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The Cryosphere
Discussions

Interactive comment on "Brief communication: Interest of a regional climate model against ERA5 to simulate the near-surface climate of the Greenland ice sheet" *by* Alison Delhasse et al.

Anonymous Referee #2

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I would like first to thank the authors for a well written, clear and easy to follow paper. The comparison of the appealing model ERA5 with "its older version" Era-Interim and some RCM is of interest to the field, however I do think that the work done in this paper is incomplete and needs some improvements to obtain sustainable conclusions. As a general idea, I would like to see more "proofs" or arguments of some of the statements claimed by the authors (see detailed comments below). 1.- The AWS used do not cover the whole ice sheet and I was wondering why not using some other available data as the one provided by GC-net stations. 2.- The reasons given by the authors to exclude some AWS from the study need a better argumentation: "differences between interpolated elevations" shouldn't be a problem as long as the elevation correction is

performed. The authors claim, "as the comparisons were not improved we concluded that applying such a correction would add more uncertainties than using the raw modelled fields without any correction" which from my perspective is wrong: if the correction needs to be done, it needs to be done, the fact of doing it cannot rely on the results you are getting. Another reason of removing some AWS are "unfavourable comparisons" or "unfavourable statistics", I am quite reluctant of accepting those as fair reasons unless some more specific information about them is provided (percentage of missing data, values that are totally out of range because of measurement errors. . .) 3.- "all the models succeed in representing the daily variability of the surface pressure", in my opinion this cannot be concluded just by seeing values on Table S3-S7. Actually, some biases and RMSE are higher than I expected. 4.- The authors write several times about "statistical significance" when no hypothesis testing procedures seems to have been applied. Hence, without a test statistics we cannot conclude "just by eye" if a value is or not significant. Please be careful with that. 5.- "RMSE representing 30% of the daily variability" (p.5 l. 8) I am not sure how the authors could have computed that. Similar to p.7 l.12. 6.- "Two distinct elements can explain the statistical differences between the representation of T2M by the models considered here. First, the difference in altitude between the station and the corresponding interpolated model elevation" (p. 5 l.18). This is confirmed after having written at the end of page 4 that the elevation correction was not needed (see also item 2 in my comments).

7.- "To conclude, MAR shows the best accuracy when modelling T2M which might also lead to a better representation of the surface melt (not evaluated here) and therefore of the SMB." (p.7 l.8) I think this is too much to be concluded from the analysis the authors performed.

8.- "the correlation of the wind speed is neither sensitive to the vertical level used in MAR (2 m vs 10 m) nor to switching the forcing from EI to E5." (p.7 l.24) A statistical test has not been performed so I do not think the authors can claim that those correlations are not sensitive to vertical levels.

9.- In the discussion section be careful with using the terms “statistical significance” when no testing procedure has been applied.

10.- I agree to RMSE being a common element that does not need to be explicitly defined, but for the centred version at least the formula should be provided in the supplementary material.

11.- As probably a possible extension of this work some other measurements (more than annual or summer means) should be taken into account to fully analyse the behaviour of ERA5 against any of the other models.

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