Reviewer’s report

Title: Primary weight maintenance: An observational study exploring candidate variables for intervention

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Reviewer: Pedro Teixeira

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This paper analyzes a very large number of individual questions as potential correlates of 10-year natural weight changes in a large Swedish cohort. The goal was stated as aiming to study primary weight maintenance (PWM), an interesting concept. I have some questions and concerns regarding this study and how it is presented.

A central limitation in this paper is the questionnaire used. It apparently consists of “31 questions” and “166 predictors” (items?) but readers are not informed as to how exactly these questions were selected and whether they were subject to any factorial treatment or psychometric analysis. Besides some questions fitting into a few “ideal types” (apparently merely on qualitative data, which is not clearly explained), there is no more information as to any systematic attempt to organize the items into dimensions, eliminate redundant items, test validity and reliability of factors, etc. As a result, items are treated as individual variables, resulting in an extremely difficult, and largely meaningless, exercise when authors have to interpret results. This is made worse by the various weight/age/gender groups used to split the analysis. The authors end up spending a lot of time analyzing the data “quantitatively” (x number of predictors were significant for this group and Y number for that – e.g. Table 1, 2, 5, 6 provide information of quite limited inferential value) which is of limited use. The risk of type I error or chance significant findings is also large is such an analysis.

One recommendation is that authors make an a priori attempt to organize the 166 items into meaningful categories by using factor analysis or other data-based method, before they perform any data analysis of a predictive nature regarding outcomes. I think they should start with simple correlation analysis between discrete dimensions/variable and weight change and move on from there.

Another recommendation (especially if the previous recommendation is not followed) is to reduce the number of covariates and categories and merely adjust for some of them (e.g. age and initial weight) in regression analysis (especially when they appear to change the relation between the predictor and the outcome). If these covariates prove to change the nature of the associations between predictors and weight change, then consideration can be given to using separate categories.

This appears to be a retrospective study, i.e., the predictors were assessed after
the weight change, which is a major limitation due to the potential for reverse causality. This should be acknowledged in the paper (if this is correct) and its implications thoroughly discussed.

Although this is not mentioned in the study aim, it appears that a key goal of the study was to assess the validity of a priori “ideal types” for PWM (e.g. see initial paragraph in Discussion). This should be made clear earlier in the paper.

The Discussion section could be better organized. In addition, there are instances when results are interpreted in inaccurate ways. For instance, in the bottom paragraph of page 14, the different number of significant predictors for men and women should not, in my view, be interpreted as evidence that women find it more or less difficult to lose weight. Easiness to predict weight loss and easiness to lose weight are two different questions which may or may not be related.

In my view, the authors are excessively concerned with translating their findings into intervention-relevant information. Considering the study’s limitations (e.g. retrospective study) and its very descriptive nature, authors might limit themselves to describe associations and possible implications to understand the natural course of obesity and weight maintenance, a sufficiently worth goal considering how little evidence exists.

There is a large literature on predictors of weight control (e.g. systematic reviews by Elphag et al and by Teixeira et al., in Obesity Reviews; data from the US National Weight Control Registry), which is ignored but could be informative. Generally, many of the references cited in the paper are old and I wondered whether the authors have a firm grasp of the obesity literature.

Quality of written English: Acceptable

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

Declaration of competing interests:

I declare that I have no competing interests