Dear Reviewer

We deeply thank you for your reading and the remarks provided on the manuscripts. All your comments have been considered and the references have been included. Hereafter, you will find our answers to your questions (in green).

Best regards,

Review2 of "New insights into the decadal variability in glacier volume of an iconic tropical ice-cap explained by the morpho-climatic context, Antisana, (0°29' S, 78°09' W)" by Basantes-Serrano et al., (2022).

General comments

This article describes the decadal changes in glacier volume in the Antisana ice cap located in the tropical Andes, Ecuador. The authors have used photogrammetric and remote sensing techniques to provide a long-term geodetic mass balance for the Antisana ice cap. Overall, there has been a lack of long-term glacier mass balances studies in this region. For this reason, additional information and novel insights into the past and current state of tropical glaciers are very welcome. In general, I think this is a well-presented and worthwhile piece of research and could help increase our knowledge about the spatiotemporal patterns of glacier volume changes. The topic is timely and highly relevant for various research branches including glaciology, hydrology, and climatology. I am very much in favor of seeing this manuscript published, and would like to make the following suggestions.

We really thank the positive comments provided for the reviewer to our work. We hope the new version of the paper will match the Journal requirements.

Methods

- The authors used state of- the art remote sensing and photogrammetric techniques to generate digital elevation models to estimate volume changes. The authors also applied state-of-the-art post-processing techniques (including co-registration, gap filling, outliers filtering, etc.) to provide a complete series of glacier elevation, volume, and area changes for the whole massif-volcano. However no information about the glacier area estimation.

Aerial photographs allow extracting surface area for each geodetic survey. These data were used to compute glacier-wide mass balances according to Equation 5. Ice-cap outlines and glacier boundaries were manually digitalized in stereo mode following the limits of the glacierized catchments. Thus, we estimate a total reduction of 42% in surface area for the whole ice cap, but also the surface area change for each period. These results were reported in Section 4.1, however, we agree with the reviewer and we include a specific figure of the surface area changes of each glacier in the supplementary material. Now, you can find in L192: *"Surface areas for each geodetic survey were manually digitalized in stereo mode following the boundaries of the glacierized catchments."*

- They also evaluate the effect of the morpho-topographic and climatic variables on glacier volume changes. However, in some sections, they mixed morpho-topographic-climate or vice-versa. In the title the use morpho-climatic. I suggest being consistent with the terms and clearly stating the variables evaluated.

Now is adjusted

Volume to mass changes conversion:

- The authors used one conversion factor (density) of ice volume change (850 kg m-3). However, very little discussion is associated with the choice of this number. Why just this value? Are the uncertainty ranges sufficiently? (±60 kg m-3). The authors also report that during the period 1965-1978 all the glaciers gain mass (moderate). Maybe it is possible to present density scenarios (e.g. Seehaus et al., 2019). For instance, a second scenario of two different conversion factors for areas below and above the ELA (e.g. Kääb et al. 2012).

Unfortunately, it is not possible to state density scenarios based on ELA because we do not know the ELA value at that time, however, we agree with the comment of the reviewer. In the new version of the manuscript we include two density assumptions (see Uncertainty analysis section). We also discuss in detail the implication of this assumption (see Results and discussion section).

You can now read: *"Second, regarding the uncertainty related to the density assumption, we analyze two extreme scenarios: First, we consider an average density recommended by Huss (2013) of $\bar{\rho}$ = 850 kg m^{-3} with a plausible uncertainty range of σ_ρ = ±60 kg m^{-3}. This value is appropriate for a wide range of conditions and when no information on firn pack changes is available (Huss, 2013; Zemp et al., 2013). However, moderate mass gains occurred in the second study period for which the conventional density assumption may not be true. Taking advantage of firn compaction data in two shallow core (mean depth ~14m) extracted from the summit of the Antisana volcano in February 1996 and November 1999, respectively (Calero et al., 2022; Williams et al., 2002), we propose a second scenario with an average density value of $\bar{\rho}$ = 564 ± 64 kg m^{-3}, indicating that the mass gain or loss was mostly comprising firn."*

We also include a section to discuss the sensitivity of the mass balance to the density assumption. Now you can read as follow:

"4.2 Sensitivity of the geodetic mass balance to the density assumption

In most of the geodetic studies, when there is no information available about changes in firn pack it is strongly recommended to use a conservative density value such as the one proposed by Huss (2013), especially in periods of mass loss. However, in our glaciers, the second period (1965-1978) is characterized by mass gain and a density value close to the density of ice could led to an overestimation of the mass balance. Assuming a density of 850 kg m^{-3} both in the accumulation and ablation areas for 1965-1978 period, the mass balance increase to 0.06 m w.e yr^{-1} which is within the uncertainty of the mass balance. In addition, during 1998-2009 period, seven glaciers in the Antisana ice cap are close to equilibrium with a slightly positive or negative mass balance no matter what density scenario is assumed. Given the small difference between both assumptions, we decide to apply an average density value of 850 kg m^{-3} when mass losses prevails, and when positive conditions are present we use an average density of 564 kg m^{-3} according observational data in the summit of the Antisana ice cap (Calero et al., 2022; Williams et al., 2002)."

Uncertainties:

- Overall, no details are mentioned about glacier area estimation or source. How did you obtain the glacier areas? How was the uncertainty of glacier mapping considered? No details about the uncertainty of the glacier area are included (not included in your error propagation equation).

We reply this in the first comment. We also include an uncertainty value related to the surface area glacier determination, now you can read in the Uncertainty analysis section: *"Fourth, uncertainty in surface area determination of glaciers σ_{Sg} considers a buffer zone of 1-pixel surrounding the glacier boundary (**¡Error! No se encuentra el origen de la referencia.**), and is computed by following the same approach as used to determine the uncertainty when no elevation measurements are available (see Equation 8) but replacing $S_{g.void}$ for $S_{g.b}$ which is the surface area of the buffer zone. "*

Specific comments:

Title: I am not fully convinced with your title. I would suggest restructuring the title since this study signifies the first long-term geodetic mass balance /volume change, and also because Antisana ice cap more than "iconic" is a benchmark glacier for the inner tropics.

In the sake of clarity the title is changed to *"New insights into the decadal variability in glacier volume of a tropical ice-cap explained by the morpho-topographic and climatic context, Antisana, (0°29' S, 78°09' W)"*

Abstract: Please provide numbers of volume or mass changes for this section. Strong and slight mass loss can sometimes be subjective.

Ok, numbers were included

21 -> what about the climatic variables

The description is given in L25

80 -> it seems that it was an important eruption in 1800. 370 -> is there any signal of geothermal activity in the Antisana glacier? This could explains the surge event?, at least it is a factor that is should be considered since is an active volcano (although its last eruption was in 1800). Is there any fumarolic activity?

At our knowledge there is was not volcanic activity reported over the past 400 years (personal communication from ML Hall, 2014). The volcano has been considered as a dormant volcano for more than a century and there is no evidence for geothermal activity or a local decrease in ice due to hot streams on the glaciers and the surrounding terrains (personal communication from P Ramon, 2014). Nevertheless, we cannot reject with 100% of certainty that surge event is not related to an increase in basal melt due to heat transfer from the volcano. Unfortunately, heat fluxes have not been measured to confirm this hypothesis. If geothermal contribution exist, this would be very local.

Following your comment and a pretty similar one by Reviewer 1, we added this hypothesis in the manuscript:

- In section 4.3: "*In the present case, it could be hypothesized that sub-surface heating enhancing basal melt might be part of the triggers of this surge event, but no volcanic activity has been evidenced*."
- In the Conclusion: *"To our knowledge, no similar event has been reported in the tropics to date, thus more research is needed before being able to conclude on the internal (ice-flow dynamics) or external factors (climate, sub-surface heating due to volcanic activity) that triggered such an event."*

115 -> did you scan the negatives? or how was the digitalization process for the aerial photographs?

Yes, we have all the films scanned, also we find by the chance the original calibration reports for the cameras. What is normally very difficult to access. We clarify this point, and you can now read *“The aerial films were scanned at 14 µm resolution using an Intergraph PhotoScan TD system. All the calibration reports of each sensor were available, this information is essential to reconstruct the geometry of the sensor at the moment of the aerial acquisition.”*

135 -> did you apply any correction (GCP points) to the Pleiades image? Some of the images present some displacement.

As is described in the manuscript, Pleiades imagery is oriented by using rational polynomial coefficients (RPCs) which were provided by the ancillary information of the satellite. A bias evaluation shows that Pleiades elevation data was displaced around 8 m above the ground relative to the 2009-Dem (i.e., geodetic reference), this is probably explained by the fact that the triangulation and geometric adjustment of Pleiades images was based on the RPC model without including GCPs. Thus, the Pleiades DEM had to be adjusted horizontally and vertically by performing a co-registration procedure proposed by Nuth and Kääb, (2011).

212 - 216 -> Please check this, you have included the internal ablation due to the heat transfer in the subglacial interface layer and due to heat released due to glacier dynamics in your uncertainties. However, I think this is not necessary. To my knowledge, the geodetic mass balance is providing the total glacier mass balance including internal ablation (Cogley et al., 2011). Hence the uncertainty from the geodetic estimation should be enough.

We agree with reviewer, the geodetic mass balance covers the internal and basal components of the surface balances. We are sorry for this mistake, we remove this part in the new version of the manuscript following your suggestions. We also update the uncertainty analysis section.

214 -> include the area error/uncertainty into your propagation equation. No details about the uncertainty of the glacier area mapping are included.

Please refer to our reply in the Uncertainties comments. We also update the uncertainty analysis section.

280 -> what do you consider as morpho-topographic features just an elevation profile? Please provide clear detail about the morpho -topographic -climate variables. In the title of your study, you just included the morpho-climatic. Please be consistent throughout the text.

The text was edited according your suggestion.

320 -> Table 4 -> the periods should be 1956-1965; 1965-1979, 1979-….etc…did you calculate the dhdt using these dates? You stated in line 237 that the time was not adjusted. I think that the results from â…period and 1956-2016 should be included in your uncertainty estimation as well (e.g. Brun et al., 2017; Menounos et al., 2019 -systematic errors-).

Is well known that the best period for carrying out aerial surveys in this part of the Andes is during the less rainy season, when glaciers are free of snow cover (i.e., September to January) (see Figure 1). Four geodetic survey (1956, 1965, 1979 and 2016) were carried out very close to the beginning of the hydrological year (i.e. from December to January), therefore, we assume a fix-date reference for these years. For the other survey dates conducted at floating-date reference (i.e., August 1997 and September 2009), the survey difference covers a time span of about four months. Only one small glacier G15α (0.28 km^2) has monthly mass balance observations dating from the mid-1990s (Basantes-Serrano et al., 2016). Although the glaciers are very close to each other, there is not possible to assume a linear mass balance evolution based on the mass balance rates of G15α due to the variability of mass balance from glacier to glacier.

Additionally, the sum of the glacier mass balance calculated from the five sub-periods ($\Sigma_{sub-period}$) does not correspond exactly with the mass balances calculated for the full period (1956-2016). Note that the full period covers from Feb-1956 to Dec-2016, thus we assumed a fix-date reference of 61 years. Unlike Brun et al., (2017) and Menounos et al., (2019), however, in our study each period begins where the previous one ends, therefore the discrepancy between the ($\Sigma_{sub-period}$) and the geodetic balance of the full period cannot be explained by differences in survey dates but mainly by data gaps.

To evaluate the systematic error due to the mass-balance processes occurred between the date of the geodetic survey and the end of the hydrological year, we consider a linear glacier surface evolution hypothesis based on the geodetic mass balance. Now, you can read: *"[…] therefore no time adjustment is is possible and we kept the original dates for the mass balance estimations, this is called floating-date reference. To evaluate the systematic error due to the survey difference $\sigma_{t.ref}$, we assume constant monthly mass balance rates at the glacier surface based on the geodetic mass balance. Then, the monthly mass balance is multiplying by the number of months to match the hydrological year. The uncertainty due to the time reference is evaluated as the residual between the annual mass balance at floating-date and the annual mass balance at fixed-date.*

371 -> It is a confusing sentence. Ice flow dynamics are also a response of climate variations.

This sentence was removed.

405 -> table 5 -> Just morpho-topographic? Please indicate what is Bm and 'Bm

Solar radiation was included in the caption. Now the declaration of the variables is in the caption of the table. Ok, the text is added to explain those variables *"Considering the mass balance, (Bm) is for all the glaciers in the ice cap and ($'Bm$) is excluding the outlier glaciers (G1a, G5 and G16)."*

I missed a comparison of your results with those from Braun et al., (2019) and Dussaillant et al., (2019). Although they used RGI_V6 glacier outlines to estimate volume change over a limited period, it is a good opportunity to check their number with more high-resolution data as you have shown here.

We evaluate the dh/dt coverage computed from ASTERIX technique by Dussaillant et al., (2019) and from this study to make a comparison for similar periods. This information was added in supplementary materials as Appendix S1. It is worth mentioning that this is an issue that is currently evaluated in hydrological terms for the entire ice cap and it will be presented in a future work. You can read now:

*"**Appendix S1.** Comparison with previous estimates of elevation change*
To evaluate the agreement between the elevation changes observed in this study and previous geodetic estimates from Dussaillant et al., (2019), we select a portion of 9 km2, in the western

side of the ice cap. See the Randolph Glacier Inventory (RGI) v6.0 for more details. This location was selected because of the limited number of data voids in both datasets (Table S2).

Table S2. *Average elevation change and percentage of surface area covered by dh-samples.*

Dussaillant et al., (2019)	***Data coverage (%)***	***This study***	***Data coverage (%)***
-0.95 m a^{-1} (2000-2009)	*75*	*-0.28 ± 0.06 m a^{-1} (1998-2009)*	*85*
-0.07 m a^{-1} (2009-2018)	*65*	*-0.17 m $a^{-1}\pm0.06$ (2009-2016)*	*84*
-0.28 m a^{-1} (2000-2018)	*99*	*-0.25 m $a^{-1}\pm0.06$ (1998-2016)*	*99*

For the full period (1998-2018) we found a good agreement in the elevation change rates, which is not the case for the sub-periods (1998-2009) and (2009-2018) where a noticeable discrepancy is observed (Fig S6). Unlike 1998-2018 period, the dh/ht maps obtained by ASTER imagery tend to be noisy during short periods when the data gaps are around 30%. Data gaps may be related to the presence of cloud or snow cover that prevent the determination of reliable elevation changes. These issues have been previously reported over large Patagonian ice fields (Dussaillant et al., 2019), and could also be the case for a region as humid as the inner tropics where Antisana ice cap is located. However, in spite of the difference observed in ASTERIX data, they are able to capture the similar trend of elevation change rate obtained by this study.

Negative conditions observed in Dussaillant et al., (2019) for the 2000-2009 period are suspicious and does not match with the conditions observed on Antisana glaciers in a similar period (1998-2009), this period has a 25% of data voids resulting in an underestimation of the mass losses."

Figures:

The figures are clearly meant to support the overall study, but they also present some issues for the reader. Figures 1 and 2 -> maybe both images can be merged, and the insert table can be inserted as a normal table.

Figures 1 and 2 present a lot of information. The first one is about the local context the second one is about the regional context. We believe that merging the two images can lead to a misinterpretation and to keep the clarity of the data we prefer to keep the images separated.

Figure 3 -> It is difficult to follow the colors. Is it possible to change the brightness of the plot? it is not possible to identify the colors (opaque). In the period 1956-2016, there are data gaps mainly in glaciers from G4 to G7. I was wondering how you managed the samples in these accumulation areas (gray). The same for 2010-2106 period.

We agree with the comment. In the new manuscript we update the color scale by increasing the colors to be able to distinguish the ranges of elevation change. The data gaps in the elevation difference layers are present in the upper reaches of the glaciers (accumulation zone), in areas with: i) low contrast (e.g., snow cover patches), ii) image saturation, iii) cloud coverage, and rugged topography (slope > 45°). Thus, we removed dh outliers considering a similar approach applied by (e.g., Braun et al., 2019; Brun et al., 2017).

We also evaluate the systematic bias due to data gaps, and we confirm that all the dh-samples are randomly distributed over the glacier. A new figure has been added in supplementary materials (Fig. S2). In the sake of clarity, you can now read in the Uncertainty Analysis section (point fourth): *"In addition, it is worth mentioning that the dh coverage for all periods are evenly distributed over the glacier surface, which reduces the likelihood of inducing some spatial biases in the quantification of glacier elevation changes (Fig. S2 in supplementary materials)."*

References

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