Clim. Past Discuss.,
doi:10.5194/cp-2015-175-RC3, 2016

Climate of the Past
Discussions

Interactive comment on "Last Interglacial climate and sea-level evolution from a coupled ice sheet-climate model" *by* H. Goelzer et al.

A. Ganopolski (Referee)

andrey@pik-potsdam.de

Received and published: 16 February 2016

The manuscript by Goelzer et al. presents results of the first fully interactive simulation of climate and ice sheet evolution during the penultimate glacial termination and the last interglacial (LIG) using an Earth system model of intermediate complexity. The authors show that reconstructed temporal dynamics of sea level during the LIG can be successfully reproduced by their model. The authors for the first time demonstrated that disintegration of the last fraction of the West Antarctic ice sheet (WAIS) at the beginning of LIG can be solely explained by the dynamical response of the ice sheet to sea level rise. The manuscript presents in depth analysis of the processes and feedbacks operating in the system supported by a set of sensitivity experiments. The manuscript is well-written and properly illustrated. I believe this is an important scientific contribution and I would recommend it for publication in CP after minor revision.

General comments

1. Although the manuscript by Goelzer et al. is not the first paper produced in the framework of the same project and many technical details have been already described in Loutre et al (2014) and Goelzer et al (2015), for the readers' convenience a more detailed description of experimental design would be helpful. In particular I would suggest (i) provide information of how surface mass balance of ice sheets was simulated and give in the table the values of semi-empirical parameters; (ii) explain how temperature and precipitation anomalies from low-resolution climate component were applied to high resolution ice sheet models and how changes in ice sheet elevation and extent were accounted for; (iii) how simulated ocean temperature anomalies were used to compute submarine melt of ice shelves; (iv) how one-way coupling experiments have been performed; (v) how "present" GrIS and AIS have been simulated.

2. I have a question concerning scaling technique to reconstruct Northern Hemisphere (NH) continental ice sheets during penultimate termination. According to the manuscript, evolution of NH ice sheets were prescribed using Lisiecki and Raymo (2005) benthic stack L&R04 and the Fig. 4 from Goelzer et al. (2015) shows that according to L&R04 the termination was only half-way at 130 ka with the global sea level still ca. 50 m below present. This would imply existence of large continental ice sheets in the NH which is consistent with the Fig. 2 from Goelzer et al. (2015). However, according to the Figure 10 (top) from the new manuscript, the volume of NH ice sheets at 130 ka was only 10 meters in sea level equivalent which is only 10% of their LGM value. If I misunderstood your approach, please clarify.

3. To prevent GrIS from complete melt, the authors scaled down simulated temperature anomalies used for calculation GrIS surface mass balance. This is somewhat surprising in a view that simulated glacial-interglacial global temperature change in the model is only about 2C which is much less than results of PMIP2 and 3 models which simulated global LGM cooling of 4-5C. Moreover, uncorrected simulated GrIS temperature anomalies during LIG are only about 3C which is still well below "NEEM temperature

reconstructions”. It would be useful to show simulated summer temperature anomalies over the GrIS because summer temperatures are the most important for ice sheet mass balance.

4. While I have no problem with the pragmatic decision to scale GrIS temperature anomalies down, I am missing an explanation why the authors decided to use the factor 0.4 as the reference value and considered 0.3 and 0.5 as the upper and lower limits. I wonder whether simulation for scaling factor 0.4 is better than for other two, can the value 0.5 can be accepted or rejected by empirical constraints and whether any larger scaling factors can (or cannot) be ruled out? I believe that at present the only thing we can say with some confidence about GrIS during LIG is that melting of more than half of modern GrIS would be difficult to reconcile with the existing empirical constraints. Any number below 3 meters is equally probable and therefore implied accuracy of reported “1.4 m” significantly underestimates uncertainties of this estimate. I also found it noteworthy that three numbers for the range of GrIS contribution during LIG (0.6, 1.4, 2.8 m) given by the authors are almost identical to the values given in the recent paper by Calov et al. (2015, CP): 0.6, 1.4, 2.5 m.

5. While the estimates of GrIS contribution fall well within the range reported in a number of previous studies, dynamical collapse of the WAIS during LIG is new and very important finding presented in the manuscript. Thereby it would be interesting to learn more about the mechanisms. The authors show that Antarctic ice volume overshoot is not related to enhanced surface or subsurface melting, as was proposed in some previous studies, but mostly of dynamical WAIS response to prescribed global sea level rise. In this relation I have a question. What is the crucial difference between the penultimate and the last glaciations which explains this overshoot: much faster sea level rise during the penultimate glaciation or the fact that sea level from Grant et al. (2012) overshoots Holocene sea level by ca. 10 m already at the beginning of LIG? The authors mentioned that they performed similar simulations with the L&R04 sea level reconstruction. Since L&R04 stack suggests a slower rate of sea level rise and

does not overshoot present sea level during LIG, I wonder what is the WAIS dynamics in this experiment.

6. Although the mechanism for the WAIS disintegration found in the study by Goelzer et al. differs from that proposed by Holden et al. (2010), I do not believe that the modeling results presented in the manuscript under consideration can be used to rule out completely importance of submarine melt for stability of the WAIS. The reason is that simulated in the current study bipolar see-saw is very weak compare to other modeling results and paleoclimate data. The later reveal significant temperature overshoots at the beginning of LIG essentially everywhere in the SH, and the magnitude of temperature overshoots (above present) in different Antarctic locations was at least several degrees. At the same time, in the work by Goelzer et al. (2015) only a tiny (0.2C) temperature overshoot is seen in subsurface South Ocean temperature (Fig 7b) and essentially nothing in SH or Antarctic temperatures. This seems to be a typical feature of the LOVECLIM model (e.g. Menviel et al., 2015, EPSL). I believe, this potential caveat of the current study should be mentioned in the discussion.

Specific comments

L 82 It should be Pollard et al. (2015)

L 182 What is the meaning of “dynamically computed”?

L 183 Does “governing” means here “major”?

L 187 “. . . assumes ice volume to be independent of deep-sea temperatures” This incorrect formulation. In fact, the sea level reconstruction based on Red Sea d18O, unlike benthic d18O, does not require information about deep-sea temperature because it based on planktonic forams. It is also affected by temperature (sea surface temperatures) but to a lesser degree than benthic d18O.

L 223 Would be useful to clarify how the “stand-alone ice sheet forcing” was defined for penultimate glacial cycle.

L 255 Would be interesting to know why “the retreat of the WAIS” in the interactive experiment “occurs 2 kyr later compared to the one-way experiment”

L 310 I fully agree that if “NEEM temperature reconstruction is applied uniformly in space and over seasons, than in any model GrIS will melt completely. However, if Eemian warming had strong seasonality, as proposed by Merz et al. (2015, CP) with large warming in winter and small warming in summer, then in combination with some other factors, “NEEM paradox” can be resolved.

L 322 See my previous comment

L 355. As I already stated in general comment, not much happened in the Southern Hemisphere in response to freshwater forcing in the Northern Hemisphere. This is why it is not surprising that Antarctic temperature is so flat.

L. 370 Would be useful to show also ocean (subsurface) temperature in the respective figure.

L. 411 Which “environmental forcing” is meant here?

L. 412 It should be Pollard et al. (2015)

L. 428 “Ocean expansion is steep. . .” Rather I would say “the fastest sea level rise due to thermal expansion . . .”

L. 440 “0.42+-0.11” This is a typo. Chapter 5 of AR5 does not contain this number. Instead it referrs to the only available estimate of thermal expansion during the LIG of 0.4 +-0.3 m by McKay et al. (2011). In such case I would recommend to cite original publication rather than IPCC report.

L. 452 “0.42+-0.11” m is not the estimate of glacier contribution to sea level during the LIG but rather the maximum possible sea level rise due to melting of all existing at present glaciers and small ice caps. Obviously, there is no reason to believe that all glaciers melted completely during the LIG and therefore real contribution of glaciers and ice caps during LIG was probably much smaller than 0.4 m.

L. 523 "…by preventing tundra warming affecting proximal ice sheet margins". This is not very clear.

L. 539 Please correct doi of Berger's paper

L 575. Correct reference is "Science, 349, doi: 10.1126/science.aaa4019, 2015"

Figure 1. Brovkin et al (1997) is not in the reference list

L 717 I suppose this is not original Grant et al. (2012) reconstruction but its smoothed version. Please, make it clear.

L 746 Does "forced" here means the same as "one-way"?

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2015-175, 2016.