

The Cryosphere Discuss., referee comment RC1  
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## Comment on tc-2022-70

Anonymous Referee #1

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Referee comment on "New insights into the decadal variability in glacier volume of a tropical ice cap, Antisana (0°29' S, 78°09' W), explained by the morpho-topographic and climatic context" by Rubén Basantes-Serrano et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-70-RC1>, 2022

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**Review** 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-20<sup>th</sup> 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.

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.

The density used to drive elevation changes into mass 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  $850 \text{ kg/m}^3$ , 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).

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  $\pm 1$  standard deviation from a reference period) and the station measurements as lines.

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.

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

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?

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

L 180: Could you explain this spatial optimisation?

L 206: I assume *Scor* needs a capital S.

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

L 313: termini (plural)

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.

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

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

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.

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

Table 5: Explain Bm and '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).

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.

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

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).

L 680: Consider deleting Hastenrath 1981.

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