Dear Dr. Dijkstra,

Please find enclosed a thoroughly revised manuscript. We have carefully addressed all comments by the two referees. Their detailed and thoughtful comments and constructive suggestions have greatly improved our work, for which we are very grateful.

We believe that Earth System Dynamics is the best outlet for our work, in spite of alternatives by the same publisher such as Geoscientific Model Development or The Cryosphere. ESD offers a very broad and attractive scope that does not exclude the introduction of new tools such as numerical models. Notwithstanding, we strengthened the scientific discussion of the existing and new simulations in the revised manuscript as suggested by Reviewer #2.

A point-by-point list of changes and responses to the comments of the reviewers is found below. Changes in the revised manuscript are highlighted with blue text.

We hope that with these extensive and substantive revisions, the manuscript is now acceptable for publication in ESD.

Thank you for your editorial work. We are looking forward to your further communication.

Best regards,
Basil Neff on behalf of all authors

Referee #1 (Bas de Boer)

Overall recommendation

The manuscript by Neff et al. described a vertically integrated 2-D ice-sheet model. The novelty of the model is that due to the use of simplified dynamics, it can be used for long-term (paleoclimate) simulations and large ensemble simulations, as presented in the paper. Although there are quite some studies that even use 3-D ice sheet models (still with similar dynamics, i.e. only ice on land is simulated), this model can yet be an additional model, especially coupled to a climate model, to be used for paleoclimate glacial studies. In the introduction and conclusions, I do miss some specific references on similar work that has been conducted, see specific notes below.
In conclusion, the main idea behind the paper is good, but there are quite a few comments that need to be addressed, specifically on the scientific methods (point 4 below). Also, as stated below the language is not fluent in some parts of the manuscript. Moreover, including all comments and adjustments that need to be made, I suggest that this paper is accepted, but with major revisions.

General comments

1. Does the paper address relevant scientific questions within the scope of ESD?
Yes.
Response: No action required.

2. Does the paper present novel concepts, ideas, tools, or data?

The novelty of the model and especially where it can be used for (including referencing) should be more highlighted in the introduction. Although not a new and completely novel idea, I do think it is a useful additional.

Response:

The introduction have been revised to more clearly highlight the usefulness of our model and to clarify the contribution of this manuscript (See response to point 7).

3. Are substantial conclusions reached?

No. Mainly the model itself is presented and tested with different climate forcing fields and compared with reconstructions of ice sheet topography.

Response:

While it is true that a large part of the manuscript is dedicated to a thorough description and testing of the new model, we also show applications that are made possible by its numerical efficiency. The hysteresis simulations represent idealized tests that quantify ice sheet stability for different ice sheet volumes. This part has now been expanded with transient simulations of rapid temperature changes. To our knowledge, no comparable study exists for Northern Hemisphere ice sheets. Previous studies address this important research question with time slice simulations (Abe-Ouchi et al., 2013). Our work highlights a shortcoming of hysteresis simulations when used to interpret the response of ice volume to transient changes in the forcing climate, as has been proposed by Abe-Ouchi et al. (2013). We believe that applications such as this in addition to the fundamentally important work of developing new tools should be considered a substantial contribution to the published literature. The new tool and the analysis method we present enable a wealth of new research questions that are not feasible with existing models because of their numerical cost. This first publication on the description of the model and characteristic applications to show its potential is a necessary first step to enable such applications.

4. Are the scientific methods and assumptions valid and clearly outlined?

Rather well. In Section 2.1 on ice dynamics it should be clearly noted that this type of model can only be used to model ice on land.

Response:

A short statement has been included.

I have a few comments on specific parts of the model:

Bedrock relaxation

A more standard way of calculating the bedrock change is the ELRA (Elastic Lithosphere, Relaxed Asthenosphere) model rather than the local isostatic model used in the manuscript (see Le Meur and Huybrechts, 1996). At least a more elaborate discussion the ELRA model, which can be readily implemented since you use a 2-D domain, would be good to include at some point. Other referencing that might be useful (e.g.): Zweck and Huybrechts (2005), Van den Berg et al. (2008).

I do not require you to change your bedrock model, but please add a note on this also in the Conclusions and outlook for future work.

Response:

We implemented the ELRA model in our ice sheet model and ran test simulations. It produces results that are comparable to the current LLRA model, i.e., with local lithosphere. One unexpected but arguably predictable result of these tests was a significantly increased computational overhead when compared to the current set-up. Due to the non-local effect on the bedrock, with a characteristic scale-length of about 130 km, the ice load of about 600 neighboring grid cells has to be taken into account. This increases the number of calculations for each time step by about two orders of magnitude compared to the efficient ice dynamics.
Our original plan was to complete one simulation with the ELRA model and show the results in the manuscript. Due to technical difficulties with the implementation and the very long integration time, this simulation has not finished yet. The ice thickness at the most recent time step (Year 47,000) is included in Figure 1 of this document. Differences of the ELRA model compared to the LLRA model are minor. Moreover, it is reasonable to assume that differences during the ice build up are larger than in equilibrium.
In light of these results we decided not to pursue the ELRA for this paper but to consider it in a subsequent upgrade of the model.

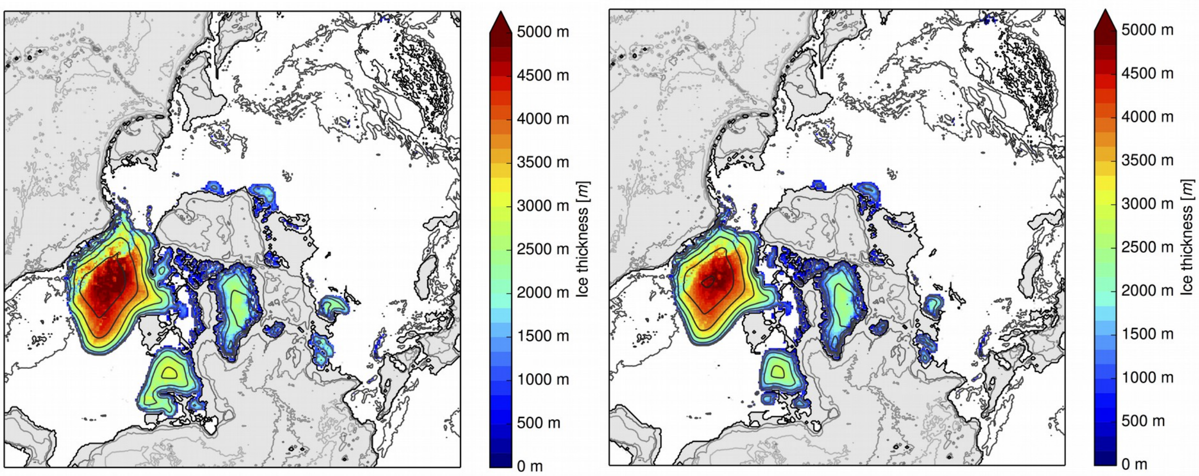

Figure 1: Ice thickness after 46,000 simulated years (not in equilibrium) with different bedrock models. On the left the so-called "local lithosphere relaxing asthenosphere" (LLRA) model with a comparable volume of -77.2 mSLE. On the right the "elastic lithosphere relaxing asthenosphere" (ELRA) bedrock model with a volume of -61.2 mSLE.

In addition to the simulation with the ELRA model on the Northern Hemisphere, we also performed tests on an idealized domain, similar to the EISMINT moving margin experiment (Fig. 2 in this document). These simulations confirm that our implementation of ELRA performs as intended.

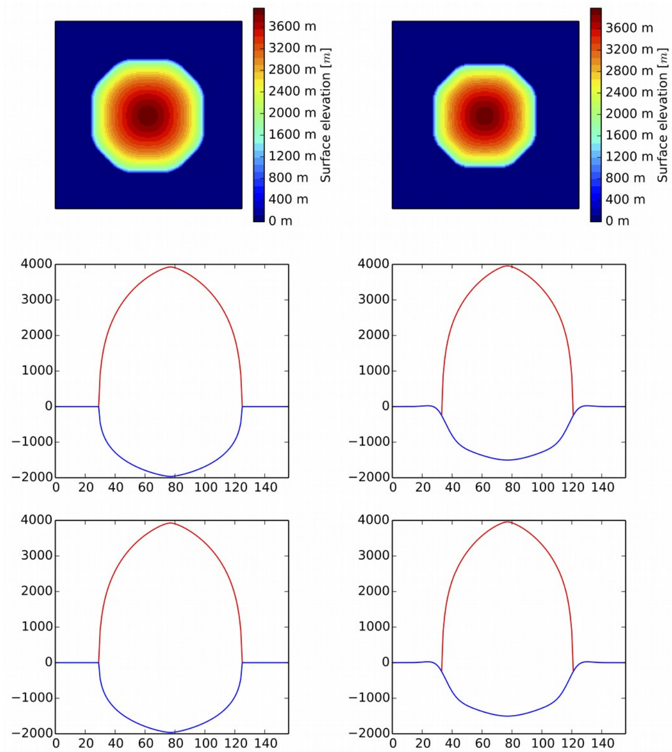

Figure 2: Test simulations on a square domain to see the influence of the different bedrock model. On the left the so-called “local lithosphere relaxing asthenosphere” (LLRA) model with a comparable volume of virtual 125 mSLE and a summit elevation of 3946 m. On the right the “elastic lithosphere relaxing asthenosphere” (ELRA) bedrock model with a volume of 97.3 mSLE and a summit at 3980 m. Also note the simulation of a small periglacial bulge in ELRA. **Top:** ice thickness; **middle:** section in x-direction; **bottom:** section in y-direction.

Surface melt

Can you explain why you choose to use the PDD melt model with only one parameter and not two? Again, other types of models can be discussed. As shortly touched upon in the conclusions (bottom of page 1420).

Response:

The ice sheet model has been written with a strong focus on simplicity and numerical efficiency, but also with very low requirements with regard to the forcing fields. Therefore, the model does not calculate the extent of the snow cover nor does it need such data as a boundary condition in its present form. The second reason why we have not used a more complex version of the empirical PDD method to describe the surface mass balance is that we are currently working on a physical model to describe the snow and firn layer, extending work by Greuell and Konzelmann (1994) and Reijmer and Hock (2008), as outlined on page 1420.

However, there is a very general argument to be made on the use of empirical parameterizations with multiple free parameters. Since PDD parameters are only very weakly constrained by physics, they can be chosen from a very wide range of potential values. Adding a second parameter and another degree of freedom to the optimization would enhance the agreement with observed fields at the cost of computational efficiency and further weakening the physical basis of the model. In summary, we found that a single PDD factor yields results of sufficient quality for the envisioned applications of our model. The use of the minimal version of PDD also avoids potential overfitting.

This information is included in section 2.3 of the revised manuscript (previous section 2.2).

SMB

With respect to a statement on the seasonal cycle on page 1404. Please add a few lines in section 2.2 how you actually calculate SMB, this is a bit unclear. Do you calculate annual accumulation and ablation separate before adding up and interpolating to the ice grid, did you try to calculate SMB on a daily or monthly time scale perhaps?

Also, I am wondering if the precipitation correction you apply in equation (5) on page 1403 is necessary when you already use an LGM precipitation field. These kind of effects on both precipitation and temperature are all included in the CCSM-LGM simulation, due to the boundary conditions that are applied (large ice sheets, lower CO2, etc.) and the use of an actual atmospheric model! I suggest you give the paper of Zweck and Huybrechts (2005) a good read. They have used both LGM and PD simulations of a GCM to force their ice-sheet model for the NH as well, using a glacial scaling factor to simulate ice volume over the last glacial cycle, also a good paper for comparison with another simulation.

Again, it should be made very clear if this desertification effect is needed when you use LGM climate forcing.

On the other hand, I think the height-desertification effect should be included when your reference climate does not include the ice sheets (for example over North America and Eurasia in the PD or PI GCM simulations). See also section 3.2 in de Boer et al. (2013) and references therein how this could be done when using a PD climatology as reference (might be something you could use for future studies).

Response:

We are grateful for these valuable comments. Temperature and precipitation data of the climate model are first interpolated onto the ice sheet model grid, before the accumulation and ablation terms are calculated individually. It is important to correct the temperature data for the changes in elevation due to the growing or decaying ice sheet and this can only meaningfully be done on the grid of the ice sheet model, hence after spatial interpolation. We use daily climatology data to better represent variations in surface air temperature and precipitation that in some regions show large variations. Also, especially in dry continental climates the bulk of annual accumulation might be due to sporadic precipitation events, i.e., a few moisture-bearing storms. Accumulation and ablation are calculated for one full year, to match the time step of the ice dynamics. We reordered section 2.3 (previous section 2.2) to clarify how we calculated the SMB and added the information above.

We expanded the description of the desertification effect to better explain when it is used and to what effect. Importantly, precipitation is only reduced when the ice sheet surface is above that already accounted for in the GCM. The GCM topography is one input data set in our model. There is a possibility to amplify the GCM precipitation in regions where the ice sheet surface is below that of the GCM. In practice, this is limited to the spin-up of our model when the Laurentide ice sheet is not present yet, but it is important to achieve a sizable ice sheet in this region because the GCM simulates a very dry climate for the LGM, as expected.
This approach is formally different from both Zweck and Huybrechts (2005) and de Boer et al. (2013). The former interpolate between two GCM simulations, one for glaciated and one with ice-free conditions, which accounts for some of the important interaction of ice sheets and precipitation. The approach used by de Boer et al (2013) is very interesting because it explicitly takes into account the intensification of precipitation near topographic barriers, which

is very important at the ice sheet margin. However, since state-of-the-art GCM simulations are available for the time period covered in our manuscript and their surface topography is well-constrained by multiple generations of LGM ice sheet reconstructions, we trust that our approach produces reasonable results for the application at hand. Again, all of this information has been included in the revised manuscript in section 2.3 on the surface mass balance.

5. Are the results sufficient to support the interpretations and conclusions?
Yes they are.
Response: No action required.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
Yes. Some specific remarks on the details of the experiments are given below.
Response:
We greatly expanded the model description, also to answer the specific comments raised below.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
Own contribution should be stated more clearly in the introduction. Some references are missing when discussing previous work. For example, you could add a few lines on other (ice-sheet) model studies that use an ensemble approach, for example: Robinson et al. (2011) or Stone et al. (2013).
Response:
The introduction and the summary sections have been revised to more clearly highlight the usefulness of our model and to clarify the contribution of this manuscript. Several references have been added to the introduction. In addition, the beginning of section 4 now explicitly references three relevant studies that used a similar ensemble approach.

8. Does the title clearly reflect the contents of the paper?
Yes, a nice and concise title.
Response: No action required.

9. Does the abstract provide a concise and complete summary?
Yes it does.
Response: No action required.

10. Is the overall presentation well structured and clear?
The structure of the paper is good. I suggest section 4.2 is moved to the front of section 4 (so will be section 4.1), such that the model is first tested for the pre-industrial case.
Response:
We agree that this order may appear preferable, but we believe it would mislead readers and confuse the purpose of the optimization procedure. We optimized the model only for LGM data and use the preindustrial simulation primarily as a cross-validation test. It is possible that the best model version for the LGM produced unrealistic amounts of ice or spurious ice sheets when forced with preindustrial climate. This is not the case which supports the optimization scheme.
Therefore, we would like to keep the present ordering of sections.

11. Is the language fluent and precise?

There are quite some sentences that are not that fluent, also I found some grammatical/spelling errors here and there. I suggest that all authors read the manuscript thoroughly after revisions have been made. Specific suggestions and comments are given below.

Response:

Thank you very much for the detailed comments, we included almost all of them in the revised manuscript. Many parts of the manuscript have been rewritten to clarify the content and present it more precisely.

Specific comments

Page 1396

Line 6: Replace 'sinking' with 'deformation'; Add 'change' after sea level.

Response: Text adaptations done

L20: Rephrase: 'based on 5 million year long simulations' implies that a simulation has been carried from 5 Myr ago to the present.

Response: Sentence rephrased

Page 1397

L1-2: Change 120 to 'about 130' and replace Waelbroeck reference with more recent references: Lambeck et al. (2014) and Austermann et al. (2013).

Response: Sea level changed to about 130m, References replaced.

L 4: Change: .. that ice volume 'and temperature'.

Response: 'and temperature' added

L 4-15: This part basically discussed the mid Pleistocene transition, or at least the interaction on orbital changes, whereas the following lines (16-29) more describe the physical interactions of ice sheets with the climate, rather then the MPT. I suggest the part from 4-15 can be removed, and the second part (L16-29) should be condensed towards the more specific topics that can be addressed by the Bern2D ice sheet model.

Response:

We think this part of the (early) introduction is important to motivate the development and use of a vertically-integrated ice sheet model and to provide an overview of physical mechanisms that have been put forward as potentially causing the strongly nonlinear relationship between summer insolation and ice sheet volume.

We merged the two paragraphs to clarify their connection. In the following, now third, paragraph of the introduction, we added a statement to emphasize how we think our model and the work of this manuscript contributes to this discussion.

Page 1398

L 6-8: Other references that can be included are: Ganopolski and Calov (2011) and/or Stap et al. (2014).

Response: References added

Page 1399

Equation 1: put brackets around: ($D\nabla Z$) and add arrows on top of ∇ to indicate arrow format (since representing both x and y directions). Replace SMB with M, only use single

letters in equations.
Response: Neither brackets nor arrows are mathematically necessary or represent common usage. We use the form d/dx or d/dy for the one-dimensional derivative (e.g. equation 2). In equation 1, we added the brackets, but not the arrows as this would clutter the presentation in our view. Note that the paragraph following equation 1 explicitly states that ∇ is two-dimensional.

Page 1400
L 1: Replace SMB with M, and but (SMB) after 'surface mass balance'.
Response:
We replaced “SMB” with “M” in equation 1 and replaced all “SMB” with the explicit “surface mass balance” in the text to avoid confusion due to multiple abbreviations.

L 11: Replace 'so' with 'such'
Response: 'so' replaced with 'such'

L12: Equation 2: put a subscript 'ice' after the density ρ.
Response: (Equation 2): subscript added

L 14: Also refer to Table 1 for parameters A and n.
Response: Reference to table 1 inserted

L 16: Replace 'chose' with 'adopt'.
Response: 'chose' replaced with 'adopt'

L 20: Add section number 2.2.
Response: Section number added

L 23: After hydrostatic equilibrium add reference to: Le Meur and Huybrechts (1996).
Response: Reference added

L 25: Change to: deforms under the loading (or pressure) of the ice
Response: Sentence rephrased

Page 1401
L24 page 1400 to L2: Reformulate the entice sentence, such that it describes the following process: increase in ice thickness, leads to a depression of the bed, leads to warmer surface temperature, leads to an increase in melt, leads to a decrease in ice thickness.
Response: Has been rephrased.

L 3: It should be stated here somewhere that this specific bedrock model describes:
local isostatic adjustment.
Response: We expanded the last statement in this paragraph. Line 11 on the same page (of the original manuscript) stated that the bedrock is adjusted only locally.

L 16: Remove 'resolution'.
Response: This has been changed.

L 16-17: Rephrase to something like: It is assumed that bedrock deformation is in close isostatic equilibrium today with ice over Greenland.
Response: Sentence rephrased

L 17-20: Please clarify here why you apply the local isostatic correction on Greenland. I assume this is because you start all your simulations with no ice on the NH..
Response: Explanation added

L 21: Change section number to 2.3.
Response: This has been changed.

L 22: Remove (SMB).
Response: The abbreviation SMB is written out everywhere in the revised manuscript and therefore not needed anymore.

L 22: Change to: the accumulation minus the ablation
Response: Explanation of SMB changed to “the accumulation minus the ablation”

L 23-24: Change to: drives the flow of ice, i.e. equation (1).
Response: Changed

L 24: The SMB
Response: This sentence has been rephrased in response to previous comments.

L 24: 'daily surface-air temperature'? Please clarify this, since this is not the time step of the ice-sheet model (it is stated later on that this is 1 year). Do you use daily temperatures to calculate the (thus) annual mean SMB for the ice-sheet model. How is this done for the long simulations, you continuously use daily temperatures, or how are they adjusted? Please explain.
Response:
The SMB is calculated for a one year, based on daily temperature and precipitation fields. Ablation is calculated from the sum of all daily temperatures over one year, multiplied with the PDD factor. The accumulation is the sum of the precipitation of all days with an average temperature below 0°C, although this threshold is changed in the ensemble simulations. Daily temperatures and precipitations are both adjusted for changes in surface elevation in all simulations. These corrections are performed before calculating accumulation and ablation. The SMB calculations are repeated every 50 time steps to reduce the computational cost.
We comprehensively reworked section 2.3 (previous section 2.2) to clarify all these issues.

<u>Page 1402</u>
L 1: “Accumulation .. C” is a strange sentence to start with. Do you mean: Here, we assume that accumulation depends on the surface-temperature alone, it is calculated as the cumulative precipitation below T_{acc} = 0 °C. Also explain cumulative, do you add up all precipitation over 1 day, a month, a year?
Response: This sentence has been rephrased as part of the reordering of the previous section 2.2 (now 2.3).

L 17: Change to: (e.g. Ritz et al., 1997)
Response: “e.g.” has been added

L 22-24: Table 2 does not seem so useful to me. Maps of temperature and precipitation over the area (climatology) of both the PI and the LGM would be very useful to interpret your results, especially why there is no ice during the LGM over Eurasia..

Response:

We added a figure to illustrate the CCSM4 PI climate and the LGM-PI anomalies. Nevertheless, to have the most important parameters of the climate forcing present without consulting an additional paper we would like to leave Table 2.

<u>Page 1403</u>

L2: Please clarify the resolution, in Table 2 you state that it is one degree.

Response: The version of CESM1 with an atmosphere resolution of 0.9° ×1.25° is commonly referred to as the one degree version. We realize that this might be confusing for the very attentive readers and thus clarified the caption of table 2.

L4: Remove a 'not'.

Response: “not” removed

<u>Page 1404</u>

L1-2: Change to: To remove the bias of the present-day CCSM climate, temperature of the surface of the CCSM simulations is subtracted from ..

Response: Start of sentence changed

L 9: Change subsection number to 2.4.

Response: Changed to subsection 2.4

L10-12: The specific domain you focus on could be mentioned earlier, for example in section 2 (top of page 1399). Moreover, that 80% of the changes in ice volume during the LGM happen on the NH is not a sound reason for me, you can leave that statement out. State it more like you want to focus on one region first, in this case the NH, and not on Antarctica (yet).

Response:

We agree that the model domain should be mentioned at the beginning of section 2 and made a short addition to the text there. Concerning the statement in question here, we also agree that is was poorly phrased. We propose a small but in our view very effective change: replace “because” with “where”.

L17-18: Remove: 'it is impossible to implement a seasonal cycle.'.

Response:

This sentence has been removed.

<u>Page 1405</u>

L1: Add section number 2.5

Response: Section number added

L 6-7: Change to something like: This offset is the equivalent total ice volume of the Greenland ice sheet of 7.36 m (Table 1).

Response: Sentence changed

L 10: I don't think you need to add a reference for the total volume of the ocean.
Response: Reference has been removed

L10-11: Change to: .. are presented as change in global mean sea level equivalent (m s.l.e.) relative to no ice on the NH. Replace mSLE with m s.l.e. everywhere in the manuscript.
Response: We would prefer to keep the current notation.

<u>*Page 1406*</u>
L7: Change to: above the bed.
Response: Changed

<u>*Page 1407*</u>
L 6: I would not include Denton, 1981. There are far more many new reconstructions that can be used, as cited. Primarily, the ICE-5G reconstruction should be used here, since it was the lower topography boundary condition for the CCSM LGM simulations. This should be noted here. Although a new reconstruction ICE-6G has emerged recently (Peltier et al., 2015)
Response:
Denton (1981) is cited because the reconstruction is also used in (Clark and Mix, 2002) and represents the upper limit (-132 m) of the gray bar. We included several and also older reconstructions so that the range of acceptable values from the ensemble is not too small. This information has been added to the revised manuscript. Also, the most recent reconstruction by Peltier et al. (2015) is mentioned now. Its estimate for LGM sea level falls within the range that we used before. The entire paragraph has been moved down to improve the clarity of the manuscript.

L19: Remove a 'the'
Response: This has been removed.

L27: Replace 'weak' with 'small'.
Response: “weak” replaced with “small”

<u>*Page 1408*</u>
L2: 'the mean values', which mean values do you refer to here?
Response: Refers to the mean values of E. Explanation added.

L4: 'minimum sea level'. It might be easier to refer here to the change in ice volume rather than the change in sea level. Do this consistently throughout the manuscript.
Response: Changed to “maximum ice volume”

<u>*Page 1409*</u>
L5: Replace: A longer relaxation
Response: changed capital letter to a small one.

L21-24: I would mainly focus here on Ice5G since this is the ice-sheet topography that was used for the CCSM simulations. Ice6G (Peltier et al., 2015) could be included.
Response:
Several reconstructions are used to obtain a range of reasonable sea level values for the LGM, as explained above and stated more precisely in the revised manuscript. For the evaluation of the geographical distribution, we use ICE-5G, because is was considered the

most robust at the time when we did the analysis. We now reference the most recent ICE-6G reconstruction but since its reconstructed ice distribution does not significantly differ from ICE-5G, we did not repeat this part of our analysis.

Page 1410
L1: See comments below on Figure 5
Response: Please see our response below to Figure 5.

L7-8: Throughout the manuscript you refer to the eastern and western Laurentide ice sheet. Be consistent in the whole manuscript, calling it the North American ice sheet might be better. In principal one should refer to the western part of the ice sheet as the Cordilleran and the eastern part as the Laurentide (see e.g. Clark and Mix, 2002).
Response: The manuscript has been revised following this suggestion.

L 10: Again the specific structure of Ice6G is different, this could be mentioned.
Response:
Indeed ICE-6G suggests rather large changes in ice topography, that we were not aware of. In particular, the very thick ice ridge over the center of the North American continent is not present in the more recent reconstruction, which makes it more similar to our simulation. This manuscript has been revised to include this information.

L13: Change to: In the LGM-bs simulations, the Eurasian ice sheet …
Response: Sentence changed

L 21-22: Remove "The Bering ... right)", more focus on regions where you have ice, and how this compares to ICE-5G.You should definitely check why you have no ice over Eurasia in these simulations (also see comments above on SMB).
Response:
The Bering Strait region is mentioned because it highlights an important difference between the climate forcing LGM_{uc} and LGM_{bs}.
A sentence regarding the underestimated ice in Eurasia has been added. There is only a short note about the constant forcing, a more detailed description is part of the new paragraph about the constant climate forcing in the section "Multiple equilibria in Northern Hemisphere ice volume" (4.3).

Page 1411
L 4: Replace 'minimum sea level', with 'maximum ice volume'.
Response: "Minimum sea level" replaced with "maximum ice volume"

L 6-7: Change to: Therefore, from now on only the ensemble LGM_{bs} simulations are considered in the results section.
Response:
Sentence changed. The overall text is also adapted to "ice volume" instead of "sea level" if the context supports it.

L 11-end of section 4.1: Why using mean values of your parameter space and compare that?. The mean parameter value completely depends on what parameter space you look at, and is not related to the actual results. Mainly in this section you should focus on the simulation that represent the observations (Ice-5G in this case) the best. The mean of all simulations can be *used, but merely to show where simulations agree or not. So the comparison between the ensemble mean and the best representative simulation is still valid. See comments to Figure 5 and 6 as well. Also, in the caption of Table 4, the mean values can be removed from my point of view.*

Response:

This might be a misunderstanding caused by imprecise wording, for which we apologize. We do not show the average of our parameter space, but the average of simulations that are consistent with the range of reconstructed LGM sea level reduction. We rephrased the caption of table 4 and the corresponding paragraph formerly on page 1411.

Page 1412

L 9: As stated above, place this section before section 4.1.

Response:

We believe that this would lead to confusion about how the best guess tuning parameters of the PI simulations are obtained. We therefore prefer to keep the current ordering.

L 24: Replace Amante and Eakins citation with Bamber et al. (2013). The difference between your Greenland ice volume and the volume of Bamber et al (as stated before 7.36 m) is quite large. Can you add an explanation for this?

Response:

We changed the citation to the more recent publication by Bamber et al. (2013). The excess ice volume in our present day simulation is mostly due to the coarse resolution of our model. The narrow ablation zone of the steep Greenland ice sheet is not sufficiently resolved. Moreover, much of today's ice discharge on Greenland is due to fast flowing outlet glaciers. These cannot be simulated in our model, again because of insufficient resolution of the narrow fjord systems but also because these rapid flows require a more comprehensive representation of the ice dynamics than the shallow ice approximation. Both mechanisms are a lesser concern for the continental-scale LGM ice sheets. All of the above information is included in the revised manuscript.

Page 1413

L 6: I am wondering why your simulations are that long? Please clarify in the text how your equilibrium experiments are set up. Is this long time integration necessary? The 100.000 year steady state seems quite long, did you check if it can be shorter, or is this the optimum length of a steady forcing?

My main concerns are with the temperature range you use. Since you use here either PI or LGM simulations, to me it feels more logical that you focus on this range of climatologies. Therefore, I suggest you keep your range around these climates, for example from +7 to -2 relative to the LGM (or +2 to -7 relative around the PI).

Your range of ice volume would be much more realistic, more in the light of a possible future experiment over Plio-Pleistocene glacial cycles, and not the really large ice sheets that did not exist over the past 1 million years.

Response:

For the hysteresis simulations, we chose a very slow gradual change in temperature so that the ice sheet is in continuous equilibrium with these changes. For a finely resolved hysteresis curve, this is computationally much cheaper than to run a great number of uninitialized simulations into equilibrium for each temperature offset. The relatively long duration of the 100,000 year equilibrium simulations was deliberately chosen in order to be sure that these simulations are fully equilibrated.

A new simulation has been added to Section 3 to illustrate that our model conserves mass, as requested by Reviewer #2. The accompanying figure shows that even though we employ a constant climate forcing, the simulation does not fully reach its steady state until after 200,000 simulated years. We hope that this answers also the current question satisfactorily.
As for the choice of forcing climate or a combination of several for the hysteresis, or the temperature range covered, we agree with all points made by Reviewer Bas de Boer and we are grateful for these suggestions. They are a valuable guide for our future work on this topic. However, for the present study, our aim was to keep the complexity of this concept to a minimum. In particular, we did not want to compound the effects of a change between different climates and the associated complex changes in temperature and precipitation patterns and seasonalities with the fundamental dynamic response of the ice sheet model to a temperature offset, which in itself is already surprisingly complex. The revised manuscript contains new simulations that represent a rapid change in temperature from the various equilibrium points on the hysteresis. One of the findings is that the hysteresis does not do justice to even these highly idealized changes in climate. Our own conclusion at this point is that the hysteresis is a potentially very useful tool to better understand the dynamics of Plio-Pleistocene glacial cycles, but also that some refinement of this method is necessary.

L 9: Remove (Table 2).
Response:
To have the most important parameters of the climate forcing present without consulting an additional paper we would like to leave Table 2 in the manuscript.

<u>*Page 1414*</u>
L1-3: This statement is in itself quite okay, As long as climate is cooling (although not happened in the cold climates of the past million years) the ice sheet can grow to the south because of the vast land mass. This can be already concludes from your initial steady state simulations shown in Fig. 3-4.
Response: This is correct.

L6-end: To make the three processes clear within the text, introduce them by starting the sentence with:
Firstly, ...
Secondly, ...
Lastly, ... (or And Thirdly, ..)
Response: Beginning of paragraphs changed

L15-16: Change to: individual ice sheets over North America, the Cordilleran and the Laurentide ice sheets, that combine to one big North American ice sheet.
Response: The manuscript has been revised as suggested. Please also see our reply to comment page 1410 L 7-8.

L17: Also add reference to Bintanja et al. (2008)
Response: Reference added.

L23-25: Change to: .. turns positive southern part of the North American ice sheet and its retreats quickly, especially the Cordilleran ice sheet disappears ..
Response: Sentence changed

L 25: Change to: The Laurentide ice sheet is not in ..
Response: Beginning of sentence changed.

Page 1415
L1-6: Similar behaviour has been described in de Boer et al. (2013), see Section 4.3 and Fig. 10 in our paper.
Response: Reference added

L 29: Clarify in the text why the constant climate forcing is unrealistic.
Response:
The constant LGM climate forcing is a poor representation of climate variations during the last glacial cycle. Considering the available literature, it is reasonable to assume that the Eurasian ice sheet of the LGM would not have grown in a continuous LGM climate. We agree that the original explanation was not sufficient and expanded this part considerably. It has also been separated into a new paragraph and two new references have been added in support of our results.

Page 1416
L 17-18: Is this mentioned before? It should also be stated clearly in Section 2.5 (Sea level section).
Response:
It is mentioned in section 2.5. The relevant sentence in Section 2.5 is extended to make it more clear.

L 17: Replace 'eustatic' with 'global mean'.
Response: Replaced

L 20: Do not start a new paragraph here.
Response: This paragraph has been merged with the following.

L23-25: What do you want to say with the sentence "Strong ... (2004)."?
Response: Sentence is rephrased to make the meaning clear.

Page 1417
L1-3: This statement can be removed. There are numerous of examples of simulations with (although 3-D) ice-sheet models, a few of which are referenced here. Some examples: Zweck and Huybrechts (2005), Charbit et al., (2007), Bonelli et al., (2009), Bintanja et al., (2005, 2008), De Boer et al. (2013, 2014).
Response:
The text has been changed to reflect our aim and the usefulness of our model more clearly.

Page 1418
L 3-4: Reword to something like: Although the ice sheets during the LGM where not in equilibrium, …
Response: Reworded

L 6-11: Remove this part: "LGM .. .. ice sheet"
Response:
We prefer to keep this paragraph to provide some perspective on the degree of realism of our

simulations and to understand the build up process of the North American ice sheet. Furthermore does it explain why a constant climate forcing may be unrealistic in the beginning (Comment from you an Page *1415 L29*).

L20: Replace 'further' with 'future'
Response: “further” replaced

<u>Page 1419</u>
L26: Change to: yields an ice volume
Response: Typo removed.

<u>Page 1420</u>
L 23: There are a quite a few examples of (3-D) ice-sheet models that employ a more comprehensive approach to calculate the SMB, in particular ice melt, that are largely based on the energy balance at the surface and including insolation effects (important for long-term studies on orbital time scales). You could check for example: Van den Berg et al. (2008), Robinson et al. (2010), de Boer et al. (2013).
Response:
We added two more of the suggested references.

<u>Page 1421</u>
L1: There are quite a few studies (with ice-sheet models and climate models) that did long-term simulations. You might refer to a few of these: Ganopolski and Calov (2011); Stap et al. (2014).
Response:
We rephrased this paragraph to better highlight previous work. The suggested references have been added.
Stap et al. (2014) work with zonally integrated/averaged ice sheet-climate models, which is an interesting but greatly simplified approach. We also refer to it in the introduction to the manuscript.

<u>Figures and Tables</u>
Table 1: citation for density is not needed.
Response: citation for density removed

Table 2: could be removed, since you did not carried out these experiments yourself..
Response:
As stated before, to have the most important parameters of the climate forcing present without consulting an additional paper we would like to keep Table 2 in the manuscript.

Table 4: Why mention the mean values of the parameters, see comments above. Please explain in the caption the meaning of '# Members'.
Response:
As outlined above, this is probably based on a misunderstanding. These are not the averages of the entire parameter space but only of those simulations that yield a sea level anomaly consistent with the range of reconstructions. We reworded the caption to avoid future confusion and to clarify the meaning of #Members.

Fig. 3: I think there is an overlap with the panels (a) and in (d) right, where you show the same experiments. Just using panels b-d would be sufficient to show all experiment. You should explain the panels from top to bottom, starting with (a). If you decide to not use panel (a), state the different Beta values in the caption.

Response:

There is a small overlap of information within the different panels. Due to its dominant influence on the ice volume, we show the spread of different values of beta also in panels b-d. However, they do not include information on the percentiles, which is why we prefer to maintain panel a) as well.

Fig. 5: Can be removed and Fig. 6 should be adjusted. Averages look good for interpretation of an ensemble, but do not depict an actual realization of your model.

Response:

We fully agree as this was our motivation to show two separate figures in the original manuscript. The original figure 5 shows the two ensemble averages. The original figure 6 shows the physically robust simulation with a single set of parameters. We believe it is worthwhile to keep figure 5, because (1) it illustrates that even though the ensemble averages do not necessarily yield a physically consistent ice distribution, they do produce reasonable results, and (2) to highlight differences between the two ensembles. Note that (original) figure 5 is discussed in detail in the main text of the manuscript. Removing the illustration would reduce the readability of the text.

Fig. 6: Add another panel that includes the best representative simulation of the LGM_{uc} simulations.

Response:

Simulations of the LGM_{uc} ensemble are discarded based on the analysis above. One main result is that those simulations that produce a sea level anomaly consistent with reconstructions do not simulate a connection between the Laurentide and Cordilleran ice sheets. Therefore they are not discussed further.

We hope that the revised manuscript clarifies this rationale.

Referee #2

This paper aims at presenting a new ice sheet model which can be used for paleoclimate studies. If focus was only on the model, then Geoscientific Model Development (GMD) would have a much better venue than Earth System Dynamics (ESD). But the authors also want to show that interesting results can be obtained with this model, in particular the hysteresis of the northern hemispheric ice sheet distribution versus global temperature offset (section 4).

On both aspects, however, the paper is not sufficiently developed to recommend publication and a major revision is needed. Below suggestions are given to improve the paper; only major issues are mentioned.

Response:

We agree that GMD would have been an almost equally well suited journal for our work. However, in our opinion this would have put too much emphasis on the model development aspect of the manuscript. Ice sheet models of similar complexity as ours have been presented before, but at that time the focus on numerical efficiency was motivated by the lack of computational power. The novelty of our approach is the deliberate choice to trade detail in the ice dynamics for the possibility to run very long simulations and large ensembles. Thus, it is very important to acknowledge that our model does not compete with current state-of-the-art ice sheet models. Our model rather fills a gap in the hierarchy of ice sheet models between more simplified and often unphysical models and comprehensive, but computationally heavy ice sheet models. We propose to use this new tool to comprehensively study glacial cycles and the dynamics of ice sheet-climate interactions. Some first experiments with idealized climate transitions are included in the manuscript to illustrate the type of analysis that is enabled by the new model. It is this broader aim that motivated our decision to submit this work to ESD instead of the more specialized GMD or 'The Cryosphere'. We trust that the editorial decision to accept the paper for pre-publication in ESDD took the wider scope of the manuscript into consideration.

1. In section 2, many essential details of the model are not given. For example, it is unclear how the surface mass balance (please use M instead of SMB in (1)) is precisely computed from the temperature and precipitation. The model uses a C-grid but, as only h and B appear to form the state vector, how are the variables staggered? What is the time step and is this accurate enough over long integration periods?

Response:
We added one paragraph to the end of section 2.1 to explain how the flow of ice is implemented on the staggered grid and explicitly state the time step of the model. We also included a new figure to visualize the details.
Concerning the surface mass balance, section 2.2 (now 2.3) has been reordered and extensive additional details have been added, also in response to the comments above by Reviewer Bas de Boer.

2. What is important to show in section 2 is that an integral mass balance (over the model domain) is satisfied in the model. As boundary conditions at the ice interface are dealt with in a rather sloppy way, such an integral balance may be easily violated with possibly large consequences for the solutions. If there is no integral balance of mass, the paper cannot be published.

Response:
This is a very valuable comment and we apologize of this oversight. We agree that it is very important to demonstrate that mass is conserved by the model, in particular for a model that is intended to run over multiple million years where imbalances could severely undermine the results.
We carried out a new simulation with the full model on the Northern Hemisphere domain, for which all relevant fluxes were recorded at every time step. Technically, this is a boolean flag that can be activated for future use if changes are made to the model. The results, including the new figure 2 in the manuscript, are reported in section 3 after the idealized benchmark simulations (EISMINT).
In summary, the model conserves mass close to machine precision. Unaccounted fluxes at each time step are about 11 orders of magnitude smaller than physical fluxes and stochastically centered around zero. Thus, the long-term mismatch is less than one liter of ice per year for the domain that covers all relevant regions of the Northern Hemisphere. All the above information as well as the new figure 2 have been added to the revised manuscript.

3. The most interesting results are presented in section 4.3 but these are only described, without any further analysis on the mechanisms of the hysteresis behavior. For a paper in ESD, it is important that further analysis should be done. So what feedbacks (mass balance-height, marine ice-sheet instability, ..) determine the transition from a solution on one branch in Fig. 8 to the other?

Response:
The experiments in section 4.3 illustrate one of the main strengths of the new model and we are heartened to see this being recognized. We have expanded this section with a series of experiments that simulate a rapid warming starting from certain equilibrium points on the hysteresis (black dots in figure). The results provide insight into the usefulness of the hysteresis as a concept for past changes in global ice volume. In particular, a rapid warming generally does not lead to a new equilibrium on the hysteresis but a previously unvisited steady state with a different geographical distribution of ice volume. This result has not been shown in this way before. We believe that communicating this result is important, also because of a recent publication that employ a similar hysteresis as an explanation for 100,000 year glacial cycles that involve many and repeated rapid warming events (Abe-Ochi et al., 2013).
Additional detail is also obtained concerning the hypothesis that smaller ice sheets are inherently more stable to warming than ice sheets at the glacial maximum, put forward by Paillard (1998). We find that this is not generally true, because it largely depends on regional thresholds for ice loss. A relatively small ice sheet may also experience rapid ice loss if one of its regions becomes unstable. This is interesting because it illustrates the importance of these small regions and the need to have reliable simulations of their climate back in time even though the problem of glacial cycles is usually treated as a global phenomenon.
Section 4.3 has been expanded to twice its size in the original manuscript to discuss these new simulations and their findings. A new figure 13 has been added.