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Climate of the Past
Discussions

Interactive comment on "Antarctic climate and ice sheet configuration during a peak-warmth Early Pliocene interglacial" *by* Nicholas R. Golledge et al.

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The fate of the AIS in the future is of great importance owing to its capability to rise global sea-level by 60 m. Lack of long-term instrumental records hamper our understanding of the behavior of AIS, especially the EAIS, in the 21st century. Geological evidence and simulations for a past warmer-than-present world could advance our knowledge on how AIS may respond to a warmer climate. Golledge et al. investigated the AIS in the Pliocene that is frequently argued as a potential analogue for future world. Although numerous modeling works have been performed targeting at the Pliocene AIS, ranging from offline to fully coupled climate-ice sheet simulations, their work differs with previous ones mainly in the so-called "tipping point" analysis.

>>> We agree that our work is novel partly because of the tipping point analysis, but this is actually only a small part of the study, not the main focus. In beginning this work, our main point of difference compared to other studies was that we decided to focus on the early Pliocene, rather than the mid Pliocene, because the early Pliocene is very rarely (if at all) studied, and yet has interglacials characterised by summer insolation that is greater than occurs later in the Pliocene. In the revision we will improve the clarity of our introductory text so that this is clear.

However, I have large concern on effectiveness and implication of the "tipping point" analysis performed in this work. In my opinion, the level of warming needed to melt an ice sheet completely or a key region (e.g., ice over Wilkes Subglacial Basin in the Pliocene) is considered to be a critical threshold, or tipping point. For example, the tipping point for the Greenland ice sheet is about 1.6 oC (Robinson et al., 2012). The authors performed the so-called "tipping point" analysis, but give no efficient information on the actual tipping point.

>>> We would like to point out a fundamental difference between a tipping point and a threshold, terms that the reviewer uses synonymously. A tipping point is a transient feature, whereas a threshold is non-temporal. During the evolution of an ice sheet it may be that a tipping point is reached in which the trajectory of evolution changes. This is what our study investigates. This is not the same as defining a single temperature at which the ice sheet may be stable or unstable in a given area. Consequently it is not possible from our tipping point analysis to provide information on a threshold, the two phenomenon are simple different entities. However, in our revision we will make this distinction clearer, and will refer the reader to a recent paper that *does* quantify threshold temperatures for individual catchments (Golledge et al., 2017, GRL).

In addition, the technique used may be inapplicable here as the climatic forcing is constant.

>>> Actually, this is the whole point of the tipping point analysis, and is what makes it

so useful. If we had imposed a time-varying forcing, then accelerations in the mass loss of our simulated ice sheets could be attributable to changes in the forcing. This is *not* what we show. By analysing the timeseries data from a constant forcing experiment, the tipping point analysis is able to not only show where genuine system instabilities occur, but also to provide information on the timescale over which this instability evolves. A detailed explanation of tipping point analysis can be found in Thomas, 2016, QSR. In our revision we will clarify these aspects so that the utility of our approach is evident.

In this way, I think the signal detected is the time needed to melt parts of ice sheet for a given forcing, such as these shown in Fig. 8.

>>> Yes indeed, the analysis shows that gradual surface lowering leads to a critical point at which margin destabilisation takes place, and rapid mass loss ensues.

Besides, as Wilkes Basin is a key region for the stability of the Pliocene AIS, it is necessary to analyze temporal evolution of ice volume over there and perform the "tipping point" analysis. [Robinson A, Calov R, Ganopolski A. Multistability and critical thresholds of the Greenland ice sheet. Nature Climate Change, 2012, 2(6): 429-432]

>>> The reviewer is correct to point out that the Wilkes basin is a key area, and for this reason we dedicated text and figures to the specific investigation of ice dynamics in this area. We show clearly that initial surface lowering from atmospheric warming leads eventually to thinning and flotation, and subsequent grounding-line retreat that is accompanied by mass loss. However, we do not apply the tipping point analysis to this sector in isolation, because 1) we have already shown clearly how this area responds to forcing, and 2) our purpose with the tipping point analysis is to see how these various regional sensitivities play out in terms of a continentally-integrated value (which is ultimately what is important for sea level rise).

Other concern is on the uncertainty in the modeled AIS. The values of ice sheet model parameters are poorly constrained due to the limited observations over Antarctica, which may introduce an uncertainty into the simulated AIS. For example, Yan et al.

(2016) indicated that the largest source of uncertainty in the modeled Pliocene AIS is derived from ice sheet model parameters, which result in a range of 10.8 m in sea level equivalent. I recommend that the authors should perform several sensitivity runs to test whether the so-called "tipping point" is greatly affected by parameter uncertainty. [Yan, Q., Z. Zhang, and H. Wang (2016), Investigating uncertainty in the simulation of the Antarctic ice sheet during the mid-Piacenzian, J. Geophys. Res. Atmos., 121, 1559–1574, doi:10.1002/2015JD023900].

>>> There are two approaches to dealing with ice model parameter uncertainty in these kind of studies. One approach simply undertakes thousands of experiments (a large ensemble) with incremental changes in each of several key parameters, such as flow enhancement factors. The results are then subsequently analysed with respect to observational constraints to establish which ensemble members are consistent with the data. We do *not* adopt this kind of approach. Instead we follow a more targeted methodology in which model parameter choice is incrementally refined through an iterative procedure in which we constrain our model to fit the present-day ice sheet geometry and surface velocity field. To achieve a good fit we adjust ice flow parameters based on expert judgement, not in an unguided manner as is done with ensemble approaches. The result is a spun-up, thermally and dynamically equilibrated ice sheet simulation that is the best fit to observational constraints that is possible by tuning available model parameters. All our Pliocene experiments are run from this starting point. Thus whilst we agree with the reviewer that parameter uncertainty can be a large source of error, we argue that our approach removes this uncertainty prior to our undertaking the prognostic experimentation. In our revision we will clarify this, and highlight the detailed description of our tuning procedure which we have described previously in other papers (e.g Golledge et al., 2015, Nature).

Additionally, the simulated absolute temperatures with RCM are generally consistent with proxies, though a bias of 1-2 oC is found. So I think it is useful to drive the PISM with outputs from the RCM directly. However, the authors employ an "anomaly" method to construct the Pliocene forcing used in PISM. The method should be justified. The authors can also compare the simulated temperature anomaly with reconstructed anomaly or compare the newly constructed Pliocene forcing with reconstructions. In this way, they can test which method is better, the "direct method" or the "anomaly method".

>>> Again, the reviewer is correct is suggesting that using the RCM values directly would be a useful approach. This is precisely what we do. We also use the RCM values with additional biases added, to account for uncertainties as described in the paper. But the simulations using the un-adjusted RCM fields are there explicitly for comparison, so the reader can assess for themselves what the impacts of the adjustments are. We will make sure this is even clearer in our revision.

Specific comments: Page 2, line 4: it should be "2-4 oC" warmer in the mid-Pliocene.

>>> OK.

Page 2, line 23: How the sea surface temperature is set over land? It is set to land temperature or others? The temperature over subglacial basins are important and affect the simulated ice sheet retreat.

>>> We apologies for not making this clear. For areas that are ice-covered at the start of the run we prescribe a uniform temperature of 271.2 K, essentially the sea-water freezing point. This avoids potential errors that could be introduced by interpolating ocean fields landward, but we recognise that this may lead to underestimated basal melt in subglacial basins during ice sheet retreat. In the revision we will make this clear.

Page 4, line 25: please add a brief description on the parameterizations of sub-shelf melting in PISM.

>>> OK, we will add this.

Page 5, line 24: how long the model is integrated? 10 kyr? Does the model reach

quasi-equilibrium? Please clarify this in the manuscript.

>>> Page 5, line 29-30: "Simulations are run for 10 kyr." Figure 8 illustrates the evolution of the ice sheet in the range of scenarios discussed. From this figure it can be seen that under some climatologies a near-equilibrium state is reached, whilst under others it is not. This is described in the Results section, on pages 7 & 8.

In Fig. 4: How the temperature anomaly over sub-shelf region is calculated? Is WAIS also removed in the control run? Actually, the RCM used cannot simulate oceanic temperature below ice shelves that is required in PISM.

>>> The ocean forcing applied to the ice sheet model from the RCM simulations is shown in Figure 2 and described in the text. The RCM simulation uses a geometry that does not include WAIS. This does not affect the grounded ice, however, since the temperature field is only used to compute basal shelf melt. In the simulations that use the sub-grid basal melt interpolation, this temperature field will also result in melt at the grounding line (see page 5 for explanation).

In Fig. 5: How many experiments are carried out? Nine? If so, as the number of experiment is not large enough, the results from each experiment can be plotted as a dot rather than dashed lines in Fig. 5, which may cause misunderstanding. Besides, the work of Yan et al. (2016, JGR) can be added here.

>>> In total we present results from 18 experiments, in which we separate out the two suites that use or do not use the sub-grid melt scheme. The solid black line represents all members of the ensemble, whilst the thinner lines represent the two components. These latter curves are provided simply to illustrate their relationship to the ensemble curve, and we do not ascribe any statistical significance to them particularly, they simply offer an additional level of detail that helps the reader identify the consequences of the two grounding-line schemes. We will add the values from Yan et al. 2016.

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