Reviewer’s report

Title: The Effects of an Integrated Care Intervention for the Frail Elderly on Informal Caregivers: A Quasi-Experimental Study

Version: 4 Date: 16 February 2014

Reviewer: Mary Janevic

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Review: Janse et al, “The effects of an integrated care intervention for the frail elderly on informal caregivers: A quasi-experimental study”

This manuscript describes a study using a quasi-experimental design to assess the effects (at 12 months) of a newly-introduced ‘integrated care model’ on health, burden, and quality of life of caregivers of the frail elderly. Strengths of the study include a well-described study rationale, the use of an appropriate comparison group and analytic approach, and measurement across several domains that might be affected by the intervention. This paper also has a number of areas that could be strengthened and/or clarified, as described below.

DISCRETIONARY REVISIONS

1. (Background) A definition of “frail elderly” should be provided. The literature cited by the authors includes research on dementia caregivers, caregivers of people with rheumatoid arthritis, and the elderly generally.

2. (Intervention) It would be helpful to have 2-3 more specific examples of the types of assistance that informal caregivers receive as part of the integrated model—for both in-home and out-of-home caregivers. This will give the reader a better idea of how it might be expected to decrease burden and affect other outcomes. Something like a brief ‘vignette’ or case study could be illustrative.

3. (Discussion)—any data from this study to show if care recipients improved? Will this be forthcoming in another publication?

4. (Results) Table 1 is not needed; this information is adequately described in the text.

5. (Results) Table 5 does not convey a lot of important additional information regarding the effect of the intervention—and its omission should be considered—could possibly add the total percentage in variance explained by each regression model to Table 4.

COMPULSORY REVISIONS

1. (Methods) Is perceived health measured on a 1-5 (per text on p. 8 and questionnaire) or 1-100 (per Tables 2-3) scale?

2. (Methods) The authors may want to provide more information about how the patients and caregivers were recruited into the research study sample (presumably it was only a subset of all patients receiving the intervention at
participating clinics), and what the response rate was. How were control group caregivers identified? Were patients/caregivers recruited as dyads—i.e., did both patient and caregiver have to agree to complete the research questionnaires in order to be in the research study? It seems that this is the case from the discussion of loss to follow-up on p. 11, but should be made explicit.

3. (Background, Methods) For each outcome, it should be clear why the intervention would be expected to have an effect, and in which direction. The discussion on p. 4 makes clear that the intervention is supposed to reduce objective (and thereby subjective) burden; it is reasonable that this would also affect the caregiver’s quality of life. However, it is less clear how and why the intervention would affect other outcomes such as perceived health and hours in various types of caregiving tasks. This should be better supported by the authors.

4. (Methods) Is any information available to characterize the health status of the patients? Burden is also determined by the health status and needs (e.g., ADLs) of the patient (as acknowledged in Limitations section), so it would be ideal to control for these variables as well, if the data is available. At a minimum the criteria for patient participation in the integrated care intervention should be listed. Models should also control for the caregiver’s relationship to care recipient (child vs. spouse etc.)

5. (Analysis) Because control group data is available, there is no reason to assess the statistical significance of within-group pre-post changes. In Table 3, limit results to between-group analyses; and discuss notable within-group patterns in the text but without regard to their statistical significance.

6. (Analysis) For the variable “perceived change in health” it’s not clear that it should be treated as a linear, continuous outcome. If a respondent had excellent health a year ago, “about the same” would be the best reasonable outcome for this person. In other words, 5 is not necessarily better than 3.

7. (Results) Here and throughout the paper, causal language (e.g., “the WICM significantly reduced subjective burden”, “co-residence negatively affected perceived health”) is not warranted by the study design. Instead the authors should use language of association/correlation; e.g., “being in the intervention group was associated with greater reductions in subjective burden”.

8. (Results) Also, according to Table 4, the Carer QOL sum score was significantly (p>.10) greater for the intervention group—it does not appear that the association of being in the intervention group with subjective burden was significant. The Limitations also refer to CarerQoL as being significantly associated with the intervention, but the preceding page says that no effects were found on quality of life. Unclear if this is a mistake or due to how these concepts are being defined. It’s also not clear exactly which outcome was related to household tasks was significant—was it the # of other caregivers performing them (as appears on Table 4), or whether or not the caregiver (respondent) performed them?

9. (Results) For ease of reading, please keep measure names consistent and consistently organized across tables and text; e.g., keep the 4 categories in Table 3 in Table 4 as well, and use the same names in all places—e.g.,
“CarerQoL” is in Table 4 but is not referred to that way in Table 3. With so many outcomes, it is important to be clear and consistent in this way.

10. (Discussion)—Again, some confusion here about what outcome was significant—subjective burden or QOL?

11. (Discussion)—As noted above, the discussion about the WICM increasing the likelihood of caregivers assuming household tasks is confusing. First, in Table 3, it looks like the measure is “number of caregivers performing household tasks.” Second, it’s not clear in which direction it was hypothesized that the intervention would have an effect. Also unclear why “improvements in the care recipient’s status” would lead to the finding of no change in personal care tasks.

12. (Limitations) The discussion about the effect that the loss to follow-up may have on results should be expanded; for example, if the most burdened caregivers dropped out, would this be different by group (intervention or control)? Which way would it bias the results?

13. (Limitations) Another notable limitation is the large number of statistical tests performed in this study and no statistical correction for multiple comparisons.

14. (Limitations) The argument that the intervention is not as appropriate (or effective) for out-of-home caregivers would be stronger if the authors gave some examples of intervention components that would not be helpful to this group, and/or if subgroup analyses actually showed a reduced intervention effect on this group.

15. (Recommendations) Although longer-term follow-up may allow more time for certain results to develop, authors should also note that in this population there would be a natural trend toward health declines in the care recipient and increases in burden among caregivers (meaning that a control group would be essential to identify any intervention effects), as well as participant attrition and mortality among care recipients.

**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.