

Interactive comment on “Simulation of the last glacial cycle with a coupled climate ice-sheet model of intermediate complexity” by A. Ganopolski et al.

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First, we want to express our thanks to the reviewer for the constructive and useful comments. Below, we give response to the general comments and the changes we will apply to the manuscript to improve the paper. We will also correct or modify the text of the manuscript and the figures according to the specific comments.

1a. “... The formulation in Appendix A would benefit from more discussion, addressing:
a. In Eq. (A1), this dust source is proportional to M , the annual surface ice melt. Does this mean that the source is literally englacial debris that melts out on the ice surface? Or more reasonably, is M a proxy for basal outwash, with the main dust

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source being basal debris and sediment flushed out by merging basal streams? As it is, the formulation seems circular, with dust both being deposited onto the ice-sheet surface by Q/τ in (A2), and simultaneously being released (from the ice surface?) proportional to M in (A1)”

Response: We agree that the description of the glaciogenic dust module requires further clarifications and this will be done in the revised manuscript. Concerning the reviewer’s question of why the surface melt (M) enters the equation for dust sources, we did not assume that the glaciogenic dust originate from the melted surficial ice. Of course, the glaciogenic dust originates from the sediment transport beneath the ice sheet. The term M in eq. (A1) was aimed to distinguish between the advancing phase of the ice sheet (small M), when sediment flow through the margin of the ice sheet is small, and the retreating phase (large M), when a large amount of unconsolidated sediment becomes exposed to wind erosion. However, we realized that this parameterization looks somewhat artificial and can cause confusion. Therefore, we used the time since the submission of the manuscript to rework this part of the model. Now, we have introduced more physically based parameterizations, where the sediment flux through the ice-sheet margin and its accumulation and decay are taken into account explicitly. This new approach does not include anymore surface melt M and will be described in the revised version of the manuscript.

1b. “The formulation in Appendix A does not include deposition and long-term accumulation of dust on ice-free land (Q/τ term in (A2) for such points). Significant amounts could accumulate, which could then be redistributed by winds onto the ice. This is presumably the equivalent of loess, and if added to the model, could be validated against today’s observed glaciogenic loess distributions”

Response: Indeed, deposited dust can be entrained and transported by wind again before the sediment will consolidate and vegetation cover will appear. But this is a too complex process to be treated even in state-of-the-art dust cycles models. However, we indirectly account for this process by prescribing a millennial-scale residence time

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for the sediment exposed after the retreat of the ice sheets. As far as the observed loess distribution is concerned, they are actually taken into account to constrain our dust model. Loess thickness, especially in America, is very heterogeneous and ranging from centimeters to tenths of meters, which also reflects the processes of water erosion and local geographical factors. However, a typical thickness of the losses deposited during MIS 2 is about one meter, which corresponds to the dust deposition rate of the order of magnitude of 100 g/m²yr. This is close to the value of dust deposition in the vicinity of the ice sheets simulated with our model.

1 c. "Alongside Fig. 8, it would be interesting to show the relative amounts of glaciogenic vs. non-glaciogenic dust sources, in order to give an idea of the relative importance of the two"

Response: This is a useful suggestion and the figure will be changed accordingly.

2." On pg. 2281, it is suggested that PDD vs. SEMI is the reason that previous models with PDD produced only small precessional ice-volume variations. But this could instead be due to many other competing or canceling factors (as discussed in pt. 1 above). It would be more convincing to try PDD in the current model, combined with other compensating allowable parameter variations to see what ice-age results can and cannot be achieved"

Response: The reviewers, probably, would not be surprised to learn that we are not able to simulate realistic glacial cycle, when using the standard PDD scheme. This does not imply that this is not possible – some workers apparently manage to do that, and we have no intention in this paper to argue that one should not use the PDD approach for modeling glacial cycles. However, we believe that our physically based energy-balance scheme has obvious advantages over the semi-empirical PDD approach, in particular, that it allows us to account explicitly for the impact of dust on the ice-sheet mass balance. The latter, as it was shown in the paper, is important for the simulated glacial cycle. Therefore, we see no sense in trying to re-tune our model

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to get the right glacial cycle with the PDD scheme. Instead, we will show in the revised manuscript a comparison of diagnosed surface mass balance over the ice sheets using two different approaches.

3. "In the discussion on pg. 2290, it is perhaps misleading to deal with total ablation (which increases with ice sheet area) in relation to phasing with orbital variations. The local melting rate on the ice-sheet southern flanks (m/yr) probably has little phase shift from summer insolation"

Response: Of course, local ablation is strongly correlated with summer insolation. The point we want to make here (and we will make it clearer in the revised version of the manuscript) is that total ablation depends on both: the forcing and the size of the ice sheets. Therefore, total ablation can lead the orbital forcing appreciably. This, in turn, means that ice-sheet volume should not necessarily lag behind the astronomical forcing – this assumption is often used, e.g., for orbital tuning of paleoclimate records and by the opponents of the Milankovitch theory. In fact, the ice sheet can melt completely even before the maximum of summer insolation is reached, as it happened during Termination II. In general, one should not expect a fixed phase relationship between forcing and ice volume – even the sign of the phase shift can change over the glacial cycle.

4. "As described here and in earlier Calov et al. papers, the model produces interesting internal fluctuations in the ice sheet and AMOC related to D-O and Heinrich Events. But beyond that, it is unclear here whether they are more closely related to HE's or D-O's or both. For instance, does the model produce the observed sequences of increasing D-O's culminating in an HE? Are all the modeled fluctuations associated with a surge in the Northern Laurentide ice sheet? Some more discussion would help. Incidentally, the paper emphasizes the stochastic nature of the fluctuations, but some recent papers suggest the deterministic timing (relative to SH) may be important for terminations (Barker et al., Nature,2009; Wolff et al., Nature Geosci.,2009; Cheng et al.,Science,2009).

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Response: In our simulations, both, Heinrich events and DO events have stochastic nature, since no forcing with corresponding periodicities are prescribed. The HEs have the same origin as described in Calov et al. (2002) and are associated with sporadic surges from the Laurentide ice sheet over the area of Hudson Bay and Straight. These massive (order of magnitude of 0.1 Sv) freshwater pulses considerably weaken the Atlantic meridional overturning circulation and bring the Northern Hemisphere climate into “stadial” conditions. However, there are many more simulated DO events (i.e. abrupt warming events over the North Atlantic realm) than HEs. These events are caused by short-term random fluctuations in the freshwater flux into the North Atlantic Ocean which, as it was demonstrated in Ganopolski and Rahmstorf (2002), under glacial climate conditions can cause abrupt transitions between different modes of operation of the Atlantic thermohaline circulation. Whether or not, there is a fundamental synchronization between Heinrich and DO events is beyond the scope of our paper, which is devoted primarily to the orbital scale climate variability. The same is true for the leading role of the SH in the termination of glacial cycles proposed in the papers mentioned by the reviewer. We believe that our modeling results support the Northern origin of initiation and termination of the glacial cycles and that they are in line with the classical Milankovitch theory. Of course, other workers are free to explore alternative hypothesis.

5. “Several figures could show more useful information. . .”

Response: We agree and will modified the figures in accordance to the reviewer’s suggestions

Interactive comment on Clim. Past Discuss., 5, 2269, 2009.