Author’s response to reviews

Title: Knowledge translation in health: how implementation science could contribute more

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The revision note has been attached as a document. It is copied and pasted below.

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Knowledge translation in health: how implementation science could contribute more Michel Wensing, Ph.D.; Richard Grol BMC Medicine

We thank both reviewers for their helpful comments. Their reviews have been copied below and our (numbered) responses have been added. Although the comments were labelled as minor, we have substantially revised the manuscript in the light of the reviews. Finally, we have clarified and elaborated some sentences throughout the manuscript.

Reviewer #1 (Carole Estabrooks)

The authors have written an opinion piece on a significant issue in healthcare research and within health systems. I have been in the knowledge translation/quality improvement field for over 20 years and share many of their observations. Each of the key issues they raise is readily evident if one steps back and assesses the extant literature both theoretical and empirical. In sum I am highly enthusiastic that this paper or a modification of it be published. Because these are extremely senior and well respected scientists in these fields, the likelihood of impact is increased. However I think that to maximize the impact some additional work is required.

They make what could be a clarion call for change - and while I concur 100% its impact will be strengthened with some modifications. Some examples of how it might be modified ....

* The stakeholder section was not as fully developed as it could be, even in a short opinion piece. Stakeholders are a wildly heterogenous group, from multiple jurisdictions, groups,
populations, levels. This was not reflected nor those groupings even at a high level identified, e.g., patients/citizens (and how the move we read about in the "helix" literature lays out the societal changes that have led to calls for the democratization of knowledge. This underpins much of the patient engagement movement. There are rich and substantive literatures on Modes 1, 2, and 3 discovery that could perhaps be at least referred to.

Response 1:

It is good to know that this experienced academic in improvement/knowledge translation shares many of our observations. Both her suggestions (mode 2 knowledge generation; stakeholders are heterogeneous) have been added to the section on stakeholder involvement.

* I suspect the partnered research agenda implied in some of the "Mode" work could is a larger issue than has been identified, called integrated KT in Canada, this "movement" is as they suggest met with high levels of enthusiasm and near evangelism in some circles - but robust evaluation is nearly absent ... this work takes much time and resources applied over time. We ought to be rigorously examining if it matters when we work this way.

Response 2:

Thanks for the suggestion. We have added a sentence on the results of a literature review of research on integrated KT, which supports this view and our opinion (Gagliardi et al. 2016; ref 20).

* The theoretical frameworks, etc., section could be enriched somewhat. No mention is made of for example, Rogers' globally influential work and how if one just but reads it in its iterations one would know that little new is being presented in many areas. More might be said of the abject failure to test theories or to even demand they be testable. Or that they actually have theoretical underpinnings.

Response 3:

The section on theories now starts with the observation that a few theories, such as the Diffusion of Innovations theory by Rogers, dominated the field in the past (with a reference to an historical bibliographic analysis by this reviewer, ref 15). We would qualify the willingness to test conceptual ideas among researchers as mixed, and have included this in the section. We also added the observation that many frameworks lack explicit links to higher-level theories.
* The call for programmatic research could offer more with respect to what that really might look like and whether our current funding structures offer any hope of supporting it. For example, such work will require longitudinal support of emerging and successful team (with proven ability over time to deliver) over time - with clear accountability structures. Some national funders have very low thresholds with respect to accountability and delivery. This is not easy to achieve.

Response 4:

Academic infrastructures, incentives and cultures vary across the world, but we have tried to describe a number of generalizable yet specific recommendations for programmatic research at the end of the Discussion section.

* In fact funding and academic structures should probably receive more attention. Career Universities have barley nudged a needle on changing any of their tenure and promotion practices - centuries old frameworks persist. And those are potential powerful instruments for change - as is substantial funding. Training is a major component of this and if this were just a bit more elaborated would make a richer paper.

Response 5:

In the final sections of the Discussion, we have included some more text on accountability and recognition of the performance of research on improvement and implementation in healthcare, which probably generalizes to applied health research generally.

* Some additional international examples of some of their points would be useful and broaden the perspective, I think, of the piece and may increase appeal.

Response 6:

In the introduction, we presented research on the Quality and Outcomes Framework in general practice in the United Kingdom (a pay-for-performance reimbursement) as example of a study in the field. Much of the remaining text is critical about the research enterprise, so the inclusion of specific examples would imply that single studies are used for demonstration of our critique. It is not our intention to critique specific studies or researchers, and this may distract from the main messages. One way around this issue might be to use our own studies as examples, but this feels much like self-promotion, even if we would highlight the limitations. We have done our best to include small examples throughout the manuscript, but we would prefer to refrain from the inclusion of more elaborated examples.
As this will be read by many with no implementation or improvement orientation or experience, it might be warranted to offer up some of the standard (and sometimes quite compelling if not well supported by hard empirical evidence) "lead in" about why we think it really rally matters if we right this ship. There are numerous reports and some well cited papers.

Response 7:

We completely agree that the rationale for the implementation and improvement research in health needs to be clarified for a broad audience, but we felt that the societal, practical and scientific relevance of research on health improvement and implementation had already been sufficiently pointed out at the start of the manuscript. We have now added one sentence to the introduction to state explicitly that calls have been made to harness the field (with a reference to Eccles 2006). We are aware that many other reports and papers with related messages exist, but we did not want to overload the manuscript. The number of literature references has substantially increased as a result of the revisions. If the editor feels that the claims need to be better referenced, we are happy to add more references.

I wondered if it would be useful to offer more clarity on terms in the field, could be presented in a box for example so as not to lengthen the piece too much.

Response 8:

Boxes are not possible in BMC journals, as far as we are aware, so we added a few sentences with elaboration of different fields in the introduction section of the manuscript. There are obviously many more terms in the field (e.g. implementation interventions, barriers and facilitators for implementation), but it is beyond the scope of this contribution to elaborate on those. Some of the cited papers (e.g. Graham 2006, Theobald 2018) provide clarification of some of the terminology.

It is an opinion piece and should not descend (or ascend) to dizzying depths or heights of theorizing, not by any means. But to be received with the weight and impact by the science community that it deserves, some more underpinning and nod to substantive areas of theory and schools of thought may be warranted.

This is an important perspective at this time. It is provocative and maybe would benefit from being even more so.
Response 9:

Thanks for the encouragement, which we used to extend and sharpen the discussion section, and particularly its last paragraphs.

Thank you for the opportunity to review this manuscript.

Reviewer #2 (Tracy Finch)

This paper represents a commentary on the status of the field of implementation science, and a call towards activity within the field that will make implementation science more impactful in terms of changing (improving) healthcare practice.

* The paper refers to mixed and overall moderate impacts of implementation interventions - the papers cited here are from 2003 and 2012. Although I don't disagree that this point is likely to still hold, I wonder if there are recent examples that could be cited that show a promising move in the right direction? I am a co-investigator on a project that has developed an implementation intervention and is rigorously testing this for impact (uptake and spread of online mental health services), using trial methodology. For example, in relation to Table 1, our study (see [www.implementall.eu](http://www.implementall.eu)) is designed to address all 5 issues to an extent (arguably, only not fully in relation to row 4 - evaluation of outcomes - if you mean 'health' outcomes - as we are evaluating impact for implementation related outcomes). I am also involved in another project that has developed and is evaluating quality improvement packages of interventions for tracheostomy care in England (see: [https://www.health.org.uk/improvement-projects/improving-tracheostomy-care-national-quality-improvement-programme](https://www.health.org.uk/improvement-projects/improving-tracheostomy-care-national-quality-improvement-programme)). Using rigorous, mixed methods design with implementation related measurements over time, collaboration based implementation interventions, and data collection that includes patient outcomes and service delivery data. While neither of these projects have results or published outputs as yet, there may be others that could offer examples of the intended direction of travel as set out in the paper. Showing some examples might help extend the arguments beyond what is (currently) not being done and what needs to be done, to ways in which it can be achieved.

Response 10:

Let us first state that we appreciate the efforts of researchers and the value of specific research projects in the field, also in recent years. Our analysis and the proposed ways forward focus on a higher level than single studies, which might be exceptions and not necessarily indications of new trends. Our analysis also covers recent years; the various revisions have led to the addition
of some recent references. In our view, the newest literature (e.g. after 2012), is consistent with the older literature and does not overwhelmingly show improvement regarding the highlighted aspects. As a response to the comment, we have included a few recent examples of programmatic research in the discussion section, as these seem a promising step forward (references: Ivers & Grimshaw, and Dixon-Woods). We have also mentioned recent developments in the area of implementation outcomes (see response 12)

* 'Misfit between problems and approaches' - the points resonate well with my experience of implementation research over almost 20 years. I think one of the issues to address (or at least acknowledge) is that implementation science focused research needs to become more 'real world' (practice based), and for ways to be developed that allow practitioners (through capacity building; changing attitudes/values towards the work involved in implementing change) to inform better fit of problem and approaches. Implementation Science provides evidence of approaches and tools that can help, but the link still needs to be developed in terms of the role of practitioners in this process. Again, there are some positive moves in this direction - some of the toolkits and 'translation devices' that have been recently published (eg 'how to pick a theoretical approach?' - see Lynch et al 2018 for more end user targeted publication [1]) and Birken et al [2] for more structured comparison and selection amongst theories) are (I believe) starting to address this gap between the (IS) knowledge holders and the 'doers'. Perhaps referring to examples of positive developments may help strengthen the arguments here.

Response 11:

This is an interesting comment, because it seems to reflect differences between researchers and perhaps between different parts of the world. In our part of the world, many researchers of improvement and implementation are actually close to the ‘real world’ (e.g. as clinicians), but not well aware by theories, frameworks, and methods in the field of quality improvement, knowledge translation and implementation science. In some other parts of the world, it seems that many researchers in this field are familiar with theories and frameworks, but less with healthcare practice or policy. Some sentences to elaborate this point have been added in the section on concepts, theories and frameworks.

* 'sub-optimal effect measures' - I would agree that, in many studies, effect measures are inadequate or inappropriate to the outcomes being studied. The table suggests a need to design and validate a new generation of outcome measures. Two points are worth considering here. Again, I think there is already evidence of a move in this direction, both in terms of building a compendium of suitable (and with sound scientific properties) outcome measures for implementation research - see Lewis et al [3]; and in terms of developing their
use so as to fit better with change efforts in real world healthcare settings - see Glasgow & Riley [4] 'pragmatic measures'.

Response 12:

It is fair to say that recent and ongoing work focuses on the pragmatic use of outcome measures in implementation research, which we have now added in the section on outcome measures. However, this work does not address the concerns expressed in the section, so we kept it largely as it was.

An important point of clarification that is required for the paper concerns the category of outcome measures that the authors are referring to in the arguments for improvement. Picking up a point above, in Table 1, and checking the text for the subsection 'decreased appreciation of rigorous designs for effect evaluation', it's not clear whether the authors mean rigorous evaluation of outcomes in terms of clinical/health related, implementation outcomes, or both (and it may be useful to refer to Proctor's 2011 conceptual framework [5] for outcomes in implementation research).

Response 13.

The section on designs for effect evaluation focuses on study designs, but we understand that the choice of outcomes may be seen as part of study design. Therefore we have included a sentence that outcomes are discussed in the next section on outcome measures. The section discusses our concerns regarding outcomes measures.

I think my overall comment is that, while the paper sets out challenges that I recognise (as an implementation scientist), and agree should be advanced through more thoughtful and rigorous research, my outlook may be somewhat more optimistic. I would like to see examples of how these issues are already starting to be addressed.

Response 14.

While this paper is critical, we do intend to enhance the field in a positive way and we therefore include constructive suggestions alongside critique. We very much tried to keep a positive tone throughout the manuscript.