Reviewer’s report

Title: Learning as an outcome of involvement in research: What are the implications for practice, reporting and evaluation?

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Reviewer: Patricia Wilson

Reviewer’s report:

I enjoyed reading this commentary which explores a currently contested area in patient and public involvement - whether and how the impact of PPI should be evaluated. I think the paper has the potential to make a very valid contribution to the current debate. However, while I acknowledge the authors’ note on p. 25 that the commentary is based on their experiential knowledge rather than theory or evidence, I think that some of the arguments need firmer underpinning to make a more plausible case. In particular:

1. Abstract, first line. I am not convinced that public involvement emerged coincidentally at the same time as evidence-based medicine. I would suggest that both arose as a response to the decline in public trust in medicine influenced by a number of medical scandals and failures.

2. The commentary fails to acknowledge that public involvement is also a policy response rather than just being seen as an intervention. Evaluating PI as a policy programme brings an alternative viewpoint that is not included in the commentary, leaving it as a binary discussion (which I acknowledge may be the intention but does not capture the full spectrum).

3. The commentary starts by critiquing the attempts to evaluate PI through reductionist methodologies such as RCTs. However, only a couple of examples are given and I was not convinced by the arguments that such approaches to evaluate PI are widespread. Indeed, I think that there is now increasing acknowledgement that other methodological approaches are more appropriate to evaluate multi-factorial programmes. Nonetheless, I also think the commentary does make a very good and clear point that it is the conceptualisation of PI as an intervention that they argue is most problematic.

4. While some outcomes (and the list is certainly not exhaustive) of PI outcomes are presented, I would find it useful if the authors defined what they understand as impact, as outcomes and impact appeared to be used interchangeably. Having a more comprehensive understanding of impact beyond instrumental impact, would also address some of their concerns discussed in the paper.

5. The other area that is argued against are reporting standards. Again, I think this case needs to be strengthened as only research from the reductionist paradigm is alluded to, whereas reporting standards have also been developed, for example, for qualitative
research (where objectivity and replicability are not the focus), and realist methodologies. If the authors' are trying to argue that the GRIPP2 checklist fails to address subjectivity and the researcher's journey, then this case could be made clearer.

6. Again, while acknowledging that the authors are drawing on a selected amount of literature to support their arguments, I think much of the case they are making was found in the RAPPORT study, including: 2 way learning (p5); need for training (p15); whole team involvement and sign-up to PI (p16); and the need for synergistic relationships as the foundation for PI (p17). Indeed, this latter finding from RAPPORT influenced the Going the Extra Mile report (http://dx.doi.org/10.1136/bmjopen-2017-017124), and as this is supportive of the authors' arguments I was surprised it wasn't cited.

7. The writing is clear and understandable, and as stated earlier a pleasure to read. However, I note that the Journal's guidelines say that a commentary should be short. I think that this paper would merit some editing down, it has some repetition and could be punchier in places.

I hope my comments are helpful and I would encourage the authors' to tighten up aspects of the commentary as it would be good to have this viewpoint published!

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