Comment on "Cosmogenic-nuclide data from Antarctic nunataks can constrain past ice sheet sensitivity to marine ice margin instabilities" by Anna Ruth Weston Halberstadt et al., (https://doi.org/10.5194/tc-2022-213)

The paper investigates the use of cosmogenic nuclide measurements from nunataks across the interior of Antarctica to evaluate the performance of ice sheet model simulations over past time periods. Under certain assumptions and requirements, the inferred frequency at which the sample sites have been covered by ice can be used as a proxy to determine a cumulative distribution function for ice sheet thickness in the surroundings of the nunatak. Uncertainties aside, the same quantity can be derived from an ice sheet simulation for that location, and thus a direct comparison between model and data is possible. In addition, the authors propose a metric based on the model version of this function to identify regions where different model realisations exhibit large discrepancies in their integrated behaviour. According to the authors, future measurements over some of these regions could provide valuable benchmarking capabilities for ice sheet models used in both paleo reconstructions and future projections.

In my opinion, model-data integration is a topic in ice sheet modelling that has been undeservedly left as a secondary problem due to computational limitations that, so far, have made it difficult to create bridges between the field and modelling communities. As shown by the authors, the method explored in this study is a valuable approach that can, in principle, be used to evaluate virtually any modern paleo ice-sheet set-up. As more precise and abundant field data are gathered, and as the capabilities of state-of-the-art ice-sheet models increase, the applicability of this method will likely improve. As such, the core of the study is a nice addition to the scientific literature and, in this sense, I support its publication in TC. However, there are several points which I found problematic in the way the authors chose to showcase the applicability and significance of the method, particularly within the modelling set-up. I think that these choices do affect the conclusions, and that confirmation that this is not the case is needed before acceptance of the manuscript. I will describe these points as part of my general comments below, and if necessary elaborate on them in the specific comments after that.

General comments:

1. As the authors acknowledge ("Criterion 1" in Discussion), the model simulations need to have sufficiently different CDFs for the method to be usable as a constraining tool. The authors then generate these two sufficiently different model realisations by including/excluding a representation of the ice shelf hydrofracturing (HF) and marine ice cliff instability (MICI) processes, and by simultaneously scaling up/down the time-dependent ocean temperature shift that the model uses to derive melt rates at the base of ice shelves. To clarify my concern, and to prevent specifics worries being diluted in a single comment/response, I will divide it into several smaller points:

A. The authors put both HF and MICI in the same category as the stronger/weaker ocean forcing, and name them all "marine ice margin instabilities". I agree with HF and MICI, as these processes are inherent to ice dynamics, i.e., the modelled ice is reacting (or not; through crevasses and calving) in some consistent way to some time-varying external boundary conditions (e.g. surface temperatures, meltwater ponds, accumulation). If a fixed change in the <u>external</u> forcing is applied, both model realisations will react very differently, with the one including both HF and MICI potentially generating positive feedbacks that can trigger significant ice sheet retreat (e.g. [1]). In other words, a model with HF and MICI *is* more sensitive to a given change in the external forcing.

B. However, applying different ocean temperature shifts is no more than simply applying different changes in the external forcing, not inherent to ice dynamics: apply the same change in ocean temperatures (the <u>external</u> condition) to both models, and the resulting thinning/thickening of ice will be the same. Here, ocean temperature reflects an uncertainty not in model sensitivity, but in the external forcing. In other words, both models are *equally* sensitive to a given change in ocean temperature, and thus equally (un)stable and their responses equally (non-)linear. In this regard, the study is not constraining marine ice instabilities, but simply stronger vs. weaker forcing conditions.

C. A way to include different sensitivities to a given ocean temperature change would be to use two distinct ocean melt parameterisations, e.g. one where total melt is linearly proportional to ocean thermal forcing and another that is quadratic. Those two will react very differently to, say, a 1 K change in local ocean temperatures. Another would be to test melt under partially floating grounding line cells vs not doing it (e.g. [2]), which as a bonus is, in my opinion, equally controversial as HF and MICI. Both options work directly on an inherent ice dynamics quantity, namely dH/dt, instead of the external forcing. That I would consider closer to a "marine ice margin instability", together with HF and MICI.

D. Why such a strong difference in the two end-member simulations? HF and MICI alone (albeit only in tandem) were enough to generate a huge difference for the Pliocene in [1]. Adding on top of that two full degrees Celsius to each side of the ocean temperature shift for the sensitised model feels overkill. And that's not even considering the extra warming added during interglacials to the Amundsen Sea area. I imagine that the method would struggle with less extreme differences, i.e. with fulfilling Criterion 1. What happens when only HF and MICI are used, but not different ocean forcings? Are the resulting CDF's still different enough to make the method usable? Could it be that the sensitised CDF fit is poorer than the desensitised one because of this extreme ocean influence? See also my specific comment L38-39 below.

E. I have the inkling that the ocean forcing is dominating the apparent bimodality of the sensitised CDF, which for me would be problematic, since i) I consider it as external forcing, not as an instability, and ii) it is, as far as I can tell from the manuscript, heavily parameterised. If this is the case, then the method is not constraining marine ice margin instabilities, but simply which parameterised forcing is less unrealistic.

F. A way to confirm this would be to run a third simulation with HF/MICI, but with the same ocean as the desensitised run (i.e. [-1 1]), and a fourth simulation with the ocean full temperature difference applied (i.e. [-3 3]), but with no HF/MICI. That would provide a clearer way of interpreting the model results, and –more importantly-- an assessment of the applicability of the method.

G. If my concerns are confirmed, most of these issues can be alleviated a bit by toning down the title and avoiding specific claims, e.g., “ice sheet sensitivity to marine ice margin instabilities” -> “strong vs weak ice-sheet retreat and advance histories”.

2. While reading the paper, it is not fully clear to me what exact version of the PSU3D ice sheet model the authors used, and therefore how different processes are exactly represented in the study. The authors first introduce (L28) the model with some results from the 2015 version [1], but then cite (L169) the 2021 version [3]. Then, they mention (L172) that the approach in the 2009 version [4] is needed for the external forcing, for which I assume they use a modification of it since modern data sets of surface air temperature, precipitation, and subsurface ocean temperature are here mentioned (none of which is part of [4]), but this modified approach is only modestly described, and without any formulae (e.g. L173 “scaled based on a combination of factors”). Then, an additional parameterisation is mentioned for ocean temperature shifts (L175). Finally, L191 mentions an additional modification for the Amundsen Sea, only for the sensitised model, and only during interglacials:

A. In this version of the manuscript, it is not possible to get a clear idea of the forcing implemented. An appendix is needed where this and every other change with respect to [3] is provided clear and transparently. These details are important since the conclusions might depend on them (see my general comment 1).

B. With the above modifications, this is a different model and thus it does not make much sense to reference previous studies in terms of specific model behaviour. The introduction mentions the difference between models with and without HF and MICI (L30 onwards), which sets the ground for the rest of the paper in terms of instabilities, retreats, sea level contributions, and the inability of previous attempts at discerning between such models (L35). Finally, the goal of the paper and the sensitised vs. desensitised models are introduced (L38-42) within this context. This whole introduction heavily uses Fig 1, which represents data not from this study, but from [1]. After the significant work that running 5 Myr simulations I assume implies, which contain almost all periods covered by [1], [3], and [4], and the fact that this is a different model version, I expect the authors to illustrate their points, and in particular Fig 1, using the output from the model version used in this study. This will be a great chance to showcase the performance of both simulations for not only the Pliocene and present-day, but other crucial benchmark periods, such as 125 ka and 21 ka. See my specific comment L170-171 below.

3. Missing critical reference for the insensitivity of results to model resolution (L171-172). I assume you want to cite one of the model intercomparison initiatives where results seem to suggest that coarse grid resolution models can bypass the 100s-of-meters grounding line resolution requirements, when using e.g. Schoof’s flux parameterisation or a sub-grid interpolation of basal friction at the grounding line (I am also assuming you are using the former). However, those intercomparison exercises usually employ idealised geometries that are far from the steep topographic features found in Antarctica, especially around nunataks:

A. I'd like to see some kind of indication that the results presented in this study and/or the version of the model used by the authors are/is resolution-independent under a similar Antarctic set-up.

B. If such an indication does not currently exist: I understand that 5 Myr is in general computationally expensive, but comparison against sub-set periods, say, even 500 kyr at 32/20 km and 250 kyr at 16km (assuming a ~ten-fold increase in comp. time per doubling of resolution) should allow you to confirm the claim in question, at least regarding the scope, assumptions, and conclusion of this study. This is valid even when the geological data would require longer time spans: the goal is to check how different are the CDF's for limited sub-set periods between simulations at varying resolutions, and not against the data.

Specific comments:

L26 – Could you please elaborate on what you mean by "ambiguous", and add it to the text? The word is used a few times throughout the manuscript, but the only time it is elaborated is in L451, where data are not dense enough (that would be insufficient) or taken from an approach-irrelevant elevation (that'd be uninformative). That is not a problem of the data, though.

L28 – Maybe a matter of opinion, but I would argue that is the present climate (~415 ppmv, ~1 ºC over Pre-Industrial) state the one approaching the peak mPWP states (~350 - ~450 ppmv, ~2.0 - 4.0 ºC above Pre-Industrial, inc. higher sea level), at least when acknowledging the poor paleorecord, uncertainties, and biases. Perhaps removing this sense of direction altogether? References [5] and [6] below (and therein) contain classic, neutral examples of this comparison.

L30 – This would be a good place to introduce the concepts of ice shelf HF and MICI, after "… ice margins (Fig.1)" and before "Model runs…". These concepts are used extensively in this study, and it would be nice to minimally describe them here. Otherwise, the reader is left hanging until L43, where "ocean temperature" is added to the mix (see my general comment 1), only HF is mentioned (not described), and where my brain predicted MICI to be also mentioned (but it was not). This is because [1] show that, at least in their highly parameterised and somewhat speculative (their words) simulations both HF and MICI are needed in tandem to get a retreat similar to that shown in Fig 1c. The reader is then left hanging again until some more info is given in section 3.1, which was in my experience distracting.

L38-39, L56 – This connects to my general comment 1. If the aim of the paper is to explore how to differentiate between margin instabilities, and Fig 1b and 1c are given as examples, then by using buffed and nerfed ocean shifts compared to the homogeneous +2 K that [1] used for their Pliocene, then the answer to the interesting question is obscured: "If we adapted both simulations F1b and F1c in [1] to a 5 Myr run (i.e. turning [1] into [4]), would we be able to discern between them using geological data and CDF's?" That's a beautiful question, but by modifying the ocean the answer is biased.

L71 – Section 2 was a pleasure to read.

L129 – There it is: “given the same external temperature and accumulation forcing”. I fully agree.

L135 – “By a more linear”, perhaps? There and elsewhere

L136 – endmember -> end-member (e.g. L124)

L138 – “Gray bars”, maybe?

L148, L161 – Here MICI is mentioned, but not ice shelf HF. See my comments above on “Line 31”. Consistency is needed on how you define and utilise these terms throughout the manuscript.

L150 – See my general comment 1, and specific comment L39-39, L56 above. Marine ice sheet instability (MISI) is not something that you can (or rather should) “turn on or off” in ice sheet simulations; it is a consequence of a retreating grounding line on a retrograde bedrock slope. If you try to supress it by using colder ocean temperatures during warm periods (colder than the +2 K needed and justified by [1] themselves), and then concluding that geological data support this, that’s a very bold statement.

L162-165 – That’s a long parenthesis. I’d suggest let it be its own full sentence.

L170-171 – See my general comment 3.

L172-180 – See my general comment 2A. It would be much clearer to add an appendix with the exact formulations (i.e. in equation form) mentioned/used in this study.

L173 – Which modern forcing data sets? Model-based (e.g. RACMO, MAR) or observational (e.g. WOA)? See my general comment 2.

L181 – Do you limit the lower shift in ocean temperatures based on the local pressure-freezing point of sea-water? Otherwise (assuming no supercooled water processes are resolved by the model) you could be assuming a below-freezing ocean in many areas during glacials. That would create mass “out of thin water”, artificially making your ice sheet advance and your ice shelves proliferate.

L184 -- Same as specific comment L162.

L186 – Missing parenthesis at the end of the TraCE reference

L187 – I guess you mean “desensitized” here.

L189-190 – This is key to avoid model biases contaminating your conclusions, and it surely needs a figure supporting this claim. The easiest would be to show Pliocene (classic), 125 ka (recent interglacial example), 21 ka (recent glacial example), and transiently reached 0ka, for both simulations. A figure like that would be much more informative than current Fig. 1. See my general comment 2B.

L191-196 – See my general comment 1. As it is surely evident by now, I am not easy with forcing the models by using different ocean temperature shifts and calling that a margin instability. Here, an even stronger difference is applied between the two simulations. What happens without this 1.5 K extra increase? How is the factor applied only during interglacials: as a shock once a time-point is reached, or smoothly as the interglacial is approached? Why is that trend not already in the modern ocean data set used as forcing? Even within quotes, a +1.5 K warming is hardly noise for a simulation that is limited to a modest +1 K above modern.

Lines 197-208 – Again, a summary of the mathematical expressions of the code for key processes discussed in the paper (e.g. atmosphere/ocean forcing and how it evolves, ice shelf HF, and MICI, …; i.e. all about how both simulations differ) is a currently missing appendix. Otherwise, the reader is forced to navigate through the last 10 years of PSU3D development without a clear idea of the precise version of the model used in this study.

L220-222 -- See my general comment 1. I would argue that using different ocean temperatures in your simulations qualifies as "a property of the forcing dataset", which could be in fact playing a primary control on your characteristic model behaviour. By the way, if I understand your method, d18O is not the only forcing used, but insolation and CO2 are involved as well. It'd be nice to have frequency distributions for those as well, and a distribution of the final weight used in the forcing parameterisation. I suspect the additional scaling added to the ocean temperature shifts, which differs between the models, could be bimodal as well. In other words, a frequency distribution of the desensitised ocean temperature shift would be more centred around 0 (by definition, since it is capped between -1 and 1 K) than the sensitised one, which would sit more often in either of the extremes. In that case, I would think that the bimodality of the sensitised simulation is strongly correlated to the ocean temperature applied, which in this case I consider part of the forcing, rather than an ice dynamical instability represented in the model. Could you please confirm this?

L223-228 – I am not convinced. See my general comment 1. Even if the simulations resemble the conceptual example, this could be a "self-fulfilling prophecy" case, with the external ocean forcing the main suspect.

L242 – "desensitized" perhaps?

L261 – MICI not mentioned.

L268 – You mean Fig. 6c, maybe?

L294 – "Previously" not needed, in my opinion. Sounds to me like a past publication.

L377 – "inverted for the fraction of time ice covered". Feels like something is missing here.

L378 – What time period do these data cover? Are you comparing it to the model CDFs computed for the same period?

L392 – That's confusing. Then against what were the models calibrated?

L400 – "differences", thus "result"

L403-404 – Bold statement. Difficult to assess without a clear quantification of the isolated impacts of a highly uncertain ocean forcing. Even if that were the case, then the desensitised simulation would need to be further evaluated against several other constraints (e.g. [7]), since it was "not benchmarked against any geological or glaciological data" [8].

L407, L409 – Consistency using "data set" vs "dataset". Here and elsewhere.

L417 – "Matlab code" sounds too specific, what about "approach" or "algorithm"?

L421 – "WAIS at this site" -> "this site", maybe?

L421-422 – I would say "and has, in this case, no resolving power". There is always the chance that in the future, a model will be able to better resolve this area, and perhaps generate distinct CDF's. Same with L429.

L445 – If I understand the method correctly, you should choose an integration time matching the field data temporal span, or?

L453 – Although (to my surprise) it exists, "resolvably" does not seem to fit the context here.

L472 – "Collection" is singular, thus "is", perhaps?

L509/510 – Not sure that the data are used to differentiate between the two models, as the models can be separated without the data. Maybe "evaluate the performance"?

L522-526 – I think that the strength of these claims needs to be checked against my concerns above. In any case, and before such a hypothetical strong implication (L524) could be trusted, a significant amount of validation would be needed. Considerably higher resolution models, with the ability to zoom in around outcrop areas and correctly approximate the dynamics of ice in high complexity topographies, would be first. A way more realistic external forcing would be needed as well. And then, validation of the results against a myriad of other geological constraints and modern observations would be required, especially if the validation of the model has the ultimate goal of enabling a simulator of future ice sheet dynamics.

L546, L548: links seem unnecessarily complex: https://doi.org/https://doi.org/

Figure 1 – This figure can be much, much improved. As a minimum, I would expect a way to quantitatively interpret the contours and colormap, including the name of the plotted quantity (ice surface elevation I assume). Maybe a consequence of the colormap choice, but where are the ice shelves extents and their thickness in subplot a (and presumably b and c)? Modelled boundaries (grounding line and calving front) would help too. Adding the present-day observed grounding line and calving fronts in a distinctive manner, to allow for a quick evaluation of the model's ability to reproduce the present state of the ice sheet, would be useful to the reader as well. You can optionally take the opportunity to add some general Antarctic locations here as referenced in the paper (for the reader's convenience), as precise locations are introduced only starting on Fig 7, central panel. By the way, the caption of Fig 4 mentions that the location of Pirrit Hills is shown in "Fig. 7e" (instead of central panel), which is confusing as it seems that you are using the lowercase letters to reference both locations on a map and subplots (with the latter being wrongly referenced anyway). In any case, I'd rather have it replaced by a figure showing fata from the simulations carried out in study (see my general comment 2B and specific comment L189-190).

Figure 2 – Those bars are grey, in my opinion. That rectangle with the arrow is black, but I am not sure about the need for it, or its accuracy: the subs d and e do not seem to correspond. Figure needs more love: It seems to me that you forgot to correctly white-rectangle-cover the "5"s in the horizontal axis of subfigure b. "Scaled" is not capitalised in subfigures a and b; same with "time" and other words. Please make the capitalisation consistent across all figures. Style of "(f,g)" not consistent, recommend using "Desensitized … (f), while sensitized … (g)". Subfigures f and g are counterintuitive: the linear response shows actually more retreat than the nonlinear one, which contradicts the rest of the paper. What process would cause something like that? Is the figure made up or from the simulations? Subs i and j would benefit from using the same colour scheme, preferably colour-blind friendly, to show corresponding surface levels.

Figure 3. What are the dates in d and e? I thought the model runs between 5 Ma and present. Are these from the current study? What is the y-axis in b and c? Some figure details need a minimal explanation in the captions, otherwise they assume the reader knows already those details.

Figure 4 – Photo credits? You might get some copyright warnings later.

Figure 6 – Why is D so big over the continental shelf and beyond? Does the ice reach the model domain boundaries?

Figure 7 – Why the (l) only for Byrd in the caption?

References:

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