Reviewer's report

Title: Prehypertension and long-term risk of type 2 diabetes: 35-year prospective population based cohort study of men

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Reviewer: Konstantinos Tsilidis

Reviewer's report:

This is an interesting study that prospectively evaluates the association between systolic and diastolic blood pressure with type 2 diabetes development. The findings seem to agree with other published work. Please read below for some important comments/suggestions.

Major Compulsory Revisions

1) Please list in the Results what was the median or mean follow-up time in your study (not just the maximum) and compare this number to the length of follow-up in other published studies.

2) You definitively have some residual confounding issues in your study. The limitations section in the Discussion should be expanded, where you should discuss potential residual confounding for specific variables and how this could have affected your results. Specifically, you have a never, former, current variable for smoking. Smoking duration or intensity is missing. Physical activity is crudely measured, no use of times/week of activity or something similar quantitatively. You have measured BMI, but this is an incomplete marker of abdominal obesity, which is important for diabetes development. Diet-related variables are also missing. What have other published studies adjusted for? Any difference in their findings based on which variables they adjusted for? To assure readers that residual confounding does not have a big role on your findings, I would suggest to perform some sensitivity analyses: i) re-run models after excluding all current smokers and ii) re-run models after excluding overweight and obese individuals.

3) Another potential explanation for your findings is detection bias, where hypertensives have increased medical surveillance and are closely prone for diagnosis of other diseases, like diabetes. For this reason and for reasons of reverse causation as well, I would suggest performing another sensitivity analysis, where diabetes cases that developed in the first 5-10 years after baseline are excluded. This will most likely not influence your results because of the small number of cases that developed early, but better be safe than sorry.

4) I read with interest the section in the Discussion, where you report the repeated blood pressure measurements in a random subsample. This should be part of the Methods section instead. You should also discuss whether this misclassification of BP is expected for some reason to be differential according to
diabetes diagnosis or not?

5) The variable for the use of anti-hypertensive medication is not optimal, because it is contaminated by the effect of hypertension. It is better to study and compare the effect between different drug classes among hypertensives. If you do not have such information, then it is better to develop a hypertension variable using some cut-points for SBP and DBP plus any use of drugs, and test its association with diabetes.

6) In Table 2, please show more than one multivariable models, so that people can judge the amount of confounding present in the study. You could present a model adjusted only for age and/or BMI plus the fully adjusted model next to it, and also discuss which of the adjustment variables influenced more your results, if any.

Minor Essential Revisions

1) The background section of the Abstract (especially two first sentences) needs some rephrasing because there are several linguistic mistakes.

2) How many of the 956 diabetes cases came from the death certificates?

3) It would be interesting if you could offer an explanation for the inverse association seen in Table 1 between smoking and SBP.

4) In Table 1, age seems not to be associated with SBP. Is the p-value<0.0001 a typo? Please also add a footnote on how the p-values were evaluated. It would be nice also to see not only the %current smokers, but also the % former and never smokers on the table. The characteristics should be better sorted so that all the blood pressure or obesity variables are near each other.

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