Dear Editor and dear reviewers,

We thank all the reviewers and the Editor for this second round of reviews. Below we provide a point-by-point response to all the comments. The original comments are in the blue colored cells of the table and our responses are in the white cells. The changes in the text are highlighted in orange font. We did some additional minor editorial changes to improve the clarity of the text, and added a figure (detailed in response to **R3C2** below).
Former lines 187 to 197 now reads as:
"From the analyses of the satellite images and the modeling of wind erosion, we conclude that large parts of the fallen snow are likely eroded or re-mobilized after deposition, adding a degree of complexity in the precipitation estimates. Potocki et al. (2022) used a stationary scaling factor to compute effective precipitation, whereas our analysis suggests that this should instead exhibit seasonality. The higher effective precipitation in the monsoon (from reduced wind speeds) that we find here would make it easier to re-establish a snowpack over the glacier -- something that Potocki et al. (2022) inferred was unlikely to occur in their 'ice' experiment. Hence, the South Col Glacier may not have thinned as Potocki et al. (2022) concluded because the high ice melt rates required do not have a chance to occur as the glacier surface remains snow covered throughout the monsoon."

We hope that we addressed all comments in a satisfying way and that the changes we suggest are acceptable at this stage of the revision process. We can make larger changes in the structure, and move some figures from the appendix back to the main text if needed.

All the best,
Fanny and co-authors

Handling Editor (Thomas Mölg)

[HEC1] Dear authors, After taking over this manuscript and studying the MS records, I am convinced that your article will be a valuable addition to the literature. Its evolvement represents an openness in the very best scientific sense, which is one of the essential quality flags of scientific research. I would like to thank all involved parties: the authors of this manuscript as well as the authors of the Potocki et al. (2022) study; the reviewers; the colleagues who contributed comments; and the former editor from who I took over.
Thanks for your positive appreciation of our work. We now also thank all the participants to the lively discussion and added the following sentence in the acknowledgement section: "We thank the editors (Francesca Pellicciotti and Thomas Mölg), the five reviewers (including Ann Rowan, Horst Machguth and Martin Lüthi), Enrico Mattea, Marcus Gastaldello, Nicolas Steiner and the authors of Potocki et al. for insightful and constructive comments and vivid discussions, which improved the quality of this manuscript."

Please consider some closing, minor remarks from the reviewers for preparing the final version of this manuscript (in particular, I would ask to think about whether the reference to "Antarctic blue ice" is necessary; one could simply call the albedo of the study glacier "blue ice-type albedo" and mention that a somewhat higher value is likely).

We removed the reference to Antarctic blue ice, and the sentence now reads as:
"Indeed, the ice of South Col Glacier appears very blue and might have a larger albedo than 0.4, as it is observed for blue ice-type albedo that can reach 0.5 to 0.6 (Smedley et al., 2020)."

Also, and as discussed with you, we would welcome a formal change of this manuscript, which means it should not be a "brief communication" (it is too lengthy in the meantime). I invite you to think about the following and change the manuscript type:
- Take brief comm. out of the title. If you want to indicate that this is kind of a comment on a different paper, you could start your title with "An alternative perspective on ..." and make it clear in the abstract what study you are referring to.

We agree that our manuscript does not compile with the "Brief communication" format. We changed the title, which is now: "Everest South Col Glacier did not thin during the period 1984-2017"

- You could make the abstract a bit longer, if you want.

We expanded the abstract to include an explicit reference to Potocki et al. (2022). It now reads as:
"The South Col Glacier is a small body of ice and snow (approx. 0.2 km^2), located at the very high elevation of 8 000 m a.s.l. on the southern ridge of Mt. Everest. A recent study by Potocki et al. (2022) proposed that South Col Glacier is rapidly losing mass. This is in contradiction with our comparison of two digital elevation models derived from aerial photographs taken in December 1984 and a stereo Pléiades satellite acquisition from March 2017, from which we estimate a mean elevation change of 0.01 ± 0.05 m a^{-1}. To reconcile these results, we investigate some aspects of the surface energy and mass balance of South Col Glacier. From satellite images and a simple model of snow compaction and erosion, we show that wind erosion has a major impact on the surface mass balance, due to the strong seasonality in precipitation and wind, and cannot be neglected. Additionally, we show that the melt amount predicted by a surface energy and mass balance model is very sensitive to the model structure and implementation. Contrary to previous findings, melt is likely not a dominant ablation process on this glacier which remains mostly snow-covered during the monsoon."

- In my opinion, you do not have to change the structure. You could add one sentence in the introduction, saying that this research evolved from a comment-type manuscript and, therefore, does not follow the common structure.

Kind regards,
Thomas Mölg

Handling Editor &
Co-Editor-In-Chief TC

Thanks for this suggestion, we changed the beginning of the last section of the introduction which is now: “In this article, we explore [..]. The structure of this article is rather uncommon, as it evolved from a comment-type manuscript instead of a stand-alone research.”

Reviewer 1

[R1C1] This submission led to an unusually lively discussion, which might even become a case study of how interactive journals can operate. The authors have responded robustly to review and community comments, and have revised their manuscript accordingly. I recommend that a final paper can now be published after copy editing for minor writing errors (a notable one is that “abusively” on line 220 should be “incorrectly”).
We thank the reviewer for their positive comment and modified L220 accordingly

Reviewer 2 (Martin Lüthi)

[R2C1] Dear collegues The scientific editor asked me for an independent assessment of this publication. I have not followed the debate and have not read all the arguments in the long discussion on TCD. Overall, I find this manuscript very convincing. For the purpose of the determination whether this glacier has undergone important geometry changes, Chapter 2 would be sufficient. Careful analysis of different DEMs that are referenced on surrounding bedrock show little to no change. This is a simple story, entirely conclusive and convicing. As an outsider (but with quite some experience working on and modeling high-altitude glaciers), I do not see any reason to debate this conclusion. If the methodology has been correctly applied (which I cannot judge without doing it myself), I think the results are solid and convincing. The arguments given in the Potocki paper are, on the other hand, less convining and likely incorrect. It seems that their dating of the ice core relies on a method with very high error bars, while they could not even detect the expected tritium peak in 1966 or volcanic SO4 layers that are commonly used to establish an ice core chronology. Mass balance modeling in such an extreme geometry cannot work. Extreme wind drift changes accumulation/ablation rates by huge amounts within 10s of meters, and surface geometry is mostly shaped by winds and sublimation, and not by the amount of theoretically deposited

snow. The strongest proof is Fig 1 in Potocki: bare rock and thick ice next to each other prove that any mass balance model is bound to fail. In my eyes, this glacier should be considered a huge, persistent cornice, almost entirely shaped by the action of extreme winds. Nevertheless, the author do an admirable effort to discuss and even to model many processes that are difficult in extended flat areas (such as an ice sheet), and nearly impossible in conditions like on a mountain saddle located in the free atmosphere and mostly above the weather systems. Given my hand-wavy assessment above, and conviced that any thickness change of 10s of meters would be easily visible on photographs (which apparently is not the case), I am conviced that this manuscript is a solid piece of work. Therefore I recommend that it be published after correcting a few typos mentioned below. Sincerely, Martin Lüthi
We thank the reviewer (Martin Lüthi) for his positive assessment of our manuscript.
109 fourteen 14 307 bergschrund (not ng)
Modified accordingly
Fig 4: Temperature in degrees C might make this more readable
Changed accordingly

Reviewer 3 (Horst Machguth, Enrico Mattea and Marcus Gastaldello)

[R3C1] 1. Introduction We thank Brun et al. for the revised version of the manuscript. We believe the revised manuscript reads well and concise. Basically, we only have a few minor comments. However, Marcus Gastaldello, an MSc student currently simulating firn processes at Colle Gnifetti (Switzerland/Italy) with COSIPY, came across a number of issues in the code of COSIPY. These issues have recently been communicated to the COSIPY developers. The model version used in the revised manuscript (and in the paper by Potocki et al.), however, might be affected by these issues. We highlight these issues below for Brun et al. being able to assess their relevance to the simulations.
We thank Horst Machguth, Enrico Mattea and Marcus Gastaldello for their positive comments and for their interest in COSIPY. We provide a point-by-point response to these comments

below.

[R3C2] 2. Known issues with COSIPY
The model employs an L-BFGS-B or SLSQP algorithm to iteratively solve the surface temperature for each time-step, by minimising flux residuals in the surface energy balance. This solver is constrained with an upper bound of the melting point of ice and uses the result of the previous time-step as an initial guess. Unfortunately, there appears to be a susceptibility for the algorithms to prematurely terminate on the upper bound of 273.16 K, before the actual convergence of the energy fluxes - particularly if the previous surface temperature value was at this temperature. Whilst this does not produce additional melt, since there is no positive excess energy, the reported surface mass and energy fluxes are incorrect. In addition, there is a missing pair of parentheses in the calculation of the ground heat flux: this issue was amended by the model developers in early August 2022, but was likely still present in the calculations of the paper by Potocki et al.

Thanks for reporting these issues. We are actually aware of both issues that we also reported to the developers of COSIPY. Our version of the model includes both bug fixes. We did a number of tests and found that they have no impact on the predicted melt rates in the case of our simulations.

We suspect additional changes in the code, as we did not manage to perfectly match the results from Potocki et al. (2022), as shown in Figure R1. We propose to add this figure to the appendix of our manuscript, as it shows the sensitivity of the predicted melt to the choice of the interpolation depth at which the subsurface/ground heat flux is calculated. It also shows some problems in COSIPY when this depth reaches the typical size of a near surface layer in COSIPY.

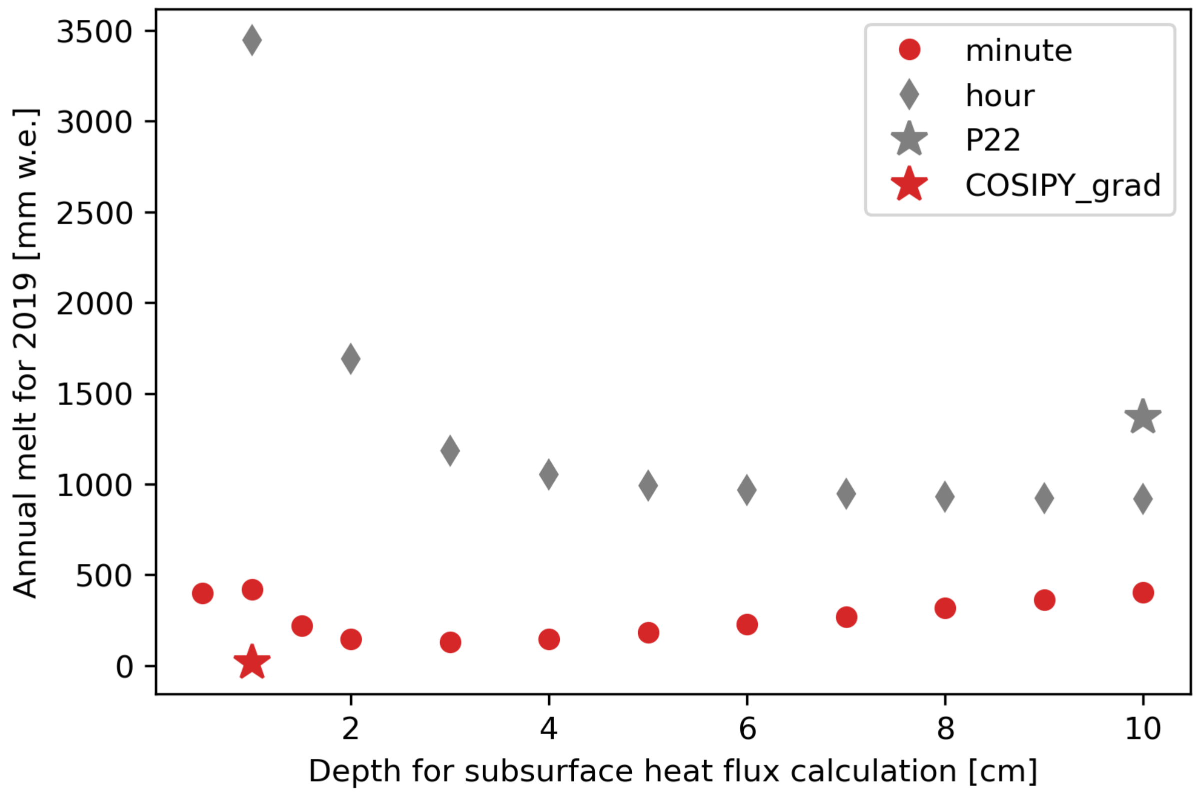

Fig. R1: Sensitivity of the annual melt for the year 2019 to the choice of the temperature interpolation depth (zlt2) in COSIPY. Note that the parameter zlt1 is set as zlt2/2. The red symbols correspond to simulations run at a minute time step and the grey ones correspond to simulations run at an hour time step. P22 is the original simulation from Potocki et al. (2022).

[R3C3] Thermal diffusion through the sub-surface layers is determined by resolving the Fourier heat equation with a second order, central difference scheme. However, the scheme uses a fixed/Dirichlet boundary condition on the basal node that effectively constrains the thermal regime to the user-defined, initial basal temperature, set in the constants file.

We are aware of this choice made by the developers of COSIPY. The boundary condition has no influence on the amount of surface melt predicted by COSIPY for the short runs (one year) we tested. However, we suspect that for longer runs the remeshing routine might introduce some unexpected behavior that likely breaks the energy conservation. In the figure R2, we show the mean temperature of the domain and the temperature of the bottom node of COSIPY simulations similar to the ‘ice case’ of Potocki et al. (2022). We do not fully understand how these issues are handled in the code, but they might create extreme diffusive fluxes at the boundary (especially at the ice/rock interface).

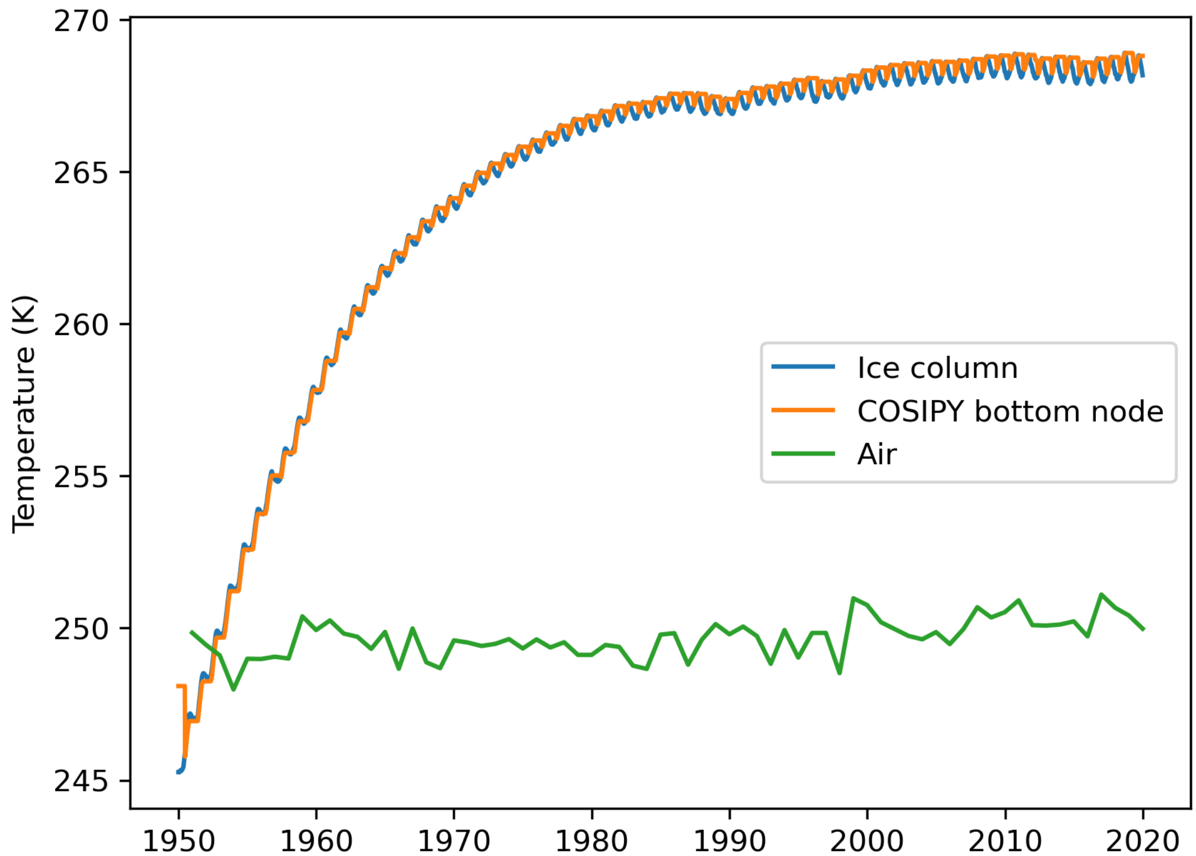

Fig. R2: Annual air temperature and temperature of the ice column and ice bottom sampled every 5 days at 6:00.

[R3C4] A volumetric approach is used to determine the composition of sub-surface layers, representing them in terms of a fractional proportion of ice, water and air. Within the refreezing module, energy is not properly conserved during latent heat release. The calculation of the internal energy increase of firn layers only accounts for the fractional mass of the converted water, as opposed to the whole layer. This results in a substantial under-estimation of the layers subsequent temperature increase. Furthermore, water is distributed to layers via a bucket approach constrained by their irreducible water content, prior to refreezing. This significantly restricts the true refreezing potential of sub-surface layers as it should be constrained by the cold content and volumetric limits of the layer.
We emphasize that the most critical of these issues, the erroneous calculation of refreezing in firn and snow, might be irrelevant to the ice-only simulations by Brun et al. and Potocki et al.

Thanks for highlighting these issues, we were not aware of them. We do not think that they impact our ice only simulations, but we will keep them in mind for applicability to other contexts.

[R3C5] 1. Detailed comments on the revised manuscript by Brun et al.
See the point-by-point responses below
Line 47: Correct us if we are wrong, but does the resolution depend on the location on the image (higher resolution for areas closer to the camera, coarser for areas further away)? This would be relevant in the extreme topography of the Everest range. Hence, if we do not misunderstand, 0.5 m is an average value? We suggest mentioning that 0.5 m is an average value.
You are correct, the resolution depends on the distance between the camera and the target. The 20 Dec. 1984 Washburn's Learjet flew at about 12 000 m a.s.l. The focal length of the Wild RC-10 camera is six inches (approximately 152 mm). The diapositives were scanned with a 1693 dpi scanner (in other words one pixel is 1.3 10^{-5} m). This means that for a point located 3 000 m below the plane (at Everest summit), the pixel size is approximately 25 cm, and it is 35 cm for a pixel located 4 000 m below the plane (i.e. at the elevation of South Col Glacier) and would be 50 cm for a pixel located at 6 000 m. We added "mean" to specify this point.
Lines 79-85: South Col Glacier is the main focus of the paper, so maybe its dH result should be described before the dH result of the rest of the Everest region? (Although that provides a nice contextualization). Up to the authors.
We prefer to keep the order as is to highlight that other glaciers in the Everest region are thinning, even at elevations higher than 6500 m.
Line 94: Suggest "in the thickness" instead of "of the thickness"
Modified accordingly
Line 109: Remove the number and leave "Fourteen".
Done, thanks for catching the typo.
Lines 119/120: Reword: " except at the on the lower cliff "
Done, thanks for catching the typo.
Lines 156/158: suggest removing "at" after "averaging"
Done
Line 220: "abusively" - while we agree with the concept, is this proper wording?
As suggested by reviewer 1, we replaced "abusively" with "incorrectly"
Lines 228-229: Could the Authors provide some more details on how the model initialization works when precipitation is switched off? Does the grid after spin-up consist entirely of impermeable glacier ice, during the whole simulation of 2019?

We start from an ‘ice case’ simulation, where the exposed surface consists of ice most of the time. In the restart file we used, the density is at 917 kg/m3 for all layers, confirming the presence of ice over the whole profile. We added this precision in the text: “At the start of the simulation the snow thickness is zero, and the domain consists only of impermeable ice.”
Line 280: “local mass balance rates approaching 2 m a-1,” Shouldn’t it read -2 m a-1?
Thanks for pointing this out. It should read -2 m a-1.
Line 282: Suggest writing “~2000”. Lines 288: As above.
Done
Line 293: Suggest adding the uncertainty.
Done
Line 298: suggest "mean density" instead of "density"
Done
Line 307: typo in "bergschrung", should read “bergschrund”
Done
Line 308: suggest "horizontal velocity" instead of "velocity" (possibly remove "horizontal" at the next line if it sounds too repetitive)
Modified accordingly
Line 333: suggest "or" instead of "nor".
Done
Figure 3: Suggest including "accumulation efficiency" in the legends of subplot b and c.
Done
Figure A4 still mentions a now-removed panel about Crocus.
Thanks for pointing this out, it is now removed.

Reviewer 4 (Ann Rowan)

We thank the reviewer 4 (Ann Rowan) for suggesting to accept our manuscript as is.