

Interactive comment on “A note on the influence of atmospheric model resolution in coupled climate–ice-sheet simulations” by Marcus Löffverström and Johan Liakka

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I would like to thank the two anonymous reviewers and Irina Rogozhina and Raymond Sellevold for their comments and critical reading. My following remarks are thought to help in putting together your final responses.

Based on all the comments, there will have to be some expansions to the manuscript, however without having to give up on the general character of this study of being presented in a rather concise format (which I support). I suggest to pay attention to two points in particular.

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1) A bit more analyses of the results (as I assumed in my access review). – I would in any case suggest to tackle the lapse rate point by sensitivity analyses, and elaborate a bit more on the atmospheric model differences (causes).

2) More justification on the study design, see in particular the comment by Irina Rogozhina (and partly RC2). – I would like to add to this issue that the paper would also profit from clarifying the term "coupled" in the given context. Your study is clearly "one-way" (or "standalone", as you say in the paper) with regard to the ice sheet model. Most probably, for the general reader "coupled" would imply "interactive". Therefore, (a) be cautious with using "coupled" here (e.g., not in the title, see RC2 as well) and (b) provide a short argument early in the paper why standalone simulations make sense and/or are still state-of-the-art in face of the comments by Irina Rogozhina. RC2 also suggests in this regard you could argue with the objective of your study.

Small things to consider:

(i) Please explain/argue briefly why only 12 years are simulated with the AGCM. For the general reader, this might be unclear since you talk in the introduction about ice sheet response time scales of hundreds to thousands of years.

(ii) At which time step does the ice sheet model receive its input (daily or monthly)? Please state it explicitly.

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