

The Cryosphere Discuss., referee comment RC1  
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## **Comment on tc-2021-394**

Anonymous Referee #1

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Referee comment on "A probabilistic framework for quantifying the role of anthropogenic climate change in marine-terminating glacier retreats" by John Erich Christian et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2021-394-RC1>, 2022

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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.

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.

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.

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 occurred within that ensemble's decorrelation period? Do these matters imply that the conclusions drawn in this section are dependent upon the decorrelation period chosen?

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?

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.

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?

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.

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.

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.

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.

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?

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.)

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.

Conclusions Line 521: natural fluctuations in climatic forcing