

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Coexisting service-related factors preceding suicide: a network analysis
<b>AUTHORS</b>	Rex, Malin; Brezicka, Thomas; Carlström, Eric; Waern, Margda; Ali, Lilas

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Simon, Gregory Kaiser Permanente Washington Health Research Institute
<b>REVIEW RETURNED</b>	07-May-2021

<b>GENERAL COMMENTS</b>	<p>General comment: The topic is certainly of interest to a broad clinical audience. The overall approach is novel, and the findings make a unique contribution. This version of the manuscript lacks necessary detail regarding key aspects of the source data. It is also essential to acknowledge that these data were “filtered” through the expectations and beliefs of investigators generating original reports.</p> <p>Specific comments: Many more details are needed regarding process and content for the RCA reports used as source data. Specifically: Did these include all suicide deaths in specific health service areas or only suicide deaths among those receiving care? Who conducted these analyses or investigations? What data were available for these investigations? Did original investigators have access to all records from all service settings? Was some structure or standard process followed? What was the form/format of resulting reports? What criteria or time periods were used to define being “in contact” with services (page 6) or service setting (Table 1). How was “primary diagnosis” (Table 1) classified? Was this based on recorded encounter diagnoses or the opinion of post-mortem investigators? How were conflicting diagnoses reconciled? What hierarchy or prioritization was used to determine “primary” diagnosis? Does “missing data” regarding diagnosis and service setting indicate that records were not available or that no diagnoses were given and no health services were used? Use of the term “adverse event” (e.g. top of page 8) will be confusing. Most readers will consider suicide attempt or suicide death to be an adverse event. The authors should find some other term to describe care gaps or possible deficiencies identified by these investigations. The discussion should emphasize that these analyses depended on reports generated by investigators rather than original source data (e.g. medical records or interviews). Consequently, findings</p>
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	may be influenced by the expectations and preconceptions of those investigators. This is an important limitation
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<b>REVIEWER</b>	Williams, Matt N Massey University
<b>REVIEW RETURNED</b>	22-Aug-2021

<b>GENERAL COMMENTS</b>	<p>1</p> <p>Review of “Coexisting service related factors preceding suicide: a network analysis”</p> <p>Reviewer: Matt Williams</p> <p>Dear editor and authors,</p> <p>Thank you for the opportunity to review this paper, which describes a network analysis of “root cause analysis” data collected for 217 suicides in Sweden. The topic of the paper (suicide) is obviously a crucially important one. The paper is concisely written, the data source is a valuable one, and the authors have conducted a very painstaking analysis. I do nevertheless have some critical points to raise.</p> <p><b>Abstract</b> I won't say too much about the abstract now because obviously it'd need to be revised pending modifications to the main sections, but one suggestion I have is not to use undefined abbreviations (RCA, NITHA) in abstracts.</p> <p><b>Background</b> The background/introduction is very concise. This is something I appreciate as a time-strapped reviewer! However, I did feel like it lacked a clear description of a gap in knowledge that needed filling. The authors say:</p> <p>“This suggests a need for patient safety research that acknowledges the complexity of the suicidal process by elucidating interactions among various factors. In the current study, we used network analysis to map and analyse interdependencies among adverse events, root causes, and recommended actions in post-suicide RCAs submitted to a national database.”</p> <p>This is a description of an aim, but I think it could be a lot clearer. Currently, beyond the description of the data source and method, all this aim boils down to is an intent to report some relationships (“map and analyse interdependencies”). That tells the reader little about what this study is really intended to find out. It's important to understand that we can't just feed data into a network analysis and expect statistical software to output meaningful insights; we need to have a clear idea of what the network analysis is for. I.e., are you using network analysis to generate hypotheses about causal effects of some root causes on others, which you can test later? To generate or test an explanation for an observed set of zero-order correlations? Something else? A clearer description of aims (and a justification for why those aims need to be sought) would have</p>
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been helpful here. This is perhaps the most important point in my review.

Another thing which I think might have helped with the background section (and the article generally) is to engage more with literature outside of medical journals. There is a rich contemporary literature on network analysis techniques in psychology (e.g., Borsboom et al., 2021; Epskamp, Borsboom, et al., 2018; Epskamp, van Borkulo, et al., 2018), and drawing on this literature might have helped the authors to identify (and fulfil) aims for this study. The authors do draw on some methodological resources about social network analysis, but given that the data in this paper do not pertain to social networks, I suspect the authors may find that the literature on psychological networks contains more relevant advice.

I know that the journal has a low word limit, but I was mildly surprised to see little coverage of previous papers which have applied network analysis to suicide risk factors (a non-exhaustive list of examples: De Beurs et al., 2019; Graziano et al., 2021; Rath et al., 2019; Shiratori et al., 2014). Reviewing such papers might have helped the authors to identify what gaps in this existing knowledge could be filled by using the “root cause event” data.

Relatedly, I would gently suggest the authors don't claim “To our knowledge, this is the first network study based on post-suicide audits”. Shiratori et al. (2014) arguably meets this definition (albeit they don't use the word “audit”), and it's very possible there are other similar studies too. Claiming to be the first is always risky, unless you've reviewed previous literature very systematically.

#### Methods

The method section provides a thoughtful step-by-step description of data processing and analysis. I nevertheless felt like there needed to be a more comprehensive description of the nature of the raw data itself. The data is described as being “root cause” data, and I appreciate that label may come from the NITHA database, but the reality is that a post-suicide audit can only generate the most tentative inferences about the causes of suicides (it's not experimental data, after all!) Furthermore, the information in the data does not seem to attempt to identify “root causes” (or not in the sense that this term would usually be used), but rather seems to comprise attempts to identify problems with treatment or client management(?)

I also think the manuscript needs to provide a clearer description of the differences between “adverse events”, “root causes”, and “actions”. And then in a more concrete sense, what does this data look like? Is each report a spreadsheet or list of specific points? Or a narrative report? Something else? Overall there needs to be more description of the data source so that the reader can adequately grasp what this data comprises and what its limitations are.

In terms of the analysis of data into “themes”, the authors say that a data collection tool was developed – that's excellent, but what was this tool? An algorithm? A procedure for humans to follow? And where can the reader see it? Please use linked supplementary materials if necessary.

Again, I understand the journal has a tight limit for words and number of tables etc., but the fact that the reader has to refer to supplementary material to get a description of the themes (e.g., their definitions) isn't ideal. If the reader doesn't know what the themes/nodes are, the network analysis is impossible to interpret, so if at all possible this should really go in the main text. Examples of each theme from the raw data might have been helpful also.

Regarding the network analysis, the authors use a rule of  $p \leq .05$  for detecting edges. This isn't completely unreasonable, but it's just one of many approaches to regularisation and parameter selection for a network analysis, from not regularising at all (Williams et al., 2019)[1], to informative priors in a Bayesian approach (Williams, 2021), to the lasso (Epskamp & Fried, 2016), to step-up search algorithms (Epskamp, 2020). A rationale for this choice would have been useful.

The authors say that Spearman's rho was used for correlations between nodes. This isn't a problem per se, given the description of the thematic analysis I would have assumed the data was binary(?) In that case, a Pearson's coefficient and a Phi coefficient and a Spearman's rho are all equivalent.

The authors say that an adjacency matrix was calculated; this might just be me, but I find this term ambiguous sometimes, because I've sometimes seen it used to refer to a matrix showing whether edges co-occurred. Presumably in this case it is rather a partial correlation network(?) Or perhaps a zero-order correlation network? If it's the latter, that really needs a rationale, since it then means that the edges in the network are estimated without controlling for the other nodes in the network.

The authors combine several measures of centrality to identify "sentinel nodes". This isn't unreasonable, but I would have liked to have this seen this approach guided by contemporary methodological literature on centrality, which has identified limitations of some of these measures (Bringmann et al., 2019).

It's not entirely clear to me why the authors chose to combine the "adverse events", "root causes" and "actions" data in the same network; this could be perfectly sensible, but a rationale would have been helpful.

#### Results

The results section is brief and logically structured, and focused primarily on identifying clusters of related nodes.

Table 3 contains useful information, but it is understandable to the reader only if they understand what the degree and eigenvector scores mean. I'm not sure that these are clearly defined in the manuscript (?), beyond indicating that they are measures of centrality.

In the Detected Cluster section, the authors say "Cluster analysis of the major theme network based on leading eigenvectors yielded four clusters". I think this is potentially a little confusing for readers in the sense that it becomes unclear whether the authors are using network analysis to detect clusters (often called "communities"; see Hevey, 2018) of nodes, or literally using cluster analysis (which is a

separate form of statistical analysis, albeit there may be some connection between the two in this study that I'm not grasping).

#### Discussion

The discussion focuses mainly on describing and interpreting the four clusters of nodes. I struggled a little to grasp what important findings had been produced here. Yes, the study identified some clusters/communities of nodes, but how does this advance our theoretical understanding of the causes of suicide, or our practical understanding of what to do about suicide risk? Just identifying groups of related variables isn't necessarily a substantial contribution to knowledge.

The points about safe interventions made in the top paragraph of page 13 are all sensible, but are not things that follow from the findings of this study in any direct way.

#### Conclusion

I agree that "Applying network analysis to occurrences and adverse events in complex healthcare systems can elucidate patterns of associated factors and contribute to a better understanding of the mechanisms involved." But has this study contributed to a better understanding of the mechanisms involved in suicide? If so, how in specific has it done this?

#### Other sections

I appreciated the inclusion of a clear strengths and limitations section.

The data availability statement says that "Data are available upon reasonable request". I know this is a commonly used phrasing but it leaves ambiguity as to what constitutes "reasonable". I would instead suggest considering the degree to which the data can be adequately de-identified, and then making a very specific plan for how data can be accessed (and under what conditions). I suspect, incidentally, that some aspects of this data could be shared openly online (e.g., nodes/themes and frequencies), whereas other aspects might present greater identifiability risks. Please take the prospect of data sharing as something that require serious consideration; this is something reviewers and readers need to adequately interrogate the evidence in the article (a useful resource here is Meyer, 2018). Relatedly, I strongly suggesting sharing the code/syntax and other tools used to analyse the data openly online (e.g., on osf.io); this material should present no identifiability risks, and it'd be very helpful for me and other readers to understand how specifically the analyses were conducted.

#### My recommendation

I deeply respect the quantity of work that the authors have put into this study, and their engagement with a crucially-important topic. At the same time, at a broad level I think there is a lack of clarity of purpose here – what specifically is this study trying to find out? Then at a more specific level there are several areas where the reporting and analysis could be improved.

I have a degree of hesitancy about which outcome to suggest here, but I'm ultimately going to tick "Major Revisions". I mention hesitancy because I generally think that R&R decisions should mostly be used in cases where it's very plausible that a single revision could result in a decision of accept (or accept with minor

revisions). Multiple rounds of review followed by a decision of reject are awful for authors. Here, the fact that I am suggesting returning to the drawing board and considering what the core aims of this study should be means that the outcome of peer review for a revision is quite hard to predict. As such, I suspect this might be a “high risk” R&R (although obviously the decision depends on the other reviews and the editor!) I would nevertheless be happy to review a revision if one is sought and submitted, and more generally I look forward to any future research conducted by this group.

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Graziano, R. C., Aunon, F. M., LoSavio, S. T., Elbogen, E. B., Beckham, J. C., Brancu, M., Beckham, J. C., Calhoun, P. S., Dedert, E., Elbogen, E. B., Fairbank, J. A., Hurley, R. A., Kilts, J. D., Kimbrel, N. A., Kirby, A., Marx, C. E., McDonald, S. D., Moore, S. D., Morey, R. A., ... Dillon, K. H. (2021). A network analysis of risk factors for suicide in Iraq/Afghanistan-era veterans. *Journal of Psychiatric Research*, 138, 264–271. <https://doi.org/10.1016/j.jpsychires.2021.03.065>

	<p>Hevey, D. (2018). Network analysis: a brief overview and tutorial. <i>Health Psychology and Behavioral Medicine</i>, 6(1), 301–328. <a href="https://doi.org/10.1080/21642850.2018.1521283">https://doi.org/10.1080/21642850.2018.1521283</a></p> <p>Meyer, M. N. (2018). Practical tips for ethical data sharing. <i>Advances in Methods and Practices in Psychological Science</i>, 1(1), 131–144. <a href="https://doi.org/10.1177/2515245917747656">https://doi.org/10.1177/2515245917747656</a></p> <p>Rath, D., de Beurs, D., Hallensleben, N., Spangenberg, L., Glaesmer, H., &amp; Forkmann, T. (2019). Modelling suicide ideation from beep to beep: Application of network analysis to ecological momentary assessment data. <i>Internet Interventions</i>, 18, 100292. <a href="https://doi.org/10.1016/j.invent.2019.100292">https://doi.org/10.1016/j.invent.2019.100292</a></p> <p>Shiratori, Y., Tachikawa, H., Nemoto, K., Endo, G., Aiba, M., Matsui, Y., &amp; Asada, T. (2014). Network analysis for motives in suicide cases: A cross-sectional study. <i>Psychiatry and Clinical Neurosciences</i>, 68(4), 299–307. <a href="https://doi.org/10.1111/pcn.12132">https://doi.org/10.1111/pcn.12132</a></p> <p>Williams, D. R. (2021). Bayesian estimation for Gaussian graphical models: Structure learning, predictability, and network comparisons. <i>Multivariate Behavioral Research</i>, 56(2), 336–352. <a href="https://doi.org/10.1080/00273171.2021.1894412">https://doi.org/10.1080/00273171.2021.1894412</a></p> <p>Williams, D. R., Rhemtulla, M., Wysocki, A. C., &amp; Rast, P. (2019). On nonregularized estimation of psychological networks. <i>Multivariate Behavioral Research</i>, 54(5), 719–750. <a href="https://doi.org/10.1080/00273171.2019.1575716">https://doi.org/10.1080/00273171.2019.1575716</a></p> <p>[1] The citations to “Williams” here are to Donald Williams (no relation). I have not suggested any citations to my own work in this review.</p>
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## VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Dr. Gregory Simon, Kaiser Permanente Washington Health Research Institute

Comments to the Author:

General comment: The topic is certainly of interest to a broad clinical audience. The overall approach is novel, and the findings make a unique contribution. This version of the manuscript lacks necessary detail regarding key aspects of the source data. It is also essential to acknowledge that these data were “filtered” through the expectations and beliefs of investigators generating original reports.

- Thank you for your encouraging comments and for your suggestions for improvement of this paper. We have updated the information about the RCA teams and the RCA process in the main text.

Specific comments:

Many more details are needed regarding process and content for the RCA reports used as source data.

Specifically: Did these include all suicide deaths in specific health service areas or only suicide deaths among those receiving care? Who conducted these analyses or investigations? What data were available for these investigations? Did original investigators have access to all records from all service settings? Was some structure or standard process followed? What was the form/format of resulting reports?

What criteria or time periods were used to define being “in contact” with services (page 6) or service setting (Table 1)

How was “primary diagnosis” (Table 1) classified? Was this based on recorded encounter diagnoses or the opinion of post-mortem investigators? How were conflicting diagnoses reconciled? What hierarchy or prioritization was used to determine “primary” diagnosis?

Does “missing data” regarding diagnosis and service setting indicate that records were not available or that no diagnoses were given and no health services were used?

-Thank you, the information about the data source have been updated under Materials in the Methods section on pages 4-5.

Use of the term “adverse event” (e.g. top of page 8) will be confusing. Most readers will consider suicide attempt or suicide death to be an adverse event. The authors should find some other term to describe care gaps or possible deficiencies identified by these investigations.

-Thank you for your remark. We have replaced the term “adverse event” with “deficiency” throughout the entire paper.

The discussion should emphasize that these analyses depended on reports generated by investigators rather than original source data (e.g. medical records or interviews). Consequently, findings may be influenced by the expectations and preconceptions of those investigators. This is an important limitation.

- Thank you for this important comment. This subject is discussed both in the Methods section (pp. 4-5) and under Strengths and limitations (p.13).

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Reviewer: 2

Dr. Matt N Williams, Massey University

Comments to the Author:

Reviewer: Matt Williams

Dear editor and authors,

Thank you for the opportunity to review this paper, which describes a network analysis of “root cause analysis” data collected for 217 suicides in Sweden. The topic of the paper (suicide) is obviously a crucially important one. The paper is concisely written, the data source is a valuable one, and the authors have conducted a very painstaking analysis. I do nevertheless have some critical points to raise.

- Thank you for kind words, for reading our paper so carefully and for providing us with so many helpful recommendations on what to improve.

Abstract

I won't say too much about the abstract now because obviously it'd need to be revised pending modifications to the main sections, but one suggestion I have is not to use undefined abbreviations (RCA, NITHA) in abstracts.

- Thank you, all undefined abbreviations and acronyms have been removed from the abstract.

Background

The background/introduction is very concise. This is something I appreciate as a time-strapped reviewer! However, I did feel like it lacked a clear description of a gap in knowledge that needed filling. The authors say:

“This suggests a need for patient safety research that acknowledges the complexity of the suicidal process by elucidating interactions among various factors. In the current study, we used network analysis to map and analyse interdependencies among adverse events, root causes, and recommended actions in post-suicide RCAs submitted to a national database.”

This is a description of an aim, but I think it could be a lot clearer. Currently, beyond the description of the data source and method, all this aim boils down to is an intent to report some relationships (“map and analyse interdependencies”). That tells the reader little about what this study is really intended to find out. It's important to understand that we can't just feed data into a network analysis and expect statistical software to output meaningful insights; we need to have a clear idea of what the network



analysis is for. I.e., are you using network analysis to generate hypotheses about causal effects of some root causes on others, which you can test later? To generate or test an explanation for an observed set of zero-order correlations? Something else? A clearer description of aims (and a justification for why those aims need to be sought) would have been helpful here. This is perhaps the most important point in my review.

- Thank you, the aim has now been revised to

“The overall objective of this paper was to analyse service related factors involved in the complex processes that precede suicide in order to identify potential targets for intervention.”

Another thing which I think might have helped with the background section (and the article generally) is to engage more with literature outside of medical journals. There is a rich contemporary literature on network analysis techniques in psychology (e.g., Borsboom et al., 2021; Epskamp, Borsboom, et al., 2018; Epskamp, van Borkulo, et al., 2018), and drawing on this literature might have helped the authors to identify (and fulfil) aims for this study. The authors do draw on some methodological resources about social network analysis, but given that the data in this paper do not pertain to social networks, I suspect the authors may find that the literature on psychological networks contains more relevant advice.

- Thank you for this advice. We have read the suggested references with great care and updated our reference list accordingly.

I know that the journal has a low word limit, but I was mildly surprised to see little coverage of previous papers which have applied network analysis to suicide risk factors (a non-exhaustive list of examples: De Beurs et al., 2019; Graziano et al., 2021; Rath et al., 2019; Shiratori et al., 2014). Reviewing such papers might have helped the authors to identify what gaps in this existing knowledge could be filled by using the “root cause event” data.

-Thank you, after taking part of the references mentioned above, we have rewritten the Previous research section on page 4.

Relatedly, I would gently suggest the authors don't claim “To our knowledge, this is the first network study based on post-suicide audits”. Shiratori et al. (2014) arguably meets this definition (albeit they don't use the word “audit”), and it's very possible there are other similar studies too. Claiming to be the first is always risky, unless you've reviewed previous literature very systematically.

- Thank you for your remarks. We have removed this claim from our paper.

#### Methods

The method section provides a thoughtful step-by-step description of data processing and analysis. I nevertheless felt like there needed to be a more comprehensive description of the nature of the raw data itself. The data is described as being “root cause” data, and I appreciate that label may come from the NITHA database, but the reality is that a post-suicide audit can only generate the most tentative inferences about the causes of suicides (it's not experimental data, after all!) Furthermore, the information in the data does not seem to attempt to identify “root causes” (or not in the sense that this term would usually be used), but rather seems to comprise attempts to identify problems with treatment or client management(?)

I also think the manuscript needs to provide a clearer description of the differences between “adverse events”, “root causes”, and “actions”. And then in a more concrete sense, what does this data look like? Is each report a spreadsheet or list of specific points? Or a narrative report? Something else? Overall there needs to be more description of the data source so that the reader can adequately grasp what this data comprises and what its limitations are.

- Thank you for this comment. Clarifications regarding the RCA process, the workflow of the RCA team and the data source have been made both under the heading Materials, in the Methods section. In terms of the analysis of data into “themes”, the authors say that a data collection tool was developed – that's excellent, but what was this tool? An algorithm? A procedure for humans to follow? And where can the reader see it? Please use linked supplementary materials if necessary. Again, I understand the journal has a tight limit for words and number of tables etc., but the fact that the reader has to refer to supplementary material to get a description of the themes

(e.g., their definitions) isn't ideal. If the reader doesn't know what the themes/nodes are, the network analysis is impossible to interpret, so if at all possible this should really go in the main text. Examples of each theme from the raw data might have been helpful also.

- Thank you, we have included the description of the themes in the main text and added a column with examples to help specify the definitions of the themes.

Regarding the network analysis, the authors use a rule of  $p \leq .05$  for detecting edges. This isn't completely unreasonable, but it's just one of many approaches to regularisation and parameter selection for a network analysis, from not regularising at all (Williams et al., 2019)<sup>1</sup>, to informative priors in a Bayesian approach (Williams, 2021), to the lasso (Epskamp & Fried, 2016), to step-up search algorithms (Epskamp, 2020). A rationale for this choice would have been useful.

The authors say that Spearman's rho was used for correlations between nodes. This isn't a problem per se, given the description of the thematic analysis I would have assumed the data was binary(?) In that case, a Pearson's coefficient and a Phi coefficient and a Spearman's rho are all equivalent.

The authors say that an adjacency matrix was calculated; this might just be me, but I find this term ambiguous sometimes, because I've sometimes seen it used to refer to a matrix showing whether edges co-occurred. Presumably in this case it is rather a partial correlation network(?) Or perhaps a zero-order correlation network? If it's the latter, that really needs a rationale, since it then means that the edges in the network are estimated without controlling for the other nodes in the network.

- Thank you. After considering the downsides of the previous method for estimating the network (pruning of a correlation network), we chose to replace this with a regularisation model. The entire Data analysis section (pages 7-8) has been updated accordingly.

The authors combine several measure of centrality to identify "sentinel nodes". This isn't unreasonable, but I would have liked to have this seen this approach guided by contemporary methodological literature on centrality, which has identified limitations of some of these measures (Bringmann et al., 2019).

- Thank you. We have revised the centrality indices (node strength and edge strength) to include in the paper. The centrality indices betweenness, closeness and expected influence are included as supplementary material.

It's not entirely clear to me why the authors chose to combine the "adverse events", "root causes" and "actions" data in the same network; this could be perfectly sensible, but a rationale would have been helpful.

Thank you. After having updated the aims of the paper, actions were removed from the data analysis. The terminology has also been revised, so that "adverse events" and "root causes" are replaced with the terms "deficiencies" and "contributing factors". We have decided to keep them in the same network for the major themes but separate them for the presentation of the subthemes.

## Results

The results section is brief and logically structured, and focused primarily on identifying clusters of related nodes. Table 3 contains useful information, but it is understandable to the reader only if they understand what the degree and eigenvector scores mean. I'm not sure that these are clearly defined in the manuscript (?), beyond indicating that they are measures of centrality.

- Thank you. A revised description of the centrality indices measured in the paper can be found on page 9.

In the Detected Cluster section, the authors say "Cluster analysis of the major theme network based on leading eigenvectors yielded four clusters". I think this is potentially a little confusing for readers in the sense that it becomes unclear whether the authors are using network analysis to detect clusters (often called "communities"; see Hevey, 2018) of nodes, or literally using cluster analysis (which is a separate form of statistical analysis, albeit there may be some connection between the two in this study that I'm not grasping).

- Thank you, we have replaced the term “cluster” with community throughout the paper. The term community detection is used instead of cluster detection.

#### Discussion

The discussion focuses mainly on describing and interpreting the four clusters of nodes. I struggled a little to grasp what important findings had been produced here. Yes, the study identified some clusters/communities of nodes, but how does this advance our theoretical understanding of the causes of suicide, or our practical understanding of what to do about suicide risk? Just identifying groups of related variables isn't necessarily a substantial contribution to knowledge. The points about safe interventions made in the top paragraph of page 13 are all sensible, but are not things that follow from the findings of this study in any direct way.

- Thank you for this comment. The entire discussion section (pp. 11-12) has been reprocessed and updated.)

#### Conclusion

I agree that “Applying network analysis to occurrences and adverse events in complex healthcare systems can elucidate patterns of associated factors and contribute to a better understanding of the mechanisms involved.” But has this study contributed to a better understanding of the mechanisms involved in suicide? If so, how in specific has it done this?

- Thank you. We have updated our conclusion in order to be more precise in how we think the study contributes to the field of suicidology. The revised phrase is as follows:

“Applying network analysis to investigate patient safety adds to previous research by elucidating patterns of associated factors which may be unclear when only incident rate is considered. The results shows that failed assessments and cancelled treatments during follow-up are both frequent and have a high centrality in relation to the other nodes in the network and could function as a warning sign for exacerbation. Organizational instability, in terms of understaffing, shortages of resources and suboptimal work procedures are also prominent features of the networks. Although comparative studies are needed before any final conclusions can be drawn, focusing on these areas may improve patient safety in suicide prevention.”

#### Other sections

I appreciated the inclusion of a clear strengths and limitations section.

The data availability statement says that “Data are available upon reasonable request”. I know this is a commonly used phrasing but it leaves ambiguity as to what constitutes “reasonable”. I would instead suggest considering the degree to which the data can be adequately de-identified, and then making a very specific plan for how data can be accessed (and under what conditions). I suspect, incidentally, that some aspects of this data could be shared openly online (e.g., nodes/themes and frequencies), whereas other aspects might present greater identifiability risks. Please take the prospect of data sharing as something that require serious consideration; this is something reviewers and readers need to adequately interrogate the evidence in the article (a useful resource here is Meyer, 2018). Relatedly, I strongly suggesting sharing the code/syntax and other tools used to analyse the data openly online (e.g., on osf.io); this material should present no identifiability risks, and it'd be very helpful for me and other readers to understand how specifically the analyses were conducted.

- Thank you for this advice. Please visit our repository at osf.io, to which we have uploaded our final data sets and the R scripts used.

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Once again, we would like to express our gratitude towards both the reviewers for taking of their precious time to read and comment on our paper. After adjusting the manuscript according to their recommendations, the clarity of the paper was much improved.

All authors agree with the changes made and we hope that these corrections fulfill the requirements as per reviewer comments. Please feel free to contact me if there should be any questions or concerns. We are looking forward to your response.

On behalf of all the authors,

With kind regards,

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### VERSION 2 – REVIEW

<b>REVIEWER</b>	Simon, Gregory Kaiser Permanente Washington Health Research Institute
<b>REVIEW RETURNED</b>	07-Dec-2021

<b>GENERAL COMMENTS</b>	<p>Some specific questions regarding data presented in Table 1 have not been addressed:</p> <p>What criteria or time periods were used to define being “in contact” with services (page 6) or service setting (Table 1)          How was “primary diagnosis” (Table 1) classified? Was this based on recorded encounter diagnoses or the opinion of post-mortem investigators?          How were 3 (9) conflicting diagnoses reconciled? What hierarchy or prioritization was used to determine “primary” diagnosis?          Does “missing data” regarding diagnosis and service setting indicate that records were not available or that no diagnoses were given and no health services were used?</p>
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<b>REVIEWER</b>	Williams, Matt N Massey University
<b>REVIEW RETURNED</b>	17-Dec-2021

<b>GENERAL COMMENTS</b>	<p>Review of bmjopen-2021-050953.R1 “Coexisting service related factors preceding suicide: a network analysis”</p> <p>I thank the authors for the opportunity to review this revised manuscript. It seemed to me that the authors did an incredibly thorough job at addressing my points. The revised manuscript is very clear, with uncertainty and limitations appropriately addressed. At this stage I just have a selection of very minor suggestions:</p> <p>1. In the abstract the authors say, “no final conclusions could be drawn”, almost immediately followed by a Conclusion subsection (suggesting conclusions were drawn). The same phrase is also used in the “Network stability” section. Perhaps this juxtaposition could be resolved with an alternative wording, such as “our conclusions should be regarded as involving substantial uncertainty”?</p>
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	<p>2. Page 3, "Network analysis [...] is useful for modelling complex patterns of mutual, reinforcing relationships among positively correlated variables." This is true enough, but network analysis isn't limited to only studying positively correlated variables.</p> <p>3. Page 4, "Previous studies based exclusively on post-suicide RCA material, including systematic reviews, metaanalyses, and observational studies report inadequacies in cooperation,[62, 67-70] accessibility to care,[45, 68] assessments of suicidal risk,[67-71] and follow-up[72] as the main deficiencies in suicide prevention." - Main deficiencies in which healthcare system/country? Or are these the main inadequacies globally?</p> <p>4. Re. Data extraction and processing, I think it would be helpful to share the data coding tool with readers, e.g., as a supplementary file or uploaded on the osf. My apologies if it's already available and I've missed it (the journal's file management system is a bit confusing)</p> <p>5. Page 7, the mention of "499 registered deficiencies and 462 contributing factors" implies a distinction between registered deficiencies and contributing factors. This distinction subsequently becomes quite important in the data analysis. Could you provide a more explicit description of the distinction between these concepts?</p> <p>6. Page 8, "suppressing non-zero connections to exactly zero", you might mean suppressing small connections to exactly zero? If all non-zero connections were suppressed to zero, there would be no edges.</p> <p>7. Page 8-9, "Lack of adequate information was also a common explanatory factor, accounting for 15 percent of all cases": "Accounting for" implies that this was the cause of the suicides, when we don't really know that. Maybe an alternative wording such as "being identified in 15% of cases" would work instead.</p> <p>8. In the Network Stability section, it wouldn't necessarily be clear to a reader without a background in network analysis what these results are or why they matter. Could you perhaps try to fit in a few sentences providing a little more explanation?</p> <p>9. Relatedly, it'd be good to make it clearer what the CS-coefficients relate to. The bootnet package documentation says, "This coefficient denotes the estimated maximum number of cases that can be dropped from the data to retain, with 95% probability, a correlation of at least 0.7 (default) between statistics based on the original network and statistics computed with less cases." (The 0.7 threshold is somewhat arbitrary, as I understand it).</p> <p>10. Page 9, "Nodes with fewer but stronger connections will be considered more central and thus yield a higher value, than nodes with many weak links." This can be the case but is not necessarily so (it's possible for a node with many weak links to have higher centrality than a node with a few strong connections). You could probably just exclude this sentence.</p> <p>11. Page 10, since closeness, betweenness and expected influence aren't reported in the main text it's probably not necessary to define them here (potentially saving words for other revisions).</p> <p>12. Page 9, which are "all the nodes involved"? Is this some subset of nodes, or literally all the nodes in the network?</p> <p>13. Page 12, "the transition to outpatient services seem to be associated with elevated risk levels" – I'm not sure the data here can really show that, since this study didn't study relationships between factors and suicide risk per se.</p> <p>14. In the "Strengths and Limitations" section it might be worth acknowledging that this was a cross-sectional study, limiting the</p>
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	<p>capacity to identify the directions of effects? (Not that this would really have been possible to remedy, but it's still worth mentioning) 15. It's excellent to see that the data and code is available via the OSF. It would probably be worth putting a link to this in the Data Availability Statement, rather than (or in addition to) including it in the reference list.</p> <p>I suspect that each of these suggestions could be resolved very readily, and I congratulate the authors on a very impressive revision.</p> <p>Matt Williams</p>
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## VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Dr. Gregory Simon, Kaiser Permanente Washington Health Research Institute Comments to the Author:

Some specific questions regarding data presented in Table 1 have not been addressed:

What criteria or time periods were used to define being “in contact” with services

(page 6) or service setting (Table 1)

- Thank you for this remark. The definitions for being “in contact with services were collected from the documentation stated in medical records and booking systems available to the RCA teams at the time of the investigation. The text has now been clarified on pages 5-6.

How was “primary diagnosis” (Table 1) classified? Was this based on recorded encounter diagnoses or the opinion of post-mortem investigators? How were conflicting diagnoses reconciled? What hierarchy or prioritization was used to determine “primary” diagnosis?

- Thank you for this remark. The primary diagnosis was based on information from the medical records. However, as we only had access to the final RCA reports, we have not been able to scrutinize how the RCA teams processed the original raw data. The text on pages 5-6 has been updated to clarify the information about the raw data.

Does “missing data” regarding diagnosis and service setting indicate that records were not available or that no diagnoses were given and no health services were used?

- Thank you for this remark. The final reports reflect a variability concerning to what extent data has been included. In some cases, particular facts about the medical condition or specific circumstances have been omitted. Although we do not know the exact background to this, it may have been done to protect the integrity of those deceased. Table 1 (pages 5-6) has now been updated to clarify this.

Reviewer: 2

Dr. Matt N Williams, Massey University Comments to the Author:

Review of bmjopen-2021-050953.R1 "Coexisting service-related factors preceding suicide: a network analysis"

I thank the authors for the opportunity to review this revised manuscript. It seemed to me that the authors did an incredibly thorough job at addressing my points. The revised manuscript is very clear, with uncertainty and limitations appropriately addressed. At this stage I just have a selection of very minor suggestions: 1. In the abstract the authors say, "no final conclusions could be drawn", almost immediately followed by a Conclusion subsection (suggesting conclusions were drawn). The same phrase is also used in the "Network stability" section. Perhaps this juxtaposition could be resolved with an alternative wording, such as "our conclusions should be regarded as involving substantial uncertainty"?

- Thank you for this comment. The text on page 2 and page 11 has been rephrased to resolve this contradiction.

2. Page 3, "Network analysis [...] is useful for modelling complex patterns of mutual, reinforcing relationships among positively correlated variables." This is true enough, but network analysis isn't limited to only studying positively correlated variables. •

- Thank you for this remark. The word "positively" has been excluded from the sentence (page 3).

3. Page 4, "Previous studies based exclusively on post-suicide RCA material, including systematic reviews, metaanalyses, and observational studies report inadequacies in cooperation,[62, 67-70]accessibility to care,[45, 68] assessments of suicidal risk,[67-71] and follow-up[72] as the main deficiencies in suicide prevention." - Main deficiencies in which healthcare system/country? Or are these the main inadequacies globally?

- Thank you for this comment. Page 4, paragraph 2 has been updated with information concerning setting of each previous study.

4. Re. Data extraction and processing, I think it would be helpful to share the data coding tool with readers, e.g., as a supplementary file or uploaded on the osf. My apologies if it's already available and I've missed it (the journal's file management system is a bit confusing)

- Thank you for this comment. Information on how to find our shared data can be found on page 8 and page 16.

5. Page 7, the mention of "499 registered deficiencies and 462 contributing factors" implies a distinction between registered deficiencies and contributing factors. This distinction subsequently becomes quite important in the data analysis. Could you provide a more explicit description of the distinction between these concepts?

- Thank you. The distinction between deficiencies and the contributing factors have been updated on page 6, paragraph 2.

6. Page 8, "suppressing non-zero connections to exactly zero", you might mean suppressing small connections to exactly zero? If all non-zero connections were suppressed to zero, there would be no edges.

- Thank you for this remark. The sentence has been rephrased to "The operator reduces spurious edges by suppressing minimal connections to exactly zero."

(Page 7.)

7. Page 8-9, "Lack of adequate information was also a common explanatory factor, accounting for 15 percent of all cases": "Accounting for" implies that this was the cause of the suicides, when we don't really know that. Maybe an alternative wording such as "being identified in 15% of cases" would work instead.

- Thank you. The sentence has been rephrased to "Lack of adequate information was also a relatively common explanatory factor and was identified in 15 percent of all cases." (Page 11.)

8. In the Network Stability section, it wouldn't necessarily be clear to a reader without a background in network analysis what these results are or why they matter. Could you perhaps try to fit in a few sentences providing a little more explanation?

- Thank you. The text about network stability and CS-coefficients on page 11 has been updated according to the given advice.

9. Relatedly, it'd be good to make it clearer what the CS-coefficients relate to. The bootnet package documentation says, "This coefficient denotes the estimated maximum number of cases that can be dropped from the data to retain, with 95% probability, a correlation of at least 0.7 (default) between statistics based on the original network and statistics computed with less cases." (The 0.7 threshold is somewhat arbitrary, as I understand it).

- Thank you. Please see the answer to question 8.

10. Page 9, "Nodes with fewer but stronger connections will be considered more central and thus yield a higher value, than nodes with many weak links." This can be the case but is not necessarily so (it's possible for a node with many weak links to have higher centrality than a node with a few strong connections). You could probably just exclude this sentence.

- Thank you. This sentence has been deleted.

11. Page 10, since closeness, betweenness and expected influence aren't reported in the main text it's probably not necessary to define them here (potentially saving words for other revisions).

- Thank you. This section has been deleted.

12. Page 9, which are "all the nodes involved"? Is this some subset of nodes, or literally all the nodes in the network?

- Thank you. The text has been updated to "Although the nodes involved

in this subset all scored high in strength..." (Page 12.)

13. Page 12, "the transition to outpatient services seem to be associated with elevated risk levels" – I'm not sure the data here can really show that, since this study didn't study relationships between factors and suicide risk per se.

- Thank you. The sentence has been rephrased as follows:  
"While adverse

events concerning security at the inpatient facilities were rare, the transition to outpatient services was frequently mentioned in the post-mortem audits. Transitions imply a change in primary caregiver and a shift from short-term to long-term treatment goals. A connection to elevated risk levels could be expected, although the direct relationship has not been investigated in this study." (Page 14.)



14. In the “Strengths and Limitations” section it might be worth acknowledging that this was across-sectional study, limiting the capacity to identify the directions of effects? (Not that this would really have been possible to remedy, but it’s still worth mentioning)

• Thank you. This sentence has been included in “Strengths and Limitations” at page 15.

15. It’s excellent to see that the data and code is available via the OSF. It would probably be worth putting a link to this in the Data Availability Statement, rather than (or in addition to) including it in the reference list.

• Thank you. A link to the OSF has been included and can be found at page 16.

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Once again, we would like to express our gratitude towards both the reviewers for taking of their precious time to read and comment on our paper. After adjusting the manuscript according to their recommendations, the clarity of the paper was much improved.

All authors agree with the changes made and we hope that these corrections fulfill the requirements as per reviewer comments. Please feel free to contact me if there should be any questions or concerns. We are looking forward to your response.

On behalf of all the authors,

With kind regards,

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