

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.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Evaluation of a knowledge translation strategy to improve policy making and practices in health promotion and disease prevention setting in French regions: TC-REG, a realist study
AUTHORS	Martin-Fernandez, Judith; Aromatario, Olivier; Prigent, Ollivier; Porcherie, Marion; Ridde, Valéry; Cambon, Linda

VERSION 1 – REVIEW

REVIEWER	Dean, Elizabeth University of British Columbia, Physical Therapy
REVIEW RETURNED	14-Feb-2021

GENERAL COMMENTS	<p>Overview</p> <p>This manuscript describes a realist evaluation study of knowledge translation (KT) strategies implemented locally in relation to cancer prevention in France. The protocol was published in 2017 in Implementation Science. Study participants included decision makers and field professionals working in prevention and public health services operating in regions of France (i.e., ARS, IREPS, and their partners). Qualitative research methods were used to extract input from relevant stakeholders (i.e., local, regional and inter-regional) in the form of 2 seminars, 82 interviews, 18 observations, and 4 focus groups over an 18-month period. Eight middle-range theories emerged from the cumulative data. These theories and related analyses generated seven operational and contextualized recommendations for KT strategies which were designed to inform regional policy-making regarding health promotion and disease prevention. Based on the findings, the investigators formulated two operational perspectives to facilitate KT for stakeholders engaged in health promotion and disease prevention in France.</p> <p>Substantive Comments</p> <p>I have two distinct overarching responses to this work. The first addresses standard issues related to my technical review of the work; the second addresses issues related to academic integrity regarding authorship credits and discrepancies between the original protocol (published in 2017) and this written submission.</p> <p>1. The multi-pronged body of work has been well-conceived, designed, executed and described. In addition, the original protocol was published in 2017 and the findings, which are extensive, have been written up three years later. The work is superbly crafted in terms of its theoretical basis, development of middle-range theories, and extrapolation of the ideas in a scholarly manner, to practical applications for stakeholders. This is exceptional given the extensiveness and diversity of the data collection. I also applaud the</p>
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investigators for their commitment to the scientific writing of the manuscript and having two professional native English-speaking proof readers and editors involved to ensure the quality of the written presentation. I can rate the work as exceptional on these dimensions. I only have a few relatively minor suggestions for revision. My priority would be to have the work published for the health care community to review, critique, integrate and use the valuable and practical evidence-based information to advance KT in relation to lifestyle-related non-communicable diseases, which has been sorely inadequate, in my view, in the western world (given what we know unequivocally about these conditions and their contributing factors, not only for cancer).

My comments for consideration for revision include the following. There is mention of special reference to 'cancer prevention', here and there. But, this focus needs to be more consistently mentioned throughout the manuscript including the Conclusion. Given mention of 'cancer' per se does not seem to be singularly important, can the investigators not simply refer to 'health promotion and disease prevention' generally (I suggest in that order)?

I also suggest the investigators examine the terms 'health promotion' and 'disease prevention' and use these combined in most places, but refer to them individually as needed. Somehow the combined focus is lost in some places. It would be also worth contextualizing 'disease prevention' by at least in the Introduction defining what you mean, most likely 'lifestyle-related non-communicable diseases' given the stated focus on cancer. These suggestions will tighten up the focus of the work, in turn, the relevance and application of the recommendations for stakeholders.

2. This response is my more critical one, as without attention to this point, the minor revisions will not matter. In my view, this work cannot be published without transparency about the investigators and their roles between the protocol publication (2017) to the reporting of the findings in this submission. My reasoning for this is as follows.

The present submission lists six authors Judith Martin-Fernandez, Olivier Aromatario, Ollivier Prigent, Marion Porcherie, Valéry Ridde, and Linda Cambon.

Authors' contributions to this submission are listed as:

'LC supervised the study. JMF, OA and LC drafted this article and all authors revised the manuscript. The project design was developed by LC and OA. OP, OA collected the data. OA and LC analyzed the data under the supervision of LC. JMF contributed to part of this analysis. All authors read and approved the final manuscript.'

However, there are discrepancies in the authors and their contributions in the publication of the original protocol (titled slightly differently, but not substantially, i.e., 'Evaluation of a knowledge transfer scheme to improve policy making and practices in health promotion and disease prevention setting in French regions: a realist study protocol' published in Implementation Science volume 12, Article number: 83 (2017)0.

This original protocol published had 9 authors, namely:

Linda Cambon (listed as the primary author on that publication, now the senior author on this submission, which is acceptable), Audrey Petit, Valery Ridde, Christian Dagenais, Marion Porcherie, Jeanine Pommier, Chrisine Ferron, Laetitia Minary and François Alla.

	<p>Only three of these nine authors are common both to the published protocol (2017) and this submission (under review). In addition, there are three new authors on this submission who do not appear on the published protocol. This is most unusual, almost irregular.</p> <p>In terms of authors' contributions with respect to the original protocol, these are given as: 'Authors' contributions LC and AP drafted this article and all authors revised the manuscript. The project design was developed by LC and AP. LM, FA, JP, MP and CF were involved in implementing the project and in developing the evaluation design. VR and CD were involved in the design of the middle range theory. All authors read and approved the final manuscript.'</p> <p>Given issues around intellectual property and academic integrity, these discrepancies need to be clearly explained. Why are six of the nine authors on the original protocol not included in this submission? How did three additional authors become involved with this study and warrant authorship after the original conceptual work and development had apparently been done?</p> <p>In addition to an explanation for these observations, I believe it behooves the editor of BMJ Open to request that the six authors on the original protocol but not on this manuscript submission each be requested to write a formal letter to the editor stating that they have relinquished their intellectual property and rights to this work and do not seek either authorship or acknowledgement for their original contributions, which appear substantial. This declaration needs to be signed by each of them independently before publication can be considered. Even should they minimally opt for acknowledgement, each of them needs to agree to this.</p> <p>This second point is of paramount importance. I am unable to recommend publication of this exceptional work without assurances of authors' intellectual and operational contributions and these being much more transparent.</p> <p>I would prefer not leap to conclusions, but should there have been a fracturing of relationships within the team, this is most regrettable. The work is exceptional in several respects and constitutes an important contribution to the literature which obviously necessitated considerable melding of perspectives and ideas to see the work to fruition. Thus, this should be a time of celebration for the investigative research team. Whatever, I do hope there is resolution among the investigators that is satisfactory to all and in the interest of preserving academic integrity in academic publishing.</p>
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REVIEWER	Johnston, Lucy Edinburgh Napier University, Health and Social Care
REVIEW RETURNED	15-Feb-2021

GENERAL COMMENTS	<p>Abstract: It is not clear from this what was done and what was found - it does not summarise with enough clarity the aim, method, results, conclusion. I am not sure what "raw results" is meant to convey to the reader.</p> <p>Methods: Whilst it is clear that a substantial amount of work has been undertaken, this article fails to convey in a logical, transparent way what was done in sufficient detail. Data is also (Table 1) presented within the Methods Section - and this is difficult to digest -</p>
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	<p>due to its location in the text and its unwieldy presentation. On page 11 - the authors present 'Initial middle range theories' - it may be these are their Initial Programme Theories - but it unclear where they were generated from. Also BOX 1 gives some contextual factors (Ce/Ci) - but the text states that "none were identified". The intervention is not at all well defined. This is clear in the protocol perhaps - but as a stand alone article, the authors should describe better and in more detail what was actually being evaluated.</p> <p>Outcomes: These are not clearly set out and at present the results section does not provide enough detail as to the analysis/insights to say with confidence whether the recommendations/conclusions are justified or supported by the analysis. The presentation of results is insufficiently detailed. It is not clear how the extensive data was actually used to develop, support, refute or refine the programme theory. The use of the term Middle Range Theory throughout should be re-considered. The intervention is reported as involving the following " Supporting access to and adaptation of usable evidence, Strengthening professionals' skills in analyzing, adopting, and using the policy briefs, Facilitating the use of evidence in organizations and processes". The conclusions state that "Success was defined as the plan's ability to i) enable public health stakeholders to address the challenges of KT and ii) bring about changes in public health policy and practices (i.e., integration of evidence-informed public health and collaborative practices)". A major weakness of this article is that it does not provide any analysis of how these worked, what outcomes were seen/observed and as such where and how the 'new' middle range theories can help implementation in different areas. Whilst these remain unclear, the reader cannot relate or use the presented analysis. Perhaps the results are presented at a level too high or too broadly - and a focus on a smaller number of more specific / tested CMOs would be more effective and of use to the reader.</p> <p>The authors should consider how they can better set out to a reader how a realist perspective was actually used in the analysis - and work to strengthen and clarify the presentation of what they say they found. They should also re-reflect on the stated strengths and limitations of the study and use this process to determine in much, much greater detail where and in what ways the data supports the findings and their subsequent recommendations and conclusions.</p> <p>The authors should look at how others more experienced in RE have worked to do this within journal articles. The thoughts and ideas set out in the protocol for this study have not been carried forward into the reporting of the analysis and findings. For example the descriptions of possible outcomes of KT (ie 3 different ways of using knowledge) are not revisited or interrogated in any detail. Without more detail on what the 'outcomes' specifically of TC-REG as an intervention actually were - it is impossible to have a view on whether the suggested context and mechanisms, taxonomy, recommendations, perspectives on implementation and 8 middle range theories have any meaning or use to those seeking to enhance KT in their own workplaces.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Dr. Elizabeth Dean, University of British Columbia

Comments to the Author:

Review of manuscript titled 'Evaluation of a knowledge translation strategy to improve policy making and practices in health promotion and disease prevention setting in French regions: TC-REG, a realist study' (bmjopen-2020-045936)

Overview

This manuscript describes a realist evaluation study of knowledge translation (KT) strategies implemented locally in relation to cancer prevention in France. The protocol was published in 2017 in Implementation Science. Study participants included decision makers and field professionals working in prevention and public health services operating in regions of France (i.e., ARS, IREPS, and their partners). Qualitative research methods were used to extract input from relevant stakeholders (i.e., local, regional and inter-regional) in the form of 2 seminars, 82 interviews, 18 observations, and 4 focus groups over an 18-month period. Eight middle-range theories emerged from the cumulative data. These theories and related analyses generated seven operational and contextualized recommendations for KT strategies which were designed to inform regional policy-making regarding health promotion and disease prevention. Based on the findings, the investigators formulated two operational perspectives to facilitate KT for stakeholders engaged in health promotion and disease prevention in France.

Substantive Comments

I have two distinct overarching responses to this work. The first addresses standard issues related to my technical review of the work; the second addresses issues related to academic integrity regarding authorship credits and discrepancies between the original protocol (published in 2017) and this written submission.

1. The multi-pronged body of work has been well-conceived, designed, executed and described. In addition, the original protocol was published in 2017 and the findings, which are extensive, have been written up three years later. The work is superbly crafted in terms of its theoretical basis, development of middle-range theories, and extrapolation of the ideas in a scholarly manner, to practical applications for stakeholders. This is exceptional given the extensiveness and diversity of the data collection. I also applaud the investigators for their commitment to the scientific writing of the manuscript and having two professional native English-speaking proof readers and editors involved to ensure the quality of the written presentation. I can rate the work as exceptional on these dimensions. I only have a few relatively minor suggestions for revision. My priority would be to have the work published for the health care community to review, critique, integrate and use the valuable and practical evidence-based information to advance KT in relation to lifestyle-related non-communicable diseases, which has been sorely inadequate, in my view, in the western world (given what we know unequivocally about these conditions and their contributing factors, not only for cancer).

My comments for consideration for revision include the following. There is mention of special reference to 'cancer prevention', here and there. But, this focus needs to be more consistently mentioned throughout the manuscript including the Conclusion. Given mention of 'cancer' per se does not seem to be singularly important, can the investigators not simply refer to 'health promotion and disease prevention' generally (I suggest in that order)?

I also suggest the investigators examine the terms 'health promotion' and 'disease prevention' and use these combined in most places, but refer to them individually as needed. Somehow

the combined focus is lost in some places. It would be also worth contextualizing ‘disease prevention’ by at least in the Introduction defining what you mean, most likely ‘lifestyle-related non-communicable diseases’ given the stated focus on cancer. These suggestions will tighten up the focus of the work, in turn, the relevance and application of the recommendations for stakeholders.

The authors thank the reviewer for her valuable opinion on this paper. As requested, we changed the wording on this topic

2. This response is my more critical one, as without attention to this point, the minor revisions will not matter. In my view, this work cannot be published without transparency about the investigators and their roles between the protocol publication (2017) to the reporting of the findings in this submission. My reasoning for this is as follows.

The present submission lists six authors Judith Martin-Fernandez, Olivier Aromatario, Ollivier Prigent, Marion Porcherie, Valéry Ridde, and Linda Cambon.

Authors’ contributions to this submission are listed as:

‘LC supervised the study. JMF, OA and LC drafted this article and all authors revised the manuscript. The project design was developed by LC and OA. OP, OA collected the data. OA and LC analyzed the data under the supervision of LC. JMF contributed to part of this analysis. All authors read and approved the final manuscript.’

However, there are discrepancies in the authors and their contributions in the publication of the original protocol (titled slightly differently, but not substantially, i.e., ‘Evaluation of a knowledge transfer scheme to improve policy making and practices in health promotion and disease prevention setting in French regions: a realist study protocol’ published in Implementation Science volume 12, Article number: 83 (2017)0.

This original protocol published had 9 authors, namely:

Linda Cambon (listed as the primary author on that publication, now the senior author on this submission, which is acceptable), Audrey Petit, Valery Ridde, Christian Dagenais, Marion Porcherie, Jeanine Pommier, Christine Ferron, Laetitia Minary and François Alla.

Only three of these nine authors are common both to the published protocol (2017) and this submission (under review). In addition, there are three new authors on this submission who do not appear on the published protocol. This is most unusual, almost irregular.

In terms of authors’ contributions with respect to the original protocol, these are given as: ‘Authors’ contributions LC and AP drafted this article and all authors revised the manuscript. The project design was developed by LC and AP. LM, FA, JP, MP and CF were involved in implementing the project and in developing the evaluation design. VR and CD were involved in the design of the middle range theory. All authors read and approved the final manuscript.’

Given issues around intellectual property and academic integrity, these discrepancies need to be clearly explained. Why are six of the nine authors on the original protocol not included in this submission? How did three additional authors become involved with this study and warrant authorship after the original conceptual work and development had apparently been done?

In addition to an explanation for these observations, I believe it behooves the editor of BMJ Open to request that the six authors on the original protocol but not on this manuscript submission each be

requested to write a formal letter to the editor stating that they have relinquished their intellectual property and rights to this work and do not seek either authorship or acknowledgement for their original contributions, which appear substantial. This declaration needs to be signed by each of them independently before publication can be considered. Even should they minimally opt for acknowledgement, each of them needs to agree to this.

This second point is of paramount importance. I am unable to recommend publication of this exceptional work without assurances of authors' intellectual and operational contributions and these being much more transparent.

I would prefer not leap to conclusions, but should there have been a fracturing of relationships within the team, this is most regrettable. The work is exceptional in several respects and constitutes an important contribution to the literature which obviously necessitated considerable melding of perspectives and ideas to see the work to fruition. Thus, this should be a time of celebration for the investigative research team. Whatever, I do hope there is resolution among the investigators that is satisfactory to all and in the interest of preserving academic integrity in academic publishing.

Dear Dr. Elizabeth Dean, we do understand your concern and wish these elements can address your concern:

AP and JP contributed in 2016 to the writing of the protocol that we submitted for funding. This is why their contribution was valued in the protocol article. They left the research filed in 2017 (before the project really started) for completely different functions without having left any way of contacting them, so it was impossible for us to keep them informed of the progress of the study or its results. Hence, they did not participate in the work presented in this article and the one on the taxonomy, neither in its conception, nor its analysis, nor its writing.

FA, like AP and JP, contributed at the time to the review of the protocol and the article describing the protocol. He was unable to continue his contribution because he reoriented his own research work. He is now, together with the last author LC, the co-director of the research team of the authors of the article. He is therefore aware of these results and the submission of the article without having claimed his participation in the co-authorship. He left the project in 2018 and did not participate in the work presented in this article and the one on the taxonomy, neither in its conception, nor its analysis, nor its writing.

LM had written the quantitative data collection part of the TC REG study in the response to the call for funding. This part was not retained in the course of the project, which was refocused on a qualitative approach as described in the protocol article and this article. Nevertheless, we had associated it with the article describing the protocol because it had participated in the preparatory meetings and thus contributed to the general reflection before the beginning of the study. However, in accordance with the ethical standards, she has not contributed at all to the rest of the project and should not be included in this article: she left the project in 2018 and did not participate in the work presented in this article and the one on the taxonomy, neither in its conception, nor its analysis, nor its writing. Finally, due to disputes between the two universities, she served notice that her participation was formally terminated on 12 Dec 2018, and that her contribution had been valued by the protocol paper.

CF is thanked on behalf of the FNES, this is what was defined with her during the last steering committee of the project (not in writing).

Finally, CD was asked to be associated with this article and in view of his own experience in the subject of knowledge transfer, he declined.

To conclude, we followed the international rules specify that we must ascribe legal authorship to all those persons who have played a substantial role in the project, preparing the results, and/or in analyzing and interpreting the results; have taken part in drafting the article, or have carried out a revision of the text representing a significant contribution in terms of its intellectual content; endorsed the final version of the manuscript.

In accordance with these ethical rules and as described in the "Authors' contribution" section, the authors of the submitted article have played a substantial role in the design of the collection tools, and/or the data collection and the data analyses, and/or the interpretation of the results, the writing and the approval of the article. Those who contributed more marginally are mentioned by their institutions (FNES, IREPS) or personally in the "Acknowledgements" section.

The difference in authorship is linked to the professional hazards that led some to leave the project before its effective designing and implementation while others joined it.

Reviewer: 2

Mrs. Lucy Johnston, Edinburgh Napier University

Comments to the Author:

Abstract: It is not clear from this what was done and what was found - it does not summarise with enough clarity the aim, method, results, conclusion. I am not sure what "raw results" is meant to convey to the reader.

We rewrote the abstract trying to follow these remarks and the journal's guidelines.

Methods:

- Whilst it is clear that a substantial amount of work has been undertaken, this article fails to convey in a logical, transparent way what was done in sufficient detail.

We reorganize the paper and rewrote some sections in order to be comprehensible and clearer.

- Data is also (Table 1) presented within the Methods Section - and this is difficult to digest - due to its location in the text and its unwieldy presentation.

We changed the presentation and the content of this table.

- On page 11 - the authors present 'Initial middle range theories' - it may be these are their Initial Programme Theories - but it unclear where they were generated from.

The initial middle-range theory presented in page 11, is generated from the literature review and the first seminar both presented in the "TC-REG intervention" section. This theory is considered as a middle-range due to its sufficient abstraction, logical derivation and its fallibility; these 3 key rules were presented by Pawson in 2008 as the main characteristics of middle-range theory(1).

- Also BOX 1 gives some contextual factors (Ce/Ci) - but the text states that "none were identified".

We added the word external that was missing.

- The intervention is not at all well defined. This is clear in the protocol perhaps - but as a stand alone article, the authors should describe better and in more detail what was actually being evaluated.

We added a section (called the "TC-REG intervention") to describe the intervention more clearly.

- Outcomes: These are not clearly set out

We added details in the data collection section and in the table 1 regarding the outcomes

- at present the results section does not provide enough detail as to the analysis/insights to say with confidence whether the recommendations/conclusions are justified or supported by the analysis. The presentation of results is insufficiently detailed. It is not clear how the extensive data was actually used to develop, support, refute or refine the programme theory.

We developed the data analysis section and detailed our analysis process more thoroughly.

- The use of the term Middle Range Theory throughout should be re-considered.

The use of the term MRT is following the same key rules presented before:

a theory is considered as a middle-range due to its sufficient abstraction, logical derivation and its fallibility.

- The intervention is reported as involving the following " Supporting access to and adaptation of usable evidence, Strengthening professionals' skills in analyzing, adopting, and using the policy briefs, Facilitating the use of evidence in organizations and processes". The conclusions state that "Success was defined as the plan's ability to i) enable public health stakeholders to address the challenges of KT and ii) bring about changes in public health policy and practices (i.e., integration of evidence-informed public health and collaborative practices)". **A major weakness of this article is that it does not provide any analysis of how these worked, what outcomes were seen/observed and as such where and how the 'new' middle range theories can help implementation in different areas.** Whilst these remain unclear, the reader cannot relate or use the presented analysis. Perhaps the results are presented at a level too high or too broadly - and a focus on a smaller number of more specific / tested CMOs would be more effective and of use to the reader.

We added information and reorganize the paper in order to be more comprehensible and specific hoping that these modifications address these issues.

- The authors should consider how they can better set out to a reader how a realist perspective was actually used in the analysis - and work to strengthen and clarify the presentation of what they say they found.

We developed more precisely our method and results.

- They should also re-reflect on the stated strengths and limitations of the study and use this process to determine in much, much greater detail where and in what ways the data supports the findings and their subsequent recommendations and conclusions.

We hope that the added information and reorganization ease the understanding of the process of this study and analyses.

- The authors should look at how others more experienced in RE have worked to do this within journal articles.
- The thoughts and ideas set out in the protocol for this study have not been carried forward into the reporting of the analysis and findings. For example the descriptions of possible outcomes of KT (ie 3 different ways of using knowledge) are not revisited or interrogated in any detail.

Initially we planned to classify the outcomes in three categories but since it appeared that these categories of use were in fact mechanisms leading to the use of scientific knowledge, our sole outcome is the use of scientific knowledge.

- Without more detail on what the 'outcomes' specifically of TC-REG as an intervention actually were - it is impossible to have a view on whether the suggested context and mechanisms, taxonomy, recommendations, perspectives on implementation and 8 middle range theories have any meaning or use to those seeking to enhance KT in their own workplaces.

More information regarding the outcome were given in the "intervention section" and the "data collection section".

Editor(s)' Comments to Author (if any):

- Please include at least one clear limitation of the study or study design in the Strengths and Limitations section.

Done

- Please make sure that all names included in the paper are anonymised.

Done

- Thank you for providing the explanation of the authorship status in response to the concerns raised by reviewer Dean, which we shared with the reviewer. The reviewer felt partially reassured but still feels strongly that some confirmation from the authors listed on the protocol that they are aware and accept that they are not authors on the

results paper or satisfied with an acknowledgement in the current manuscript. Therefore, please can you contact the previous authors to provide assurance for the reviewer.

- Please note that although reviewer Dean gives a very favourable assessment of the write up in the paper, reviewer Johnston finds the paper very much lacking in clarity and detail. Please ensure there is sufficient information clearly provided in the manuscript to satisfy their concerns.

VERSION 2 – REVIEW

REVIEWER	Dean, Elizabeth University of British Columbia, Physical Therapy
REVIEW RETURNED	22-Jul-2021

GENERAL COMMENTS	<p>bmjopen-2020-045936R.1 - Evaluation of a knowledge translation strategy to improve policy making and practices in health promotion and disease prevention setting in French regions: TC-REG, a realist study</p> <p>I trust now that the authorship issues and discrepancies between the original protocol for this study and this final paper have been suitably and acceptably resolved to the satisfaction of all previous and existing contributors and authors.</p> <p>I remain favorably disposed to this work. I have four remaining points that are important and readily addressed, and several editorial comments.</p> <p>1. First, my comments in my previous review included: ‘There is mention of special reference to ‘cancer prevention’, here and there. But, this focus needs to be more consistently mentioned throughout the manuscript including the Conclusion. Given mention of ‘cancer’ per se does not seem to be singularly important, can the investigators not simply refer to ‘health promotion and disease prevention’ generally (I suggest in that order)?’ This point has yet to be clarified as reference to ‘cancer’ suggests this was the focus. The reader needs to understand whether TC-REG is specific to cancer in France or is this a health promotion and disease prevention in general. The issue of context is important to both contextualize the work and understand implications for meaningful generalization. Could it be said that the TC-REG intervention was developed with the focus on cancer, but has been extended to non-communicable diseases in general and can be used as a prototype for chronic disease management in general? This needs to be clearer.</p> <p>2. Second, TC-REG intervention itself warrants more detail, so that the reader can see parallels between that program and ones that may exist in their countries. Given limitations of word count in the Abstract, a few tweaks would help (and parallel edits are indicated for the body of the text):</p> <p>For example, rather than,</p>
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Intervention: the TC-REG intervention combined various activities: Supporting access to and adaptation of usable evidence, Strengthening professionals' skills in analyzing, adopting, and using the policy briefs, Facilitating the use of evidence in organizations and processes. The TCREG intervention aimed to increase the use of evidence in cancer prevention, health promotion and disease prevention in four regions of France.

Say something like:

Intervention: France's TC-REG intervention aims to increase the use of evidence in cancer prevention, health promotion and disease prevention across four geographic regions. The intervention combines various activities: Supporting access to and adaptation of usable evidence; Strengthening professionals' skills in analyzing, adopting, and using policy briefs; and Facilitating the use of evidence in organizations and processes in France.

Having made the suggested edit (which has implications for slightly re-crafting the text), I am struck by the use of past verb tense in the original statement (I have changed this to present tense in the suggested edit above, but this may need to be changed back). The authors' use of the past tense ('aimed' and 'combined') suggests that the program no longer exists. If this is true, then the relevance of this work could be questioned. I presume however that this was simply a grammatical slip given the study was conducted in the past. Even though the study was conducted in the past, it is likely that the intervention itself is still ongoing. This needs to be abundantly clear.

Finally, with respect to this point, cannot reference to cancer be removed?

3. Third, in the Limitations, it needs to be stated that the extensive transcripts generated by this series of qualitative investigations did not undergo systematic translation based on the accepted established standard, the back-translation method. This is important particularly in reporting qualitative findings because the potential for investigators' infusing bias and influencing the interpretation is high (even inadvertently). Adhering to such a method helps establish the truthfulness and validity of the verbal reports. The method requires the use of several translators equally proficient in the two languages. They first translate to the language usually English (the language of publication), and then independently translated back to the original language. The level of agreement between the original, in this case, French transcripts, and the translated back version can be quantified and an established criterion of agreement, set. Discrepancies are usually resolved through discussion and consensus. The degree of agreement and need to resolve discrepancies are typically reported.

4. Despite the value of qualitative studies, unless there are objective measures to establish the agreement of the verbal reports with reality, cannot be established. In many places, the word 'reported' would help reflect this, as we cannot be sure about the 'reality'.

Editorial Comments

Page 4 Line 10. After first use of 'knowledge translation' follow it with the abbreviation, and then use the abbreviation in the section point.

Page 7. Re:

	<p>'The aim of the TC-REG study was to evaluate the impact of this support process to influence the decisions and preventive practices in four regions of France. This study documented the mechanisms, processes, the configurations (i.e., Contexts/Mechanisms/Outcomes [CMOs]) [26] and the conditions of effectiveness established as a result of this support to ensure KT. RAMESES II reporting standards for realist evaluations were used [27].'</p> <p>I do not believe the 'impact' was evaluated. Replace with 'reported impact, and at the end 'reported effectiveness'. Impact and effectiveness are distinct to reported impacted and reported effectiveness.</p> <p>Page 9. Line 24. Change 'four region' to 'four regions'</p> <p>Page 28 (page number right hand upper corner) Line 6. Change 'can' to 'could'</p> <p>Page 9 Line 24</p> <p>The sentence states that the intervention was implemented 'differently' across the four geographic regions in France. The word 'differently' warrants explanation. Why is this being said? On what basis was this concluded? What are the implications and specifically for the design and execution of this study?</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Dr. Elizabeth Dean, University of British Columbia

Comments to the Author:

bmjopen-2020-045936R.1 - Evaluation of a knowledge translation strategy to improve policy making and practices in health promotion and disease prevention setting in French regions: TC-REG, a realist study

I trust now that the authorship issues and discrepancies between the original protocol for this study and this final paper have been suitably and acceptably resolved to the satisfaction of all previous and existing contributors and authors.

I remain favorably disposed to this work. I have four remaining points that are important and readily addressed, and several editorial comments.

We thank the reviewer for her comments and constructive remarks that helped to improve the manuscript quality.

First, my comments in my previous review included: 'There is mention of special reference to 'cancer prevention', here and there. But, this focus needs to be more consistently mentioned throughout the manuscript including the Conclusion. Given mention of 'cancer' per se does not seem to be singularly important, can the investigators not simply refer to 'health promotion and disease prevention' generally (I suggest in that order)?'

This point has yet to be clarified as reference to 'cancer' suggests this was the focus. The reader needs to understand whether TC-REG is specific to cancer in France or is this a health promotion and disease prevention in general. The issue of context is important to both contextualize the work and understand implications for meaningful generalization. Could it be said that the TC-REG intervention

was developed with the focus on cancer, but has been extended to non-communicable diseases in general and can be used as a prototype for chronic disease management in general? This needs to be clearer.

The TC-REG project aimed to assess the impact of an accompanying support process for the use of evidence by decision-makers working in prevention and public health services. The reference to the cancer prevention was actually the topic of the knowledge transfer plan and this KT could have been on another subject. Since the cancer prevention was here a gateway to a KT plan the authors didn't mention it throughout the manuscript focusing on the improvement of policy making and practices in health promotion and disease prevention.

2. Second, TC-REG intervention itself warrants more detail, so that the reader can see parallels between that program and ones that may exist in their countries. Given limitations of word count in the Abstract, a few tweaks would help (and parallel edits are indicated for the body of the text):

For example, rather than,

Intervention: the TC-REG intervention combined various activities: Supporting access to and adaptation of usable evidence, Strengthening professionals' skills in analyzing, adopting, and using the policy briefs, Facilitating the use of evidence in organizations and processes. The TCREG intervention aimed to increase the use of evidence in cancer prevention, health promotion and disease prevention in four regions of France.

Say something like:

Intervention: France's TC-REG intervention aims to increase the use of evidence in cancer prevention, health promotion and disease prevention across four geographic regions. The intervention combines various activities: Supporting access to and adaptation of usable evidence; Strengthening professionals' skills in analyzing, adopting, and using policy briefs; and Facilitating the use of evidence in organizations and processes in France.

We've made the modifications in the abstract and added modifications in the introduction and in the description of the intervention.

Having made the suggested edit (which has implications for slightly re-crafting the text), I am struck by the use of past verb tense in the original statement (I have changed this to present tense in the suggested edit above, but this may need to be changed back). The authors' use of the past tense ('aimed' and 'combined') suggests that the program no longer exists. If this is true, then the relevance of this work could be questioned. I presume however that this was simply a grammatical slip given the study was conducted in the past. Even though the study was conducted in the past, it is likely that the intervention itself is still ongoing. This needs to be abundantly clear.

The TC-REG intervention ended in December 2019, we added this information in the manuscript (at the end of "TC-REG intervention" section).

Finally, with respect to this point, cannot reference to cancer be removed?

In order to be accurate and specific the authors prefer to leave the reference to cancer.

3. Third, in the Limitations, it needs to be stated that the extensive transcripts generated by this series of qualitative investigations did not undergo systematic translation based on the accepted established standard, the back-translation method. This is important particularly in reporting qualitative findings because the potential for investigators' infusing bias and influencing the interpretation is high (even

inadvertently). Adhering to such a method helps establish the truthfulness and validity of the verbal reports. The method requires the use of several translators equally proficient in the two languages. They first translate to the language usually English (the language of publication), and then independently translated back to the original language. The level of agreement between the original, in this case, French transcripts, and the translated back version can be quantified and an established criterion of agreement, set. Discrepancies are usually resolved through discussion and consensus. The degree of agreement and need to resolve discrepancies are typically reported. Since all of our qualitative investigations were in French we didn't translate it to analyze it, and regarding the data presented in this paper it was written in French and then translated by two native translators and checked again by our native French speaker researchers. We added a mention of this translation method in the manuscript.

4. Despite the value of qualitative studies, unless there are objective measures to establish the agreement of the verbal reports with reality, cannot be established. In many places, the word 'reported' would help reflect this, as we cannot be sure about the 'reality'. We added the word reported at several sentences throughout the manuscript.

Other minor Comments

Page 4 Line 10. After first use of 'knowledge translation' follow it with the abbreviation, and then use the abbreviation in the section point.
Done

Page 7. Re:

'The aim of the TC-REG study was to evaluate the impact of this support process to influence the decisions and preventive practices in four regions of France. This study documented the mechanisms, processes, the configurations (i.e., Contexts/Mechanisms/Outcomes [CMOs]) [26] and the conditions of effectiveness established as a result of this support to ensure KT. RAMESES II reporting standards for realist evaluations were used [27].'

I do not believe the 'impact' was evaluated. Replace with 'reported' impact, and at the end 'reported effectiveness'. Impact and effectiveness are distinct to reported impacted and reported effectiveness.
Done

Page 9. Line 24. Change 'four region' to 'four regions'
Done

Page 28 (page number right hand upper corner) Line 6. Change 'can' to 'could'
Done

Page 9 Line 24

The sentence states that the intervention was implemented 'differently' across the four geographic regions in France. The word 'differently' warrants explanation. Why is this being said? On what basis was this concluded? What are the implications and specifically for the design and execution of this study?

We changed this sentence as it referred to elements presented in the "TC-REG intervention" section.