Author’s response to reviews

Title: The effect of a Gerontology Nurse Specialist for high needs older people in the community on healthcare utilisation: A controlled before-after study

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Version: 2 Date: 11 Dec 2017

Author’s response to reviews:

Many thanks for the reviewer’s insightful comments. The authors have responded to each comment below.

Reviewer reports:

Sascha Köpke (Reviewer 1):

- Most importantly, reporting (although based on CONSORT) is not sufficient. To be of relevance for readers, especially within other healthcare systems, the intervention and the control intervention must be described in much more detail. Therefore the authors should use appropriate reporting guidelines as e.g. the TIDieR guidance (Hoffmann et al. 2014). Also checklists on complex interventions as e.g. CReDeCI2 (Möhler et al. 2015) may be helpful.

Important details have been added to the methods section to explain the intervention and comparison group in much more detail. The TIDieR has been utilised and the checklist added as an additional supplement. The original published manuscript which explains the intervention in much more detail as well as descriptive quantitative results has also been clearly cited in the methods section.
- "Quasi-randomised" is a quite unspecific expression and I suggest to refer to "controlled before-after study".

The manuscript has been amended accordingly.

- Please provide information on sample size determination. In the CONSORT checklist you state that this can be found on page 5/6 which is not the case as this information is merely on how the sample was recruited.

Details pertaining to sample size have been added to the methods section (pg7).

- There are definitely more than "two limitations". Therefore, this section should be expanded. First there is the lack of randomization for which you should give a reason as this could have easily been done.

Agreed and the limitations section has been expanded to include the lack of randomisation.

Also, you should mention the lack of a process evaluation and of patient-reported outcomes as e.g. quality of life.

Patient-reported outcomes was not included as this was not within the scope of this study. This is not a limitation, but has been included as a recommendation in the conclusion section.

Process evaluation did occur in the form of a project work group which met monthly to oversee the project and provide guidance. This group comprised the GNS involved in the study, key health professionals including nurse practitioners and geriatricians as well as key researchers involved in the study. A description of this has been added to the methods section of the manuscript.

The fact that the study had not been registered prospectively should also be mentioned.

This study was not registered prospectively as it is not a RCT. The authors do not believe this is a limitation. Details of the trial registration on pg3 clearly state registration was retrospective.
It is a major limitation that "time constraints" led to assessing only a selection of potential participants. Please make clear that (and why) this did not lead to a selection bias.

All identified high risk older people were unable to receive a CGA within the study timeframe as only one GNS was employed and completing all assessments for qualifying participants within the study timeframe was beyond the GNS capacity. Nevertheless, it is important to state that this should not have introduced a selection bias as the older people assessed were determined based on surname, not functional ability. This important point has been added to the limitations section and further clarification provided in the results section.

- The conclusions must be expressed more carefully. As the intervention did not prove to be superior to usual care, there surely is no way to claim that the intervention "may be supporting older people to remain at home". It should be clearly stated that the intervention was not effective and that the proper study design to assess effectiveness of such an intervention is a RCT.

The authors agree with this comment and have adjusted their conclusions accordingly.

Apart from these major issues, there are a number of further concerns (following the outline of the manuscript):

- P.5: There is no need to list the outcomes at the end of the introduction as this is done later anyway.

Agreed, the outcomes have been removed from here.

- P.5 Please report the study period here (which is only mentioned later).

The study period has been clarified and added here.

- P.5: Give more information about what is (urban) primary healthcare in the local context (see above).
A definition and explanation of primary healthcare services in the New Zealand context has been provided.

- P.6: How do people usually get "enrolled in one of the primary healthcare practices"?

Enrolment is voluntary, however, most individuals are enrolled within a local primary healthcare practice. Voluntary enrolment has been explained in the methods section.

- P.6.: Please give more information about the recruitment process e.g. who posted the screen, which might be of importance to assess the response rate.

Additional information has been provided. The GNS posted the BRIGHT screen.

- P.8: The study has been performed 5-7 years ago, so please discuss the reason for this as well as the up-to-dateness of the results.

The study timeframe has been clarified in the methods section. The intervention was between October 2010 and August 2012, however, data was collected for one year before and after these dates. Therefore data was collected up until August 2013. Therefore, the authors believe this data is still ‘up to date’ as final data collection is well within the last five years.

- P.8: Please give more information about the recruitment of control group participants.

Further details and clarification have been provided under the methods; comparison group section.

- P.8: Please discuss the validity of routine hospital data used as outcomes.

Additional information around the collection of data, unique identifiers and the database used has been provided.

- P.9ff: Please be more cautious in reporting and discussing "statistical significance". There are many results and as you did not adjust for multiple testing and in the absence of a formal power
calculation, significant results are most likely chance findings. Please be more cautious in reporting e.g. "higher mortality" (P.12).

The authors agree and the results and discussion section have been amended accordingly.

- Table 3: Please give results for total and mean numbers in one row (e.g. 344 (0.39) etc.). For readmission, please give number and % of people with readmission (i.e. % of the whole sample) and then how many of these had >2 readmissions. It would be more informative to see the number of "participants with at least one hospital admission" in addition to the number of admissions.

As requested these amendments have been made in table 3. Except for the >2 participants with readmissions. The authors have provided justification within the methods section as to why they categorised <3 and ≥3 readmissions. "Hospital re-admissions were classified as either less than three admissions in a year or three or more admission in a year. This aligned with previous research which defined three or more admissions in a year to be frequent re-admissions (Kirby, Dennis, Jayasinghe, & Harris, 2010).”

- P.13: As stated above, please discuss results with more caution as findings were hardly "intriguing" and differences mostly based on chance or bias.

Agreed. Subsequent amendments have been made in the results and discussion sections.

- P.15: The passage on the "systematic review" is already stated on p.14.

This has been amended.

- P.15: The last sentence of the second paragraph is incomplete and contains a typo ("hosptialisation").

Amended.

- P.15/16: References [25] & [26] have been confused.
Amended.

Sandra Zwakhalen (Reviewer 2):

Terms used: The terms used, like care support models, geriatric nurse specialist, care coordination and CGA, strongly link to each other and to other care concepts e.g. integrated care, case management etc. This terms used need some clarification and definition. Needs to see a consistency in used terms and clarify the definition the authors refer to.

The authors have revised the introduction to ensure these terms are defined and clarified.

What is the evidence on effectiveness with regard to care coordination and CGA. This should be included on page 4 to clarify the concept and to provide more insight in possible outcomes.

Multiple references are provided in the introduction which support the use of care coordination in relation to reducing hospitalisations and improving patient outcomes.

A meta-analysis of RCTs is cited which stated that CGA is effective at improving patient outcomes as well as being cost-effective.

Abstract:

Conclusion is rather short and needs revision.

The conclusion has been revised.
MAIN FILE:

Introduction/ background:

On page 4 case finding by screening is mentioned to identify frail or high-risk older patients. Furthermore it is stated that individualised interventions are required in addition. However, no evidence or references are used.

Evidence has been added to support this statement.

The reader needs insight about the evidence of case finding and interventions. What interventions are aimed for and on what outcomes and for what patient groups? Now it is only stated on page 4 that CGA is effective in improving patient outcomes…..

The authors have revised the introduction to ensure the intervention is clear, including case finding and CGA. Definitions as well as literature to support effectiveness and outcomes are provided.

The intro seems to clarify that studies have already demonstrated effectiveness. The question is why this study is needed? The relevance could be clarified and strengthened.

The GNS embedded within the primary healthcare practice is what makes this model of care different. As well as the strong integration between primary and secondary care. This has been clarified within the introduction section.

At the end of the introduction the aim is stated. However the intervention that is aimed for remains unclear. In my opinion it is not clear at all for the reader what is mentioned with health care utilisation of systematic case finding for community dwelling high risk older people and subsequent CGA and care coordination intervention.

A clear research question is missing.

Description of the intervention and the aim have been revised and clarified in the introduction. The main hypothesis has also been added to the introduction section.
Method

It would be helpful for the reader if the context of home care in Auckland is explained.

A definition and explanation of primary healthcare services in the New Zealand context has been provided.

In the design section, the authors state that an innovative intervention model was instigated..... Is this really so innovative at the moment? What is common care like in case that authors are convinced it is innovative?

Yes it is innovative. The crucial innovative features of this study are that the GNS will be based within the primary care setting as well as strong integration with secondary care. This has been clarified in the introduction as well as the methods section.

Related to this aspect, I wonder is the control group received co-interventions that strongly link to the intervention, like case management or screening for frailty etc…

No the comparison group don’t receive case management or screening. This has been clarified in the methods section.

How were the health care practices selected? Why were these not randomised?

The two intervention healthcare practices identified that they wanted to implement this new model of care, therefore, this study was a pragmatic evaluation and randomisation of healthcare practices was not possible. It was envisaged that in the future, findings from this study would be used to inform a larger RCT. This has been clarified in the methods and limitations section.

Page 6. The Brief Risk Identification for Geriatric Health Tool was used to case find high risk older people…. A reference is required and more in-depth information about the validity and reliability of this tool. Why was this tool selected?
The following was stated about the BRIGHT tool, including two references regarding specificity and sensitivity. An additional reference has been provided about a large cluster randomised controlled trial which used the BRIGHT screen. “The Brief Risk Identification for Geriatric Health Tool (BRIGHT) was used to case find high risk older people. This screening tool is a straightforward 11 item self-administered survey which has demonstrated high sensitivity and specificity (Boyd et al., 2008, Kerse, Boyd, McLean, Koziol-McLain, & Robb, 2008).”

On page 7 it is mentioned that a mixture of specific questions and assessment tools were used….. This needs to be specified. With the information that is currently provided it is impossible to replicate study methods used.

This has been revised and amended with more detail provided about the CGA.

Comparison group received care as usual according to page 7. What is care as usual in these practices?

This section has been added to and usual care has been clarified.

The data collection took place more than 5 years ago, almost 7 years ago? This raised the question what happened in the previous 7 years between the data collection and now. It is almost unimaginable that the care concepts have not changed meanwhile and the context stayed that the same over the last 7 years.

The study timeframe has been clarified in the methods section. The intervention was between October 2010 and August 2012, however, data was collected for one year before and after these dates. Therefore data was collected up until August 2013. Therefore, the authors believe this data is still ‘up to date’ as final data collection is well within the last five years.

A figure or flowchart that includes the measurements, data source and moments of measurement would be helpful.
A flow diagram has been added on page 13 which outlines the flow of participants throughout the study.

Could the authors specify how the outcomes were assessed/quantified in detail?
Additional information around the collection of data, unique identifiers and the database used has been provided.

The analysis paragraph mentions that t-test were used, what t-test were used and why?
t-tests were used for comparisons of age, hospital LOS, mean hospitalisations, and mean ED presentations. This has been further clarified in the analysis section.

Results
A flowchart that includes the participants on the moments of measurement would be helpful.
A flow diagram has been added on page 13 which outlines the flow of participants throughout the study.

The results demonstrate that the participants identified as high risk were older. Is this difference in characteristics causing any bias?

It is confusing to read that the intervention group includes 517 respondents and the comparison group includes 883. This is the initial group that was screened and not the intervention group.

The authors believe these are the two groups – intervention and comparison as the screening was part of the intervention. The intervention group was screened (as a part of this the healthcare practice was informed of results if the individual was identified as ‘high risk’). The intervention group then received further interventions including a GNS assessment. This initial screening process was not offered at all to the comparison group and therefore seen as a part of the ‘intervention’. This clarification has been added in the methods section.
The tables include cells with 0, these should be deleted.
This has been amended.

It would be helpful to structure the findings in main outcome and secondary outcomes.

Sub-headings have been added in the results section for ‘main outcome’ and ‘secondary outcomes’

Was there a process evaluation performed alongside this study? How sure are the authors that the intervention was delivered as intended?

Process evaluation for this study occurred via a project work group which was established to oversee the process of the study and provide expert guidance. This group met monthly and comprised the GNS involved in the study, key health professionals including nurse practitioners and geriatricians as well as the leading researchers involved in the study. This description has been added to the methods section of the manuscript.

Discussion:
What were the authors' hypotheses before the start of the study?
The main hypothesis has been added at end of the introduction section.

Based on previous comments the discussion needs some adaptation. At least the authors should reflect on the intervention that seems a rather diffuse concept.

Amendments have been made to the discussion as well as the introduction and methods which provide a clearer definition of the intervention and explicitly describe why this intervention is innovative.