Community comment 5 - Tom Matthews

[CC5-1] This is a very valuable review. I commend the authors for attending to the debate in so much detail.

However, I wish to make a brief correction. It is stated that Potocki et al. (2022) did not assess the sensitivity of their SEB results to albedo. However, they did indeed perform this sensitivity assessment as part of a 'bounded' uncertainty quantification (Smith et al., 2018), whereby parameter values were perturbed to their plausible min/max values in order to estimate equivalent, plausible min/max rates of ablation. As part of that assessment (detailed in the SI of Potocki et al., 2022), the albedo was varied between 0.3 and 0.5. The higher albedo reduced the melt rate, but did not materially affect their SEB conclusions (i.e., the physical plausibility of substantial ice melt).

I also note that whilst the ice at the drill site was cleaner that the ice circled in the image below (shown in Matthews et al., 2022), the blue ice areas of Antarctica are clearly not a good surrogate for albedo at the South Col Glacier as a whole, so would encourage any such analogy to be made with caution. The ice circled is indeed likely to have been darkened through ablation (and may well have an albedo appreciably below 0.4). The assignment of the 0.4 value by Matthews at al. (2020) and Potocki et al. (2022) was an estimate, but an informed one based on visual site inspections/comparisons by the authors.

I also suggest cautious interpretation of the reviewers' 'spin-up' experiments. Presumably the reduced melt is because a perennial snowpack develops? This is questionable, because the precipitation data used by the authors of this review are tied to the surface energy balance results of Potocki et al (precipitation was corrected by the authors to match ablation in the first decade of the simulation). With another model formulation that simulates less ablation, the precipitation would be reduced too (so a perennial snowpack would likely not develop in the spin-up period).

[ACC5-1] Thanks for the clarifications, no reply needed from our side here. And any modeling approach is presently very limited by the absence of in-situ data, such as albedo values at least. The point raised by the reviewers about SCG albedo being potentially higher than 0.4, and potentially reaching 0.6 is probably relevant for most parts of SCG not visible on the picture shown by Tom Matthews. When looking at pictures of SCG on the internet, we were actually surprised by how difficult it is to tell if the surface is snow or ice (see for example: https://www.mountainpanoramas.com/_p/_p.html?panoid=2022_M1&labels=on), suggesting that exposed ice has actually a very high albedo.

[CC5-2] Importantly, this review, and the work of Brun et al., build on Potocki et al. to highlight the sensitivity of the South Col Glacier to SEB modelling uncertainty. This is very interesting in its own right. If Potocki et al.'s model formulation is close to being 'right', it suggests an extreme sensitivity to the maintenance of a protective snowpack at the South Col. This can remain true, even if Brun et al. are right in that the South Col Glacier did not actually thin as Potocki et al. suggested (i.e., because the snowpack has mostly been preserved).

If the conservative model configurations results of this review and Brun et al. are correct, however, then rapid thinning is unlikely even if ice becomes exposed for long periods of the year.

The resolution of this debate matters for our understanding of what may happen in the future at the South Col Glacier, and possibly for other ice masses at extreme elevation. Accordingly, I suggest that the discrepancy between results should be a call for urgent further research.

There are bright possibilities for that in the near future. First, the albedo at the South Col Glacier can be constrained using satellite measurements (Bessin et al., 2022). Second, high-resolution radar measurements can be used to identify melt events (Scher et al., 2021). Given that melt events are regularly detected at the South Col (Steiner, this discussion), we must reject the parameter values and model structures which generate no melt. Indeed, given the comment by Steiner in this forum showing melt events throughout June-October, I would also suggest that setups generating minimal melt rates (Brun et al., and Machguth and Mattea) – even when ice is exposed – are unlikely to be appropriate. Although Steiner's results cannot quantify the magnitude of melting, they show that it is not a rare occurrence. Indeed, if some melting is detected in June and October, melt rates in the peak Monsoon months of July and August – when moist enthalpy is at a maximum and wind speeds are at a minimum (Khadka et al. 2021) – would be considerably higher for an exposed ice surface. Quantifying how much higher – with perturbed parameter ensemble model runs constrained by satellite observations and the weather station data -- should be a high priority for future research.

[ACC5-2] We agree with Tom Matthews' comments for future research about this SCG. We want to add two important concerns that Tom Matthews did not mention: 1. wind related processes (erosion, sublimation, drift) are likely the most important processes to look at, if a dedicated study is undertaken on SCG, 2. regardless of the amount of melting at the surface, a thorough quantification of how much meltwater refreezes at the surface is necessary. We note that ice and snow albedo quantification is not possible from Venus images, as it does not have the 1.6 µm SWIR band needed to compute albedo (Bessin et al., 2022). However, Sentinel-2 images could be suitable (Naegeli et al., 2017).

New references cited

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