Comment on tc-2022-166
Anonymous Referee #3

Referee comment on "Brief communication: Everest South Col Glacier did not thin during the last three decades" by Fanny Brun et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-166-RC3, 2022

Review comments on "Brief Communication: Everest South Col Glacier did not thin during the last three decades" by Fanny Brun et al.

1. General comments:

This paper reports the surface elevation change of a small (0.2 km²) Himalayan glacier located at a high elevation (~8000 m a.s.l.) for a period from 1984 to 2017. The analysis was performed by comparing two DEMs constructed from aerial photographs taken in 1984 and satellite images acquired in 2017. The motivation of the study is a recent publication (Potocki et al., 2022), which estimated an ice thinning rate of ~2 m a⁻¹ based on the analysis of an ice core drilled from this glacier and surface mass balance modeling. In contrast to the rapid thinning rate reported by Potocki et al., the DEM differencing showed little change in the surface elevation. To explain the two inconsistent results, numerical experiments on wind erosion of snow and surface mass balance were performed, as well as inspections of glacier surface conditions with satellite images. Based on the series of analyses, the authors concluded that ablation due to melting was overestimated by the numerical experiment by Porocki et al. (2022).

Considering the importance of glacier changes in the Himalayas as well as the unique location of the studied glacier, the estimate of ~2 m of ice loss every year at 8000 m a.s.l. has a large impact on the research community and society. Therefore, I appreciate the authors’ effort to inspect the glacier change with a different approach. I think the DEM analysis is reliable enough to exclude the possibility of such rapid thinning. Therefore, I support the swift publication of this manuscript on Cryosphere.
2. Concerns

(1) Numerical modeling

It is a good idea to report the result of DEM differencing as a short article. However, the manuscript is not really a “Brief Communication”. The effort of the authors is acknowledged, but in my opinion, this glacier is not suitable for numerical experiments using a model developed somewhere else. Moreover, the importance of snow erosion is clear on such a location even without numerical simulations. The satellite images tell us a lot more than the erosion model. My suggestion to the authors is to keep the modeling part as simple as possible. For example, experiments with shorter spatial and temporal resolutions of the COSIPY mass balance model nicely showed that heat conduction into the ice was possibly missed in Potocki et al. (2022). However, I am worried about the use of Crocus because the model is not validated in the extreme environment of the studied glacier. Why not simply compare the two COSIPY models to discuss possible shortcomings?

(2) Retention, refreezing and superimposed ice

I am wondering if the authors consider retention of meltwater in a firn layer or ice crucks, and subsequent refreezing and superimposed ice formation. I believe these are important processes related to melt in cold environments. Isn’t it likely that melt happens, but it refreezes and does not leave the glacier?

(3) Setting an “ablation area” (Line 273–289, Fig. A6)

The authors set a boundary of ablation and accumulation areas to assess the importance of glacier flow in the ice thickness change. However, it is odd to set such an imaginary boundary because the idea of accumulation and ablation zones does not work on such a small glacier. Further, the assumption of uniform emergence velocity (or thickening due to vertical straining) over the “ablation area” is not realistic. My suggestion is to estimate the velocity and its gradient from the ice thickness and temperature to confirm 2 m of thickening due to vertical straining is not possible at the coring site.
3. Specific comments

Line 19: “estimated that contemporary thinning rates — or ablation rates,” >> This is confusing. What was estimated by Potocki et al. (2 m a−1) is “negative surface mass balance”, I think.

Line 27: “Automatic Weather Station” >> automatic weather station

Line 32: “1.5 m a−1” >> Here and in other places, please make it clear if it is water equivalent, snow depth, or ice equivalent.

Line 76-77: This is already mentioned in Line 25.

Line 106: The terminology is not clear to me because: (1) erosion occurs after snow deposits on the surface and (2) precipitation includes snow drifting away before deposition. Why not like this?

- Precipitation: all snow falling on the glacier surface

- Deposition: snow attached to the glacier surface, a part of the precipitation

- Erosion: snow removed from the glacier surface after the deposition

- Accumulation: deposition minus erosion

Line 110-111: “… the most similar …” >> Are you talking about the inland of the Antarctic ice sheet? Isn’t it much drier than the studied glacier? I do not think high elevations in the Himalayan mountains and Antarctica are so similar.

Line 114: “offline nature” >> What do you mean? The erosion model is decoupled from the climate model?
Line 116: “as a function of surface snow density only” >> Wind speed?

Line 127-128: Not clear what “uncorrected precipitation” and “tuned estimates” are. Can you clarify the sentence?

Line 138: “falling snow is not eroded” >> It sounds odd because erosion occurs for deposited snow, but not for falling snow.

Line 142: “191 mm w.e.” >> Is this what you wrote in Line 128? If yes, please avoid repetition. Please also be consistent with the unit.

Line 149: “The wind erosion model is simple and has large limitations.”?

Line 150: “act as a negative feedback” >> It sounds strange to me that density increases as a function of erosion, because Equation A7 is not like that. Maybe, “regulate”?

Line 151: “snowfalls disappear” >> It sounds odd if you mean snow disappears from the glacier surface. “snow on the glacier disappears”?

Line 155: “predicted” >> “reproduced“?

Line 160: “eroded or re-mobilized after deposition” >> This is the correct use of “deposition”, but it is wrong according to the definition by the authors.

Line 162-164: Please revise this sentence because (1) it is self-evident that “deposition efficiency is not constant”, and (2) it does not imply “erosion is a major ablation process”, and (3) the last clause “that is not constant in time” is redundant.

Line 167: “thus integrate the surface energy balance over a much longer period” >> What do you mean?

Line 202: “15 min” >> Isn’t it 1 min as stated in Line 186?
Line 287: “velocity deformation” >> “velocity due to ice deformation”?

Line 293: “continental type” >> This sounds odd. I think ice flows slowly because the glacier is small.

Line 295: “incoming precipitation depositions” >> Is this term usual in glaciology? I have never seen it before.

Line 319: “impossible” >> Maybe “very difficult”?

Figure 4 caption Line 3: “predicted” >> “estimated” or “simulated”? 