Review

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.

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.

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.

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. 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 of 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.

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).

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).

5. Are the results sufficient to support the interpretations and conclusions?
Yes they are.

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.

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).

8. Does the title clearly reflect the contents of the paper?
Yes, a nice and concise title.

9. Does the abstract provide a concise and complete summary?
Yes it does.

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.

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.

Specific comments

Page 1396
Line 6: Replace 'sinking' with 'deformation'; Add 'change' after sea level.
L20: Rephrase: 'based on 5 million year long simulations' implies that a simulation has been carried from 5 Myr ago to the present.

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

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.

Page 1398
L 6-8: Other references that can be included are: Ganopolski and Calov (2011) and/or Stap et al. (2014).

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.

Page 1400
L 1: Replace SMB with M, and but (SMB) after 'surface mass balance'.
L 11: Replace 'so' with 'such'
L12: Equation 2: put a subscript 'ice' after the density ρ.
L 14: Also refer to Table 1 for parameters *A* and *n*.
L 16: Replace 'chose' with 'adopt'.
L 20: Add section number 2.2.
L 23: After hydrostatic equilibrium add reference to: Le Meur and Huybrechts (1996).
L 25: Change to: deforms under the loading (or pressure) of the ice

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.

L 3: It should be stated here somewhere that this specific bedrock model describes: **local** isostatic adjustment.
L 16: Remove 'resolution'.
L 16-17: Rephrase to something like: It is assumed that bedrock deformation is in close isostatic equilibrium today with ice over Greenland.
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..
L 21: Change section number to 2.3.
L 22: Remove (SMB).
L 22: Change to: the accumulation minus the ablation
L 23-24: Change to: drives the flow of ice, i.e. equation (1).

L 24: The SMB
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.

Page 1402
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?

L 17: Change to: (e.g. Ritz et al., 1997)
L 22-24: Table 2 does not seem so useful to me. Maps of temperature and precipitation over the area (climatology) of both the P-I and the LGM would be very useful to interpret your results, especially why there is no ice during the LGM over Eurasia..

Page 1403
L2: Please clarify the resolution, in Table 2 you state that it is one degree.
L4: Remove a 'not'.

Page 1404
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 ..
L 9: Change subsection number to 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).
L17-18: Remove: 'it is impossible to implement a seasonal cycle.'.

Page 1405
L1: Add section number 2.5
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).
L 10: I don't think you need to add a reference for the total volume of the ocean.
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.

Page 1406
L7: Change to: above the bed.

Page 1407
L 6: I would not include Denton, 1981. There are far more many new reconstructions that can be used, as cited. Primarily, the ICe5G 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 ICE6G has emerged recently (Peltier et al., 2015)
L19: Remove a 'the'
L27: Replace 'weak' with 'small'.

Page 1408

L2: 'the mean values', which mean values do you refer to here?

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.

Page 1409

L5: Replace: A longer relaxation

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.

Page 1410

L1: See comments below on 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).

L 10: Again the specific structure of Ice6G is different, this could be mentioned.

L13: Change to: In the LGM-bs simulations, the Eurasian ice sheet ..

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).

Page 1411

L 4: Replace 'minimum sea level', 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.

L 11-end of section 4.1: Why using mean values of your parameter space and compare to 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.

Page 1412

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

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?

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.

L 9: Remove (Table 2).

<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.

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

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.
L17: Also add reference to Bintanja et al. (2008)
L23-25: Change to: .. turns positive southern part of the North American ice sheet and its retreats quickly, especially the Cordilleran ice sheet disappears ..
L 25: Change to: The Laurentide ice sheet is not in ..

<u>Page 1415</u>
L1-6: Similar behaviour has been described in de Boer et al. (2013), see Section 4.3 and Fig. 10 in our paper.
L 29: Clarify in the text why the constant climate forcing is unrealistic.

<u>Page 1416</u>
L 17-18: Is this mentioned before? It should also be stated clearly in Section 2.5 (Sea level section).
L 17: Replace 'eustatic' with 'global mean'.
L 20: Do not start a new paragraph here.
L23-25: What do you want to say with the sentence "Strong ... (2004)."?

<u>Page 1417</u>
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).

<u>Page 1418</u>
L 3-4: Reword to something like: Although the ice sheets during the LGM where not in equilibrium, ...
L 6-11: Remove this part: "LGM .. .. ice sheet"
L20: Replace 'further' with 'future'

<u>Page 1419</u>
L26: Change to: yields an ice volume

Page 1420
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).

Page 1421
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).

Figures and Tables

Table 1: citation for density is not needed.
Table 2: could be removed, since you did not carried out these experiments yourself..
Table 4: Why mention the mean values of the parameters, see comments above. Please explain in the caption 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.

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.

Fig. 6: Add another panel that includes the best representative simulation of the LGM-uc simulations.

References

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