PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

This paper was submitted to a another journal from BMJ but declined for publication following peer review. The authors addressed the reviewers' comments and submitted the revised paper to BMJ Open. The paper was subsequently accepted for publication at BMJ Open.

(This paper received three reviews from its previous journal but only two reviewers agreed to published their review.)

ARTICLE DETAILS

TITLE (PROVISIONAL)	COMBINING BRIEF CONTACT INTERVENTIONS (BCI) INTO A DECISION MAKING ALGORITHM TO REDUCE SUICIDE REATTEMPT: THE VIGILANS STUDY PROTOCOL.
AUTHORS	Duhem, Stephane; Berrouiguet, Sofian; Debien, christophe; DUCROCQ, Francois; Demarty, Anne-Laure; Messiah, Antoine; Courtet, Philippe; Jehel, Louis; Thomas, Pierre; Deplanque, Dominique; Danel, Thierry; WALTER, Michel; Notredame, Charles-Edouard; VAIVA, Guillaume

VERSION 1 – REVIEW

REVIEWER	Luke Kalb, PhD USA
REVIEW RETURNED	10-Apr-2018

GENERAL COMMENTS	The current paper is a study protocol. Overall, it was fairly well written. The eventual findings will be interesting to countries with socialized medicine. However, there were a number of awkward sentences, which is likely due to a non-native English speaker authorship. I recommend a close read and edit by a native english speaker before publication. Below is a list of comments. 1. The title is very long and unclear. Please shorten. 2. The entire manuscript is very lengthy. The authors should be able to cut at least 20%. I do not think understanding the protocol requires detailed instructions about every study aim. 3. The 10-day call for only those who attempted >1 time seems concerns me about those who had their first attempt. I know this is a resource issue but a phone call is quite brief. In the same vein, the 6 month follow-up is a really longtime. I also wonder if mailing is the best approach, opposed to text messaging. I expect an extremely high non response. 4. The following sentence were awkward: p6., line13-14; p. 6, lines 34, 35; p.15, line 54, 55; p. 16, line 5, 6. 5. The interpretation of the IRR is wrong on p.7, line 23. An IRR of .66 is a 32% reducation (1-IRR), not a 1.66 increase in less frequent attempts.

	6. The postcards in 3.2.3 could be better described 7. It would be helpful to provide the actual questions in the quantitative appraisal on p.15 8. The power analysis should probably be deleted. It has to do with only 1 analysis (the logit model) and there are many analyses. It is confusing why one choses only this. Plus, it is not really a power analysis. there is no discussion of power or sample size. They only talk about the # of variables they can include in the model. 9. I am concerned about stigma. Giving someone a "crisis card", which effectively identifies them as a suicide attempted raises concerns about labeling 10. This protocol would be greatly strengthened by a community participatory board. This is a very "top down" approach that is driven by the medical institution. 11. I like the qualitative part. However, 50 interviews is not enough to generalize the findings. I recommend excluding that term.

REVIEWER	Alexander Millner Harvard University
REVIEW RETURNED	11-Apr-2018

GENERAL COMMENTS	This paper outlines the VigilanS program, which is an intervention aimed at people just discharged from having made a suicide attempt. The paper clearly outlines the program/algorithm, the goals of the project and the analyses to test its effectiveness. The paper is well-written and straightforward and the proposed project seems valuable. I have only a couple comments/questions. First, the authors state that post-discharge suicide deaths account for about 5% of suicide deaths and the pre-cursor program, ALGOS, reduced re-attempts by 5.6%. Was ALGOS also associated with a reduction in suicide deaths compared with usual care? If not, the authors should transparently state this. Second, the authors discuss cost effectiveness in terms of targeting the post-discharge period and the use of Brief Contact Interventions but do not propose any formal cost-effectiveness analyses or outcome or for the proposed project. I think a cost-effectiveness analysis could provide a lot of benefit. Presumably each intervention costs additional money to implement. Although, as the authors state, all the pieces of the ALGOS program were superior to the individuals pieces, perhaps some provide minimal improvement at large cost. I am not proposing that the authors decompose the program in order to test the cost effectiveness of each component but I do wonder whether the authors could propose ways in which the cost of each intervention is at least reported and considered and potentially analyzed. In addition, it's important to compare the cost to the cost of usual care.

REVIEWER	Dr Claire Kelly Mental Health First Aid Australian and Deakin University
REVIEW RETURNED	13-Apr-2018

GENERAL COMMENTS	Less a revision than some needed clarifications. I need to say as well that there are no CONSORT, PRISMA or STROBE statements attached, and I can only assume that these processes formed part of the inclusion in the clinical trials register. My apologies if I have

	understood that incorrectly. First of all I think this is an extremely important study, and if the protocol is effective, it has potential for manualisation. I do have some points I would like clarification on. Page 8 Line 29 and Page 14 line 39 – The term 'suicidal gesture' is used at times. This is a bit ambiguous. Does this refer to medically non-serious suicide attempts? Is non-suicidal self-injury differentiated or excluded? The term 'gesture; can be very trivialising if it is not defined, and a clearer definition might be better. What is the argument for leaving the index follow-up for 10-20 days for those with a previous attempt? It seems very long – is it in addition to usual care from a clinical team or is this likely to be the only follow-up post-discharge? Also, does this mean that first attempters have no contact until 2 month postcard? Page 15, qualitative analysis – you say 50 will be selected. Is this with an aim of getting a lower number to agree, or will you continue to sample until you have 50? I can only guess that acceptance would be low in the patient sample, but I am not aware of any relevant research that might be used for reference. Are these individuals being selected on an ongoing basis and if so, is it after the full 12 month enrolment is complete or after the standard 6 month contact has occurred? Page 16 line 23~ – the KSS scale. Is this a translation or broader adaptation? Has it been validated, and if not, is there a chance to use a large enough sample to do a validation study of some kind? Page 17 line 2 – what constitutes 'reachability'? Is that after the 3 attempts described earlier? Page 19 line 23 – 'borderline patients' – is this supposed to be 'patients with borderline personality disorder' or are you referring to a threshold of some kind? This needs to be specified (useful to remember to use person-first language, as well, especially in relation to highly stigmatised illnesses). I wish you the best of luck with this important study. It has tremendous potential.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Luke Kalb, PhD
Institution and Country: USA
Please state any competing interests or state 'None declared': None

Please leave your comments for the authors below
The current paper is a study protocol. Overall, it was fairly well written. The eventual findings will be interesting to countries with socialized medicine. However, there were a number of awkward sentences, which is likely due to a non-native English speaker authorship. I recommend a close read and edit by a native english speaker before publication.

Dear reviewer,
We thank Reviewer 1 for his careful review and valuable comments.
We addressed below his remarks and queries.

Below is a list of comments.

1. The title is very long and unclear. Please shorten.

We thank Reviewer 1 for this remark. The title has been shortened and modified. The manuscript is now untitled: "Combining brief contact interventions (BCI) into a decision-making algorithm to reduce suicide reattempt: The VigilanS Study protocol."

2. The entire manuscript is very lengthy. The authors should be able to cut at least 20%. I do not think understanding the protocol requires detailed instructions about every study aim.

As requested, we significantly shortened the manuscript.

3. The 10-day call for only those who attempted >1 time seems concerns me about those who had their first attempt. I know this is a resource issue but a phone call is quite brief. In the same vein, the 6 month follow-up is a really longtime. I also wonder if mailing is the best approach, opposed to text messaging. I expect an extremely high non response.

We thank Reviewer 1 for these questions. The phone call was scheduled at 10-20 days for patient with a previous suicide attempt for two reasons.

We paid careful attention to implement our algorithm following the evidence based principles. In previous studies from our team, we found 30 days phone calls to be significantly efficient in reducing suicide attempt in non-first attempters. However, we also evidenced that about 30% of those who nevertheless attempted suicide di so between D15 and D30. The shortened delay we chose in accordance (10-20 day) was assessed in a recent article. (Vaiva et al. J Clin Psy, In press).

First attempters also benefit from the treatment as usual, which means that at least an appointment is scheduled after emergency discharge. In addition, it is important to note that every patient, including first attempters, are provided with a "crisis card" during the discharge process . Therefore, they also contact our service 24/24, 7/7 which is a solid complementary safety guarantee.

In parallel of the Algos Study (Vaiva et al. J Clin Psy, In press), an independent team from the French Institute for Public Health Research (IRESP) conducted a qualitative survey to gain a more in-depth understanding of how the system was perceived by patients and to collect opinions about how to improve the system. Preliminary results suggested that ALGOS evaluation phone calls at 6 months were perceived as genuine prevention interventions by patients. We estimated that the 6-months delay was sufficiently remote in time to reassess the patient away from the initial crisis

4. The following sentence were awkward: p6, line13-14; p. 6, lines 34, 35; p.15, line 54, 55; p. 16, line 5, 6.

We corrected these sentences.

5. The interpretation of the IRR is wrong on p.7, line 23. An IRR of .66 is a 32% reducation (1-IRR), not a 1.66 increase in less frequent attempts.

We thank Reviewer 1 for having pointed out this possibly misleading presentation of the IRR. We re-wrote the reference to this result in a more straightforward manner.

6. The postcards in 3.2.3 could be better described

We have specified our description of the postcards in the manuscript (p 10)

7. It would be helpful to provide the actual questions in the quantitative appraisal on p.15

Questions have been added in the quantitative appraisal (p 15)

8. The power analysis should probably be deleted. It has to do with only 1 analysis (the logit model) and there are many analyses. It is confusing why one choses only this. Plus, it is not really a power analysis. there is no discussion of power or sample size. They only talk about the # of variables they can include in the model.

The power analysis has been deleted.

9. I am concerned about stigma. Giving someone a "crisis card", which effectively identifies them as a suicide attempted raises concerns about labeling

We thank Reviewer 1 for these questions. In a study on patient satisfaction regarding further telephone contact following attempted suicide (Gruat et al. Vécu subjectif du recontact téléphonique après tentative de suicide. Encephale. 2010 Jun. French) suggested that BCIs are well-accepted by the patients. No notion of stigma was reported.

10. This protocol would be greatly strengthened by a community participatory board. This is a very "top down" approach that is driven by the medical institution.

VigilanS is the release and generalization of the ALGOS protocole in an open healthcare offer. The ALGOS algorithm was evaluated by an independent team from the French Institute for Public Health Research (IRESP) who conducted a qualitative survey on patients. The development of the research was based on this qualitative survey of ALGOS study. This survey allowed to collect patient's opinions to improve the system according to these priorities, experiences and preferences.

11. I like the qualitative part. However, 50 interviews is not enough to generalize the findings. I recommend excluding that term.

We thank Reviewer 1 for this remark. We will randomly select 50 patients from the whole admission list stratified by age, gender, history of suicide attempt and origin Partner Centre. A sample of 50 is a usual size for qualitative analysis in this population (see Milner A et Al Male suicide among construction workers in Australia: a qualitative analysis of the major stressors precipitating death. BMC Public Health, 2017). In addition, we didn't find any reference to the generalizability in the manuscript.

Thank you so much for reviewing our work

Reviewer: 2
Reviewer Name: Alexander Millner
Institution and Country: Harvard University
Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below
This paper outlines the VigilanS program, which is an intervention aimed at people just discharged from having made a suicide attempt. The paper clearly outlines the program/algorithm, the goals of the project and the analyses to test its effectiveness. The paper is well-written and straightforward and the proposed project seems valuable.

I have only a couple comments/questions.

Dear reviewer,
Thank you for this review and comment.
We addressed below your remarks and queries.

First, the authors state that post-discharge suicide deaths account for about 5% of suicide deaths and the pre-cursor program, ALGOS, reduced re-attempts by 5.6%. Was ALGOS also associated with a reduction in suicide deaths compared with usual care? If not, the authors should transparently state this.

We thank the Reviewer 2 for this comment. The following sentence has been added: "We found no significant superiority of ALGOS in terms of death by suicide, probably due to a lack of statistical power related to the rarity of the event (3 suicides in the ALGOS group vs 8 suicides in the control group)"

Second, the authors discuss cost effectiveness in terms of targeting the post-discharge period and the use of Brief Contact Interventions but do not propose any formal cost-effectiveness analyses or outcome or for the proposed project. I think a cost-effectiveness analysis could provide a lot of benefit. Presumably each intervention costs additional money to implement. Although, as the authors state, all the pieces of the ALGOS program were superior to the individuals pieces, perhaps some provide minimal improvement at large cost. I am not proposing that the authors decompose the program in order to test the cost effectiveness of each component but I do wonder whether the authors could propose ways in which the cost of each intervention is at least reported and considered and potentially analyzed. In addition, it's important to compare the cost to the cost of usual care.

The following paragraph has been added
"Its medico-economic viability. Even if VigilanS if proven efficient, an important question will remain as to whether the gain in terms of number of prevented suicides and suicide attempts is rationally proportionated to the expenses incurred for the algorithm. To answer this issue, we will conduct a two-steps medico-economic assessment of the program. First a micro-costing procedure will allow for performing a cost-effectiveness study. The costs of all the components of the algorithm taken separately, as well as their combination, will be proportionated to the number of avoided attempts and deaths, and compared to the as-usual treatment. Second, a cost-benefit analysis will complete the cost-effectiveness study by estimating the direct and indirect costs of the prevented suicides and suicide attempts in terms of consumption of care and medical goods and loss of productivity."

Thank you so much for reviewing our work

Reviewer: 3
Reviewer Name: Dr Claire Kelly
Institution and Country: Mental Health First Aid Australian and Deakin University
Please state any competing interests or state 'None declared': None declared.

Please leave your comments for the authors below
Less a revision than some needed clarifications. I need to say as well that there are no CONSORT, PRISMA or STROBE statements attached, and I can only assume that these processes formed part of the inclusion in the clinical trials register. My apologies if I have understood that incorrectly.
First of all I think this is an extremely important study, and if the protocol is effective, it has potential for manualisation. I do have some points I would like clarification on.

Dear Reviewer,

We thank Reviewer 3 for her encouraging comment. We addressed below her remarks and queries

Page 8 Line 29 and Page 14 line 39 – The term 'suicidal gesture' is used at times. This is a bit ambiguous. Does this refer to medically non-serious suicide attempts? Is non-suicidal self-injury differentiated or excluded? The term 'gesture; can be very trivialising if it is not defined, and a clearer definition might be better.

We thank Reviewer 3 for this comment. We agree this term may be misleading. We have replaced the term 'suicidal gesture' by "suicide attempt"

What is the argument for leaving the index follow-up for 10-20 days for those with a previous attempt? It seems very long – is it in addition to usual care from a clinical team or is this likely to be the only follow-up post-discharge? Also, does this mean that first attempters have no contact until 2 month postcard?

We thank Reviewer 3 for these questions. The phone call was scheduled at 10-20 for patient with a previous suicide attempt for two reasons. First, this algorithm for suicide prevention «VigilanS»

combines several BCIs that showed a significant reduction in the number of SA repeaters, including systematic telephone contact (effective in those with a previous suicide attempt) and «crisis card» (effective following a first attempt). We assessed the efficacy on reducing suicide attempt in a previous study (Vaiva et al. BMJ, 2006). As we found this intervention efficient in reducing suicide attempt in non first attempters, we included it in our algorithm. In this study, the phone call was schedulled at 30 days, we found that 30 % of reattempts occurred between d15 and 30. Thus we defined the 10-20° day period as the best. Moreover, the efficacy of this intervention was assessed in a recent article (Vaiva et al. J Clin Psy, In press).
First attempters has also treatment as usual, which means that at least an appointment is scheduled after emergency discharge. They also can contact our service using the "crisi card". During the discharge process, patients were provided with a "crisis card"11 which included a local phone number which could be called 24/7

Page 15, qualitative analysis – you say 50 will be selected. Is this with an aim of getting a lower number to agree, or will you continue to sample until you have 50? I can only guess that acceptance would be low in the patient sample, but I am not aware of any relevant research that might be used for reference. Are these individuals being selected on an ongoing basis and if so, is it after the full 12 month enrolment is complete or after the standard 6 month contact has occurred?

We thank Reviewer 3 for this remark. We agree the sentence might have been misleading. We will randomly select 50 patients from the whole admission list stratified by age, gender, history of suicide attempt and origin Partner Centre. A sample of 50 is a usual size for qualitative analysis in this population (see Milner A et Al Male suicide among construction workers in Australia: a qualitative analysis of the major stressors precipitating death. BMC Public Health, 2017). These patients will be interviewed after they complete the 6-month follow-up period.

Page 16 line 23~ – the KSS scale. Is this a translation or broader adaptation? Has it been validated, and if not, is there a chance to use a large enough sample to do a validation study of some kind?

We thank Reviewer 3 for this comment. We agree that as this scale has not been validated yet, this assessment should not be integrated in our study protocol. The following sentence has been removed:

"To test these assumptions, we built an ad hoc questionnaire inspired from Batterham's "Literacy of Suicide Scale" (16), named "Knowledge of Suicide Scale" (KSS) that is currently under validation. This questionnaire is designed to test general epidemiological knowledge, as well the extent to which participants endorse common misconceptions about suicide (e.g., "When one has decided to take one's life, nothing can be done to prevent the death"). The KSS will be administered to every healthcare professional in the Partner Centres who will potentially be in contact with suicidal patients. The total score for the questionnaire will be considered our judgement criterion for literacy improvement and, as such, will be compared before (at the Partner Centre opening) and after (9 months later) the implementation of the programme"

Page 17 line 2 – what constitutes 'reachability'? Is that after the 3 attempts described earlier?

We thank the reviewer for this remark. As described last paragraph of page 9: patients will be declared "unreachable" if we cannot contact with them despite 3 call attempts scheduled at different days and different times, the program sends him/her 4 postcards within 5 months.

Page 19 line 23 – 'borderline patients' – is this supposed to be 'patients with borderline personality disorder' or are you referring to a threshold of some kind? This needs to be specified (useful to remember to use person-first language, as well, especially in relation to highly stigmatised illnesses).

We thank Reviewer 3 for this comment. By 'borderline patients' we meant 'patients with borderline personality disorder'. The sentence was edited as following "Which adjustments are needed for patients suffering from borderline personality disorders"