Reviewer's report

Title: Developing a programme theory to explain how primary health care teams learn to respond to intimate partner violence: A realist case-study

Version: 4  Date: 29 October 2014

Reviewer: Pat O'Campo

Reviewer's report:

Major compulsory revisions:

The authors have been moderately responsive to the reviews. For some requested revisions, avoiding repetition, clarifying some issues about the intervention, the authors have been responsive. However, many issues remain outstanding, especially about the realist approach to the study. Some points which remain unaddressed will be raised again in an attempt to be clearer about the points needing modification. On that point, I wonder if the realist component of this evaluation was a subset of the overall research project and that what is reported in this paper is a mix of the realist findings and other findings (e.g., process evaluation). If this is the case then perhaps the authors could focus the paper even more on the realist methods and findings of their research. See specific comments below on the realist aspects of this study that could be further improved.

1. The results in the abstract are focussing on program activities rather than on emergent mechanisms or demi-regularities. What changes are being brought about and how are they being brought about?

2. The authors are talking about IPV responses at the country level yet the study itself is of a single clinic meaning that findings will not be generalizable to the region not to mention the country. I think that this particular mismatch—having the background focus on countries but the evaluation happening to a single clinic—requires more attention in the revision of the paper. One suggestion is to drop, for the most part, a focus on the issue of intervention at the country level.

3. Related to this issue is that it is not clear whether “the case” is the IPV activities for the region of Murthia which is described in depth in the section entitled “the case” or whether the case is the single clinic, “La Virgin”. If the latter, which seems to be the focus of data collection and analytic efforts, then again the authors should minimize the long description of the region. If the case is the region, then the authors should discuss the limitations of having only one clinic in the region and how that impacts findings about the region.

4. In fact still too little information is provided about LV to fully understand what “activities” and “strategies” are actually being evaluated. For example LV has implemented “malaise” meetings but we have no information on what proportion
of clinicians in LV attend regularly. Similarly, other activities are not described with sufficient detail to enable the reader to understand the specific activities nor the ‘reach’ of these activities.

5. The authors claim that their initial program theory is the typical way that program theory is developed in realist evaluation. The authors do not address the critique that actual mechanisms (the critical ingredients of a realist program theory) is missing altogether from what they present. A strategy or program activity (e.g., PHC team learning, IPV management, training, information systems), which appears in abundance in the PT, are not mechanisms (Wong et al. 2013; Pawson and Tilley 1997). It would be useful for the authors to re-review Chapter 6 in Pawson and Tilley as that chapter contains several examples of mechanisms, how to generate them from evaluation data and how to link them to program theory. Moreover, increasingly there are other citations that are useful guides to ensuring that mechanisms are uncovered such as deSouza 2013, Bygstad & Munkvold 2011, just to name a few. Bygstad & Munkvold even have a framework and outline steps for analyzing realist data to reveal mechanisms (see page 5).

6. The methods do not seem to match the realist case study approach. For example, Appendix 2 data collection questions/approach seem more like a process evaluation rather than instruments designed to promote the development of or test a program theory. No questions are about mechanisms or theory, rather they are about strategies or process (e.g., sections 1 and 2, Appendix 2). Relatedly, if the authors are trying to generate CMOs it is not clear why C’s and O’s are separated from Ms (section 5) since mechanisms are very specific to outcomes. It would seem that the evaluation, if trying to develop realist theory, should focus on the questions in section 5 as those are closest to generating information about mechanisms. These questions seem to be attempting to capture what is changing and seem wholly appropriate to what the purpose of the study seems to be.

7. Why was thematic analyses undertaken? Were the themes specific to the activities outlined in PT1? Given that this is a realist evaluation, I would have thought that the authors would have described how they analyzed the data to obtain information on “unseen” mechanisms (e.g., see deSouza 2013 and Bygstad and Munkvold 2011). Rather the analysis seems more like a typical qualitative data analysis.

8. The results are not presented in a way that either represents what the authors say they are searching to report (CMOs) nor a realist orientation to the language used to report their findings. As just one example the authors note on page X: In examining mechanisms that operate at the micro-to-micro level, we found that pre-existing relations, team culture and the overall organisational culture were important elements for generating team learning and team work. Personal interest in the topic was a key individual factor shaping responses.

What is an element here? Team culture is not a mechanism because
mechanisms talk about the change brought about by program activities and team culture or ‘overall organizational culture’ or “personal interest” alone are not mechanisms. Where are the “how” and the “why” in their descriptions? So what does an ‘element’ represent in the realist scheme? If we take Bygstad & Munkvold’s perspective as summarizing the perspective of those realist who write about mechanisms, they note that “the concept of emergence is central to the workings of mechanisms: it is often a combination of objects that will trigger a mechanism, and produce an outcome that is dependent on, but not reducible to the objects.” (page 5) but this conceptualization of mechanisms is not present in this paper.

This style of presentation continues throughout the results yet no where are mechanisms consistent with realist approaches presented. Rather activities and strategies are described but no “triggers” for specific outcomes. No generative mechanisms seem present in their findings.

9. While the authors cite Yin, they seem to have not reported on key aspects regarding rigor emphasized by Yin such as the issue of triangulation and convergence of evidence as just two examples. Thus we are unable to assess the strength of this particular case study.

10. Some statements in the discussion seem unsupported by the results. For example, the start of the Discussion states that “La Virgen has managed to engage a sufficient number of members actively responding to IPV” where were these data reported in the results? The discussion seems to read more like report of process evaluation rather than on realist evaluation findings.

11. The authors present Figure 2 (PT2) at the start of the results and it is not clear if this is their refined theory that they obtained as a result of the findings or as they state an illustration of how mechanisms can operate at different levels and if the latter then how this Figure relates to the findings. Moreover, additional confusion around PT2 arises from the authors response to reviewer comments about PT2, specifically that “PT2 is a better starting point for a full scale realist evaluation than PT1.” Yet no mention about this idea is presented in the paper. Is PT2 or PT1 the starting point for the realist evaluation? At the very least the role of PT2 should be clarified.

Archer MS: Realist social theory: the morphogenetic approach. 1995 Cambridge, UK: Cambridge University Press


DeSouza D., Elaborating the Context-Mechanism-Outcome configuration (CMOc) in realist evaluation: A critical realist perspective. 2013, Evaluation 19(2) 141-154


**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:**

I declare that I have no competing interests