Author's response to reviews

Title: interRAI Home Care Quality Indicators

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Author's response to reviews: see over
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Ms. Catherine Olino
Editorial Office, BMC Geriatrics

Dear Ms. Olino,

We thank you for the opportunity to submit a revised manuscript. We believe that all concerns of the three reviewers have been addressed and we append our response to the reviewers.

In addition, one of the states with which we were working, where program sites include many persons who are at the cusp of entering a nursing home, asked that we do one more review to ensure that all appropriate high risk ADL and cognitive covariates are considered. This resulted in the addition of about one new covariate on average to the 23 HC-QIs.

We believe that we have addressed all of the concerns and await your review of our manuscript for potential publication in *BMC Geriatrics*.

Sincerely,

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Authors’ responses to referee comments are listed in bold type below. Our response to the issues raised by the three reviewers follows:

REVIEWER 1

3. A couple of additional sentences on the data sources should be added (see below).

The client level data from Europe include: a cross-country home care cohort (ADHOC), where each country is represented by clients from a small number of home care programs; and from Finland where we had country wide home care data. In Canada, data came from all home care clients in the Provinces of Ontario and Manitoba, and a large sample of home care client sites from Nova Scotia. The home care data in the United States came from all state supported home care clients in Massachusetts, Michigan, and Georgia.

6. There is a need for more clarity on the composition of the database used.

See above.

7. The QI described are second generation and build upon first generation QI’s. It would help the reader if some more information on the first generation QI’s would be given, in one or two paragraphs. For example, the sentence “In addition, where there were alternative forms, we avoided the complex specifications common in many of the first generation HC-QIs in favor of more direct measures.” should be better clarified. As should a couple of other references to the first gen QI’s.

We agree that this sentence is confusing and it has been deleted. There were a few instances of such shifts but not sufficient to warrant so strong a statement.

Does this sentence: “Initially, the set of covariates used in the earlier development of HC-QIs was consulted, but because of sample size limitations in the earlier work few covariates were identified.” (p7) mean that the sample size for the development of the first gen QI’s was too small or that there was previous research on the second gen QI’s that was based on insufficient data?

In practice, the larger sample gave us the opportunity to better identify the full, most appropriate covariate structures. As with all chained research efforts, the newer analysis builds off of the former – with all of the weaknesses and strengths of the prior research.

On p9: Europe is not a country. The European countries involved should be mentioned, but the data can be pooled into European data. In general, some more information on the sources and representativeness of the data would be welcome.
See above, the sample description has been further clarified. In addition, on page 9 we clarified the text as follows: “the summary HC-QI quality scale scores are contrasted by political states/provinces pooled within wider areas (Europe, Canada, and the United States).”

P17: why would some people in the field exclude the second summary quality indicator? Why do the authors not agree and think it is meaningful?
Competing interests and contributions: I suppose RSK should be KRS?

The issue was not the exclusion of the QI scale itself, but rather the exclusion of the last measure in the scale – the receipt of flu vaccination. This measure is no longer a part of the scale and thus the confusing text has been dropped.

REVIEWER 2

At page 5 I am wandering whether 16 experts (from usa, canada and europe) is enough to be a little bit representative? How was their distribution?

The intent in the statement and in the approach was to indicate the broad, cross-national review process followed. We now note the countries from which the panel members came: Australia, Canada, Check Republic, Finland, France, Poland, Sweden, Switzerland, and United States.

Because the differences between the regions are important. For me, and this my most important remark, it is not clear how to interpret the differences and how in the field, we have to manage and interprete these changes. Europe is not at all homogenous. Even my own country, which I supposes was one of the programm sites, there are important differences between Flandres and Wallonia.

We concur, as one drills down there will always be differences. Canada is different from the US. But so are there differences across Canadian provinces. And within a province program sites will differ one from another. We clarify this point in the text.

My most important question for revision is to explain the issue of interpretation and benchmarking better, besides the statistitical advies , what I am not able to give

See above. The benchmarking standards are further discussed in the text.

REVIEWER 3
1. The original list of 64 QI should be provided and reasons for exclusion (either a., b. or c.), leading to the selection of 23 QI, should be listed;

Clarified in the text.

2. QI related to the organization of the HC service (i.e. patients/nurse ratio) are not considered in the study. Reasons for excluding these QI should be mentioned.

In our home care data set there are no explicit measures that would permit us to create such a QI.

3. Most relevant limitation of this selection process relates to the fact that it is mainly related to consensus or expert opinion (point b. ‘discussion with providers’ and point c. ‘discussion within InterRAI group’). Was clinical relevance of the considered QI assessed during the selection process (data linking QI with clinical outcomes)?

Yes, this was one the points considered by the focus group panels and the interRAI reviewers. We make this point more explicit in the text.

4. 7 QI are excluded because of a prevalence < 3% in the overall sample. However prevalence of QI can widely vary depending on country. Therefore it would be reassuring to demonstrate that prevalence of these QI does not vary widely in the 3 geographic areas (Europe, Canada and US) considered in the study;

Only those potential QIs with consistently low values have been excluded.

5. Point b. (selection of QI based on discussion with providers) is poorly defined in the manuscript and it is not clear how many providers were interviewed, who are them (MD? Nurses? Else?) and which criteria were used to ‘evaluate the candidate measures’ based on the interview.

Text slightly modified.

6. Also point c. (selection of QI based on approval from InterRAI group members) is poorly defined. Were members asked to rate their approval? Was there a specific question(s) they had to answer?

They were asked to judge both whether the QI was defined properly (e.g., the numerator worked and appropriate covariates were in place) and that the QI would be seen by practitioners and program managers to have obvious face validity when applied within the home care context.
7. It is definitely surprising to see that relevant QI are not included in the final list. For example a large body of research (also performed by InterRAI fellows) has focused on use of antipsychotics, indwelling catheter use, polypharmacy. These are certainly relevant clinical QI which deserve attention. At the opposite 3 QI on pain are selected in the final list. Did InterRAI fellows exclude the above mentioned QI from the final list? How was this process performed? The second step (adjustment process) is well performed and I do not have relevant comments about it. My only suggestion relates to the length of the section entitled ‘The adjustment process’ which can be substantially shortened.

Data

The referenced QIs have been created for use in nursing home environments, not home care. In fact, the required measures are not even included in interRAI’s home care assessment tools.

8. Data analyses are performed in a huge sample of more than 300,000 home care clients from Europe, US and Canada. This sample is poorly defined and a more in-depth description of methodology of home care clients and services recruitment, inclusion and exclusion criteria, assessment procedure (who performed the assessment?) and ethical issues (including informed consent) is needed.

See above

9. Prevalence of quality indicators on pain is somehow surprising. Based on data in figure 4, in the US sample proportion of ‘daily severe + pain’ exceeds 25%, but proportion of ‘pain not controlled’ is about 5%. How is this possible? Can daily severe pain be considered differently than ‘pain not controlled’?

Other issues:

A type in earlier draft, it has been corrected.

10. The introduction section should more clearly state the aims of the paper and the reasons for creating a summary measure.

Updated text is provided.