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Interactive Comment

Interactive comment on "Uncertainty of the CO_2 threshold for melting a hard Snowball Earth" *by* Y. Hu and J. Yang

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Reply to reviews on "Uncertainty of the CO2 threshold for melting a hard Snowball Earth"

We thank the reviewer's careful review and important comments, which are very helpful for improving our manuscript. Replies to the specific comments are as follows.

1. My concern about this manuscript is the methodology. The model setup is curious and potentially flawed. The authors state (p. 1340) that sea ice is prescribed by fixing surface temperature over ice below the model freezing point (-1.8 C). Because of this approach, low-level air temperatures asymptote to the freezing point (p. 1341, Fig. 1). Another issue is that sea ice surface temperatures can and (in the real world) do fall

below -1.8 C. The surface temperature of sea ice is essentially a balance between the surface heat budget and heat diffused between the sea-ice surface and the underlying ocean (which should have a temperature near the freezing point). By specifying a sea surface temperature of -1.8 C, it is very likely that an artificial heat source has been added to the sea ice surface. This could partly explain why CAM3 is warmer than FOAM at low CO_2 levels, and complicates any comparisons between the models.

It is our fault that we did not make the sea-ice prescription clear. In model setup, an ice-covered ocean is prescribed by "keeping ice-surface temperature below -1.8° C (the model melting point of water)", rather than "fixing it at -1.8° C". Below this model melting point, sea-ice surface temperature varies, depending on energy budget on the surface. The model setup actually acts as an artificial heat sink (cooling effect) as CO_2 levels are sufficiently high and sea-ice surface temperature is close to -1.8 ° C.

2. Both Pierrehumbert (2004) and Le Hir et al. (2007) spinup a snowball Earth using low pCO_2 levels (100 ppmv), and then use this snowball state as an initial condition for higher CO_2 experiments. This method alleviates the need to prescribe a fixed surface temperature and allows sea ice to melt if conditions permit. Why didn't Hu and Yang follow this approach? (A justification is warranted.) How much does it influence their comparison with the FOAM and LMDz? Additional experiments are almost certainly required to address this.

To our understating, Pierrehumbert (2004) used a coupled atmospheric-oceanic GCM, with which initial snowball-earth conditions were obtained by running the AOGCM for low CO_2 levels (below 100 ppmv). However, as pointed out by Pierrehumbert (2005), his AOGCM simulations are just like atmospheric GCM simulations with prescribed sea ice. In contrast, Le Hir et al. (2007) used an atmospheric GCM, in which an ice-covered ocean is prescribed, similar to ours (They wrote "To simulate a hard snowball Earth, we have prescribed an ocean covered in sea ice and a continental surface . . .". See their paper, page 278, section 2: Model and experiment design). Therefore, we think that the difference in model setup has little influence on simulation results. Moreover, the CO_2 threshold of 0.45 bars in Le Hir et al. (2007) is also a result of extrapolation because their simulations would also have the problem of the asymptotic behavior as CO_2 level is sufficiently high and ice-surface temperature is close to the model melting point.

3. I don't completely understand the focus on FOAM and the Pierrehumbert (2004) study. It would seem that the comparison with the LMDz model is just as important. In this sense, the paper seems unbalanced. An analysis and discussion of why CAM3 and LMDz deglacial CO_2 levels differ would be very welcome.

We agree with the comment that the paper is unbalanced in comparison with Pierrehumbert (2004, 2005) and Le Hir et al. (2007). One reason is that Pierrehumbert (2005) has detailed analysis in cloud physics and clear-sky greenhouse effect, and that the atmospheric component of his coupled model is an old version of the model we use, so that detailed comparison can be made. In contrast, Le Hir et al. (2007) have relatively brief analysis on these issues. In the revised version, we will make changes to have the paper more balanced, as suggested by the reviewer. In fact, our results are more consistent with Le Hir et al. (2007).

4. There are two comments in the Results section that could use additional explanation. On p. 1342, "... location of the maximum clear-sky greenhouse effects also shows different meridional shifts ..." On p. 1343, "... cloud layer is lifted to between 300 and 500 hPa." Presumably this is because tropospheric warming at high CO_2 reduces saturation at low levels. A statement to this effect should be added.

We agree with the comments. A few more sentences will be added to explain the results in the revised version.

5. In general the manuscript reads well. Additional editing is required in some places to fix grammatical and spelling errors, and clumsy language, for example, the first sentence of the Abstract. Also, p. 1341, "faster" should be "greater". On p. 1343, "averagely" should be "on average".

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We agree that we need to make careful corrections in the revised version, including the errors pointed out by the reviewer.

6. The title is more accurately “Model dependency of . . .” rather than “Uncertainty of . . .”. The study doesn’t quantify the uncertainty, or indicate whether any of these models are approaching the true deglacial CO2 level.

Yes, the title suggested by the reviewer is better than the current one. It will be changed in the revised version.

7. On p. 1339, it is not clear what “. . . as consistent conditions are considered” means.

We agree that this statement is not clear. Pierrehumbert (2005) reviewed the results of CO2 thresholds obtained with EBMs. He pointed out that these thresholds are obtained with very different conditions, such as no cloud effects, different surface albedo, different horizontal diffusivity for heat transport, and so on. He estimated that the CO2 threshold for these EBMs would be about 0.2 or 0.3 bars if same conditions are used. We will make this statement clearer in the revised version.

Interactive comment on Clim. Past Discuss., 6, 1337, 2010.