Editor's and referees' comments in plain text and author responses in *italics*

Editor (B. Marzeion):

While the reviewers agree that the revisions lead to a substantial improvement of the manuscript, some rather substantial questions about the methodological decisions taken by the authors remain, and a clarification is likely to have strong impacts on the conclusions of the manuscript (potentially even questioning the "unprecented" included in the title of the manuscript).

We thank the reviewers for their further insights. However, we note that our methodology is, in fact, well documented and is widely used in the western U.S. To address the questions raised about our methodology, we have now added further discussion of the decision to map actively flowing glacier area rather than include stagnant, debris-covered ice in the mapping. Nevertheless, we would like to clearly point out again that three of the reviewers, including two who work extensively on western U.S. glaciers, had no issues with our mapping of actively flowing glacier area and exclusion of stagnant ice. In fact, one reviewer, M. Pelto, asked us to insert a statement about how robust our methods were.

To clarify the impact of our methodological decision on the conclusions of the manuscript, most notably, we now add in the buried ice mapped by Fountain et al. (2023) that they explicitly excluded as part of the glacier (i.e., neither did they list it with the glacier name nor did they include with it its glacier geographical identification number, which they did do for other parts of other glaciers that had turned into snowfields/stagnant ice bodies) to our glacier areas in the discussion section. We also include all other stagnant ice/snowfield bodies that are linked to glaciers by name and glacier geographical identification number. Importantly, these inclusions still find that 21st century retreat is unprecedented with respect to the prior 120 years and that our conclusions do not depend on our methodological choice. Note that we must choose this methodology of mapping and documenting changes in only flowing glacier ice if our results are to be compared to all other studies of western U.S. glaciers and past glacier extents on Mt. Hood. In so doing, we clearly demonstrate that our conclusions are robust regardless of how one defines a glacier.

Reviewer 3 (M. Pelto):

We appreciate that reviewer 3, M. Pelto, finds our manuscript is significantly improved and does not require further review. We have made his suggested changes in our revision here.

Line 38: We have made this change.

Line 80: We appreciate this comment.

Line 83: We have added this information.

Line 125: We have added this sentence and note how this reviewer, M. Pelto, who particularly works on Pacific Northwest glaciers finds our technique to be "robust" and "effective".

Line 146: We have reworded this sentence according to the reviewer's suggestions.

Line 196: We have reworded this sentence according to the reviewer's suggestions.

Line 297: We have removed the word "inferred" as the reviewer asks.

Line 300: We appreciate that the reviewer finds our images in Figure 3 to be useful.

Line 335: We have added a statement on precipitation's lack of a long-term trend.

Line 350: To answer the reviewer's question, no, Frans et al. (2016) did not include any albedo changes beyond a snow-albedo aging function that had maximum snow albedo tuned to 0.80 and then an ice albedo tuned to 0.27. Thus, particulate darkening of the glacier surface from dust/fire-sourced black carbon/etc. was not included. Debris cover influence on mass balance was explicitly modeled.

Line 375: We now indicate that these are fall Chinook, summer steelhead, and Coho.

Line 385: We have reworded this sentence according to the reviewer's suggestions.

Reviewer 4 (F. Paul):

The revised ms has greatly improved compared to the first version. The now annotated photos guide the reader much better through the observations, the methods are clearer described and the study appears to be more focussed. However, from the response of the authors to my comments and the unchanged results, I conclude that it was not sufficiently clear what I criticised. The authors arrived at their very high area loss rates for the 2000-2023 period because they changed the definition of a glacier, largely by introducing the vaguely described term 'active ice'. For this, they use (qualitative) geomorphological characteristics of advancing glaciers (L141/2) to define where a glacier terminus is rather than (quantitative) information about glacier flow. As the listed characteristics do not apply to retreating or down-wasting glaciers, they looked at the wrong place for the terminus. How they identified it in the field (when being in the middle of a debris-covered glacier) remains a bit mysterious. They write about repeat photography and that they took ice margins (mapped from multiple Sentinel-2 images) to the field (L122-126), but they neither show the outlines derived from this mapping nor describe how they have determined the region of stagnation without flow fields. Anyway, also stagnant ice has to be included.

We respectfully, but strongly, disagree with the reviewer. We understood his earlier comments but disagree with him on what constitutes a glacier, using a very common definition that has been employed for all prior glacier mapping in the western U.S. Therefore, we did not change the definition of a glacier and we clearly referenced our peer-reviewed methodology. We also described how we determined stagnant ice from actively flowing ice again, all using peer-reviewed methods. Furthermore, the reviewer does not provide any peer-reviewed reference justifying why stagnant ice has to be included. In fact, we find no peer-reviewed publication for *the western U.S. on the changes in glacier extent that included stagnant ice in their calculations. Inventories do exist that note such stagnant ice, but these inventories do not calculate changes in glacier extents and explicitly exclude stagnant ice from their glacier areas as a separate non-glacier ice body. As such, we find these comments to be the reviewer's subjective opinion rather than based on an objective body of scientific research, like upon which we have based our methods and study.*

The authors state their approach of glacier extent mapping is consistent with prior methods (L117), but it is not. As the authors did not provide glacier outlines for 2023 or 2004, I had to use the 2015/16 outlines from Fountain et al. (2023) for a comparison (Figure 1). This revealed that the authors removed the polygons marked as 'buried ice' by Fountain et al. (2023) for Coe and Eliot glacier although these regions were also included in the study by Lillquist and Walker (2006). This decision (to not say manipulation) resulted in a strong area loss for Coe and Eliot. Also images available in the internet* show the terminus of Eliot at a different place (close to its 2004 position). For Coe one can clearly see the real terminus (marked by a darker frontal part) on the image in Fig. 1B (a bit below the 2004 outline).

In fact, the reviewer is misrepresenting the results of Fountain et al. (2023). Furthermore, the reviewer also seems to overlook the fact that 23 years have passed since Lillquist and Walker (2006) mapped the extent of Eliot and Coe glaciers and that these glaciers have, in fact, changed in the first 23 years of the 21st century.

First, we did note the stagnant "buried ice" and that it is now almost fully disconnected from the actively flowing Coe and Eliot glaciers by 2023. In 2016, there was greater physical connection, as seen in Fountain et al. (2023), but Fountain et al. (2023) explicitly mapped these ice bodies as separate non-flowing, non-glacier ice bodies.

Second (and to reiterate), Fountain et al. (2023) explicitly did not include these two buried ice bodies in the extents of Coe and Eliot. In fact, they did not consider them to be part of any glacier, which, if the reviewer properly represented their results, would be clear. For glaciers like Glisan or Zigzag that were breaking up into multiple stagnating and stagnant ice bodies in 2015/2016, Fountain et al. (2023) labeled each of these separate polygons with the glacier name, even if it was a snowfield (not a flowing glacier). They also supplied the glacier geographical identification number for each polygon even if it was a snowfield. Contrary to this, Fountain et al. (2003) did not label the separate polygons of buried ice with the name of Coe or Eliot, nor did they supply the glacier identification number for these buried bodies. As such, Fountain et al. (2023) clearly did not consider these two buried ice bodies to be parts of Coe or Eliot glaciers. We thus followed their conclusion. Indeed, we clearly showed the actively flowing glacier outlines of Fountain et al. (2023) and our 2023 outlines, with reviewer 2 (A. Fountain) raising no concerns over our use of his definition of what constitutes Coe and Eliot glaciers.

Third, Lillquist and Walker (2006) mapped that ice was actively flowing in 2000 for much of the Coe and Eliot termini that Fountain et al. (2023) mapped as separate stagnant buried ice bodies in 2015/2016. We included the flowing-in-2000 parts of these ice bodies in their length records (Lillquist and Walker, 2006) as well as in their 2000 areas (Jackson and Fountain, 2007) from which we calculate the rate of change. By 2004, at least in the case of Eliot, these ice bodies *were stagnating. Jackson and Fountain (2007) determined ice flow and stagnation by measuring boulder movement between 2004 and 2005 for the debris covered terminus of Eliot. They then explicitly excluded the stagnate part of the terminus from their 2004 glacier outlines, parts of the glacier that were still flowing in 2000 as documented by Lillquist and Walker (2006) and Jackson and Fountain (2007). Fountain et al. (2023) subsequently determined that the whole debris covered terminus had stagnated by 2016 and thus explicitly excluded it from Eliot Glacier (and did a similar explicit exclusion for Coe). These actively flowing glacier 2004 outlines were also used as modeling targets by Frans et al. (2016). To then compare our calculated rate of retreat to that simulated by Frans et al. (2016), we must only include changes in the area of actively flowing glacier ice as that is what Frans et al. explicitly modeled.*

In summary, the reviewer is misinterpreting these results that clearly focused on mapping actively flowing glacier ice and explicitly excluded stagnant ice.

For the massive shrinkage of Ladd Glacier the situation is more difficult, as the authors largely followed the interpretation by Fountain et al. (2023), who decided to ignore the debris-covered parts and divided Ladd into five pieces. In fact, these pieces are all still connected and its 2023 extent is not very different from 2004. This might be difficult to see in other images, but in my previous comments I have asked the authors to use the very high-resolution image from the ESRI Basemap for interpretation. This also would have helped to get more realistic extents for Sandy Glacier. For its northern part and terminus region the 2023 extent is still very similar to 2015/16, so their shrinkage here is overestimated. The region with strong area loss since 2004 in its south-western part has indeed little ice left, but the ridge still has a few small glaciers. These have to be included for a correct calculation of area changes. This also applies to Zigzag, where the ice patch marked with a 2 in Fig. 3A and B has to be included; that the ice is probably stagnant does not matter when calculating area changes.

All of these "pieces" that the reviewer refers to are stagnant ice bodies as of 2023 and we excluded them from the extent of Ladd's actively flowing area following the definition of a glacier in the western U.S. The same holds true for Sandy Glacier. We did, in fact, use the 2023 ESRI image (see our Figure 1) as the reviewer requested. However, that image did not change our glacier mapping as we had meticulously over multiple years used numerous images along with high-resolution Google Earth images and extensive field testing. The fact that our final mapping of ice limits did not change with the new ESRI image is a testament to our effort and accuracy. The issue here is not our mapping but rather what one considers part of the glacier. We reiterate that we followed the methodology used for all prior such efforts in the western U.S., whereas the reviewer has a different definition of what constitutes a glacier. Please see more discussion of this issue below.

Hence, the strong area loss and retreat of several glaciers reported by the authors for the past 20 years are strongly overestimated for at least three of the larger glaciers, leading to wrong conclusions. In fact, several of the glaciers (Ladd, Coe, Eliot) have barely changed their extents since 2004 when following the interpretation of earlier studies. In the case the authors would like to correct their datasets and report the real area/length changes (please note that providing glacier outlines for review is mandatory), I have a few more comments below. In its current form the results are misleading and should better not be published.

The extents of Ladd, Coe and Eliot have significantly changed in the last 20 years, which is easily seen in the aforementioned studies of Fountain et al. (2023) and Jackson and Fountain (2007). We feel that the reviewer's statement is rooted in a misrepresentation of prior work. (Note, as we said in our previous reviewer response, we will provide the glacier outlines as a shapefile in due course and prior to publication.)

However, to address the reviewer's comments in a constructive manner, we have now included a discussion of these two different definitions of a glacier. Specifically, we have added the following paragraph to our methods:

"In measuring glacier dimensions, it is important to be clear on what is considered a glacier and thus included in the glacier extents. Clarke (1987) noted in his review of glacier research since the 1820s that "the most interesting property of glaciers is that they flow", using the present tense. The U.S. Geological Survey defines a glacier as "a large, perennial accumulation of crystalline ice, snow, rock, sediment, and often water that originates on land and moves down slope under the influence of its own weight and gravity" (U.S. Geological Survey, 2024, https://www.usgs.gov/faqs/what-glacier). The NSIDC, which hosts data for GLIMS, defines a glacier as "an accumulation of ice and snow that slowly flows over land" (NSIDC, 2024b, https://nsidc.org/learn/parts-cryosphere/glaciers/glacier-quick-facts). However, the NSIDC has another definition of a glacier as: "a mass of ice that originates on land, usually having an area larger than one tenth of a square kilometer; many believe that a glacier must show some type of movement; others believe that a glacier can show evidence of past or present movement" (NSIDC, 2024a, https://nsidc.org/learn/cryosphere-glossary/glacier). Therefore, our mapping of only actively flowing glacier ice follows many traditional definitions of what constitutes a glacier, but we recognize that another definition of a glacier exists that includes ice that used to move and is now stagnant. We return to our decision to map only actively flowing glacier ice, rather than include stagnant ice, in the Discussion section."

And then in the Discussion section:

"We now return to our finding of unprecedented 21st century retreat of glaciers on Mt. Hood based on mapping only actively flowing glacier ice and comparing 2023 areas to equivalent actively flowing glacier areas in 2015/2016, 2000 and earlier years. If we had followed the other interpretation of what constitutes a glacier and included all stagnant ice in the 2023 glacier extents, would retreat in the 21st century still be unprecedented? As a test, we added to the 2015/2016 and 2023 areas of glaciers (Table 1) the area of additional snow/ice bodies noted in the Supplement mapped by Fountain et al. (2023). We conservatively assumed that these snow and ice bodies did not change in area from 2015/2016 to 2023, which was not the case as some of these ice bodies disappeared (see Supplement). With this additional fixed area, the change in glacierized/perennial snow area on Mt. Hood from 2015/2016 to 2023 was >14±1% versus the change in area for actively flowing glaciers of 18±1%. We thus conclude that defining a glacier as a flowing ice body does not greatly change the percentage of glacier area loss in the last seven to eight years.

As another test, we looked at the seven glaciers with records back to 1907 (Fig. 4, 5) (Jackson and Fountain, 2007). We added their peripheral 2015/2016 stagnant buried ice/stagnating ice/perennial snowfield area that Fountain et al. (2023) mapped as separate from the contiguous flowing glacier area to our 2023 glacier areas (Table S2). This additional area was 1.085±0.041 km^2. About 0.718 km^2 (~66%) of this area was in the two buried ice bodies that were explicitly mapped as separate from and not parts of Coe and Eliot glaciers as of 2016 (Fountain et al., 2023). Another 12% of this area was in stagnating ice/perennial snowfields that had separated from Ladd Glacier as of 2016. We then compared these new larger glacier/ice/snow areas to the 2000 areas for these seven glaciers, noting that this is not an equal comparison. First, we again assumed that the areas of these peripheral snow/ice bodies did not change in the last seven to eight years, which is not the case. Second, the 2000 areas of these glaciers only included contiguous actively flowing glacier ice and explicitly did not include such peripheral snow ice/bodies or contiguous stagnant ice (Jackson and Fountain, 2007). With these assumptions, we found that these seven glaciers lost area at >1.07±0.1 % yr^{-1} 2000-2023, which is still unprecedented with respect to 20th century average rate (0.31±0.03 % yr^{-1}) and the fastest 20th century rate (0.56±0.02 % yr^{-1}) (Table S2). Similarly, the retreat of Eliot, Coe, and Ladd glaciers remained unparalleled relative to both the average and fastest 20th century rates despite containing 78% of the non-flowing ice area added to 2023 extents (Table S2). White River's rate of area loss was also unequaled with respect to the 20th century average and remained within uncertainty of its fastest 20th century rate (Table S2). Newton-Clark and Sandy glaciers likewise still had unmatched rates of retreat in the 21st century whereas Reid's rate did not change (no additional ice added) (Table S2). Therefore, our finding of unprecedented 21st century retreat of glaciers on Mt. Hood does not depend on how one defines a glacier."

Public interest in glacier changes
As these days any reported glacier changes are of high interest for a large public and critically assessed by 'sceptics', the results presented must be as solid, reproducible and correct as possible. This is not the case here and the wrong assignment of glacier boundaries result in a rather dramatic shrinkage for several glaciers. I would thus not publish the study as it is now and recommend re-working the outlines using the available very high-resolution satellite image. Ladd and Newton Clark Glacier will grow a bit compared to 2015/16, but Zigzag, Eliot and Coe will become comparable to this and earlier extents, allowing calculating consistent change rates from 2004 to 2023. As a note, the authors refer to the year 2004 in Fig. 1B, to 2003 in Table 1 and to 2000 in Table 2. The different dates need to be clarified.

First, our outlines do not require reworking because they are based on our use of the definition of a glacier as an ice body that flows. Second, we followed the methods used to map the prior extents of these glaciers in 2015/2016, 2000, 1981, etc. We acknowledge that the reviewer is actually asking us to change to use a different definition of a glacier; however, we respectfully disagree that this change is necessary or practical for our study for the reasons we detail above. However, third and most importantly, as we show above, our finding of unprecedented glacier retreat in the 21st century is robust against how one defines a glacier, addressing any concern over how "sceptics" may look at our work.

As for the issue of 2004 versus 2000, we had explained this in our original submission, with the sentence cut in the revisions. We have now added this back:

"We focused on the change from 2000 to 2023, rather than 2000 to 2004 and then 2004 to 2023, to make the duration over which the most recent intervals of change were measured closer to the earlier temporal spacing."

We also use the 2000 area rather than the 2004 area to calculate changes following the comment/suggestion of reviewer 1. Specifically, he/she/they requested that we determine rates of area change (noting that these are for actively flowing glacier ice alone) from 2000 to 2023 and 1907 to 2000, which we did.

Field vs. remote sensing

In my view consistent measurements of glacier length changes is best done in the field, whereas mapping of glacier extent and area changes is the domain of (orthorectified) aerial or satellite imagery acquired under the best possible snow conditions. To obtain meaningful results, the spatial resolution of the imagery used for this should be higher towards smaller glaciers. The 15 m panchromatic band of Landsat is likely the upper resolution limit for the comparably small glaciers of Mt. Hood, mapping with 10 m Sentinel-2 as performed by the authors is certainly more accurate. For this region the authors have also a 0.3 m resolution image with near-perfect snow conditions. Combined with their knowledge about the topography of Mt. Hood and its glaciers, very accurate outlines can be generated based on subtle changes of contrast and/or colour, often also for debris-covered glaciers. I would argue that this is nearly impossible in the field when being on the glacier in a sea of rocks. When including the 'stagnant ice', the 2023 termini for Coe and Eliot will be close to the orange circles in Fig. S1B. As a note, the images displayed in Figures S6 to S8 for Eliot, Coe and Ladd do not show their termini, but regions higher up. So they are of limited use to illustrate changes of the terminus.

We agree with the reviewer that the time of image acquisition is critical, which is why we chose to use Sentinel-2 imagery to supplement higher resolution Google Earth imagery and then the ESRI imagery. However, the latter higher resolution imagery often come from inopportune times for determining ice margins, particularly Google Earth. The ESRI imagery is better in timing (albeit still before the lowest point of snow coverage in 2023), but as noted above, it did not change our mapped glacier limits because we used our combined field and remote sensing approach. As we noted, we relied on field measurements that can be real-time compared with the most recent satellite imagery over four years and three field seasons.

Furthermore, please note that reviewer 3 (M. Pelto) disagrees with the reviewer (F. Paul) by stating: "The combination of satellite imagery delineation of margins with ground truth mapping relative to the images, is a robust effective technique."

Comparison with other sources

In Table 1 the authors compare their glacier extents to those from 1981 by Driedger and Kennard (1986) and in Table 2 to year 2000 areas from Jackson and Fountain (2007). But in the latter study the areas are listed for the year 2004 rather than 2000 and for the 1981 extents the authors authors do not show the mapped extents. I assume the authors have these as they provide areas for 1981 with two decimals instead of one. It could then be shown that the interpretation of glacier entities is consistent and related outlines should be added to Fig. 1B. For consistent area changes, all parts belonging to the largest glacier extent need to be included.

First, see above for 2004 versus 2000 as Jackson and Fountain (2007) provided glacier areas for both years. Second, we report the Driedger and Kennard (1986) areas to the same significant figures place that they reported them (up to three significant figures) after converting millions of feet squared to kilometers squared. The maps/outlines are only available in paper format or scanned paper in PDF; they are not digital otherwise we would have added them to Fig. 1B. And once again, Driedger and Kennard (1986) only mapped actively flowing glacier ice for 1981 as it was a U.S. Geological Survey study, who defines a glacier as an ice body that flows under its own weight. We cannot go back and recreate their mapping and geophysical surveys from 1981 to fit the different definition of a glacier to which this reviewer ascribes.

Comments by other reviewers
In several cases the authors replied that the other reviewers have not mentioned my comment as an issue. Whereas I agree that this is a possibility to ignore a requested change, I think in this case an 'outside' view could still be worth considering as the other reviewers might have worked along the same guidance documents. It is also the right of the authors to say study xyz has been peer-reviewed and published, so the results presented there should be correct, but sometimes this is not the case and different views remain. When it comes to mapping glacier extents this is likely more usual than agreement. In short, I added my comments also because the other reviewers did not notice the issues.

We respectfully, yet strongly, disagree with the reviewer here. Two of the other reviewers, A. Fountain and M. Pelto, are the leading experts in western U.S. glacier change. We used the same methods for mapping glacier change that they used, including detailed exclusion of stagnant ice from actively flowing ice and only calculating changes in glacier area based on actively flowing ice area. Furthermore, their mapping in the past is based on such qualitative methods for detecting ice flow that we use here. In only one instance has a flow field been produced for a glacier in Oregon, and that is for Eliot Glacier on Mt. Hood. Here, Jackson & Fountain mapped displacement of boulders on the terminus of Eliot. They then used these observations to explicitly exclude from the 2004 Eliot ice margin a still connected yet stagnant section that was flowing in 2000.

Relation to climate
I would strongly recommend removing the climatic interpretation (Figure 6 and Table 3). The sample is too small, each glacier has a different response time, some glaciers did flow over or now end at steep rock cliffs (creating dead ice) and glaciers are debris-covered to a variable extent. All this creates non-linear responses of variable magnitude that should not be compared to some fixed mean values of the climatic history. Of course, glaciers retreat and shrink when it is getting warmer, but that's it. In this region I assume that also precipitation amounts could have an impact on glacier length changes, as these seem to correlate well with small advances of several glaciers. However, due to the backward temporal averaging of the time series this is difficult to say.

We respectfully disagree with this reviewer that this section should be removed, and our opinion is shared by three other expert reviewers. In fact, reviewer 1 requested we expand (not remove) on this relationship, which we pushed back upon doing because we felt it involved some poorly constrained assumptions. Reviewers 2 (A. Fountain) and 3 (M. Pelto) only requested we address glacier response time in terms of our correlations, which we have done. M. Pelto then found our paper was "significantly improved" and suggested acceptance with some minor suggestions with no further review.

Furthermore, while the number of glaciers with length records is only five, we disagree that this is a major issue because all five have significant correlations with temperature, which is consistent with a robust signal. We also feel it is important to assess this glacier-climate relationship as the prior study for this region found that these glaciers were not responding to climate change (Lillquist and Walker, 2006), which was further put forward in Menounos et al. (2019) that concluded there was no change in Mt. Hood glacier mass in the 21st century. As such, this is an important analysis, particularly given the above worries by the reviewer (F. Paul) about climate sceptics.

See below response about concerns over glaciers flowing over cliffs and other contributors to non-linear retreat.

Please note that the 920 m retreat of Ladd Glacier between 1984 and 1989 reported by Lillquist and Walker (2006) is also suspicious. The authors of that study have likely decided to interpret the lower part of the glacier in 1989 as dead ice (they write 'ice-cored ground and lateral moraine') and have assigned a new terminus. While this can be required for some glaciers, such jumps in the time series disqualifies it for the statistical analysis performed here (as it relates to a much longer evolution that has little relation to the change in a specific year).

We disagree that such changes disqualify Ladd Glacier from such analysis. Furthermore, we feel strongly that such a priori selection of glaciers for climate-length relationships would not be consistent with scientific best practice. However, to constructively address the reviewer's comment, we now have added a sensitivity test to our results section to address this abrupt retreat in Ladd Glacier. We also conduct a similar sensitivity test for White River Glacier, which also has a clearly non-climate related forcing: the development of fumaroles in the upper reaches of White River Glacier in the late 1800s.

Specifically in the Results:

"Lillquist and Walker (2006) found that the ~560 m retreat of White River Glacier from 1901 to 1938 was facilitated by the development of fumaroles whereas the ~920 m retreat of Ladd Glacier from 1984 to 1989 was due to recession over a cliff (Table S1). To test the significance of these non-climate related recessions on the correlation of these two glaciers' lengths with the records of three different temperature seasons over two backward-running means, we removed these intervals of retreat from the length records and recalculated correlations. White River Glacier's retreat was still significantly correlated ($p < 0.05$) with 30-year and 15-year backward-running mean-annual, May-October and June-September temperature changes and their r^2s changed only at the third decimal place. Ladd Glacier's retreat was still significantly

correlated ($p < 0.05$) with the six temperature-change records, albeit r^2s (0.56-0.83) were generally lower than for the full-length records (0.65-0.82).”

Apart from some further small remarks, I do not further comment on details of the ms, as I expect substantial changes to the text should the authors decide to get the glacier extents corrected and resubmit the study.

Our glacier extents are correct according to one of the two definitions of a glacier. We clearly state our methods and the definition we used that is widely used in the western U.S. In fact, we know of no study that included stagnant ice in their glacier areas. The one example that the reviewer puts forward is from Fountain et al. (2023); however, as discussed in detail in our response above, the reviewer mischaracterizes the mapping of Fountain et al. (2023) and ignores their explicit exclusion of stagnant ice from the actively flowing glacier area.

Small comments
I still found 8 times the word glaciated instead of glacierized.

We have made these changes but note that this is also a controversial term with a debated definition going back a century.

L398: That all data are available from the figures and tables presented in the text is not true. The key dataset (the glacier outlines from different points in time) where all analysis is based on are not provided. This is mandatory for a proper review of such a detailed study of individual glaciers.

We added the appendix as a Supplement, but the Copernicus submission system only allows one file to be uploaded in this section. So, we think the shape files probably counts as a “Data set”, which the system states “Data sets, movies, animations, or computer programme code should be deposited in FAIR-aligned data repositories.” Alternatively, we could specify the shape files as an “Asset” once we have a DOI in a data repository for it. Given the limitations of the submission system, we made the decision to resubmit everything but the shape files for the revision, and then plan to add the shape file data set/asset prior to publication if the manuscript was accepted for publication (the shape files were not needed for the review process in our opinion but should be available publicly at the time of publication). To reiterate, we will make the shape files available on a FAIR-aligned data repository and provide the DOI as an asset associated with the manuscript. We note that this is the same process that the reviewer (F. Paul) is using for a paper in review at The Cryosphere (“Reconstructed glacier area and volume changes in the European Alps since the Little Ice Age” available at https://doi.org/10.5194/egusphere-2024-989). However, to clarify our intention and address the reviewer’s comment, we have now added placeholder text in the Data Availability section of the manuscript (the actual DOI for the asset will be added prior to publication).

Fig. 3E shows lots of seasonal snow cover. The image should not be used for a visual comparison of glacier extents.

We respectfully disagree because we feel that the changes we note are clear. For example, we note that reviewer 3 (M. Pelto), who works in the field on glaciers in this part of the world, agrees with us, found our images to be "quite useful", and does not share the opinion of this reviewer (F. Paul).

Figs. 4 and 5: The plots need axis bars and tick marks also on the opposite site of the graph.

We have added these to the graphs.

Fig. 6: Instead of 'Normalized length' (which is hard to imagine) one could also work with length changes. Please also provide the length changes for all glaciers in a table. Now most of them are spread in the glacier descriptions of the supplemental material. Please also use different symbols when using green and red lines in the same plot (maybe replace the green with blue).

We originally used the change in length for regression. Then, however, reviewer 2 (A. Fountain) asked that we use normalized length instead, which we do in this revised manuscript. We describe normalization in our methods and its application here is reasonable. Namely, this normalization allows to place the change in length of a glacier in perspective of the total glacier length. It is essentially the same process as looking at percent change in glacier area rather than total area change, which this reviewer (F. Paul) asked us to do. As such, we prefer to use the normalized length, consistent with reviewer 2's (A. Fountain's) suggestion.

Also, as requested by this reviewer, we now provide in Table S1 the length records for the five glaciers, updating the existing records to 2023.

Finally, our shade of green passed the review of one of our authors who is color blind, but we changed the color to blue to alleviate any concerns and address the reviewer's comment.