

# ***Interactive comment on* “Evaluation of dynamically downscaled near-surface mass and energy fluxes for three mountain glaciers, British Columbia, Canada” by Mekdes Ayalew Tessema et al.**

## **Anonymous Referee #1**

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## **Review of “Evaluation of dynamically downscaled near-surface mass and energy fluxes for three mountain glaciers, British Columbia, Canada”**

### **General remarks**

Tessema et al. (2018) compare meteorological fields simulated using the atmospheric model WRF with *in situ* measurements for three glaciers in British Columbia, Canada, and assess differences in point simulations of glacier surface energy and mass balance using these two datasets as forcing. The authors examine the impact of

model resolution and correctly specifying the underlying land surface type on their analysis, as well as compare with the positive degree-day method. Based on this work, the authors draw conclusions about the feasibility and success of using dynamical downscaling with WRF to produce forcing for glacier simulations.

This manuscript joins only a small number of other studies on this topic, and the analysis is strengthened by the availability of numerous observations for model evaluation. The manuscript is logically organized and well written, although at times unnecessarily convoluted, and the topic is well suited for The Cryosphere. However, I have a number of concerns about the numerical modelling, outlined below, that I think need to be addressed before publication. Given these issues, some of the conclusions presented in the paper are not supported by the presented analysis. Improving the study by providing more accurate atmospheric simulations will greatly strengthen its contribution to cryospheric community.

### Major comments

1. There are two key issues with the atmospheric simulations:

a. The authors have not justified their choice of model physics. The configuration does not match any of the references on Page 6 (P6), Line 9 (L9), but in any case, those studies focused on different mountainous and climatic regions. As the authors mention, the choice of physics has a large impact on WRF results and should be optimized based on a subset of the observational data or justified in some way.

b. The land surface in the closest grid cell to the observations in the ablation zones is not classified as land ice. This inconsistency is easy to fix manually in the geo\_em files before running the WRF pre-processing programs. Manual correction in the finest resolution domains (WRF 2.5 and 1.0) would appear to result in only one incorrect land-use categorization at observational points, for the southern off-glacier AWS at

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Castle Creek (cf. Figure 2). Reliable conclusions about the model's ability to reproduce local meteorological conditions and katabatic flows cannot be drawn when the bottom boundary conditions are incorrectly specified, and as a result, there are no glacierized grid cells neighboring the one containing the station (WRF 1.0 at Castle Creek) or there is only a single glacierized grid cell (WRF 2.5 at Nordic Glacier).

The authors attempt to address the second issue by manually changing the land-surface type in the grid point containing the AWS at Nordic Glacier. However, this is the smallest glacier studied and may represent an underestimate of the impact of atmosphere-glacier feedbacks on the presented results. In addition, the horizontal resolution of the finest domain (WRF 2.5) is not well suited for this study site, as the authors acknowledge on P27, L15. For these reasons, I think the simulations should be repeated with accurate bottom boundary conditions (see minor comment 2 about SST). This change would also help to streamline and simplify the manuscript (i.e., the authors could remove P6, L29-34; P13, L17-26; P14, L24-29; Section 3.4).

Please see my minor comments for a few more questions and concerns about the WRF simulations.

2. The approach to the glacier simulations underutilizes the information provided by WRF, perhaps due to the incorrect land-surface categorization. For example, for most of the SEB simulations presented in the paper, daily mean albedo is specified from observations for both AWS- and WRF- forced runs (e.g., P20, L1; P21, L8) rather than using the simulated WRF value (which should be optimized in the source code, see minor comment 9). They use a calibrated value for fresh snow density and apply a temperature threshold for determining the frozen fraction of precipitation, however both of these fields are available from the microphysics scheme. This approach, in particular the albedo treatment, makes the comparison between the two forcing datasets less

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informative than it could be.

### Minor comments

1. P6, L7: Why did the authors create a new set of domains for the simulation down to 1-km grid spacing at Castle Creek? Other options would be to nest a fourth domain or use the WRF program ndown to force two separate D3s of 2.5-km and 1-km grid spacing. The latter would be the most consistent in terms of lateral boundary information, and both would be more numerically efficient.
2. P6, L15: Why was SST kept constant and how does this impact the simulations, some of which exceed two months in length? This time-varying field is provided by ERA-Interim and is easily incorporated into the simulations using the wrflowinp bottom boundary updates.
3. P6, L24: Please provide the exact spin-up time and a reason for this choice.
4. P7, Table 3:
  - a. Are the timesteps correct? If yes, is the model solution stable and physical with a timestep of 20 times the grid spacing at Castle Creek?
  - b. In complex terrain, diffusion should be computed in physical space (diff\_opt set to 2) for more accurate results where coordinate surfaces are sloped.
5. P8, Figure 2: The authors state that they updated the land-use data using ESA CCI, however certain areas that appear to be at least 506. P9/10: The paragraph explaining the bulk aerodynamic method could be removed, since it is well established and the reader is referred to Fitzpatrick et al. (2017).
7. P10, L15-16: What ice/snow albedos are supported by the measurements?
8. P16: I suggest showing the comparison for precipitation, as it plays a role later in the modelled surface height changes.
9. P24, L8: The albedo for glacierized grid cells is a prognostic variable in WRF. The default lower bound (variable ALBICE in phys/module\_sf\_noahmp\_glacier.F) is set unrealistically high at 0.67 and should be changed to a value consistent with bare ice for more realistic simulations and atmospheric feedbacks.
10. P26, L2: Please rephrase to match what is described in the methods.

11. P28, L17: Further optimization may be possible. For comparison, we run a three-domain configuration down to sub-kilometer grid spacing with dimensions exceeding 300x300x50 and are able to complete more than 20 simulation days in one day of wall-time on 500 processors.

### Technical comments

1. I suggest changing “near-surface” to “surface” in the title, as only surface mass and energy fluxes are considered.
2. P1, L5: for clarity, I suggest changing “nested within the ERA-Interim” to “forced at its lateral boundaries by ERA-Interim.”
3. P1, L6: change “spatial resolution” to “grid spacing.”
4. Section 2.1: I suggest referencing Figures 2 and 3 where applicable to make it easier for the reader to follow the station locations.
5. P6, L2: “advanced research version of the WRF model.”
6. Throughout the paper, please change “(see text)” to refer to the relevant section.
7. P14, L28 and elsewhere: I think the phrase “(not shown)” is overused in the manuscript. I suggest removing some of the statements if they are not important or introducing supplementary material.
8. P27, L4-5: This sentence appears nearly word-for-word in Collier et al. (2013).

### Tables Figures

Figure 1: I suggest adding shaded model topography.

Figures 2 and 3: Please label the axes or provide a scale. For Figure 3, is there a pink triangle?

Table 3: Please provide the grid dimensions.

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