PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Factors influencing suicide risk assessment clinical practice: protocol for a scoping review
AUTHORS	Sequeira, Lydia; Strudwick, Gillian; Bailey, Sharon; De Luca, Vincenzo; Wiljer, David; Strauss, John

VERSION 1 – REVIEW

REVIEWER	Ron Kessler Harvard Medical School USA
REVIEW RETURNED	04-Oct-2018

GENERAL COMMENTS	I understand the goal, which I think is interesting. But does enough literature exist to justify a systematic literature review? I have never seen any empirical research on this question and I'm not even sure I would know where to begin if I was to design such a study myself. Focusing on your 3 research questions: What do you mean by "affect t the suicide risk assessment process"? In my initial reading it occurred to me that would could mean • The decision whether or not to do an in-depth evaluation (As you know, only ab out half the people who present to an ED with suicidality get an in-depth evaluation eve n at centers where such evaluations are carried out) • Or the depth of the assessment – merely using a superficial screen or more in-depth assessment? • Or are you interested in the predictors of the accuracy of risk evaluations (e.g., studies comparing prediction models with clinical judgments ask clinicians to make judgments and then examine prediction accuracy, which is typically 0. Are you interested in predictors of individual differences in prediction accuracy)? But as I continued reading and got to the section of the paper on (5) Collating, summarizing, … I saw that you are actually interested in subtle issues. You say "The first will include a breakdown of the non-patient specific factors affecting the suicide risk assessment process, while the second will report on results of the types of cognitive biases that emerge within the suicide risk assessment process." But how in the world do you plan to do this? Does published research exist on these questions? I would hope that a preliminary look was taken to make sure there is a rich literature of this sort, or else your review is doomed from the beginning. But if such a literature exists, I'm not sure why I have never seen any such studies. I think you need to address this point explicitly in this paper. Do you have reason to believe that such papers exist based on a preliminary look? The 3rd question, In comparison, strikes me as easy to address. I have seen at least one report from the UK on such a program, although my recollection is that it was not very impactful.

REVIEWER	Lynne Gilmour NMAPH-RU, University of Stirling Scotland. UK
REVIEW RETURNED	16-Oct-2018

GENERAL COMMENTS	This is an important area of research, a timely and ambitious review. The authors may wish to consider the following points: 1) The abstract does not reflect the level of analysis that the review authors plan to conduct as is indicated in their methods section. Whether this level of analysis is appropriate for a scoping review is also potentially questionable. 2) The review authors do not provide a clear justification for why they have decided to do a scoping review as opposed to a systematic review. A systematic review would support their desire to analyse their findings. The Joanna Briggs Institute guidelines for scoping reviews may be helpful to the reviewers. 3) The relevance of the discussion around algorithms in the introduction is not clear since they are not used to inform clinical judgement. 4) There is a lack of reference to the large evidence base that shows that the use of clinical tools to assess suicide risk have not been shown to have effective, and in fact the possibility of risk of harm cannot also be excluded. The sentence in the introduction at the end of paragraph 2 that could be related to this does not convey this clearly. 5) The introduction section could be improved by being more succinct in what they are trying to say and improving the structure. 6) They mention in their methods section that they plan to use simple descriptive statistics. This is not commonly part of scoping review methodology. They do not provide enough detail upon this should they wish to conduct statistical analysis. E.g. details of any software package, size of confidence intervals, median, mean, frequencies, or variables. 7) Their use of an integrated knowledge translation approach should come much earlier. Also, from the way in which they present this review process, it could be read that results / findings may be re-visited to ensure they meet the audience's needs perhaps incurring risk of bias. 8) They do not need to list the places that they wish to publish their findings – "appropriate peer review journal" would cover all. 9) They could be more specific about the anticipated outcomes of this particular review. Their section six, reads as fairly generic statements that could be said of any scoping review.

VERSION 1 – AUTHOR RESPONSE

Comments/ requests:	**Authors' response:**
Reviewer 1: Dr. Kessler	

1. I understand the goal, which I think is interesting. But does enough literature exist to justify a systematic literature review? I have never seen any empirical research on this question and I'm not even sure I would know where to begin if I was to design such a study myself.	Thank you for this comment, we agree with the seemingly narrow scope of this topic, and were also surprised by our initial scan of the literature. From our preliminary search of the literature, we have identified 860 articles within Medline. A quick title/ abstract scan of the first 50 articles have retrieved 15 articles that we are considering for full text review. We believe that through searching additional databases, and reviewing the remainder of the abstracts, we will have enough of a base to general preliminary themes on the topic. Papers from our initial title/abstract scan include titles such as "Primary care providers' views regarding assessing and treating suicidal patients", "How do healthcare professionals interview patients to assess suicide risk?", "Doctor's and patients' facial expressions and suicide reattempt risk assessment", to name a few. Finally, we have chosen to perform a scoping review instead of a systematic review due to the lack of standardized effectiveness outcomes available within this literature. As suggested by the Joanna Briggs Institute's (JBI) methodology for scoping reviews, our aim for this scoping review is to develop a "concept map" of the available evidence, which can be further used to explore more specific concepts within a future systematic review.
2. Focusing on your 3 research questions: What do you mean by "affect the suicide risk assessment process"? In my initial reading it occurred to me that would could mean •The decision whether or not to do an in-depth evaluation (As you know, only ab out half the people who present to an ED with suicidality get an in-depth evaluation eve n at centers where such evaluations are carried out) •Or the depth of the assessment – merely using a superficial screen or more in-depth assessment? •Or are you interested in the predictors of the accuracy of risk evaluations (e.g., studies comparing prediction models	Thank you very much for this feedback, we agree with the need to further clarify our research questions. We have also included details about the different levels of suicide risk assessment that can be performed within our introduction, which we hope speaks to both the decision whether or not to do an indepth evaluation, and the decision to perform a superficial or more in-depth assessment. We have also incorporated your suggested wording to further refine our question: We have updated our first question to read: "What non-patient specific factors influence the suicide risk assessment process (i.e. how a clinician conducts a suicide risk assessment, and how they arrive at their final clinical judgement, given their scope of practice)?" Since many of the studies that speak to the clinical experience around suicide risk assessment do not simultaneously measure accuracy clinical prediction

with clinical judgments ask clinicians to make judgments and then examine prediction accuracy, which is typically 0. Are you interested in predictors of individual differences in prediction accuracy)?	outcomes, we are not specifically looking for predictors of accuracy.
3. But as I continued reading and got to the section of the paper on (5) Collating, summarizing, … I saw that you are actually interested in subtle issues. You say "The first will include a breakdown of the non-patient specific factors affecting the suicide risk assessment process, while the second will report on results of the types of cognitive biases that emerge within the suicide risk assessment process." But how in the world do you plan to do this? Does published research exist on these questions? I would hope that a preliminary look was taken to make sure there is a rich literature of this sort, or else your review is doomed from the beginning. But if such a literature exists, I'm not sure why I have never seen any such studies. I think you need to address this point explicitly in this paper. Do you have reason to believe that such papers exist based on a preliminary look?	Thank you for this important comment. We too weren't sure whether there would be a body of published literature on this topic. To ensure this, we have taken a preliminary look. Through expanding on some of our search terms, including clinicians' "experiences" and "attitudes of health professionals" in addition with suicide risk assessment concept, we have discovered studies that report on the contextual factors affecting the clinical decision-making process. As mentioned in our response to a previous comment (#1), we hope that the number of initial search results, as well the results of a pilot title/abstract screening process helps clarify the types of studies we are looking to include within our final scoping review.
4. The 3rd question, In comparison, strikes me as easy to address. I have seen at least one report from the UK on such a program, although my recollection is that it was not very impactful.	We appreciate this comment, and agree that this is a more direct and easy to address review aim. Where possible, we will review outcomes on evaluation of such programs, and will include within our final review previous relevant reports (such as Pisani et al. (2011)'s "The Assessment and Management of Suicide Risk: State of Workshop Education" originating from the United States, and other similar reports from the UK and worldwide).
Reviewer 2: Dr. Gilmour	
1. This is an important area of research, a timely and ambitious review.	Thank you for your thoughtful comments, we agree that this is an important topic to review.

2. The abstract does not reflect the level of analysis that the review authors plan to conduct as is indicated in their methods section. Whether this level of analysis is appropriate for a scoping review is also potentially questionable.	Thank you for this comment, we have edited our abstract to include the following – A tabular synthesis of the general study details will be provided, as well as a narrative synthesis of the extracted data, organized into themes using the Situated Clinical Decision Making framework We have included in our comment below (please see response to comment #7) our updates to the numerical analysis, clarifying that we will only be looking at synthesizing general article details and not numerically analyzing any results.
3. The review authors do not provide a clear justification for why they have decided to do a scoping review as opposed to a systematic review. A systematic review would support their desire to analyse their findings. The Joanna Briggs Institute guidelines for scoping reviews may be helpful to the reviewers.	We have chosen to perform a scoping review instead of a systematic review due to the lack of standardized effectiveness outcomes available within this literature. The literature base has many articles with disparate research aims and heterogeneous in study designs, and therefore we feel that a scoping review is better warranted. Thank you for referring us to the Joanna Briggs Institute's methodology for scoping reviews. As suggested by these guidelines, our aim for this scoping review is to develop a "concept map" of the available evidence, by identifying themes found within the research. We hope these themes can be further used to explore more specific research questions within a future systematic review. We will also ensure that we use the PRISMA-SCR reporting for scoping reviews.
4. The relevance of the discussion around algorithms in the introduction is not clear since they are not used to inform clinical judgement.	Thank you for this comment, we have removed this section from the manuscript to ensure that only relevant information is present within the introduction.
5. There is a lack of reference to the large evidence base that shows that the use of clinical tools to assess suicide risk have not been shown to have effective, and in fact the possibility of risk of harm cannot also be excluded. The sentence in the introduction at the end of paragraph 2 that could be related to this does not convey this clearly.	This suggestion was helpful in helping us reframe our argument around the importance of clinical judgement with regards to suicide risk. We hope that including the language you have suggested has helped clarify this point.
6. The introduction section could be improved by being more succinct	Thank you, we have added in some additional details about the variety of suicide risk assessments that are practices, removed the discussion on clinical

in what they are trying to say and improving the structure.	algorithms, and edited the introduction. We hope that this has improved the structure and helps make our point more succinct.
7. They mention in their methods section that they plan to use simple descriptive statistics. This is not commonly part of scoping review methodology. They do not provide enough detail upon this should they wish to conduct statistical analysis. E.g. details of any software package, size of confidence intervals, median, mean, frequencies, or variables.	Thank you for this comment, we have updated our description of step 5 (collating, summarizing and reporting the articles) to specify that we will be producing a tabular synthesis of metadata from our studies, and will not be conducting any statistical analysis of the study results – Additionally, a tabular synthesis of the distribution of studies geographically (i.e. country of origin), distribution of studies by different clinician (e.g. nurses, primary care doctors) and patient populations (e.g. inpatient, outpatient, community mental health), methodology adopted (i.e. study design details) will also be included. This synthesis will be accompanied by a narrative synthesis, and will focus on metadata of the studies, and not consist of any statistical analysis of the results from the various studies. Using Covidence, we will be able to create a PRISMA flow chart and tabulate the required results. Finally, a qualitative narrative synthesis of the content of included articles. We have also included specific literature review software (i.e. Covidence) that we will be using for screening, summarizing and synthesis purposes.
8. Their use of an integrated knowledge translation approach should come much earlier. Also, from the way in which they present this review process, it could be read that results / findings may be re-visited to ensure they meet the audience's needs perhaps incurring risk of bias.	Thank you for this helpful feedback, we have moved up our integrated knowledge translation approach to the "Patient and Public Involvement" within the methods section. As suggested by the Arksey and O'Malley framework (and the Joanna Briggs Institute methodology for scoping reviews), we have described at what stage we will request consultation and the reasoning behind our need for this step- We will include consultation from stakeholder clinicians (i.e. an interdisciplinary suicide risk working group within a mental health hospital). Through providing these clinicians with preliminary results of the scoping review, they will be consulted on for suggestions for additional helpful references, and for providing insights that are beyond those found within our thematic analysis. Additionally, we will also consult with a patient advocacy group (i.e. the empowerment council within a mental health hospital) to gather the perspective of those with lived experience. We acknowledge the possibility that consulting with clinical stakeholders can introduce bias within the final

	results, and has added a line within our Strengths and Limitations section to include this - Consultation with content experts will be included to mitigate some of the limitations, *however it should be noted that this process can also introduce a risk of bias to the final findings*
9. They do not need to list the places that they wish to publish their findings – "appropriate peer review journal" would cover all.	We appreciate this suggestion, and have removed the titles of journals. To ensure consistency, we have also removed the names of possible conferences.
10. They could be more specific about the anticipated outcomes of this particular review. Their section six, reads as fairly generic statements that could be said of any scoping review.	Thank you for this suggestion, we have included specific examples of anticipated outcomes of this review, focusing on how the results can be used to direct future research – "Future research can focus on measuring the impact of each contextual factors on a clinician's assessment of suicide risk. The results from this review can contribute toward developing appropriate qualitative interview guides or aid in survey development for studying such research questions." We have also included anticipated outcomes of how results of this review can contribute toward improving education and training – "With regards to improving training and education, the results of this review can improve clinicians' awareness of the biases that exist within the suicide risk assessment process, helping them improve on more nuanced behaviours of this practice."

VERSION 2 – REVIEW

REVIEWER	Lynne Gilmour University of Stirling - Scotland -UK
REVIEW RETURNED	27-Nov-2018

GENERAL COMMENTS	The authors have addressed all comments made in the previous review and the revised manuscript reads really well. I recommend it for publication. NB: There are a few Americanised spelling typos to be amended.