**Review of Bauer and Ganopolski, 2016, CPD**

This study compares two different approaches to simulate the ablation component of the surface balance over ice sheets. As there is currently quite some discussion on different ablation or melt schemes, this is a timely and relevant research topic, and the results are suitable to be published in Climate of the Past. However, I do have several general, and many smaller comments that I think should be discussed before final publication in Climate of the Past.

**GENERAL COMMENTS**

1) The ablation simulated through the positive-degree-day (PDD) method is compared to the ablation as simulated by the surface energy balance approach (SEB) in the coupled climate and ice sheet model set-up. Another ablation scheme that is currently quite popular is the insolation-temperature-melt (ITM) approach (see for example, Robinson et al., 2011; Robinson and Goelzer, 2014). I understand that additionally assessing this melt scheme would be a lot of extra work, but this alternative approach should as least be mentioned and referred to in the discussion. One of the likely reasons why the PDD method cannot perfectly capture the SEB simulated ablation evolution over long time scales (such as a glacial cycle) could be because it does not account for insolation changes. The ITM method does include the effect of varying insolation on melt (although it might have other drawbacks). Please discuss.
2) I also miss a section in the introduction explaining the time period you focus on. Explain why the last glacial cycle, and give some background information. Introduce terms like inception, termination, LGM, and Holocene. And give dates for your "target windows", and call it "target periods" or similar.
3) Also lacking is a discussion of your reference simulation with respect to geological reconstructions of the ice sheets over the last glacial cycle, and other modelling approaches.
4) The set-up of the "offline" and "online" PDD ablation methods need to be explained in more detail (e.g. page 4, line 25)
5) Discussion resolutions: what is the effect of the rather large gridboxes used in CLIMBER and SICOPOLIS?
6) Also, the PDD method is originally developed for daily temperature input, as are the literature values for PDD factors and the standard deviation for temperature. You use 3-day mean temperatures. Please discuss.
7) What are the initial conditions for the (reference) simulation(s)? Same as pre-industrial? Does that mean only ice on Greenland (how much?), or where else? A map of the initial ice distribution for the reference simulation would be helpful.
8) Basing the selection of the PDD factors for the online simulations on the best PDD factors for the offline simulations is not very convincing. I though the whole point was to show that interactions/feedbacks between the climate and ice sheets are important. Why not test a range of factors,

and select the best through some statistical evaluation, such as the rms-error approach used for the offline simulations?

9) Some scientists do not have access to a climate model or not the computational resources to run it over long time scales, and therefore do not have access to SEB-derived ablation. Could you give a recommendation on how to best apply the PDD method. I.e. emphasize testing different PDD values, use a short time period, select one ice sheet, …?

**SPECIFIC AND TECHNICAL COMMENTS**

Some sentences/sections are not very clear. I made several suggestions (see below), but would highly recommend a thorough English language check.

Abstract, lines 10-18: not clear, please rewrite. Make clear that you tested a range of literature values, and that it was not possible to find one set of PDD values that result in a good fit of both the American and the European ice sheets to your reference simulation. Neither can fixed values satisfactorily explain the ablation evolution over the entire glacial cycle for the individual ice sheets.

Abstract, line 18-19: change to: According to our simulations, the SEB approach is superior to the PDD methods when simulation Northern Hemisphere glacial cycles. This is partly due to the SEB approach including effects of change snow albedo, which is particularly important for the American ice sheet margins.

Page 1, line 20: change “gains and losses” to “fluctuations”

Page 2, line 1: Is it correct to say that the surface mass balance is the **main** factor affecting the evolution of ice sheets? What about calving, and basal processes? Needs a reference.

Page 2, line 6: rewrite “very close to each other”.

Page 2, line 23: change to “demonstrated that feedbacks between climate …”

Page 2, lines 24-31: Here add some more information on the ITM method

Page 2, line 32: change “problem” to “disadvantage”

Page 3, line 10: change to “Charbit et al. (2013) who discuss the effect of different PDD parameterizations on Northern Hemisphere ice evolution, we”

Page 3, line 15, change to “ice volume evolution”

Page 3, line 19-20, change to “evaluate the resulting glacial cycle simulations against sea level reconstructions.”

Page 3, line 29: please explain what you mean with “balance year”

Page 5, line 18: delete “supposedly”

Page 6, line 12: change to “on North America and in Eurasia extending up to 120E. Note that the Greenland ice sheet is not included in the selections, but is part of the NH total.”, or similar.

Page 6, lines 19-28: Please rewrite, could be shortened as well.

Page 7, line 3: The surface mass balance is also positive during periods of ice volume reductions (e.g. ~110ka, 90ka), indicating that a positive surface mass balance does not automatically lead to the build-up of ice. Please explain.

Page 7, line 6: change to “the Atlantic meridional overturning”

Page 7, line 11: change “control” to “tuneable”

Page 7, lines 28-30. Confusing to read, please rephrase.

Page 8, section 3.2: Why is the entire ensemble discussed for the rms-error, and only a selection for the anomaly/offset m? Figure 4 could be replaced by a figure similar to Figure 5, but than for the anomaly m. The information of the original Figure 4 can also be seen in Figure 6, especially if you add a (blue) line for the PDD-derived ablation evolution of the simulation that fit best to the reference simulation, over the entire 130ka.

Page 8, lines 18-19: Change to “Figure 6 shows the PDD-derived ablation evolution for the American and European ice sheets for the entire ensemble.”

Page 8, lines 22-23: describe the “shorter” time intervals using “inception” and “deglaciation”

Page 8, lines 27-32: bit redundant, it was already clear from Figure 6 an Table 2 that different PDD factors are needed for different ice sheets. Maybe shorten?

Page 9, lines 1-9: make more clear that here the spatial patterns are investigated, not anymore the time evolution.

Page 9, lines 14-15: change to “.. the ice sheets are coupled through the PDD methods. In doing so, processes ignored by the PDD method, such as the impact from changing snow…”

Page 10: change “The simulation X” to “Simulation X”

Page 11, lines 8-16: Make clear that this discusses the offline simulations. Is it possible that the American ice sheet is less well simulated in the offline PDD method because the PDD scheme does not account for dust deposition?

Page 11, lines 17-19. Unclear, please rewrite. (blurred?)

Page 11, discussion of Figure 12 is also not clear, please rewrite.

Conclusions, lines 10-13: too technical. This means that different sets of PDD constants should be used depending on (1) the ice sheet, (2) the time period interested in. Right?

Figure 1: Some indication of the orbital forcing should be included. Maybe summer 65N insolation or the Milankovitch parameters precession/tilt?

Figure 9: change “topography” to “coastlines”

Figure 11: change “15ka” to “21ka” and “topography” to “coastlines”

**REFERENCES**

Robinson, A., Calov, R., and Ganopolski, A.: Greenland ice sheet model parameters constrained using simulations of the Eemian Interglacial, Clim. Past., 7, 381-396, doi:10.5194/cp-7-381-2011, 2011.

Robinson, A. and Goelzer, H.: The importance of insolation changes for paleo ice sheet modeling, The Cryosphere, 8, 1419-1428, doi:10.5194/tc-8-1419-2014, 2014.