Reviewer's report

Title: Effect of lifestyle intervention for (pre)diabetics in real-world primary care: propensity score analysis.

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Reviewer: Paulina Vermunt

Reviewer's report:

Review report on the manuscript 'Effect of lifestyle intervention for (pre)diabetics in real-world primary care: propensity score analysis' by Joris Linmans et al.

As there is a great need for effective (nation-wide) lifestyle intervention programs for (prevention of) type 2 diabetes, I read this manuscript with great interest.

Discretionary revisions:

* In the baseline matching covariates: is a difference in educational level accounted for in socio-economic status?

* To my opinion, there’s no such thing as ‘nearly significant’. Results are either significant or not. A better expression would for example be that a trend was observed, which was however not significant.

Minor essential revisions:

* I agree with the authors that implementation of lifestyle interventions in real life settings is challenging and can therefore not be expected to have the same effectiveness as in experimental settings (see for example the review of Rosal, 2008 and the article from Simmons, 2010). However, the authors describe the translation of lifestyle interventions for (prevention of) type 2 diabetes in real life settings as ‘problematic’, which is to my opinion not supported by the literature mentioned (1 to 2.5 kilos weight loss compared to weight gain in the general population is not problematic). In line with this comment: the background- section is written in a very negative manner, which does not tempt to continue reading.

* In the methods-section: Motivation to lifestyle change is one of the inclusion criteria mentioned. However, the authors do not mention how this motivation is measured.

* In the methods-section: the authors do not describe what ‘usual care’ consists of.

* In the methods-section: the authors mention that four out of ten centra were selected for inclusion of participants: on what criteria were these four selected and could this lead to bias?

* Are prediabetics part of the regular diabetes management program? And if not,
how / when were outcome measures of this group registered?

* The authors do not show how they arrived at their sample-size calculation and whether the lack of significant results can be due to a lack of statistical power.

* In the methods section: the authors describe very well how they performed the propensity score matching. However, to my opinion further detail is required on the secondary analyses, in which intervention effectiveness (with corresponding p-values) is assessed.

* In table 1: the authors should reflect on the major differences between groups for for example age and marital status (which are both known to influence behavioral change).

* In table 1: for the frequencies: both N and % are mentioned, which does however not correspond with the legend. Furthermore, mentioning only % would to my opinion would lead to a more clear presentation of the results.

* In the results-section: only some of the information from the tables are described in the results section. The authors could at least for example list which outcome variables were analysed to make the results-section more consistent.

* The authors do not reflect on the differences between prediabetics and diabetics, while motivation (and effectiveness?) of prediabetics may for example be lower than that of diabetics.

* The authors do not reflect on the differences between their study and other studies in primary care, which found much larger results on for example body weight (for example: intensity of the intervention, baseline risk profile).

* Participants were included in the study if they were referred by a professional (and had at least one lifestyle consultation). The authors should reflect on the percentage of patients that was referred to the program and how this (subjective?) referral method could lead to bias in the results.

* In the discussion-section: the authors do not discuss that their measure of exercise behavior is the opinion of the lifestyle coach and is therefore very subjective.

* In table 2 / 3: decreases in for example fasting glucose within the intervention group were relatively large: were decreases of this magnitude also not significant within groups? (I assume ‘we found no relevant changes in both the intervention group and the control group’ means no significant differences within groups were found?). Could this be due to a lack of statistical power?

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the
statistics.

Declaration of competing interests:

I declare that I have no competing interests.