Clim. Past Discuss., 5, C258–C260, 2009
www.clim-past-discuss.net/5/C258/2009/
© Author(s) 2009. This work is distributed under
the Creative Commons Attribute 3.0 License.

Climate of the Past Discussions

Interactive comment on "Mechanisms and time scales of glacial inception simulated with an Earth system model of intermediate complexity" *by* R. Calov et al.

R. Calov

calov@pik-potsdam.de

Received and published: 25 May 2009

Reply to Referee #2

1. A better discussion about the connection of transient and equilibrium results is demanded. First of all, we would like to clarify that page 612, line 4 is not the only mention on the connection between transient and equilibrium results in the paper. We agree that the treatment of this is somewhat sparse in the discussion section itself. But in the body of the paper, there is plenty of material on this issue. The entire section 4 links equilibrium results (in terms of thresholds) with transient response and our considerations on the undershoot of threshold needed to simulate ice initiation in transient S_IDon model

configuration (page 608, lines 1 to 24) addresses this topic. Further, does point 2 in the conclusion (page 613, lines 25 to 27 and page 614, lines 1 to 8) summarises material on interrelation between equilibrium and transient simulations. What happens if MF > S_IDoff becomes clear in Fig.2, Fig. 3 and Fig. 4 and the corresponding text in the manuscript. Once more, in transient simulation MF has to drop about 20 W m-2 below the glacial threshold (which is determined via equilibrium simulations) to initiate glacial inception. This is because of the inertia of the system caused by slow spreading of ice in mountainous regions in the high north of the Northern Hemisphere. About the "kinks" (strong increase) in Fig. 5b, there is a mention on page 607, third paragraph. We agree that a discussion on this strong increase might be useful, because a misunderstanding could appear here. The sharp increases can be seen, less pronounced, in ice volume too (see Fig. 4). These "kinks" do not correspond to a threshold in our definition. As emphasised in the paper, we determine a threshold with equilibrium simulations, i.e., a threshold is strictly tied to equilibrium. This is due to the fact that a unique determination of a threshold is impossible via transient simulations, because such a "threshold" will depend on the rate of change of the forcing. The reason why we see such sharp bends in Fig. 5b is the strong positive snow albedo feedback. In other dynamical systems with weak positive feedback, nothing distinct might be seen in transient respond, although there could be a threshold detectable via equilibrium states. At this point, we would be reluctant to phrase "something like crossing of S_IDoff". Just for clarification, our S_IDoff does not correspond to the kinks (strong increases) in Fig. 5b. We extended the discussion section including some more discussion on that therein.

2. Page 602, line 15: The referee asks us to give more details on the method which yields the stability diagram. In particular, (i) how MF changes with the precessional angle and (ii) on seasonal and latitudinal change of insolation. About the method of our stability analysis, there are already three paragraphs in the paper (page 601, line 28; page 602, lines 1 to 29; page 603, lines 1 to 14). (i) The dependence of MF on the precessional angle was already (nearly) explained (page 602, lines 10 to 11): "..., which corresponds to a change from warm to cold Northern Hemisphere boreal

summer conditions." We clarified this further by introducing a change as follows: "Such a variation in the precessional angle corresponds to a lowering in MF, which causes a change from warm to cold Northern Hemisphere boreal summer conditions." (ii) On the seasonal and latitudinal variation of orbital forcing there is already a mention on page 603, line 21 to 24 ("Although our bifurcation analysis is worked out in the MF phase space, our model computes the full orbital forcing in season and in latitude. . . .").

3. Page 616, lines 1 to 20: We included the solution with gamma function in the paper now. Of course, the sentence of Chebyshev is valid for indefinite integrals. But the integral in Eq. (A4) is a definite integral. This was overlooked by the first author of the paper. The corresponding text in manuscript is corrected too. We are grateful for having obtained this suggestion.

Interactive comment on Clim. Past Discuss., 5, 595, 2009.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper