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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.

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Reply to Referee #1

Reply to General Comments

1. More background to the debate on Ruddiman's overdue Holocene glaciation hypothesis and the debate itself is demanded. And further, referee 1 asks us to include more details on how exactly our paper adds to the debate. In response, we would like to say that we already treated Ruddiman's hypothesis in the introduction (page 598, lines 22 to 29), the discussion (page 612, lines 8 to 21) and the conclusion (page 614, lines 9 to 14). Now, we further strengthened the Ruddiman aspects of our paper. But we think

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that our one sentence on Ruddiman's hypothesis in the abstract is not such prominent as suggested by the referee. Therefore, we adequately extended the introduction and discussion sections on Ruddiman's work. In particular, we welcome the referee's suggestion to substantiate our statement on the prescribed CO2 of 220 ppm which, in our words, "by far accounts for radiative CH4 effect". First, we weakened this statement by replacing "by far" with "safely". Second, we now give an estimate for the CH4 effect together with citations. Our estimated value of 227 ppm CO2 radiative equivalent, corresponds to maximum impact what Ruddiman's CO2 and CH4 can cause together. The 220 ppm CO2 value used in the simulations is even lower than our 227 ppm estimate. Additionally, we kept the CO2 constant in our simulation. Therefore, we really do not overstate the effect of our 220 ppm CO2 value if using the word "safely".

2. Concerning our simulations related to Ruddiman's hypothesis, more discussion and clarification of overlap with work by Claussen et al. (2005) is requested: Mainly, this was already done in our first submission. In the discussion section (page 612, lines 8 to 21), we pointed out that we address a critic by Ruddiman on Claussen's work concerning CH4. Now, we additionally elucidate how our manuscript relates to Claussen's paper in the introduction.

3. The suggestion to perform simulation in with realistic CO2 and CH4 variation is very useful. Indeed, we plan to submit a paper on this. Further on, we are convinced that the CO2 equivalent approach is the adequate one in the context of the given paper, because we inspected the maximal possible impact with our low value of applied CO2. At this point, we would like to emphasis that the main topic of the paper is rather on mechanism, time scales and threshold of glacial inception in general. The relation of our paper to Ruddiman's prominent hypothesis is just natural and, therefore, we included a discussion on it.

Reply to Specific Comments

1. Greenland is fixed in the ID-off simulations, because we cannot make Greenland's

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ice move without ice dynamics (ID-off). For clarification, we added the sentence "because in the latter case no ice movement is possible." into the revised manuscript.

2. Page 603, line 19. It is very unlikely that there is a further equilibrium with small ice volume. The additional equilibrium computations with fixed forcings showed that there is a sharp transition from interglacial to full glacial state in the stability diagram. Between MF of 475 and 476 W m-2 with the very low inland-ice volume, the continuous curve is still "too transient", because near the bifurcation our driving of MF is still too slow to fully capture equilibrium. This is stated in the paper. We add some further explanation hereafter. The low part of the volume curve in Fig.1 (between 475 and 476 W m-2) has the same characteristics as, e.g., the lower red curve in Fig.4, but on another scale. It is due to ice spreading. Technically, one can regard the MF axis in Fig. 1 as an artificial time axis (connected via the applied function which relates simulation time with MF). If one would stretched the low-volume curve between 475 and 476 W m-2 somewhat one would see similar curve as the low red curves in Fig. 4. The very low part and very short part of the ice-volume curve in the SIDon configuration in Fig. 1 is a (minor) artefact, because our MF in the method with slowly varying forcing changes still not slow enough. It is not a further equilibrium state.

3. We think, Fig.4 is meant here. The stepped behaviour originates most possibly from the coarse CLIMBER-2 resolution. At PIK, development of next generation EMIC CLIMBER-3 with finer resolution and improved physics is in progress.

4. We are reluctant to include the value of CO2 anomaly radiative forcing for comparison with MF, because they do not compare. While CO2 is a global forcing, boreal summer insolation acts only local on high northern Hemisphere latitudes.

5. P608, line 26: We added "(AGCMs, AOGCMs)". These abbreviations were explained before.

6. Page 611, line 4. Done

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7. Here, we reply about section 6 und the question whether the assumption whether CLIMBER-2 is a good proxy for a GCM in terms of response to accelerated forcing and whether this can be backed up with examples. Of course, we cannot prove that CLIMBER-2 is good proxy for any GCM, because GCMs differ considerably in their response to various forcing. However, we performed numerous comparison with different GCMs and found that the CLIMBER-2 model response to different forcings, such as CO2, solar, volcanic, changes in vegetation cover, etc. on different time scales is fully consistent with the majority of GCMs (Ganopolski et al., 2001; Bauer et al. 2003; Petoukhov et al. 2005; Plattner et al., 2008).

8. Page 612, line 25-29: The sentences are deleted now.

Reply to Minor Comments, Typos etc.

Nearly, all minor comments are implemented in the revised manuscript. But we left the sentence in the abstract as it was, because it does say, what we intended to say. Further, the permitted words in the abstract are limited and later on in the text more details on the setup are given. Because we removed the “e” and “d” of MIS 5 in the text, there is no need to label them in Fig. 5. Labelling the details of MIS 5 in the figure, would be even confusing, because the figure contains, from its design, MIS-1 and MIS-11 curves too.

Reference

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