslaer E: Overall versus socioeconomic health inequality: a measurement framework and two empirical illustrations. Health Econ 2004, 13(3):297-301]. Regarding econometric method, we tried to avoid presenting mathematical equations and explain it in words to help a general reader. In addition, we cited several references for more interested reader with more econometrics knowledge.

2. As well, it would be best served by using a map showing the variation of economic variation along with suicide variation. This would make the findings more apparent and interesting.

Reply: We have added a map into the manuscript to show the geographic variation of suicide mortality across the provinces. This is especially useful as we mentioned in the text that the western provinces of Iran had higher rate of suicide mortality and a map is a good tool to show this. However, regarding variation of suicide mortality along with economic changes, we have already showed these variations using scatter plots of suicide mortality and HDI. We believe that these scatter plots are easier to understand by a reader than a map of two variables.

Reviewer 3

1. One concerns a possible typographical error in the final sentence of the first paragraph in Methods. The sentence should state "HDI" (for Human Development Index) rather than just "HD". The "I" appears to be missing.

Reply: We thanks the reviewer for bringing this to our attention. We have now corrected this.

2. My second point relates to the statement in the Discussion (pg8) that a recent study has reported divorce as one of the risk factors for completed suicides [1]. The authors make this point in support of their assertion that divorce rates are higher in Iranian provinces with low socio-economic status, as these provinces also have higher than average suicide rates. It may indeed be true that divorce is a risk factor for both attempted and completed suicide, but the paper referenced in support of this relationship appears to offer a weak source of evidence for this notion, with apparently unadjusted odds ratios and a high proportion of missing data making clear conclusions around the relationship between divorce and suicide difficult to draw [1]. The paper in question showed just 71 total cases of attempted (54 cases) and completed (17 cases) suicide by divorced persons, compared with 16,499 total cases for single persons (15,216 attempted vs 1,283 completed) and 12,165 cases for married persons (10,793 attempted vs 1,372 completed). Of greatest note is that 24,365 total cases (23,154 attempted vs 1,211 completed) have missing data on marital status. Given the enormous level of missing data and low recorded cases for divorced persons, it is quite possible that the conclusion drawn in that paper about suicide fatality rates amongst divorced persons is wrong. Using that

evidence to support a statement about suicide rates amongst divorced persons in this current paper under review is therefore problematic. I'm reluctant to see the existing reference [1] used in support of the notion of a relationship between divorce and suicide in Iran. I suggest the authors re-think their approach here. Perhaps they could suggest that divorce may be a contributing factor, but that there is limited empirical data available to confirm this.

Reply: We agree with the reviewer's comment and we have modified the text accordingly and cited other relevant papers instead.

VERSION 2 – REVIEW

REVIEWER	Francis Mitrou Telethon Kids Institute The University of Western Australia Australia
REVIEW RETURNED	28-Jul-2014

GENERAL COMMENTS	My previous comments have been addressed adequately.
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