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

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1. Introduction

We thank Brun et al. for the revised version of the manuscript. We believe the revised manuscript reads well and concise. Basically, we only have a few minor comments. However, Marcus Gastaldello, an MSc student currently simulating firn processes at Colle Gnifetti (Switzerland/Italy) with COSIPY, came across a number of issues in the code of COSIPY. These issues have recently been communicated to the COSIPY developers. The model version used in the revised manuscript (and in the paper by Potocki et al.), however, might be affected by these issues. We highlight these issues below for Brun et al. being able to assess their relevance to the simulations.

2. Known issues with COSIPY

The model employs an L-BFGS-B or SLSQP algorithm to iteratively solve the surface temperature for each time-step, by minimising flux residuals in the surface energy balance. This solver is constrained with an upper bound of the melting point of ice and uses the result of the previous time-step as an initial guess. Unfortunately, there appears to be a susceptibility for the algorithms to prematurely terminate on the upper bound of 273.16 K, before the actual convergence of the energy fluxes - particularly if the previous surface temperature value was at this temperature. Whilst this does not produce additional melt, since there is no positive excess energy, the reported surface mass and energy fluxes are incorrect. In addition, there is a missing pair of parentheses in the calculation of the ground heat flux: this issue was amended by the model developers in early August 2022, but was likely still present in the calculations of the paper by Potocki et al.

Thermal diffusion through the sub-surface layers is determined by resolving the Fourier heat equation with a second order, central difference scheme. However, the scheme uses a fixed/Dirichlet boundary condition on the basal node that effectively constrains the thermal regime to the user-defined, initial basal temperature, set in the constants file.

A volumetric approach is used to determine the composition of sub-surface layers, representing them in terms of a fractional proportion of ice, water and air. Within the refreezing module, energy is not properly conserved during latent heat release. The calculation of the internal energy increase of firn layers only accounts for the fractional mass of the converted water, as opposed to the whole layer. This results in a substantial under-estimation of the layers subsequent temperature increase. Furthermore, water is distributed to layers via a bucket approach constrained by their irreducible water content, prior to refreezing. This significantly restricts the true refreezing potential of sub-surface layers as it should be constrained by the cold content and volumetric limits of the layer.

We emphasize that the most critical of these issues, the erroneous calculation of refreezing in firn and snow, might be irrelevant to the ice-only simulations by Brun et al. and Potocki et al.

1. **Detailed comments on the revised manuscript by *Brun et al.***

Line 47: Correct us if we are wrong, but does the resolution depend on the location on the image (higher resolution for areas closer to the camera, coarser for areas further away)? This would be relevant in the extreme topography of the Everest range. Hence, if we do not misunderstand, 0.5 m is an average value? We suggest mentioning that 0.5 m is an average value.

Lines 79-85: South Col Glacier is the main focus of the paper, so maybe its dH result should be described before the dH result of the rest of the Everest region? (Although that provides a nice contextualization). Up to the authors.

Line 94: Suggest "in the thickness" instead of "of the thickness"

Line 109: Remove the number and leave “Fourteen”.

Lines 119/120: Reword: “ except at the on the lower cliff “

Lines 156/158: suggest removing "at" after "averaging"

Line 220: "abusively" - while we agree with the concept, is this proper wording?

Lines 228-229: Could the Authors provide some more details on how the model initialization works when precipitation is switched off? Does the grid after spin-up consist entirely of impermeable glacier ice, during the whole simulation of 2019?

Line 280: “local mass balance rates approaching 2 m a-1,” Shouldn’t it read -2 m a-1?

Line 282: Suggest writing “~2000”.

Lines 288: As above.

Line 293: Suggest adding the uncertainty.

Line 298: suggest "mean density" instead of "density"

Line 307: typo in "bergschrung", should read “bergschrund”

Line 308: suggest "horizontal velocity" instead of "velocity" (possibly remove "horizontal" at the next line if it sounds too repetitive)

Line 333: suggest "or" instead of "nor".

Figure 3: Suggest including "accumulation efficiency" in the legends of subplot b and c.

Figure A4 still mentions a now-removed panel about Crocus.