Referee comment 3:

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:

[RC3-1] 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.

[ARC3-1] We appreciate the positive evaluation of our paper, and want to thank the reviewer

2. Concerns

[RC3-2] (1) Numerical modeling

[RC3-2a]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.
[ARC3-2a] We agree with the last sentence and please look at ARC1-3 for a detailed reply. We want to keep the communication as brief as possible, and some sections have been shortened such as all considerations concerning glacier flow, and ablation/accumulation areas. We also removed all simulations with Crocus and the referring text as well, to clarify the message and to keep the mass/energy balance modeling part as simple as possible.

[RC3-2b]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.

[ARC3-2b] We agree with the reviewer but still think that the deposition model and the Venµs images analysis complement each other well. Moreover, the model allows a rough quantification of the deposition efficiency at this high altitude site, which is valuable for our analysis. Regarding snow erosion, we thus prefer to keep both numerical simulations and satellite image analysis.

[RC3-2c]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?

[ARC3-2c] We agree with the reviewer's analysis and decided to remove all sections and simulations with the Crocus model. Indeed, Crocus is not evaluated in the extreme environment of the studied glacier, and we do not have any way to evaluate it. As a consequence, it does not provide relevant additional information compared to the sensitivity tests performed with Cosipy. Removing the Crocus section allows to keep the modeling as simple as possible, and to reduce the length of the paper to respect a brief communication format.

[RC3-3] (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?

[ARC3-3] We agree with the reviewer that refreezing, in a firn layer or deeper in the glacier, is likely to play a role in the case of surface melting in such a cold environment. Looking at refreezing is definitely an important point to accurately assess the glacier mass balance but it would require some specific model. Indeed, a precise quantification of the refreezing process is beyond the capacity of the simple modeling approach applied in this study, and would require a dedicated glacial hydrology model. This is beyond the scope of our brief communication.
Moreover, in our study, we intentionally applied the COSIPY model (with different numerical implementations) in an extreme and unrealistic case when ice is always exposed at the glacier surface, to maximize the mass loss in order to test whether Potocki et al. (2022) results are reliable. In such a case of an icy surface, the COSIPY model predicts that the totality of the melt water is evacuated from the glacier, and no refreezing occurs. This unrealistic configuration is useful to test whether Potocki et al. (2022) melt rates are robust but does not allow us to quantify refreezing or superimposed ice formation.
Anyway, our purpose is not to provide an accurate modeling of the SCG surface mass balance, but rather to highlight that the default COSIPY outputs lack robustness when applied to the SCG (see ARC1-3). We do not think that the inclusion of refreezing processes is necessary to achieve this goal.

A new sentence has been added in the revised manuscript to address this issue: "Additionally, even though there were some melt on this glacier, it is likely that a large amount of this meltwater would refreeze at the surface in case of the presence of snow or firn, or would form superimposed ice. While COSIPY accounts for refreezing in snow, in the case of pure ice all the melted water percolates and finishes as runoff , limiting its applicability in such a cold context."

[RC3-4] (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.

[ARC3-4] We agree with this comment and decided to remove all this section regarding ice flow, ablation and accumulation area (see ARC1-4 for a complete reply). We do not know how feasible it is to estimate the velocity and its gradient from the ice thickness and temperature since we do not have measurements neither of ice thickness nor of ice temperature.

<u>3. Specific comments</u>

[RC3-5] 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.
[ARC3-5] Agreed. We changed "ablation rates" into "negative surface mass balance"

[RC3-6] Line 27: "Automatic Weather Station" >> automatic weather station
[ARC3-6] Done

[RC3-7] 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.
[ARC3-7] Fixed

[RC3-8] Line 76-77: This is already mentioned in Line 25.
[ARC3-8] Agreed. We removed the L25.

[RC3-9] 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

[ARC3-9] We indeed use a different terminology in our paper, considering that deposition = precipitation - erosion compared to the reviewer's definition where our deposition = their accumulation, and we do not consider snowdrift. The semantics on this subject are not yet consensual and still vary greatly from one paper to another. Our main concern is that processes are defined without ambiguity for the reader. We agree to follow the terminology suggested by the reviewer, which may provide a clearer description of the accumulation/ablation processes. The manuscript has been adapted accordingly, and we added the following text at the beginning of section 3.2: "Hereafter we define accumulation as the snow that is deposited to the glacier surface (i.e. a fraction of precipitation) minus the erosion (i.e. the snow that is removed from the glacier surface after the deposition). The accumulation can thus be negative when erosion exceeds deposition."

[RC3-10] 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.
[ARC3-10] We agree that such comparison is subjective, and not very meaningful given that Antarctica has very different and diverse weather conditions, and that we do not have any quantitative comparison. We removed this sentence then.

[RC3-11] Line 114: "offline nature" >> What do you mean? The erosion model is decoupled from the climate model?
[ARC3-11] The snow erosion module in MAR is implemented directly in the surface scheme of the model, which is coupled to the atmospheric module. In our paper, instead of using a climate model, we extracted the physics from the surface scheme of MAR to compute erosion rates using reanalysis data as inputs, with no interactions with the atmosphere. In that sense it is an off-line approach. However, for clarification we rephrased as follows:e:
"Here we develop a simplified analytical approach expressed in a 1D vertical framework, in which the erosion model is only forced by the meteorological variables with no further interactions between the surface and the atmosphere."

[RC3-12] Line 116: "as a function of surface snow density only" >> Wind speed?
[ARC3-12] *Yes erosion rates are computed as a function of wind speed too. This is now specified in the text.*

[RC3-13] Line 127-128: Not clear what "uncorrected precipitation" and "tuned estimates" are. Can you clarify the sentence?
[ARC3-13] The sentence has been clarified as follow:
"However, instead of using artificially reduced precipitation (averaging at 66.9 mm a^{-1}) to implicitly account for wind erosion which is missing in Potocki et al. (2022), we prescribe uncorrected precipitation rates (averaging at 191 mm a^{-1}) as we intend to explicitly model wind erosion."

[RC3-14] Line 138: "falling snow is not eroded" >> It sounds odd because erosion occurs for deposited snow, but not for falling snow.

[ARC3-14] This is a good point, the sentence is ambiguous. By following the terminology suggested in comment RC3-9, it will naturally become clearer. We rephrased the entire sentence as “A large proportion of deposited precipitation is not eroded, and the accumulation efficiency gradually increases.“

[RC3-15] 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.

[ARC3-15] The bracket has been removed to avoid repetition, but we included the adjective “uncorrected” to avoid any confusion.

“… the annual uncorrected precipitation ranges from 147 to 259 mm w.e., and only 0 to 51% …”

[RC3-16] Line 149: “The wind erosion model is simple and has large limitations.”?

[ARC3-16] rephrased accordingly, thanks

[RC3-17] 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”?

[ARC3-17] Equation A7 is only called if erosion occurs: densification occurs when the model erodes, and an increase in density decreases the erosion rate. We followed your suggestion.

[RC3-18] Line 151: “snowfalls disappear” >> It sounds odd if you mean snow disappears from the glacier surface. “snow on the glacier disappears”?

[ARC3-18] Thanks, we accepted your phrasing

[RC3-19] Line 155: “predicted” >> “reproduced”?

[ARC3-19] Accepted

[RC3-20] 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.

[ARC3-20] We have adapted the terminology so this sentence is now right. Thanks.

[RC3-21] 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.

[ARC3-21] About (1), the temporal variability of erosion is mentioned in comparison to Potocki et al. who implicitly take into account the effect of erosion by reducing precipitation by a constant factor over time (see our response ARC3-13). About (2), we rely on the fact that a large part or all of deposited snow is eroded (which corresponds to periods of low to negative values of the deposition efficiency in Fig 3b in the original manuscript) to suggest that erosion is a major ablation process over the corresponding specific time periods. About (3), you’re right, this is redundant. We rephrased accordingly. The new sentence now reads:

“Second, the fact that the deposition efficiency is not constant in time, together with the low to negative values of the deposition efficiency at the end of the monsoon, suggest that (1) the temporal variability of accumulation cannot be properly resolved by reducing precipitation by a constant factor over time, and that (2) wind erosion is a major ablation process in this area.”

[RC3-22] Line 167: “thus integrate the surface energy balance over a much longer period” >> What do you mean?

[ARC3-22] This sentence was not clear, so we removed it.

[RC3-23] Line 202: “15 min” >> Isn’t it 1 min as stated in Line 186?
[ARC3-23] This sentence was removed.

[RC3-24] Line 287: “velocity deformation” >> “velocity due to ice deformation”?
[ARC3-24] Yes, you are right and it is changed accordingly. Anyway, this section has been removed.

[RC3-25] Line 293: “continental type” >> This sounds odd. I think ice flows slowly because the glacier is small.
[ARC3-25] This section has been removed.

[RC3-26] Line 295: “incoming precipitation depositions” >> Is this term usual in glaciology? I have never seen it before.
[ARC3-26] Not very usual in glaciology indeed. This has been changed into:
“A large fraction of precipitation (> 60 %) is eroded, limiting the accumulation”

[RC3-27] Line 319: “impossible” >> Maybe “very difficult”?
[ARC3-27] Changed accordingly

[RC3-28] Figure 4 caption Line 3: “predicted” >> “estimated” or “simulated”?
[ARC3-28] “Predicted” replaced by “estimated”