Response to Brun et al. re Potocki et al. (2022)*

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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.

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.

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:

(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. 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. 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.

(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.

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 ($\tan(0.5) \times 3048 = 26.6$ 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.

(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.

(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).

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*.”

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.

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.

(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).

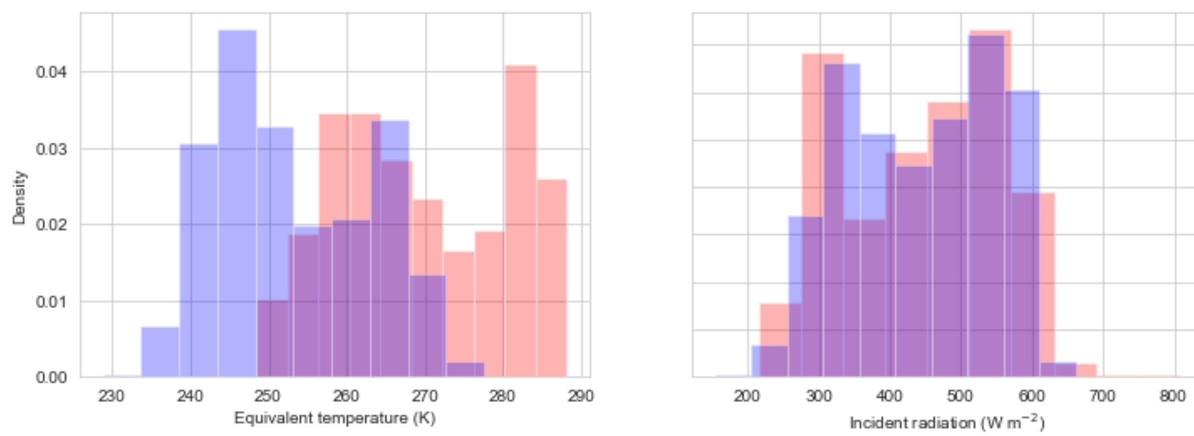
(6) B22 assert that there is no evidence for any substantial ice melt having occurred due to an absence of fluvial features on the ice or off-glacier. The authors of P22 discussed this via email with the B22 authors, but some relevant parts of that discussion were omitted in B22: (1) the gently sloping 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 m^2 area of the SCG it means a volume of 3,200 m^3 evacuated per day, so a mean runoff of 0.04 $m^3\ s^{-1}$. 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).

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).

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.

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