Reviewer’s report

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

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Reviewer: Tracy Finch

Reviewer's report:

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) 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 ), 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.

'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.

'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'.

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).

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.


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