Revisions of "Analysis of the simulated feedbacks on large-scale ice sheets from ice-sheet climate interactions"

Dear Editor and referees,

we like to thank the two referees and editor for the time spend on reviewing this manuscript and for the many very helpful comments they provided. We think the referee comments have helped us to substantially improve the presentation of this work.

With best regards,

Zhiang Xie, Dietmar Dommenget

Referee #1

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 Dommenget 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 author 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 (CO2 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

Response: Thank you for your thoughtful review and insightful comments. We respond to

each of the comments below. We also clarify in our responses to the other comments that the model has a realistic mean state, due to the flux correction terms and a realistic climate sensitivity, including a realistic water vapor feedback. We also better explain the motivation for the solar radiation forcing, arguing that, although idealized, it is somewhat realistic.

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 dependent on the prevailing wind direction. For example, foehn events lead to drier and warmer conditions on the lee side of a mountain range.

Response: That is correct. Now the sentence has been modified as "Precipitation intensity is often also linked to mountain slopes, as steep topographical changes typically result in heavy precipitation over upwind slopes" in main text.

"ice latent heat" (p. 2, L44) -> I would replace the term "ice latent heat" with "latent heat of melting" throughout the text.

Response: Thanks for your suggestion. "ice latent heat" has been replaced with "latent heat of melting" while "ice latent heat feedback" has been replaced with "ice melting latent heat feedback".

"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.

Response: In response, the sentence now reads as follows:
"In addition to the five feedbacks outlined above, which we will mainly discuss in this paper and listed in Table 1, there are several other feedbacks associated with the climate-ice sheet interaction."

"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?

Response: We are running "*realistic*" simulations, as each simulation is related to a control simulation that is closed to today's mean climate. Without flux corrections this would not be the case and would strongly alter the outcomes not only on a regional scale. We are also arguing that our response experiments, although idealized, are not entirely unrealistic. See also response to the other points. We have added a sentence in the model description to better highlight that the flux correction will ensure a realistic mean state in the control simulations.

"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?

Response: We have now included an appendix explaining all important equations of the GREB-ISM. We hope this now clarifies how the topography affects different processes, including the transport of heat and moisture.
While the GREB-ISM does not have complex atmospheric circulation changes as they would be simulated in GCMs, it does have some simulation of changes in atmospheric transports. Given that the literature on AGCM simulations on these time scales are rare or non-existing, we think that the discussion of the GREB-ISM results is relevant and can give a first order approximation against which future more complex simulations with AGCMs can be compared.

"CO2 concentration" (p. 5, L155) -> CO2 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 radation (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 sesnitive enough to small variations in insolation. 2) For that reason you also have to reduce CO2 to 40ppm [sic!] (L163), a unrealistic value (for any geological time scale). It's not a typo, is it?

Response: The reviewer comments make it clear that we have not well motivated our solar and CO_2 forcing experiments. We therefore revised section 2.3 (Design of sensitivity experiments) to better argue for the forcings and explain the motivations.

In short summary, solar insolation variations over the past million years are in the order of 20 W m^{-2} for a 24 hrs mean in summer for the higher latitudes, which corresponds roughly to about 5% of the solar constant. This is based on analysis of data from Huybers and Eisenman (2006). See also Abe-Ouchi et al. (2013). However, these solar radiation variations are not globally uniform, but have complex meridional and seasonal patterns that are different at different time scales. To simplify the experiments we conduct a -5% solar radiation reduction scenario.

Since the GREB model does not consider the carbon cycle, we can for the purpose of these experiments consider both, the solar and atmospheric CO_2 variations, as external forcings, and focus on understanding the climate-ice sheet feedback in the presence of solar insolation and CO_2 forcings.

In the CO_2 reduction scenario, the CO_2 concentration drops from 340 ppm in the control to 40 ppm in the scenario, which is not something that has been observed in the past million years, but was chosen to mimic a global mean response similar to the solar radiation reduction scenario. The separation of the solar and atmospheric CO_2 in these two scenarios allow us to analysis potential differences in the climate response to the difference forcing agents. Both scenarios present a relatively strong forcing, but are not entirely unrealistic in amplitude of response. Both scenarios allow the growth of large-scale Northern Hemispheric continental ice sheets, which is important for the analysis of ice-sheet feedbacks.

Reference:

Abe-Ouchi, A., Saito, F., Kawamura, K., Raymo, M. E., Okuno, J., Takahashi, K. and Blatter, H.: Insolation-driven 100,000-year glacial cycles and hysteresis of ice-sheet volume, Nature, 500(7461), 190–193, doi:10.1038/nature12374, 2013.

Huybers, P., & Eisenman, I. (2006). Integrated summer insolation calculations. IGBP PAGES/World Data Center for Paleoclimatology Data Contribution Series, 79.

"A control simulation" (p. 5, L158) -> Please include a plot for your control simulation, e.g., for Tsurf.

Response: We have now included a figure in the appendix to show the annual mean Tsurf and ice distribution (Fig. A1).

"We designed the forcings of the FULL experiments to allow the growth of large-scale Northern Hemispheric continental ice sheets" (p. 5, L160) -> What is the equivalent radiative forcing to your CO2/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 scearios. 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 scenario in half.

Response: We like to keep both scenarios, as it is not immediately clear that they would result into the same response. Solar radiation forcing and CO_2 forcing do have regionally different forcing strength (e.g. solar is stronger in the tropics), and the feedbacks discussed in this study are also potentially different for different forcing types (e.g. ice-albedo feedback). Since, both forcings are active during ice-age cycles, we think the discussion is relevant, even though the response differences are small. This as such is an important result.

“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).

Response: We have included additional explanations and relevant equations related to the physical processes and switches in the 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 γ instead of c.

Response: Our reference to Goose et al. was misleading. We are not following Goose et al. in detail. We follow a simple linear energy balance equation, as also discussed in many other studies. We now cite Forster and Gregory (2006) as an example.

“global mean of about -7°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).

Response: We have now added a sentence for the climate sensitivity of the GREB model. The global surface temperature change at equilibrium for a doubling of CO_2, is found to be 2.85°C, which is similar to most CMIP6 models.
Given that the forcings applied in our experiments are very different from a doubling of CO_2, we think it is not helpful to state an abstract climate sensitivity value, but rather state the global mean temperature change for the given forcing, as we do in the manuscript.

"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.

Response: This part mainly focused on the zonal mean ice thickness change. The "much weaker" refers to less reduction from FULL experiment of zonal mean ice thickness in HEAT experiment compared with ALBD experiment (Figure 7b, d). The text in manuscript has been updated:

"While the ice melting latent heat feedback is also a positive effect, its impact is considerably weaker compared to the ice-albedo feedback. This is evident in the simulation results, where the ice melting latent heat feedback leads to ice sheet response of several hundred meters near 70°N (Fig 7b, d), whereas the ice-albedo feedback results in virtually no ice sheets."

"indicating that this feedback is mostly an amplifying feedback" (p. 8, L265) -> What does "mostly" imply here?

Response: We revised the text to better explain this. It was related to most regions.

"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?

Response: In the manuscript, we have conducted an extra experiment to explore this process in section 4.4. Given the length of the manuscript, we have to leave further analysis to future work, which we suggested also in the final discussion section.

"there 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.

Response: We think that it would be better to include them to give a full global climate discussion. The "qualitatively different" is relevant for these locations.

"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.

Response: Apologies for the confusion caused by the repeated reference to "detail in the next section." Upon revisiting the structure of the paper, we have made adjustments to provide a more coherent flow of information for the readers. We have addressed the ice sheet blocking effect in section 2.2 and connected it to the detailed experiment conducted in section 4.4:
"The ice sheet blocking effect induces opposite anomalies in the regions north and south of the ice sheets. A comprehensive analysis of this feedback will be presented in section 4.4."
The relevant repeated section jumps are deleted.

Additionally, the reference to "detail in the next section" related to global sea level change has been removed as it is redundant.

"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.

Response: We have simplified the statement to avoid confusion. We now only mention ice transport, which is also introduced in the GREB model description.

"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.*

Response: While it is true that our study does not incorporate atmospheric circulation changes and stationary wave patterns associated with topography feedback, it is important to note that these are not the sole factors contributing to the overall impact. This includes changes in the surface temperature by increased elevation, reduced humidity and related precipitation, and reduced atmospheric transports of heat and moisture. Given that the literature on AGCM simulations on these time scales are rare or non-existing, we think that the discussion of the GREB-ISM results is relevant and can give a first order approximation against which future, more complex simulations with AGCMs can be compared.
We have now included an appendix explaining all important equations of the GREB-ISM. We hope this now clarifies how the topography affects different processes, including the transport of heat and moisture. This illustrates that the GREB-ISM is indeed sensitive to topographic changes.

"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 worthwile exploring different albedo schemes?

Response: We indeed agree that this is an important aspect that needs further investigation. We do highlight this in the final summary section and suggest further studies.

"longer snowing seasons" (p. 11, L358) -> Can you quantify this? E.g., from X days to Y days.

Response: We have now quantified this. The increase are up to 80 dyas in the first 20yrs of the scenario simulation.

"snowfall rates" (p. 11, L359) -> Is it larger snowfall rates or accumulated snow throughout the longer winter season?

Response: It is an increase in accumulated snowfall, which we now stated in the manuscript.

"control climate" (p. 12, L379) figure caption says FULL, and I thought control is present-day with no large NH ice sheets.

Response: We apologize for the confusion caused by the mistake in the figure caption. You are correct that the term "control climate" refers to the present-day climate with no large

Northern Hemisphere ice sheets. The figure caption has been updated to accurately reflect the experimental setup.

"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)

Response: We have now included an appendix explaining all important equations of the GREB-ISM. We hope this now clarifies how the topography affects different processes, including the transport of heat and moisture.
There are a number of processes that affect the atmospheric transport. They are related to equations A2, A4, A9 and A10.

"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?

Response: That is correct. In the topography-only experiment described in the paper (Figure 17a, b), we manipulate the surface elevation (z_{topo}; see model equations in the appendix) of the grids to represent the presence of ice sheets as mountains. The modifications in surface elevation lead to alterations in various factors, including surface temperature, diffusion rate, and precipitation, which are influenced by the new mountainous topography. This experiment allows us to isolate the effect of topography changes on the climate system.
We have now included the appendix with the model equations to better illustrate what the model is simulating.

"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?

Response: In our current version of GREB-ISM v1.0, fully dynamic-coupled atmospheric and oceanic circulation is not included. However, there are alternative approaches that can be employed to address this limitation. One possible method in the GREB-ISM is to incorporate another prescribed wind field derived from the Last Glacial Maximum (LGM) and assume linear changes in the meridional and zonal wind fields from the present-day conditions to the LGM conditions based on global sea level changes. This approach would require additional adjustments to the flux correction scheme and the implementation of benchmark experiments. While these developments are beyond the scope of our current study, we acknowledge the importance of considering more realistic simulations of circulation changes and will explore these possibilities in future. However, we do not want to add more discussions in the manuscript, to keep the discussion short.

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)

Response: We have carefully worked through the manuscript again to improve the presentation.

Specific (or technical) comments:

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

Response: Done.

“yrs” (p. 1, L9) -> "years"

Response: Done.

“response” (p. 1, L13) response of what? Surface temperature?

Response: We revised it to “response of the climate system ”.

“has” (p. 1, L22) -> "have"

Response: Done.

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delete “will” (p. 1, L25)

Response: Done.

“model simulations” (p. 1, L26) system using climate model simulations.

Response: We revised the sentence.

“relation” (p. 1, L31) -> "relationship"

Response: Done.

“albedo” (p. 1, L32) -> "ice-albedo"

Response: Done.

“snowfall” (p. 2, L35) -> just "snow"

Response: Done.

“,” (p. 2, L39) -> no comma here

Response: To better express the idea, the sentences have been changed as: “The snowfall feedback is closely linked to the topography feedback, as the decrease in precipitation due to surface temperature drop is also influenced by the elevated surface height of ice sheets.”

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delete “essentially” (p. 2, L50)

Response: Done.

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delete “As a result,” (p. 3, L82)

Response: Done.

“temperature tendency equation” (p. 6, L189) No such equation is shown.

Response: We have now included an appendix in which the equation is show and this is now referenced in this section.

“$c_{ALBD} = c_{FULL} - c_{NOALBD}$” (p. 7, L219) → Shouldn't it be:

$c_{ALBD} = \frac{c_{FULL} - c_{NALBD}}{c_{FULL}}$, accoring to *Goose et al. (2018)* ?

Response: The reference to Goose et al. has been misleading. We follow a simple linear energy balance equation, as also discussed in many other studies. We now cite Forster and Gregory (2006) as an example.

“lifting” (p. 8, L242) Please use a different terms, as this could suggest to mean (tectonic up)lifting which it doesn't.

Response: “topography lifting” is replaced with “surface elevation rise”.

“gird” (p. 8, L246) -> grid

Response: Done.

“,” (p. 9, L269) -> no comma

Response: Done.

“but” (p. 9, L270) replace with "and"

Response: Done.

"all feedbacks have a direct feedback” (p. 9, L298) -> What? Rephrase.

Response: We revised this section.

“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.

Response: We revised this section.

“latter” (p. 10, L314) Not clear if this refers to "weaker for the surface temperature" or "the snowfall feedback" from the previous sentence.

Response: To avoid misunderstanding, it has been changed to: “The comparable strength of ice melting latent heat feedback and snowfall feedback is an interesting finding.”

“as we only consider” (p. 10, L315) replace with "as can be seen in the"

Response: Done.

“significant” (p. 10, L319) What do you mean by "significant"?

Response: We revised the sentence and now state “There is a clear topography feedback for the ice volume …”.

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delete “adjacent” (p. 11, L332)

Response: We revised the sentence to better highlight the local and global effects.

“abortion” (p. 11, L333) absorption

Response: Done.

“The effect relatively strong in the Arctic” (p. 11, L337) -> There is a verb missing: "is"

Response: Added now.

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delete “conceptually” (p. 11, L339)

Response: Done.

“what has been described” (p. 11, L339) And what is that?

Response: More detail has been included now: “In general, the ice-albedo feedback in our simulations is conceptually similar to what has been described in previous studies, where the increase in ice cover leads to an increase in surface albedo, resulting in decreased absorption of solar radiation and subsequent cooling (Fyke et al., 2018; Willeit and Ganopolski, 2018).”

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delet “above physical process of the” (p. 11, L340)

Response: Done.

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change “is” to "are" (p. 11, L344)

Response: Done.

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change “Snowfall rate” to "Snow" (p. 11, L347)

Response: Done.

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delete “Most” (p. 11, L348)

Response: Done.

“local or zonal mean” (p. 11, L350) -> Which one is it? Having the equation for precipitation would be useful.

Response: It is both and we revised the text. We have now included the model equations in the appendix, which better illustrates how precipitation is calculated.

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change “northern hemisphere” to "Northern Hemisphere" (p. 11, L354)

Response: Done.

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delete “clear” (p. 11, L355)

Response: Done.

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change “decrease” to "decreased" (p. 11, L360)

Response: Done.

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delete "The development of the" (p. 11, L361)

Response: Done.

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delete "The ice latent heat required to melt ice is substantial." (p. 12, L366)

Response: Done.

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delete "substantial" (p. 12, L368)

Response: Done.

"allow the ice sheets to accumulated" (p. 12, L369) -> check grammar

Response: Now it has been changed as: "In the context of the seasonal cycle, it plays a crucial role in overcoming the warm summer season and facilitating the accumulation of ice sheets from one winter to the next."

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change "sheet" to sheets (p. 12, L382)

Response: Done.

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delete "clearly" (p. 12, L388)

Response: Done.

"(Fig. 7i,j)" (p. 12, L393) -> I assume you mean Fig. 8.

Response: Yes, you are right. Sorry for the confusion and it has been corrected.

“NPREP” (p. 13, L397) -> This should be listed in Sect 2.3

Response: This was a typo. And it should be “NSNOW”.

“s” (p. 13, L404) -> capital "S".

Response: Done.

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change “lowers” to "drops" (p. 13, L406)

Response: Done.

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change “bedrock shallower” to "a bathymetry lower" (p. 13, L407)

Response: Done.

“,” (p. 13, L414) -> no comma

Response: Done.

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delete “but” (p. 14, L440)

Response: Done.

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change “minor” to "small" (p. 14, L440)

Response: Done.

“This” (p. 14, L442) -> What does "this" refer to here? Please, clarify.

Response: The sentence has been changed to “However, the ocean circulation can also modify the remote influence.”

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change “does simulate” to "simulates" (p. 14, L442)

Response: Done.

(p. 14, L443) Please add: ", a limitation of the GREB-ISM." to "..., but not in the oceanic heat transport."

Response: Done.

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delete “further” (p. 14, L445)

Response: Done.

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change “most significant” to "dominant" or "strongest" (p. 14, L445)

Response: Done.

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delete “like to be” (p. 14, L447)

Response: Done.

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delete “clearly somewhat” (p. 15, L467)

Response: Done.

“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."?

Response: It has been changed: “The above discussion of feedbacks, while providing valuable insights, is idealized and does not encompass all important aspects.”

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delete “As already mentioned above” (p. 15, L467)

Response: Done.

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change "later" to "latter" (p. 15, L472)

Response: Done.

“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?

Response: It has been deleted.

“mm dy-1.” (p. 31, L635) -> I don't know what that unit is

Response: It is millimeter per day. They are all changed to “mm d^{-1}” in both manuscript and figures.

Referee #2

This study uses a simplified coupled ice sheet-climate model to analyze feedback between large-scale ice sheets and the climate system. The climate part is a global energy balance model with an invariant wind field. The ice sheet model has four vertical layers and a positive degree day scheme to calculate the surface mass balance. One of the main findings is that the albedo feedback dominates ice growth. This is not groundbreaking in itself but I welcome the author's approach to take advantage of a computationally tool to systematically test the sensitivity of the ice-climate system.

The scope of the manuscript is interesting and well-suited for The Cryosphere (although to me Climate of the Past is an even better fit). My criticism focusses on the limitations of the GREB model and the extent to which they are tested and discussed in the manuscript. I think these aspects must be addressed before publication.

Response: Thank you for your valuable comments and suggestions. We revised our manuscript to address the reviewers point in respect to the limitations of the GREB model and the aims of this study. Please, see our response to the specific comments below.

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1) It is difficult to understand to what degree the results depend on model limitations. If I understood correctly, GREB uses a prescribed and time-invariant wind field. This average field and (invariant) statistics about its variance are then used to prescribe moisture transport. This is a strong limitation as previous studies have found the dynamic response of, e.g., the stationary wave pattern (Löfverström and Liakka, 2016) or local circulation changes around ice sheets (Merz et al., 2014a,b) to be very important. Also, the ocean circulation in GREB cannot change, which is another strong limitation. I understand that testing the assumptions that went into making the efficient model can only be tested fully in a more complex model, but it should be possible to estimate some simplifications by adjusting model parameters within GREB. As an example, there must be a parameterization for meridional heat transport by the ocean that could be changed to approximate changes in the circulation. Such changes are believed to be essential for climate-ice sheet interactions on longer time scales. Similarly, the lack of a dynamic response of the atmosphere to the growth of the Laurentide ice sheet

should be tested. How important are the feedbacks in the presence of this additional effect? This needs to be quantified.

Response: The reviewer is correct in pointing out the limitations of the GREB-ISM model regarding the prescribed and time-invariant wind field and the absence of dynamic changes in ocean circulation. It is indeed difficult to evaluate the limitations of the GREB model, given the lag of observations or more complex model simulations.

In this study, our aim was to present a series of sensitivity experiments focusing on different feedbacks. It is indeed valid to look at model parameter variations and how they affect the results, but given the length of the current manuscript, we think this is beyond what can be done within this one study.

We tried to improve the introduction of the GREB model and its limitations. We further discussed the results carefully and pointed out potential limitations. It is the nature of such simplified model simulations that they can only give a first guess. In the final section we discuss a number of avenues on how this study should be continued to address the model limitations and potential model parameter uncertainties (e.g. ice albedo, ocean/atmosphere circulation).

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2) Related to this first point, I would like to see a more detailed discussion of the merits that the author's approach holds. Why should simulations with a strongly simplified model be considered by journals and their readers? How can these simple models answer questions that more sophisticated models cannot? Does the manuscript in its present form really take advantage of the low computational cost of GREB-ISM? I do not think so as it appears to only present a handfull of simulations, each only representing a few hours of time on a regular single CPU. Why were not more changes in GHGs tested? Why not different changes in solar irradiation? Also, what does a 5% reduction in solar output mean for simulations that run over 100,000 years? Is this a constant offset? Does it have seasonality? GREB-ISM can, and maybe should, be used to do more comprehensive tests of Milankovitch forcing, including the importance of time scales (obliquity, precession, etc.).

Response: We have tried to better introduce the GREB model and highlight its usefulness for this study. In our manuscript, we have conducted over 14 pairs of fully transient sensitivity

experiments using GREB-ISM, with each about 100,000 yrs long. In total these are more than 1.5 mill. yrs. of simulations, which would be challenging to accomplish with more computationally expensive models. The total time cost for these experiments exceeds more than 10 days, even in our simplified model setting. Therefore, while the computational cost is low compared to more complex models, we have made efficient use of GREB-ISM to isolate and analyze specific feedback processes, offering valuable insights into the ice sheet-climate interactions.

Regarding the suggestion of exploring more comprehensive tests of Milankovitch forcing, we agree that it is an interesting topic for future investigation. However, our current study aims to provide a basic concept of how the Earth system responds to solar and greenhouse gas forcing in a global uniform way. The consideration of Milankovitch forcing introduces additional complexities and non-uniformities to the system, which deserves a dedicated analysis in a separate study. We appreciate the suggestion and will keep it in mind for future research.

Minor comments:

- The manuscript requires language editing. I think that running individual paragraphs through GPT or similar would probably solve 95% of the issues.

Response: A proofreading has been done and we corrected some parts with help of GPT.

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line 138: "Atmospheric blocking" usually refers to a specific type of circulation anomaly, different from what is meant here.

Response: It has been changed to “ice sheet blocking effect”.

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line 179: Most (all?) of the processes described here cannot be addressed with GREB-ISM

Response: The GREB-ISM is able to simulate these topography and snowfall feedback processes. The detail of physical process and relevant equations for each switch in the GREB-ISM is now included in the appendix.

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figure 6: This only presents anomalies. How does the ice topography of FULL without perturbations look?

Response: The results of the control run have been included in the appendix (Figure A1).

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figure 10: The division by 10 for one of the columns must be immediately visible in the figure without the need to read the caption.

Response: Thanks for your suggestion. We now use "$\frac{1}{10}$ALBD" in the xlabel to indicate the downscaling manufacturing process.

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figure 12: What does "mm/dy" mean? Per day (d) or per year (yr)?

Response: It is millimeter per day. They are all changed to "mm d^{-1}" in both manuscript and figures.

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figure 14: I found this figure and the corresponding text difficult to follow and cannot say I am convinced.

Response: We revised the discussion of the text, the organization of the figure and the figure caption to improve the presentation.

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figures in general: I think there is too many figures for the relatively straightforward point that the manuscript is trying to make.

Response: We have included multiple figures to provide a comprehensive and robust presentation of our findings. Each figure serves a specific purpose in illustrating different aspects of our analysis and helps support the main points of the manuscript. We believe that the inclusion of these figures enhances the clarity and rigor of our study.

Literature (if not yet included in the manuscript):

Merz et al. 2014b: https://doi.org/10.1002/2014JD021940

Merz et al. 2014a: https://doi.org/10.5194/cp-10-1221-2014

Response: Done.
