Review of 'The Aneto Glacier (Central Pyrenees) evolution from 1981 to 2022: ice loss observed from historic aerial image photogrammetry and recent remote sensing techniques' submitted by Vidaller et al for publication in *The Cryosphere*

In this manuscript, the authors examine the historic and recent geometric changes to Aneto Glacier, the largest remaining glacier in the Central Pyrenees. They leverage a variety of topographic data sources to measure area and thickness changes since 1981, with a focus on the last 3 years due to the availability of UAV surveys. They also conducted field GPR measurements of ice thickness to map the contemporary bedrock and reconstruct the current and historic ice thickness. They then use the former to assess bedrock topographic variability and possible overdeepenings.

In general the assemblage of field measurements and historic surveys provides a rich overview of changes to Aneto Glacier, and the documentation of the demise of an iconic Pyrenean glacier will definitely be a worthwhile contribution to the literature. However, I have some major concerns relating to several aspects of the manuscript, all of which should be addressed before the manuscript will be suitable for publication.

1. Research aim. The research aims need to be reformulated somewhat. I also do not think the authors have addressed their aims in the discussion: e.g. anticipating the evolution of other temperate mountain glaciers (L70) or determining the inflexion point in glacier mass loss (L85). Meanwhile 'showing' the speed of glacier change is not really a scientific objective.
2. Clarity of presentation of results. The authors refer to thickness change, but in some cases their meaning is not entirely clear or consistent with the literature. I believe that they are determining the slope-perpendicular surface change (as normally derived from the M3C2 algorithm in CloudCompare), but this differs strongly from the typical surface height change values reported from geodetic studies (e.g. Hugonnet et al., 2021) and the authors need to be clear about that difference, as well as to check that it is what they wish to present. I would recommend also presenting traditional thinning values as well as the total volumetric change of the glacier for each period of interest. This feeds into the need to improve the discussion/justification for considering surface areas vs planimetric areas (again, I recommend reporting both values), which are useful for distinct analyses. More importantly, in some cases the authors seem to equate mean changes in ice thickness change (ie thinning) with changes to the mean ice thickness (e.g. L265-269, L389-392); these quantities differ fundamentally due to changes in glacier area due to thinning, and the authors need to reconsider these comparisons.
3. Gaps in presentation of the results. The authors have compiled visually aesthetic figures, in general, but I think there are some fundamental gaps in their presentation of results. I would specifically request that they present the GPR transects in the main manuscript, along with an assessment of the overall uncertainty of their bedrock estimates (partial uncertainty from the crossing points in Table S2, but also consider the radar uncertainty in general), provide thinning maps for each period of the UAV surveys, include off glacier measured elevation changes (Figure 3 appears cropped) and an empirical uncertainty assessment of the thinning rates, which is necessary as the authors do not seem to have used independent GCPs to assess the accuracy of their UAV-derived elevation models. I also think that the thinning/volume change results would benefit from a temporal summary plot (also depicting uncertainties), as in e.g. (Ragettli, Bolch, & Pellicciotti, 2016).
4. Demonstration of the suitability of TPI for identifying bedrock overdeepenings. This is an interesting idea as an alternative to topographic-routing approaches to identify sinks, but the authors need to demonstrate that it is useful for this purpose – does this approach work

to identify bedrock sinks in mountainous terrain? Analytically-speaking, I am concerned that, as TPI is also dependent on local topographic high values, the sinks that the authors identify are actually due to the neighbouring high topography of glacier headwalls, rather than locally low values of bedrock topography. The authors would also do well to compare to a conventional drainage-based identification of topographic lows.

5. Discussion. The discussion could be improved in several ways, and ideally should provide new insights. At present, the comparison to past studies (both Vidaller et al, 2021 and Campos et al, 2021) does not provide a clear, consistent explanation for discrepancies between values reported in past studies (note also my point 2 above relating to potential discrepancies between the physical variables measured, as well as the lacking rationale for surface area instead of planimetric area). This should all be clarified, but more importantly the Discussion should be geared to critically evaluate their own results and to provide new insights. The authors make a very casual assertion of albedo change and melt season duration prolongation, but without evidence, and attribute the accelerated mass losses to warming, without presenting analysis of a climatic record. I also miss a critical evaluation of the possibility of lakes based on the TPI results. Most importantly, though, I feel the study misses a chance to postulate on the future changes of this system. As select examples: at the current rates of volume loss, how long do we expect Aneto to last? When would the hypothesized overdeepenings become exposed? Is the headwall likely to provide a microclimate that can prolong the glacier's life? Or, indeed, does Aneto still qualify as a glacier – is it moving dynamically? – and when may it be too small to be considered as such? Although the field data they have collected is commendable, I feel that the authors need to go further examining some of these aspects if they wish this study to be a meaningful contribution to the literature.

Minor comments:

L21. Please check 'glaciated' vs 'glacierized'

L39. (Huss & Hock, 2018) is a great modelling study but is not 'observed'

L44. Misplaced colon :

L48. I suggest refraining for using 'ice thickness wastage', which is a vague term, and instead using volume loss or thinning, here and throughout the manuscript. Similarly, for clarity I would recommend 'area loss' instead of shrinkage.

L49. Can you be more specific about which volume change data is lacking? Clearly there is now the (Hugonnet et al., 2021) dataset, but perhaps this is not sufficiently precise or long-term?

L52-53. Please refer to thinning rates for this comparison.

L55. 'considered' is redundant here

L59. 'having an additional protection figure' – unclear what you mean here – I suppose that the authors refer to additional societal value?

L66. 'comprehension' -> analysis

L67. Is this the largest spatio-temporal dataset of glacier observations? I don't think that is necessary motivation for this study

L68-71. Please consider the scientific objectives: 'showing' consequences is not a valid scientific motivation (implies bias), but investigating/measuring/understanding is.

L72. Please use either 'subject of annual monitoring' or 'subjected to annual monitoring'

L91. I do not understand what is meant by 'sectors can be delineated…'

L94. 'longest period ever observed' seems like hyperbole – have there been investigations of the LIA extent? LGM?

L108. Is this the annual isotherm or a seasonal isotherm?

L110. This formulation of the temperatures is not very clear to me (L112-115 for the Aneto summit is much clearer). Over what period are these the maximum and minimum mean annual temperatures?

L141 Please check how to acknowledge this source in the 'Acknowledgements' section of the manuscript. Can you ascertain any information with regards to the precision of geospatial positioning of the data?

L142. Do you use any independent check points?

L159. How does using the same protocol imply reduced uncertainties? I can imagine the argument that it implies similar uncertainties to a prior study, but this should be borne out with direct measurements.

L162-3. Please rephrase this statement – have you then assessed the distribution of deviations on stable ground? Is this from individual check points or the distribution of absolute deviations, or?

L192. Have these data been submitted to GlaThiDa (Welty et al., 2020)?

L199. (also 237-239)How was the division done? Is this totally random, or patch-wise? Note that your radar returns are nearly continuous and autocorrelated, so a random subsetting is likely to give a very good cross-validation result, but has little to no information with regards to the accuracy of your interpolated product.

L207. Strong slope = steep. Please give a slope value?

L211. Is this the 3D or xy geolocation accuracy? I disagree that using the same protocol will give the same results – this is dependent on weather conditions (especially wind and humidity) as well as the stability of camera focus (for example).

L217. M3C2 gives the surface-perpendicular distance, rather than thinning. Does it also give a total-volume-change estimate?

L222. Please note the uncertainty of this assumed value.

L225. The TPI method is an interesting idea, but it is not very clearly justified – in particular because TPI values are extremely low directly beneath steep areas, whether or not the location is a topographic low. Is there a justification for using TPI instead of a drainage model, as is commonly used (Linsbauer, Paul, & Haeberli, 2012)?

L240-242. Do you have an estimate of the uncertainty of delineation from the 1981 survey?

L245. 300% larger than?

L251. By 'no movement' you seem to mean that the terminus is not retreating higher, is that correct? Or did you assess surface velocity?

Table 1. Please consider providing uncertainty estimates for your area-change assessments.

L265. Does this 'mean ice thickness loss' correspond to the thinning (i.e volume change over the glacier area) or the change in the mean ice thickness over time? These are two very different properties that need to be clearly disambiguated (both are worth noting!).

L275. Please consider presenting volume changes (loss is a negative number).

Figure 3. If I understand correctly, this figure presents the surface lowering (ice thinning). Please also indicate the extents of the glacier in 1981 and 2022 on the upper panel.

Figure 4. Please indicate the locations of the GPR lines on this left panel. On the right panel, I would consider a box plot rather than the range of thicknesses (the dots), which will give a better sense of the distribution of thicknesses for each elevation

L298-300. This is Methods.

L302-303. Glacier thickness was high -> glacier was thick

L306. Please use past tense also for the 2022 observations.

Figure 5. Panel A looks quite noisy due to localized artefacts in the 1981 dataset, which you could probably justify filtering out. Could you use these reconstructed geometries to look at the volume-area relationship for this glacier (Bahr, Pfeffer, & Kaser, 2015)?

L325. Misplaced period after the Westoby citation

L325-330. I think some more justification and demonstration for the TPI method is needed – can you validate the approach with now-exposed overdeepenings, for instance the Indominato Lake from the 1981 data? How does this compare to a flood-filled glacier bed DEM? How is this method better for identifying overdeepenings?

Figure 6. The locus of low TPI values below the headwall is due to the headwall, rather than due to the subglacial topography.

L344. Was there any accumulation area in 2021 or 2022?

L345. Neither PlanetScope nor Sentinel-2 are sufficiently high resolution?

L358-367. I feel that some of this discussion could simply be confusion about what a previous study reported, and how this calculation works. The mass balance is the mean value of thinning, not the change of the mean thickness. It is clear that the mean thickness will not decrease at the same rate as the surface thins, because the mean thickness is only calculated for the glacier area that is still glacier. Please reconsider this section. I would suggest to also refer to the Hugonnet et al (2021) results for this domain, even if they are imprecise due to resolution and so on. In fact, I would strongly recommend a figure compiling and comparing the mass balance observations available from this study, Vidaller et al (2021), Campoes et al (2021) and Hugonnet et al (2021), as well as the observed spatial extents.

L374-382. I have a hard time understanding the authors' argument here – what is the advantage that they see in investigating surface area changes instead of planimetric changes? As there is a considerable deviation due to the high surface slopes, should the authors not simply report both?

L388. Have the authors compared the spatial distribution of measured ice thicknesses from their own observations and Campos et al (2021)? I see clearly that there is a statistical difference but the authors need to demonstrate that this is not simply due to spatial biases in sampling. How do the observed bedrock elevations compare in space?

L401. This link to hot summers in recent years has not been explicitly made, and would require a comparison of (best if long-term) measured mass balance along with climate information. Although this is very likely to be true, the authors have not tested or demonstrated this, only that the glacier is continuing to rapidly lose mass.

L409-414. Please write this more clearly, e.g. heat -> energy. Also, have you evaluated the albedo changes (not demonstrated here), or is this a hypothesis? Same for the longer exposure of the glacier – please clearly differentiate between observations and conceptual discussions.

L411-412, L424-426 The references to Shaw et al (), Otto (), and Yue et al () need to be reformatted.

L450. Realizing that this would entail a change of scope, I think it would be very worthwhile to also consider the future evolution of this glacier and its neighbours. See e.g (Huss & Fischer, 2016).

L478. Alignment issue with this reference

L470 – Several reference formatting issues. Please ensure that you follow the guide for The Cryosphere.

References:

Bahr, D. B., Pfeffer, W. T., & Kaser, G. (2015). A review of volume-area scaling of glaciers. *Reviews of Geophysics*, *53*(1), 95–140. https://doi.org/10.1002/2014RG000470

Hugonnet, R., McNabb, R., Berthier, E., Menounos, B., Nuth, C., Girod, L., … Kääb, A. (2021). Accelerated global glacier mass loss in the early twenty-first century. *Nature*, *592*(July 2020). https://doi.org/10.1038/s41586-021-03436-z

Huss, M., & Fischer, M. (2016). Sensitivity of very small glaciers in the swiss alps to future climate change. *Frontiers in Earth Science*, *4*(April), 1–17. https://doi.org/10.3389/feart.2016.00034

Huss, M., & Hock, R. (2018). Global-scale hydrological response to future glacier mass loss. *Nature Climate Change*, *8*(2), 135–140. https://doi.org/10.1038/s41558-017-0049-x

Linsbauer, A., Paul, F., & Haeberli, W. (2012). Modeling glacier thickness distribution and bed topography over entire mountain ranges with glabtop: Application of a fast and robust approach. *Journal of Geophysical Research: Earth Surface*, *117*(3), 1–17. https://doi.org/10.1029/2011JF002313

Ragettli, S., Bolch, T., & Pellicciotti, F. (2016). Heterogeneous glacier thinning patterns over the last 40 years in Langtang Himal. *The Cryosphere*, *10*(5), 2075–2097. https://doi.org/10.5194/tc-2016-25

Welty, E., Zemp, M., Navarro, F., Huss, M., Fürst, J. J., Gärtner-Roer, I., … Li, H. (2020). Worldwide version-controlled database of glacier thickness observations. *Earth System Science Data*, *12*(4), 3039–3055. https://doi.org/10.5194/essd-12-3039-2020