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,

Review 1 to Basantes-Serano et al. (2022): "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)" submitted to The Cryosphere.

The authors present a study of the photogrammetrically derived surface elevation changes of the Antisana ice cap. The surface elevation changes since the mid-20th century are separated into five subsets, each roughly representing a decade, converted into mass changes, and correlated to their morpho-climatic setting.

Overall, this manuscript is well structured and presents a very sound study. The research questions are laid out clearly, the methods are explained and applied properly. The significance of the study is high, because it extends our knowledge of one of the best studied glaciers in the tropics, a region where long-term glacier observations are particularly rare but highly demanded for calibration of glaciological and hydrological models.

I have some comments to the text, datasets and methods, which I assume to be a major revision.

We thank for your positive and useful remarks. In the new version, the manuscript has been updated following your comments.

My main points of concern are:

The coverage of dh-samples (L 178-184): I suggest adding a figure (supplement) showing the data coverage across the glacier hypsometry. I just wonder if the coverage is evenly distributed or if certain elevation bands or larger areas are systematically missing and the authors possibly introduce a bias.

We check for that possibility of systematic bias, 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)."*

The density used to drive elevation changes into mas changes (eq. 5). I'd like the authors to elaborate a bit more on the density used. I know, it is convenient and mostly enough to use the approach of Huss (2013), especially in periods of mass loss. However, the second period in this study (1965-1978) is characterized by mass gain and this raises the question of possible lower densities than 850 kg/m³, because it needs several decades to compact snow to ice. It would be worth to look into possible other sources of density (e.g. Williams et al., 2002).

Reply R1: We agree with this comment. The new version of the manuscript considers two density assumptions (see Uncertainty analysis section). One is a conservative scenario recommended by Huss (2013) for the periods of mass losses and a second scenario is based on observational data from Williams et al. (2002) for the periods of mass gains.

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)."

L 99: At what altitude is the maximum precipitation rate recorded? On tropical mountains the positive vertical precipitation gradient often reverses above a certain altitude. Is this the case for Antisana as well?

Gridded precipitation: It would be good to have some explanation why ERA5 precipitation was selected for the analyses. There are other gridded precipitation data sets like the PSL South America daily gridded precipitation, GPCC, or a recently published data set for the Peruvian and Ecuadorian water sheds (Fernandez-Palomino et al., 2021). Ideally, the authors add a figure (supplement) relating stations M003 and M188 to the gridded data. For example, show in two panels the time series of monthly air temperature and precipitation from a gridded data set (monthly box plots or shading ±1 standard deviation from a reference period) and the station measurements as lines.

The highest rate of precipitation (3,700 mm yr-1) is at 3,800 m asl in the western side of the volcano, to 7 km away from the ice cap (see Figure 1 in Ruiz-Hernández et al., (2021)). Based on ERA5 reanalysis Ruiz-Hernández et al. (2021) analyze the circulation patterns linked to the precipitation variability at 86 stations in the surrounding of the Antisana volcano. Authors shows a strong spatial variability of precipitation between the western and the eastern side of the cordillera. Note that Antisana ice cap is a transition zone between regions under Pacific influence and regions controlled by the atmospheric processes of the Amazonian basin. Thus, the annual precipitation in the inter-Andean valley varies from 400 mm yr^{-1} to 1500 mm yr^{-1}. Instead, the headwater on the Napo River displays some values of annual precipitation that may reach up to 6000 mm yr-1 on Amazon foothills. Also, the authors report a positive annual gradient of precipitation of 200 mm per km of altitude on the inter-Andean valley, which is in line with a hypothesis proposed by Basantes-Serrano et al. (2016) that could explain almost balanced conditions on the Antisana 15α Glacier for 1995-2012 period.

Consequently, ERA5 database was used since this new reanalysis benefits from several improvements in numerical weather prediction based on vast amounts of historical measurements. Likewise, ERA5 reanalysis allows us to interpret the climate behavior in a wide range of pressure levels over the region (see Figure 8). Moreover, ERA5 has a much higher temporal (since 1950 at daily time step) and spatial resolution (30 km) than previous global reanalysis.

The additional dataset mentioned by the reviewer are not suitable for the focus of this study. The following are some of the limitations:

1. RAIN4PE dataset was published by Fernandez-Palomino et al. (2022) as a result of the combination of precipitation data including ERA5 reanalysis among others data sources. Although RAIN4PE has a high spatial resolution (10x10 km), it covers only a part of our study period i.e., from 1981 to 2015.
2. Unfortunately, we did not have access to the PSL South America daily gridded precipitation dataset because the website is broken https://psl.noaa.gov/data/gridded/data.south_america_precip.html .

Now, you can read: *"This data set was selected because it well represents the long-term climatology of this region (Fig. S3 in supplementary materials), moreover it covers several geopotential levels and the entire study period."*

Comments:

Title: I suggest rewording the title omitting "new insights" and "iconic". The first, because apart from the surge of G8 I speculate nothing is really new, and I assume most results are an upscaling or a validation of assumptions based on earlier studies of G15 or other tropical glaciers. The latter, because it reads more lurid than scientific.

We agree partially with the reviewer, although our extended dataset allows to confirm a synchronous response between the glacier mass changes and climate in the long term, we consider there are some original results as the almost balanced conditions observed in the second period; a differential response between eastern and western glaciers, and of course the surge event in G8.

The title was adjusted, you can now read: *"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)"*

L 43: Here you could connect to Nicholson et al. (2013), who compare the micrometeorological conditions of small tropical glaciers.

Ok, the reference is now included.

L 154: Could you explain in a half sentence why the geometric characteristics were optimal?

The sentence was a bit confusing. In fact, it is better to mention the photogrammetric adjustment instead of geometric characteristics. You can now read: *"As the 2009 geodetic survey presents the best the photogrammetric adjustment…"*

L 180: Could you explain this spatial optimisation?

The geostatistical framework for spatial optimization of dh coverage is described in detail in Basantes-Serrano et al. (2018). Nevertheless, the procedure is described focusing on the sample design, the amount of measurements, and the type of model used to adjust the spatial structure of the dh. We encourage readers to review the geostatistical procedure in depth by referring to the article devoted to this approach. However, the new version of the paper includes a brief description of the method. Now, you can read: *"[…] relies upon the spatial variability of the elevation change to densify a sampling network to optimize the quantification of the surface-elevation change."*

L 206: I assume Scor needs a capital S.

Ok, corrected

L 248: I think you should state how many ERA5 grid cells cover your domain (Fig. 2).

For the analysis about the climate drivers, we use a grid cell over the station location, this is included in the text L 257. We believe it is not necessary to mention again to avoid redundancy.

L 313: termini (plural)

Ok, adjusted

L 350 and whole chapter 4.2. I think the two groups from Fig. 5 are confused with the two groups in the text. The figure caption says group I are the glaciers at the Pacific side. In the text it is the other way round. Please correct.

Ok, now the text in the caption agrees with the text in the description.

L 364: Is this mass gain (1998-2009) also detectable in the precipitation time series? (Another reason to add a figure about precipitation).

Figure 8 shows a positive anomaly of specific humidity that begins in the late 1990s and strengthens during the last few years. This anomaly is coincident with an increase in temperatures. This is mentioned in the text: *"In the period following the late 1990s - early 2000s, slightly negative or even positive mass balances were documented. In humid conditions, presumably more continuous cloudiness over the volcano helped reduce shortwave radiation at the glacier surface. On the other hand, precipitation and slightly colder episodes could have maintained the snow cover long enough to protect the glaciers from the energy available for melting."* We also mention a plausible effect under the presence of ENSO events during this period; SOI index displayed more frequent cold conditions which could contribute to reduce mass losses.

L 369: Please, elaborate on the role of subsurface heating as possible reason for the surge. L78 suggests that Antisana is an active volcano.

Although the volcano is considered to be active, there is no indication of volcanic activity 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, surface deformation 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 the 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, 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 over the past four centuries*."
- 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, *surb-surface heating due to volcanic activity*) that triggered such an event."

L 371/372: This is an odd sentence and consider deleting it. I even doubt the message is true, because the ice flow dynamics are a consequence of the climate variations, and especially at longer time scales glacier response times will be met. Then ice flow dynamics reflect climate variations. The reference (Thompson) is missing in the reference list.

Ok, the sentence was deleted.

L 400: This sentence is difficult to read. Consider restructuring.

In the sake of clarity the text was adjusted, you can now read: *"The higher explained variance when the very unbalanced glaciers were included may be due to the morphometrical characteristics of these glaciers, whose maximum altitude is below 5,300 m a.s.l., leaving a wider ablation area than the accumulation area, thus leading to a bigger reduction in surface area."*

Table 5: Explain Bm and 'Bm.

Ok, the text was edited to explain those variables *"Considering the mass balance for all the glaciers (Bm) of the ice cap and excluding the outlier glaciers (G1a, G5 and G16) ($'Bm$)."*

L 409: I suggest adding a few sentences on the concept of the reference surface balance (Elsberg et al., 2001; Harrison et al., 2005; Huss et al., 2012).

Ok, references were added concerning the reference mass balance: *"This exercise was inspired by the work on the reference mass balance at annual time step conducted by Elsberg et al. (2001); Harrison et al. (2005) and Huss et al. (2012), which were focused on the effect of climate in the glacier response without taking into account the change in geometry due to flow dynamics."*

L411: I don't fully understand why the mass balance should be overestimated in this case. When referring to a larger surface area the specific mass balance should become a smaller number, thus an underestimation as shown in L 414 and Fig. 7. Same problem in L 435 (underestimation). Maybe a clearer wording does the job.

Sorry for this mistake. We edited the text according your suggestion.

L 432: … many regional studies: Please add the references.

Ok, references are included: *(Braun et al., 2019; Dussaillant et al., 2019)*

L 470 et seq. and Fig. 8: Very interesting figure. Consider adding a note, that tropical glaciers are known to be particular sensitive on moisture/precipitation/clouds (Mölg et al., 2009; Prinz et al., 2016; Sicart et al., 2005).

In the sake of clarity the text was adjusted. You can now read: *"As it is well known glaciers in this region are particularly sensitive to humidity and as a consequence to precipitation and clouds (Mölg et al., 2009; Prinz et al., 2016; Sicart et al., 2005),"*

L 680: Consider deleting Hastenrath 1981.

Ok, adjusted

References:

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