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 #1

review of "A probabilistic framework for quantifying the role of anthropogenic climate change in marine-terminating glacier retreats" by Christian et al.

This study considers the question of how to quantify the impact on glacier retreat of externally-forced climatic trends, in the context of internal climate variability. The study uses large ensembles of simulations of a simple glacier model to illustrate several points. (The members of each ensemble each having a different realisation of random climatic variability.) One conclusion is that the influence of a climatic trend on a single glacier geometry may be quantified by comparing ensembles of simulations with and without the trend. Another conclusion is that the influence of a climatic trend may be quantified by considering a population of glaciers with different geometries, all under a common forcing. These conclusions may also be stacked, considering a population of glacier geometries under a population of trends. The results illustrate a variety of philosophical and physical considerations that must be addressed in order to make statements about whether contemporary ice sheet retreat is linked to human behaviour.

My assessment is that this is a very nice study that deals elegantly with an important and very challenging problem. The paper is extremely lucid and thought-provoking throughout, and I feel it has taught me a lot. One could perhaps argue that the conclusions are not particularly surprising, but I think that is a very good thing – in my opinion all the best science leads the reader to an understanding that feels intuitive and ties everything together. In this paper, the authors have bought some of the key principles of climate-change attribution to glaciology, and I think the glaciology community could benefit hugely from taking these on board. Further, I feel that the strong nonlinearity of the glacier dynamics studied here means that this attribution work has features that are rarely found elsewhere in the climate change literature.

I offer a few comments below but I have no reservations about this paper. I think the authors should be allowed to consider my comments below and respond to them however they wish, including ignoring them entirely. I'm happy to read the responses but don't require this prior to publication.

Many thanks for this thoughtful and encouraging review. There are some great points raised below, and we learned a lot by addressing them - especially regarding the behavior of probability within the ensembles. Please find our responses below:

Comments (in order of appearance in the text)

Introduction: The introduction frequently mentions Antarctica, but after a while in the following methods section it became clear that the simulations here only explicitly deal with marine-terminating glaciers. The ice streams of main interest in Antarctica all have ice shelves. I feel the paper could be clearer on the extent to which the results apply to the case in which an ice shelf buttresses the glacier. Perhaps the authors can state at the very beginning of the paper that they don't study that case, but that the ideas are broadly applicable, and maybe return to that in the discussion.

We agree it's worth noting this early on. We've added the following text to the introduction:

> *It is worth noting here that our model simulations do not include floating ice shelves, which can alter the thresholds for dynamic instability via lateral buttressing (e.g., Gudmundsson et al 2012). We focus instead on cases where stability is a function of bed topography, which simplifies our analysis of glacier variability near instabilities. Accordingly, we orient model parameters and the discussion of results around marine-terminating glaciers in Greenland, where floating ice plays a lesser role than in Antarctica. However, inasmuch as strongly buttressed glaciers and ice streams are still subject to climate variability and may be prone to instabilities, many of the fundamental points for attribution could be adapted for such settings (albeit with additional ice-shelf dynamics to capture).*

Section 2: I was a little confused about what the authors mean by frontal ablation. To me, 'ablation' means melting. However, maybe this is meant to be a combined melting+calving rate? But then, the model does not represent any floating ice and the ice is calved wherever it goes afloat, at a rate equal to the flow speed. I assume the ablation rate is applied to the face after it has been calved at floatation. I guess the ice retreats happen because an increase in ablation displaces the terminus back a little, then the ice speeds up, thinning the ice, and causing further retreat of the floatation line. So, I am a bit confused. I assume I am just showing my inexperience here as this is a well-established model, but perhaps the authors can discuss this a little in the paper as I think the mechanism is relevant.

The frontal ablation is meant to be very general, and agnostic regarding melt or calving (we feel "ablation" is sufficiently general to include multiple mass loss processes). The sequence described above is more or less how we interpret it as well - we've added a bit more description of the process where frontal ablation is introduced in Section 2.1, and note that the main point is to introduce a time-varying ocean forcing.

We simulate ocean forcing very generally by adding a frontal ablation term to the mass-conservation equation at the grid point closest to the terminus (see Supplement for numerical implementation). ***This frontal ablation term amounts to an additional output flux beyond that dictated by the local ice velocity. Compared to the case without the frontal ablation term, this results in a retracted terminus with a steeper surface slope, such that ice flow balances the additional output flux.*** *For real glaciers, flux anomalies could be driven by variable calving or submarine melt, or a combination;* ***here we simply interpret these as flux anomalies at the terminus driven by variable ocean conditions. The most salient aspect is that frontal ablation is a localized flux term that we can force to vary in time.***

Section 3: In Figure 3a, why do 68 of the ‘natural control’ experiments retreat during the decorrelation period that precedes the time period shown in the figure (i.e. only 932 are left out of 1000 started)? Does this imply the period shown, in which no retreats occur, is not representative? I also note that the simulations in panel b are retreating right from the start of the period shown, so presumably these plotted retreats are just a continuation of the population of retreats that occured within that ensemble’s decorrelation period? Do these matters imply that the conclusions drawn in this section are dependent upon the decorrelation period chosen?

This comment raises several interesting and important points, and we have run a number of additional simulations to investigate these issues.

First: for the case of fig. 3a in particular, the 68 excluded did not exhibit sustained retreat during the decorrelation period, but had temporary excursions landward of the peak. So this is a consequence of estimating the probability of retreat only from those simulations with termini seaward of the peak at t=0. As we note, this is done to focus only on sustained retreats within the 150-yr interval, but it does have the consequence of eliminating some “stable” fluctuations, which is most evident in the special case of zero sustained retreats. One could set the threshold back from the peak, though in cases with unstable retreat, this would then admit some that started before the experimental interval. The timescales of glacier fluctuations and retreat make it unclear exactly when retreat becomes unstable, so any threshold will be somewhat arbitrary. We thought the clearest would be to base the analyses on the termini seaward of the bed peak at t=0.

As to the broad and important whether the probability varies with time: We ran several long (2000 year) ensemble simulations to better understand this, and made several observations that motivate some changes to the text and figures.

Primarily, we found that a longer initialization period is needed to avoid the effects of noise induced drift. The drift is such that the mean terminus position under variability (estimated from the ensemble-mean of simulations without retreats) is further from the bed peak (seaward) than the steady-state, which makes retreat slightly more likely following the start steady state (figure below). This effect persists longer than we initially realized, so we have re-run the ensemble simulations for Figs 3 and 5 with a longer initialization period (250 years) and re-calculated the probabilities. The retreat probabilities are slightly lower, but the plots are qualitatively the same and the overall conclusions are not affected.

Even in the absence of noise-induced drift, it is important to understand whether the probability of retreat is constant in time. We are estimating the probability as n_r / n_s, where n_r is the number of retreats in the experimental interval and n_s is the number of ensemble members that have survived the initialization period without retreating. That is, n_s diminishes as more and more glaciers retreat. Note that this probability is fundamentally a conditional probability - i.e., the probability of retreat in the experimental interval, conditioned on the fact that a glacier hasn't yet retreated. We'd argue this conditional probability is most relevant for the attribution question focused on some particular observed retreat. However, it is worth contrasting it with the joint probability n_r / N, where N is the total ensemble size (i.e., the probability of any of the N ensemble members surviving the experimental interval, *and* then retreating during the observational interval).

In these additional ensemble simulations, we calculate both n_r / n_s and n_r / N over progressive 100-year intervals of a 2000-year, 5000-member ensemble run (figure below). The probability is enhanced in the first couple of centuries, before the noise-induced drift stabilizes. n_r / n_s is fairly stable, but with some caveats. There is some jitter due to sampling error (which we address in a later response), as well as a long-term increase over 2000 years. We infer that the latter is because the variance of glacier fluctuations is under-sampled initially, due to the millennial timescales of interior ice dynamics. The ratio n_r / n_s also becomes less stable as the ensemble size decreases. Note that n_r / N continually decreases; the rate of retreats drops off with the number of surviving ensemble members.

So, there are caveats for short initialization periods (noise-induced drift), and drawbacks for very long initialization periods (the effective ensemble size diminishes, and computational demands are higher). We think a 250-year initialization period is a reasonable tradeoff, even if it doesn't fully account for the longest timescales of ice dynamics. n_r / n_s is fairly stable over this time frame, as both n_r and n_s drop off as more glaciers retreat. (Note that this behavior approximates a Poisson process, where the probability of an event occurring in some fixed interval is constant; of course each glacier can only retreat once, but across the ensemble, the (conditional) probability of retreats per unit time would be constant for a pure Poisson process. There are some caveats due to long glacier memory, but it can still be a useful statistical model to keep in mind.)

We have revised the description of the ensemble methods to clarify discussion on the noise-induced drift and how probabilities are treated. We will also add the figures below to the supplement, to describe the noise-induced drift and the long-range test of retreat probabilities.

Rather than starting all simulations with a strictly steady-state glacier, we initialize simulations with a 250-year period of stochastic forcing. This is necessary because of noise-induced drift that occurs at the onset of stochastic forcing, due to nonlinearities in ice dynamics (e.g., Robel et al., 2018). Indeed, we find that the steady-state grounding line position is closer to the bed peak than the long-term mean under noisy forcing, slightly enhancing the likelihood of at the beginning of simulations initiated from steady state (supplemental figure SN).

From each aleatory ensemble, we estimate the probability of sustained retreat as the number of retreats within the 150-year experimental interval (as defined above), divided by the total number of simulations with termini seaward of the peak at the beginning of the interval. That is, we are fundamentally focusing on a conditional probability of industrial-era retreat (i.e., conditioned on the glacier not having already retreated). We ran several long ensemble simulations to assess how this conditional probability varies in time, and find it to be fairly stable after the noise-induced drift decays, though with some additional caveats at millennial timescales (see supplement and figure SN). We assess the role of anthropogenic forcing by comparing the probability of retreat between an ensemble with constant mean climate and another ensemble with an anthropogenic trend in the mean climate added to all members (Section 4).

Noise-induced drift:

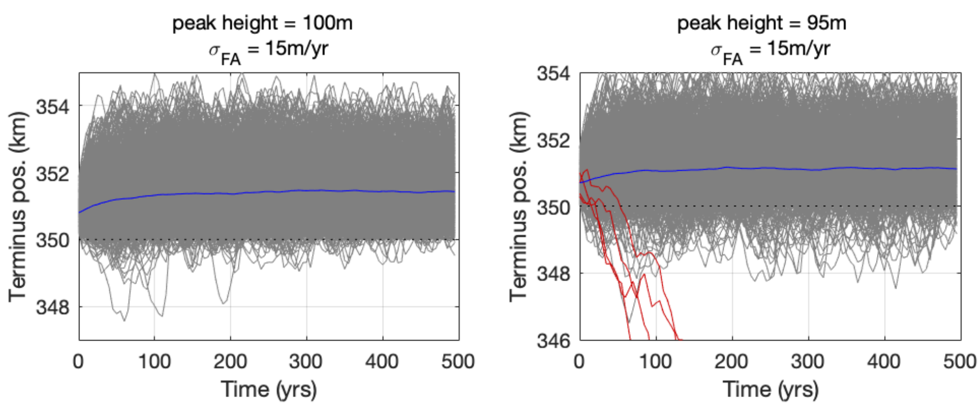


Figure SN: (a) Noise-induced drift can be illustrated via the ensemble-mean terminus position (blue line) in cases with no unstable retreats (which would bias the mean). The drift decays over roughly century timeframes. (b) as for (a), but with a slightly lower bed peak, which moves the system closer to the threshold. Note that a few retreats occur initially, but none after the noise-induced drift decays. This illustrates how the probability of retreat is inflated shortly after starting from the steady-state initial condition, which is closer to the peak than the long-term mean. The same effect occurs for cases with non-zero long-term probabilities of retreat.

Long-run probability test:

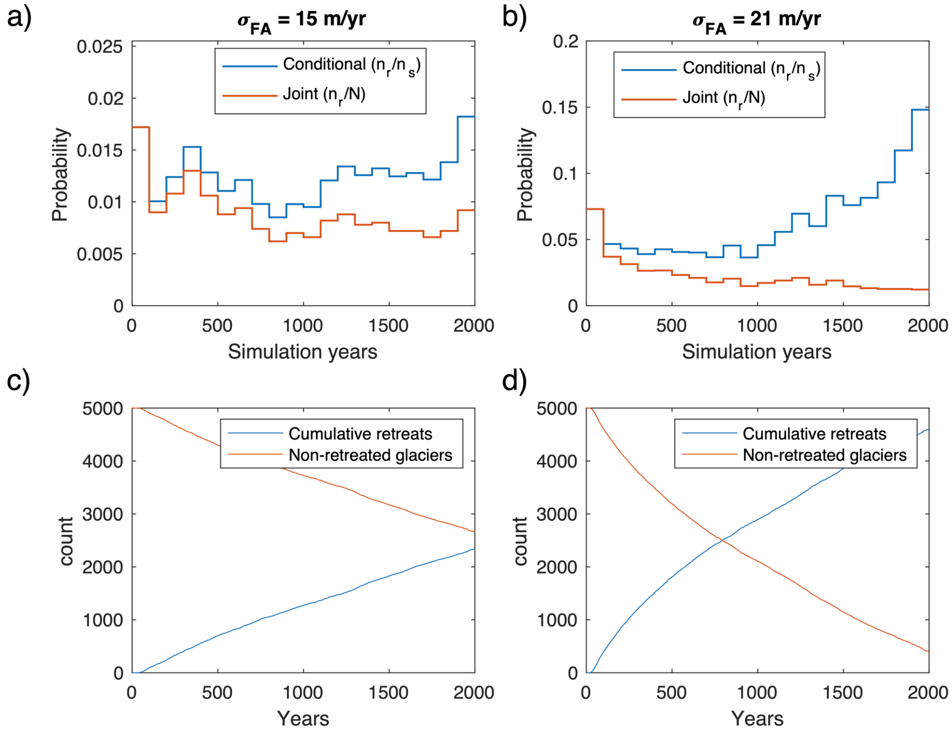


Figure SN: (a) Probabilities of retreat over 100-year intervals in a 2000-year, 5000-member ensemble run, with a bed peak height of 94 m and σ_{FA} = 15 m/yr. Blue markers track the conditional probability (the metric used in our analyses). Red markers track the joint probability, which decays as more and more ensemble members retreat. Note the effects of the initial condition in the first century or two, and the long-term rise in conditional probability (b) As for (a), except with σ_{FA} = 21 m/yr. (c) Cumulative retreats (blue) and non-retreated glaciers (red) throughout the 2000-year ensemble simulation with σ_{FA} = 15 m/yr. (d) As for (c), except with σ_{FA} = 21 m/yr.

Section 3.1: When varying model parameters, the authors find that the probability of retreat is a function of the distance of the steady-state terminus from a bed peak. To be clear, are the authors saying that they think this relationship is causal – i.e. a larger displacement of the terminus is required to reach the peak and trigger retreat – or just a correlation – i.e. the underlying physics of the glacier have been changed in such a way as to enhance instability?

Yes - we think the main effect is the proximity to bed peak. Changes in the underlying physics might also play a role in stability, and this probably accounts for some of the spread in the curves in Fig. 3c. But the overall shape of the curves suggests that getting the preindustrial proximity to peaks is the first-order issue. We've added a sentence to clarify:

> *It is possible that parameter perturbations can affect dynamical stability in other ways (e.g., Parizek et al. 2013), but the similarity of these curves for qualitatively different*

parameters (sliding, mass balance, and bed geometry) indicates that in this case, proximity to the bed peak is the main effect.

Section 3.2: (line ~260) I didn't quite follow the physics here. I naively feel that an increase in discharge could enhance the advection of thicker ice towards the bedrock high, hence advancing the terminus. Is it always the case that an increase in discharge is the crucial destabilising factor? I guess it is actually an increase in ice divergence that is needed to thin the ice and retreat the terminus? By the way I am aware of other literature that draws relevant conclusions concerning the frequency response of ice streams to climatic perturbations (e.g. 10.1098/rspa.2012.0180, 10.1002/2017GL075745) though that is only in the ice shelf-buttressed case.

The increase in discharge was meant to refer to that occurring due to retreat into deeper water (once the terminus has crossed into the region of reverse slopes), as opposed to the initial forcing. In that sense the increase in discharge is crucial to the MISI mechanism, but we agree the initial perturbation could be a different type of forcing, e.g. a drop in surface mass balance which would drive retreat by decreasing flux from the interior. We've reworded this paragraph to clarify the sequence:

The importance of persistent climate anomalies for triggering sustained glacier retreats is related to the timescales of transient ice dynamics. Consider an initial terminus fluctuation driven by anomalous frontal ablation. If the terminus retreats past the bed peak and into deeper water, discharge will increase due to the strong dependence of ice flux on grounding-line thickness (Schoof 2007). Independent of the initial forcing, this drives dynamic thinning and further retreat (i.e., the marine-ice-sheet instability mechanism begins). These changes are not instantaneous; ice flow near the terminus evolves on multidecadal timescales (Robel et al., 2018), so retreat is reversible if the climate forcing anomaly recovers before significant changes in the inland ice flow occur (Fig. 3b). However, the longer the terminus persists behind the bed peak, the more interior ice is lost to the increased discharge. At some point, dynamic thinning and retreat will proceed to the point where the terminus cannot recover even if the initial forcing reverses, and thus the marine-ice-sheet instability takes over in driving the retreat.

Section 4: (line ~302) I didn't quite click with the language that a background trend makes the positive anomalies more persistent. Which is more important: the slow thinning of the glacier that I assume accompanies the (quadratic?) time-integrated ablation anomaly, or the fact that any given positive ablation anomaly is larger?

Our intention was to relate that a trend increases the duration of (positive) melt excursions from the preindustrial mean. But we agree the language could be clearer, as this is somewhat different from a purely statistical notion of persistence. Whether the integrated or instantaneous change in anomalies is more important is also great question. Both should play a role during a trend, but we agree it isn't immediately clear from these experiments which dominates, so we've run some additional analyses (figure below) to isolate the integrated component. It plays a large role, but we agree the direct effect of simply making ablation anomalies more positive should be mentioned too. We've re-framed the paragraph slightly to clarify these two effects, and include the additional figure in the supplement.

> *Why is there such a difference the early-onset and late-onset cases? There are two main effects to consider. First, an external forcing trend makes all positive frontal ablation anomalies more extreme. For the late onset trend, there is simply a shorter window in which more-positive anomalies affect the probability of retreat. Second, the response timescales of of ice dynamics also play a strong role. A glacier's response lags forcing on century timescales, so even if the final magnitude of the trends are the same, the earlier-onset trend will push the average terminus position closer to the threshold within the experimental interval. This makes random variability more likely to trigger sustained retreat. We compared these two effects by assessing the probability of retreat only after trends of varying duration, and found that the lagged dynamic response indeed plays a large role (Supplemental figure X). This is essentially the same principle that differentiates irreversible retreats from reversible retreats in the absence of a background trend; the glacier response reflects the forcing anomaly integrated over decades or longer (Fig. 4).*

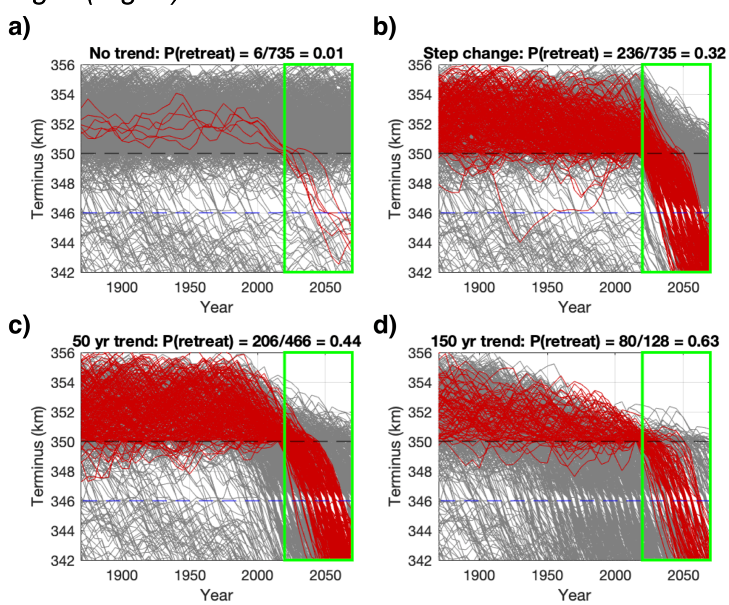


Figure. SN: One way we can isolate the effect of a glacier's integrated response to a trend is by assessing the probability of retreat only after different forcing scenarios. Here we focus on the 50 years after four different forcing scenarios: a) No trend; b) a step change at 2020; c) a 50 year trend from 1970-2020; d) a 150-year trend from 1870-2020. The total changes in frontal ablation are the same, so that after 2020 the distribution of frontal ablation is the same in each scenario (with the exception of (a)). Differences in the probability of retreat from 2020-2070 (green boxes) therefore show the effects of past forcing, namely that long-term trends push the *average terminus position closer to the bed peak. Note that the effective ensemble size drops off under long term trends because many retreats occur during the trend, but the overall effect of the trend is clear.*

Section 4 General: The paper discusses the difficulty of defining a retreat metric, which I fully sympathise with. In this section, the paper determines the effect of a climatic trend on the probability of a retreat of a given distance within a fixed time frame – e.g. before the end of a 150 year run. Under this approach the probability is variable and the time frame is fixed. I wondered if the authors had also considered the inverse approach – asking what is the 'time-to-emergence' of a fixed probability of retreat. E.g. if we choose to be interested in a 50% probability of retreat, the authors could determine how long any given trend would take to induce such a probability (compared to a no-trend scenario). This would have the advantage that the outcome is not a function of the arbitrary duration considered (replacing that with the arbitrarily chosen probability). I recognise that the approach currently taken may be more appropriate to historical attribution, and the time to emergence idea is usually used for projections. My guiding principle here is that under ANY nonzero climatic trend, eventually ALL glaciers will have retreated. So, to me, a time-to-emergence metric reflects that situation.

The time-of-emergence metric is an interesting alternative approach, and could potentially offer additional intuition surrounding the general problem of variability near thresholds. The complementarity between probability and wait time is a useful concept to consider, as the retreats bear some resemblance to a Poisson process. And as you note, such an approach might be highly relevant to comparing the anthropogenic vs. stochastic effects on the timing of future retreats, for glaciers now poised on further-inland bed peaks, or those whose current stability is ambiguous. However, we think it is best to keep the focus in this study on the fixed interval-approach, as it focuses specifically on the whole industrial era. Although the exact onset of glacier-relevant forcing is uncertain (as we discuss), the industrial era (e.g., late 19th Century on) does provide a concrete interval to focus on and avoids the need to pick an arbitrary probability threshold.

Section 4 General: Is there a significance test that needs to be applied here? If we see a difference in retreat probability between ensembles with and without a trend, or with two different trends, surely we need to determine that difference is statistically robust? I cannot immediately think what would be the appropriate test, but I assume it would tell us what ensemble sizes are needed to establish a given retreat-probability difference between two ensembles at a stated confidence level.

This is a good point - there is certainly some sampling uncertainty in these probability estimates and it is useful to understand how it depends on ensemble size. Briefly, we can treat each ensemble member as a Bernoulli trial where "success" is a retreat during a certain interval. The probability of success depends on the chosen interval and model parameters, but for a given set of choices the probability is identical for each simulation. Estimating the probability from N simulations (trials) is a common problem, with standard formulae for estimating the error (which is typically proportional to 1/sqrt(N)). For the conditional probability which is our focus, we have to combine the error from the spinup period and the experimental interval. We've arrived at a metric for the standard error, which we will derive in the supplement. For the range of parameters in our simulations, the differences between forced and unforced ensembles are highly significant. For example, in the new simulations for Fig. 5, the unforced scenario has a 0.04 probability of retreat, and we estimate the standard error at 0.007. The effect of the trend is thus far beyond sampling error.

We want to stress that this degree of significance ONLY reflects statistical sampling issues, and no other uncertainties. We expect that uncertainties in model parameters/physics or in the forcing will likely dominate in attribution assessments for real glaciers, and these will need their own treatment. For this reason, we are reluctant to add confidence intervals to the probabilities in the panel titles (e.g., Fig 5) since it would be hard to convey the context that it is only one source of error - we want to avoid giving the impression of a highly precise method to readers just skimming the figures. However we will note these issues in the main text, and provide detail on estimating sampling error in the supplement.

New text in section 5:

> *We estimate the sampling uncertainty for these probability estimates using standard formulae for ensembles of independent trials (see supplement). We find standard errors of roughly 0.01–0.02 for the ensembles in Fig. 4, making the effect of the trend far greater than sampling error. However, we stress that this is only one source of error, and uncertainties in model parameters can have a much larger effect (Fig. 3).*

Section 5 General: This section assumes that all glaciers in the population have identical climatic forcing, which seems a little restrictive to me. For example in Greenland, all glaciers experience similar atmospheric conditions and far-field ocean forcing, but that is quite different to saying they have the same SMB and frontal ablation rates, which are determined by very local features such as ice topography, fjord geometry etc. I believe the logic assumes that if neighbouring glaciers have different retreat history, that can only be caused by terminus bed geometry, which I don't believe is always the case.

We completely agree this is a major simplification. The main goal here is to consider variety in glacier's proximity to topographic thresholds. We can achieve this solely with a set of random topographies, and since we aren't focused on simulating a specific glacier or region, we didn't see much benefit in adding variations in each glacier's climatology as well. We do note at the beginning of the section that there are other factors leading to heterogeneity, but we agree the simplification should be more clearly flagged. We've added some text to emphasize this here:

The synthetic glaciers we present below thus do not represent the full spectrum of glaciers that could be found in a region, though *similar experiments could be conducted in a more complex ice-sheet model including these other factors.*

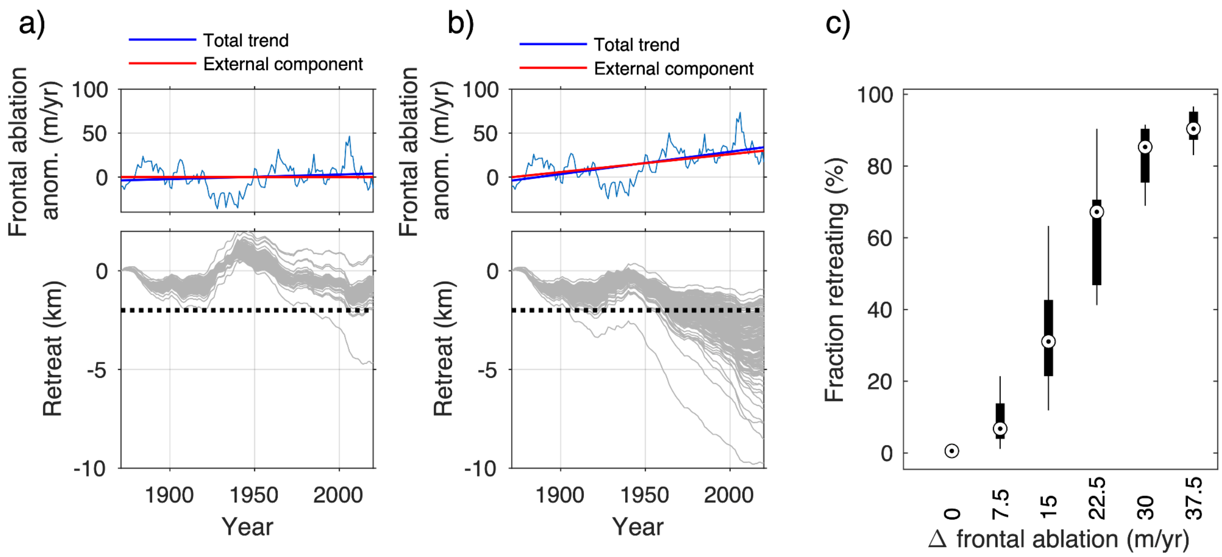
And also where we state that the forcing is identical for each glacier:

We force each glacier in the population with the same frontal ablation anomalies in order to mimic regionally coherent climate variability. ***This neglects a number of factors that can cause ocean forcing to vary widely between individual glaciers (e.g., Straneo et al., 2011; Wood et al., 2021) but our focus remains on simplified experiments to illustrate attribution---here with a variety of topographies.***

Section 5 Figure 7: I was initially surprised that the ensemble in panel a has only one member that advances. I believe this is telling me that there is a statistically significant internally-generated trend in the climatic forcing (towards retreat). This means that the ensemble is 'primed' such that when the external trend is added in panel b, lots of glaciers retreat. This is useful for illustrative purposes, but it is not mentioned and I think the authors should be open about this situation. They could potentially add an internally generated trend line to panel a. They could add a red dot to panel c illustrating that this chosen realisation sits above the mean fraction retreating for 30 m/y (I assume). Probably the best thing would be to select a different realisation that has zero internally generated trend.

We checked the internally-generated trend in panel a) and it is fairly small over the 150-year period (~7 m/yr), though there are some large decadal trends in both directions as expected in natural variability. In panel a, that we have one glacier advance to a new bump, one retreat to a new bump, and the rest not transitioning (still fairly tightly clustered), suggests to us that this isn't a terribly biased realization. It may be fairly good for illustrating the natural state - one might expect to see some sustained advances or retreats in a population, depending on local geometry. Similarly, approximately 90% of glaciers retreat in panel (b), which is indeed somewhat above the median in panel (c) but not an outlier, so we think this is a reasonable realization to show.

However, we like the idea of adding the internally-generated trend line for full transparency, and will add that to the revised figure:



Section 5 General: As with section 4, what statistical testing would be required to demonstrate that a population of glaciers was retreating under climatic forcing, relative to a population fluctuating with no climatic trend, at a given confidence level?

The tests for sampling error that we mention above could be considered if an aleatory ensemble were run for the population of glaciers, but as noted above, it is worth distinguishing sampling issues that could be ameliorated by running larger ensembles, vs. uncertainties in model parameters and other assumptions that are baked into the ensembles. We expect that these epistemic uncertainties are likely to be a larger barrier for overall confidence. These issues might require statistical testing themselves, but would likely depend on the particular case. This is related to an overall comment from the other reviewer (see below), regarding the sensitivity of attribution assessments to our confidence in the various assumptions that must go into them. We've tried to shore up statements that a range of assumptions will need to be tested, and we expect this would apply to the population-based attribution framework too.

Section 5 General: As a closely related point to the one above, I found myself wondering what is wrong with just asking what fraction of glaciers in an area have retreated in the real world. If enough Greenland glaciers are monitored, over a long enough time period, any net retreat implies a climatic trend in forcing must be important, does it not? Then the question becomes how many glaciers and how long a time period need to be monitored to provide a given confidence level. This re-states my 'time of emergence' point above. I can't quite link this concept to the work in section 5, but I bet the authors can. (Plus, I bet enough Greenland monitoring data are available to provide a pretty high confidence level.)

We agree with the point that the overall fraction could be a strong metric of change on its own, but we would also stress that for *attribution* of that change, we would still need to compare it with a no-anthropogenic-forcing counterfactual case. We have great observations showing the ubiquity of retreats in recent decades, but for attribution we still to understand how much this goes above and beyond the response to a strong multi-decade trend associated with internal variability; we need a reference point in an unforced world (which typically requires models, in the absence of detailed preindustrial observations). Additionally, a long-term net retreat might still occur if variability pushes a terminus off of a bed peak, which is part of why we think the counterfactual probability or fraction-retreating is so important.

The point about the duration of observations does seem very relevant - and the observations of early retreats on some glaciers in Greenland raise a similar question. This in part motivated our discussion of these early retreats in section 6, and they might provide useful case studies in future work.

General: Even if the existence of important climatically driven changes in a glacier can be established, that does not imply that the climatic changes are anthropogenic.

We completely agree - assumptions about the anthropogenic forcing are fundamental to attribution. Because attribution conclusions are sensitive to these assumptions, care is needed when inferring the anthropogenic component of a forcing mechanism (e.g., atmospheric or ocean temps). This is indeed why we highlight the difference between early-onset and late-onset trends, and focus a discussion section on the uncertainty in determining the anthropogenic signal in forcing. We did point out that observed climate trends don't necessarily partition the anthropogenic component (2nd paragraph of 6.1), but we will add some further emphasis that attribution of the forcing is a key step:

> *These effects are very clear in the synthetic experiments, where the difference between ensembles* ***---that is, an anthropogenic climate trend---*** *is simply imposed.* ***However, this trend must ultimately be inferred from observations and models of climate, which is an attribution task of its own.*** *When targeting real glaciers, it will be important to evaluate assumptions about the onset* ***and magnitude*** *of* ***anthropogenic*** *trends built into the model simulations.*

And later:

> *Our results suggest that assessing uncertainty in the ~~evolution~~* ***anthropogenic component*** *of local climate forcing will be very important for understanding the robustness…*

Conclusions Line 521: natural fluctuations in climatic forcing

Fixed.