## Comments to the reply by Bakken-French et al.

From the reply of the authors to my earlier comments I conclude that a main point of disagreement is the ***glacier definition***. I start with this point and add details about the climatic interpretation later. As a first step, I had a detailed look at all the publications cited by the authors as well as some additional documents* that I hope would confirm their view to exclude stagnant ice from glacier mapping. Apart from the fact that the authors assign this region subjectively without using flow velocities, I have not found any evidence in the cited and additional literature that buried/stagnant ice had to be removed. If at all, it is mentioned as a possibility rather than mandatory. For example Beason et al. write on P20 (or 1) 'identifying surface velocities on glaciers at the Park to delineate active and stagnant ice'. This means they just distinguish the two types rather than removed the stagnant part and they used flow velocities for it. This is also my interpretation of the polygons marked as 'buried ice' in the inventory by Fountain et al. (2023). These polygons can be marked, but have to be included.

Also Lillquist and Walker (2006) (LW06 below) mention the separation as a possibility, but they are also not applying it. They have seemingly also not applied the rules they have listed for glacier terminus identification. I have compared the dated outlines in their publication to the aerial photography available online** and in particular the oblique 1946 images for Coe and Eliot Glacier reveal strongly down-wasted glacier tongues, basically only consisting of ice-cored lateral moraines. Still, the termini where placed at the end of this assemblage of buried or maybe dead ice remnants. I would thus not take the rules literally.

To conclude this part, I had a look at the UNESCO guidelines by Müller et al. (1977) and the GLIMS Analysis tutorial. The former states: 'Inactive ice must be included in the inventory for hydrological purposes. Marginal and terminal moraines should be included if they contain ice. The "inactive" ice aprons which are frequently to be found above bergschrunds should be regarded as part of the glacier. Glacierets and snow patches of large enough size - if perennial - should also be included in the inventory.' The latter describes under points 4 and 5:
'4. A stagnant ice mass still in contact with a glacier is part of the glacier, even if it supports an old-growth forest.' and '5. All debris-covered parts of the glacier must be included.'

Hence, to be consistent with the earlier interpretations of terminus positions, the literature cited by the authors as well as current guidelines, but also to avoid subjective assignment of glacier areas, the buried or stagnant ice has to be included. I see no way around this. As a way forward, I suggest using the outlines from Fountain et al. (2023), merge the polygons marked as 'buried ice' with the main glacier and then modify them to the 2023 extent, e.g. as visible in the very high resolution satellite image from the ESRI Basemap. Apart from the noticeable retreat of the Coe terminus, also the retreat and lateral melt of Eliot should be updated. For Ladd Glacier one has to first glue the five polygons from Fountain et al. (2023) together, but without the original images a new interpretation of the terminus will be difficult. If the image is not available, I suggest skipping this glacier for this year from the change assessment.

For glaciers that have now two or more parts (e.g. Zigzag/Sandy), all parts included in the former extent have to be summed up for calculation of area changes. I would not do a comparison of change rates when including and excluding the stagnant ice. As you do not have flow velocities, you cannot say where the stagnant ice is and related area or length changes would be arbitrary. So I would skip this stagnant ice discussion altogether. Instead, please have a discussion about the difficulties in identifying termini of down-wasting glaciers. This can be nicely illustrated and allows others to connect to this global-scale mapping problem.

The second main point I want to make is on the ***climatic interpretation***. As mentioned before, most of the glaciers in the study region can hardly be used for a climatic interpretation, as they are either heavily debris covered (e.g. Ladd, Coe, Eliot) or calving (Reid, Newton-Clark). This decouples their responses to climate change and makes calculation of response times challenging. On the other hand, a closer look at the Mt. Hood glacier images available online** combined with the accumulation season precipitation and cumulative length change plots presented by LW06, reveals a fast response of the glaciers to related fluctuations (e.g. advances following increased precipitation). This fast response can likely also be expected from the steepness of the terrain. So, if the authors wish to keep some climatic interpretation, I suggest stripping it down to the basics and continue the time series presented by LW06. This means:

- add a note on the difficulties to interpret glacier fluctuations in this region in climatic terms
- continue the analysis by LW06, showing only curves for winter accumulation and summer ablation without an artificial delay or averaged values
- use cumulative length changes rather than normalized length
- discuss the obviously fast/sensitive reaction to precipitation changes in the past and contrast this with the now dominating retreat/down-wasting due to increasing temperatures (and all the problems related to it for glacier extent mapping).

Regarding the suggested text changes describing the importance of flow for an ice body to be named a glacier, I also suggest to not present it. First, the authors do not use or have flow velocities to apply the definition and second, the cited definition by NSIDC as well as the one by Cogley (2011) says 'evidence of *past* or present movement/flow'. So even if the 'stagnant ice' does not flow today, there is multiple evidence that it has done so in the past. In other words, also these definitions do not allow removing possibly stagnant ice from the area.

I hope the above explanations help clarifying my objections and the authors can revise their ms accordingly. Most of the points in their reply will likely settle when we agree on these main points. As a note, the comments from my earlier reviews are still valid and should thus be considered as well. I have not repeated them here.

* USGS Circular 1132 by Fountain et al. (1997), Mennis and Fountain (2001) in PE&RS and Chapter 17 in the GLIMS Book by Fountain et al. (2014).
** https://glaciers.us/glaciers.research.pdx.edu/image-galleries/oregon-cascades.html

Cogley, J. G.; Hock, R.; Rasmussen, L. A. et al. (2011): Glossary of Glacier Mass Balance and Related Terms; International Hydrological Programme (IHP) of the United Nations Educational, Scientific and Cultural Organization (UNESCO).

Müller, F., Caflisch, T. and Müller, G. (1977): Instructions for Compilation and Assemblage of Data for a World Glacier Inventory. Swiss Federal Institute of Technology Zurich.

Mennis, J.L. and Fountain, A.G. (2001): A spatio-temporal GIS database for monitoring alpine glacier change. Photogrammetric Engineering and Remote Sensing 67 (8), 967–975.