Green = reviewer comments, black = responses

We thank the reviewer for their careful reading and thoughtful comments. Their perspective has helped to significantly improve the clarity of the manuscript and we are grateful for their time spent evaluating our work. We have responded with comments interspersed into their review.

In this study the authors use a simple model of glacier dynamics, based on mass continuity and assumptions about spatial variations in mass balance, to assess the disequilibrium of Alaska Glaciers. The advantage to this approach, as opposed to using higher order modeling, is that it can be done quickly for large numbers of glaciers without requiring a lot of inputs. The authors conclude that Alaska glaciers are in a severe state of disequilibrium, having only undergone 27% of the retreat necessary to be in equilibrium with today's climate. The observation that the glaciers are in a severe state of disequilibrium is perhaps not too surprising, and the simplicity of the model and uncertainty of the data used in the model raises some questions about exactly how far the glaciers are from equilibrium. To me, the more interesting result is the discussion of how the climate warming scenario affects glacier disequilibrium (the increased warming trend over the past several decades is much more important than the warming that occurred in the early 20th century) and how glacier geometry affects disequilibrium. I found myself wondering whether the authors have tried looking at spatial variations in glacier disequilibrium. I think it could be interesting to see whether glaciers in certain parts of Alaska are closer/farther from equilibrium.

The main cause of systematic regional variations in disequilibrium would be if there are systematic variations in glacier response times. That will likely outweigh any systematic variations in the time-series of the forced anthropogenic climate response. The response time is generally correlated with glacier area (which we show in Fig. C1, so that would be the best starting point to estimate disequilibrium.

Overall I think this is an interesting and thought provoking paper. I have a few general comments on the paper, mostly aimed at clarifying the glacier model and assumptions associated with it, as well as handful of specific comments.

General comments

1. Section 2 is important for setting the stage for the rest of the paper and laying out the model assumptions. With that in mind, I think this section needs some additional details and clarifications, and perhaps some re-structuring.
a) The section immediately starts off with a figure illustrating the idea of committed retreat, but the figure is based on equations that appear much later in the section. Consider starting with the model description.

We choose to begin with the figure to visually guide a reader about the concept of disequilibrium and to introduce the definitions we are setting up. We feel that if we dove straight into the model equations before doing that, it would divert a reader's attention away from the core principle. We have extensively rewritten the figure caption, adding a title and much more detail leading a reader through the panels. We've also rearranged much of the introduction text so that the math and figure are closer together. We do want a reader to see a graphical illustration of the disequilibrium before diving into equations.

b) I'm not super familiar with the linearized model, and so in reviewing this paper I spent some time also reading Roe and Baker (2014) and Christian et al. (2018), as well as Roe and O'Neal (2009). It appears to me that the model really originates in Roe and O'Neal, or at least is mostly explicitly spelled out in the appendix of that paper. Consider citing that paper.

Thanks for taking the deep dive! The lineage of our model does begin with Roe and O'Neal, though we think our later expositions were clearer. We now include a reference.

c) I think the model assumptions need to be spelled out more clearly. As I understand it, the linearized model assumes (i) an initial steady-state climate and glacier geometry and (ii) small departures from steady-state. I think the authors are attempting to address (i) in lines 65-69, but not very explicitly and it can be easy to miss this assumption because it is presented a few paragraphs before the model.

We have added a clearer statement that model parameters are taken to be constant over the length scales of interest, and included references to a couple more papers where similar linear models have been used to study climate-glacier linkages.

d) Equation 2 is described as the equilibrium length response to a step change in climate; isn't it just a change in a climate from one steady-state to another?

The reviewer is right - it is the same as going from one steady state to another (although we note there is no such thing as a truly steady state in nature). We describe it as a step-change so as not to have to define the time-dependence of the change in mass-balance forcing. We also use it as an opportunity to make a small point: other disequilibrium studies have assumed an exponential dependence of the glacier response, which gives a significantly different estimate than that of the model used here. By doing it this way, we make the point without calling out specific studies, which is unnecessary. We think it is also a little more mathematically precise to talk about a step change, and then talk about a linear trend.

Then, equations 3 and 4 describe the length anomaly and fractional equilibration for linear climate trends, but the sentence after equation 4 says that it applies for any climate trend. Can these equations all just be written in terms of a general change in climate? It's a bit confusing as is.

We use the linear-climate-trend solutions for some of our analyses, because the physical dependencies on parameters can be more clearly seen. The original language was confusing about which of the parameters gets canceled, thank you. We have also changed the language to be clearer that it is the cancellation that applies for any climate trend, not equation (4) itself.

2. The fractional equilibration (equation 4) depends on the time since the start of the climate trend, the glacier thickness, and the mass balance at the terminus. The authors use t_0=1880, ice thickness from regional glacier thickness maps that are known to have large errors, and mass balance profiles from (presumably) representative glaciers. The authors do test the sensitivity of their results to some extent, but I also wonder if it would be useful to use propagation of uncertainty to explicitly show how the model results are impacted by the uncertainty in the model parameters. And I may have missed it, but I don't think the authors tested the impact of their choice of t_0 other than to reference another study.

Uncertainty for any single glacier is best judged from figure 9. If a reader has a specific glacier in mind, with perhaps better information than we had for its thickness or terminus mass balance, they can evaluate the value of f_eq for their preferred tau. Estimating uncertainty for our distributions of f_eq (i.e., across the full population) does not really have an objective approach. If errors among the population are uncorrelated then they would tend to cancel out, leaving the population distribution relatively unaffected. We do report the impact of using different thickness and AAR datasets. We now specifically note that the results are not sensitive to the choice of 1880 as a starting point because, in the realistic scenarios we examine, the bulk of the warming occurs in the last 50 to 100 years.

3. The captions for Figures 3-7 indicate that the panels show the probability density functions and cumulative distribution functions (but note that the caption for Fig. 6 seems to have these flipped). However, I don't think that is the correct terminology. These look like histograms. When you integrate a PDF from x_1 to x_2, you should get the probability that a sample falls within that range, whereas here you are showing the fraction of the samples that fall within predefined bins. Personally I think the better way to plot this data would be use to empirical cumulative distribution functions and complementary distribution functions. I think histograms can sometimes be misleading when sample sizes are small, such as at the tails of the distributions. Regardless of how you choose to plot the data, make sure that the terminology is correct.

Thank you! We were calculating probability mass, which did not match our labels. We have revised Figures 3-7 so the PDF curves are now calculated to sum to 1, as suggested.

We agree that using empirical cumulative distribution functions (eCDFs) is a valid approach and have tested both methods to confirm the difference is minimal for our dataset (shown below). Because we focus on the population as a whole, we prefer the binned approach to visualize the distribution's shape at an appropriate level of detail.

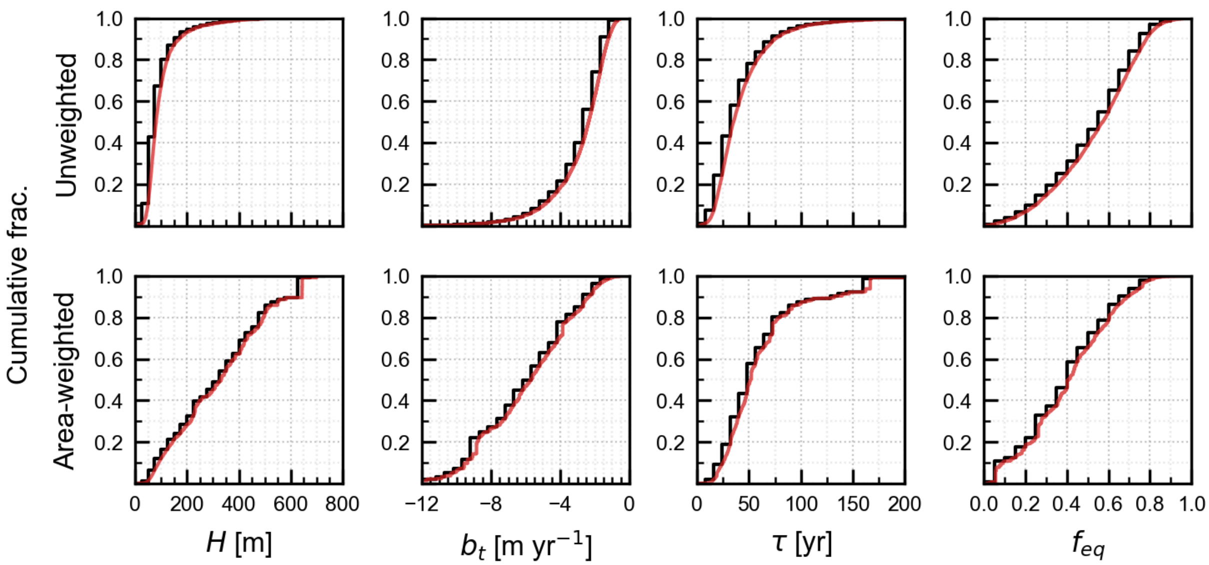

Figure R1. A comparison of using binned CDFs (black) vs. eCDFs (red) to display the cumulative distributions shown in Figs. 3, 5, 6, and 7. The top row shows unweighted distributions (blue in the original figures). The bottom row shows area-weighted distributions (orange in the original figures).

Specific comments

Title and throughout: This is pedantic, but I think it should be "Alaska glaciers" and not "Alaskan glaciers" since "Alaskan" is an adjective. For example, see https://www.adfg.alaska.gov/static/home/library/pdfs/writersguide_section6.pdf

We were unaware of the convention! This seems to be a common confusion as we found a mix of usages in other papers. We are happy to contribute to correcting the misusage! We have changed to "Alaska" everywhere.

L12-13 and later: I'm struggling to wrap my head around the area weighting. I understand the equations, but it's not entirely clear to me what the weighting does.

The area weighting is helpful because of the large disequilibrium of the major bodies of ice that contain most of the glacier mass. This perspective can get lost in the population distributions when the large glaciers get weighted the same as the many very small glaciers. When introducing the area weighting, we now say "Area weighting gives greater emphasis to the larger glaciers in which most of the glacier mass resides, making it more relevant for an assessment of the aggregate mass of Alaska glaciers."

L28: Do I understand correctly that the Johannesson time scale emerges naturally in your model, which is why it makes sense to use it in this analysis instead of that of Harrison (for example)? Might be good to point that out.

The Johannesson timescale is the standard geometric timescale for alpine glacier response. The Harrison timescale tries to also capture the height-mass-balance feedback, but imposes an extra geometrical constraint and parameter. It is most appropriate for shallow-sloped glaciers. It is worth bearing in mind, but not adopting wholesale.

L42: Models, at least if done correctly, should be spun up with the climate so that they are capturing the current dynamical state.

We agree wholeheartedly! And in our experience, there are variations in the literature in how (and for how long) alpine-glacier models are spun up. It is one goal of ours to highlight the magnitude and variations in the dynamical state of glaciers, which should ideally go into decisions about how to spin up models. We've clarified our intent with this statement.

L46: The response time depends on geometry, so I'm not sure why you are also mentioning geometry here.

This can be seen from eq. (2) or eq. (3). Committed retreat depends on both tau and beta. Beta reflects additional aspects of glacier geometry.

L60: How can they be identical if their response times are different? They must have different geometries and/or climates.

This passage is deleted from the revised text, but there is no problem with having two glaciers that share a common value of beta, but a different value of tau.

L62: "… change in climate from a steady state." ?

We've rephrased this in the revised manuscript.

L68: Suggest "will tend to recover on its own" or "will recover on its own if there is no trend in climate".

Thanks for noting this. We've removed the word 'recover', because it is unnecessary and potentially confusing.

L180: Is it okay to treat \tau as a constant and not something that changes during a warming trend? Maybe something to discuss in Section 2. And also, here you state that you are estimating \tau prior to the start of the warming trend but you are using modern thickness estimates. (I see that you come back to this later in line 271, but again, I think being more upfront about the model and assumptions would be helpful.)

Thank you, the way we did this was confusing. When presenting the model we are now explicit that parameters are assumed constant. We've also added a few more general references of where such models have been used to understand climate/glacier interactions.

We've removed the phrase about estimating tau prior to the warming, which is distracting in that location. The discussion about tau is now in a single place. The bottom line is that uncertainties in H and b_t are more important than the exact moment of time tau is estimated for. We indicated conditions under which the assumption of constant tau is weakest, and we point readers to the section where we consider the impact of uncertain tau on our results.

L280: It took me a little while to remember that f_{eq} was fractional equilibration and to remember what that meant. It could be helpful to remind the reader occasionally; for example, in Figure 7 you could consider indicating that the left side of x-axes indicates more committed retreat and the right side indicates less committed retreat.

Thanks, we agree this would help. We've repeated the definition of f_eq ahead of showing Figure 7, and included how to interpret the limits of low and high f_eq. We also included similar guidance in the caption of Figure 7.

L295: I guess this should be Eq. (4)?

Thank you, good catch!

L323-326: This gets back to my confusion about the model that I discuss in the general comments. Are you using a linear trend for each year? Meaning that Equation 4 is for a linear trend, and so my confusion is related to lines 110-111. So it seems like you are essentially applying a perturbation each year. Are you also then updating the ice thickness and response time? A glacier's response time will change quite a bit as it thins and retreats.

We really, really apologize for this - this was careless equation-referencing on our part, based on legacy drafts. The reviewer is correct that eq. (3) was for a linear trend only, and is right to be confused by what we had. We have rewritten this sentence, and now refer to the correct equation. For a general climate history, we integrate our model (eq. 1) forward in time, using constant coefficients. The impact of those coefficients changing is discussed elsewhere. We've responded elsewhere as to how the revised manuscript clarifies the impact of that assumption on our results. Sorry again.

L431: Do we know how this "dramatic disequilibrium" compares to past climate events?

The degree of glacier disequilibrium is set by the magnitude and rate of climate change. For the typical, global-scale, hockey-stick-shaped climate history over the last two-thousand years, the current disequilibrium would be unprecedented over that period (see Roe et al., 2021). At the local scale, we lack the information to make such strong statements on millennial timescales.