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Climate of the Past Discussions

Interactive comment on "The role of orbital forcing, carbon dioxide and regolith in 100 kyr glacial cycles" *by* A. Ganopolski and R. Calov

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First, we wish to express our gratitude to the reviewer for the constructive and useful comments. We respect the reviewer's skepticism and below we discuss his/her major concerns. We will also modify the text of the manuscript according to the specific comments.

General response

The reviewer's comments are mostly related to the Baseline Experiment, which we consider a secondary issue for this paper, because this is precisely the same experiment as was described in our previous paper (Ganopolski, et al., 2010, Clim Past; hereafter G10) only extended back in time. It is important to realize that the Baseline Experiment

is strongly influenced by the explicit information about real glacial cycles through the GHG forcing, which contains both strong 100-kyr cycle and the right timing of glacial terminations. Therefore, the reasonable agreement between the Baseline Experiment and the data (at least in terms of global ice volume) is not too surprising and has been achieved in a number of previous simulations of glacial cycles. Moreover, if the model was able to simulate reasonably well the last glacial cycle, as it is shown in G10, why should it fail for previous several cycles? The planet was the same, the physics was the same and the forcings were essentially the same. It is also natural that the agreement between model and paleodata degrades when we go back in time, since earlier glacial cycles differ in some important aspects from the most recent ones. This is likely to be due to gradual evolution of the boundary conditions caused by glacial erosion, sediment transport, etc., which are not taken into account in our experiments, since we kept geography and sediment distribution constant during the whole model run.

The key question of the reviewer is whether "very the very good agreement with data comes from good physics or good parameter tuning". The answer is that the good agreement is achieved from both accurate model tuning and using an adequate physics. As it is discussed in G10, the Baseline Experiment was selected from a large ensemble of simulations. We also show in G10 that simulated glacial cycles are sensitive to the choice of model parameters, which is consistent with previous studies. On the other hand, one should not overestimate the power of tuning, since the ability of a model to simulate adequately the system dynamics ultimately depends on the model formulation, the set of described processes and model parameterizations. Although CLIMBER-2 belongs to the class of models of intermediate complexity, it is still a rather complex one and, unlike conceptual models, it is not based on a single assumption and does not have one parameter which allows to yield good agreement with data. As any real model, CLIMBER-2 is based upon many assumptions and incorporates a larges number of parameters and parameterizations, many of them are crucial. Figure 11 from G10 clearly illustrates this fact. It shows that the use of different sliding parameterizations, parameterization of the orographic effect on precipitation and the effect of dust deposition are of comparable importance for the simulated glacial cycle. Obviously, different combinations of model parameters may lead to similar results and we do not claim that our choice is the perfect and final one. At the same time, we believe that our modeling approach has important advances and advantages compare to most of previous modeling attempts. In particular, we use a physically based surface mass balance scheme (Calov et al. 2005, Clim Dyn), whilst most of other modeling works relay on semi-empirical approaches, such as the positive degree day method. Recent paper by van de Berg et al. (2011, Nature Geoscience) clearly demonstrates the necessity of a physically based approach to simulate correctly the response of the ice sheets to the orbital forcing. The use of a physically based surface mass balance scheme also enables us to account explicitly for the effect of the dust deposition on surface albedo (G10), the importance of which for the mass balance of continental ice sheets was independently confirmed by Kriner et al. (2006, Clim Dyn). We also use a rather advanced downscaling technique, in particular, unlike most of previous studies, we explicitly account for wind direction in the parameterization of orographic effect on precipitation, which considerably enhances the quality of simulated precipitation field. The model also explicitly accounts for the vegetation and oceanic feedbacks. These and other advantages of our approach are described in details in our previous papers. Obviously, such a complex (although of intermediate complexity) model contains many important parameters and some of them are not well-constrained by observational data or theoretical analysis. In particular, the lack of modern analogues for the Laurentide and the Fennoscandian ice sheets makes it difficult to constrain parameters in the model for the glaciogenic dust deposition, since the only available data are LGM deposition rates at several locations south of the Laurentide and Fennoscandian ice sheets. Obviously, our model has limitations. In particular, its atmospheric component has a very coarse spatial resolution and does not account for the effect of stationary orographic waves. But in spite of these limitations and uncertainties, we believe that our approach is arguable the most advanced among those which were previously used to simulate glacial cycles.

Specific comments

1. The reviewer is absolutely right that the parameterizations of the effect of dust deposition on surface albedo, basal sliding and surface mass balance are among the most important, and modeling results depend strongly on the choice of these parameterizations and corresponding model parameters. This was already discussed in G10, but we agree with the reviewer that the paper will benefit from a more thorough discussion of the mechanisms leading to resulting glacial cycles. This will be done in the revised manuscript.

2. Indeed, paleoreconstructions suggest that the maximum area of the Eurasian ice sheets (especially in Eastern Europe and Siberia) during penultimate glaciation was significantly larger than at LGM. Our model does not simulate a much larger Fennoscandian (and smaller Laurentide) ice sheet during penultimate glaciation compared to the last glacial maximum. This can be explained by the fact that the history and the magnitude of the orbital and GHG forcings prior to both glacial maxima were rather similar and, as a result, simulated temperature changes over Siberia for the end of MIS6 and MIS2 are rather similar. If, indeed, penultimate Eurasian ice sheet was much larger, than this implies that our model underestimates the sensitivity of the Fennoscandian ice area to the orbital forcings. In particular, it is possible that the background dust deposition rate taken from GCM simulations for the LGM (Fig 9b in G10) is too high over this area and restrict eastward advance of the Fennoscandian ice sheet thus making its area less sensitive to climate forcing than it was in reality. (This issue was also discussed in Colleoni et al., 2009, Glob Planet Change). In the future, we are planning to avoid this problem by using a fully interactive dust model instead of prescribed GCM output. We will follow the reviewer's recommendation and will show separate graphs for the temporal evolution of both major ice sheets in the Baseline Experiment.

3. The precise timing of glacial terminations is not the issue of our paper, because timing of terminations beyond the last one is not known accurately enough. It is true that many workers tried to derive the mechanisms of glacial terminations from the

analysis of timing of terminations and came to completely different conclusions: from full support of the Milankovitch theory to the statement that the Milankovitch theory is wrong. We approach this problem from the opposite direction. We show that in our model the timing of terminations is primarily controlled by eccentricity and precession, whilst obliquity plays a secondary role. We also do not believe that lead and the lag analysis is very helpful in understanding of the behavior of such strongly nonlinear system as the climate-cryosphere system is. We presented our arguments on this issue in Ganopolski and Roche (2009, Quat Sci Rev).

4. A number of previous studies did get strong 100 kyr cyclicity even with constant CO2 and Pollard, probably, obtained the most realistic one. It is correct that in his case two major mechanisms responsible for glacial terminations are the multimillenial bedrock relaxation time scale and the parameterization of ice calving at the southern flank of the ice sheets. The first process is accounted for in all ice sheet models. The second also operates in our model during termination. This happens when during the ice sheet retreat depressed bedrock is filled with water and calving can occur along some part of the southern margin of the ice sheets. Another nonlinear mechanism involved in the termination is the fast sliding of the ice sheets over sediment-covered areas. However in our model these mechanisms alone (without dust) are not sufficient to achieve a complete deglaciation. These issues were discussed and illustrated in G10, but we will restate the most important points in the revised manuscript. We do not understand why the Reviewer thinks that “this contradicts the regolith discussion”. The presence of the thick regolith in the northern North America increases the ice sheet sliding and enhances glaciogenic dust production, which leads to the terminations of glacial cycles each time when summer insolation rises significantly, i.e., when precession and obliquity variations are in the right phase, which happen approximately every 40 kyr. This is, by the way, already demonstrated in Fig 11a (red line) in G10, even though in this case the 100 kyr cycle was present in the GHG forcing.

5. We did not claim here that in none of the previous studies glacial cycle were some-

how related with the eccentricity through the mechanism of phase locking. We just say that the fact that terminations occur every 2 or 3 obliquity cycle or every 4 or 5 precessional cycles alone does not explain why terminations should always occur at a certain phase of eccentricity. However, we will modify this sentence to avoid misunderstanding. We will also include a more comprehensive comparison of our finding with previous works.

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