Referee comment 4 - Horst Machguth and Enrico Mattea

Review of “Brief communication: Everest South Col Glacier did not thin during the last three decades” by Brun et al.

Horst Machguth and Enrico Mattea, Department of Geoscience, University of Fribourg, Switzerland

1. Introduction

[RC4-1] The study of Brun et al. argues that South Col Glacier has not changed substantially since 1984. Their finding contradicts Potocki et al. (2022) – in the following Potocki et al. – who claim that the glacier has thinned at their drill site (8020 m a.sl.) by about 55 m. The timing of the thinning is unclear but it is suggested by Potocki et al. that the climate at that elevation warmed substantially in the 1950s and even more substantially in the later 1990s. We divide out review in a general section which refers to both studies, and a section of detailed comments focusing on Brun et al.

[ARC4-1] We thank Horst Machguth and Enrico Mattea for their detailed review of our communication and for bringing new material contributing to the overall discussion

2. General comments on South Col Glacier changes

[RC4-2] Brun et al. contradict that South Col Glacier has thinned dramatically. They do so by comparing two digital elevation models representing different points in time. The evidence provided by Brun et al. appears sound and corresponds to state-of-the-art. Nevertheless, we chose an independent way of assessing whether the glacier has changed or not. To do so, we compared historical photos of South Col Glacier to recent images. We obtained images taken during the 1956 Swiss Everest/Lhotse expedition; the second ever expedition to summit Everest and the first to summit Lhotse. The images are publicly accessible at https://alpinfo.ch/en/portrait/historical-notes/expeditions/everest-lhotse- 1956/ . We have also considered photos from the two Swiss 1952 Everest expeditions (also publicly available via the above link), but found that images from 1956 are optimal for the comparison. We compare two historical images from 1956 to recent images. Figure 1 shows the perspective from somewhere in the vicinity of the South Col AWS, looking slightly down on the plateau of South Col Glacier. Figure 2 shows the view towards the tongue of South Col Glacier, looking upward from what was maybe Camp V or VIa during the 1956 expedition.

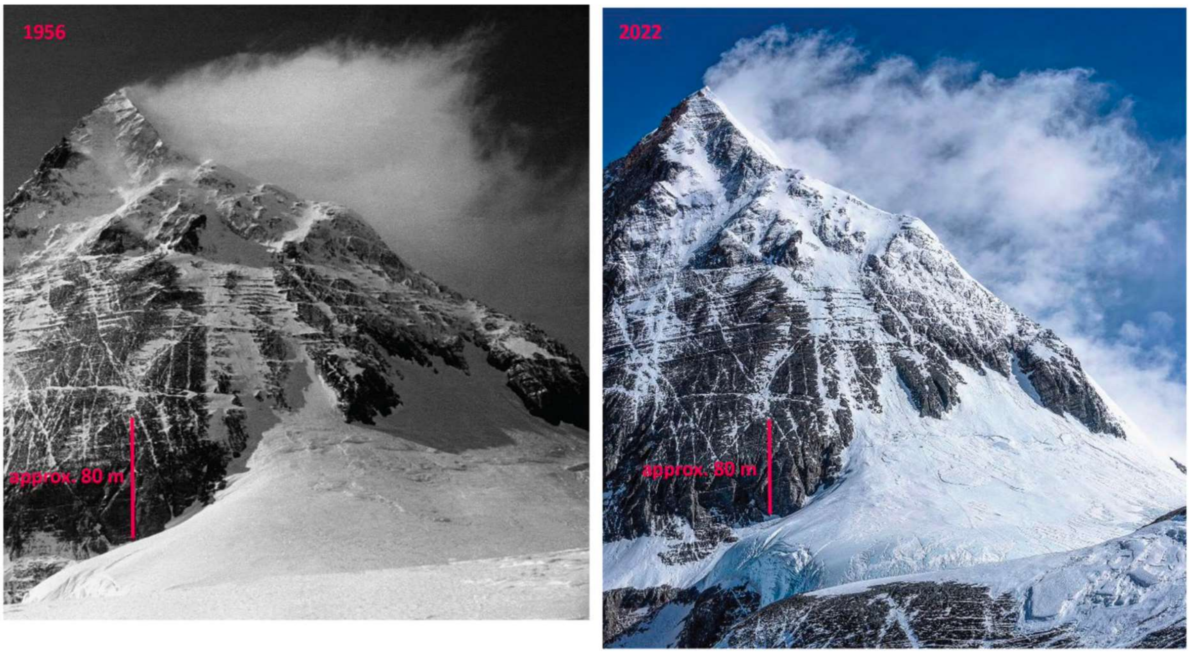

Fig. 1: Comparison of a historical image of South Col Glacier, recorded in May 1956 (left) to an image taken in May 2022 (right). For scale an approximate elevation difference has been drawn based on the Pléiades DEM by Brun et al. Note that the two images were taken from two different viewpoints, with the 1956 picture taken from a point somewhat more east and closer to South Col. Image courtesy of the Swiss Foundation of Alpine Research (1956) and Tim Mosedale (2022, https://timmosedale.co.uk/).

The comparison in Fig. 1 shows, if at all, small changes in South Col glacier. There is no support for the claim of Potocki et al. that the glacier has thinned in excess of 50 m. The near-absence of changes is most obvious at the glacier margins. Admittedly, changes are more difficult to assess in the glacier centre where Potocki et al. have drilled. Nevertheless, the glacier appears also to have changed little at the drill site. If the glacier would have thinned 55 m in the centre, then one would expect an even more pronounced change at its tongue. Such a thinning would be obvious as the ice cliff is in close proximity to the drill site (less than 200 m apart). However, recent imagery shows that the glacier tongue is at the same location as it was in 1956 (Fig. 2) and the ice thickness appears unchanged. The tongue appears similarly active as in the 1950s (see for example an excellent 2022 overview of South Col Glacier: https://www.mountainpanoramas.com/___p/___p.html?panoid=2022_M1&labels=on).

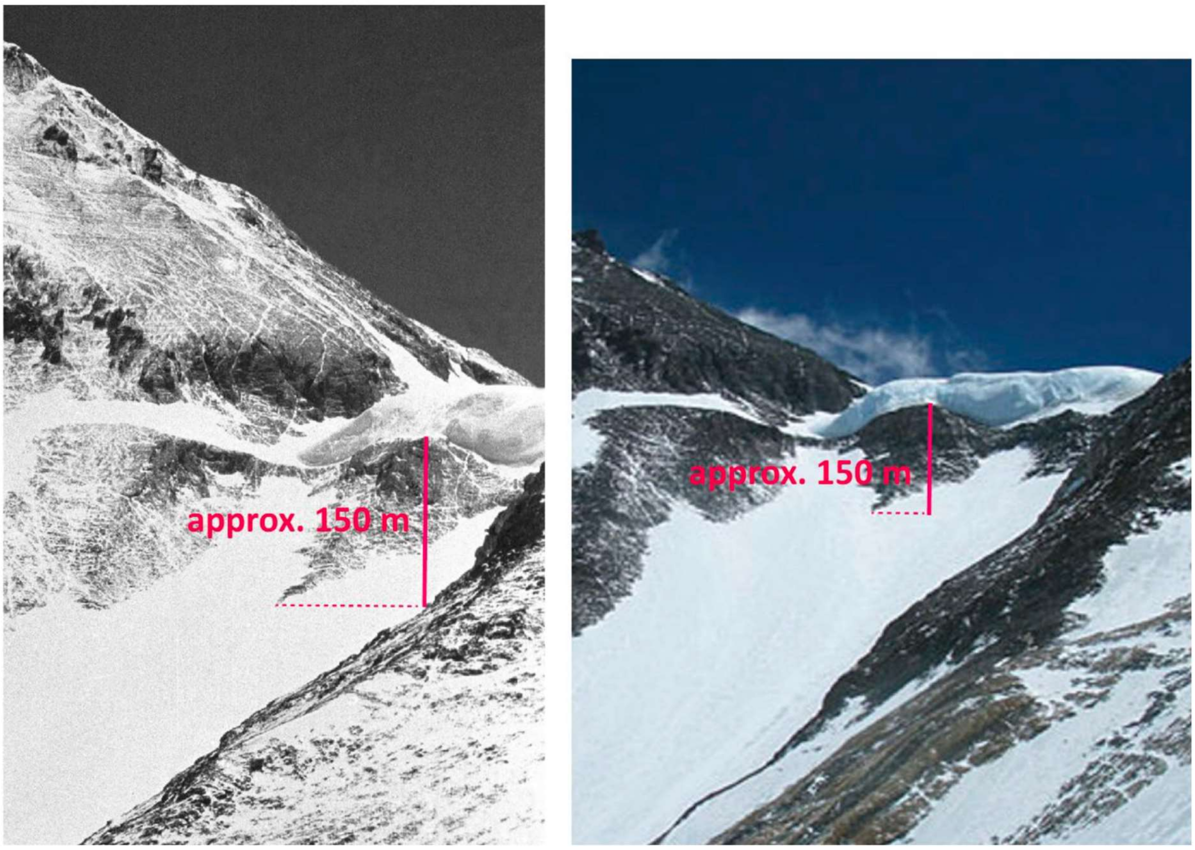

Fig. 2: Tongue of South Col Glacier in 1956 and 2008. Left: detail of a historical image of South Col Glacier, recorded in May 1956 by the Swiss Everest/Lhotse expedition. Right: detail of an image taken in May 2008. Note that the two images were taken from somewhat different viewpoints, with the 1956 image taken from a higher elevation. Image courtesy of the Swiss Foundation of Alpine Research (1956) and https://spaceref.com/science-and-exploration/scott-parazynski-everest-photo- update-4-june-2008 (2008).

We investigated whether glacier changes are detectable near the actual South Col where the glacier surface appears very flat and the ice looks rather thin (directly east (right) of the "Everest South Col" marker in the linked panoramic photograph). Interestingly, also there the situation in 1956 appears similar to more recent photos (not shown). We note that there are many good images available for South Col, for different points in time, providing excellent possibilities to investigate whether glacier changes took place or not.

[ARC4-2] We thank Horst Machguth and Enrico Mattea for backing up very convincingly our DEM analysis using high quality photography. They indeed confirm the lack of thinning (and frontal retreat) of South Col Glacier. We refer to the material published in this review by adding: "This observation can be extended back in the past, as the comparison of photographs from the Swiss expedition to Everest in 1956 with photographs from 2022 shows that there are no visible changes in South Col Glacier (Machguth and Mattea, 2022)."

3. General comments on the surface mass balance modelling

We argue that uncertainties in available model input parameters are too large to make any reliable statement on the mass and energy balance at South Col Glacier, based on model simulations. These uncertainties also affect the model comparison presented in Brun et al. We detail our argumentation on the example of ice albedo at South Col and by running a third surface mass and energy balance model for South Col Glacier.

[RC4-3] The problematic of parameter uncertainties: Both Brun et al. and Potocki et al. use an ice albedo value of 0.4. The value has been measured at Base Camp, at approximately 5400 m a.sl. (Matthews et al., 2020). The ice at the surface of South Col Glacier is referred to as blue ice (Brun et al.). Albedo values for Antarctic blue ice are substantially higher, typically in the range of 0.6 to 0.65. (e.g. Reijmer et al. 2001; Genthon et al. 2007; Smedley et al. 2020). While Antarctica's blue ice areas might not be representative for South Col blue ice, it is also questionable to use an albedo value measured on glacier ice 2500 m lower than South Col. At Base Camp melt processes and surface ice conditions differ substantially from South Col. Also, the glacier ice has been formed under different conditions (cold on South Col vs. possibly temperate at Khumbu glacier), further affecting ice albedo.

Both Potocki et al. and Matthews et al. (2020) do not perform sensitivity tests with ice albedo, regardless of the extreme importance their studies assign to short wave radiation. In the case of South Col, critical uncertainties are not limited to surface albedo alone. A parameter sensitivity study is mandatory as soon as some parameters have relevant uncertainties. In the case of South Col, even a simple sensitivity study, as demonstrated below, might show that the range in possible outcomes of model simulations is simply too large for model results to be deemed reliable.

A thorough sensitivity analysis is also missing in Brun et al. While COSIPY is used in two different constellations of model numerics, CROCUS is not subject to any sensitivity assessment. Sensitivity to uncertainties in model input has neither been evaluated for the COSIPY variants nor for CROCUS. We understand that this is beyond the scope of the study by Brun et al. They also clearly state that uncertainties in any simulation for South Col are too large, given our current knowledge of meteorological conditions and mass balance. Nevertheless, we gained the impression that Brun et al. somewhat consider CROCUS the benchmark for other models. While this could be true, it would require demonstrating that CROCUS is more robust to changes in model numerics and comparing sensitivity of all models to input parameter perturbations. In this sense, we would like to ask Brun et al. to check their manuscript for any explicit or implicit “model hierarchy” and to further emphasize the problematic of poorly constrained model input and the absence of a parameter sensitivity study.

[ARC4-3] We totally agree with Horst Machguth and Enrico Mattea that, because in-situ atmospheric and glaciological data at SCG are lacking, many important model parameters, including albedo, and forcings are poorly constrained. It follows that any modeling study aiming at producing realistic estimates of surface mass balance at SCG should include a parameter sensitivity study. However, as the reviewers noticed, this is not our goal here. Instead, we wanted to highlight the lack of robustness of any modeling results in such specific conditions. We acknowledge that the inclusion of Crocus in the original version of the manuscript might have blurred this message. We do not consider Crocus as the benchmark for other models. Indeed, neither Crocus nor any other available mass-balance models are validated in the extreme environment of the studied glacier, and such a validation is simply out of reach due to the lack of data. Therefore, we have decided to withdraw Crocus simulations in the new version of the manuscript. We now focus on demonstrating the lack of reliability of results produced by Cosipy for this glacier. Indeed, these results turn out to be thoroughly different if another equally-reasonable numerical treatment of the subsurface heat flux in the surface energy balance is implemented instead of the original one. We think that this is enough to show that uncertainties on obtained results are so strong that no conclusion can be drawn from them. In such a context and to address our objective, we believe that a parameter sensitivity study is useless and is even meaningless. See also ARC1-3 for a detailed reply concerning the objectives of the mass-energy balance modeling

[RC4-4] Simulating South Col Glacier surface mass and energy balance using EBFM: The energy balance and firn model (EBFM, van Pelt et al., 2012) was developed following Klok and Oerlemans (2002) and the subsurface model SOMARS by Greuell and Konzelmann (1994). The model has recently been modified for use on Abramov Glacier, Kyrgyzstan (Kronenberg et al., 2022) and for Colle Gnifetti, Swiss Alps (Mattea et al., 2021). Here we deploy the version by Mattea et al. (2021) for a series of South Col model sensitivity experiments.

The model uses a skin layer formulation, calculating surface energy fluxes from meteorological variables. The surface energy balance equation is solved for surface temperature and mass fluxes, including melt and sublimation rates. Surface albedo is bounded by constant values for fresh snow, firn, and ice (respectively αfresh, αfirn and αice); it evolves as an exponentially decaying function of time since the last significant snowfall (defined by a minimum precipitation rate Pmin).

At each time-step, the computed surface boundary conditions drive a Lagrangian simulation of the glacier subsurface: it consists of a stack of NL layers able to move freely along the depth axis, following the addition or removal of mass at the surface. A new layer is added at the top whenever snowfall and riming push the topmost layer thickness beyond threshold zs.

Notable omissions in the EBFM include penetration of short-wave radiation and wind erosion of snow (less significant for the simulation of an ice surface). As in COSIPY and Crocus, terrain reflections and topographic shading are also ignored; they are expected to play a minor role in the overall energy balance (e.g. Mattea et al., 2021).

We use the same downscaled ERA5 data of Potocki et al. and Brun et al. to force the model. Table 1 reports the main results of our sensitivity runs.

Table 1: summary of sensitivity EBFM runs. Shaded column headings indicate model parameters, the other columns are model results. Melt M is shown in bold.

Id	Period	Spinup	*NL*	P_{min}	z_s	α_{fresh}	α_{firn}	α_{ice}	*M*	*S*	*D*	*E*	*C*	α_{mean}	Q_g
1	2000-2019 hourly	None	50	2.5e-8	0.10	0.83	0.52	0.39	**0.0007**	0.241	0.012	0.000	0.000	0.80	0.25
2	2000-2019 hourly	None	50	2.5e-6	0.10	0.83	0.52	0.39	**0.7750**	0.428	0.004	0.062	0.000	0.47	3.2
3	2000-2019 hourly	None	50	2.5e-6	0.01	0.83	0.52	0.39	**0.5948**	0.489	0.002	0.039	0.000	0.45	17
4	2000-2019 hourly	None	2000	2.5e-6	0.01	0.83	0.52	0.39	**0.5201**	0.487	0.002	0.036	0.000	0.46	18
5	2000-2019 hourly	None	500	2.5e-6	0.01	0.83	0.52	0.39	**0.5529**	0.493	0.002	0.038	0.000	0.46	20
6	2019 minutely	None	500	2.5e-6	0.01	0.83	0.52	0.39	**0.3539**	0.327	0.005	0.036	0.000	0.63	6.9
7	2019 hourly	None	500	2.5e-6	0.01	0.83	0.52	0.39	**0.3520**	0.342	0.005	0.032	0.000	0.63	11
8	2000-2019 hourly	None	500	2.5e-6	0.01	0.83	0.60	0.60	**0.0490**	0.355	0.007	0.007	0.000	0.65	7.8
9	1950-1999 hourly	None	500	2.5e-6	0.01	0.83	0.60	0.60	**0.0282**	0.323	0.006	0.005	0.000	0.65	6.8
10	2000-2019 hourly	50 yr, run 9	500	2.5e-6	0.01	0.83	0.60	0.60	**0.0178**	0.351	0.007	0.004	0.000	0.65	7.8
11	1950-1999 hourly	None	500	2.5e-6	0.01	0.83	0.52	0.39	**0.3486**	0.439	0.003	0.026	0.000	0.48	13
12	2000-2019 hourly	50 yr, run 11	500	2.5e-6	0.01	0.83	0.52	0.39	**0.4609**	0.480	0.003	0.032	0.000	0.46	14

1. M, S, D, E and C are annual means of melt, sublimation, deposition, evaporation and condensation. αmean is the mean surface albedo. Qg is the mean annual subsurface heat flux.
2. Pmin isinmw.e.s-1;zs inm;M,S,D,E,Cinmw.e.yr-1;Qg inWm-2;allotherparametersare dimensionless.
3. Changes in model setup from one model run to the next are highlighted.

The ERA5 meteorological series contains extremely frequent, small precipitation events, which constantly reset surface albedo to the fresh snow value (αmean in run 1). As such, in runs 2 through 12 we increase Pmin by 100x, to effectively disable the albedo from being restored in most cases.

With a layer thickness limited at 10 cm and a time-step of 1 hour (run 2), the EBFM calculates mean annual melt amounts of 0.78 m w.e. over 2000-2019, which corresponds to half of the values reported by Potocki et al.; notably, mean albedo over the modeled period is 0.47, which is about 25 % lower than Antarctic blue ice albedo.

Even before altering physical parameters, we note that the numerical setup has a significant impact on model results: forcing the use of 10x thinner grid layers (run 3), computed melt amounts drop by about 30 %. (Mean subsurface heat flux also has a five-fold increase, but the value remains reasonably low – unlike what is reported for COSIPY by Brun et al.). The maximum depth of simulation also affects the results somewhat (runs 3, 4, 5): annual melt amounts increase for shallower grids, from 0.52 m w.e. (at 20 m maximum depth), to 0.55 and 0.59 (respectively at 5 m and 50 cm). Unlike the COSIPY result by Brun et al., a finer time resolution of 1 minute (with linearly interpolated climate variables) does not reduce melt at all in the EBFM (runs 6 and 7).

Most importantly, in run 8 we test a standard value of 0.60 for the albedo of blue ice. This simple change reduces melt rates by 90-95 % compared to the glacier-ice default value of 0.39 (run 5). A further reduction by more than 60 % occurs when running the model after a spin-up period of 50 years (run 10). The latter observation also holds true for simulations with the default (lower) albedo values (run 12).

Such a high sensitivity to the albedo parameters indicates a very high degree of uncertainty in the simulated energy balance at South Col, and raises serious concerns on the applicability of any albedo values not measured in situ.

Computed sublimation rates in the EBFM are in all cases comparable (20-50 cm w.e. yr-1) to the results of Potocki et al. and Brun et al. Still, parameters involved in the calculation of turbulent fluxes (such as surface roughness lengths) are known to be poorly constrained, especially in high accumulation areas (e.g. Mattea et al., 2021). Therefore, if sublimation plays a major role in the surface mass fluxes at South Col, its modeling uncertainties are likely also significant for the overall error budget, and should be investigated.

In conclusion, also a skin-layer, or skin-temperature model appears to be able to predict, in its basic configuration, no melt for the South Col Glacier. Relatively small perturbations in model parameters, however, are sufficient to change model output substantially, reaching from almost zero up to ~50% of the melt simulated by Potocki et al.

[ARC4-4] We sincerely thank Horst Machguth and Enrico Mattea for performing new simulations using EBFM at South Col glacier, with the same forcing data as Potocki et al. (2022) or Brun et al. (2022), and testing different numerical setup or model parameters. Their results in table 1 nicely illustrate that model outputs (mainly melt) are highly sensitive to the model numerical setup or to key parameters such as the albedo. We totally agree with their conclusions and the concerns they raise on the applicability of any mass-energy balance model in this extreme environment. We indeed reach the same conclusions with the COSIPY model (see ARC1-3 for a complete reply). As stated in ARC4-3, we did not perform any parameter sensitivity study in our communication. Since the model is not suitable in such an environment, this is finally meaningless. But we expect to have a large sensitivity to albedo, and to a lesser extent to roughness lengths, as shown for EBFM. We added a sentence about the sensitivity to ice albedo value:
"We also rise some awareness about the parametrization of albedo, and the fact that the blue ice of South Col Glacier might have an albedo larger than 0.4, as observed for Antarctic blue ice that has an albedo of 0.5 to 0.6 (e.g., Smedley et al., 2020). A higher ice albedo would dramatically reduce the melt totals, as suggested by the sensitivity tests of Potocki et al. (2022). and Machguth and Mattea (2022)."

<u>4. Detailed comments on the manuscript by Brun et al.</u>
[RC4-5] Line 26: We suggest spelling out JJAS where it is first mentioned.
[ARC4-5] Done

[RC4-6] Line 36: This communication is not brief. Depending on the editorial guidelines, it could also be published as a normal paper.
[ARC4-6] If the Editor agrees, we prefer to keep this contribution as a brief communication, because this study has been mainly conducted to reply to Potocki et al. (2022) and consequently, it would lose its interest as a normal paper. Moreover the lack of in-situ data prevents from performing a reliable analysis of mass-energy balance, which would be valuable in a normal paper. To keep this communication short, we have removed or shortened some sections: all Crocus simulations and all text referring to glacier flow and emergence velocities.

[RC4-7] Lines 54-57: Could you quantify "as the range of elevation change values were higher here"? How are "minor data voids" defined?

[ARC4-7] The standard deviation of dH values above 6800 m is higher than below 6800 m due to the presence of high magnitude (±~ 35 m) elevation differences associated with previously described [ARC1-2] crevasse block movement on the Lhotse and Kangshung face.

To fill small data voids we computed a smoothed version of the dH grid where the value of each cell was derived as the mean of a surrounding 5 x 5 cell (10 x 10 m) window. Data voids in the original dH grid smaller than this window size could then be filled with the 'mean' values.

We have slightly reworked the text in Section 2 of the manuscript to better describe these parts of the dH data processing:

"Following DEM differencing, surface elevation change data (dH) were filtered to remove outliers, with values outside the range of five times the standard deviation of dH estimates within 50 m elevation bands removed below 6800 m a.s.l. Above 6800 m, or from the base of the much steeper Lhotse face, we applied a threshold of three times the standard deviation of dH estimates, as the range of elevation change values here include high magnitude elevation changes (± ~35 m) associated with crevasse field evolution captured by both DEMs. To fill small data voids we computed a smoothed version of the dH grid where the value of each cell was derived as the mean of a surrounding 5 x 5 cell (10 x 10 m) window. Data voids in the original dH grid smaller than this window size could then be filled with the 'mean' values."

Note that these steps are mostly relevant for glaciers other than SCG, as no pixel was excluded from SCG, whereas on average of 7.2 % of the pixel were excluded for the other glaciers, due to their steeper slopes and more variable dH.

[RC4-8] Lines 156/157: The article by Brun et al. criticises most results from Potocki et al. However, here one of their results is cited as if correct, to support the argumentation by Brun et al. For the purpose of assessing the study, we suggest, to argue based on independent results also where the results from Potocki et al. fit the own argumentation.
[ARC4-8] Agreed. This sentence has been removed

[RC4-9] Line 177: Citation: It appears that this is basic knowledge generally understood. We would like to ask the authors to cite a more original reference.
[ARC4-9] Tubini et al. (2021) has been replaced by Anderson (1976)

[RC4-10] Lines 262/263: Please add at least one more original citation or remove the citation here. It is long known how ice dynamics transport ice from the accumulation to the ablation area.
[ARC4-10] The citation has been removed

[RC4-11] Lines 278-289: We do not fully understand the idea behind this calculation. The authors first state that the glacier is in balance, as shown by the DEM differencing. Then the ablation values of Potocki et al. are used to estimate at which speed the ice would need to flow so both conditions are fulfilled, that is (i) glacier is in equilibrium and (ii) ablation is ~2 m a-1. But Potocki et al. do not claim that the glacier is in equilibrium, hence we do not understand what is supposed to be shown here.

[ARC4-11] The idea here was to exclude the possibility that the 1.5 m w.e./yr melt suggested in Potocki et al. (2022) is compatible with a mean dh=0, i.e. the melt would be compensated by emergence. Anyway this section has been removed (see ARC1-4 for a complete reply)

[RC4-12] Line 290-293: The argumentation could be clearer. The motivation of the previous paragraph is already unclear to us (see above). Then, without an introduction or explanation, another method, based on other assumptions, is used to estimate flow velocity in South Col glacier.
[ARC4-12] The section has been removed - see ARC4-11 and ARC1-4 for a complete reply)

[RC4-13] Line 293: Is the term "continental" the right term here? It is a monsoon influenced glacier, likely cold and frozen to the bed. While indeed arid (albeit not only because of low precipitation but also because of strong wind erosion), annual fluctuations in air temperature, characteristic for continental glaciers, are not particularly large (e.g. Suppl. Fig. 8 in Potocki et al.).
[ARC4-13] Agreed. The entire section has been removed and the term continental abandoned.

[RC4-14] Figures 1 and A6: Mayewski et al. state in their comment on Brun et al. that the latter placed the drill site at the wrong location. It appears to us that the drill location as visualized by Brun et al. is correct, the only difference being that Brun et al. express elevation in meters above the ellipsoid while Potocki et al. use elevation in meter above the geoid. Nevertheless, it appears that in Fig. 1 the contour lines are in meters above the ellipsoid (the drill site is slightly below 8000 m) while the elevation of Lhotse peak is given in meters above the geoid. The caption of the figure does not indicate which elevation datum was used. As readers might be familiar with elevations of South Col, Lhotse Peak and Everest, and these well-known numbers are in meters above sea level, we suggest that elevations on maps are expressed in relation to the same datum.

[ARC4-14] We appreciate the reviewer highlighting this point relating to the use of geoid heights versus ellipsoid heights, which is an oversight on our part. We confirm that our DEMs were generated to represent the height above the ellipsoid rather than the height above geoid, hence the difference in elevation at the ice core drill site. We have corrected the Pleiades DEM using the EGM2008 geoid model and replotted the contours used in Fig. 1 to ensure consistency with other landmark heights in the area. At the point of the drill site (27.977211, 86.929861, taken from Potocki et al. 2022), the Pleiades DEM estimates the elevation to be 8003 m. Note that there is a +/- 10 m uncertainty in the absolute elevation from Pléiades DEM, which does not affect the DEM difference. We have altered the Figure caption to state that indicated elevations are height above the geoid.

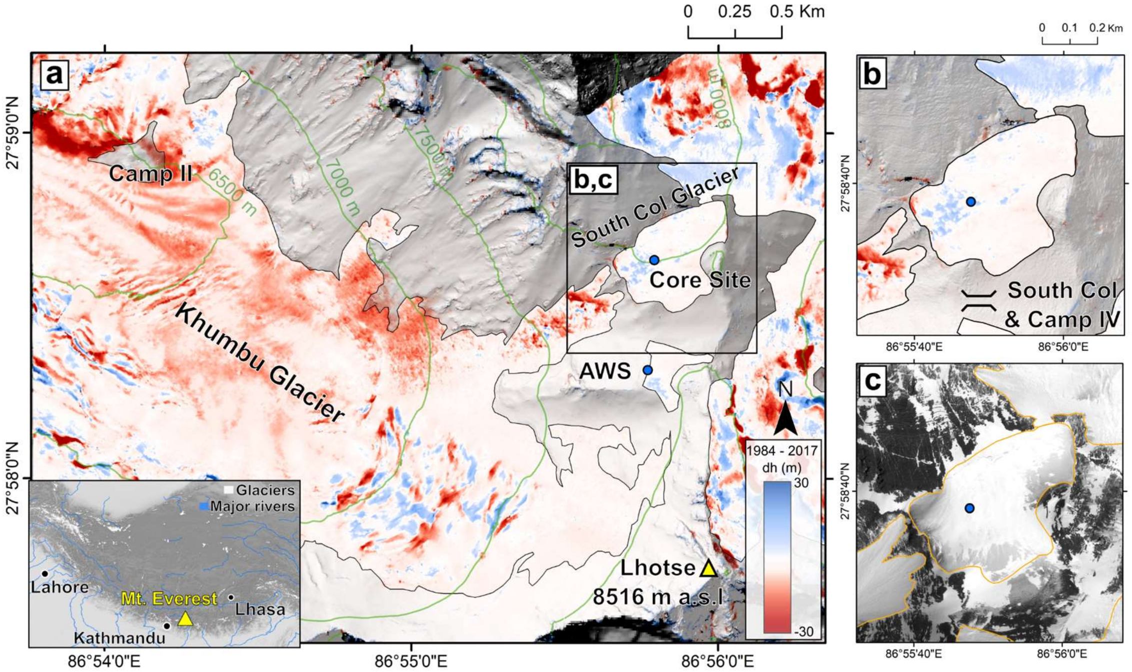

Figure 1. Surface elevation change over the Western Cwm (a) between 1984 and 2017, and over the South Col Glacier (b). The location of the ice core and AWS from Potocki et al. (2022) are shown with blue dots. Background is a shaded relief from the Pléiades DEM. The conditions at the surface of the South Col Glacier on 23 March 2017 are captured by a Pléiades orthoimage in panel c (Pléiades, copyright CNES 2017, Distribution Airbus DS). The inset of panel a shows the location of Mount Everest in the broader context of High Mountain Asia. All indicated elevations are expressed as height above the geoid.

Acknowledgements

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