Reviewer’s report

Title: The contribution of risk factors to socioeconomic inequalities in multimorbidity across the lifecourse: A longitudinal analysis of the Twenty07 cohort

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Reviewer: Wojciech Drygas

Reviewer’s report:

The paper prepared by internationally recognized authors from the University of Glasgow is of high interest for large group of experts dealing with the problem of socioeconomic inequalities and their relationship with health status, lifestyle factors and multimorbidity. The problem of multimorbidity in disadvantaged populations is of high importance both from theoretical point of view as well for preparing effective preventive interventions. Despite the large health inequalities literature the paper of Katikireddi and co-workers based on longitudinal observation of Twenty07 cohort is original and well-written. Results and conclusions are of high practical importance.

However some supplementary information is needed as well as some critical points should be discussed by authors before printing the paper:

1. Although definition of multimorbidity is very simple, various methods are used in international literature to collect data on multimorbidity and to classify the population with multimorbidity. Multimorbidity in West of Scotland is probably quite different as compared with multimorbidity in Brazil, China or in Russia. I mean the frequency of most prevalent chronic diseases in population analyzed and their clustering. Probably a table or figure with description of most prevalent diseases and if possible classes of diseases would be advisable.

2. The authors claim they analyzed five behaviour-related risk factors (smoking, alcohol consumption, diet, BMI and physical activity) as predictors of multimorbidity in Twenty07 cohort. In fact they analyze in the present paper not "diet" but "fruit and vegetables " consumption. Probably other important diet habits like salt consumption, red meat, vegetable oil consumption or saturated fats content could significantly influence developing multimorbidity in longitudinal observation.

Similarly, the description and classification of "physical activity" is not quite satisfactory. The authors have not analyzed or described in the paper various patterns of physical activity, thus leisure-time, work-related, domestic or commuting activity. Even the individuals exercising at least 20 minutes and > 3 days could not be described as very physically active.

I think these limitations of the study should be better described in the discussion.

3. I wonder why in the present analysis such important socioeconomic factor like education level is not taken into consideration at least in the discussion. Education level and health literacy could
significantly influence both behaviour-related risk factors as well other factors related to multimorbidity in long-term prospective study.

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

Yes

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

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

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