Reviewer's report

Title: Increasing risk of hyperglycaemia and the impact of high household income in transforming Rural China

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Reviewer: Barbara Schumann

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Reviewer’s comment on the manuscript by Fu et al. “Increasing risk of hyperglycaemia and the impact of high household income in transforming Rural China”

The article by Fu et al. describes a well conducted study addressing socio-economic risk factors of diabetes, an important issue of epidemiological transition in non-western countries. Methods, results and limitations are described and discussed generally in a critical manner; they adhere to relevant standards for reporting. The data seem sound; apparently no related work is unacknowledged. The article is generally well written and has a clear structure. However, I recommend a copyediting by an English native speaker. A range of critical issues should be addressed, as described in the following.

Minor issues not for publication

Table 1: wrong percentage for male medical insurance (996.2%)

Discretionary revisions

1. Table 1: Numbers and % of IFG and DM by sex might be added, including test for difference.

2. Tables 2 and 3: The column title “No.” is a bit misleading. Also, percentages for the respective numbers in this column could be provided. The legend in tables 2-4 should provide definitions (cut-off values) for IFG and DM.

3. Low income and hyperglycaemia

It is interesting and could be discussed that, although not significant, low income is associated with lower risk of IFG (adj. OR 0.74), but with higher risk of DM (OR 1.74).

4. Comparison with prevalences in China

The authors claim that prevalences of IFG and DM in the study population were similar to those found in urban China. This is not fully correct, according to the numbers provided. Furthermore, if prevalences are not age-standardised and include different age groups, such a comparison is not very informative.

5. Definition of physical exercise / adjustment in regression models

According to the discussion chapter, it appears that physical exercise includes only leisure sport activities, but not physical activities at work or homework, such
as gardening. Please provide a more concrete definition in section 2.3. The term “physical exercise” should be used consistently in text and tables rather than “regular exercise”.

Prevalences of physical exercise are very low (~2%), so this variable could be omitted from the regression models.

6. Assessment of income

To quantify household income by self-ranking is highly unreliable and prone to bias, which is also mentioned by the authors in the discussion section. They claim that this measure was highly correlated to per capita income (in Yuan). However, a correlation coefficient of 0.44 should not be considered as high, even if it is statistically significant.

Self-rated relative household income as defined by the authors describes income a) in terms of subjective wealth, b) relative to others’ incomes, and c) regardless of household size (number of members). Objective income per capita in Yuan, on the other hand is a) more precise, b) is independent on income of others in the community, and c) takes household size into account. Thus, the two do not measure the same thing. I would suggest to calculate regression models with objective income as a continuous or ordinally scaled variable additionally to models with self-rated income.

Minor essential revisions

7. Introduction

It is stated that income, education and occupation are associated with low diabetes risk (2nd paragraph). Do you mean by that high income, education and occupational position? Also, the literature shows diverse results in that respect; e.g. Maty et al. (2005) found no effect of income and occupation in adjusted models.

8. “Significance”

When mentioning “significance” e.g. in section 3.3, the authors seem to refer to statistical significance. However, the level of significance (#) has not been declared before. I recommend to do that in the methods chapter. See also 17).

9. Occupation

It should be mention which occupations other than farmers were included, especially white-collar professions. Otherwise the comparison of farmers with “non-farmers” does not provide much information.

10. Response: Please indicate total response proportions as well as by sex in section 3.1 (based on numbers of those eligible and those included in the final analyses).

Major compulsory revisions

11. Title / Abstract

The title “increasing risk of hyperglycaemia” is misleading, as the results are based on a cross-sectional study and conclusion about trends cannot be made.
In the abstract it should be mentioned which covariates were included in the regression models. The conclusion of the abstract “High household income was significantly associated with an increased risk of hyperglycaemia” is not supported by the data analyses. These show a statistically significantly increased risk of IFG, but not of diabetes in the high income group (see also 17).

12. Research question
The research aim is not stated well (introduction last paragraph). The conceptual/medical distinction between impaired fasting glucose and diabetes should be apparent in the research question.

Was the focus put on income only after data collection, because results seemed, unlike for education and occupation, to support previous studies?

13. Recruitment of participants (2.3)
It is not fully clear how the eligible study population was identified (“Data collectors … visited each household… and identified eligible study subjects”). Please indicate how both households and eligible subjects were defined and identified. The process seems prone to selection bias if identification is solely up to the data collectors and the persons present in the household at the time of the visit. This might be an issue for the discussion chapter, too.

14. Adjustment for confounders
The authors describe results of unadjusted and fully adjusted regression models of the association of social status on hyperglycaemia. However, the adjusted model does not contain only potential confounders (age and sex), but also factors such as BMI and alcohol intake which should be considered as intermediate factors rather than confounders. I suggest to add a regression model with age and sex as the only covariates. It will give valuable insights and also have higher power to detect statistically significant differences between income groups.

15. Role of education
As the article deals with socio-economic status and diabetes, the results concerning the (absent) impact of education should be addressed and compared to the impact of income in the discussion chapter.

16. Discussion / conclusions
A more detailed discussion of key results regarding the prevalence of IFG and diabetes as well as the association of socio-economic indicators (education, occupation, income) with hyperglycaemia should be provided. According to the data, both low and high income are associated with a higher risk of diabetes, although this result is statistically non-significant. The authors seem to focus on the role of high income, which conforms with results of other studies, and neglect the controversial role of low income on impaired fasting glucose and diabetes.

A comparison of participants who provided blood samples with others is given in the last paragraph of the discussion, which is very useful. However, it should be explained who the “others” exactly are and on which data the comparison is based. Are there estimates about the social status and diabetes of...
non-respondents?
The authors should interpret their results either in terms of statistical significance (which level?) OR in terms of effect size and width of confidence intervals. If significance is the main criteria, the conclusion “High household income was a risk factor for the prevalence of hyperglycaemia” is misleading, because high income was associated significantly only with IFG but not with DM. Although high odds ratios in diabetes were found for low and high income, these turned out to be statistically non-significant because of low numbers. The problem of statistical power should be mentioned in the discussion.

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

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.