Major compulsory revisions

1. The authors could give more attention to the premise/theoretical framework of the [G]OLD study, either in the Introduction or in the Methods section. In addition, the philosophy behind the Chronic Care Model deserves more thought, especially since components of the model are used to guide a transition and are therefore prominently featured in the evaluation.

2. The paper gives insufficient insight in the use of the theoretical framework. There seems to be a lack of correspondence between the description of the framework in the method section and the way that the results are presented, and the description of the operationalization of the framework’s constructs is, in my opinion, inadequate; the authors provide a useful Table with research questions (Table 1), but fail to appropriately describe how they answered these questions (i.e. there was insufficient information on procedures, measurements and outcomes). Furthermore, there was limited theoretical and methodological information about how the authors investigated the envisioned transition from reactive to proactive care, and how this matched the premise and components of the Chronic Care Model. In addition, the authors investigated fidelity, referring to it as ‘quality of the intervention’ (Saunders’ definition of the term). However, when discussing fidelity outcomes in the Discussion section, the authors refer to a study conducted by Hasson, who uses a definition and operationalization of implementation fidelity different from Saunders. Hasson’s approach to assessing implementation fidelity builds on a conceptual framework proposed by Carroll et al. (2007), which is currently considered the most accepted method of fidelity assessment.

3. Besides Figure 1, the authors provide limited information about the intervention components (and the existing information is scattered throughout the paper), and do not specify what components they consider to be ‘active’ (i.e. what components they envision to contribute to the desired intervention effect). Since process evaluations are, among other things, performed to explain outcomes of effect evaluations, it is important to distinguish between the different intervention components.

4. The description of the planned delivery could have been better articulated. For
instance, the authors write that the intervention intended to establish linkages between local professionals and organizations, but fail to specify what these linkages are, how the intervention aims to achieve them, and what the intended delivery is in terms of, for instance, frequency and duration.

5. The authors did not include a paragraph in the Method section to address qualitative quality procedures.

6. The article could benefit from a more concise, condensed Results section. Choices for titles and location of headings do not always seem evident. Further, presented outcomes are not always appropriately defined in the Methods section.

7. It would benefit the paper if the authors pay more attention to the conciseness of their writing when continuing their work on this article. In addition, there were several syntax and grammar issues, and some paragraphs were challenging to understand and interpret. An exception is the Discussion section, which is overall well-written.

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

**Quality of written English:** Not suitable for publication unless extensively edited

**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