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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Simulations of the last interglacial and the subsequent glacial inception with the Planet Simulator" *by* M. Donat and F. Kaspar

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General comments

The authors use a reduced-form GCM to explore climatic changes at the end of the last interglacial. The model comprises atmosphere, slab ocean including sea ice, and vegetation. Effects of these different components are estimated in several equilibrium simulations. In addition, a transient simulation of the fully coupled model has been performed. Glacial inception is investigated in terms of changes in permanently snow-covered areas. The paper is clearly structured and written. While this type of simulations are certainly valuable and there are interesting results, I find the analyses and discussion presented in the paper lack a certain carefulness. This is why I suggest a major revision before publication.

Major comments

In general, you ran quite a large number of simulations but discuss most of them only very briefly.

1. Chapter 3, line 12f: "However, when all subsystems are active the reaction is much stronger than the sum of the single effects. This illustrates ...". In REF, the cooling in AO/AV as compared to A and in particular the strong cooling in AOV as compared to AO/AV (as seen if fig. 1 for REF) above all illustrate that the ocean surface and vegetation you simulate in AOV/REF is quite different from what you prescribe in your (partly) uncoupled model runs REF. Which means that the land and ocean surface you prescribe here is quite far from being in equilibrium with climate (which results in the temperature bias of up to 3 degrees globally). To which state did you fix your ocean/vegetation in A, AO, AV? The point is that if now you analyse your simulations for 125 and 115 kyBP, it is a serious problem to say which part of the climatic effect of ocean and vegetation changes is "real" and which part is just due to the fact that the surface conditions in the (partly) uncoupled runs were chosen inappropriately (remember we work in a nonlinear system). This is even more a problem as the temperature bias mentioned above is much larger than any of the differences between 115 kyBP and 125 kyBP (or REF).

At least, a sound discussion of all this is needed.

2. In section 4.3, you describe the Kukla et al. hypothesis as "... the meridional moisture transport could have been increased. This could have led to the fast build-up of ice-sheets by increased snowfall." Later in the section, you write that "The fact that the total precipitation ... shows no significant differences ... already argues against Kukla's hypothesis". But total precipitation DOES increase, e.g. in July and August which are probably crucial months in terms of permanent snow cover!? Why does the snow fraction of total precipitation not change significantly in DJF (cf fig 4) despite sig-

nificantly lower temperatures at 115 kyBP, e.g. how is snow fall parameterized? Then you describe that there are "no significant differences for ... the moisture transport" but don't show any figure or give numbers. As a result, from what you wrote I am not fully convinced that you really disproved Kukla's hypothesis. In other words I still think the effect he describes could have some impact.

In general, a few numbers (e.g. regional temperature differences, areas of snow and sea-ice cover,) could help to make results of chapter 4 more concrete.

3. The discussion in chapter 6 should be more critical. How do the results compare to other models and data (to some extent, you mentioned this in different parts of the paper), some more references would be needed, for example when you mention ice-core reconstructions. How relevant could be the inclusion of ocean heat transports? To what extent could the timing of crossing the insolation threshold in the transient run depend on the model? You state that the Planet Simulator underestimates temperature changes as compared to other models or reconstructions and blame this to reduced complexity and resolution. To what extent could the lack of ice sheets matter here? Concerning Kukla: Your simulated 115-125 kyBP changes in snow volume are probably several thousand times smaller than reconstructed or simulated differences in ice volume. Could this additional amount of water you would need for the build-up of ice sheets really be supplied just by cooling (and quick enough, i.e. in a few thousand years) as you claim or would you probably still need changes in the moisture transport or other factors?

4. You use a vegetation model and explicitly investigate the impact of vegetation changes. But you do not say a word about neither the model nor how the land surface looks like and changes at the end of the last interglacial. This should be discussed.

5. Chapter 1: The description of what is the advantage of the Planet Simulator as compared to other models is written a bit too general: — Line 23: What is a "typical EMIC"? It seems to me that EMICs can be all different kinds of models, including

reduced-form GCMs. Also the resolution is not necessarily very different from the one you used here. — Line 25f: But many GCMs also couple different sub-systems (and even a 3D-ocean). What does "much more" efficient mean?

Further comments

Chapter 1: 1. Milankovich was not the first who discussed an astronomical theory for ice ages (see e.g. James Croll).

2. Why did you choose 125/115 kyBP instead of, for example, max/min summer insolation? The Eemian is a whole period. Glacial inception is a process.

3. You mention existing papers for the time period investigating effects in atmosphere and atmosphere-ocean models. What about existing results on vegetation effects (I ask for that as you also include a vegetation model in your simulations)?

4. Line 19f: Seems to me that by definition, to simulate glacial inception, an ice-sheet model is needed.

5. Line 23: Please introduce the abbreviation "EMIC".

Chapter 2: 6. What about changes in CH_4, NO_x, or other components?

7. Line 26f: Even slight (how much is it?) differences in e.g. CO_2 can have a significant effect if you are investigating threshold processes. So this is not an argument, I think.

Chapter 3: 8. Line 2f: I guess, the reason why the Planet Simulator reaches an equilibrium quite fast is simply the slab (instead of 3D) ocean. What about vegetation dynamics?

9. Line 15: "... slightly lower temperatures ..." - doesn't seem so for AO!?

10. Line 22: "0.7K" does not fit to fig. 5 (nor to the text in section 5).

Chapter 4: 11. Page 1352, line 20f: The explanation you give on page 1353, line 9ff for changes over land being bigger than over ocean is much better.

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12. Pages 1352/1353: So the bottom line of the temperature descriptions is that for the 125 and the 115 kyBP runs, the JJA signal in the higher northern latitudes persists into DJF (and dominates the annual signal) - because the absolute amount of insolation (changes) is larger?

13. Page 1353, line 24ff: Why is JJA temperature at 115 kyBP around Antarctica much higher? And why is DJF temperature at 115 kyBP higher in parts of the Arctic?

14. Page 1355: You focus on a discussion of longwave radiation but what about the other fluxes?

15. Page 1356, line 23 to page 1357, line 8: Most of this could be deleted as it just repeats what was already written before.

Chapter 5: 16. I guess your transient simulation is an AOV run? And it started from AOV/125ky?

17. Page 1358, line 7f: "This damped behavior ...". I don't understand what you mean here.

18. How do you calculate global snow volume?

19. Page 1358, line 16: Why is the temperature decrease time-lagged? Is it also the case for the northern hemisphere only?

Figures: 20. An additional insolation figure would help.

21. Fig. 1 shows annual values, I guess. What do the error bars show?

22. Fig. 3: "Annual minimum" means values for one certain day? Could be helpful (e.g. for comparison with fig. 2) to include a fig for REF (also in fig 4).

Interactive comment on Clim. Past Discuss., 2, 1347, 2006.