Author's response to reviews

Title: The effectiveness of knowledge translation strategies used in public health: a systematic review

Authors:

Rebecca LaRocca (burnsreb@hhsc.ca)
Jennifer Yost (jyost@mcmaster.ca)
Maureen Dobbins (dobbinsm@mcmaster.ca)
Donna Ciliska (ciliska@mcmaster.ca)
Michelle Butt (buttml@mcmaster.ca)

Version: 2 Date: 31 May 2012

Author's response to reviews: see over
The information discovered in the following systematic review should be published in the journal BMC Public Health, because of the new and valuable information that has been discovered on the effectiveness of knowledge translation strategies used in public health settings. While there is a need to build the evidence informed capacity of public health practitioners, there is no previous systematic review recently published, to our knowledge, that has examined which strategies are most effective in this population. The information found in this review will be of great value to practitioners who wish to encourage the uptake of research evidence in related healthcare settings. This systematic review is relevant to BMC Public Health as its focus is on the effectiveness of strategies used to promote the uptake of research findings into public health practice.

The authors have declared no competing interests and have adhered to the PRISMA guidelines for systematic reviews and a statement to this effect has been added in the revised manuscript. The authors have carefully taken not of the referees' comments and have corrected the grammar throughout the manuscript. We have included a point-by-point response to each of the referee's concerns below.

Reviewer's report

Title: The effectiveness of knowledge translation strategies used in public health: a systematic review

Version: 1 Date: 16 April 2012

Reviewer: Neale Smith

Reviewer's report:

REVIEW

“The effectiveness of knowledge translation strategies used in public health: a systematic review”

Background

- The background presented in this paper is generally solid. An explicit definition of KT is provided, drawn from the existing literature. The authors provide a concise articulation of the different levels at which barriers to evidence-informed decision making (EIDM) may exist... though I think they should specify that they are offering examples of barriers rather than full citation of these. The authors acknowledged that the examples of barriers were not full citation of these. (pg 4)

- They clearly indicate that effectiveness of KT strategies in public health is an area which has been little addressed, with only a single previous systematic review (now
more than a decade old) being identified. I do note that the authors’ focus is upon the use of research evidence, but they seem implicitly to use this concept interchangeably with “knowledge” more generally; a more explicit definition of their area of interest and note that scientific/research findings are one among many different types of evidence may be appropriate here. The authors acknowledged that important evidence can come from a variety of sources (pg 5). We also reviewed the document and used the term 'research' instead of 'knowledge' where more appropriate.

Methods

• There are some aspects related to study selection (pg 8ff) that I would like to see clarified. It appears initially that the authors wish to limit inclusion to studies in which KT strategies are directed to public health practitioners. It is not entirely clear through the paper, however, whether or not administrators/policymakers/decision makers are included in this group. I thought at first based on the description at page 8 that they were not, though later comments cause me to question this. For instance, the summary of the Di Noia et al piece (pg 13) notes that policy makers were one of the audiences addressed in that study; on page 21, the authors draw on other work by Orton et al which is specifically based on research with policy makers. Policy makers and administrators were added to the list of individuals that was defined as "public health practitioners" because they are important and in several of the included studies. So that the list is not exhaustive, the authors do state that the list is not limited to these individuals and that any individual whose focused on preventative care is included. (pg 8)

• More importantly, the authors describe (page 9) the type of outcomes that they are interested in as conceptual, instrumental and/or strategic use of research findings. Those are categories well-established in the literature and fully appropriate—however, I didn’t find that they actually used these when reporting on outcomes later in the paper. See for instance page 14 at line 12, where it is not clear what type of use underlines the idea of ‘improvement’. These concepts also don’t seem apparent in the data given in Tables 1 and 2, which presumably is where it should appear. Nor are they consistent with the presentation in the Results section which is organized instead around impacts on practice and knowledge. The "types of outcomes" section was reorganized. Instead of using Orton et al to describe the outcomes, the authors decided it was more appropriate to describe the outcomes as "change in knowledge, skill or practice" since this is how we had structured the rest of the paper including the outcomes tables. Examples of each are given. Using Orton et al. to define the outcomes was deemed inappropriate because they include change in attitude as a conceptual change and the review authors do not include changes in attitude in our outcomes. (pg 9)

Results

• The results are generally well-presented. The description given in the text is
consistent with Figure 1. There are some claims here which seem more appropriate to the Discussion, e.g., at page 14 where the authors suggest that change in knowledge “often resulted from more interactive KT strategies”. That claim itself seems perhaps more strong than warranted by the data, which appears to me based on 2 papers only. The authors reviewed the results section and moved claims to the discussion when more appropriate. We also made sure the wording is less strong so as to be more speculative. Similarly, I think the authors are pushing the data when they include comments such as (also pg 14) “use of knowledge brokers ... showed a trend towards a positive effect”-that seems a pretty weak claim. No changes made here. This was simply reporting results of the Dobbins et al. paper, that there was a positive trend that was reported in this study. See more on this issue below.

- I’d ask the authors to revisit the claim in the last sentence on page 15, related to the Theory of Planned Behaviour. I puzzled on this for some time and simply can’t figure out exactly what they mean to say. More information was added by the authors that described the extraneous events to help clarify. The last sentence was removed.

Discussion

This is where I have most concerns with the paper as presented, and for the most part I think this is because the authors seem to be striving to make ‘something’ out of the ‘nothing’ which they found. That is, I fully agree with their general point that there is very little literature which assesses the effectiveness of KT strategies in public health. They found only 5 articles for inclusion, after all. But given that these articles are so different from one another-in the audiences they address, in the strategies they report on, in the outcomes they address (and other factors as summarized on pg 19) -- it seems premature to reach any conclusions/synthesis from this. Rather, I’d suggest that since there is still so little literature to work with, to conclude that any strategy might work or not work is speculative at best. I don’t think the authors should feel any shame in saying forthrightly that no firm conclusions are yet to be found. The authors agree with this viewpoint which is why these exact limitations are clearly stated (pg22-24). For example it is stated in the conclusions that ‘no singular KT strategy was shown to be effective in all contexts’. It is also stated that ‘due to differing factors, including characteristics of the users, the providers, the intervention, and the organizations where the interventions may have been implemented, it is difficult to predict the effectiveness of KT interventions or suggest their effectiveness will remain constant in differing contexts’. The major limitation is listed as: 'the difficulty disentangling whether the KT strategy itself was effective or whether it was in fact the context in which it was delivered'. In addition the authors state: Due to the variability in the type of KT strategies and implementation of these strategies, as well as differences in data collection between the included studies it is difficult to estimate the magnitude of the impact. With such variation, a meta-analysis was not warranted. Therefore the following provides a narrative synthesis of the results which should be interpreted cautiously.(pg 14).

We did not add more because we felt it would be repetitive however, the authors did go
through the manuscript and made wording less strong to ensure the speculative nature of conclusions drawn. For example: One explanation ‘may be’.

- Given this, I find the efforts they make here to bring in explanations from the literature a stretch. ‘Dose’ for instance is addressed on pages 16-17, but it isn’t clear why this concept is brought forward. I’m not sure that the discussion of the Dobbins paper (pg 17) is actually talking about the concept of dose/exposure at all. This is drawn from Dobbins et al and the fact that there may have been discrepancies in the ability of the interventions to be implemented, especially the knowledge brokers. The wording was changed to ‘exposure’ instead of ‘dose’ to be more clear.

- Their comments on Di Noia et al that the use of the internet “may have increased exposure to the intervention” seem highly speculative and not directly drawn from the original study as reported. This is not drawn from the original study. This comment is the authors’ speculations as to why the internet ‘may have’ increased exposure. The authors have made this more clear.

- I think at the top of page 18 the authors need to separate concepts more clearly. It reads as if ‘passive’ and ‘simple’ are being used to mean the same thing, and ‘interactive’ and ‘multi-component’ likewise; but I think these are separable. The authors made revisions to ensure these concepts were presented as separable. Again, given that only 5 studies form the basis of the analysis, I think it is hard to claim that we’ve learned anything definitive about the relative merit of simple or complex KT interventions in affecting public health practice. The authors suggest that “certain single, passive strategies were also shown to be ineffective”—by my read that is a conclusion based upon one paper and in that sense not really the product of a systematic review. In accordance with the other referee’s comments, the authors revised and stated the actual number of studies so as to not be misleading. We feel this also helps address/clarify this comment. For example instead of stating ‘the majority of studies addressed.’ We revised to state actual numbers. For example, ‘Two of the three studies found statistically significant between group differences.’

- Finally, the authors note (pg 22) that “the quality of evidence included in this review was moderate”, but I didn’t note where in the text or supplemental files the evidence to justify this claim was reported. This is based on risk of bias tables so the authors put a note in the text to refer to those files. The authors do provide a good summary of strengths and limitations of their research (though I would perhaps challenge the statement, pg 22, that use of self-report measures is “inherent” to the KT literature, though it no doubt is common) Noted-changed to ‘common’. They suggest (pg 23) that mixed methods and qualitative designs may be useful in increasing understanding of KT in public health; I absolutely agree, though it is something they cannot comment on any further here since such study designs were screening out of the review. They were screened out of this review but may be valuable future research considerations which is stated in the text.
Reviewer #2 report

Title: The effectiveness of knowledge translation strategies used in public health: a systematic review

Version: 1 Date: 10 April 2012

Reviewer: Laure Perrier

Reviewer’s report:

The authors have selected an intriguing topic and done an exhaustive job of searching the literature. This manuscript requires some revision and could be strengthened with additional detail to the methods, along with attention to grammar and writing for the whole manuscript. In particular, the authors should consider:

Major Compulsory Revisions

Overall

1. It is suggested the paper be edited for grammar and writing.

• The grammar in the manuscript needs attention. There are numerous errors - one example is the over-use of semi-colons.

• The writing in the manuscript also requires attention. One example is the over-use of ‘however’: e.g. In Background: 1) “However, translating best available research evidence into programmatic change is a complex process.” - Consider using: Translating best available research evidence....; 2) “However, this is the only related systematic review....” - Consider using: This is the only related systematic review....

The above revisions were done and the authors edited the entire manuscript for grammar and writing.

Background

1. Last paragraph: The first 3 sentences need to be re-considered - These are taken from a talk and speakers can make bolder statements (e.g. keynote speakers are meant to offer provoking ideas; an audience can more easily distinguish they are giving an opinion, etc.). Manuscripts do not offer the same context and must rely on factual statements that can be referenced with evidence. Keynote speakers comments taken out as the reviewer indicates.

Methods: Data Sources
1. Second paragraph:

- Ciliska is identified as doing the environmental scan but is not an author on the document referenced. Reference corrected.

- Environmental scan document (Anderson 2009) does not describe Methods.

For this reason it is not possible to: 1) determine if the scan was exhaustive; 2)determine if the scan included searches of literature databases that would include research studies (9 of the 10 documents identified were reports) - Based on this, it does not support the statement that there are few high quality intervention studies. The authors kept information about the environmental scan in and re-worded to state that the related literature found was lacking instead of "few high quality intervention studies were found" as to not be misleading.

Methods: Data abstraction

1. Clarification and more details are needed in the reporting of the first level of screening:

- Did agreement need to be reached before the full-text was retrieved? The screening process was clarified by stating: Studies deemed to be potentially relevant by either reviewer were retrieved for full-text review. The full-text was assessed for relevance independently by two reviewers: the primary reviewer (RL) and the second reviewer (JY). Agreement needed to be reached by both reviewers for inclusion and quality assessment (pg 11).

2. At the second level of screening: What was done if there were discrepancies in agreement? Does the statement at the end refer to data abstraction only, or to screening as well? It is now noted that the process used for discrepancies (which was described) was used at all stages:

Disagreements that occurred during the screening or data extraction were discussed until consensus was achieved. A third reviewer was consulted if consensus could not be reached (pg 12).

To add more clarity the authors also divided this section into: Screening, quality assessment and data extraction and provided details for each.

3. The authors state in the manuscript they followed Cochrane methods: If this is the case, data abstraction should be done independently by 2 reviewers rather than one person doing data abstraction and having a second person verify the information. Given there were only five studies included, this seems feasible to implement. Unfortunately to expedite this phase of the project the authors decided to only use one reviewer to extract data and had a second reviewer check all of the data.

Discussion
1. The first two sentences lack impact due to poor writing. Writing requires more attention here to make the Discussion more effective. Revised as the reviewer indicates.

2. Exposure or ‘dose’ of the intervention: If the section is about the length of the intervention, it is best to simply state this, e.g. Exposure or length of intervention. If ‘dose’ requires quotes, this creates confusion with regards to its meaning. Also, ‘dose’ is inconsistently written with and without quotes. This is also addressed by the other referee. The word ‘exposure’ instead of ‘dose’ seems to be preferred and therefore revised as the reviewers indicate.

3. Hanbury study: Authors state the educational session “…appeared to be only one day in length”. This information needs to be verified before reporting on it. The intervention was confirmed by the author to be one day in length. Wording therefore changed as the reviewer indicates.

4. “Internet….allows providers to study independently, for little or no cost”: This statement needs a reference. Online courses can have registration fees that may not universally be considered ‘little’. This was removed as the reviewer indicates.

Funding

1. This should be reported by the authors Funding reported as the reviewer indicates.

Flow Chart (Figure 1)

1. Give reasons for why 341 articles were excluded, e.g. not a relevant population, etc. The reasons were added to the manuscript: The most common reasons studies were judged as not relevant were that the intervention was not a knowledge translation strategy or information on relevant outcomes were not reported (pg 12).

Minor Essential Revisions

Background

1. The reference to the Stone and colleagues systematic review: Consider being more explicit about this review, e.g. that their findings are within the context of screening related to immunisation and cancer. Without this contextual information, referring to this review is confusing and less convincing that the current systematic review is unique and necessary. Contextual information added: Stone and colleagues evaluated KT intervention components classified as provider reminders, provider feedback, provider education, provider financial incentive, and organizational change to increase screening related to immunisation and cancer. (pg 5).

2. Italicize and use quotes for CIHR definition of knowledge translation Done

Methods: Data Sources
1. Second paragraph:

- For the statement, “...in accordance with experts...”: The experts need to be identified - it is unclear if the authors mean Ann McKibbon here. Done

- There is a focus on Canadian references and context. If the authors wish to focus specifically on the Canadian context, this should be stated explicitly. No the statement regarding 'Canadian' facts were removed so as to not focus specifically on the Canadian context. The other references are global based and therefore remain in the manuscript.

- If the focus is global, references supporting this need to be offered. The manuscript is more novel and impactful if presented within a global context. References related to global context are included.

- EPOC should be identified as a group within the Cochrane Collaboration. Done

3. Identify what is meant by ‘limited results’ when searching trials registry at EPOC, e.g. list the number of citations. Unfortunately this number was not recorded, however the authors have expanded on what was meant by 'limited results'.

4. The methodological filters from EPOC are not validated - since they are no longer available on the EPOC website this suggests they should not be used. Methods filters were available at the time this search was done. They are included in the search as an extra file.

5. The “online registries of research relevant to KT or public health” need to be identified. Done

6. The conference proceedings, etc. need to be identified, e.g. which sources? Done

7. The date the search was conducted should be reported. Done

8. The manuscript states the Cochrane Library was searched. The Additional Files list only the Cochrane Systematic Reviews were searched. This needs to be clarified and reported consistently. Done

Methods: Types of intervention

1. For the statement, “...building evidence informed capacity”: This needs to be defined. Concept defined as the reviewer indicates (pg 9).

Methods: Types of participants

1. ‘Dietician’ is spelled incorrectly (correct spelling = dietitian) Spelling Corrected.

2. It is unclear if the definition of public health is taken directly from the Public Health Agency - if it is, consider italicizing and using quotes for this definition. It is paraphrased and therefore referenced.

3. The wording is awkward for the inclusion and exclusion criteria. It is much easier for the reader to simply state, ‘the inclusion criteria is...’, and ‘the following were
excluded......’Revised as the reviewer indicates. However, this section could not be as point form as requested. The public does not often know who public health practitioners are (as seen in the first reviewers’ comments) and therefore more detail was required in this section to provide clarity.

Methods: Data abstraction

1. It would be helpful to know more about the tool from Effective Public Health Practice Project - For instance, was it pilot tested? Why was it chosen as useful to this project?

   Information added related to the tool: The data extraction tool has been pilot tested and refined over use in more than 20 reviews (pg 11).

Results: First 2 paragraphs

1. For the statement, “The systematic review by Stone...was not included”: Systematic reviews were not listed as eligible in the inclusion criteria for study types See Pg 10- Relevant systematic reviews were also included.

Results: Participants and settings

1. For the statement, “All five studies were in English”: The review only searched for research literature in English so no other languages are eligible This statement was removed as the reviewer indicates.

2. Using the word “majority” is misleading, e.g. “The majority of studies....” and “The majority of strategies...”. Given that five studies are being described, it is best to state the actual number, e.g. “Three of the five studies were targeted at.....” The authors revised this by using the actual number of studies throughout the results and discussion.

Results: Change in practice

1. For the statement about the Dobbins study, “...improved significantly from baseline to follow-up”: It would be helpful to readers to describe what this improvement was (e.g. decision-making) within the first or second sentence to provide clarity. Sentence added to provide clarity: This study evaluated the effectiveness of KT strategies on evidence based decision making and the number of public health policies and programs implemented which documented the inclusion of research evidence pg (15).

2. Time series analysis: This needs to be explained more clearly - The last two sentences describing local and national event are written as though the reader has prior knowledge or more insight into the study than would be expected More insight and description added related to the extraneous events.

Discretionary Revisions

Abstract

1. Consider listing data sources in abstract Listed as the reviewer indicates.
Methods

1. It would be helpful to readers if a tangible example be given for instrumental change and strategic change. This paragraph was restructured as per referee #1 comments. Instrumental and strategic change taken out but examples were added by authors to describe each outcome (change in knowledge, skill and practice).

2. Given the content of the studies identified, the EPOC Risk of Bias Tool may have been considered for quality appraisal of all the studies rather than just the one interrupted time series study. The authors disagree. We felt that Cochrane’s risk of bias tool for RCT’s was most appropriate for RCTs included in this review and were most comfortable/experienced with using that tool. EPOC’s tool was used for the ITS design because it took into considerations related to ITS designs that would have been not applicable to the RCTs.

Discussion

1. Internet should always be capitalized. Done

Conclusions

1. 1st paragraph: A recommendation to consider is that a realist review may provide insight into the examination of this question, e.g. a realist review is aimed at discerning what works for whom, in what circumstances, in what respects and how (see Pawson 2005 J Health Serv Res Policy). Thank you-viewpoint added.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests