The studies stated aim was to analyse sex-specific time trends in the survival of patients aged 25-64 years for up to 23 years after a first myocardial infarction (MI) in Northern Sweden. Given the null finding the authors have instead elected to highlight survival findings for the cohort. An alternative interpretation of their findings is the apparent absence of differences in median time to survival following a first MI in 25-64 year old Swedish men and women.

General remarks
The paper is easy read and intelligible but would benefit from additional methods and more discursive interpretation.

Methods could be strengthened; e.g. how was the introduction of troponin biomarker testing handled in accordance with the “strict WHO MONICA criteria”, and whether data was available for all registered cases, if not, how was this handled in the analysis.

A greater appreciation for the data presented is more likely if the limitations raised in reference [16; also a co-author on this paper] were evidenced in the present study.

The standards of reporting should be tightened throughout the manuscript; e.g. in the abstract a 20-year survival is referred to; no reference to the null finding in either of the conclusions; include an average follow-up time; is any additional clinical information available other than mean age and sex to describe the characteristics of the four sub-cohorts.

A more considered discussion, in particular the limitations would add impact and context to the studies findings. Consider balancing the conclusion by reporting no difference in median survival time between men and women.

Additional specific remarks:
1. p7 3rd paragraph 3rd sentence: The contribution of lowering cholesterol level on reducing CHD mortality in Sweden is well raised but then subsequently glossed over when discussing the findings of the Northern Sweden MONICA study (p8 2nd paragraph) as a possible moderator on the results of the present study. Also, were rates of diabetes in men and women available?
2. P8 1st paragraph 2nd sentence: “That primary prevention has worked less well in women” is overstating the present studies findings. Your co-author in reference [16] offers up the introduction of troponin testing and increased sensitivity for the diagnosis of MI in women as a plausible explanation as to why the rates of first MI in young women may not have declined. Further, they go on to say the number of events particularly in those under 55 years of age is small and caution concluding remarks. Alternatively, its possible that no room for substantial further improvements in CHD mortality in young women is likely.

3. P5 results 1st paragraph, 3rd sentence: There is mention of a declining age on presentation with MI among women over time, which was not evident in men. Again, I note an apparent discrepancy to data reported in reference [16] for first events in women up until 2004 where no age decline was reported. Assuming your analysis is correct how would it inform the interpretation and understanding of differences between men and women in the present study?

4. P9 3rd paragraph: Consider including a remark about the possibility of MI severity being a confounder with interpreting your findings.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**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'