Reply to community comments

The following pages contain a point-by-point reply to the comments provided by the community.
Each of the community's comment (CC) is numbered. If a comment contained several points, we numbered them, and addressed them individually in our author replies (ACC).

Community comment 1

Response to Brun et al. re Potocki et al. (2022)*
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[CC1-1] In our paper (Potocki et al., 2022 – hereafter 'P22') there were two major findings: (1) that extremely rapid ice loss is possible once a protective snowpack is ablated away; and (2) this appears to have happened at the South Col Glacier (SCG) as evidenced by the presence of surface ice on the SCG dated at ~2000 years ago, indicating the loss of a significant portion of effectively what we consider to be at least currently a "stagnating glacier".
Brun et al. (hereafter B22) challenge both findings of P22. We welcome their paper and agree that more research, despite the extreme conditions involved in undertaking this research, is needed. Notably additional ice coring, mass balance stakes and an ice radar survey to more fully decipher ice dynamics and mass balance. However, we question the evidence used by B22 to underpin their conclusions. Our queries are outlined below – dealing first with finding (1) of P22 ("could the SCG thin so rapidly") and then finding (2) ("did the SCG recently thin"). Note that (1) is related to the surface mass/energy balance modelling of B22; (2) is concerned mostly with their DEM analysis. Accordingly, we organise our comment under the headings: "Energy Balance Modelling" and "DEM Analysis". Note that there is naturally some overlap between these sections.

[ACC1-1] We appreciate the detailed comment and discussion offered by Paul Andrew Mayewski. We agree with the description offered here that the Potocki et al. (2022) put forward two main findings, that are both challenged based on (i) our analysis of the COSIPY's implementation and (ii) our DEM analysis.

[CC1-2] Energy Balance Modelling
B22 conclude that substantial ice melt may not be physically plausible (even under extreme insolation). That is, they challenge the evidence that the SCG could thin at the rate proposed by P22. Their reasoning is that P22's conclusion is not robust to all modelling assumptions. Notably, if the conductive heat flux is calculated by the COSIPY model using a finer resolution near the surface (the 'grad' experiment), or using a different model (CROCUS) substantial ice ablation cannot occur even if a snowpack is removed. This is a very interesting result, but is it physically plausible? Can such high conductive heat fluxes be maintained if the sub-surface was warming so much? Might increasing the resolution only near the surface (and not at all depths) create a 'cold' bias? Also, the temperature profile used to initialize COSIPY might be inappropriate for the B22 experiment: it was the outcome of a spin-up under the P22 setup -- i.e., with a much-reduced conductive heat flux. If spun up with the P22 grad method, the sub-surface temperature profile would very likely be different. In other words, there's a physical inconsistency between the spin up and the B22 experiments.
In the context of the above, we (P22) note here that both the COSIPY grad and CROCUS model variants are completely untested in an environment like the SCG. Indeed, CROCUS, as B22 explains, is primarily a snowpack model. By contrast, P22 used the 'default' COSIPY model code (i.e., with the conductive heat flux computed using a less-fine resolution near the surface). The setup used in P22 has been tested and shown to perform well against observations, including on Zhadang Glacier (at >5,600 m) on the Tibetan Plateau (a cold and dry environment not too dissimilar from the SCG). Note that the agreement between COSIPY grad and CROCUS and divergence from COSIPY P22 noted by P22 may just highlight a shared weakness of those model configurations; it is not reassurance of their physical realism. It would also be very helpful if B22 explained which (if any) other COSIPY parameters were changed in their study, and if so, how results varied across those ensemble runs. That is, even if their model variants produce physically plausible results, B22 will only have demonstrated that the P22 conclusions are not robust to all (reasonable) modelling assumptions, but no real insight into just how non-robust they are. This is particularly important because P22 performed an uncertainty assessment, perturbing model parameters across a broad range of plausible values. P22's conclusion about the plausibility of substantial ice melt was robust across all scenarios considered.
Taken together then, we caution that B22 give the impression that their conclusions about the plausibility of substantial ice loss should be given as much weighting as those of P22. However, without further work by the B22 authors, this is not the case: (i) the ability of their models to capture the surface energetics at the SCG remains to be determined; and (ii) their uncertainty assessment is too narrow to give an adequate perspective on the robustness of P22's conclusions.

[ACC1-2] We think the goal of our modeling has been somewhat misunderstood. As detailed in the response to the other reviewers (ARC1-3 and ARC4-3), our goal is not to produce a better estimation for the melting of SCG, but to highlight a structural limitation of COSIPY that impedes its current application to SCG.

The difference between the COSIPY_grad and the original COSIPY configuration is not that the spatial resolution was refined near the surface. Rather, we modified COSIPY so that the computation of the sub-surface heat flux (abusively called ground heat flux in the COSIPY model description) used for the surface energy balance is performed as close as possible to the surface. This choice is not arbitrary as it follows from physics (e.g. Eq 4 of Sauter et al., 2020) and is the standard implementation in other skin-layer models (e.g. Covi et al., 2022). Note also the computation of the sub-surface heat flux appears as well for the computation of the temperature evolution of the first sub-surface layer (as a source of energy entering the ice from the surface). The COSIPY_grad version is thus internally consistent from this point of view.

Since (i) the default choice of COSIPY to compute the ground heat flux for the surface energy balance using the temperature in the first 10cm is not imposed by physics and (ii) that this choice has a large influence on surface melting, we conclude that no firm conclusions can currently be drawn from the use of COSIPY at SCG. Note that the sensitivity to the model structure and implementation is not specific to COSIPY, as shown by Machguth and Mattea [RC4-4].

We agree that the inclusion of Crocus simulations in our study blurred the message by giving the impression that the agreement between Crocus and COSIPY_grad should be understood as a proof of their correctness. We have removed all references to Crocus in the new version of our study and clarified the aim of the surface mass balance modeling section.

[CC1-3]2. DEM Analysis
B22 argue that, based on the difference in DEMs constructed for 1984 and 2017, the SCG has not thinned. We welcome this analysis, but have several observations that challenge their findings and overall interpretations:
[CC1-3a] (1) We note that B22's Figure 1 has our ice core at the wrong elevation. The ice core was not collected below 8000m but rather at 8020 m, which would place it within B22's so-called 'accumulation' zone.
[ACC1-3a] Please refer to ARC4-6 regarding the elevation of the ice core in Figure 1.

[CC1-3b] In addition, it is not clear why in their Figure A6, B22 define a particular line as the transition between accumulation and ablation based on a single day's image, implying this as the equilibrium line. Such a differentiation would be very sensitive to the timing of image acquisition and should instead be based on a much longer record of mass balance.
[ACC1-3b] Please refer to ARC1-4 regarding Fig A6 and the delineation of ablation and accumulation zones

[CC1-3c] As briefly acknowledged as a possibility (around L300 of B22), the steep snow slope designated as an accumulation area on Figure A6 is likely comprised of avalanche material (as evidenced below by the tongues of avalanched material) and is, therefore, not a standard snow accumulation region. The SCG surface downslope from the avalanche tongues is clearly exposed ice with patches of seasonal snow cover. Whether or not the SCG currently even has an accumulation area (at best very small) there is ice core and modeling evidence for current thinning/ablation up to at least the elevation (band) of 8020 m, which would be dominated by ablation indicating that the SCG currently has a negative mass balance. At lower elevation bands (than the ice core location) surface ablation/thinning rates of SCG is likely even stronger. The presence of clear banding and the identification of "annual layers" in the SCG ice core suggest that avalanching has not been the accumulation source for the SCG in the past, therefore something has happened, notably the transition to a stagnating glacier with its upper reaches comprised of avalanched snow.

[ACC1-3b] Our study indeed clearly challenges the statement that SCG currently has negative mass balances (since 1984) and Reviewer 4 provided additional evidence that there was no thinning of SCG since 1956 (see RC4.2). Please refer to ARC1-2 for the accuracy of our DEM differencing analysis. Our mass-energy balance modeling tests with different numerical implementations also suggest that there is no clear modeling evidence of large melt at SCG (see ARC1-3). We do not completely understand why the presence of banding would be an argument against avalanche dominated accumulation, because the deposition area of avalanches usually spreads out over a horizontal area, but we agree that the presence of exposed ice shows that parts of SCG are dominated by ablation processes. As the glacier has not been thinning for the last thirty to sixty years, it implies that ablation is fully balanced by ice fluxes in its current state. Given that the glacier is small and cold, ice fluxes have to be very small, and thus ablation is likely equally very small.

However, our DEM difference covers only for the most recent period, and thinning might very well have occurred between before 1984 (or 1956 according to RC4-2). We clarified this point in the revised manuscript (section 4): "Note that our present study focuses on the period 1984-2017 (or 1956-2022; Machguth and Mattea, 2022) where no thinning is observed, but we cannot exclude any thickening and/or thinning episodes anterior to this period, potentially explaining why Potocki et al. (2022) observed ice as old as 1966 years at the surface of their ice core."

[CC1-4a] (2) B22’s Figure 1 shows that there are regions both above and below 8000 masl on the north side of the SCG that reveal thinning and thickening up to 30m for the period 1984-2017. This seems at odds with B22’s statements that “... the distribution of dH on South Col Glacier is rather homogeneous and not different from the distribution of dH over ice-free areas or glacierized areas located within the same elevation range.” Indeed, according to B22’s Figure 1, dH over SCG is actually *at odds* with the *highly variable thickness change* over all other glacierized terrain at similar elevations. B22’s Figure A1 obscures this by averaging over very large gains and losses to show a mean dH close to zero (minor comment -- what is the uncertainty shading supposed to represent in the right-hand panel of Fig. A1?). In addition, it seems unlikely that the south-facing SCG would experience little to no ablation while large parts of the north-facing Rongbuk Glacier would experience significant (up to 30m) ablation as indicated in Figure A1. Indeed, the upper branch of the Khumbu – only ~250 m from the SCG and with a similar (SW) aspect – did thin by tens of meters according to Figure 1.

[ACC1-4a] As noted in [ARC1-2], we believe that the points raised in [CC1-4a] relating to the patterns of elevation change evident in the Western Cwm amount to misinterpretation of the dH grid we present in Fig.1:

- CC1-4a states 'there are regions both above and below 8000 masl on the north side of the SCG that reveal thinning and thickening up to 30 m for the period 1984-2017'. This pattern of elevation change is also present to the southeast and southwest of the South Col Glacier and is related to the advection of seracs and crevasses (which may have opened or closed over the study period) down slope by the flow of the Khumbu and Kangshung Glaciers. This pattern was also acknowledged by reviewer 1 (Ann Rowan) and further discussed in ARC1-2. Resp. Fig. 1 (ARC1-2) provides an example of the evolution of the crevasse field on the Lhotse face above Khumbu Glacier and the associated elevation differences caused by this process.

- The authors of CC1-4a state 'B22's Figure A1 obscures this by averaging over very large gains and losses to show a mean dH close to zero'. This comment is not applicable to panel A of Figure A1, which shows the distribution of *all* dH values (not average values) over the surface of the South Col Glacier and over stable, off-glacier terrain, between which there is very little difference. Panel B of Figure A1 provides an illustration of the variance of the dH data through the elevation range covered by the DEMs in the form of the Normalised Median Absolute Deviation (NMAD). This plot indeed shows that NMAD is highest between ~7200-7800 m, where elevation change patterns associated with advection of seracs and crevasses are common. The magnitude of NMAD over the elevation range covered by South Col Glacier is much lower (1.92 m), indicating elevation change of lower variance is predominant at this height. Finally, it must be noted that the averaging of dh values over elevation bands is not used to "obscure" any relevant signal as suggested by this comment, but in order to analyze the elevation changes in a way that is not affected by ice flow.

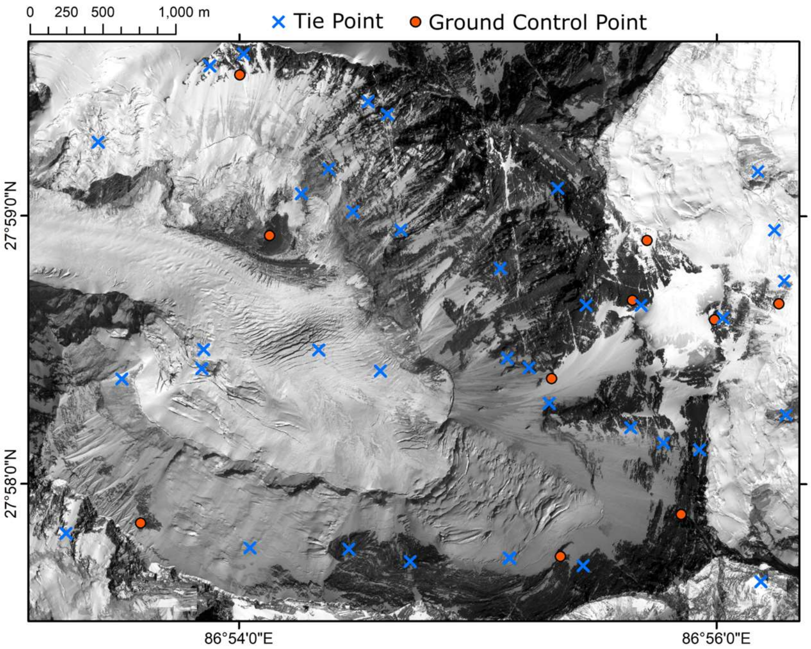
- The authors of CC1-4a suggest that 'it seems unlikely that the south-facing SCG would experience little to no ablation while large parts of the north-facing Rongbuk Glacier would experience significant (up to 30m) ablation as indicated in Figure A1'. We want to clearly state that our dH data do *not* cover the Rongbuk Glacier, which is on the northern flank of Mt. Everest, ~4 km from the South Col Glacier. The area of the dH grid highlighted here covers the upper reaches of the Kangshung face on the eastern side of Mt. Everest, which again is characterised by a large number of seracs and crevasses blocks, the movement of which has been captured in our pair of DEMs. The ~30 m elevation differences here are likely related to this ice flow, rather than a surface mass balance process.

We recognize that the initial version of this manuscript did not describe these patterns in a way that can be understood by a wider audience and therefore made several changes to section 2 to improve this, as summarized in ARC1-2.

[CC1-4b] We agree with B22's multiple mentions of the complications inherent with imagery interpretation for SCG and other high elevation regions and suggest that these might be hindering their Figure 1 results. We understand that The Pléiades DEM was generated from military-level satellite stereo images (0.5 m ground resolution). The only question is the accuracy of 1984 DEM which is not mentioned by B22. Our understanding is that the DEM was generated from images collected by an airplane at 10,000 feet (3048 m) above the top of Mount Everest according to the article from which the 1984 images were obtained. An aircraft under these conditions inevitably has vibrations induced by airflows. Even though the Wild RC-10 camera was aimed straight down, the vibration of the airplane would always introduce a slight angle to the camera's nadir looking direction. We do not know how accurately the nadir looking direction was maintained since no information about the image acquisition was provided. Assuming only a 0.5 degree departure from the nadir direction, then the error of location on ground is over 26 m (tan(0.5) × 3048 = 26.6 m). For Mount Everest, the 26.6 m of displacement could result in a several meter-level error in elevation due to the drastic change of topography within a short distance.

[ACC1-4b] Whilst we appreciate the concerns raised in CC1-4b regarding the accuracy of the derived DEMs, we find the comments aimed at the 1984 imagery somewhat unjustified.

The accuracy of the 1984 DEM is primarily dependent on the source of the Ground Control Points (GCPs) used to fix the location of the trio of images we used during DEM extraction. Here we use the Pléiades DEM and orthoimagery as the source of GCPs for the 1984 DEM given the lack of suitable field-based GCPs, for obvious logistical reasons. Response Figure 3 (below) shows the distribution of tie points (used to relate features visible in overlapping images to one another), 90 of which were placed over the overlapping area of the three aerial photographs, and GCPs used in the generation of the 1984 DEM (10 of which were used to relate the overlapping images to ground coordinates). The 1984 aerial photography and Pléiades imagery are of comparable resolution (0.5 m) and so the same features used as GCPs were easily identifiable in each set of images.



Resp.Fig.3. Location of Tie Points and Ground Control Points used during the extraction of the 1984 DEM from three overlapping aerial photographs. Ground Control Points were identified over features which we could confidently assume did not move between the dates of the 1984 aerial photograph acquisition and the 2017 Pléiades scene (i.e. off-glacier terrain).

Photogrammetric software such as PCI Geomatica (now CATALYST) considers the user-provided tie points and ground control points in a 'bundle adjustment', which calculates the position and orientation of the camera and the 3D position of points across each scene. The residuals from this process (Response Table 1) provide a summary of the agreement between the calculated positioning of points in the images and the provided ground control data. The RMS of the GCP residuals (in x, y and z-direction) are all low, which shows that our processing has closely matched the position of features identified in both the 1984 imagery and the reference Pléiades scene. These errors are significantly lower than those hypothesised in CC1-4b.

Res. Table 1. Residuals (in metres) of tie points and ground control points used during the 1984 DEM extraction process.

	RMS (X, Y, Z)	**X Bias (StDev)**	**Y Bias (StDev)**	**Z Bias (StDev)**
Tie Points (90)	0.410, 0.397, 0.290	-0.002 (0.411)	0.002 (0.398)	0.003 (0.291)
Ground Control Points (10)	0.763, 0.941, 1.927	0.099 (0.757)	0.054 (0.939)	0.357 (1.893)

Still, as an additional step to ensure the precise location of the 1984 DEM in relation to the Pléiades DEM, we undertook a subsequent coregistration procedure (described in the manuscript) to remove any remnant minor shifts. The magnitude of these shifts were calculated using off-glacier (stable) terrain dH statistics (see Nuth and Kääb, 2011) and were all less than 2m. This is a common procedure involved in geodetic glacier mass loss studies.

Finally, the reconstruction of surface topography through photogrammetry actually benefits from variations in stereo angle (i.e. departure from nadir pointing), where more than one 'perspective' of a feature can improve its reconstruction in a DEM. For instance, the Pléiades satellites can acquire three images in sequence (forward, near-nadir and backward) with variable stereo angles (sometimes >10°) which in combination provide superior coverage of DEMs over glacier surfaces (Deschamps-Berger et al., 2020).

Deschamps-Berger, C., Gascoin, S., Berthier, E., Deems, J., Gutmann, E., Dehecq, A., Shean, D., and Dumont, M.: Snow depth mapping from stereo satellite imagery in

mountainous terrain: evaluation using airborne laser-scanning data, The Cryosphere, 14, 2925–2940, https://doi.org/10.5194/tc-14-2925-2020, 2020.

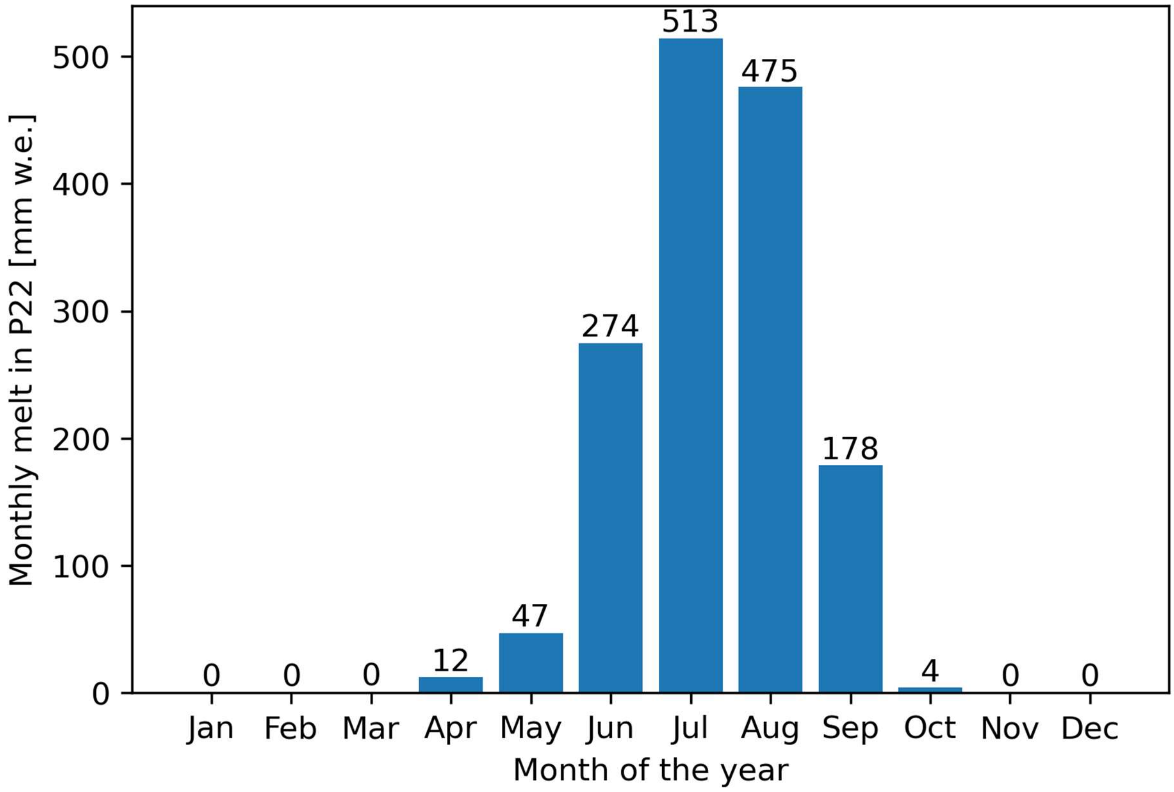
[CC1-5] (3) We find the information provided in B22's Figure A6 a bit perplexing. In particular, as stated by B22. ... "the shaded hashed area represents glacierized area that might belong to SCG, but it is not possible to conclude solely from satellite imagery." Might this not be part of the areal loss assumed in P22 over the last three decades? Clearly the image recognition for this region is still in question.

[ACC1-5] Figure A6 has been removed (see ARC1-4) and Reviewer 4 showed there was no frontal retreat since 1956 (RC4.2). We agree that our delineation of South Col Glacier remains disputable, especially in the southern edge of the glacier where the Pléiades imagery does not allow to clearly identify a glacier front. However, we remain confident that our outline of South Col Glacier covers most parts of the glacier.

[CC1-6] (4) B22 present an interesting display of seasonal snow variability (their Figure 2). This is used to argue for it being unlikely that the SCG is snow-free during the monsoon, although they do not in fact include any images from the critical months of May and June, before arrival of the monsoon. In turn, they reason that this helps explain their conclusion that the SCG has not recently thinned, because P22 required snow-free conditions during the monsoon to drive the widespread melting needed to ablate the SCG. We certainly agree with B22, that the SCG is covered by snow during the latter portions of the monsoon in August and September, but question their assertion that snow cover is present in May through early to mid-July when insolation is at its annual maxim. Albedo data from the South Col AWS confirm a largely snow-free surface from November 2019 through mid-July 2020 (Bessin et al. 2021). In addition, the three years considered by B22 may not be representative of longer term conditions. A similar view of seasonal variability during several earlier image periods would have been interesting to include. To this end, we note the availability of twice-daily images from Mt Everest's Basecamp in Nepal (see below) should help shed ever more light on this issue, given that changes in snow-cover above 8000 masl are clearly visible. Details of these photographs are in Grey et al. (2022).

[ACC1-6] This is strange that P. Mayewski and others claim that Brun et al. (2022) do not include any Venµs images in May and June although in Figure 2, there are images from 27 April 2019, 25 May 2019, 14 June 2019 and 2 July 2019. The whole image dataset (267 images in total, including images in May and June) from 27 November 2017 to 30 October 2020 is available to everybody in a Zenodo repository (Brun, 2022, cited line 89 of the original MS, and referenced line 462 of the original MS). We thus invite the authors of CC1-6 to look at all images available on-line.

Actually, Potocki et al. (2022) try to explain their ~55 m thinning over the last ~25 years with a change in surface state, from snow/firn covered surface to ice-exposed surface, implying a step change in melting due to the enhanced absorption of incoming solar radiation. Independently from the fact that the melt rates simulated by Potocki et al. (2022) over ice exposed surface are questionable (see ARC1-4 for the debate concerning the sensitivity of COSIPY to different numerical implementations), the series of Venµs images (Brun, 2022) clearly shows that SCG is entirely or partly snow-covered during long periods of the year, including periods when solar radiation is not maximum but still intense (the second half of the monsoon). P22 simulate annual melt rates that are on average 653 mm w.e. for the August and September months for the period 1950-2019 (**Resp. Fig. 4**). On average for the whole period, these two months contribute to 43% of the annual melt in P22. We agree that Venµs satellite has been operating only for 3 years (2017-2020) and this period might not be representative of longer term conditions, but we observe the same pattern of snow seasonality during these three years. Additionally, Sentinel-2 (2016 - present; https://drive.google.com/file/d/1ygZhRU5cHFK4CildxRojMNUrxGiYYIJB/view) and Landsat 5 images (1988 - 2011; https://drive.google.com/file/d/14EdWEJXWNKA2BlyXg0xmSqXIsFDK1n3k/view1) show the same temporal pattern for an extended period, and suggest that July is also snow covered most of the time. The Venµs images additionally show that P22 cosipy simulations do not reproduce the seasonal cycle of snow presence.



Resp.Fig.4.: monthly melt rates (mm w.e.) for the ice simulation of P22 for the period 1950-2020

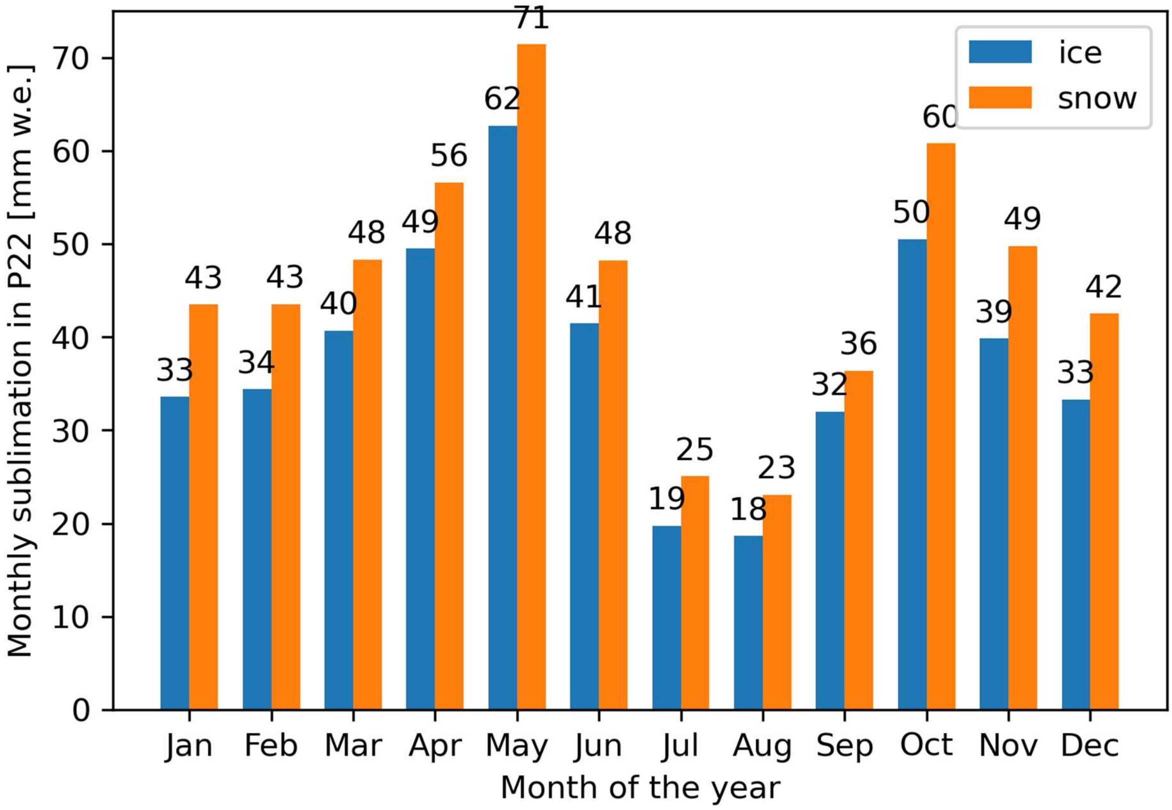
We do not see how twice-daily images taken from Everest Base Camp in 2021 can shed light of the long-term seasonality of the snow-cover over SCG because the glacier is not visible from base camp, is not comparable with the southern steep and rocky flanks of Mt Everest circled in red in the Figure shown in CC1-6, and the dataset is even shorter than that of Venμs images.

[CC1-7a] A related issue with B22's assessment of seasonal snow cover is their deployment of an empirical wind redistribution model. Several limitations of it are mentioned, but the most significant – and arguably so great that it should rule out its inclusion -- is that the SCG environment is so different from the (ice sheet) environment it has been tried and tested in. First, the SCG is in a very complex topographic setting and likely subject to very high small-scale wind variability due to shear-induced turbulence. This matters acutely when considering wind redistribution because maximum gusts set the upper limit on erosion potential. Second, the atmospheric pressure at the SCG is approximately one-third that of sea level. Even if air density is a parameter within their model, what evidence do they have that an empirical scheme developed and applied at much lower elevations/higher pressure behaves realistically in such a different environment? Taking the B22's own words (~L250 in the context of mass balance modelling) "[models]... developed and tested in specific conditions, ...[should not] be applied directly to other conditions, such as the very specific conditions of South Col glacier, without extensive validation."

[ACC1-7a] Here we agree that the sentence quoted by CC17-a regarding the applicability of models was insensitive and we rephrased it in the revised manuscript as follows: “We thus highlight here that COSIPY does not appear to be suitable to model mass balance in the specific conditions of South Col glacier without extensive validation”. However, we feel that the rest of the comment is a bit unfair, regarding the fact that model development is often an uncertain, but necessary, step towards a better understanding of processes, especially in the absence of field observations. The model presented in our manuscript is grounded on physics and we never claimed it to be accurate. We just think that it provides additional evidence, together with Venus images, that wind erosion is a non-negligible process. It stresses the fact that modeling surface mass balance without explicitly accounting for wind erosion, as done in any COSIPY simulation, will necessarily lead to results that are questionable. We modified the title of section 3.2, which now reads ”The potential of wind erosion”.

[CC1-7b] The point about the monsoon possibly being a time when the SCG is snow-covered seems to be made most strongly with the Venus images and basic physical reasoning about the wind speeds and precipitation occurrence. The empirical model is so uncertain that we argue it detracts rather than adds to this argument. Perhaps instead B22 would consider replacing this with modelling of the SCG surface mass during the monsoon. Forcing their COSIPY and CROCUS model variants with the P22 precipitation (which they inhibit in their ice model runs) should give more useful insight into the extent of snow cover during this critical time of the year. All lines of inquiry (Venus images, physical reasoning, and their empirical model) already identify the monsoon as a period of minimal wind deflation, so a more important question is to what extent monsoonal snowfall is melted or sublimated away – not least because all models (and both studies) agree on the very high importance of the latter.

[ACC1-7a] None of the current surface energy balance models (COSIPY in the configuration grad or P22 and Crocus) is able to represent the observed dynamics of snow cover during monsoon for South Col Glacier. There are many reasons behind this, but two important ones could be the large uncertainty on precipitation (see also **ACC1-7c** below) and the albedo parameterization. We do not discuss this in detail here, but it is clear in all simulations that sublimation is not playing a major role during monsoon, and especially during July-August, because of the very high relative humidity and low wind speed. Res. Fig. 4 shows the monthly values of sublimation for the snow and ice scenario of P22, which reaches a minimum during July-August.



Resp.Fig.5: monthly sublimation rates (mm w.e.) for the ice and snow simulations of P22 for the period 1950-2020

[CC1-7c] In the context of the above, B22 assert (around L165) that P22's estimates of precipitation are highly uncertain because they tune them to match ablation over an arbitrary period. We agree that they are uncertain, but note that the long-period of integration (10 years) protects somewhat against sampling variability. We also note that P22's precipitation estimate (mean of 191 mm a-1) was an order of magnitude greater than suggested by previous work (Salerno et al., 2015). There are reasonable explanations for that (including that the latter used poorly-shielded instrumentation to measure precipitation, hence a high risk of under-catch); but if B22 include our suggestion to model the SCG mass balance during the monsoon, we suggest that they keep this in mind: P22's precipitation estimates are unlikely to be biased low.

[ACC1-7c] We completely agree that precipitation estimates are extremely uncertain, however we would like to stress that the precipitation gradient is extrapolated over more than 3000 m of elevation difference in P22, which is very large given the variability of precipitation gradients in mountainous environments. Additionally, we want to caution about the precipitation calibration strategy applied in P22. First, as shown in B22, the surface mass balance model of P22 is very uncertain. Precipitation estimates calibrated with this model thus inherit these uncertainties. In P22, the annual precipitation estimate is balanced by annual sublimation, which is very sensitive to the choice of roughness length. Second, this calibration strategy does not account for wind erosion, which is likely a non-negligible phenomenon. Third, the tuned precipitation estimate is very sensitive to the modelled surface mass balance, and thus to the state of the glacier surface. In particular, it seems that the South Col Glacier was already presenting an ice surface during the 1953 British Expedition to Everest. This is shown by Alfred Gregory's photographs, which we cannot reproduce here for copyright reasons, but that are available in the book *Alfred Gregory: Photographs from Everest to Africa*, and online here: http://www.alfredgregoryphotographs.com/Ev14.html. Exposed ice would lead to a completely different surface mass balance in COSIPY simulations and hence a completely different precipitation tuning factor. As a consequence, it seems impossible to know whether the P22 precipitation estimates are biased high or low.

Gregory, A. *Alfred Gregory: Photographs from Everest to Africa*. Penguin Random House, 2008. https://books.google.fr/books?id=R8J8GQAACAAJ.

[CC1-8] (5) We believe that the P22 core was drilled in a stagnating glacier that has a seasonally reconstituted accumulation zone comprised of continually avalanching snow and ice. In this context, we note that the arguments invoked by B22 about implied low ice velocity being evidence of no/limited ice melt (due to low mass turnover) are not relevant; they are just another way of describing their (alternative) hypothesis – that P22 drilled their ice core from the ablation area of glacier in balance. Under the P22 stagnation (and thinning) hypothesis, there is clearly no requirement for the ice flux to balance the implied ablation!

We also emphasize that our estimated ice loss was not just a guess, it was based on the identification of annual layers in the 10m ice core (verified by seasonality similar to our other Himalayan ice cores), radiocarbon dating of the near top and the bottom of this core, and depth/age data developed from the Rongbuk Glacier ice core which we (along with our Chinese colleagues) recovered ~5km north of South Col Glacier at 6518 masl in 2002 (Kaspari et al., 2009).

[ACC1-8] All considerations about ablation/accumulation zones and ice flow have been removed (see ARC1-4). Actually, the point was to say that given that there is no thinning (as evidenced by Brun et al. (2022) DEM differencing or picture comparison provided by Reviewer 4 - see RC4-2), ablation rates as high as 1.5 m w.e./yr simulated by Potocki et al. (2022) are only possible if they are compensated by an emergence velocity of the same magnitude. Otherwise, if not compensated, it would result in thinning. We argue that such a large emergence velocity is very unrealistic for this nearly stagnating glacier.

We do not question the dating of the ice core, which we are convinced was done in a rigorous way, but to our knowledge, the dating of the bottom of the core is available neither in P22, nor on CCI repository (https://www.icecoredata.org/cci/Others.html). We only question the interpretation of this dating, especially in terms of thinning. The only ways to reconcile our observation of no thinning (which was also done in a rigorous way following state-of-the-art methodologies) and the ice core dating are 1) that ice melt was compensated by emergence velocity, or 2) that the ice core is in an area of near balance or slight ablation over the past 2000 years. Explanation 1 is very unrealistic for this small glacier while explanation 2 is coherent with the seasonal presence of blue ice at the surface, the windy conditions at the col and the wind erosion model we proposed.

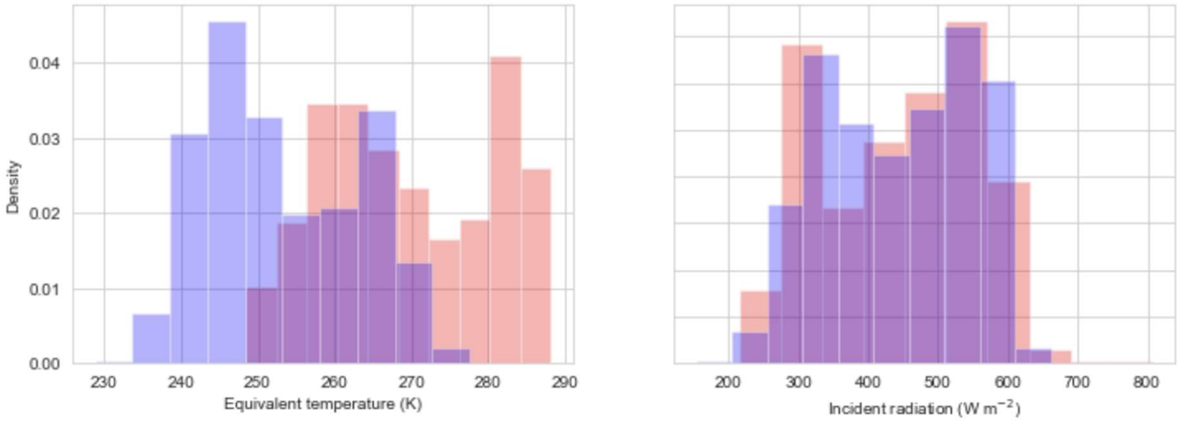
[CC1-9] (6) B22 assert that there is no evidence for any substantial ice melt having occurred due to an absence of fluvial features on the ice or off-glacier. The authors of P22 discussed this via email with the B22 authors, but some relevant parts of that discussion were omitted in B22: (1) the gently sloping SCG surface (below the avalanched region) would promote evaporation of meltwater, not least because of the extreme vapour pressure gradient to be expected with a surface so much warmer than the atmosphere above. The SCG is also very small, so we should not expect "large supraglacial stream features" (visible in satellite imagery) to form. For example, P22 proposed a potential (ice) melt rate of ~16 mm/d (assuming 1.5 m w.e. lost during a 90-day monsoon). If this depth (0.016 m) is multiplied by the 200,000 m2 area of the SCG it means a volume of 3,200 m3 evacuated per day, so a mean runoff of 0.04 m3 s-1. Given that much of this would be lost to evaporation, and possibly split between multiple streams, the potential for large supraglacial meltwater features seems limited. We also do not understand which "photographs of the glacier surrounds" B22 refer to when citing no "evidence of runoff, such as stones being embedded into re-frozen water" Such features would not be visible in satellite imagery; and re-frozen meltwater would in any case quickly sublimate at the SCG. We also explained that mountaineers described to the P22 team (prior to their 2019 expedition) that they could expect to observe meltwater at the SCG during the pre-monsoon (i.e., not even the period of maximum temperatures and insolation).

[ACC1-9a] Thanks for the considerations concerning the fluvial surface features. These considerations have been removed in the revised MS. Nevertheless, P. Mayewski and others say that, in case of 16 mm w.e./day of melt, most of the water would be lost by evaporation. This is actually not physically plausible, because this process would require a tremendous amount of turbulent latent heat flux. Indeed, to evaporate 16 mm w.e./day i.e. 16/86 400 = 0.0002 kg/m2/sec, this would require a mean daily latent heat flux of 0.0002 * 2.5 10^6 = 460 W/m2, which is unrealistic especially because the wind is light in the monsoon. The latent heat flux is at least one order of magnitude lower than this value (as stated by Tom Matthews as well - see CC2-1b), so most of the melt water cannot be lost by evaporation.

[CC1-9b] We also highlight that P22 suggest melting of an ice surface – if exposed – would occur during the monsoon, when equivalent temperature (proportional to the sum of the atmospheric sensible and latent heat content) is at a maximum, insolation is closest to its peak values, and when light winds would limit the potential for cooling of the surface via the turbulent heat fluxes. Unfortunately, very few people have ever seen the SCG during the monsoon – and that extends to the imagery shared by B22! However, we can make a space-for-time substitution. The authors of P22 have spent a combined total of almost one month at Camp II (6,464 masl) during the pre- monsoon (late April to late May) in 2019 and 2022. On both occasions, meltwater was abundant, with a significant supraglacial stream present on the northern margin of the Khumbu Glacier throughout (unfortunately we did not take pictures, but we estimate it several metres in width and tens of centimetres in depth). Following from the above, this is consistent with the much larger catchment area of the upper Khumbu compared to the SCG. In early May 2022 the team also observed a saturated snowpack at the base of the Lhotse Face (>100 m above Camp II; see the foreground in the image below taken during the 2022 expedition). This melting is occurring on a surface with a higher albedo than would be expected if ice were exposed at the SCG.

During the team's last days at Camp II in early May 2022, meltwater was particularly widespread (beyond the normal confines of the aforementioned stream), with it even being necessary to excavate channels to prevent the tent from being flooded (see image below). Just six days earlier Camp II was covered by ~10 cm of snow, and the maximum air temperature throughout this period of high melt remained well below freezing at -5C.

Critically, the potential melt energy is very similar between Camp II during late-April to late May, and the SCG during the monsoon (see figure below). Indeed, the incident (short- and long-wave) radiation (which, P22 suggest, drives the melting at the SCG) is almost identical between sites (right-hand histogram). Note that the difference in equivalent temperatures (left-hand histogram) becomes ever-less important as wind speeds drop. This point relates very clearly to those raised in the energy-balance section above. That is, this space-for-time substitution suggests that, if abundant melt is evident *above* Camp II *during the pre-monsoon*, it would be reasonable to expect a similar response at the SCG during the *monsoon,* given that the former seems to be a very appropriate (perhaps even conservative) analogue for the latter (given the lower albedo of the ice at the SCG).



Above: Comparison of equivalent temperature (left) and incident radiation (incident shortwave radiation plus incident longwave radiation; right) at Camp II (red) during the pre-monsoon (last week in April to the last week in May, 2019 and 2022) and the SCG (blue) during the monsoon (July and August, 1991-2020). Note that the Camp II data were taken from the AWS at 6464 masl, and the SCG data were taken from the P22 ERA5 downscaled data (to the SCG AWS at 7,945 masl).

[CC1-9b] See RC1-15 and ACC2-1b for replies to the space-for-time substitution experiment. Actually, we welcome the observations made in CC1-9 about ice melt at Camp II, which provide clear validation of our dH data. Panels d-f of Resp. Fig. 1 (ARC1-2) show the changes around Camp II between 1984 and 2018, as captured by the aerial photographs, Pléiades scenes and subsequent DEM differencing. The majority of the ice loss here has occurred from the lowermost parts of the steep hanging glaciers above the Camp II, which have receded to expose a greater area of bedrock directly north of the moraine on which the camp is placed. The portion of Khumbu Glacier proximal to Camp II has experienced some slight surface lowering over the study period (~10 m, or 30 cm/year), with any higher magnitude changes in elevation again restricted to steeper areas of the Western Cwm affected by extensive crevassing.

[CC1-10] Taken together, point (6) indicates that B22 do not provide convincing evidence that substantial ice melt has not occurred at the SCG.

In closing, we highlight that P22 and now B22 have taken very different approaches to the study of the iconic SCG. They also reach different conclusions over whether (1) the SCG *could* thin rapidly (if ice were exposed), and (2) whether it *has* thinned rapidly.

We argue here that B22's findings – which challenged those of P22 on both counts -- are more uncertain than presented by the manuscript in its present form and should be re- examined before their paper is published.

[ACC1-10] Thanks for the interesting discussion P. Mayewski and others provided. We agree that Potocki et al. (2022) and Brun et al. (2022) have taken different approaches and reached contradictory conclusions regarding the recent thinning of SCG. We have tried to address all comments as well as possible. The main finding of Brun et al. (2022) is based on DEM differencing showing no thinning since 1984. And we hope that our tests concerning the sensitivity of Cosipy outputs to the different numerical implementations at SCG have raised some awareness about the quantification of melt in such a high elevation site.

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