Reviewer’s report

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

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Reviewer: Carole Estabrooks

Reviewer's report:

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.

* 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.

* 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.
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.

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.

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.

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.

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.

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.

Thank you for the opportunity to review this manuscript.

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