Reviewer’s report

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

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Reviewer: Neale Smith

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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. 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.

Methods
As described, the authors appear to have adhered to the main methodological precepts underlying systematic review and synthesis. The search strategy is explained in full and justified; they have undertaken both to search electronic databases and hand-search of references. It appears that a library professional (Dr McKibbon) was used by the research team to develop their approach. Multiple reviewers have been involved in determining both inclusion, and data extraction (pg 10). They have employed instruments that have been previously found useful.

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/policy makers/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. 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.

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. 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. 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.

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.

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. 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. I’m not sure that the discussion of the Dobbins paper (pg 17) is actually talking about
the concept of dose/exposure at all.

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 ‘inter-active’ and ‘multi-component’ likewise; but I think these are 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.

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.

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). 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.