## Reply to reviewer Ann Rowan

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.

**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.

**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).

***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).

***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.