<u>Author responses to review (02) of:</u> *Constraining regional glacier reconstructions using past ice thickness of deglaciating areas – a case study in the European Alps*

First of all, we would like to thank the reviewer for the valuable and constructive comments on our manuscript! The comments clearly improve the quality of this study and all comments are considered.

According to the points raised below, we extended the discussion of uncertainties regarding the transferability of the presented approach to other mountain regions (section 4.3). Furthermore, we made some changes to the presentation of results and visualization of the experimental workflow as suggested by the referee.

Our point-by-point responses are denoted below in bold. New and revised paragraphs which were included in the main manuscript are additionally indicated by the respective line numbers and bold italic text.

Referee #2

Review of Sommer et al. (2022): Constraining regional glacier reconstructions using past ice thickness of deglaciating areas – a case study in the European Alps

Summary
This paper presents a new approach for reconstructing the thickness of glaciers using an additional constraint of DEM-differencing of areas that have become deglaciated within the period of observational record, building on previous work by the authors. They apply this method to the European Alps, reconstructing ice volumes in 1970 and 2003, and show that, deglaciation preferentially occurring in thinly glaciated areas, this induces a global bias in their results similar to that found in the ITMIX2 experiments when using thickness observations from thinner parts of glaciers. They consequently derive two different empirical correction factors for the modelled ice viscosity, one based on distance from the glacier margin and one on elevation and show that including these substantially improves their reconstruction and leads to better matches between modelled and observed thicknesses than in previous studies, though still with possible large local mismatches owing to the regional-scale calibration of the correction factors. Overall, they find glacier volumes in-line with previous recent studies of Alpine glacier volume, but with the major advantage that their approach could be easily extended to areas without direct thickness observations.

All in all, I think this is a good paper with much to recommend it. Most of my concerns are of a fairly minor nature, though with a single larger one to consider. I feel the paper makes a valuable addition to the literature on estimating glacier thickness and volumes by proposing an innovative method that could be of use globally.

Samuel

Thank you very much for taking the time to review our manuscript. The comments significantly helped to improve the presentation & discussion of results in this study as well as the general legibility.

RC.02. = Comments of referee 2

Major points

RC.02.01: Applicability to other regions: The authors touch on this briefly at the end of the conclusion, but perhaps soft-pedal this problem a little too much to be considered entirely honest about it. The paper already shows that the large-scale calibration of the empirical factors leads to substantial local variation within the Alps; I thus can't help feeling that it's exceptionally unlikely that the same correction factors would work in an extra-Alpine context. I think some additional consideration of the challenge likely to be posed by trying to apply the method elsewhere needs to be included – nothing much, just another couple of sentences in the conclusion – but the current formulation is unrealistically optimistic, I feel.

> **Response: We agree with the reviewer that this is an important point. The unclear transferability of the approach to other regions is certainly one of the largest draw-backs regarding its broader application. Admittedly, the presented correction functions are somehow inherently linked to the geometries of the Alpine glaciers and respective distribution of small, medium and large glaciers. Unfortunately, we cannot entirely avoid this problem with the second experiment (~2000 ice volume reconstruction) because the overall geometries of the Alpine glaciers remain similar between the 1970s and 2000 (although glacier areas are retreating).**
> **Therefore, we extended the last part of the discussion section (4.3, l540) by:**
> ***"Eventually, the presented approach could be most beneficial in regions with large glacierized areas and sparse thickness observations where the glacier volume has to be interfered mostly from remote sensing information. However, another potential source of uncertainty, regarding the transferability of the presented correction terms to glacierized areas outside the European Alps, results from the varying regional glacier morphologies in terms of size composition and elevation range. While the found empirical relations between ice viscosity and glacier surface topography have been applied to a different observation period and larger study region ($H^{retreat}_{SIA2003}$), we expect that the scaling functions are to some degree related to the geometries and size distribution of glaciers in the Swiss and Austrian Alps. In the European Alps, this uncertainty cannot be avoided because the overall distribution of a large number of small to medium-sized cirque glaciers with few large valley glaciers remains unchanged between $H^{retreat}_{SIA1970}$ and $H^{retreat}_{SIA2003}$, despite the substantial reduction in glacierized area since the 1970s. Further, the presented relations might be linked to the geographic environment of the European Alps as glacier changes are connected to the surrounding topography and climatic conditions*** **(Abermann et al., 2011).** ***To quantify these relations between the Alpine topography, glacier geometries and the derived scaling parameters and examine the transferability, it would be mandatory to extend the presented analysis to another glacierized region with different glacier morphology, such as marine- and lake-terminating glaciers, as well as different climatic settings, which is beyond the scope of this work."***

Minor points

<u>**RC.02.02:**</u> p.1, l.13: I might say 'due to the difficulty of undertaking field surveys' or 'challenging field conditions'. Strictly speaking 'challenging field surveys' doesn't really mean what you're trying to get across – it implies the surveys were difficult but have been done, when what you're trying to say is that they're difficult and therefore haven't been done.

Response: Agree. We changed this part accordingly.

<u>**RC.02.03:**</u> p.1, l.25: I'm wondering whether the reconstructed volume in 1970 is really the right thing for the abstract when you also calculate a modern glacier volume for the Alps. My feeling is that people will be more interested in the modern value and how it stacks up to other recent reconstructions of Alpine glacier volume, or the rate of change between the two periods you've reconstructed, than the volume 50 years ago so I'd suggest re-writing the abstract along those lines, certainly if you've only got space for one highlight result.

Response: In the abstract, we did not include the volume of all Alpine glaciers for the year ~2000 because a number of previous studies have already presented volume reconstructions for the entire Alps. Therefore, our alpine-wide results should be rather seen as a proof of concept, by reproducing similar ice thickness as the reference studies (discussion section), but not as an entirely new result.
As the abstract is already slightly too long, we removed the last sentence (and the 1970s volume) entirely from the abstract (l.25-26). Thereby, the abstract emphasizes the main outcome of this study, i.e. the approach of using remote sensing data and not the calculation of new glacier volume results for the European Alps.

<u>**RC.02.04:**</u> p.2, l.35: You need to spell out what GLOF stands for before using the abbreviation.

Response: Yes, we inserted "glacial lake outburst floods" at this point

<u>**RC.02.05:**</u> p.2, l.50: 'Contrastingly'

Response: We replaced "contrasting" by "contrastingly" in l.50.

<u>**RC.02.06:**</u> p.4, Eq. 2: I can't see η used anywhere in the equation, though the text (lines 101-103) implies it should be? As a result, I'm unclear exactly how your viscosity scaling is actually being applied to the flux field to modify the inferred ice thicknesses.

Response: There is an error in the equation. "B" in Eq.2 should be "η" for ice viscosity. The reason is that we used "B" for viscosity in the first manuscript version but replaced it later by "η" because "B" is often used for mass balance and might be therefore confusing for some readers. In any case, we replaced "B" with "η" in Eq.2.

<u>**RC.02.07:**</u> p.6, l.165: 'slope-dependent'

Response: Yes, we changed this here and in the rest of the paper

<u>**RC.02.08:**</u> p.9, Fig. 1: I'm not entirely sure this figure helps explain things all that well. All the bidirectional arrows make it very challenging to work out where to start and re-reading the caption several times hasn't helped me make a lot more sense of it. I am prepared to accept that I'm not very good at understanding diagrams, but if you can come up with a more intuitive schematic, that might not hurt.

It made sense after I read Section 2.5.1, but not till then, so at the very least move it to after that section of text.

Response: We included this workflow figure in the paper in the attempt of presenting the individual stages (and structure) of the reconstruction visually but we agree that there is still a lot of room for improvements. We moved the figure at the end of the methods section (after section 2.5.2, p.11) and improved the general layout by using different color schemes (for different types of input data) and stroke widths. Additionally, the bidirectional arrows were replaced by one-directional arrows.

RC.02.09: p.11, l.295: 'elevation-dependent'

Response: See comment above & below, we changed this throughout the entire manuscript.

RC.02.10: p.12, Fig. 2: As a general point, 'dependant' is a word in English, but it's the noun form, so a dependant would be, say, your child. If you're aiming for the adjective, it's always 'dependent'. I'll stop pointing it out now, but go through and replace all instances of it (you almost certainly do not mean 'dependant' anywhere in the paper).

Response: Yes, we were obviously aiming for "dependent" and will correct this during the revision.

RC.02.11: p.15, Fig. 4: I'm not sure a linear regression is all that great a fit, based on the graph. It overestimates in the middle and underestimates at both extremes. I realise this is what the second correction factor is ultimately fixing, but is there any way you could test the impact of using a non-linear regression? Also, what is the dotted black line on the graph showing? I assume it's αtres, but then it's got a different value to that quoted in the text at l. 340. Please clarify what's going on here.

Response: In Figure 4, we deliberately applied a rather simple linear regression because the analyzed ratio of surface slope and viscosity is not a physical-based but empirical relation found for flux estimation from the different subsets of thickness observations and glacier morphologies. Therefore, we did not attempt to apply a more complex non-linear scaling of the ice viscosity because the glacier-specific correction terms are likely more difficult to be controlled. Within the Alps, glacier geometries strongly vary between large valley glaciers and relatively small cirque glaciers, i.e. the extent or hypsometric location of flat or steep glacier parts varies significantly between individual glaciers. As described in the discussion, there is (for example) already a noticeable overestimation of very flat glacier parts. With a non-linear function this bias would be likely to even increase because the scaling ratio for very flat glacier areas would be even higher.
→ Regarding the black dotted line shown in the graph at 43.75°, this is in fact the slope threshold of the linear regression (α_η^{thres}). Unfortunately, there was an error in the text at L.340 / P.13. α_η^{thres} should be 43.75°, as indicated in figure 4, and not 56.05°. The latter number is the location where the slope viscosity ratio is equal 0. However, we use the surface slope (43.75°) where the ratio becomes 1 as threshold and do not apply the correction function to steeper slopes (where almost no glacier areas are located). For the revised manuscript, we replaced α_η^{thres} in L.340 with the correct value and extended the caption of Fig.4 by adding: ***"The slope threshold (α_η^{thres}) is indicated as vertical black dotted line at 43.75°."***

RC.02.12: p.16, Fig.5: Similarly to Fig. 4, you need to explain in the caption what the dotted black line represents. Here, the value matches up with the value quoted at l. 365 for htres, so I'm confident it's that, but it needs stating in the caption.

Response: See comment above, the dotted black line represents the elevation threshold (h_η^{tres}). We will include this as description in the caption and replace the "x = 0.61" by "h_η^{tres} = 0.61" in the figure.

<u>RC.02.13:</u> p.18, Sect. 4.1: I'm wondering if you could be a little stronger here in your assertion that earlier studies might have underestimated Alpine glacier volumes. Given nearly all the recent studies are pointing in that direction, it seems to me that one would have to be extremely perverse to argue that the earlier studies weren't underestimates.

Response: We agree with the reviewer that an underestimation of glacier volume by the early studies (Müller et al., 1976; Maisch et al., 2000) is likely. We rephrased this part during revision.

<u>RC.02.14:</u> p.25, l.541: 'inferred' not 'interfered'

Response: "Interfered" was replaced by "inferred".

<u>RC.02.15:</u> p.25, l.546: 'slope-dependent', not 'slope-depending'

Response: We inserted "slope-dependent".

References

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