Author responses for tc-2021-394, "A probabilistic framework for quantifying the role of anthropogenic climate change in marine-terminating glacier retreats"

KEY

Reviewer comments

Our responses

New or adjusted text

Reviewer 2

Summary

The overarching goal of this study is to provide a framework to attribute glacier retreat to anthropogenic climate change. The authors seek to do this by performing ensemble simulations of glacier retreat in idealized geometries driven by quasi-random climate variability. Overall, I think the study is very well written and illustrated. The figures were easy to read and interpret and the text provided sufficient motivation and narrative structure to follow the thread. I have some overarching comments, some boring if highly technical comments and some minor comments about wording, but overall my comments are minor.

Thanks very much for the thoughtful and encouraging review. We especially appreciate the overarching comments that get at the fundamental questions that attribution assessments must wrestle with, and it has been a useful process to consider them. Please find our responses below:

Overarching comments.

One of the results of the manuscript is that random climate fluctuations will eventually cause glaciers to retreat. The time it takes to do so depends on the magnitude of the imposed noise. Of course if glaciers had experienced a stationary stochastic climate in the past this implies glaciers should all be in their retreated position(s) and we wouldn't observe any glaciers in their more advanced positions. This isn't really a problem for the study because the climate, has not been stationary stochastic and has variability on a range of time scales. This raises two questions.

Before responding to the two questions below, we also want to make a general comment on the conundrum as to why any glaciers would be found in advanced positions (i.e., still on peaks), as it is indeed a puzzling implication. Your point about the timescales over which climate is stationary is a good one, and other slow processes might also play in on long timescales, such as sedimentation and isostatic effects (as we note in section 6.2). An additional consideration is that since subglacial/proglacial topography tends to have many bumps, it's possible that glaciers could have previously retreated from different topographic highs and wound up on others. The attribution question is just going to be framed around retreat from whatever the glacier's pre-retreat position was. Admittedly, this is obscured by our single-peak idealized geometry, but we can see this in the simulations with random bed topography. The initialization period includes multiple peak-to-peak retreats for many glaciers, but the attribution experiment is focused on where they happen to be after 5000 years. While it's not meant to be an actual reconstruction of the last 5000 years, hopefully it adds some insight to the problem.

1. As the authors point out, modern glaciers are responding to both past and present climate forcing. We know that glaciers have advanced during colder periods. For example, some glaciers may have advanced during the Little Ice Age and then retreated and, depending on glacier size, glacier response to the Little Ice Age and other climate anomalies would overlap with the anthropogenic climate interval. However, the model clearly shows rapid retreat with little advance. This raises the question of whether the model (or perhaps geometry?) is capable of simulating prior glacier advance. If advance is not possible, then it seems possible that the model is overestimating the probability of glacier retreat in response to climate forcing (?), potentially biasing the statistical inference. To put this another way, in the authors model the glaciers will eventually retreat irrespective of anthropogenic climate forcing and the only thing that warming does is increase the probability that this occurs sooner. In this scenario, anthropogenic climate change only affects rates and not states (i.e., the time of retreat can be accelerated by warming, but retreat is ultimately going to happen irrespective). It would be more satisfying intellectually if the authors could turn around and also attribute glacier advance to periods when the climate was colder, like we have observed in the historical and paleo-records.

First, on the question of advance: For the single-peak simulations, the fact that we only see rapid retreats and not advances is a consequence focusing specifically on a glacier initially near a bed peak (combined with the hysteresis of marine-ice-sheet instability). So we'd agree that this reflects a bias towards retreat, but this is (or was) the state of many real glaciers prior to retreat. Ultimately this is the unique characteristic that we are trying to address with this framework - it's fundamentally conditioned on starting relatively near a peak. We chose the simple single-peak case as a clear way to explore this in the context of attribution, though we agree that it doesn't address how the glacier got there and this is also an important question. Indeed, we note this in the discussion section on initial conditions (last paragraph of 6.2).

The model can indeed exhibit unstable advance on retrograde slopes, though this situation would require a mean state near the bottom of a trough (for variability to trigger advance), or a nonstationary climate forcing to push it into such a region (as you suggest). The single-peak geometry isn't ideal for illustrating this, but we can explore this with our group of random bed geometries. Below, we show a group of glaciers on different bumpy topographies subject to a positive step change in surface mass balance (and the usual random frontal ablation variability). The glaciers do advance, and occasionally hit reverse slopes and undergo rapid unstable advance. We'll add this figure to the supplement.

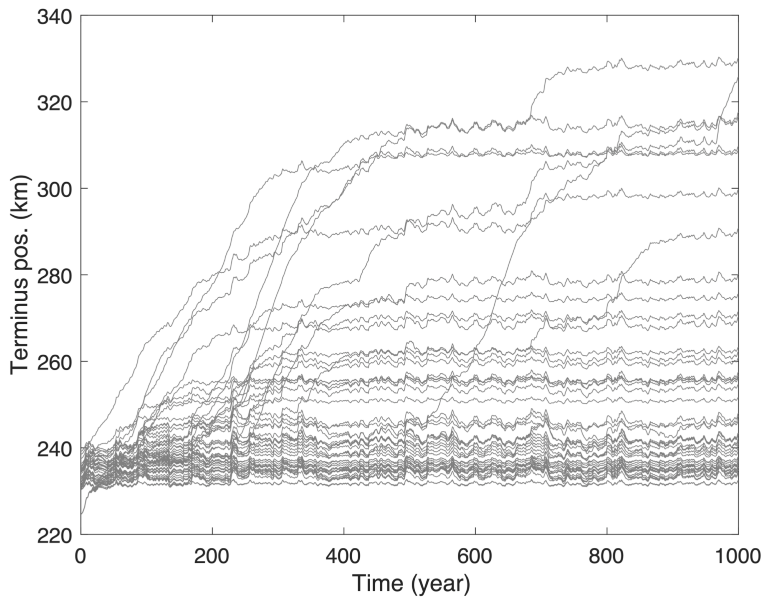


Figure SN: Response of 50 glaciers to a 30% increase in surface mass balance (along with stationary variability in frontal ablation). Each glacier has unique and random bed topographies, as described in the main text. Termini advance through the bumpy topography, including some cases with large rapid advances when they encounter retrograde slopes.

We also agree with the assessment that a non-zero probability of retreat without a trend implies that retreat would eventually occur at some point. Focusing on changes in the rate (or probability per time) is intentional - we think this is a useful way to think about observed retreats whent threshold processes are at play, since the magnitude of response after breaching thresholds (at least thus far) depends so strongly on non-climatic factors such as geometry. It can seem a bit odd that our counterfactual case implies inevitable retreat at some point, but we'd argue this implication is currently what is on the table, given the lack of attribution assessments for observed marine-terminating glacier retreats. The magnitude of the long-term response is of course a different situation under which to assess the role of anthropogenic forcing.

2. The authors break the probability distributions into a component related to (random) natural variability of the climate forcing and a component related to parameter uncertainty. This is fairly standard, at least in the glaciological literature and it follows from numerous studies in engineering. However, it makes a potentially large assumption: that we understand the system well enough that the model uncertainty largely derives from a handful of parameters that are imperfectly known. There is another possibility that also has to be considered which is that the underlying parameterizations are either not complete or fail in different climate scenarios. This seems especially relevant when dealing with submarine melt, iceberg calving, shear margin weakening, subglacial hydrology, etc, none of which are especially well understood. To be clear, my understanding is that the entire formalism presented here can be applied to any model irrespective of the models fidelity. For example, ca 2000 one could apply this same

method using Shallow Ice Approximation models that don't account for longitudinal stresses or marine ice sheet instability. These models would require much more oomph from the climate variability to drive retreat because they lack crucial physics. But the same formalism would allow "attribution". Hence, a crucial point made by Shepherd (2021) is that we also have to consider all the alternative hypotheses that could also account for the observations. Shepherd (2021) described how to do this using Bayesian analysis through the use of the "complement". The trick is that one can formally include how much confidence we have in the model vs alternative models/explanations. I don't propose that the authors utilize this approach here, but I would like to make sure that they are aware of it and urge them to consider the possibility that their model might not be as physically robust as one might assume from the discussion in the text. I will note that this is gently hinted at near line 215, but it does seem important to emphasize that the attribution is very sensitive to model assumptions and ultimately, this effect needs to be quantified.

These are great insights and we agree wholeheartedly that attribution is always conditioned on model assumptions, and the confidence therein will need to be assessed. Our analysis of the different trend scenarios follows in the same spirit: we will probably have to test over a range of assumptions about the anthropogenic climate forcing. We did try to emphasize that attribution is contingent on such assumptions, and they therefore need to be clearly stated (see last sentences in sections 6.1 and 6.3). That said, you raise a good point that this also extends to incomplete model physics, or other processes that could explain retreats. And, we appreciate being pointed to the Shepherd (2021) paper, which contains a number of points relevant to designing these experiments. We think the statements in 6.1 and 6.3 play a part in conveying this overall point, but we will also revise the "outlook" part of section 7 to round it out more broadly.

> *As discussed in the previous section, uncertainties in a glacier's preindustrial position and in the onset of anthropogenic forcing pose fundamental challenges for attribution,* ***as do uncertainties in key physical processes such as calving, submarine melt, and glacier sliding****. Despite these gaps, our view is that sufficient mechanistic understanding and observational constraints exist to motivate ensemble-based attribution assessments on well-observed glaciers.* ***In light of these uncertainties, it may be necessary to test over a range of plausible assumptions about glaciological processes and climate forcing that are consistent with observed retreats. An overall assessment would ideally combine results from this range according to our confidence in each assumption (e.g., Shepherd, 2021).***

Technical comments:

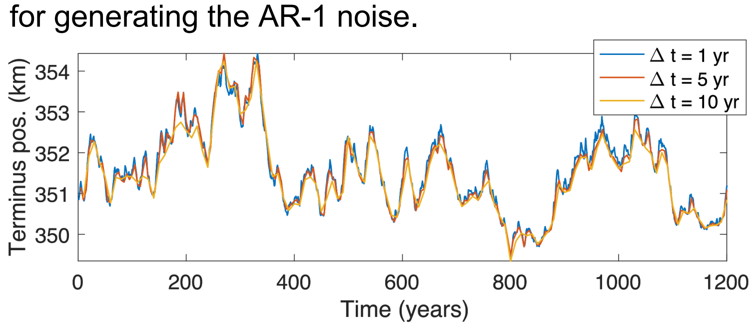
1. How do the authors define noise and what does it mean for the forcing to be ``random''? My understanding is that the authors assume mass balance has a secular component with zero-mean fluctuations super-imposed. But I'm not entirely sure how the fluctuations are defined and

I would encourage the authors to add additional details and equations about how the noise is created in the supplementary materials. More concretely, I take it that noise is added to the surface mass balance? For a zero-mean Gaussian process, the noise is not smooth and differentiable, so we would then need to integrate it in the form:
$h(t+\Delta t) = h(t) + \Delta t (f(t)+S(t)) + \sqrt(\Delta t) \sigma(t,h)$
where $\sigma(t,h)$ is the standard deviation of the Gaussian process and S(t) is the secular component and I have defined f(t) as the divergence term in the mass balance (or other terms in the equation). Note that the random noise term is multiplied by the square root of the time step in a Brownian process. There is a literature on integrating stochastic differential equations using colored (as opposed to white) noise, but that far exceeds my mathematical acumen. It would be helpful to me to see more details summarizing how the noise is created and how the stochastic differential equation is then integrated along with demonstrations of numerical convergence using both varying time step size and grid resolution. I don't request a host of convergence studies added to the paper, but a few sentences explaining that they were done and the results of the convergence experiences.

We generate random climate anomalies with the prescribed statistics outside of the model, and then read them in at each timestep, averaging as necessary for timesteps longer than the sampling interval of the timeseries (which should be consistent with your point about scaling the standard deviation by sqrt(Δt)). We'll clarify this procedure where the noise is introduced in section 2:

> *We generate* ***timeseries of annual climate anomalies (zero-mean)*** *using a Fourier transform method (see Percival et al., 2001; Roe and Baker, 2016; Christian et al., 2020). …* ***At each model time step, we add the corresponding anomaly to the mean SMB or frontal ablation term in the model's continuity equation, averaging over the time step if it is greater than 1 year. For most simulations, we use a model time step of 5 years; we found little effect on terminus fluctuations with time steps of 1--10 years, as the high frequencies of climate variability are strongly damped by the ice dynamics either way (supplemental fig. SN).***

And we will the following figure to the supplement showing the effect of a longer time steps, along with equations for generating the AR-1 noise.



2. The frontal ablation parameterization is intriguing, but I have some questions and comments about this. As I understand it, this approach involves applying a large, negative surface mass balance localized at the last grid point at the grounding line and labeling this a “flux” or frontal ablation term. This seems intuitive at first: the flux term is removing ice at the terminus. The large frontal ablation causes a surface slope between the last two grid points in the (discretized) model. That this is in fact a surface ablation parameter can be seen by moving the frontal ablation to the right hand side of the ice thickness equation. For example, writing the flux as q, an upwind finite difference has the form:

$h(x,t+\Delta t) = h(x,t) + \frac{q(x) - q(x-\Delta x)}{\Delta x}\Delta t + \frac{h \dot m}{\Delta x}\Delta t + S(t)\Delta t$

Note that the second to last term is the frontal ablation term. In the limit that Δx becomes small, the surface ablation term becomes large, leading to an increased effective surface mass balance at that point. I have messed around with this type of parameterization a lot in the past (e.g., Bassis et al., 2017) and could not convince it to converge numerically under grid refinement. Instead, this type of parameterization created an unphysical singularity in the slope/thickness of the glacier that became larger and larger as the grid spacing became finer and finer. To cure the singularity, I had to regularize the frontal ablation term, recognizing that the surface ablation needs to be spread out over a characteristic length scale (I used 1 ice thickness). Doing this cured the lack of convergence and provided more physical surface slopes when using small grid spacing. But the results will depend modestly on the regularization scheme. Because of my experience, I would recommend considering a numerical convergence study to assess if the results are independent of grid resolution, time stepping, etc. This is not to say that the authors scheme is problematic, but it would be reassuring to provide some additional tests. To be honest, the entire attribution framework would still work even if the model does depend on the grid spacing. It would just emphasize my previous point that a real attribution requires some estimate of our confidence in model physics and numerics.

Bassis, J., Petersen, S. & Mac Cathles, L. Heinrich events triggered by ocean forcing and modulated by isostatic adjustment.Nature 542, 332–334 (2017)

Thanks for bringing this issue to our attention - we hadn’t fully considered the dependence on grid size and are glad to be aware of it. We conducted some convergence tests, assessing both the steady state and transient responses. As you found, the slope right at the terminus becomes steeper and eventually the model fails to converge (our limit was around 30 m spacing). It also has an effect on the steady-state position, which would thus imply that the probability of retreat depends somewhat on the grid size. We note that it has no noticeable effect on terminus fluctuations other than a small DC shift corresponding to the change in steady-state position, but as we have shown for other parameters, this can indeed affect the probability of retreat.

Ultimately, we see this as a drawback of this particular frontal ablation implementation, which is in part related to the flotation condition (at least in our model). We agree with your assessment that it doesn't greatly affect the framework presented here (as long as the grid is consistent among simulations), but it is an important effect to note. We will add the following to the description of frontal ablation in section 2:

> *We note that because the frontal ablation is implemented at a single grid point, its effect on surface slopes and the steady-state terminus position depends on the grid size (Supplemental Fig. SN), and also creates numerical convergence issues for very fine grids (< 30 m). However, we use a consistent grid scheme whenever comparing simulations, to this does not affect our overall conclusions.*

And will note it alongside the results on parameter perturbations:

> *We note this may also include choices in numerical methods, such as the manner in which frontal ablation is prescribed (supplemental figure SN)*

Finally, we will present the sensitivity test in the supplement:

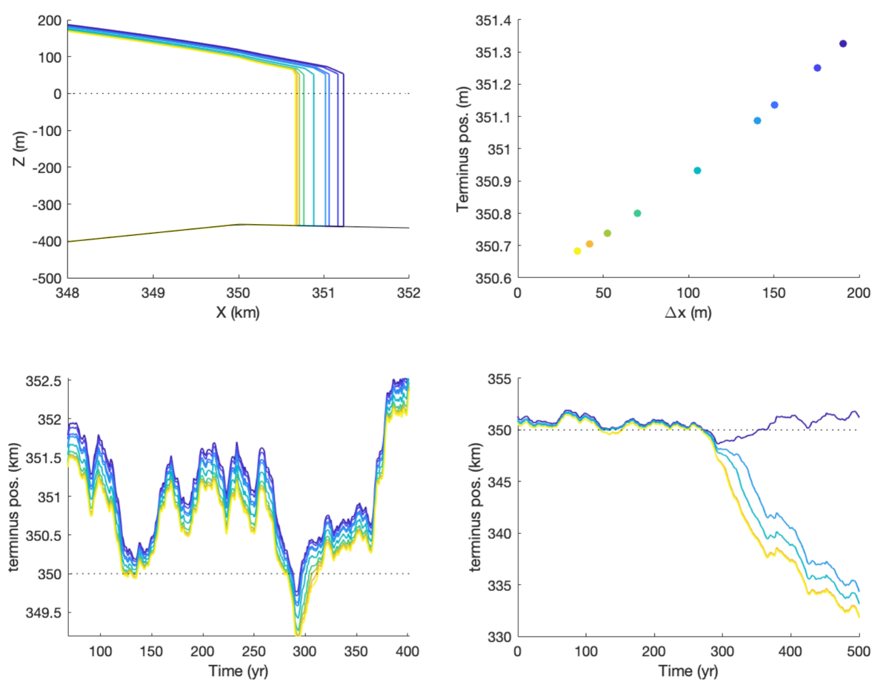


Figure SN. Convergence test for grid size. The frontal ablation term is applied at the last grid point, making its effect on local slopes dependent on grid size. This in turn affects the steady state terminus position. (a) shows steady state profiles near the terminus. (b) plots stead-state terminus against grid size. (c) shows Terminus fluctuations with each grid size. The character of stable terminus fluctuations is not very sensitive to grid size (except for the offset due to different steady-state positions). (d) shows fluctuations with a trend added, which causes retreat for some cases. For the simulation with the coarsest grid size, the offset in terminus position *(i.e., (a) and (b)) is enough to prevent retreat in this particular case. This indicates that under this frontal ablation scheme, the probability of retreat could be sensitive to grid size due to its effect on steady-state position, in the same way other parameter perturbations affect the probability of retreat (i.e., Fig. 3 in the main text).*

3. I think my biggest recommendation is that the authors conduct some numerical convergence studies. In my opinion, numerical convergence studies are like brushing your teeth: unglamorous, but essential hygiene that needs to be regularly performed to avoid unpleasant surprises. This is often done and then forgotten about. Please tell readers what you have done even if you don't show it.

Agreed - See responses above. In short, we have added a supplemental figure for the grid-size convergence issue, and also that the glacier fluctuations don't change appreciably when the model timestep is varied from 1 to 10 years.

Minor comments:

Line 275 and elsewhere: While—>Although. While is technically supposed to refer to time.
Fixed.

Introduction: Grounding line retreat in WAIS maybe related to the MISI (although also ocean forcing!), which is tied to retrograde bed slope. But there are other types of retreat. For example, the disintegration of ice shelves in the Antarctic Peninsula is not tied to bed slope (because the ice shelves are freely floating). Similarly, retreat of Petermann Ice Tongue is also not tied to bed slope. I think the discussion here is mainly focused on Greenland and grounded glaciers. This might be something worth emphasizing.

We'll clarify grounded ice early on (2nd paragraph):

> *In both Greenland and Antarctica, **the retreat of grounded ice** has been linked to….*

Bed topography in Figure 1 looks like it is piecewise continuous, but not differentiable. This can create numerical issues and problems with numerical convergence in models that assume the ice thickness is smooth and differentiable.

Correct, the idealized bed peak is mathematically sharp. This can indeed create issues with this model if the grid spacing is too coarse as the stretched grid aliases the topography as the grounding line varies. However, for our grid spacing on the order of 100 m (near the terminus), we haven't had any convergence issues even as the terminus retreats over the peak - likely because the bed slopes are relatively low even on the peak, so the vertical errors due to aliasing are only on the order of a few meters.

Line 135: Out of curiosity, why not use a one sided probability distribution (e.g, log-normal) that naturally avoids unphysical adding mass to the terminus?

This seems like a reasonable alternative approach. We had adapted an existing method to generate the Gaussian noise and later found it necessary to truncate negative values, but this would likely work just as well.

Figure 2 makes a key point: In a system that is close to a system with an instability, retreat always occurs and the only question is how long it takes for retreat to initiate. I don't know that there is strong evidence for this type of behaviors for glaciers. This might be partially because the climate is not stationary stochastic over this type of time scale.

See response to the overarching comment above, but in short, we agree that the timescale of stationarity is probably part of it, and there may well be other long-timescale processes to consider, such as changes in stability due to erosion, sedimentation, etc.. But ultimately we do see glaciers perched on bed peaks, or having retreated off of bed peaks, which motivates attribution analyses!

Equation (1) in the supplement: I think the exponent is (1/n)-1 and not 1/(n-1). Please check.
Thanks for catching this! Fixed.

Equation (4) appears to be missing an ice thickness on the right hand side? Please check.
Thanks! Fixed.