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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

Anonymous Referee #1

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General Comments

Ganopolski and Calov very concisely present a set of EMIC experiments that are simply designed to identify the relative influence on recent 100-ky glacial cycles of orbital forcing (separating eccentricity and obliquity), equivalent CO2 forcing, and the regolith. CLIMBER-2 remains the best available model for such long simulations. Ganopolski and colleagues have continued to develop and use this model to address some of the leading questions in climate and Earth System science. In this context, the present manuscript is a natural continuation of earlier work, in particular Ganopolski et al. (2010), henceforth referred to as G10. A key advance here, over earlier studies of the 100-ky cycles, is the use of "physically based and geographically explicit climate-cryosphere models", for the 800-ky simulations.

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Taken as a set, the CLIMBER-2 experiments support a conclusion that 100-ky cycles are a non-linear response of the climate-cryosphere system to the shortest eccentricity cycle, contingent on low glacial CO2 and sediment-free mid-latitude continents. This amounts to an impressive scientific achievement, although I am left with some questions regarding model-dependence of key results (see below). My recommendation is therefore that the paper is acceptable for publication in Climate of the Past subject to minor revisions.

Specific comments

1. pp. 2396, line 18 – While model-data agreement is impressive, the remark that the baseline experiment "agrees well with the empirical stack" is brief and qualitative. There are clearly some discrepancies in Fig. 1d (e.g., ice volume remains larger than stack data at terminations 5 and 7, while simulated ice volume at termination 6 is substantially reduced compared to the stack data). Can the authors say a little more about the times of less agreement and provide possible reasons. Could the stack data be less reliable at these times? Perhaps the assumption that the Southern Hemisphere contributes a constant 10% to global ice volume variations – based on Huybrechts (2002), for the last glacial cycle – breaks down in the deeper past?

2. Fig. 1 and accompanying text – Accepting the impressive baseline experiment (BE), can the authors provide relevant information on how this was achieved in the first place? My assumption is that BE uses the key ice-sheet and coupling parameters selected via the "suboptimal" subjective tuning outlined in G10? Please verify this. Furthermore, G10 find "high sensitivity of simulated glacial cycle to the choice of some modelling parameters" (Fig. 11 in G10). Such parameter sensitivity should be noted here.

3. The focus of the paper is understandably on the cryosphere, but CLIMBER-2 simulates changes in atmospheric winds and the ocean circulation (the latter evident as AMOC changes in Fig. 8d of G10). These aspects of the simulations are not considered in the manuscript. To what extent might simulated changes in associated heat

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and moisture transport influence inception and termination of the 100-ky cycles?

4. Judging from Table 1, the authors undertook a total of 55 experiments, which amounts to a formidable amount of simulation, far beyond the capability of more complex, "IPCC-class", earth system models and a clear justification for using CLIMBER-2. How much computational effort was involved? Was it necessary to resort to acceleration, as introduced in G10? If so, to what extent does acceleration compromise the present results?

Technical corrections

1. p. 2393, line 8 – correct spelling is "Ridgwell"

2. p. 2397, line 9 – for "constant CO2 concentration ranging from 180 to 300 ppm (for every 20 ppm)" suggests seven experiments, but the sentence states "We performed ten experiments . . ." – please clarify

3. p. 2399, line 7 – add a reference to the relevant figure, ". . . during the most recent termination (Fig. 3b)."

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