Referee comment 1 - Ann Rowan

[RC1-1] The brief communication by Brun et al. (2022) "Everest South Col Glacier did not thin during the last three decades" presents measurements of the mass change of Everest South Col Glacier between 1984 and 2017 from differencing of digital elevation models, and compares these to results from a set of sensitivity experiments using the COSIPY and CROCUS models. The manuscript is written in response to a paper by Potocki et al. (2022) which calculated mass loss from this glacier of 1.5 m w.e. a–1 from analysis of an ice core and the COSIPY model. The Brun et al. (2022) study finds that mass change for this glacier is within uncertainty of zero. Everest South Col Glacier is a small (0.2 km2) ice mass at 8,020 m a.s.l. on the southern side of Sagarmatha, located above the headwall of Khumbu Glacier and on the climbing route for this mountain from the south. While it is rare that so much effort is dedicated to determining the mass change of such a minor glacier, the location at extremely high elevation is used to justify the attempt with the implication that if glaciers are losing mass at present at the highest elevations in the Himalaya, then widespread mass loss is expected at all elevations.

In the first part of my review, I make several major comments and some minor (editorial) comments on the work by Brun et al. (2022) which I request that the authors address in revising their manuscript. In the second part of the review and because this work is a response to a previous study, I compared and evaluated the results of both papers. The authors of Potocki et al. (2022) wrote a response to Brun et al. (2022) in this discussion, led by the second author rather than the more junior first author, which I discuss in the second part of my review and refer to as Mayewski et al. TCD.

[ARC1-1] We would like to thank Ann Rowan for her thorough review, and the evaluation of both papers, Potocki et al. (2022) and Brun et al. (2022)

Review of Brun et al., 2022: Major comments

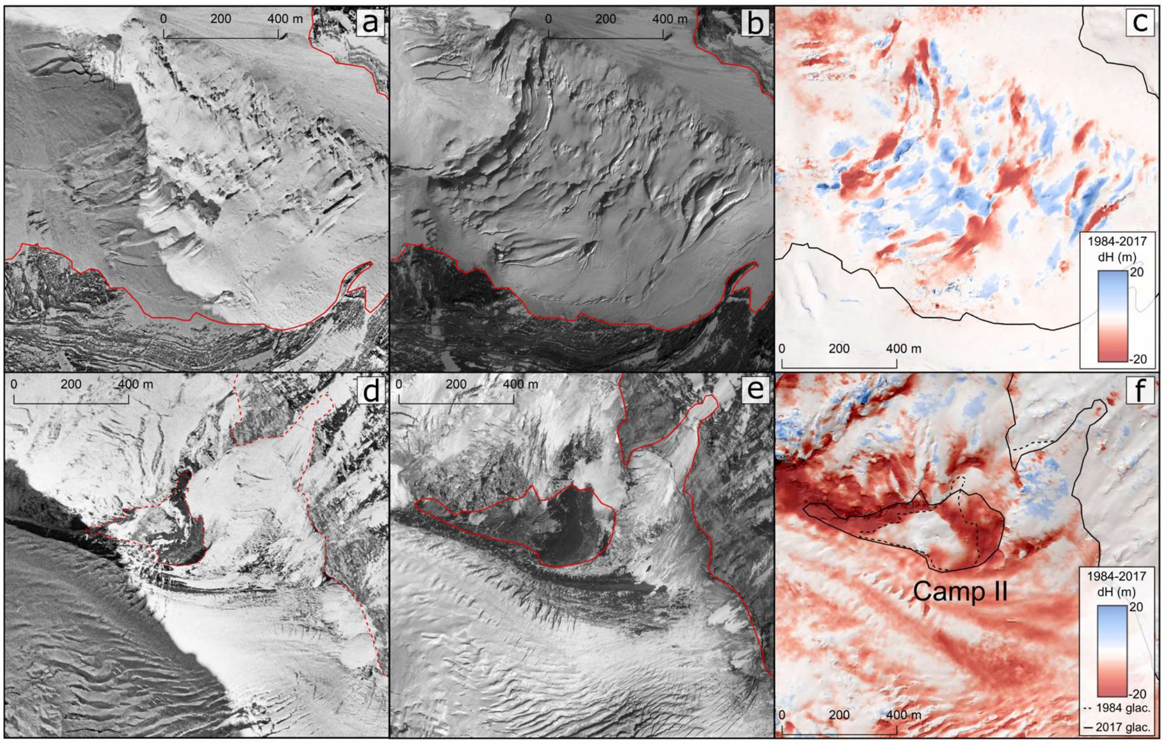
[RC1-2]

1. Glacier mass change data. Brun et al. first present an analysis of DEM data to demonstrate that no mass loss has occurred from Everest South Col Glacier between 1984 and 2017 CE. This method is well established and has been thoroughly tested for glaciers in this location, notably in the recent paper by King et al. (2020) of which one of the co-authors of Petocki et al. (2022), Alexander Tait, is also an author and therefore aware of the method. It would have been justified to end the paper after these analyses, as the results are convincing and of greater value than the modelling for the reasons outlined below.

These results could be described in more detail to make this section more accessible to a wider audience. Mayewski et al. TCD have interpreted areas of negative surface elevation change in the upper accumulation area of Khumbu Glacier as mass loss indicative of glacier wide mass balance rather than redistribution of mass within the glacier.

[ARC1-2] We appreciate the reviewer highlighting the previous successful use of optical stereo imagery to examine glacier change in the region. In this study, we examine imagery of higher resolution (0.5 m) than in many previous long-term studies in the region, to ensure we are able to generate accurate DEMs over glacier surfaces which may prove problematic to lower resolution sensors (e.g. ASTER). By doing so, we have built on previous work and have been able to examine glacier change in a more precise manner.

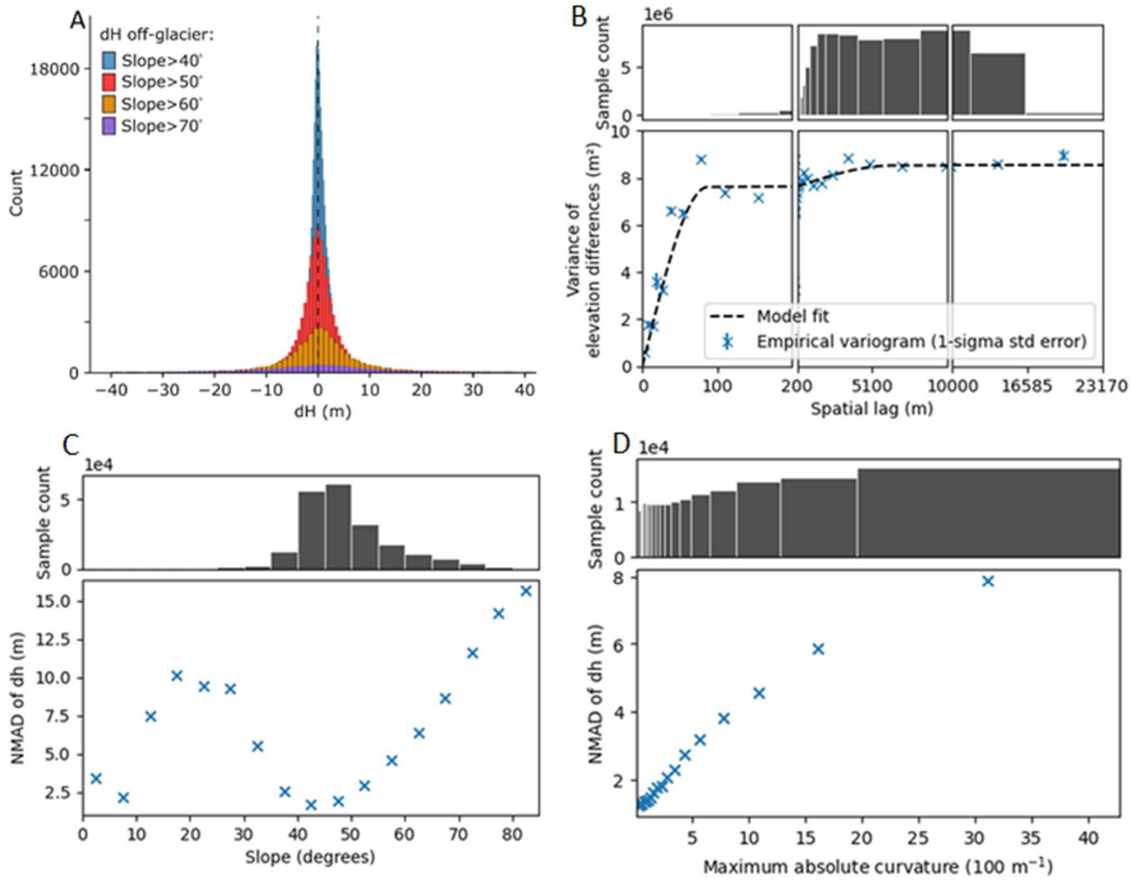
We agree with the reviewer that some areas of the dH data shown in Figure 1 have been misinterpreted as "ablation" (comment CC1-4) when they in fact show the advection of surface features of substantial relief (seracs and crevasse blocks) down-glacier by ice flow. Such features and associated patterns of elevation change are widespread on the Kangshung face directly north and east of the South Col Glacier and southwest of the South Col Glacier towards the base of the Lhotse face (Response Fig. 1).



Resp. Fig. 1: Examples of glacier surface conditions captured by aerial photographs (1984) and Pléiades imagery (2017) in the Western Cwm and associated changes in surface elevation. Top row: Crevassing of the Lhotse face in 1984 (a) and 2017 (b) and corresponding elevation change estimates (c) over the same period. The alternating positive and negative elevation difference pattern reflects the movement, opening and/or closure of crevasses. Bottom row: the expansion of the area of exposed bedrock around Camp II between 1984 (d) and 2017 (e) and associated elevation changes (f). Shadows prominent in panels a & d illustrate the winter time acquisition date of the aerial photographs, compared to the Spring (23rd March) acquisition of the Pléiades imagery (panels b & e).

We now detail these elevation change patterns at the end of section 2: "Several patterns of dH are evident over the Western Cwm and South Col Glacier surroundings which relate to both ice flow and surface mass balance processes (Fig. A1). Thinning and recession of the steep hanging glaciers on the north face of the Western Cwm is evident north of Camp II at an elevation of 6500 m a.s.l. (Fig. A1 panels d to f). Slight (10 m or less) thinning is evident over the Khumbu Glacier up to an elevation of 7000 m a.s.l. Above this height, substantial elevation change is limited to areas where ice flow has driven crevasse field evolution between the two DEM dates, primarily on the Lhotse and Kangshung faces, to the east and southwest of South Col Glacier (Fig. A1 panels a to c). Over the South Col Glacier specifically, we find a mean elevation change of 0.01 ± 0.07 m a−1 for the period 1984-2017. The distribution of dH on South Col Glacier is rather homogeneous and not different from the distribution of dH over ice-free areas or over glacierized areas located within the same elevation range (Fig. A2).”

To better illustrate the characteristics of the elevation change (dH) data over the areas highlighted in **CC3-1**, we examined dH estimates over stable ground and the steepest sloping surfaces in the study area (>40˚), where lower resolution DEMs can indeed sometimes struggle to accurately reconstruct topography, and where any positional mismatches between DEMs would be most apparent (**Resp. Fig. 2**, panel A). Over surfaces which should be expected to have been stationary between the two DEM dates (off-glacier) *and* of a surface slope greater than 40˚, the mean elevation difference is -0.04 m (n = 352553, standard deviation 4.5 m). For surfaces of a slope greater than 50˚, the mean dH is 0.21 m (n = 96285, standard deviation 6.55 m), greater than 60˚ it is 0.55 m (n = 32082, standard deviation 9.34 m), and for slopes greater than 70˚ the mean is 0.31 m (n = 7381, standard deviation 13.2 m). Close agreement between the two DEMs over stationary surfaces, particularly over steep slopes, confirms the absence of positional errors between the two datasets.



Resp. Fig. 2: A) Distribution of elevation change values off-glacier as a function of the terrain slope, for slopes higher than 40°. B) Correlation of elevation change data over different spatial scales. C) Relationship between NMAD of dH and surface slope, derived from the Pléiades DEM. D) Relationship between NMAD of dH and terrain curvature, again derived from the Pléiades DEM.

Furthermore, considering the above and the comments of RC1-2 and CC3-1, we have undertaken additional work to more thoroughly examine the uncertainty associated with our elevation change data, specifically considering the relationship between error and terrain characteristics such as slope and curvature. To do so, we followed the methodology developed by Hugonnet et al. (2021) (and more thoroughly described in Hugonnet et al. (2022)), implemented in the xDEM Python package (https://xdem.readthedocs.io/en/latest/). TWe estimated and modeled the structure of error of our elevation change data by examining the spatial correlation of errors (scale of spatially consistent noise) and the heteroscedasticity of elevation changes (per-pixel variability in error), using stable terrain (off-glacier surfaces) as an error proxy. Then, we applied this model of error to spatially propagate elevation change errors to the mean elevation change in the area of South Col glacier.

The heteroscedasticity was estimated using both the NMAD and half of the 2.5-97.5 percentiles of elevation differences, keeping the latest one as most conservative (**Resp. Fig. 2** panels C and D). Indeed, in addition to the typical error variability found with slope and curvature (gradual increase), we also observe an unusual increase in error at moderate slopes (15-35°), better captured by the percentiles. We interpret this increase in error as being related to the moderate-slope location of the sampled pixels which, for the most part, is snow covered and thus has low contrast in the stereo-images leading to lesser precision in the DEMs. While, ideally, this variability could be modeled using a DEM quality variable (e.g., quality of stereo-correlation), none was generated in the present study. Thankfully, the dependency of snow cover to slope (which is approximately the same on stable and glacierized terrain) allows us to account for it through slope. To ensure the reliability of our approach, we verified on a Q-Q plot that our error model explains almost all of the departure from normality of the elevation change distribution, meaning that almost all of the variability in elevation change error is captured by our method.

The spatial variogram was estimated using a robust median estimator and modeled with a multi-range spherical function (**Resp. Fig. 2** panel B). We found that elevation change errors are correlated at 89% (in variance) until a range of 87 m, then correlated at 11% until 6.5 km, after which they are completely decorrelated. These findings match the scales over which stable ground elevation change data is visually correlated within the dH grid of Figure 1 in the main manuscript.

Eventually, we calculated an average per-pixel error of SCG (accounting for slope and curvature variability) of 5.10 m, slightly larger than the 3.88 m average dispersion on stable terrain, because SCG is located on bright, moderate slopes that have larger errors. This leads, after spatial integration, to an uncertainty in the mean elevation change of 1.77 m (at 1-sigma level) for South Col Glacier (see notebook with more details here: https://github.com/rhugonnet/uncertainty_analysis_SCG/blob/main/uncert_SCG_from_Hugonnet2022.ipynb

We note that this improved uncertainty estimate is slightly smaller than the one shown in B22 (2.2 m), which was thus conservative.

We also note that our photogrammetry-based elevation change assessment is now backed up by the analysis of in situ photographs taken by Horst Machguth and Enrico Mattea. (see RC4-2)

[RC1-3] 2. Surface energy-mass balance modelling. Calculating the surface energy balance of a glacier in this setting is extremely challenging, as extreme winds strongly affect the accumulation and removal of snow from the glacier surface, melt processes are dominated by sublimation, and the influence of the Indian Summer Monsoon on glacier mass balance is unknown at these elevations. I consider this glacier an unsuitable candidate for any surface energy-mass balance modelling study unless a model was developed specifically for this location and constrained by detailed and representative atmospheric and glaciological data (i.e., collected at the site of this glacier over several years, rather than using empirically derived values from other settings).

Brun et al. (2022) have taken a pragmatic approach by reproducing the COSIPY model parameterisation used by Potocki et al. (2022) in a sensitivity test that considers a graduated mass balance parameterisation of the same model and a comparison with results from a snow model, CROCUS. Their results demonstrate that the simulated glacier mass change is sensitive to the model time step used, and that there are large uncertainties associated with such calculations. The model results are useful as a comparison with the approach of Potocki et al. (2022), but I suggest that the modelling work from both papers is phrased more cautiously; as potentially useful to identify where the largest uncertainties arise in estimating the mass balance of South Col Glacier, but unlikely to accurately represent glacier change.
Brun et al. (2022) also used a snow deposition model to quantify the impacts of wind on snow accumulation at this glacier and determine when the ice surface is free of snow and hence may melt. These results are compared with satellite imagery and show good agreement. This model application is more valuable than COSIPY for investigating South Col Glacier, but still contains large uncertainties. As discussed below, these results illustrate the limitations of the interpretation of the ice core data by Potocki et al. (2022).

[ARC1-3] As noticed by Ann Rowan, modelling has been used in a pragmatic approach to 1. show that the simulated glacier mass change is sensitive to the model numerical implementation (use of Cosipy-grad and Crocus) and 2. highlight the importance of snow drift (snow deposition model). The objectives of both modelling approaches is neither to produce reliable estimates of glacier mass changes, nor to provide a perfect quantification of snow drift. Indeed, we fully agree that this glacier is an unsuitable candidate for any surface energy-mass balance modelling study unless a model was developed specifically for this location and constrained by detailed and representative atmospheric and glaciological data. This was clearly stated in our original manuscript in the last paragraph (lines 313-320 of the original manuscript):

“The surface mass balance processes happening in the extreme meteorological context of South Col Glacier are complex, and our study does not reach any definitive conclusion about the relative importance of each of these processes. The lack of direct observations hampers our ability to decipher the dominant glaciological processes, and thus to model the glacier recent and future evolution in a realistic way. Specifically, stake measurements would be needed to measure the surface mass balance and surface velocity in a direct way, ground penetrating radar measurements would help constrain the ice thickness, and a number of subsurface temperature, snow-depth, snow transport or turbulent fluxes measurements would help constrain the processes. Without more data constrained knowledge, it appears currently impossible to conclude about the sensitivity of South Col Glacier to climate change, nor to predict its future evolution."

The aim of this modelling approach was more to highlight some specific processes not included in Potocki et al (2022), such as wind erosion, and to warn about the large sensitivity of the model results (melting here) to the numerical implementation. This rationale behind the use of these models was explained (lines 239-241, of the original manuscript):

"This numerical experiment demonstrates that the structure and physical implementations of a model can strongly affect the way the energy is spatially allocated and transported, leading to large variations in predicted melt despite solving, in principle, the same physical processes"

Yet, we acknowledge that the inclusion of Crocus might have blurred somewhat this message. Therefore, we decided to remove all sections and simulations with the Crocus model. We now focus our demonstration on showing that results obtained with Cosipy cannot be taken for granted as they prove to be fundamentally different when another equally-acceptable numerical implementation of the subsurface heat flux is tested. To clarify our objective, we added a new sentence at the beginning of section 3.3: "The challenge of modelling the surface mass balance":

"The purpose of the numerical simulations that we perform in this section is not to produce realistic estimates of the surface mass balance prevailing at SCG, but to show that various acceptable choices in the numerical treatment of the surface energy balance in Cosipy produce very different results in terms of melt. Thus, firm conclusions regarding melting at SCG should not be drawn based on such weakly determined Cosipy simulations."

[RC1-4]
3. Description of glacier geometry. I find it strange that Brun et al. (2022) assign an ablation area to South Col Glacier in Figure A6 and would expect emergence of ice to be minimal for this glacier as they predict. As discussed below, this interpretation seems rather strange based on the glacier's elevation relative to the local ELA. I suggest revising this figure and reframing the interpretation of the glacier as an accumulation area only.

[ARC1-4] We agree that assigning a geometry of an ablation area as done in Fig A6 is very speculative, and is based on many assumptions that are not possible to validate. In the revised manuscript, we removed this section concerning considerations of ice flow, emergence velocities and ablation/accumulation areas. We still believe that this glacier is very specific and cannot be considered as an accumulation area only, although its elevation is far above the local ELA. One compelling argument is the presence of exposed ice, which shows that ablation processes dominate, at least on some locations of the glacier. Indeed, wind plays an extremely important role for ablation processes (i.e., sublimation, snow drift and erosion). As a consequence, an ablation area is likely to exist, potentially in the lower part of this glacier, where wind is probably stronger (venturi effect due to the presence of South Col). However, the distribution could also be different, depending on the spatial variability of the wind velocity over this glacier, driving the relative importance of the accumulation versus ablation. Without further observations, nothing substantial can be said here and we prefer to remove all this section (lines 272 to 290 of the original manuscript), also to keep this paper as brief as possible.

Review of Brun et al., 2022: Minor comments

[RC1-5] Line 1 and 8: Is the glacier “iconic”? It’s very high, but otherwise I suggest it is not widely known.

[ARC1-5] The adjective “iconic” has been removed line 1, but the word “icons” is kept line 8, because glaciers are often considered as icons of climate change in the media.

[RC1-6] L9: remove “large” as this is relative to the glacier in question; “...glaciers thin at rates often exceeding...”.

[ARC1-6] Done

[RC1-7] L14 and elsewhere: check use of compound adjectives; hyphenation is not used with an adverb (ending in “-ly”).

[ARC1-7] Done

[RC1-8] L24: “challenge of conducting scientific...”

[ARC1-8] Done

[RC1-9] L28: worth noting here that the South Col AWS recorded only about five months of data (May–end summer 2019). An earlier AWS at this location installed by the Ev-K2-CNR project measured three years of discontinuous data that did represent the entire annual cycle and could be of use if further field data are required.

[ARC1-9] Following this comment and Tom Matthews' clarification, we changed the sentence to provide information on the period of functioning of the South Col AWS: “...despite the installation of an Automatic Weather Station (AWS), which was running between May 2019 and August 2022, with some gaps, on a rock outcrop close to the South Col (Matthews et al., 2020).”

Thanks for the information concerning Ev-K2-CNR station.

Comparison of the results and conclusions of Brun et al. (2022) with those of Potocki et al. (2022), in consideration of the response by Potocki et al. in TCD.

[RC1-10] A key question addressed by both papers is; what is the duration of snow cover on the glacier surface? This would indicate when the bare ice surface is exposed to incoming solar radiation and ice melt could occur. However, the occurrence of seasonal melt does not imply net annual mass loss. Determining mass change over a representative timescale of several decades requires observations of longer-term change as provided by both papers.
In the case of both papers, I consider that the COSIPY model is unsuitable for application to South Col Glacier and the associated uncertainties render the results insignificant. My group's ongoing work applied COSIPY to Khumbu Glacier including the area occupied by South Col Glacier (https://doi.org/10.5194/egusphere-egu21-8663). COSIPY was forced by downscaled CORDEX RCM outputs and constrained by AWS data including the five months of data from the Nat Geo South Col AWS (Matthews et al., 2020). In each simulation, the net annual mass balance at the location of South Col Glacier was strongly positive (>7 m w.e. a–1). We can debate the strengths and limitation of any of these model parameterisations but any existing glacier surface energy-mass balance model is unlikely to be suitable for South Col Glacier due to the significant differences in the processes that control mass balance at 8,000 m a.s.l. compared to glaciers for which these models were developed at lower elevations where the mass balance is better understood by established glaciological theory. The different datasets used by each study (e.g., DEMs of difference/an ice core) are more important indicators of glacier mass change.
[ARC1-10] Thanks for this comment and contribution to the discussion. We fully agree with this comment, and please refer to ARC1-3 for a detailed reply.

[RC1-11] More important than debating the parameterisation of models that are likely not meaningful, we should consider the glaciological context of South Col Glacier. The elevation of the glacier is about 2,000 m above the equilibrium line altitude (ELA) for this region, determined for Khumbu Glacier as about 6,000–6,400 m a.s.l. (Rowan et al., 2015; 2021). While glaciers usually melt during the ablation season due to warm air temperatures and high incoming solar radiation, this does not equate to mass loss year-on-year. It is difficult to see why a glacier 2,000 m above the local ELA would have a net annual negative mass balance. Mayewski et al. TCD refute the suggestion by Brun et al. (2022) that their core is collected from the glacier ablation area. As the entire glacier is located well above the local ELA, the entire glacier should be "accumulation area" and therefore as a small cold-based glacier, the mass of South Col Glacier is likely to remain stable over decadal timescales.
[ARC1-11] Thanks for this comment, we agree with it, except maybe that the entire glacier should be an accumulation area. See ARC1-4 where we debate the fact that there is potentially an accumulation and an ablation area, on South Col glacier.

[RC1-12] Mayewski et al. TCD refer to the glacier as have a stagnant area. This term is used to describe the tongues of debris-covered glaciers such as Khumbu Glacier where the velocity of ice flow has declined rapidly as the glacier has lost mass in recent decades. The term is not accurately applied by Mayewski et al. TCD in context of South Col Glacier, which has not undergone a change in glacier dynamics but instead has a typical (slow, deformation only) flow regime as a cold-based glacier.
[ARC1-12] Thanks for this discussion, no reply needed here.

[RC1-13] As referenced by Mayewski et al. TCD, Figure 1 of Brun et al. (2022) shows areas of negative surface elevation change in the upper accumulation area of Khumbu Glacier. These bands are often interspersed with bands of positive surface elevation change, and I would interpret that they are evidence of large avalanches onto and within the glacier, and opening or closing of crevasses close to the bergschrund. Again, it is the net annual mass balance over a representative period of years (i.e., the integration of these features across the entire glacier) that tells us if the glacier is losing mass. These features in the DEMs of difference are not evidence of glacier mass loss but represent mass redistribution within the glacier. The mass gain of >7m a–1 predicted by our COSIPY simulations of South Col Glacier indicate the source of these avalanches—75% of accumulation to Khumbu Glacier and neighbouring valley glaciers occurs by avalanching of snow from the steep slopes, in some cases initiated by wind erosion of snow at the ridge crests (Benn and Lehmkuhl, 2000).
[ARC1-13] We agree with the reviewer's suggestion that the pattern of dH data evident in the upper reaches of the Khumbu Glacier (localised, alternating bands of positive and negative elevation change) are a result of crevassing due to ice flow over this particularly steep area of the Western Cwm. We also agree that these features are not necessarily good indicators of the overall mass budget of the glacier. Please see also ARC1-2 for a complete reply.

[RC1-14] Potocki et al. (2022) interpret their ice core as representing the accumulation area of the glacier and the age of the ice collected near the glacier surface (0.1–0.7 m core depth) is about 2,000 years old ("1966 ± 179 years ago"). This period is then multiplied by the annual layer thickness for the entire core (27 mm w.e. a–1) to estimate mass loss (apparently without any correction from water equivalent to ice thickness accounting for ice density?). This calculation assumes that the age of the ice at the glacier surface is the same as at the depth measured in the core and that the annual layer thickness is consistent throughout. From their Supplementary Information, it appears that annual layers were only measured in a 0.1 m section of the core at about 6 m depth. The representativeness of these values is determined by comparison with a core from East Rongbuk Glacier at 6,518 m (Kaspari et al., 2009). However, the annual layer thickness at South Col Glacier could be much thinner if wind erosion is accounted for. The snow deposition model results from Brun et al. (2022) suggest that at South Col Glacier nearly all precipitation can be eroded from the glacier surface by wind, which would not be the case at East Rongbuk Glacier or Khumbu Glacier where the majority of accumulation is sourced from avalanching. It is therefore possible that since the Sol Col Glacier last expanded and formed the moraines identified by Petocki et al. (2022) that most or all of the annual snow accumulation is scoured off by wind and that the exposed ice surface represents the last period when the glacier expanded. These moraines are undated at South Col Glacier, but there are three possible equivalent ice-marginal moraine ages at Khumbu Glacier dated to 1.3 ± 0.1 ka, 0.9 ± 02 ka and 0.6 ± 0.16 ka (Hornsey et al., 2022).
[ARC1-14] Thanks for this information and discussion, no reply needed here

[RC1-15] The 'space-for-time substitution' suggested by Mayewski et al. TCD reasons that because snow melts at Camp 2 in April–May then ice must melt at South Col Glacier in July–August. Their photographs show melt water on the surface of the accumulation area of Khumbu Glacier at Camp 2 (6,464 m a.sl.) on a patch of rock debris. The low albedo of the debris combined with high incoming solar radiation is likely to promote snow melt, but again is not evidence for net annual glacier mass change. The substitution reasoning is not convincing; South Col is up to 20 degrees colder based on the ERA data presented by Mayewski et al. TCD and the incident radiation would presumably be much lower than given

here due to monsoon cloud cover. The uncertainties in this estimate of seasonal melt seem similar or greater than those in the COSIPY experiments.
[ARC1-15] Thanks for this information and discussion, no reply needed here

[RC1-16] In summary, there are limitations to both the DEM differencing and ice core methods. I am convinced that both papers present their results accurately and have not made errors in their data processing. However, the DEM differencing presented by Brun et al. (2022) quantifies mass change across the glacier over a representative period of several decades. The ice core collected by Potocki et al. (2022) represents only one point on the glacier and is open to an alternative interpretation in context of the erosion of snow by wind from the glacier surface. I suggest that the model results are discounted as indicative of glacier mass balance due to the limitations of simulating this extreme environment without a dedicated model driven by spatially and temporally representative measurements from South Col Glacier. The remaining question is if there is value or feasibility in collecting direct glaciological measurements from South Col Glacier. Installing and managing equipment over a sufficient timescale (>5 years) would be very challenging and expensive. We therefore need to rely on high-quality remotely sensed observations, including those presented by Brun et al. (2022), which in the last 15 years or so have greatly improved understanding of recent glacier change in the Himalaya.
[ARC1-16] We agree that the DEM differencing method has limitations, and we have made a concerted effort to more clearly describe and quantify the potential errors and uncertainties associated with this method in the revised manuscript (see ARC1-2). This method has been widely used and improved by the remote sensing cryospheric community in the last decades, and a thorough quantification of the uncertainties has been developed by Hugonnet et al. (2022), and applied in our present study. We are therefore confident that our elevation change results reflect the fact that South Col Glacier did not thin over the last 3 decades. This is indeed the main conclusion of our study, and in turn the title of our brief communication. This conclusion is in contradiction with results from Potocki et al. (2022), obtained from an ice core analysis, and we tried to understand why.
As already mentioned in ARC1-3, the goal of the numerical simulations of surface mass balance that have been performed in our study is not to assess whether significant melt actually occurs at SCG, but to demonstrate that a firm answer to this question is beyond the reach of available numerical models which are not designed for such specific environments. Yet, we acknowledge that this message was not that clear due to our initial choice to include simulations performed with both a modified version of Cosipy and Crocus models. Therefore, all references to Crocus have been withdrawn from the new version of the manuscript. We now focus our demonstration on the sensitivity of Cosipy results in terms of melt to choices in the numerical implementation of physical processes. More specifically, we show that an alternative numerical treatment of the subsurface heat flux in the surface energy balance module of Cosipy that is more conformal to the physics and consistent with implementations adopted in other skin-layer models (e.g., Covi et al., 2022 and other references to DBAM) turns out to produce fundamentally different results. We believe that this is enough to show that uncertainties on the results produced with Cosipy at SCG are so large that no conclusion can be drawn from them. We think that this demonstration is relevant to support our conclusion and, therefore, that this modelling section is worth keeping in our brief communication.

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