The article by Sutter et al., describes a large ensemble of simulations for the evolution of the Antarctic ice sheet during the past 2 Ma with a continental scale 3D thermomechanical ice-sheet model. The goal of the paper is clearly described in the title and nicely addressed throughout the description of the results, the discussion and the conclusions.
I think this is a good paper whose scientific question is clearly suitable for TC.

That being said, I think two main aspects of the paper need to be addressed before publication. They correspond to the two following general comments. Below, you will also find an extensive list of more specific and technical issues.

GENERAL COMMENTS

1) Main goal of the study:

I think this study nicely focuses on providing new information on where to look for the oldest Antarctic ice within the framework of continentally wide ice-sheet modeling. And it is also appreciable to subscribe such an approach within the context of transient simulations covering the MPT.

Nevertheless, several parts of the abstract and the introduction can be misleading because they are somehow suggesting that an important part of the paper will be devoted to analyze the effects of parametric uncertainty on the Antarctic ice dynamics during the MPT.

For example, the abstract reads: “We discuss the effects of changing climate conditions, sea level and geothermal heat flux boundary conditions on the mass balance and ice dynamics of the Antarctic Ice Sheet.” This is not strictly true, particularly so with respect to ice dynamics (there is not a single panel or comment devoted to ice velocities or changes in the ice flow dynamics as a result of the changing climate). Changes in the grounding line position (also very superficially tackled), basal temperature and thickness evolution are related to changes in ice velocities, but this relationship is not addressed in the paper.

Similar misleading sentences to that of the abstract mentioned above can be found throughout the introduction (e.g. page 2, lines 27-29) and methods (page 7, lines 5-7).

I suggest two different ways of mitigating this issue:

a) Lower the reader’s expectations concerning ice dynamics.
b) Expand the analysis on the effects that different parameters controlling the ice flow have on the conclusions of the study.

If a) is chosen, it would probably be enough to rephrase the above mentioned sentences. It should not be until you arrive to page 9, line 2 (“The main objective of this work is to assess the existence of 1.5 Myr old ice along the East Antarctic ice divide.") that the reader realizes what the real goal of the study is.

For b) to be tackled, I suggest the following issues to be included:

- Show at least one 2D plot of ice velocities (preferably during the MPT) and discuss the capability of the model to capture the observed main characteristics of PD ice flow and how these features compare to the suggested velocities plot.
- Discuss what the effects are of a change in a parameter controlling the ice flow on the ice thickness evolution for a given change in the climate forcing. For example, choosing a period of a large WAIS retreat, I expect a different reaction (to the grounding line retreat) of the interior of the ice sheet depending on the values of e.g. till_min and till_max. This is expected because, for a given grounding line retreat, the interior of the ice sheet will react very differently depending on whether the ice flow is mainly controlled by very active ice streams or by a slower deformational flow. This difference, can dictate the ice thickness evolution even at the domes making this analysis relevant for the conclusions regarding the "oldest ice".
- Similar relevant conclusions can potentially be reached if a detailed analysis is performed on the effects that changing the basal shelf melt parameter has on the entire AIS.

In its current form, these parameters (the ones controlling the ice flow and the basal melting) are just included in order to have an idea of the spread of the ensemble (which I think is still a fair approach if conveniently acknowledged) rather than using them to gain insight into the processes that control the ice thickness evolution and hence ultimately defining the location of the oldest ice..

2) Reproducibility and description of the methods:

The manuscript contains a large amount of small inaccuracies, typos and erratum, particularly so in the methods section. These, taken individually, do not constitute a major problem, but taken together have a double negative effect: i) do not allow for the suitable reproducibility of this study, and ii) make the reading of the manuscript frustrating.

SPECIFIC COMMENTS

1) Ice-sheet model and ensembles

Page 4, line 29 reads: "[...] we linearly scale the computed present day melt rates in the Amundsen and Bellinghausen Sea by a factor of 10 and underneath the Filchner Ice Shelf by a factor of 1.5. Shelf melt rates adjacent to Wilkes, Terre Adelie and George V Land in East Antarctica are also scaled by factor of 10."
What do these scaling factors do?
Are they simply multiplying (or dividing) the observed values?

Please define.

Furthermore, the above quoted sentence seems incongruent with having a parameter between 1 and 10 as a part of the ensemble controlling the basal melt values (gamma_EAIS [1;10]). Caption of table 2 refers to George V and Wilkes Lands, while the above scaling factors refer to other basins as well.
Thus, are the values of the shelf melt being changed as a part of the ensemble?
If yes, both basins simultaneously?
On the other hand, if these scaling factors reflect the uncertainty associated to the processes that determine shelf basal melt in time, why not exploring the factors of the rest of the basins as well?
Please define and clarify.

How is the shelf basal melting evolution in time achieved in the model?
Is it dependent on the temperatures of the ocean given by equation 2?
If yes, how is it done? Perhaps an anomaly method with respect to the scaled PD basal melt defined in page 4, line 29?
Please define.

P4, L32, calving parameterisations:
Are these two parameterisations (threshold and eigen) exclusive to each other?
If yes, then specify in the ensembles description that you explore the use of two different calving "laws", the threshold one with two values of its parameter and the eigen one with only one value.
If no, then please remove the eigen parameter value to the table summarizing the explored values of the parameters for the ensemble.

Table 2 shows the explored values of two parameters that are not described nor mentioned at all in the text: "sia" and "ssa". By looking at the values, I assume these refer to the enhancement factors of the SIA and the SSA parts of the simulated ice sheet. They need to be defined and properly named (sia and ssa are not parameters per se but approximations).

Table 2 shows the parameters till_min and till_max. These are not defined nor described in the text. The reader can only speculate about the possibility that these parameters have something to do with the description of page 3, last line: "[...] the yield stress (tau_c) is determined by the pore water content and the strength of the sediment which is set by a linear piecewise function dependent on the ice-bedrock interface depth relative to sea level". If the reader keeps digging and tries to identify these parameters in the references of the model given here, will still not succeed because no mention to "till_max" and "till_min" can be found in Bueler and Brown 2009, nor in Winkelmann et al, 2011. I had to go to Martien et al, 2011 (The Cryosphere) and assume that "till_min" and "till_max" correspond to the upper and lower numbers given by their equation 10. Is this correct?
Please define, clarify and cite accordingly.

2) Glacial index description.

Having the glacial index in Figure 5 and knowing that you use 3 climate snapshots weighted in time by such an index, one can have an idea of how you are forcing the ice sheet model. However, the description of the method (including equations 1 to 5) is a bit odd. Perhaps it would be simply solved by providing the values of GI_pd and GI_max (as far as I saw the value of these parameters is not given in the manuscript).

Otherwise the reader could wonder:

i) why is the value of the glacial index of ca. 0.7 during the Holocene? Does that mean you are taken a 30% of the LGM anomaly? (I guess not, and assume you put GI_pd to be approximately 0.7, so w_g in equation 3 goes to 0)

ii) Is “glacial index” the right term for a curve that goes to 0 during glacial times? Would not be more appropriate to define it as (1-GI) or simply call it “climate index”?

ii) What happens, for example when GI = 1.2 ? Do you take a linear interpolation of the Pliocene and the Last Interglacial climate fields? (I would assume so, because in equations 1 to 5 there is not any explicit differentiation of the time period, just a dependence on the values of the index, GI). If yes, can you justify it or elaborate?

Furthermore, equation 6 must contain a typo or be wrongly formulated. In its current form, the more you cool from PD the more you increase the precipitation. Is that correct?

Typos / erratum on the glacial index description:
P5, L21: “T_opd is the surface temperature” (should be T_s)
P5, L22: commas missing after “LIG” and “LGM”. I advise you to rephrase the whole sentence.
P5, last line of equation 5: It reads “0.0 pdfor GI”.

3) Other specific/technical comments:

P5, L2 reads. “To adequately capture continental ice sheet dynamics on long timescales (i.e. millennia and more), in principle, a coupled modelling approach is required to resolve climate-ice sheet interactions.”
I think I understand what you mean here, but I also believe a sentence that says “To capture A, a given approach is required to resolve B” does not make sense.

P9. L24: “The two clusters in the upper panel of Figure 5 show a present day ice sheet configuration (B1-branch) and a strong interglacial configuration in which the WAIS is collapsed (B2-branch)”.
I think this sentence needs some rephrasing.
By “.. show a present day and a strong interglacial” do you mean that their mean state is similar to the ones expected during present day and strong interglacial respectively?

P9. L24: “...resembling the waxing and waning of the marine West Antarctic Ice Sheet” “resembling” or “due to” the waxing and waning...?

Figure 4: hard to see. Because not said in the caption, I assume black (or dark grey) thin lines in the top panel correspond to the individual realisations of the B1 ensemble. But, because the Pollard 2009, deBoer2014 and Tichgelaar 2018 curves are also plotted in dark grey or black they are really not distinguishable. Why not plotting the individual members in white or light grey as for the B2 ensemble?

At the end of section 3.1 you state: “[...] all simulations with the GHF field from Purucker (2013) exhibit a collapse of the WAIS in the LIG with a much smaller percentage for both Martos et al. (2017) and Shapiro and Ritzwoller (2004).”
Why is this? I see no obvious explanation since the West Antarctica GHF values from Purucker seem lower than those from Shapiro and way lower than Martos’s.

Figure 6 left panel:
i) Are the LGM (and LIG) values given in red and blue the mean of the three particular cases shown in figure 5?
Please specify in the caption.
ii) If yes, why is the LGM mean (-6.12 m) considerably higher than the red (-9.65 m) and the blue (-10.17 m) means?
Is it because the red and blue values are simply the mean of the three particular cases and not the mean of the whole B1 and B2 ensembles?
If yes, what makes these 3 particular cases to show a bigger ice sheet than their respective sub-ensembles during the LGM?
Perhaps something related to the fact that these cases all have the lower SIA and SSA enhancement factors of the ensembles?
If yes, can we learn something from this related to the conclusions of the paper?

Figure 6 middle and right panels:

- To what particular realisations of the ensemble do the 2D plots correspond to?
- Following the PD grounding line position line, I recognize indeed its current grounding line position but also its current ice front. Is that correct? Are you plotting both? If so, I would change the legend or specify it in the caption
- Why is the LGM grounding line of the Ronne ice shelf, Pine Island and west of the Antarctic Peninsula much more retreated than the one suggested by Bentley?
- Is this a result of the particular model realization or proper to the ensemble?

Figure 6:
It would be nice to have a third 2D panel showing the simulated PD ice sheet for the same parameters as for the LIG and LGM plots.

Caption of figure 6 reads: “Middel and left panel illustrate simulated ice sheet configurations for the LGM and Last Interglacial”.

“Middel” is wronlgy spelled.
“Left” should be right. And invert the order of LGM and Last Interglacial.

P15, L6 reads: “Mean ice thickness variability for Dome Fuji and Dome C during the late Quaternary is 165 and 195 m, respectively (105 and 140 during pre-MPT).”
How is this “mean ice thickness variability” calculated? Is it the temporal standard deviation of the mean evolution of the ensemble, or has it something to do with the standard deviation of the ensemble itself?

P15, L7 reads: “Overall, the simulated present day ice cover after 2 million years at the highlighted ice core locations is in good agreement (within ≈ 5%) with the BEDMAP2 (Fretwell et al., 2013) data set.”
This sentence does not seem precise enough. Does “present day ice cover” mean ice thickness? Here it is important to be precise, because it is not the same saying that the the error of the simulated region of Antarctica covered by ice falls within 5% with respect to BEDMAP2 than saying that the simulated thicknesses are within 5%. If the latter is what you meant, how did you calculate it?

Somehow related: Why is Talos systematically too thin?

P15, L11 reads: “We apply the conditions for the existence of 1.5 Myr old ice derived in Fischer et al. (2013) to our simulations …”.
Please specify the conditions you refer to here. This would allow the reader to have an idea of your “oldest ice” results without having to look for such conditions in Fischer et al. 2013.

……

As a conclusion, I think this is a nice paper and that these comments represent only a minor revision, but I also fear that, in its current form, the reader would not appreciate the manuscript as much as they should. Therefore, I recommend the authors to correct all these minor issues and carefully go through the new manuscript in order to maximize reproducibility and a nice “flow” when reading it.