Review of “Obesity, Cardiovascular Risk Factors, and Mortality among Older Thais: A Four-year follow-up study”

This is a longitudinal study using data from the National Health Examination Survey III in almost 16,000 older men and women (>60 years of age) from Thailand. The objective was to examine the association between obesity, as well as a number of other cardiovascular risk factors, and all cause mortality.

Although I feel that this paper has potential, this study could be improved in a number of areas.

Discretionary Revisions

1. Is there any data on quality of life data from the NHES survey? If not, I would suggest adding this as a limitation.

2. The authors report using the education variable as socioeconomic status. However education only plays a partial role in determining socioeconomic status, which also includes income and occupation. Not having this data may be considered a limitation.

Minor Essential Revisions

1. There are a number of grammatical errors within the text that should be corrected. There are also errors within the references i.e. Reference #22: Incorrect title.

2. Reference #30 is missing. The references are also not formatted properly.

3. Figure #1 is missing.

4. On page 4, the authors state, “From this linkage, older persons who died from all causes, except accidents and assault, and those without information on health risk or protective behaviors were excluded”, however I think they meant to say “included”.

5. The authors use the BMI cutoffs of <18.5, 18.5-24.9, 25.0-29.9, and 30.0 34.9, which are the World Health Organization (WHO) recommended cutoffs for BMI, and should be referenced as such.

6. On page 5, the authors describe the fruit and vegetable categories, stating 1 cup = 150 mL, however 1 cup is actually equal to 240 mL.
7. Please report the confidence intervals (+/- p values) in Table 3 instead of the standard errors.
8. In the footnotes below Table 3, “Model 2” is repeated twice.
9. Rounding of decimal places is not consistent in the tables.
10. There is no “Conclusion” heading.
11. Table 3 title is incomplete – the follow-up years are typed as 2004-200”.
12. I am unclear with the timelines for data collection - was the data collected between Jan 1st and Dec 31st of 2004? Please clarify.
13. I would not consider the study to be a “4-year study” if the mean follow up time is 2.2 years.
14. BMI is not defined in the abstract.
15. WHO is not defined in the text.

Major Compulsory Revisions

1. I feel that the primary objective is a little unclear. The objective, “to examine the relationship of obesity and selected cardiovascular risk factors from the health survey with all-cause mortality among older persons”, is vague. Is obesity the primary variable of interest? Is there a lack of data specifically in the elderly Thai population? The authors refer to a gap in the literature where previous studies examined risk factors in the Thai population in mostly urban populations vs. rural populations as, however, this does not specifically relate to their objective. The statement of purpose, or the reason for conducting the study should be better defined. The authors should refer to previous research examining obesity and mortality in the elderly in the background information in obesity is their primary variable of interest.

2. On page 5, the authors state “Besides risk factors of CVDs, two risk behaviors were of interest” fruit and vegetable consumption and physical activity”. This statement is unexpected – if those are variables “of interest”, they should be mentioned up front, at least as a secondary objective. My suggestion would be to focus on 1-3 variables of interest at most i.e. obesity and physical activity in older adults, instead of all possible risk factors.

3. The authors stratified their analyses by men and women but did not provide a reason for doing so. If they are specifically looking for gender differences in outcomes, then Table 1 should also be stratified by males and females.

4. In the methods section, the authors report the total number analyzed, the response rate and missing data, however almost all of this information is then repeated in the results section. Having the totals in the results section alone is sufficient.

5. It would be preferred to report median survival time vs. mean survival time or person-years follow up.
6. The number of people in each BMI category should be included in Table 1 and in the text. The authors refer to the “low number of people with BMI 30-34.9 and 35.0+” but only in the discussion, and only in percentages.

7. On page 8, the authors state, “In comparison with the reference category of each predictive variable, these hazard ratios among older women were obviously greater than among older men.” This statement is very confusing and I am unsure of what it is referring to, as some HRs were lower in women compared to men, and some were higher in women compared to men.

8. The authors refer to the BMI cutoffs that were chosen for their analysis as potentially inappropriate given that the WHO has more recently suggested using lower BMI cutoffs in Asian populations. Why were these not used then? Or could the authors have used both and determined if there were any differences in results?

9. The authors have found a paradoxical finding between BMI and mortality – an inverse relationship which is opposite of the traditional risk factor epidemiology of BMI in the general population whereby as BMI increases, all cause mortality also increases. This has been termed the Obesity Paradox and has been observed consistently in the elderly population, however the authors make no mention of this in the introduction or the discussion. I think a discussion regarding the ‘obesity paradox’ in the elderly and how it ties in to your study should be included. The review by Oreopoulos et al, The Obesity Paradox in the Elderly: Potential Mechanisms and Clinical Implications Clinics in Geriatric Medicine, Volume 25, Issue 4, Pages 643-659 as well as a review by Artham et al “Obesity Paradox in the Elderly: Is Fatter really Fitter?” (Aging Health, 2009 Vol 5: 177) and a review by Lavie et al “Obesity and Cardiovascular Disease Risk Factor, Paradox, and Impact of Weight Loss” (JACC 2009;53:1925) could be referred to.

10. The authors mentioned that they had waist circumference and waist-to-hip ratio data. Although they chose to use BMI as the measure of obesity, there was no mention of why they chose BMI vs. the other two. It would be interesting to see if there are differences in mortality when different measures of obesity are used (or provide a reason for using BMI over the other anthropometric measures).

11. In the discussion, there is no interpretation of the fruits and vegetable consumption findings or how they relate to previous studies examining this variable.

12. There is no conclusion heading. I assume the last paragraph is the conclusion, however it is too vague and refers to “policy implications”, which was not mentioned previously in the paper.

13. The abstract conclusion is not supported by the data. The authors state that “this study supports evidence of increased mortality in low and high BMI”, but their adjusted analysis did not show increased mortality in the highest BMI groups.
14. The abstract results section is confusing. They refer to a J-shaped association between BMI and mortality, however this was seen in age-only adjusted analyses and not in the fully adjusted analyses. To truly determine if a “J-shaped” association exists, it would be helpful to perform fractional polynomial Cox Regression and then you could graph the results to display the mortality pattern.

15. The authors use the Akaike’s Criteria Information to select the most parsimonious model for Cox Regression analysis. The rational for using this method is unclear. For one thing, the sample size is large enough that it would easily accommodate 14 variables (the full model). If the authors are specifically interested in the association between BMI and mortality and/or physical fitness and mortality, then they would want to adjust for all measured potential confounders vs. trying to choose the most parsimonious model – which would not present an advantage over entering all variables. I would suggest either entering all variables in to the model, or entering them as blocks where they group the similar variables together, i.e. first block age and sex, second block co-morbidities, third block physical activity etc. Also, it is more common to check for interaction between the main variable(s) of interest and age or gender, not between hypertension and diabetes as the authors have done, unless there is a specific clinical reason for doing so.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests.