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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.

Anonymous Referee #1

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In this paper, the CLIMBER-SICOLPOLIS model is used to investigate the problem of glacial inception. There are essentially 3 main parts to the paper:

(1) A stability analysis of the model response to boreal summer insolation at 65oN, with and without ice sheet dynamics. (Fig 1.) (2) Analysis of transient response to two idealised orbital forcings, and the explanation of this in terms of 2 simple models (Fig 2,3,4) (3) Analysis of transient response to a realistic transient orbital forcing through MIS1, 5, and 11, and the effect of accelerating the forcing (Fig 5,6,7,8)

General Comments

The work done in this paper is very interesting and should be published. The only major

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problem I have with the paper is the treatment of the 'overdue glaciation hypothesis'. This is given quite prominent treatment in the abstract, but in the actual paper this section is very short compared to the rest of the analysis. More background needs to be given as to the debate itself – and Ruddiman's original work – at the moment little explanation is given of the hypothesis itself. Then, more detail needs to be given of exactly how this paper adds to the debate (the fact that glaciation is not simulated at MIS1), and more discussion on this topic (e.g. some citations need to be given for why the prescribed co2 of 220ppmv 'more than accounting' for CH4.). Also, I am at a conference at the moment and so can't check the Claussen (2005) paper in Climatic Change which also addresses this problem with the same model, but I wonder how much overlap there is, or how this work builds on the Claussen paper.

I also think it would be interesting to add simulations in which CO2 and CH4 vary realistically, as well as orbit.

Specific Comments

P601, line 25. Why is Greenland fixed in the ID-off simulation?

P603, line 19. Is it also possible that there is some small bistability near the region of bifurcation?

Fig 2 – What is interesting here is where the CLIMBER-SICOPOLIS curve deviates from the blue relaxation model and shows 'stepped' behaviour (around 70ka for 455W/m2) – any comment on this?

P605 line 19 – I am not sure that the equilibrium difference is 'small' – state actual value as a

fig 2 – can you put the ID-off results into the areal plots 2a/b. At the moment it is 'not shown' but it would be interesting to see.

P607, line 17 – How does the co2 forcing itself compare to 10W/m2? It would be useful to give this number for comparison.

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P608, line 26 – by ‘complex’ earth system models – give an example. CLIMBER is complex to a box modeller!

P611 line 4 – “which can be considered satisfactory” – I would delete this. What is satisfactory will depend on what is being studied. Similarly, remove colour shading in Fig 10 as these

Section 7: The assumption here is that CLIMBER is a good proxy for a GCM in term of its response to accelerated forcing – can this be backed up with examples?

P612, lines 25-29 – I don’t understand these two sentences!

Minor comments, typos etc.

Abstract – “one setup with snow mass balance and dynamical ice-sheet models and another with a snow mass balance model only”.

P603, line 29 “The glacial threshold (by which we mean the appearance of large areas with. . .”

P603, line 7. . . A priory should be a priori

P608, line 2 – label MIS 5e and 5d on the figure.

P610 line 4: “Therefore, this technique for a given acceleration factor will increase the interval of time between each exchange of information between the climate and ice-sheet components”

P610 line 15 – equilibrium interglacial stare – which interglacial?

P615 Eqn A1 – need to define x P615 Eqn A2 – need to define k. P615 ‘Figure 9’ would be better named ‘figure A1’?

Fig 1 – make the dashed line clearer dash – at the moment it looks just like the solid line.

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