

## Summary

The main objective of the paper "*Analysis of the simulated feedbacks on large-scale ice sheets from ice-sheet climate interactions*" by Zhiang Xie and Dietmar Dommenges is to present how different climate/ice sheets feedbacks affect the growth of Northern Hemisphere ice sheets and, how in turn the ice sheets have an impact on the global climate system. To do that the authors use GREB-ISM, a fast coupled climate-ice sheet model that has been presented in a previous study. This paper is therefore a first application of GREB-ISM. The authors find that the positive ice-albedo feedback to be the largest among the five that have been assessed. The authors conclude that without this feedback it is impossible to grow such large ice sheets (in the model). An interesting finding.

The scope of this paper is well within the scope of *The Cryosphere*. Climate feedback studies are useful, especially if they tackle such fundamental questions as the build-up of the large Northern Hemisphere ice sheets during the Quaternary.

My main criticism for this paper is the unrealistic nature of the applied forcing (CO<sub>2</sub> of 40ppm; solar insolation reduction to 95%), before the authors can even begin to study sensitivities and feedbacks. To me, this suggests that GREB-ISM is just not sensitive enough to (the more realistic) small variations in radiative forcing. Probably a lack of water vapor feedback and lack of realistic atmospheric and oceanic heat transport) to grow large NH ice sheets. I submit that it is difficult to develop a model that captures the important physical processes in a realistic manner, and at the same time computationally fast. The authors should still make sure on which end of the model type spectrum (toy model ↔ fully coupled Earth System model) their model (GREB-ISM) is located. To quote the authors:

"The simplicity of the model comes with the limitation that the dynamical mean state of the prognostic variables is relatively far away from the observed." (p. 3, L98)

However, I would still recommend this paper for publication, but only after major revisions (see my *General and Specific comments* below), because after all the readers and the community shall and will decide about the significance of this study. I know it's a lot of comments, but I hope the authors find value in the suggestions.

Good Luck!  
Mario

---

## General comments:

- "Precipitation intensity is often also linked to mountain slopes, as steep topographical changes typically result in heavy precipitation" (p. 2, L40) → It does also depend on the prevailing wind direction. For example, foehn events lead to drier and warmer conditions on the lee side of a mountain range.
- "ice latent heat" (p. 2, L44) → I would replace the term "ice latent heat" with "latent heat of melting" throughout the text.
- "In addition to the five feedbacks outlined above" (p. 3, L68) → Numbering of the feedbacks would make it clearer for the reader, e.g., as a list.
- "by introducing flux corrections" (p. 3, L99) → Why do you think a flux correction is necessary if you are not running any "realistic" climate simulations anyways?
- "prescribed wind fields" (p. 4, L103) → I can't see how you would be able to study the "ice sheet-topography" feedback in a physically meaningful way.
- "advection and diffusion of heat and moisture is scaled down for increased topography elevation" (p. 5, L143) → Please explain how this is done (equation?); Is there any literature that show how and why this works? E.g., how do you scale down advection?
- "CO<sub>2</sub> concentration" (p. 5, L155) → CO<sub>2</sub> is an external forcing because you don't account for (bio)geochemistry feedbacks.
- "and solar insolation" (p. 5, L155) → I find this misleading. Quaternary ice age variations are a result of Earth's orbital variations that affect incoming solar radiation (and their seasonal distribution). What you are suggesting is to reduce the solar insolation (to 95% of its current value.) This is far from reality, and I can only speculate why you do that: 1) You won't get glacial inception with GREB-ISM. Probably, because it is not sensitive enough to small variations in insolation. 2) For that reason you also have to reduce CO<sub>2</sub> to 40ppm [sic!] (L163), a unrealistic value (for any geological time scale). It's not a typo, is it?
- "A control simulation" (p. 5, L158) → Please include a plot for your control simulation, e.g., for T<sub>surf</sub>.
- "We designed the forcings of the FULL experiments to allow 160 the growth of large-scale Northern Hemispheric continental ice sheets" (p. 5, L160) → What is the equivalent radiative forcing to your CO<sub>2</sub>/incoming SW reduction? To

me, both experiments are equivalent, if they imply the same radiative forcing. I would therefore suggest to drop one of the two scenarios. I would even say that your results for both scenarios (and the tested sensitivities) are the same throughout, or, at least I couldn't find any substantial differences in any of the figures and numbers. As a result, you cut your and the readers' time you spent on discussing and contrasting the two scenarios in half.

- "five process switches" (p. 6, L168) → It would be useful for the reader to see how the feedbacks enter the model formulation. Please include the relevant equations from the model description paper (e.g., in an Appendix).
- "a framework to evaluate the feedback strength for ice sheet effect is used in our discussion" (p. 7, L209) → Please, give the reader more background about this feedback framework, as it is the reason for your particular design of your experiments. To quote from said paper (their page 9):

"The methodology requires explicitly identifying (1) a perturbation or a class of perturbations, (2) a response variable involved in the feedback loop, (3) the full system with all processes operating and its response to the perturbation, and (4) the reference system with the process of interest *not operating* and the reference system response to the perturbation." [my emphasis]

- " $c$ " is the feedback strength" (p. 7, L214) → While you are following the *Goose et al. (2018)* definition of a general feedback I would suggest to use  $\gamma$  instead of  $c$ .
- "global mean of about  $-7^{\circ}\text{C}$  in" (p. 8, L236) → Using the temperature response and the applied radiative forcing (reduction), this could be translated into the traditional climate sensitivity. It would be useful to see how your model climate sensitivity compares to other models (and observations).
- "much weaker" (p. 8, L263) → Can you quantify this? You compute dimensionless feedback factors, so I assume there is a way to make them comparable, at least for *ice sheet thickness* as response variable.
- "indicating that this feedback is mostly an amplifying feedback" (p. 8, L265) → What does "mostly" imply here?
- "This suggests that the build-up of the Arctic ice sheets does hinder the formation of a northern central Asian ice sheet." (p. 9) → I find this quite interesting. Is there a way to further investigate the causes of this hinderance?
- "here are coastal points" (p. 9, L284) → I think it would be useful to exclude (or mask) those coastal points from the analysis as they become qualitatively different in their climate response.
- "in more detail in the next section." (p. 10, L302) → I've read this now three or four times. It indicates that something is wrong with the structure of the paper, or of your arguments. Please, help the reader and revise the structure so the readers don't have to jump back and forth.
- "ice transport and ice sheet size feedback" (p. 10) → If you refer to these terms, please make sure that you introduce them to the reader.
- "Second, the topography feedback in those early studies also included the atmospheric circulation changes, such as stationary wave patterns, which are absent in our study." (p. 10, L326) → I think this is critical and one important reason to **not** include the topography sensitivity in your study.
- "This is an interesting subject that warrants further investigation." (p. 11, L345) → This is an opportunity you should not miss. The albedo representation in your model setup is almost too simple to trust that the feedback has any real meaning. For example, your land albedo is as small as the ocean, but should be in the order of 0.2-0.5 (e.g., bright deserts). Is it worthwhile exploring different albedo schemes?
- "longer snowing seasons" (p. 11, L358) → Can you quantify this? E.g., from X days to Y days.
- "snowfall rates" (p. 11, L359) → Is it larger snowfall rates or accumulated snow throughout the longer winter season?
- "control climate" (p. 12, L379) figure caption says FULL, and I thought control is present-day with no large NH ice sheets.
- "blocking the flow of air across the newly formed mountain ranges" (p. 12, L393) → I would like to see how this blocking looks like in practice. I assume the (u,v) winds have been adjusted (based on something, I can't find in this paper), similar to the "flux corrections", so show the blocking in terms of a vector field. (For example, Fig 5, <https://journals.ametsoc.org/view/journals/clim/25/6/jcli-d-11-00218.1.xml>)
- "without any other external forcing," (p. 13, L398) → Topography only means that ice sheets are mountains with prescribed land albedo? And only lapse rate (and wind corrections) operating?
- "Further studies with more realistic simulations of changes in the atmospheric and oceanic circulation need to be conducted to better understand the global impact of ice sheets" (p. 14, L444) → This is true in general. But how would you address this problem in your model, specifically?
- In general, this paper could benefit from proof-reading or copy-editing. There is a lot of fluff and unnecessary words (see below for a selection)

---

## Specific (or technical) comments:

- "In study" (p. 1, L8) → "In the study"

- "yrs" (p. 1, L9) → "years"
- "response" (p. 1, L13) response of what? Surface temperature?
- "has" (p. 1, L22) → "have"
- delete "will" (p. 1, L25)
- "model simulations" (p. 1, L26) system using climate model simulations.
- "relation" (p. 1, L31) → "relationship"
- "albedo" (p. 1, L32) → "ice-albedo"
- "snowfall" (p. 2, L35) → just "snow"
- "," (p. 2, L39) → no comma here
- delete "essentially" (p. 2, L50)
- delete "As a result," (p. 3, L82)
- "temperature tendency equation" (p. 6, L189) No such equation is shown.
- " $c_{ALBD} = c_{FULL} - c_{NOALBD}$ " (p. 7, L219) → Shouldn't it be:  $c_{ALBD} = \frac{c_{FULL} - c_{NOALBD}}{c_{FULL}}$ , according to Goose *et al.* (2018) ?
- "lifting" (p. 8, L242) Please use a different terms, as this could suggest to mean (tectonic up)lifting which it doesn't.
- "gird" (p. 8, L246) → grid
- "," (p. 9, L269) → no comma
- "but" (p. 9, L270) replace with "and"
- "all feedbacks have a direct feedback" (p. 9, L298) → What? Rephrase.
- "all feedbacks have an opposite sign feedback on the surface temperature over remote ice-free regions with varying strength" (p. 9, L299) → This sentence is really confusing and needs reworking. Try to clarify what you want to say here.
- "latter" (p. 10, L314) Not clear if this refers to "weaker for the surface temperature" or "the snowfall feedback" from the previous sentence.
- "as we only consider" (p. 10, L315) replace with "as can be seen in the"
- "significant" (p. 10, L319) What do you mean by "significant"?
- delete "adjacent" (p. 11, L332)
- "abortion" (p. 11, L333) absorption
- "The effect relatively strong in the Arctic" (p. 11, L337) → There is a verb missing: "is"
- delete "conceptually" (p. 11, L339)
- "what has been described" (p. 11, L339) And what is that?
- delet "above physical process of the" (p. 11, L340)
- change "is" to "are" (p. 11, L344)
- change "Snowfall rate" to "Snow" (p. 11, L347)
- delete "Most" (p. 11, L348)
- "local or zonal mean" (p. 11, L350) → Which one is it? Having the equation for precipitation would be useful.
- change "northern hemisphere" to "Northern Hemisphere" (p. 11, L354)
- delete "clear" (p. 11, L355)
- change "decrease" to "decreased" (p. 11, L360)
- delete "The development of the" (p. 11, L361)
- delete "The ice latent heat required to melt ice is substantial." (p. 12, L366)
- delete "substantial" (p. 12, L368)
- "allow the ice sheets to accumulated" (p. 12, L369) → check grammar
- change "sheet" to sheets (p. 12, L382)
- delete "clearly" (p. 12, L388)
- "(Fig. 7i,j)" (p. 12, L393) → I assume you mean Fig. 8.
- "NPREP" (p. 13, L397) → This should be listed in Sect 2.3
- "s" (p. 13, L404) → capital "S".
- change "lowers" to "drops" (p. 13, L406)
- change "bedrock shallower" to "a bathymetry lower" (p. 13, L407)
- "," (p. 13, L414) → no comma
- delete "but" (p. 14, L440)
- change "minor" to "small" (p. 14, L440)
- "This" (p. 14, L442) → What does "this" refer to here? Please, clarify.
- change "does simulate" to "simulates" (p. 14, L442)
- (p. 14, L443) Please add: ", a limitation of the GREB-ISM." to "..., but not in the oceanic heat transport."

- delete "further" (p. 14, L445)
- change "most significant" to "dominant" or "strongest" (p. 14, L445)
- delete "like to be" (p. 14, L447)
- delete "clearly somewhat" (p. 15, L467)
- "does not include all important aspects." (p. 15, L467) → I think you want to say something else, or do you really mean: "The above discussion ... does not include all important aspects."?
- delete "As already mentioned above" (p. 15, L467)
- change "later" to "latter" (p. 15, L472)
- "The GREB-ISM model can address such problems, but may also need further development to address some more complex aspects." (p. 15, L473) This is very vague and unspecific. Delete?
- "mm dy-1." (p. 31, L635) → I don't know what that unit is