Reviewer’s report

Title: Estimated 8-year cumulative incidence of diabetes mellitus among Sami and non-Sami inhabitants of Northern Norway - The SAMINOR Study

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Reviewer: Marit Eika Jorgensen

Reviewer's report:

The paper aims to compare the cumulative incidence of type 2 diabetes (T2D) among Sami and non-Sami in Northern Norway. The topic is of interest, the study is well conducted, and the paper generally well written. I have however some concerns about the methods applied.

Specific comments:

BACKGROUND:
- The hypothesis is not well stated. Why is it relevant to test a difference in T2D risk according to Sami status? Is the population suggested to differ genetically, socially, culturally from the Norwegian population in a way that should affect diabetes risk?
- From the introduction we know that baseline diabetes prevalence is not different according to ethnicity, but higher at follow up in SAMINOR 2 among Sami compared to non-Sami. Does this information apply to the total study population at baseline and follow up, or to the population participating in both surveys? While the study shows no difference in cumulative incidence of T2D across ethnic groups, the difference in prevalence at follow up must then necessarily be explained by a higher prevalence of non-incident cases among Sami due to lower mortality, lower migration rates or higher participant rate among Sami with T2D. This should be explained (and discussed) more thoroughly

METHODS
- How is the study population selected from the general population in the 10 municipalities? From households? A random geographic sample? And what is the population size in these municipalities? Have any efforts been done to ensure representation of both Sami and non-Sami? Is it likely that the name of the survey (SAMINOR) influences the motivation to participate for the non-Sami population?
- Study sample: A bit of information on the SAMINOR 1 would be useful. How many participated, how many were invited? It seems that 11,558 invites from SAMINOR 1 might have been invited for SAMINOR2. Is it correctly understood that non-participants in SAMINOR 1 potentially could have participated in SAMINOR 2? Would you be able to track such information? And would it be possible to connect to Norwegian registers for information on death, diagnosis of T2D?
- It is stated that the participant rate was 56.2% (3303 of 5875). However, usually, the participant rate is calculated from the eligible population (those still alive, still living in the relevant
municipalities). Thus, I assume, the correct participant rate is actually higher than 56.2%?

- Covariates: It seems that some of the covariates (e.g. income, SCL-10, marital status) may affect diabetes risk through other risk factors (e.g. obesity, physical activity). If the primary purpose is to adjust for confounders that may mask potential ethnic differences, I suggest that you include primarily covariates with an independent and biologically plausible influence on diabetes risk.

- Statistical analysis: The long list of separate analyses for each diabetes risk factor (beyond age and gender) seems inappropriate. Again, if the purpose is to adjust for confounders to un-mask an association between ethnicity and incident T2D, the relevant analytical approach should be a multivariable regression including all relevant confounders.

- Why is the SCL score categorized instead of using the continuous information?

- It seems inadequate to stratify for sex unless a significant interaction is observed (no indication of this in the data). The current approach assumes an interaction between sex and all covariates. It would be more correct to simply adjust for sex.

RESULTS:

- Para 2: Differences in risk factor levels for Sami and non-Sami (men and women) are described with table 2 as reference. Table 2 however, only describes differences between participants and non-participants.

- Table 3: The age-groups are VERY broad, and as mentioned before, I find no good argument for sex-stratification. For presentation I suggest instead a figure showing the cumulative T2D incidence as a function of age with a line for each ethnic group (men and women together). Alternatively that the estimated cumulative incidence is predicted for a specific age, sex, smoking status, BMI level etc.

DISCUSSION

- The discussion is very long and could easily be shortened by one third. E.g. the discussion about misclassification of diabetes with HbA1c is not really relevant here. Although the discussion about non-participation is important, this section could also be much shorter.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

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