Reviewer's report

**Title:** Life expectancy, health-adjusted life expectancy and proportion of life spent in non-optimal health by body mass index category in adult Canadians

**Version:** 1 **Date:** 13 April 2013

**Reviewer:** Sandra Reynolds

**Reviewer's report:**

This is an important article, well-conceived, well-written, and addressing a critical issue facing public health entities in virtually the entire developed world. It advances the field by examining health expectancy by BMI categories, furthering our knowledge of the impact of weight on mortality and health simultaneously.

**Major Compulsory Revisions**

This reviewer finds no issues requiring major compulsory revisions.

**Minor Essential Revisions**

There are several issues that the authors should address to make this paper stronger. They include:

1. In studies of Active Life Expectancy (disability being the health issue), the effectiveness of the Sullivan Method is considerably compromised by leaving out estimates of disability in residents of skilled nursing facilities, as they are assumed to be compromised in the ability to carry out I/ADL tasks. In this study, residents of health institutions are specifically excluded; this begs the question of whether their HRQL would also necessarily be lower than community-dwelling residents. At the very least, the authors need to address this issue in the Discussion, justifying their exclusion.

2. Results presented on pages 11-12 stemming from Table 1 should show the significant differences between BMI groups on the HUI scores.

3. Page 12: Please make clear that the discussion of differential mortality relates to data that is not shown.

4. Page 14: Discussion of Figures 1 and 2 are referred to as ‘trends’ – trends by definition are time-bound; what the Figures show is age-differences.

5. Page 15: The contribution of mortality to HALE losses in the Obese 2 category is, presumably, statistically significant; if so, it should be starred in Table 3, yes?

6. Page 18: The discussion about reverse causality is awkward. At one point, the manuscript seems to suggest that this is an issue between mortality and BMI – hard to imagine how mortality could be the cause of BMI. Please clarify.

7. Page 19: Authors refer to a previously published algorithm adjusting BMI values. Is this algorithm theirs? Please cite the relevant work.

**Discretionary Revisions**
1. The first, and most important, is whether the authors should also present Table 2 and Table 3 results using multiple age groups. This reviewer feels strongly (but it remains discretionary) that results for the 65+ group would be substantially different. Interestingly, on Page 9, the authors describe their HR method as including age categorizations of 20-64 and 65+, lending support for this possibility. This would significantly strengthen the article.

2. Page 5 and elsewhere: the authors might consider mentioning that the Dutch studies specifically address the impact of smoking, which would have an effect on the Underweight group.

3. Page 6: the description of the NPHS is “longitudinal”—could the authors confirm that it is a panel study, and not successive cross-sections.

4. Page 20: it’s really hard to believe the smoking covariate was insignificant – are they sure? Did they try it specifically on the Underweight group?

5. Figures 1 and 2: Personally, I don’t think the Stats provided at the bottom add to the results being shown – and they clutter up the figures – consider removing them?

Editorial fixes

1. Page 2, Abstract: third line in Results should specify that it is class 1 obesity

2. Page 4, Middle of the paragraph – “When the effects of BMI on health-related quality…” – consider starting a new paragraph there.

3. Page 21, Authors’ contributions: in the US, the work would be “led” not “lead” --??

4. Page 24: not exactly evident from the Reference List what the format is, but if it Medical, then the long list of authors (#9 is particularly long) should only contain the first 3 authors and then et al.

**Level of interest:** An article of outstanding merit and interest in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.