

Interactive comment on “Deglacial evolution of regional Antarctic climate and Southern Ocean conditions in transient climate simulations” by Daniel P. Lowry et al.

Anonymous Referee #2

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Review of: Deglacial evolution of regional Antarctic climate and Southern Ocean conditions in transient climate simulations Daniel P. Lowry, Nicholas R. Golledge, Laurie Menviel, Nancy A.N. Bertler (doi: 10.5194/cp-2018-69)

Overview of manuscript: The authors analysed model output for the period 18 ka to 6.5 ka (they use “kyr”), which corresponds to the period from the last glacial termination (i.e., the short warming period that marks the transition to from the last ice age to the current inter-glacial period) to the mid-Holocene. Two models were used: TraCE-21ka (a fully coupled GCM) and LOVECLIM DGns (an intermediate complexity model). Analysis consisted of: (i) looking at time and space evolution of deglaciation in the

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models, and (ii) comparing model output with proxies for surface temperature, surface mass balance, coastal ocean temperatures, and sea ice. The authors’ were not able to draw firm conclusions about the mechanisms that determine the regional differences that paleoclimate records indicate existed for this period. They were also not able to determine the strengths and weaknesses of the models in terms of ice sheet mass balance predictions. This inability to draw firm conclusions was because there are few climate model simulations of this deglaciation period to make comparisons between, and because there is a lack of high-resolution proxy data.

General comments: After reading the abstract, I was very interested to hear what the authors’ results were, but I ended up being extremely confused by the end of the manuscript and needed to re-read it several times. My confusion was mainly for the following reasons: (1) The aims and results outlined in the abstract do not appear to be consistent with what the conclusions state at the end of the manuscript; (2) Some of the figures and their captions are missing crucial information that makes them impossible to understand in isolation from the text; (3) There are some bold assertions regarding causation that do not appear to be supported by citations of the work of others or by independent analysis in this manuscript; (4) It is not clear to me how such sparse data sets can be compared to the models used. I have elaborated on these points in the specific comments below.

Specific comments: In this section, I provide specific details relating to the general comments above.

(1) The aims and results outlined in the abstract do not appear to be consistent with what the conclusions state at the end of the manuscript. The abstract states that the aim is to analyse results from two models to “better understand the mechanisms driving regional differences observed in paleoclimate models” and to “identify the main strengths and limitations of the models in terms of parameters that impact ice sheet mass balance”. The abstract then states that the “climate simulations show” a number of results relating surface warming and accumulation rates to changes in sea ice, atmo-

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spheric circulation and ice surface elevation. The abstract also states that differences between the models and the proxy data exist, and suggested that this is because of inadequate representation of Meltwater Pulse 1A and 1B. However, in the “Summary and Conclusions” section, the ice sheet elevation effect on surface temperature is worded as if it is a specific result for TraCE-21ka, whereas in the abstract it is worded as if this is true for all simulations. In the “Summary and Conclusions” section, the accumulation rates are described as having “Strong discrepancies” between the models, which the authors suggest is related to model resolution issues, and they also note that the models do not match ice core accumulation reconstructions at the WDC and EDC sites. However, in the abstract the authors merely state that the accumulation changes in the model results are “quite distinct” and that the intermediate complexity model (which is not named in the abstract, but which is LOVECLIM DGns) had “resolution enhanced bias along the East Antarctic coast”. The abstract states that variability in the relationship between accumulation and temperature has higher variability for coastal regions in the early to mid-Holocene, and state this “coincides with” atmospheric (Amundsen Sea Low) and sea ice changes. However, in the “Summary and Conclusions” section, this relationship is phrased more cautiously, with the use of “may”, “appears to” and the statement for the need of a “more detailed moisture budget analysis”. In the abstract, the mismatch between the models and proxies for the time and duration of the ACR and Younger Dryas/early Holocene warming is note, and states this is “suggesting that the Meltwater Pulse 1A and 1B events may be inadequately represented in these simulations.” However, in the “Summary and Conclusions” section, the authors state that this mismatch “may result from model bias in large-scale ocean circulation, poorly constrained boundary conditions. . .or some combination of the two”, and then mention meltwater forcing as something deglacial evolution is “highly sensitive to.”

(2) Some of the figures and their captions are missing crucial information that makes them impossible to understand in isolation from the text: (i) The blue (DGns) and black (ice core data) lines are hard to distinguish in Figures 1-5. (ii) Figure 4 shows on the left-hand side graphs changes in snow accumulation (I think this should be “accumulation

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rates” because the units are “%”, so presumably “% per 100 years” as in figure 3?) on vertical axes and degrees Celcius temperature change on the horizontal axes, but these axes are not labelled (they should be). The top graph on the left (a) is missing minus signs from the lower part of that graph’s vertical axis. Parts of graphs (f) and (h) (which are for EAIS coastal and AP, respectively) are shaded yellow, but it is not explained why in the caption. (iii) Figure 5 shows regional SST and ocean temperatures as a time series of 100 year averages (presumably means) for “TraCE” (called “TraCE-21ka” in previous graphs) and “LOVECLIM” (previous graphs called this DGns, the full title of the model is “LOVECLIM DGns”; consistency between graphs would be helpful). (iv) Figure 8 graphs are labelled (a) to (d) on both the left-hand side and the right-hand side, but the caption indicates that those on the left-hand side should be labelled (e) to (h), which is very confusing. The left-hand side graphs show 100 year averages of percentage sea ice coverage for (I presume, it does not say in the caption) the TraCE-21ka model and the DGns model, while those on the right-hand side (again, I presume) show sea ice thickness. The reader needs to assume the same color-coding for model output as in previous graphs, because there is no legend, which is confusing.

(3) There are some bold assertions regarding causation that do not appear to be supported by citations of the work of others or by independent analysis in this manuscript. This is particularly the case for causation attributed to sea ice changes. Examples include: (i) Lines 200-203: Large regional temperature differences in the model results for both models are stated to be “due to decreases in annual average sea ice coverage”. How this conclusion regarding causation was reached is not explained. (ii) Lines 203-205: Differences between the results from the two models for regional temperature increases are stated to be “primarily due to differences in modelled sea ice”. How this conclusion regarding causation was reached is not explained. Figure 8(c) indicates almost no change in Weddell Sea sea ice coverage for TraCE-21ka, but this is not discussed by the authors in this context. (iii) Having made some bold assertions regarding temperatures at lines 207-221, the authors then concede at lines 222-223 that “some of the differences between the models and ice core temperature recon-

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structions could be due to local climate effects of the ice core sites not captured in the broad regional averages of the climate models”, which raises the question of how valid any of the comparisons between the ice cores and the models are. (iv) Lines 362-365: increases in continental surface temperature are linked with sea ice changes, with the authors stating “regions displaying the greatest increases in continental surface temperature that are not associated with changing ice sheet topography occur along the continental margins. . . suggesting that albedo-driven radiative changes associated with sea ice coverage may be an important driver of regional warming differences”. This is more cautiously worded than the examples given in points (i) and (ii) above, but are still not physically justified. (v) Lines 369-370: similarly to point (iv) above, there is a lack of justification of the assertion “Changes in sea ice coverage may also explain the coastal warming differenced observed between DGns and TraCE-21ka.” (vi) Lines 382-386: similarly to points (iv) and (v) above, there is a lack of physical justification for the assertion “ the retreat of sea ice extent and reduced annual sea ice coverage in the early to mid-Holocene. . . may also introduce a greater variety of moisture sources of continental precipitation and alter the synoptic-scale variability, thereby weakening the SST-precipitation correlations in both models.” (vii) Lines 454-471: in this paragraph, the authors start with “It may be expected that the retreat of sea ice and increased area of open ocean may introduce additional moisture sources, thereby enhancing precipitation relative to temperature.” The authors then outline the main results from the literature, and summarize the results of their simulations which “do not exhibit a substantial increase in the scaling relationship with reduced sea ice coverage”. In other words, the bold assertion of a conceptual model in their first sentence is not supported by their modelling results. The paragraph ends with a call for “additional moisture budget analysis”.

(4) It is not clear to me how such sparse data sets can be compared to the models used. If I understand correctly what the authors have done, they have compared five ice cores with model output for surface temperatures from two global models, and two ice cores with model output for snow accumulation rates from two global models. As I have noted

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earlier, the authors concede at lines 222-223 that “some of the differences between the models and ice core temperature reconstructions could be due to local climate effects of the ice core sites not captured in the broad regional averages of the climate models”, which raises the question of how valid any of the comparisons between the ice cores and the models are. There is a great deal of research on comparing model results with observations for modern day climate, and I would particularly recommend the authors read Notz (2015), titled “How well must climate models agree with observations?” (doi: 10.1098/rsta.2014.0164) and the papers cited therein. Notz (2015) uses sea ice as a particular example, so it is very relevant for what the authors’ are attempting to do here. Sea ice proxies particularly lacking, so what can the authors here really say?

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