Community comment 2 - Tom Matthews

Reply to reviewer Ann Rowan

[CC2-1] I am very grateful for the substantial efforts of reviewer Ann Rowan in attempting to reconcile disagreements across three sources (Potocki et al., 2022; Brun et al., 2022 – under review here; and Mayewski et el. 2022 – a comment posted in this forum on Brun et al. 2022). This is clearly a challenging task. In this reply to Ann Rowan (AR), I wish to make a few clarifications to aid with the rest of the review process.

[CC2-1a]First, AR states: "L28: worth noting here that the South Col AWS recorded only about five months of data (May-end summer 2019)..."
However, the data (freely available here) extend from May 2019 to August 2022. The record (although interrupted in places) is therefore over three years in length.
[ACC2-1a] Thanks for this clarification and the whole observation period is now mentioned in the revised text (see ARC1-9)

[CC2-1b]Second, AR comments (in relation to the 'space for time substitution' in Mayewski et al. 2022) that it is 'not convincing' because: "the incident radiation would presumably be much lower than given here due to monsoon cloud cover" However, the incident radiation 'given here' (i.e., shown in the histogram of Mayewski et al. 2022) takes account of cloud cover. It uses the long-term (1991-2020) downscaled ERA5 insolation (which Potocki et al., 2022 showed was an excellent match to the observations). It therefore accounts for variable atmospheric transmissivity – including the effects of clouds. Note, too, that the extremely high insolation (close to top-of-atmosphere values) at the South Col AWS can be seen in Figure 4 of Matthews et al. (2020).
We make this clarification because it appears that AR was not aware of the provenance of the data used in the space-for-time comparison. A more subjective point I note here is that AR stated that a large temperature difference between Camp II and the South Col means that the melting is unlikely to occur at the latter, even if it does at the former. However, as pointed out in Mayewski et al. 2022, this temperature difference counts very little if winds are very light (which they are in the monsoon: Matthews et al. 2020, Fig. 4). Indeed, in the extreme event that the air is still, the turbulent heat flux is zero, so the lower air temperatures do not act to cool the surface (note that the right-hand side of the histogram shown in Mayewski et al. shows incident radiation, so already includes the effect of lower air temperatures on longwave radiation).
[ACC2-1b] Thanks for the clarification. This comment is for Ann Rowann. Nevertheless, we believe that this space-for-time comparison can only remain very speculative, as it is less solidly grounded than the modeling work of P22. We fully agree that radiative fluxes matter a lot when it comes to surface energy balance, however they are not the only components to take into account. As shown in our study, heat diffusion in the snow and ice plays an important role and cannot be neglected. In P22 and in COSIPY_grad simulations all the surface fluxes are similar, and still the predicted melt is completely different. We also refer to RC4-4, who shows a large variety of melt estimates for exactly the same meteorological forcings as P22. In other words, the space-for-time substitution would apply only if we would study an infinitely thin layer of ice. We also note that the temperature distributions are very different, with roughly 15 K of difference, which leaves much more room for night time cooling at SCG through sensible heat flux, despite constantly overcast conditions. We note a 1500 m elevation

difference between camp II and the South Col Glacier, which seems very large. For our alpine standards, it would mean comparing the conditions at Jungfaujoch with the tongue of Aletsch Glacier, or comparing the col du midi with the tongue of Mer de Glace.

[CC2-1c] Third, AR states that: "Determining mass change over a representative timescale of several decades requires observations of longer-term change as provided by both papers." This seems to be a misunderstanding of the methods used by Potocki et al. (2022): their conclusions were reached with the help of a 70-year record of (downscaled ERA-5) meteorological data, not just data from the 2019 season (which is the focus of Brun et al. 2022).

[ACC2-1c] Thanks for the clarification. We want to recall that COSIPY simulations performed in Brun et al. (2022) do not aim at quantifying the surface processes occurring at the glacier surface, but to warn about the extreme sensitivity of the model outputs to physical and numerical implementations. As a consequence, it does matter whether one year (2019 in Brun et al) or more are presented here. (see ARC1-3, and ARC4-3).

[CC2-1d] Fourth, AR mentions her own group's modelling work that shows annual accumulation of 7 m w.e. at the South Col. According to my understanding, this result represents precipitation minus sublimation and any melt, and hence appears physically implausible: 7 m w.e. is ~13 times the annual mean precipitation (AMP) measured at Base Camp (5,315 m asl) and ~9 times the AMP at Phortse (3,810 m asl). Considering that AR's figure is net of any sublimation (if not also melt), and both that theory and our measurements highlight a decline in precipitation with altitude, I suggest that this (non-peer-reviewed) result is physically implausible and should be discounted from further discussion.

[ACC2-1d] This discussion regarding precipitation amounts is very interesting and of particular importance for surface mass balance modelling. But we must keep in mind that the important variable is not really how much precipitation (snowfall in the case of South Col Glacier) falls at the glacier surface, but how much stays on the ground and for how long. Disentangling the true amount of precipitation from the net accumulated snow (i.e. the point surface mass balance) on the surface is impossible at this extremely windy site, and is beyond our capacities of observations or modelling. Some authors like Salerno et al. (2015) propose a decline in precipitation with altitude above 5000 m asl., but this altitudinal gradient is probably highly uncertain and anyway disturbed by topography. Moreover, the net accumulated snow on the surface is also very dependent on the interplay between wind and topography, with some locations prone to over-accumulation and others to strong erosion. In conclusion, observing one order or more of net accumulation magnitude from one site to another, only a few hundreds of meters away of each other, is common in high altitude windy sites (e.g., Vincent et al., 2017)

Salerno, F., Guyennon, N., Thakuri, S., Viviano, G., Romano, E., Vuillermoz, E., Cristofanelli, P., Stocchi, P., Agrillo, G., Ma, Y., and Tartari, G.: Weak precipitation, warm winters and springs impact glaciers of south slopes of Mt. Everest (central Himalaya) in the last 2 decades (1994–2013), The Cryosphere, 9, 1229–1247, https://doi.org/10.5194/tc-9-1229-2015, 2015.

Vincent, C., E. Le Meur, D. Six, M. Funk, M. Hoelzle, and S. Preunkert (2007), Very high-elevation Mont Blanc glaciated areas not affected by the 20th century climate change,J. Geophys. Res.,112, D09120, doi:10.1029/2006JD007407