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 20^{th} 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.

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.
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.
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.
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? 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.

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.

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.

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

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

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.

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

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

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

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

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

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.)

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.

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

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.

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