

Reviewer Responses

September 20, 2019

1 General Response

Our thanks to Stephen Price and Lev Tarasov for providing insightful feedback and suggestions, which have helped to improve this manuscript. Here we outline the major changes to the manuscript.

- Based on reviewer feedback, we now account for isostatic uplift in the model, and estimate basal traction in a more rigorous way by reducing the misfit between modeled and observed modern surface velocities. We are currently in the process of re-running all inversions to reflect these changes, but have so far only finished inversions using the Buizert et al. (2018) temperature reconstructions. Results are shown in Figure 1. These modeling updates resulted in only very minor changes in the estimated precipitation anomaly, and we do expect much change in the Dahl-Jensen et al. (1998) inversions either.
- We altered a number passages in the abstract and introduction drawing parallels between HTM and modern climate. This is an interesting discussion point, but we agree it should not be a central premise of the manuscript due to the massive uncertainties in both past and future climate.
- We provide some additional discussion / justification regarding our use of the Clausius-Clapeyron relation (Equation 4) linking temperature and precipitation.
- There is a new figure illustrating the unscented transform for a simple 2D function.
- The text has been edited throughout for clarity.

RC1 : Stephen Price

Comment

2,7: It would be useful to expand on the end of the sentence "... an increase in Arctic precipitation", and include some of the mechanisms by which that might occur (e.g, atmosphere able to carry more water vapor, more water vapor available due to uncovered and warmer oceans, etc). This could help the reader to have a better understanding for what processes you are trying to account for with your general precip-temperature scaling vs. what you are assuming might fall into the precip. anomaly value.

Response

Good suggestion. We expanded this paragraph in the introduction. The projected increase in Arctic precipitation is due to the increasing moisture content of the global atmosphere, as well as changes in atmospheric circulation caused by reduced Arctic sea ice (Bintanja and Selten, 2014).

Comment

“... a means of assessing the ...”. Maybe better to say, a means of assessing how the ice sheet will respond to warmer (and possibly) wetter future climate. The issue of overall ice sheet “stability” seems a bit further removed from this particular topic.

Response

We agree, and have altered the language here.

Comment

2, 11-12: Do you mean specifically during the HTM? It would be helpful to attach a time span to this statement.

Response

Yes, this refers to the Holocene in general, not the HTM in particular. We clarified this in the text.

Comment

2,14: “... due to the presences of more surface water ...”. This seems to imply a connection to ice sheet dynamics (via lubrication and sliding). Or, do you simply mean increased melting as a proxy for increased mass loss?

Response

Since we cannot address how increased surface melt impacts ice dynamics, we have tried to clarify this point in the manuscript. We can really only address if increased mass loss due to melt might have been offset by greater accumulation, assuming constant ice dynamics.

Comment

(last sentence: You might capture the attention of a few more interested readers here if you include some estimate for the order of magnitude reduction in the no. of fwd model evaluations required (e.g., is 10x less? 100x less?).

Response

The iterative optimization procedure requires around 120 total samples, which can be efficiently parallelized so that the total computation time is equivalent to three successive forward model runs. Assuming around 5000 MCMC samples, perhaps more with burn-in, the UT requires around 40x fewer model evaluations. However, MCMC would potentially take more than 40x longer since MCMC is difficult to parallelize. We added a bit more detail in Section 4.1.1.

Comment

General comment on the increased precipitation / decreased rate of margin migration hypothesis. It is not discussed whether or not the increased precipitation is assumed to fall as rain or snow. Presumably both could occur, in which case it seems like the precipitation anomaly could have either negative or positive feedbacks w.r.t. the base-rate of margin retreat. That is, precipitation as snow would favor increased SMB, a brake on margin retreat from melting. But, increased precipitation as rain (which is possible at lower elevations) could actually lead to increased marginal melting (or due to changes in ice dynamics). This may not apply during the HTM, e.g. if temperatures are always \geq freezing all the way down to sea level. But this could be something to consider in terms of the analogy for how the mechanism might operate under future warming.

Response

This is a good discussion point, but difficult to address from a modeling perspective. Precipitation can fall as rain or snow, but we do not model runoff or subglacial hydrology, which means we cannot account for potential changes in ice dynamics. We have added a new section on model limitations addressing this point. We will also take Lev Tarasov's suggestion of plotting the fraction of liquid v. solid precipitation, as the amount of liquid precipitation is a small fraction of the total amount.

Comment

3,2-3: Minor word choice detail, but maybe better to note as " τ_b represents the basal shear stress, β^2 represents the ...", etc. Also, formally, I believe that basal "traction" has a definition - its the basal stress tensor (not just the basal shear stress, unless you are only using that component of the tensor) dotted with the normal vector at the bed, which gives the basal "traction" components (that is, components of the traction vector). Here β^2 would be more appropriately listed as the "basal traction parameter".

Response

Thanks, this has been corrected.

Comment

This seems like a complicated way of just saying that effective pressure is a function of (scaled to) the ice thickness along flow

Response

This is correct. Equation (2) was included because it helped dispel confusion for some readers. We did a bit of shuffling and rewording in an attempt to make this less awkward.

Comment

3,6: "Basal traction" as noted above. The traction parameter what is being tuned here.

Response

Thanks, this has been fixed.

Comment

3,5-6: “roughly match” - This could benefit from something a bit more quantitative (e.g. provide an RMS or something similar).

Response

As suggested, we now estimate the basal traction parameter in a more rigorous way by minimizing the root mean square (RMS) error between observed and modeled surface velocities for modern Isunnguata Sermia. This resulted in a basal traction parameter of $\beta^2 = 1.2 \times 10^{-3}$ versus $\beta^2 = 1.6 \times 10^{-3}$. We are presently rerunning all inversions using the updated value, although it does not significantly affect the results.

Comment

3,11-12: This is a bit unclear. You are creating an annual climatology composed of monthly averages?

Response

Correct, we have edited the text for clarity.

Comment

3,19: “We take λ_p ...”. Is there a basis for this choice (e.g., a references) or is it arbitrary?

Response

This value is based on the well known Clausius-Claperyon relationship, and is commonly used in continental scale paleo ice sheet modeling studies such as (Ritz et al., 2001; Abe-Ouchi et al., 2007). We now include some additional discussion about this scaling relation.

Comment

5,16: replace “... using a fixed ... ” with “using methods discussed in Section 2.5”. This could address the redundant text comment below. 5,17: Clarify: Do you mean “a steady rate of retreat in order to give 5 km of total retreat over 1000 yrs.” (?). I think this is just worded oddly (e.g., 5 km is not a retreat rate). 5,17-18: “... using the methods ...”. This is redundant with material in the prev. paragraph. 5,18-19: “This initialization procedure is intended ...”. This is a little confusing as we don't really know what the initialization procedure is yet (its discussed in the next section).

Response

Comment

6,4: Most readers will be curious about the name, “unscented transform”. Might be worth a footnote?

Response

It turns out not to have any technical or mathematical meaning, but it is a wacky enough name to warrant a footnote!

Comment

6, eqn. 6: \mathbf{x}_0 is a scalar (the initial value of the vector x_i)? Or, is it a vector (because its bold)? Further below, it looks like you define this as a mean.

Response

Correct, \mathbf{x}_0 is a vector mean. There was a typo here. The mean vector was defined as $\bar{\mathbf{x}}$ above, but was accidentally switched to \mathbf{x}_0 in Equation (6). This has been corrected.

Comment

Section 2.5.1: It would be helpful if you could supply a figure, e.g. in an appendix, with a simple demonstration of what the unscented transform does for some simple (e.g. 2d) function.

Response

Another good suggestion, We added a new figure with a simple 2D example of the UT.

Comment

9,17: “We randomly sample curves ...”. Be explicit about what exactly curves is referring to here.

Response

We generate candidate retreat histories by drawing random samples from a multivariate Gaussian distribution with a GMRF covariance matrix. These randomly sampled vectors contain the glacier length at discrete points times. The GMRF covariance enforces smoothness, so that the retreat histories are not noisy. We have included additional detail in Section 2.5.4.

Comment

9,23-24: “Using a low temporal resolution ...”. Is the number of forward model evaluations required equal to the number of points in the $\Delta \mathbf{P}$ vector?

Response

Yes, exactly. The number of model evaluations is proportional to the number of points in the $\Delta \mathbf{P}$ vector. We now point this out out in section 2.5.5.

Comment

10,9-10: “The data assimilation procedure can be modified to account for ...”. Precede this sentence with one explaining your interest or need for doing this. I.e., make it clear that you also want to try to account for model uncertainty, not just observational uncertainty

Response

Thanks, we added an extra sentence noting that we would like to account for both model and observational uncertainty.

Comment

10, eqn. 23: Is the vector missing a comma to separate items within it? The same question applies to some vector terms included in lines 18 on this same page.

Response

Right, this notation represents a concatenated vector. We changed this to comma separated values. —

Comment

11, 14: As currently written, its confusing that in this section the Buizert temperature history is introduced and discussed first, and then only after is the Dahl-Jensen temperature history mentioned. It reads a bit as if there was an initial choice made to use Buizert, but then when the results werent favorable, you tried something else. This impression could be avoided by simply introducing the temperature history as yet another source of uncertainty that you are investigating here (albeit in a non-statistically robust way, since theres only a sample of 2). Note that consistently addressing this problem might require some minor changes to all of the previous sections. That is, while the Buizert history is mentioned multiple times before the results section, the first time we see / hear anything about the Dahl-Jensen history is in the results section.

Response

Another good point. We now introduce the Dahl-Jensen et al. (1998) temperature reconstruction earlier in Section 2.3. Ultimately, using different temperature reconstructions is a non-rigorous way of testing sensitivity to temperature.

Comment

11,12-13: There should be some discussion regarding why the Holocene precip. anomaly using the Buizert temperature reconstruction is in general, high prior to 10 ka BP (and generally falling thereafter up until 8.5 ka BP).

Response

The large precipitation anomaly in the Buizert et al. (2018) reconstruction is likely related to the dependence of precipitation on temperature. Precipitation is low prior to 10 ka BP due to low temperatures. In order to match the observed retreat chronology, considerably more precip. / accumulation is needed than is predicted by Equation (4). We added this as an additional discussion point.

Comment

11,24-25: Here, presumably you mean explicitly that you see this relationship IF using the Dahl-Jensen temperature history? Again, this feels a bit contrived as all of the material regarding the temperature history up to this point is w.r.t. the Buizert temperature reconstruction alone.

Response

Yes, this applies to the Dahl-Jensen temperature history.

Comment

12,25-26: “We find that ...” I think this requires additional clarification. That is, specify that precipitation during the HTM had to be higher (positive anomalies) relative to the precipitation values one would estimate based on standard air temperature vs. precipitation scalings.

Response

Thanks, we tried to clarify the wording here.

Comment

12, 31: “Due to high snowfall in the early Holocene ...”. It would help to add some additional discussion for why exactly this is the case, and why it also doesn't occur when using the Dahl-Jensen temperature reconstruction. This is confusing as the ΔT values prior to 9 ka BP appear similar in the two temperature records.

Response

Early Holocene warming is somewhat more gradual Dahl-Jensen et al. (1998) than Buizert et al. (2018) (Figure 1), leading to lower estimated precipitation anomalies. We have added this point in the discussion, and will also plot both temperature reconstructions on the same axes for easy comparison.

Comment

13,3-4: Another way to state this (or, a conclusion that is also relevant to state) is that multiple temperature and precipitation histories are consistent with the inferred history of margin position. This is not necessarily a surprising outcome from using an inverse method.

Response

This is true, there is no one unique solution.

Comment

13,29-30: Can you say anything about if / how these methods would work for higher dimensional parameter spaces? It's not entirely clear to me how many params. were optimized with uncertainty here. 14,1: for some problems Elaborate on which types of problems. You mean it has advantages for small param. space problems? 14,9-11: Again, it would be nice to be explicit (if you can) about where you think the dividing line is between problem (parameter) sizes that are or are not tractable with this method

Response

The unscented transform is probably most useful when the state dimension is of small to moderate size – in the realm of tens to hundreds of parameters. The advantages of the UT for smaller problems are related to the reduced number of model evaluations, as well as the ease of parallel implementation. In our case we optimize for around 40 parameters. We now mention this in the text.

Comment

14,26-29: An alternate or additional point here might be that if you were to include temperature as an additional random variable, you might be able to use it to better determine which, if either, of the temperature histories is consistent with the margin retreat history.

Response

This is a good suggestion, and we would potentially like to include a temperature inversion in the appendix. In past iterations of this work, we inverted for temperature rather than precipitation, and it would make sense to show an alternative way that one can obtain the same retreat history.

Comment

The two temperature records here are inferred in different ways and also apply to different regions (one at the ice divide, and one for all of Greenland). Do you expect that a regionally-focused temperature history (e.g. for West central Greenland) might be needed to increase confidence in the results (since they are quite different)? Do you expect the the two records here maybe at least bracket the possible values of ΔP ? The central question posed at the beginning of the paper is left hanging a bit. Does this work argue for a strong negative feedback operating to slow the retreat of (terrestrial margins of) the Greenland ice sheet as Arctic climate continues to warm? If so, then you should say so. Right now, your results sort of leave the door open to interpreting this work either way, which could be dangerous (in terms of letting someone else interpret your results however they see fit). If you think the results are unequivocal in that sense, then it might be good to be explicit about that too.

Response

Regionally focused temperature reconstructions would help decrease the uncertainty in estimated precipitation anomalies. It is possible that the two ΔP reconstructions bracket the possible values of ΔP , but this is difficult to say without formally including temperature as a random variable with its own associated uncertainty. Regardless, we believe that the results support a link between increasing temperature and increasing accumulation during the HTM. Even given uncertainties in temperature, positive precipitation anomalies are needed during the HTM to match the observed retreat histories. We expanded the discussion related to temperature uncertainty.

Comment

Figure 2: Are blue regions intended to identify areas below sea level and in contact with the ocean or just the former? The way this is labeled, “bedrock elevation and sea level change”, is a bit confusing. I can see someone interpreting the blue areas behind the terminus in the 0 ka BP plot as being connected to the ocean, when I think you are really just indicating that they are below sea level. This makes it a bit unclear if you are changing sea level in your

simulations or not (i.e., if that is part of the forcing or if you are always treating this as a terrestrial margin).

Response

Yes blue areas simply indicate regions that were below sea level. We removed this figure, and instead include isostatic uplift and relative sea level changes in the model as outlined in Section 2.1. _____

Comment

Figure 4: This figure caption would benefit from some additional clarification as to what is being shown. These are just 4 samples of glacier length history for four possibly different ΔP histories that make up the larger distribution, correct? It took me a while to figure this out. You could also be explicit about which set of lines goes with which vert axis (left or right). Also, you might consider using a diff. set of colors (or simply diff. line types?) as it is tempting to try to connect these colored lines with the margin positions shown in earlier Figs (the no. of samples shown here and the colors are similar to the no. of margin positions and the colors).

Response

Thank you, yes, there are four different glacier length histories corresponding to four possible ΔP histories. We updated the caption to clarify.

Comment

Figure 5: As in the previous figure, make it clear that the colored lines here are just a few representative histories that are part of the part of the broader distribution.

Response

Fixed, thank you.

Comment

Figure 6: While it might make the plots look a bit odd, I think it would be better if panels c and d had the same vertical and horiz. axis limits, in order to make comparison between the two easier. Same goes for panels a and b. If nothing else, you could try to include a box in panel c that shows where the corresponding area of panel d is.

Response

Good suggestion, we will update this figure when all the inversions are finished.

Comment

Figure 9: "... which uses a fixed parameter set ..." Is this equivalent to assuming that the fwd model has no uncertainties associated with it (i.e., the model is perfect and only the obs have errors)?

Response

Yes, this assumes measurement uncertainty but no model uncertainty.

RC2 : Lev Tarasov

Comment

However on the science side, I find this paper adds little novel support for the key claims made in the abstract as currently stated: “Our results indicate that Holocene warming coincided with elevated precipitation. ... The importance of precipitation in controlling ice sheet extent during the Holocene underscores the importance of Arctic sea ice loss and 10 changing precipitation patterns on the future stability of the GrIS.” Anyone taking an undergraduate course in weather and climate should be able to infer that expansion of upwind open and warmer water conditions is likely to result in proximal increased precipitation.

Response

We agree. Simple reasoning would suggest that precipitation should increase during the HTM. However, this is somewhat complicated by the dynamic, regional changes in climate that occurred during the Holocene. The retreat chronology provides additional data that can be used to constrain the timing and magnitude of changes in Holocene precipitation, and we are specifically looking at changes in precipitation that are not explained solely by changes in temperature.

Comment

Furthermore the largest precipitation discrepancies (ΔP) occur prior to 8.2 ka, when substantial ice over Baffin Island and Labrador may have affected regional atmospheric circulation, calling into question the extent to which this could be a useful analogue for future regional changes.

Response

Connections between HTM and modern warming make an interesting discussion point, but we agree that this should not be a central point of the abstract or introduction due to massive uncertainties in past and future climate. We altered the language in introduction and abstract to reflect this.

Comment

The two temperature records here are inferred in different ways and also apply to different regions (one at the ice divide, and one for all of Greenland). Do you expect that a regionally-focused temperature history (e.g. for West central Greenland) might be needed to increase confidence in the results (since they are quite different)? Do you expect the the two records here maybe at least bracket the possible values of ΔP ? The central question posed at the beginning of the paper is left hanging a bit. Does this work argue for a strong negative feedback operating to slow the retreat of (terrestrial margins of) the Greenland ice sheet as Arctic climate continues to warm? If so, then you should say so. Right now, your results sort of leave the door open to interpreting this work either way, which could be dangerous (in terms of letting someone else interpret your results however they see fit). If you think the results are unequivocal in that sense, then it might be good to be explicit about that too.

Comment

Looking at figure 6, it appears that the strongest precipitation correction (ΔP , prior to 10 ka) could be largely required as an offset to the default temperature dependence (though a definite assessment would need provision of actual regional precip and not just ΔP).

Response

This is a good point, and we now mention this in the discussion. The large precipitation correction prior to 10 ka BP is likely due to the dependence of precipitation on temperature. Baseline precipitation is low during this period due to cold temperatures, and large precipitation offset is predicted to match the observed retreat history.

Comment

Im also troubled by the simplistic precipitation parametrization for temperature dependence (eq 4). I can see it being defended as it matches what continental scale models have tended to use to date. But the context is different. This is clearly a maritime environment and precipitation will have strong dependence on upwind surface marine conditions. Based on my own modelling, I also find that the 0.03:0.11 2 sigma range is too small (at least on the lower end).

Response

We agree that Equation (4) is simplistic. It does not account for dynamic, regional changes in atmospheric circulation that influenced precipitation during the HTM. For this reason, we did not necessarily anticipate that it would yield a good match between the modeled and observed retreat chronologies. However, given the uncertainties associated with paleo climate reconstructions, we are at a loss as to how to improve this relation. The precipitation anomaly ΔP is intended to account many unknown climate factors that are not captured in by Equation (4). We now discuss this at greater length in Section 2.3.

Comment

I also find that the 0.03:0.11 2 sigma range is too small (at least on the lower end).

Response

Thanks, we will adjust this value when re-running the sensitivity tests with model updates, though we do not anticipate it will significantly impact results.

Comment

And yet here you do not take into account the latter effect. You should at least show separate solid/liquid fraction precip time series.

Response

This is a fair point, and we have decided to include a short section (2.6) on model limitation. The model does not account for runoff or subglacial hydrology. Modeling subglacial hydrology would add significant complexity to the model, and we therefore assume that water pressure is a fixed fraction of overburden pressure. Modeling runoff in a flowline model would be difficult, as there would be considerable flux in and out of the flowline. Displaying both solid and liquid fractions of the precipitation is a good suggestion.

Comment

“1D, isothermal” why isothermal? 1D models are cheap, and given the rheological dependence on ice temperature, along with basal sliding dependence on proximity to the pressure melting point, some justification is required for going with an isothermal model.

Response

There are a few reasons for this choice. Unless temperature is treated in a vertically averaged sense, resolving the vertical temperature profile requires a 2D mesh, which would increase the computational cost of the model. Sensitivity tests suggest that this complexity may not be necessary because the precipitation anomaly is not particularly sensitive to the rate factor and consequently to ice temperature. Modeled retreat is controlled primarily by surface mass balance rather than ice dynamics. These points are now included in the section on model limitations.

Comment

“Basal water pressure is assumed to be a fixed fraction $P_{\text{frac}} = 0.85$ of the ice overburden pressure” justification?

Response

Thanks, there was a missing reference here. This value comes from the median seasonal water pressure of several boreholes reported in Wright et al. (2016).

Comment

“ 5°C km^{-1} is the lapse rate.” justification for this value?

Response

Parameter values for the positive degree day model were selected to be consistent with previous paleo-modeling efforts. The lapse rate is from Abe-Ouchi et al. (2007), and we will add a reference.

Comment

“We take $p = 0.07$, which corresponds to a doubling of precipitation for every 1°C increase” 10°C increase. And why this choice? (ie can refer to later sensitivity calibration).

Response

Thanks, there was a typo here. Precipitation doubles for every 10°C increase, or increases 7% increase for every 1°C increase. Equation (4) is called the Clausius-Clapeyron relation, and the default value of λ_p is used in many other paleo ice sheet modeling studies. We added appropriate citations in the text. As you point out, sensitivity to this parameter is explored in Section 3.5.

Comment

SMB doesn't take into account impact of rain.

Response

This is correct. We do not model runoff or refreezing rain, only refreezing of melt as superimposed ice.

Comment

what dating methodology has ± 0.1 kyr uncertainty that far back in time? Explicitly indicate uncertainties.

Response

Good idea, we will report uncertainties. Dates are based on cosmogenic nuclide dating techniques, which have errors on the order of plus or minus a few hundred years (e.g. Lesnek and Briner (2018)).

Comment

Evidence suggests the terminus was terrestrial during this period in spite of changes in sea level and bedrock elevation due to isostatic uplift. As such, isostatic effects are neglected in the model.” Please briefly provide a summary of the evidence. And even if terrestrial, there could still have been significant changes in bed elevation and therefore ice surface slope, which would strongly affect ice flow. So I find this decision to ignore isostatic uplift problematic.

Response

This is supported by the reconstruction of relative sea level from Caron et al. (2018) as well as the retreat chronology (Figure 2). Isostatic uplift is now modeled.

Comment

“ $[\Delta \mathbf{p} \ \ell]^T$ as a multivariate Gaussian distribution” I have no idea what this means. Transpose of some kind of product of two vectors? farther down I see you mean the concatenated vectors. State so.

Response

Yes, thanks, this is supposed to be a concatenated vector. We changed the notation to comma separated values, which is hopefully more clear.

Comment

“Moreover, unlike in Kalman smoothing, we approximate the full posterior distribution rather than the probability distributions” I understand smoothing to be the joint determination of the full history (eg Muller and Storch, 2004) so am confused by this statement.

Response

Kalman smoothing estimates the conditional probability distributions $P(\Delta p_i | \mathbf{l})$. That is, in Kalman smoothing the other variables Δp_k with $i \neq k$ are marginalized out of the posterior distribution.

Comment

“We randomly sample curves from a multivariate Gaussian with the same covariance structure as the GMRF prior outlined in Section 2.5.3” justification as to why the GMRF provides an appropriate distribution for this context?

As for ΔP , the GMRF prior is a “smoothness” prior. We have edited the text for clarity.

Comment

what unit is ΔP in?

Response

We now state this explicitly – it is in m.w.e a^{-1} .

Comment

“is not a fullfledged substitute for MCMC methods, which can compute 30 expectation integrals to higher levels of accuracy” And which do not need to assume a Gaussian distribution

Response

The mean and covariance estimates obtained via the unscented transform can be accurate even if the underlying posterior distribution is not normal. Of course, the mean and covariance may not fully characterize the distribution (e.g. if it is multimodal). Perhaps we could include a normality test to address this?

Comment

”In practice, however, the number of function evaluations needed for MCMC methods makes them intractable for some problems” Not if appropriate emulators are available.

Response

Surrogate models, or emulators, offer another means of estimating a non-Gaussian distribution for functions that are costly to evaluate. We find the UT to be effective in our case because we only require around 40 model runs per iteration of the optimization procedure, which can be conducted simultaneously in parallel. Hence a full inversion takes the same amount of time as 3 serial model runs. Implementation of the UT is also trivial. We added a short discussion of surrogate modeling. On a personal note, I am also very much interested in comparing the UT to a surrogate modeling approach.

Comment

According to Bintanja and Selten (2014), declining sea ice may cause an increase in net accumulation over areas of Arctic land ice.” Again this statement could be made by climate system reasoning, without need for recourse to citation. ”net accumulation” is also unclear. I would take ”net accumulation” to mean net surface mass balance, in which case this statement has no support. If you just mean total accumulation, then just state so.

Response

This reference simply provides a stronger support for that line of reasoning using global climate model output. We changed “net accumulation” to “total accumulation” for clarity.

Comment

Table 1 please also show the parameter values you obtain from the Sensitivity testing inversion (section 3.5), both mode and 2 sigma bounds

Response

Good suggestion. We will include these values after re-running the sensitivity tests with the updated basal traction parameter and isostatic uplift.

References

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