PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Health profile of residents of retirement villages in Auckland, New Zealand: findings from a cross-sectional survey with health assessment
AUTHORS	Broad, Joanna; Wu, Zhenqiang; Bloomfield, Katherine; Hikaka, Joanna; Bramley, Dale; Boyd, Michal; Tatton, Annie; Calvert, Cheryl; Peri, Kathy; Higgins, Ann-Marie; Connolly, Martin

VERSION 1 – REVIEW

REVIEWER	Janhavi Vaingankar Institute of Mental Health, Singapore
REVIEW RETURNED	20-Jan-2020

GENERAL COMMENTS	The paper describes the profile of retirement village populations in New Zealand from a survey conducted across 33 villages. The paper is well-written and describes pertinent characteristics of residents. I do not have any major concerns about this publication. I found the article informative, results are clear and discussion largely highlights relevant to local context. I would like to offer the following comments/suggestions to improve its content: 1. The paper aims to provide a detailed description of retirement village population with a (possible) use in planning targeted health service initiatives. Authors should elaborate on this in the introduction. For example, would this information not be available with health authorities? Why was it necessary to conduct a study for this? what would be the advantages of this approach? 2. In line with the above, this study sample seems to be a subset of a larger cohort and/or an RCT. This has implications for generalizability of the findings. However i was unable to review this given that information in partially covered under currently unpublished articles (Peri under review; Connolly, under review). I believe, it will be useful for the readers to have access to this information. Authors may chose to wait for the other articles to be published or add summarised relevant information in the current manuscript. 3. Response rate of this study is not clear. Of the 65 villages, 53 were approached (not clear why the rest were excluded). A sample size of 572 was targeted. It is not clear how this was derived. In an earlier statement authors state that information on demographics of retirement village residents is not available. It will be good to add information on estimates used to calculate the precision for this study. Some information on the base population should be accordingly added to be able to establish a response rate for this study. Given the aim of this article is to describe the resident population so as to allow future interventions and its study design, significant attention to selection bias and response rate will be necessary. The data presented on those who were sampled and

	volunteered further compounds the problem of generalizability of the findings since there a number of differences in their characteristics. 4. From the participants surveyed, authors present quite detailed profile of the residents in terms of their current socio-demographic, financial, health behaviors, lifestyle, and health utilization profile. However, I wonder whether authors have collected more background information on them. For example, their family composition, whether they had children, siblings, etc., past occupation, or other health conditions such as mental disorders. This might be useful in determining their support network beyond the retirement village or functioning and role contribution within the village. Alternatively these could be suggested as future research directions or limitations. 5. Discussion needs to be enriched with more comparison with retirement village models elsewhere. I can understand that there may not be adequate and comparable information, however my cursory search generated few good articles. Authors may wish to draw upon such published literature to discuss the study results and implications. 7. Further elaboration of limitations (response rate, self-report measures, long study recruitment period, etc) is necessary.

REVIEWER	Dr Mikaela Jorgensen Macquarie University, Australia
REVIEW RETURNED	20-Feb-2020

GENERAL COMMENTS	Thank you for the opportunity to review your manuscript, 'Residents of retirement villages in Auckland, New Zealand: a baseline profile'. The paper providers a profile of the cohort for a larger study investigating longitudinal service use and outcomes, as well as an RCT. Descriptive studies are often disparaged, but I believe are important for having an accurate understanding of the issues in a population, before researchers can consider why or how to solve them. In that supportive context, I hope the following comments will help the authors strengthen the manuscript. Major comments: 1. Introduction. The intro is currently very short, and much of the cited literature now 8+ years old (including the authors' cited prep work for this study). Population ageing, housing affordability and health system pressures mean this is a rapidly changing space. How does this study build on recent international work in this area? What is the need for this particular study – why is it important? (I believe it is important, but this should be spelled out for the reader) 2. Abstract. Regarding the idea, 'Healthcare service providers and village operators could cooperate to design and test service initiatives that better meet residents' needs and offer cost advantages.' What is the responsibility/available resources for retirement villages to support the needs of residents? How do health/social care services and retirement villages typically interact in NZ? The authors note in the discussion that 'villages are not primarily set up as care facilities or health service providers'. I'm also unsure what is meant by the phrase 'cost advantages'. 3. Methods. Please provide more information relating to the larger project including sample size calculation – the reader cannot yet visit the 'Peri, under review' paper for these details. 4. Methods. Regarding "Residents lacking capacity to consent were excluded as required by NZ's Code of Health and Disability Services Consumers' Rights". Could the authors describe how capacity to consent was assessed (e.g. screening instrument)? At the moment I

	can only find the broad statement “If the GNS had any question about a resident’s cognitive capacity to consent”. 5. Methods. Māori people are described in this study. In Australia, ethics approval for research involving Indigenous people require additional review by a special ethics body. Can the authors confirm whether this is or is not required in NZ? 6. Discussion. The authors note that “It is clear that village residents’ health overall is better than those receiving home-based supports”. However it is not clear that the population in this study is representative of the wider retirement population (as noted by the authors, “when surveys recruit without attempting a representative sample, important self-selection bias occurs and may thus mislead”). Are there national figures that the authors can compare their sample against? Or do they have access to the overall demographic profile of the villages their population comes from? This is important if they want to extrapolate their findings beyond the participating population. If it is not representative, I believe survey weighting methods can be used. Minor comments: 1. Abstract. Unfortunately most readers of papers do not read beyond the abstract of a paper. ‘Baseline profile’ is mentioned in the title but the abstract does not provide information about the larger study that the baseline profile refers to. 2. Abstract. Please further explain what is meant by sampling vs volunteers in the abstract (I recognize that this is explained later in the paper by the abstract should stand alone). 3. Abstract. Correction needed for sentence ‘…suggests survey reports *short* be interpreted with caution.’ Also is there a word missing in ‘their demographics, socio-behavioural *needs?*..’ 4. Methods. Data acquisition. Could the authors clarify if the residents themselves had to complete the survey online? (i.e. needed to be tech savvy?) 5. Methods. Data acquisition – how were the data sources merged if the data were anonymized? (Further detail is required on the anonymization process) 6. Methods. Single imputation methods were used – could the authors include further detail on these methods (e.g. did the authors use simple mean imputation, and based on which variables)? Please also show missing counts in the tables. 7. Discussion (para 1). Could the authors clarify if this paragraph is about the current study or the study in reference 7? 8. Discussion. I am unclear what is meant by “In comparison to those not being needs assessed”. 9. Discussion. “..because we included diagnoses captured in free text, in village residents noted a wider range of diagnoses”. What does this say about the validity of the interRAI instrument? 10. Discussion. Implications – “Perhaps that distinction is relevant here, for compared to sampled residents, our volunteers were more likely to have investment income.” Could the authors further explain why the dichotomy of ‘planners’ and ‘reactors’ would influence the participation by the sampled vs volunteer groups? 11. Discussion. Why/how would imputation lead to “slight underestimates of function and dependency”? What I mean is, could the authors explain why it would underestimate and not overestimate? 12. Remove references to papers ‘under review’ if they are not accepted, or upload the submitted manuscripts to a preprint server (e.g. medRxiv).

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name: Janhavi Vaingankar
Institution and Country: Institute of Mental Health, Singapore Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below The paper describes the profile of retirement village populations in New Zealand from a survey conducted across 33 villages. The paper is well-written and describes pertinent characteristics of residents. I do not have any major concerns about this publication. I found the article informative, results are clear and discussion largely highlights relevant to local context. I would like to offer the following comments/suggestions to improve its content:

1. The paper aims to provide a detailed description of retirement village population with a (possible) use in planning targeted health service initiatives. Authors should elaborate on this in the introduction. For example, would this information not be available with health authorities? Why was it necessary to conduct a study for this? what would be the advantages of this approach?
Response: Retirement villages are not seen as part of the health system, and there is no involvement of health authorities in their operations except where residents receive home-based supports in the same way as they might if living independently. Consequently, there are no official statistics that would characterise the demographics, health or unmet health needs of village residents. This comment is now added to the Introduction.

2. In line with the above, this study sample seems to be a subset of a larger cohort and/or an RCT. This has implications for generalizability of the findings. However i was unable to review this given that information in partially covered under currently unpublished articles (Peri under review; Connolly, under review). I believe, it will be useful for the readers to have access to this information. Authors may chose to wait for the other articles to be published or add summarised relevant information in the current manuscript.
Response: The manuscripts by Peri et al. and Connolly et al. are now uploaded with this revision. We anticipate they will be published soon.
The reviewer is correct in that the larger project has several phases. Phase 1 is a survey of residents and results of all survey respondents are reported here. Phase 2 regards the surveyed residents as a cohort, tracking their post-survey health events over time and is still underway. Phase 3 is a RCT of an intervention in selected residents and is also underway. We have included some information about the two phases, but consider that further detail would be inappropriate, adding to duplication and to the length of the paper.

3. Response rate of this study is not clear. Of the 65 villages, 53 were approached (not clear why the rest were excluded). A sample size of 572 was targeted. It is not clear how this was derived. In an earlier statement authors state that information on demographics of retirement village residents is not available. It will be good to add information on estimates used to calculate the precision for this study. Some information on the base population should be accordingly added to be able to establish a response rate for this study. Given the aim of this article is to describe the resident population so as to allow future interventions and its study design, significant attention to selection bias and response rate will be necessary. The data presented on those who were sampled and volunteered further compounds the problem of generalizability of the findings since there a number of differences in their characteristics.
Response: Of those units that were randomly sampled, the response rate was 35%. In brief, we recruited 578 residents (median age=82yrs; 420 female). Of these, 217 (from 190 units) were recruited by representative sampling, and 361 volunteers (for which no response rate is calculable). Further details of the methods is covered in the methods paper (Peri et al.) which will be referenced by several other papers. We acknowledge the issues about generalisability; they are described and discussed in detail elsewhere (Connolly et al).
Sample size for the survey (Phase 1) was governed by the number needed for the RCT which followed this study and is not relevant to the findings reported here. For the precision of survey estimates, if the prevalence of a characteristic of interest is around 50%, the sample

(n=578) would provide at least 4.5% precision (50% ±4.5%) at a 95% confidence level had representative sampling been achieved.

4. From the participants surveyed, authors present quite detailed profile of the residents in terms of their current socio-demographic, financial, health behaviours, lifestyle, and health utilization profile. However, I wonder whether authors have collected more background information on them. For example, their family composition, whether they had children, siblings, etc., past occupation, or other health conditions such as mental disorders. This might be useful in determining their support network beyond the retirement village or functioning and role contribution within the village. Alternatively these could be suggested as future research directions or limitations.
We have added to the manuscript that we did not collect information about children, siblings or occupation. While there are data on other topics that will be included in other papers, there is little more in regard to support networks or contacts outside the village.

5. Discussion needs to be enriched with more comparison with retirement village models elsewhere. I can understand that there may not be adequate and comparable information, however my cursory search generated few good articles. Authors may wish to draw upon such published literature to discuss the study results and implications.
Response: Comparable data have been added from the few studies that are available for New Zealand and also some from Australia.

7. Further elaboration of limitations (response rate, self-report measures, long study recruitment period, etc) is necessary.
Response: Clarification and discussion of these methodological details and their consequences are covered in the Peri et al. and Connolly et al. papers as they will be referenced by several other papers in preparation and require additional discussion. However information has been added regarding response rates, and mentioned again later, in the Discussion.

Reviewer: 2
Reviewer Name: Dr Mikaela Jorgensen
Institution and Country: Macquarie University, Australia Please state any competing interests or state 'None declared': None declared

Dear authors,

Thank you for the opportunity to review your manuscript, 'Residents of retirement villages in Auckland, New Zealand: a baseline profile'. The paper providers a profile of the cohort for a larger study investigating longitudinal service use and outcomes, as well as an RCT. Descriptive studies are often disparaged, but I believe are important for having an accurate understanding of the issues in a population, before researchers can consider why or how to solve them. In that supportive context, I hope the following comments will help the authors strengthen the manuscript.

Major comments:

1. Introduction. The intro is currently very short, and much of the cited literature now 8+ years old (including the authors' cited prep work for this study). Population ageing, housing affordability and health system pressures mean this is a rapidly changing space. How does this study build on recent international work in this area? What is the need for this particular study – why is it important? (I believe it is important, but this should be spelled out for the reader)

   Response: There is little information about the operation of retirement villages in NZ, aside from the industry's own annual reports, the Retirement Commissioner's one-off survey (ACNeilsen 2006) and occasional surveys of volunteers of one/few villages.
   While it is clear that residents do expect health services to be available in the villages, it is unknown what the needs are or whether they are adequate. The national census cannot report even demographic characteristics of those living in retirement villages. This study is an attempt to describe the social, health status and functional needs of current residents in part to fill the knowledge gap. We have added this to the Introduction.

2. Abstract. Regarding the idea, 'Healthcare service providers and village operators could cooperate to design and test service initiatives that better meet residents' needs and offer cost advantages.' What is the responsibility/available resources for retirement villages to support the needs of residents? How do health/social care services and retirement villages typically interact in NZ? The authors note in the discussion that 'villages are not primarily set up as care facilities or health service providers'. I'm also unsure what is meant by the phrase 'cost advantages'.

   Response: Some, though not all, villages include health services as part of their marketing and/or their operations, so thus do have some responsibility. Judging from their reasons for moving into a village, and that a village nurse may be included in the management service fee, it is apparent that many residents anticipate services. This is distinct from publicly funded services provided via independent, external service organisations. This is now described in the Supplementary notes about NZ village operations.

3. Methods. Please provide more information relating to the larger project including sample size calculation – the reader cannot yet visit the 'Peri, under review' paper for these details.

   Response: see our response to the 1st reviewer's point 3 above. The current drafts of both the Peri et al. and Connolly et al. manuscripts are now uploaded with this manuscript.

4. Methods. Regarding "Residents lacking capacity to consent were excluded as required by NZ's Code of Health and Disability Services Consumers' Rights". Could the authors describe how capacity to consent was assessed (e.g. screening instrument)? At the moment I can only find the broad statement "If the GNS had any question about a resident's cognitive capacity to consent".

   Response: An Addenbrookes Cognitive Examination Revised ACE-R) score under 65, or the opinion of our research GNSs or that of the resident village manager, excluded residents lacking capacity to consent.

5. Methods. Māori people are described in this study. In Australia, ethics approval for research involving Indigenous people require additional review by a special ethics body. Can the authors confirm whether this is or is not required in NZ?

   Response: NZ ethics committees recognize that any NZ research has potential to impact Māori. Therefore, all ethics applications require inclusion of responsiveness to Māori, and this is considered prior to granting ethics approval. Further, all NZ interRAI assessors, including our research GNSs, are trained in the Meihana Model of Clinical Assessment (Pitama et al NZMJ 2014; 117:107-119 PubMed) and must update their competency in this regard every 2 years. We do not consider there is the need to explicitly state that within this paper, especially given the very few Māori who were in our study. It is our intention to discuss aspects about access to such services in a separate piece of work in due course.

6. Discussion. The authors note that "It is clear that village residents' health overall is better than those receiving home-based supports". However it is not clear that the population in this study is representative of the wider retirement population (as noted by the authors, "when surveys recruit without attempting a representative sample, important self-selection bias occurs and may thus mislead").

   Response: We acknowledge representativeness is one of the limitations of this study (see the limitation paragraph). However, to our knowledge, this study is the largest cross-sectional survey of village residents in NZ, and the evidence level of this study is likely higher than previously- reported studies in NZ. It does not claim to represent those living in the community outside a village setting, hence the comparison with those in the LiLACs cohort.

Are there national figures that the authors can compare their sample against? Or do they have access to the overall demographic profile of the villages their population comes from? This is important if they want to extrapolate their findings beyond the participating population. If it is not representative, I believe survey weighting methods can be used.

Response: The national demographic profile of village residents not available in New Zealand. Indeed, one of the reasons for embarking on this exercise was because data are not available. While we had intended to weight the survey results, use of large numbers of volunteers meant that such methods would mislead, even if we had national data by which to determine survey weights. Hence the decision to present results for sampled and volunteer participants separately.

Minor comments:

1.	Abstract. Unfortunately most readers of papers do not read beyond the abstract of a paper. 'Baseline profile' is mentioned in the title but the abstract does not provide information about the larger study that the baseline profile refers to.	Abstract now amended, with the term 'baseline' now removed.
2.	Abstract. Please further explain what is meant by sampling vs volunteers in the abstract (I recognize that this is explained later in the paper by the abstract should stand alone).	Clarification is now provided in the Abstract.
3.	Abstract. Correction needed for sentence '…suggests survey reports *short* be interpreted with caution.' Also is there a word missing in 'their demographics, socio-behavioural *needs?*..'	Corrected, thank you
4.	Methods. Data acquisition. Could the authors clarify if the residents themselves had to complete the survey online? (i.e. needed to be tech savvy?)	Described in methods paper (Peri et al). Respondents could choose, for the survey only (ie not interRAI), to go online themselves, but in practice most completed it with the GNS.
5.	Methods. Data acquisition – how were the data sources merged if the data were anonymized? (Further detail is required on the anonymization process)	Described in methods paper (Peri et al), and now clarified in this manuscript.
6.	Methods. Single imputation methods were used – could the authors include further detail on these methods (e.g. did the authors use simple mean imputation, and based on which variables)? Please also show missing counts in the tables.	The use of medians for continuous variables and modes for categorical variables is stated. A supplementary table is now provided to show the numbers with missing data.
7.	Discussion (para 1). Could the authors clarify if this paragraph is about the current study or the study in reference 7?	Paragraph revised
8.	Discussion. I am unclear what is meant by "In comparison to those not being needs assessed".	Phrase removed
9.	Discussion. "..because we included diagnoses captured in free text, in village residents noted a wider range of diagnoses". What does this say about the validity of the interRAI instrument?	We recommend that additional diagnoses be specifically requested by the interRAI instrument, especially those that impact on clinical management.
10.	Discussion. Implications – "Perhaps that distinction is relevant here, for compared to sampled residents, our volunteers were more likely to have investment income." Could the authors further explain why the dichotomy of 'planners' and 'reactors' would influence the participation by the sampled vs volunteer groups?	Our comment was based on the view that some behaviours or characteristics look to provide for the future and anticipate change eg having investment income is more likely among those who are "planners" than it is among "reactors".
11.	Discussion. Why/how would imputation lead to "slight underestimates of function and dependency"? What I mean is, could the authors explain why it would	Where there are more than 2 categories of response, the distributions of responses are

	underestimate and not overestimate?	skewed, with the more highly functioning category predominant. Since this becomes the modal category, if some are in fact at lesser level of independence, the imputed values may skew the overall counts.
12.	Remove references to papers 'under review' if they are not accepted, or upload the submitted manuscripts to a preprint server (e.g. medRxiv).	We have uploaded the current manuscripts with this manuscript, and anticipate that they will soon be available online.

VERSION 2 – REVIEW

REVIEWER	Dr Mikaela Jorgensen Macquarie University, Australia
REVIEW RETURNED	09-May-2020

GENERAL COMMENTS	Thank you to the authors for your responses and revision submission. As outlined by both reviewers, please remove references to 'under review' papers if they are not yet accepted, and include additional information as required for the manuscript to stand alone.

VERSION 2 – AUTHOR RESPONSE

One reference that was under review is now accepted,and included with other references as "in press" (Peri et al). One other is removed, but if it is accepted before proof approval we may seek its reinstatement.