Reviewer's report

Title: The sustainability of new programs and innovations: A review of methods and findings

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Reviewer: Trisha Greenhalgh

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First, let’s begin by acknowledging that sustainability is really hard – perhaps impossible – to study systematically.

Second, let me declare an interest: I study sustainability myself and am currently writing up a large empirical study using a new(ish) theoretical approach. That work isn’t yet in the public domain (because I haven’t submitted it yet), and I am therefore somewhat limited in what I am prepared to share. My own approach to sustainability is based on a systematic review already in the literature, along with a ‘grey literature’ source (a PhD thesis actually, which I found on a systematic search), neither of which these authors cite, and differs substantially from the one taken in this paper.

Specifically, in my own paper in preparation, I draw on the work of others to argue that there’s not much point using a mechanical ‘logic model’ to assess sustainability as this implies that the pinnacle of success is for the intervention to be maintained exactly as implemented some years ago, whereas any ecologist will tell you that this is the quickest way to become obsolete and then extinct! (I do acknowledge that they refer briefly to literature which challenges this model, but they don’t engage with that literature). I believe there are real dangers in creating a ‘hierarchy of evidence’ in sustainability research which naively mirrors the ones in widespread use in clinical epidemiology. I can see from the reference list that Implementation Science as a journal has already nailed its colours to the mast and that this paper isn’t making any assumptions which haven’t already been endorsed by the editors and the authors of several recent papers. But I profoundly disagree with this stance. For me, sustainability is more dynamic, more nuanced, more contextual than these authors appear to recognise.

With that background in mind, I had a lot of difficulty with this paper because I think it rests on flawed conceptual assumptions. If the assumptions are correct then they’ve done a very competent study – they’ve systematically identified lots of studies of “sustainability” and “durability” and used a robust and reproducible process to decide whether the original intervention continued in the form originally envisaged by authors. And they’ve classified the ways in which that was assessed, privileging (rightly) objective over subjective methods. But since I don’t agree with the underlying assumptions, I have problems with the paper.

Detailed points
Abstract: “more conceptually-driven research” – not clear what this means. I would have said it’s the conceptual stuff that this paper itself is weak on.

Page 4 “Sustainability must be treated as a distinct outcome” – I would say this kind of language is part of the problem, not part of the solution. I’d suggest that sustainability sits in tension with adaptation and embedding, and therein lies the key research agenda!

Page 4-5 The whole of this introduction suggests that the optimal achievement is for an intervention to be maintained without adaptation. This is not what the background literature suggests is necessarily the case. I challenge these authors to defend themselves against the charge of ‘logic model bias’.

Top of page 8 – referring to ‘factors’ and suggesting that each of these factors needs looking at. My reading of the wider sustainability literature is that it’s moving from a rather static focus on ‘factors’ (which was certainly where the interest was a few years back) towards a much more dynamic study of how different influences interact with one another and change over time, and also towards studying the things which are difficult to report (such as local micropolitics).

Page 10 – literature review search strategy seems to have been good for identifying empirical studies on sustainability and related concepts, but they didn’t describe how they identified the theoretical literature in the background section. Was this why they seem to have missed some key theoretical papers?

Page 11 inclusion criterion “factors associated with the persistence of the implementation, whether or not the primary focus of the article was sustainability”. This I think is why they ended up producing a list of factors rather than insights on how ‘factors’ play out dynamically in context.

Page 13 – an old bugbear of mine: high inter-rater agreement doesn’t mean the codings are correct, merely that both ‘independent’ assessors approached the literature with the same assumptions. But again, I recognise that this ‘objective’ approach is absolutely de rigeur for Imp Science so is ‘in paradigm’.

Page 13 – please don’t express proportions in percentages when the denominator is less than 100. It conveys a spurious level of accuracy and is bad Latin. So 5% of 61 is how many studies? Easier to say three, surely?

Page 15 “Only ten studies reported on the quality or level of fidelity of implementation, and only eight of these used a form of independent observation and assessment to evaluate sustainability outcomes.” This seems to me to be the nugget in this paper. It resonates very strongly with the empirical study of sustainability which my team are currently writing up. It also links to a most interesting and valuable literature (some of which they cite) on maintaining fidelity of an intervention in the long term. Whereas programme sustainability is an inherently problematic concept, IF a particular intervention is seen as worth maintaining, it should surely be maintained in a way that retains its fidelity. I
would reflect on this finding, dig out more detail on it, and expand discussion on it. Could for example link to the findings at the top of page 19 on processes such as training.

Page 17 “Several of our findings were surprising. First, given the emphasis on implementation in the fields of health care and mental health, relatively few studies in these areas examined sustainability.” It reads as odd that they found it surprising since they begin by citing my own team from 2004 observing that hardly anyone studies sustainability!

Page 19 – “culture embedding mechanisms” sounds like a mixed mechanical and organic metaphor which would benefit from unpacking.

Page 21 – “research to discern the relative importance of specific aspects of differing factors for differing innovations and settings is particularly needed”. Ideally yes perhaps, but I think this is another example of the fruitless pursuit of ‘factors’.

The findings seem to skirt round all the things we know in our bones are important in sustainability, such as relationships, energy, commitment, goodwill, values etc. I wonder why that was – must be an artefact of the research approach in both the primary studies in their dataset and then in their method of analysis.

An arguable omission in the whole paper – the link between sustainability of a programme and sustainability as in the ‘green agenda’. See Dave Pencheon’s work on this. He links the two.

The diagrams are really nice and clear.

What to do about this paper? I would think really hard about the assumptions, articulate them explicitly, and acknowledge in the discussion that the conclusions are only valid if the assumptions hold.

Happy as ever to see a revision

Trish G

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

No competing interests except I am writing up an empirical study on sustainability
which takes a very different approach.