

Interactive comment on “Climatic conditions for modelling the Northern Hemisphere ice sheets throughout the ice age cycle” by A. Abe-Ouchi et al.

L. Tarasoff (Referee)

lev@physics.mun.ca

Received and published: 21 March 2007

The paper provides some interesting analyses concerning dynamical controls on temperature over the northern ice sheets during the last glacial cycle along with the partitioned impact of these temperature components on ice evolution. The attainment of a crudely reasonable glacial cycle with the parameterized climate forcing extracted from the GCM based analyses is also noteworthy. Also, as cited, there have only been a few studies combining GCMs (especially of such a relatively high resolution) with ice-sheet models.

Given these three points, along with the abundance of results, the paper offers a worth-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

while contribution. However, as detailed below, the presentation of the results suffers from a lack of numerical comparisons and insufficient motivation of chosen parameters. It is for instance unclear from the analyses, why the temperature lapse rate is proposed to be about 5 K/km, a value highlighted in both the abstract and conclusions. Given the data from the GCM runs, a more detailed analysis/regression of the temperature lapse rate would significantly improve the value of the paper.

As such, I recommend acceptance once the following issues are addressed.

major comments

One major flaw is the lack of any explanation for the choice of a constant surface temperature lapse rate of 5 K/km. It would have made more sense to extract a seasonal lapse rate field from the GCM output. Also, given what I assume are the linear regression values for the effective lapse rate from the GCM in fig.2, why were these values not used? To significantly increase the utility of this paper, I would suggest that the authors linearly regress seasonal lapse rates for the model as a function of elevation and any other critical dependencies (continentality, latitude,...).

The choice of ICE-4G as the ice boundary condition for the GCM modelling is unfortunate given it has now been superseded for a couple of years by the ICE-5G chronology (Peltier, 2004). The two key differences are the presence of a large Keewatin ice dome and reduced ice extent over Asia in ICE-5G.

Finally, I would like to see some numerical values (summer and winter mean and standard deviation) quantifying the relative effects of CO₂, albedo, elevation,... Referring to tight-packed figures with just "there is a larger response for orbital effect than the CO₂" makes it difficult to get a clear picture of the relative strengths of the dynamical components.

specific/technical comments

equation variables: you need to define all your equation variables as presented (eg H

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

and h for equation #1). Not everyone will be familiar with the common definitions used by ice-sheet modellers.

references: more contact needs to be made with the past GCM analyses of the influence of ice sheets on climate. Eg. comparison of results against the sensitivity studies of not just the cited Broccoli and Manabe paper, but also Rind (JGR, 1987) and Hansen et al (1984, as cited by Rind)

#pg 302, line 26 I would suggest adding Tarasov and Peltier (1997a) to the citations given it's historical precedence (at least relative to the cited references) on the topic of the dynamical controls of the 100kyr cycle

#pg 305, ln 25 "is tested on" -> "has been tested for"

#pg 306, ln 9 Provide a better rationale for the value of the A_s parameter than "corresponds to the maximum value in the sensitivity studies presented in...". Eg. consider past inferences, present observations,...

#pg 306, eq #2 as written implies a peculiar choice of numerical grid for the ground temperature field. Is the vertical basal grid not defined relative to the basal surface? Ie, is z depth relative to contemporaneous surface elevation (usual approach) or is it depth relative to some time-independent datum such as the present day surface elevation (as written)?

#pg 307, ln 2, why is Hudson bay "treated as land toward which the ice sheet is allowed to advance"?

#Methods section: How is ice calving handled?

pg 308, in the future, I would suggest usage of an ice grid resolution no coarser than 1 degree longitude by 0.5 degree latitude (the factor 2 difference gives better equivalence between delta longitude and delta latitude distances.)

#pg 313, paragraph 1, it should be mentioned that the PMIP2 intercomparison used the

ICE-5G model for ice boundary conditions which significantly includes a large Keewatin ice dome, not present in ICE-4G. Consideration of this and other differences (Eurasian ice extent), should be given consideration in the analysis.

pg 316 eq. 10 is the reference value for γ_{area} derived from the GCM results? If not, where does the number come from?

pg 318 line 8 "the geographical pattern is quite well simulated". I would disagree here for Eurasia (which the authors admit as well on pg 319). The model results have way too extensive ice over Northern Siberia and the adjacent ocean at 15kyr as compared to current interpretations for even last glacial maximum (eg. cited Svendsen et al, 2004). This is a common problem if you check the literature.

#General fig.s: interpretation/comprehension of multiple area maps would be greatly facilitated if short descriptive subtitles were included in the plots (ie not just a, b, c, d), as is done in figs. 4-7

#Fig. 2 state in the figure caption that linear regression values for the plotted temperature change data are also shown in the figure. (I'm assuming that's what is shown).

#Fig. 3 it's a pain to flip back between plots to figure out what's shown. Instead try something like repeat of Albedo effect isolation (Fig. 1c), but for.... (Ditto for Fig. 8)

#Fig. 7 since this is on-line, use colour for all area maps to increase legibility. Also, there are some strange sequences of solid ovals (eg 2 rows in the Northwest for (d)). If these are a graphical artifact, remove them. If not, explain what they are.

#Fig. 8, given the large number of lines in the plots, it would help to have the legend ordered vertically to match the associated ordering of the sea level drops. You also need mention what I suspect is the orbital forcing curve on the top of the plots in the caption. Finally, a graphical comparison needs to be made against sealevel reconstruction (SPECMAP or Waelbroeck et al, QSR 2002) to elucidate the general validity of the ice volume chronologies obtained.

Interactive comment on Clim. Past Discuss., 3, 301, 2007.

CPD

3, S75–S79, 2007

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper