Reviewer's report

Title: Population-level effects of the national diabetes prevention programme (FIN-D2D) on the body weight, waist circumference, and the prevalence of obesity

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Reviewer: Magnolia Cardona-Morrell

Reviewer's report:

GENERAL

This paper deals with a very important aspect of primary prevention which the diabetes readership has been waiting for several years. The evaluation is topical as the original study has triggered secondary and primary prevention activity in many real world settings. The authors clearly acknowledge the previously published work upon which they are building.

The question posed by the authors is well defined and the tile clearly reflects the intent of the manuscript. The data source is sound as it comes from an initiative well-known in the public health literature. The methods for statistical comparisons for continuous and categorical variables are sound, and so is the adjusted multivariate analysis. However, clarifications are required for readers and a suggestion regarding use of individual participant data rather than population-wide estimates is made under ‘major compulsory revisions’. The discussion can be strengthened by acknowledging the limitations of the study in more detail and the conclusions can be stated in a less far reaching fashion to reflect these limitations.

MAJOR COMPULSORY REVISIONS (which the author must respond to before a decision on publication can be reached)

The Methods section is difficult to follow for readers outside Finland.

Page 6, 2nd paragraph: this paragraph confuses the reader. The study is supposed to report results of the impact of FIN-D2D, whose measurements occurred in 2004 and 2007. This paragraph suggests that data from the FINRISK 2002 and 2007 were combined with data from FIN-D2D 2004 and 2007. It is not so clear that the control areas refer to geographic areas where data were collected in 2002 and 2007 as part of FINRISK. Perhaps adding separate subheading to explain intervention and control data sources and years of collection would set the scene better and make it easier for the reader to understand how data sources were used and how control communities were selected. The abstract nicely explains what the comparison groups consisted of.

From the description of the conduct of the surveys, it appears as if the authors have access to individual participant records (from those randomly selected out
of the population register who responded to the invitation letter). If this is the case, the paper could present the results as a cohort analysis for those people who participated in both surveys and the discussion would greatly benefit from a section on the effects on these people separately from the whole-of-population results. Reporting the % weight loss would be a better indicator than % in separate weight categories. Cohort information (for both intervention and control communities) would provide real changes in weight and BMI and would add credibility to the attribution of ‘effects of the programme’ on outcomes. It would be important to also report what proportion of participants from the baseline survey also attended measurements at the follow-up assessment. As the response rates for repeat assessments is below 70% then baseline differentials in the socio-economic and risk factor profile between intervention and control communities could reveal the extent of selection bias beyond age distribution.

As the results are population-wide from the repeat cross-sectional surveys of randomly selected individuals at each time point, I would like the authors to consider: if no data are available from individual outcomes overtime, then results at the population level would be best presented as age-cohorts for each of 5-year age groups. That is, for example, instead of presenting the outcomes for people in the age group 45-54 at baseline and ‘end of program’ (which, in the ‘end survey’ would include old cohort participants and new 45-54 year olds who are measured for the first time in 2007), results could be presented for people in five-year- age-cohorts: Those who were 45-49 in the baseline in 2002, then 50-54 at the next time point in 2007. Likewise for the older age groups. This will effectively show changes for the people in a particular age-group at different time points and would provide a more direct estimate of the Programme effect over time.

For those communities where the older age-groups were not included in the baseline survey, comparisons at follow-up should exclude the older age-groups.

MINOR ESSENTIAL REVISIONS (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Figure 1. The boxes with population names and numbers should be also labelled according to whether they are intervention or control communities, for the benefit of the international readership.

Captions in tables 2 and 3 do not specify if the data in the first column refer to baseline or ‘end of the study’

Figure 2 : if the publication is black and white it would be very laborious for the reader to identify the differentials, in particular because the labels on the X axis are not present in every graph. I would suggest either using patterns or colours. Even better, I would suggest only presenting the 3 categories of Normal, overweight and obese (all subcategories aggregated).

Page 8: Changes in BMI and prevalence of obesity & Page 15 under ‘conclusions’. Given that response rates were below 63% and below, in particular
in control areas, it is unclear how the authors can make a claim that participation rates were ‘rather high’ (p 13) and that prevalence decreased. If the characteristics of non-respondents are not known, perhaps more appropriate wording could be “prevalence was lower at the 2007 survey…”

Page 14: subheadings with limitations and strengths need to be incorporated. This is because the issue of bias in the measured sample needs to be emphasised (no older people in the baseline survey in the control areas, relatively low participation rates, cross-sectional estimates and differentials between intervention and controls).

Page 15: the conclusion that ‘prevention might have been successful in the areas where the programme was established’ may not be backed up by evidence. Given the findings, where most of the differences were not significant, a more accurate conclusion is that the authors are uncertain of the true extent of the change and future surveys may shed some light on the direction of this trend.

DISCRETIONARY REVISIONS (which are recommendations for improvement but which the author can choose to ignore)

The abstract looks comprehensive but the conclusions section of the abstract needs refinement. It reads like a broad, uncertain statement and it does not suggest a way forward.

The results make mention of geographic areas not shown in the map in Figure 1 (those where the oldest age group were not included in the initial survey). For the benefit of readers who are unfamiliar with Finland, it would be useful to include the names in the map.

Level of interest: An article of importance in its field

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