Subgridding High Resolution Numerical Weather Forecast in the Canadian Selkirk range for local snow modelling in a remote sensing perspective

<u>REVIEWER 1</u>

Billecocq at al present an interesting study on refining the spatial resolution of meteorological forcings to feed a detailed snow model (ALPINE3D). The ultimate goal is to test whether this refinement improves the representation of snow microstructure as relevant to SWE retrieval algorithms from satellite. Results show improvements for the optical grain size and SWE for two seasons of data in the Glacier National Park in Canada.

Overall, the study is well conceived and the paper is well written and concise. The topic is relevant and within the scope of TC. At the same time, there are in my opinion a number of major and minor points that should be addressed before publication. Thus I am recommending a major revision.

Thank you for your valuable feedback and constructive comments. We have revised the manuscript accordingly.

The first major comment is that all snow evaluations are performed using simulated (not observed) time-series at only three locations over the study area. In the discussion, authors are clear on this being a limitation of their study (lines 312). However, I think this point should be better addressed throughout the manuscript as the main critical aspect of this work. Ideally, the best solution would be to include observations in this evaluation exercise, but it may be that such observations are not available at the considered study site. So I see two potential alternatives: (1) include results from other regions where such data are available, and/or (2) better discuss accuracy and precision of SNOWPACK simulations forced using AWS data using reference literature (e.g., https://tc.copernicus.org/articles/9/2271/2015/)

This comment was addressed in several ways. The Abbott and Fidelity stations are equipped with SR50 instruments to measure snow depths. Time series were included and the SNOWPACK simulations were evaluated against it. Moreover, a paragraph was added in the discussion to discuss the accuracy and precisions of the SNOWPACK simulations. Previous work on that matter was conducted in our research lab, and we rely on the work of Madore et al 2018 and Madore et al 2022

https://www.tandfonline.com/doi/abs/10.1080/02723646.2018.1472984
https://www.frontiersin.org/articles/10.3389/feart.2022.898980/full

Second, results are promising with regard to snow depth / SWE, but quite incremental when looking at the optical grain size and density (see line 16 and then the results section). The same could be said with regard to weather forcing data, where a clear benefit of downscaling is evident (in my opinion) for radiation and humidity, while results for temperature and precipitation are mixed. While authors are again clear on this (see the discussion section for example), and while I totally see the main point of novelty provided by the authors (line 325), I am still wondering what is the significance of this work for the global audience of TC given these mixed results and the fact that authors focused on a comparatively small region and two years of data. To overcome this, I am proposing to (1) include a clearer justification regarding the choice of this study region, including why it is important for the global readership of TC; **(2) significantly expand the Discussion section with much clearer statements of the main findings, implications, and future steps in view, and in the context, of the relevant literature**; (3) ideally, include specific research questions in the Introduction to further generalize findings.

The Study Site section has been enriched with a few sentences justification on why this study site is relevant for such a study.

This study was conducted in the Rogers Pass area of Glacier National Park (GNP), British Columbia, Canada (Figure \ref{fig:GNP_map}), which is part of the Selkirk range in the Columbia Mountains. The pass is used as a transportation corridor by the Trans Canada Highway and the Pacific Railway, making it the busiest transport corridor in Western Canada \citep{bellaire2016}. The pass is exposed to 144 avalanche paths, and as a result, Rogers Pass hosts the largest avalanche control operation in Canada \citep{delparte2008a}. The operation has been ongoing since 1965 and the site has been used as a snow research site ever since, making this area the longest record of mountain snow in Western Canada \citep{fitzharris1987, bellaire2016, madore2022}.

The Discussion section has been significantly altered, with clearer statements of the achieved results, and a perspective on how these results will transfer to the field of remote sensing, which is the application domain for the proposed framework.

Finally the Introduction has been modified to further outline the need for the proposed research and where it stands with regards to the state-of-the-art. 3 Research questions emerge from the paragraph and then answered in the Discussion and Conclusion sections.

1. How do subgridded HRDPS forecasts compare to reference Automatic Weather Stations in the simulation domain ?
2. Do the resulting atmospheric forcings lead to an improvement in snowpack modelling, especially for critical snow parameters in remote sensing applications ?
3. Which degree of spatial variability with regards to snow parameters can be reached by such a subgridding framework ?

Minor / specific comments

- Abstract: in my view, the abstract focuses too extensively on background information (up to line 10). I would recommend summarizing this background information to focus on the main findings and implications

A few sentences on the remote sensing background have been removed from the abstract, results have been updated, and an emphasis has been put on the perspectives and applications.. Here is the updated version of the abstract.

Snow Water Equivalent (SWE) is a key variable in climate and hydrology studies. Yet, designing a SWE retrieval algorithm is not trivial, as multiple combinations of snow microstructure representations and SWE can yield the same radar signal. The community is converging towards forward modeling approaches using an educated first guess on the snowpack structure. However, snow highly varies in space and time, especially in mountain environments where the complex topography affects atmospheric and snowpack state variables in numerous ways. Automatic Weather Stations (AWS) are too sparse, and high-resolution Numerical Weather Predictions systems have a maximal resolution of 2.5 km × 2.5 km, which is too coarse to capture snow spatial variability in a complex topography. In this study, we designed a subgridding framework for the Canadian High Resolution Deterministic Prediction System. The native 2.5 km × 2.5 km resolution forecast was subgridded to a 100 m × 100 m resolution and used as the input for snow modeling over two winters in Glacier National Park, British Columbia, Canada. Air temperature, relative humidity, precipitation and wind speed were first parameterized regarding elevation using six Automatic Weather Stations. Alpine3D was then used to spatialize atmospheric parameters and radiation input accounting for terrain reflections and perform the snow simulations. Modeled snowpack state variables relevant for microwave remote sensing were evaluated against profiles generated with Automatic Weather Stations data and compared to raw HRDPS driven profiles. Overall, the subgridding framework improves on average the optical grain size (OGS) bias by 18\%, and the modelled SWE by 16\% with regards to simulations driven with raw HRDPS forecasts. This work could lead up to a 7 dB improvement in the snowpack SAR backscattering modelling, and hence provides the necessary basis for SWE retrieval algorithms using forward modeling in a Bayesian framework.

- line 8: this maximal resolution of 2.5 km is likely specific for Canada datasets (?)

Yes, this sentence has been rephrased as : “Moreover, HRDPS spatial resolution is too coarse to properly represent [...]”

- line 31: this statement on models yielding biased estimates of SWE at high elevation is likely too generic. Several correction approaches in this regard have been documented, but results are very site specific (which is in my opinion the actual main challenge here)

This statement has been rephrased as:

Moreover, both observations from passive microwaves and modeling efforts yield negative biases when estimating mountain or deep-snow SWE on the global scale \citep{vuyovich2014, wrzesien2018, pulliainen2020}.

- line 49: I think that the main reason why AWS spatial interpolation in complex terrain is not accurate is because AWS systems undersample the real spatial heterogeneity of the processes (which is not mentioned here)

We agree with the reviewer, and this is what was meant behind the "local biases" in the original sentence. It has been rephrased in order to make this idea clearer to the reader:

However, they need human maintenance, are subject to outages, local biases, and in most cases undersample the spatial heterogeneity of the processes at stake (one would need an exceptionally dense array of weather stations), especially in complex terrain. As a result, AWS spatial interpolation in mountainous areas is not always accurate \citep{lundquist2019}.

- line 51: AWS systems are also prone to undercatch and so underestimation of precipitation (this is one of the main reasons why I think it would be ideal to include actual measurements of snow properties in the evaluation).

We agree with the reviewer and SR50 HS measurements have been added to the data presented in the paper. The sentence has not been modified here, as the idea behind it is to show the reader that AWS have biases (idea developed in the previous sentence), but so do atmospheric models.

- line 76: please avoid reporting units in italics

This has been corrected everywhere in the manuscript.

- Figure 1: consider including a DEM here

The figure has been modified to include elevation information for the reader.

- Table 1 and all other captions: please consider defining acronyms in captions for diagonal readers

The Table's caption has been edited.

- line 88: please specify "most of Canada"

This is how Environment and Climate Change Canada describes the HRDPS forecast. It covers the vast majority of Canada, only leaving out the most northern islands or the Arctic archipelago.

- line 100 to 110: correction factors for temperature, radiation, and precipitation are very succinctly presented, to the extent that repeatability of these experiments may be difficult. How was Eq. 1 derived (what data were used? What period? What optimization approach?). Same for Eq. 2. Why was Equation 1 used for dew point temperature too?

Descriptions for the TA and PSUM correction equations have been updated with more details. For Relative Humidity, no detail was added as the methodology we used is the exact same as the one presented in the cited reference (Liston and Elder, 2006). They present the conversion of RH to dew point temperature which is then corrected according to an elevation lapse-rate equation to account for elevation discrepancy. Then dew point temperature is converted back to RH. Having developed a correction equation for temperature more suited to our study site, in our opinion iit makes only sense to use this one rather than the standard lapse-rate equation.

Updated description for the TA correction equation:
Using all weather stations in the Park, bias in air temperature was found to have a non-linear relationship with the elevation difference between the station elevation and the original HRDPS cell elevation over the 2018-2020 period. A training set was generated by randomly selecting 75\% of this dataset uniformly across elevations, and the remaining 25\% served as validation set. The data was transposed into logarithmic space to perform a linear regression. The resulting logarithmic fit was then applied over the TA dataset when the elevation difference between the Virtual Weather Station and its overlying HRDPS cell was over 100 m.

Updated description for the PSUM equation:
Snowfall was first parameterized using an elevation lapse-rate correction. This lapse rate was computed by performing a simple linear regression of precipitation as a function of elevation. We used a dataset of four weeks of manual SWE measurements on four conventional HN24 precipitation boards placed between 1330 m and 1920 m at Mt Fidelity, all placed in areas sheltered from the wind.

- Line 136: why did you first use a 20-m DEM and now a 100-m one?
The 20 m DEM is used for the parameterization of the Virtual Station array. The 100 m DEM is the grid upon which the spatialized snow simulations will be computed. It would not make sense to run spatialized snow simulations at a 20 m resolution both from a computation performance stand-point and a spatial variability process. A paragraph has been added in the introduction to further justify this resolution choice for snow simulations:

Moreover, HRDPS spatial resolution is too coarse to properly represent the spatial variability of atmospheric parameters and SWE in complex terrain. Indeed previous work on spatial variability of SWE and atmospheric parameters have shown that a scale break appears for SWE in the [50, 100] meters grid resolution interval, [100 m, 250 m] for wind exposure, [100 m, 180 m] for vegetation height, and [90 m, 100 m] for incoming solar radiations, leaving the optimal grid resolution at a 100 m for mountain processes \citep(grunewald2010, Winstral2014, Rittger2016). Finally, 100 m resolution ties in well with operational SAR satellites products and their processing pipelines, such as Sentinel-1 or TerraSAR-X, as well as future missions \citep(Derksen2021).

- Section 3.3: the inflation approach is clear, and I generally agree with this. At the same time, microstructural parameters are (to some extent) dependent on SWE and HS (via overburden pressure and temperature gradients, for example). If authors agree with this, I would add some discussion on how this could impact these results.

We agree with the reviewer and these concerns are now raised and discussed at the end of this section :

As precipitation is usually underestimated by HRDPS, HS should be underestimated as well, which should impact the overburden pressure on basal layers. This might result in a small negative bias on density with regards to AWS driven SNOWPACK runs, depending on the amount of missing snow. For OGS, the temperature gradient in this region is low and metamorphism mainly happens through gravitational settling, leading to little variability in OGS in the snowpack \citep{madore2018}. As a result, we do not expect much impact of the inflation approach on this microstructure parameter, as the main discrepancies should come from offsets in rain-on-snow modeling, and melt/percolation events.