Response to reviewer comment #2. Note reviewers' text is shown in **blue**, with responses in **black**.

The manuscript submitted by Pletzer et al., is a preliminary step toward applying WRF-Hydro/Glacier to a cold-based Antarctic glacier. Generally, the manuscript is well written, figures and data are presented well and the subject matter is a good fit for the journal. There is potential for this approach to reveal new understanding of the MDV hydrologic system as a whole. However, in agreement with the other reviewer, this present manuscript represents in intermediate methodological step and demonstrates no real advance in scientific understanding as written.

Thank you for taking the time to review this manuscript. This review was helpful in identifying that the aim of the manuscript needed to be refined and that the gap in the literature needed to be more explicitly communicated. This feedback has significantly improved the quality of the manuscript. We have addressed the major comments by rewriting the introduction, research aim and conclusion to explicitly identify the scientific advancement of the manuscript. In addition, we have added a schematic of the WRF-Hydro/Glacier modelling framework to clarify the model setup and components used in this paper. We have also responded to the comments below.

Major Comments:

As this work is presented, the introduction suggests that the full WRF-Hydro/Glacier model is required to make inferences about glacier-stream-lake hydrologic connectivity. However, the rest of the manuscript is focused on the point-based simulation and tuning of Crocus and NoahMP rather than the full model itself. There is no scientific question or hypothesis to be addressed. This paper at a minimum should be reframed to identify and explicitly address the scientific advancement made by this paper alone, not the future applications of the model. A more impactful contribution could involve running experiments with the model.

As suggested, we have clarified the novelty and advancement of the current work, as well as explained the rational of the methodological approach by adding a schematic of the modelling framework and reframing the introduction, research aim, and conclusion. Please refer to the changes suggested in RC1 that address these points.

Minor Comments:

Why was such a short period used for model spin-up? Since the COHM met station was used for spin-up anyway, there are many years of data available from that site. Why limit it to a few months? Also, as the other reviewer notes, might this have some impact on temperature bias shown in the results?

The purpose of this study is to apply the WRF-Hydro/Glacier modelling system at a point, identify where the model needs modifications for this unique environment and provide robust solutions to ensure the onset, duration and end of melt are resolved. As noted in Lines 149-150, the ice temperatures are initialized at the mean annual temperature of -18 °C, from Obryk et al. (2020).

We changed Line 153 to: We analyzed ice temperatures at the end of the spin-up period and found that the difference between observed and modelled ice temperatures at a depth of 0.05, 0.1, 0.2, 0.5 and 2.0 meters at the beginning of December are less than 1 °C, which is within the sensor uncertainty shown in Table 1."

I'd like to see more discussion on how the 2021-22 season relates to the long-term average climate here. Was this a warm, cold, snowy, cloudy etc. season? Since the modified albedo scheme (and overall model tuning and results) were so dependent on data from a single season, it would be nice to provide more context for this season relates to typical summer conditions for this glacier. There is potential to overfit the model for this set of conditions and it may not perform well for colder or warmer seasons. Was this assessed?

We have added the following comparison to show how this season compares to the typical summer to the end of Section 3.3:

"Comparing the 21/22 season to the 1999-2022 long term average over December and January (Hosteenge et al., 2023, *in review*), we find that conditions were typical. Air temperatures were 0.2 °C below average and it was 0.1 m/s less windy. There was less cloud since incoming shortwave radiation was 43.4 W/m^2 above average, incoming longwave radiation was 6.9 W/m^2 below average and albedo was 0.02 below average."

Please provide more detail on the vertical layer thickness and spacing. Based on figure 3, it is not uniform. This scheme is critical for accurately representing processes that have strong gradients in the shallow subsurface.

We have added this text to Line 87:

"The number of and thickness of vertical layers in Crocus changes dynamically with time. Users define a maximum number of layers ($n \geq 3$) and when snowfall occurs, a new layer is added with a set of fresh snow characteristics. Over time, layers may merge with the layer below if the snow grain properties become the same. The layers at the top of the snowpack tend to be thinner to better solve the surface energy balance equation."

How was overfitting assessed for the Albedo modifications?

We agree that the term overfitting could cause confusion to readers and have removed this. We have removed Line 276 and changed Line 275 to "The newalbedo model better captures the variability in observed albedo over the melt season with a root mean square error of 0.08 compared to oldalbedo with a root mean square error of 0.35."

Please provide more detail and comparison of data, instrumentation and accuracy across the two met stations.

Added to Line 112: "The accuracy of the sensors are similar to CWG AWS and the instruments are detailed in Gooseff et al. (2022)."

L121 – Sentence is a repeat from on the previous section

Sentence has been removed.

Section 4.2 – The beginning section as well as a few introductory sentences in paragraphs elsewhere in the section are basically only telling the reader what they will be told later and are therefore unnecessary and should be rewritten.

Removed Lines 200-203 and Line 257.

L355 – this citation is incorrect.

done