Author’s response to reviews

Title: Malnutrition risks and their associated factors among home-living older Chinese adults in Hong Kong: hidden problems in an affluent Chinese community

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Author’s response to reviews:

Responses to comments made by Kimberley LeBlanc (Reviewer 1)

Thank you very much for taking the time to review the manuscript and provide useful suggestions on how to improve it. We have addressed your comments and amended the manuscript accordingly. Below shows our responses to your comments.

Comment 1

You have used several tool ex the MNA, can you please indicate if these tools were used in their original form or if they were translated? If they were translated, were they culturally adapted and validated?

Our responses:
Thank you for your comment. The Chinese version of Mini Nutritional Assessment (MNA), the International Physical Activity Questionnaire Short Form (IPAQ-SF) and Charlson Comorbidity Index (CCI) were adopted in this study. The MNA shows good diagnostic ability, with sensitivity of 0.96, specificity of 0.98 and positive predictive value of 0.97, compared with clinical status determined by physician (Guigoz et al., 1996; Vellas et al., 1999). The reliability α was 0.798 in a community-dwelling older population in Wuhan, China (Han et al., 2006). IPAQ-SF was validated locally, with an intra-class correlation coefficient of 0.79 and agreement limits of 94% compared with a physical activity log and an MTI accelerometer (Macfarlane et al., 2007). CCI was also locally validated, with the area under the receiver operating the characteristic curve (AUC) of CCI in predicting one-year mortality of 0.68 (Chan et al., 2014). We have made the amendment in the measurement of the revised manuscript.

References


Comment 2

I suggest defining what you mean my "affluent", this word can mean different things to different people

Our responses:

Thank you for your comment. The affluent cities in China were defined as the Tier 1 cities in China, with gross domestic product (GDP) over US$300 billion. We have made the amendment in the introduction of the revised manuscript.
Comment 3
How did you determine that age 60 and older was "old age"

Our responses:
Thank you for your comment. World Health Organization suggested defining old age as people aged 60 years and older for comparison among regions in the World Report on Ageing and Health (World Health Organization, 2015).

References

Comment 4
I see that you have defined all abbreviations at the end of the text. I feel it would be helpful if the first time used in the text they were defined. The reader must flip to the end of the paper to know what the abbreviations mean.

Our responses:
Thank you for your comment. We have made the amendment accordingly in the revised manuscript.

Comment 5
In the results section line 206-213, can you please state in the text if these findings were significant.

Our responses:
Thank you for your comment. Line 206-213 reported the descriptive statistics of our sample without the use of any inferential statistics. The comparison between participants with normal nutrition status and those who were at risk or malnourished were reported in Line 216-221.
Comment 6

line 257: you claim that improving vision will improve nutrition. This is a big claim, I would soften it and say that by improving eye care, there is a potential that the secondary effect will be improved nutrition among the visibly impaired population.

Our responses:

Thank you for your comment. We have revised the sentence as follows: Improvement in eye care services may pose a secondary effect to improve the nutritional status in the visibly impaired population.

Responses to comments made by Dariga U. Akasheva (Reviewer 2)

Thank you very much for taking the time to review the manuscript and provide useful suggestions on how to improve it. We have addressed your comments and amended the manuscript accordingly. Below shows our responses to your comments.

Comment 1

In general, the purpose of the work is clear. The conclusions, confirmed by the statistics, are quite to be expected. But the discussion should be more meaningful and in-depth. It should only apply to the findings of this study and not the others. This goes for the second conclusion as well. In the final multivariate logistic regression, poor appetite was the main factor that contributed to the development of malnutrition (OR 4.35, p<0.001). Probably, this was due to comorbidity. To assess comorbidity, you use the Charlson index which does not take into account many diseases, including depression. The latter is the second most important factor after comorbidity in reducing appetite. Especially considering non-representativeness of the study population, the "affluent" people usually are not in need material things, but have psychological problems. With regard to comorbidity, you explained that comorbid patients mostly refused to participate in the study. And the comorbidity was not adjusted due to the results of the univariate analysis. In this case, there is no need to talk about the causes of poor appetite according to other studies. It is better to discuss the presence or absence of relationships and possible causes of the latter according to your observational study.

Our responses:

Thank you for your comment. We pointed out that the reduced appetite may be caused by depression in this affluent Chinese community, especially when the prevalence of depression
was high (12.5%) among Hong Kong older adults (Chi et al., 2005). This indicated the need to cover multiple dimensions of geriatric problems for nutritional intervention. We have made the amendment in the discussion of the revised manuscript.

Since comorbidity measured using total CCI score had a p-value <0.25 in the univariate analysis, it was put in the backward multivariable logistic regression. However, it was not significant in the final multivariable regression analysis. Due to the fact that some potential subjects declined to participate due to illness (Figure 1), the comorbidity may be underestimated in our sample, which is one of the limitation in our study.

References


Comment 2

Authors are in the right to choose suitable tools for themselves. Therefore, the choice of the Mini Nutritional Assessment (MNA) is understandable. Actually, the MNA has been widely validated, including in China. However, while it explains your choice of the old (2003) European document (ESPEN), it doesn't explain the more recent (2012) American consensus on the issue of nutrition and malnutrition.

Our responses:

Thank you for your comment. The validation of MNA involved the use of anthropometric, clinical, biological and dietary measurement (Guigoz et al., 1996; Vellas et al., 1999), covering all the six characteristics (insufficient energy intake, weight loss, loss of muscle mass, loss of subcutaneous fat, localized or generalized fluid accumulation that may sometimes mask weight loss, and diminished functional status as measured by hand grip strength) recommended in the recent (2012) American consensus on the issue of nutrition and malnutrition (White et al., 2012), with sensitivity of 0.96, specificity of 0.98 and positive predictive value of 0.97. We have made the amendment in the measurement of the revised manuscript.

References


Comment 3

There are too many references. They should be more specific and substantive and contain citations. Due to this the number of references may be reduced. The paper contains a large number of references of normative or government documents. The topic of the paper is still gerontological, so the generally accepted opinion about the rapid economic development of China may not need a literary confirmation (reference 44).

Our responses:

Thank you for your comment. We have reduced the number of references to 68.