#essd-2024-451 - Author's response

Insights into the North Hemisphere daily snowpack at high resolution from the new Crocus-ERA5 product

General statement

We are grateful to Reviewer #1 for the constructive comments and suggestions, and to Reviewer #2 for the supportive and focused feedback. We also thank the editor for overseeing the review process. Together, these contributions have helped us improve both the clarity and overall quality of the manuscript. Below, we provide detailed point-by-point responses, with our changes highlighted in blue for clarity.

Reviewer 1

General Comment — As noted in the original review, this paper can provide an important contribution; however, many of the key concerns expressed in the first round of review remain. Importantly concerns regarding how to meaningfully visualize results remain and there is a substantial lack of supporting citations throughout. Specific comments are included below.

Reply: Thank you for recognizing the value of our contribution. We addressed each of the specific instances you highlighted in the detailed responses below. Regarding the visualization concerns raised in your comments, we carefully revised each figure to align with your feedback and ensure clarity.

As for the references, we thoroughly reviewed the manuscript and expanded the citations to better situate our work within the existing literature.

Reviewer Comment 1.1 — Introductory paragraphs require supporting citations (e.g., P2 and P3 have minimal citations).

Reply: Thank you for pointing this out. We have revised Paragraphs 2 and 3 to include several key references supporting the statements on sea ice, continental snow cover, permafrost, and Arctic feedback processes, as well as on Arctic monitoring efforts.

Reviewer Comment 1.2 — Section 2, starting at L75: arguments made in this section should be strengthened with supporting citations. This sentiment should be considered throughout the article.

Reply: We have addressed this comment by adding supporting citations throughout Section 2 (starting at line 75) and across the rest of the manuscript to substantiate each key statement, ensuring that the arguments are now systematically backed by relevant and up-to-date references.

Reviewer Comment 1.3 — L133: Since the early 2010s not "2010"

Reply: Done.

Reviewer Comment 1.4 — Section 2.3: More detail on the calculation of evaluated variables is valuable (e.g., mathematical representation of a snow depletion curve used to calculate snow cover extent).

Reply: We appreciate the reviewer's suggestion. In response, we have expanded the description in Section 2.3 to clarify how snow cover extent is evaluated in our analysis. Rather than applying a predefined analytical snow depletion curve, we examined the modeled relationship between snow-covered fraction (SCF) and snow depth using spatially weighted outputs from both study domains.

The resulting scatterplot reveals two distinct linear behaviors, suggesting a dual-regime structure in the snow depletion process: one characterized by shallow snow depths with large SCF variability, and another where snow depth increases more rapidly with SCF.

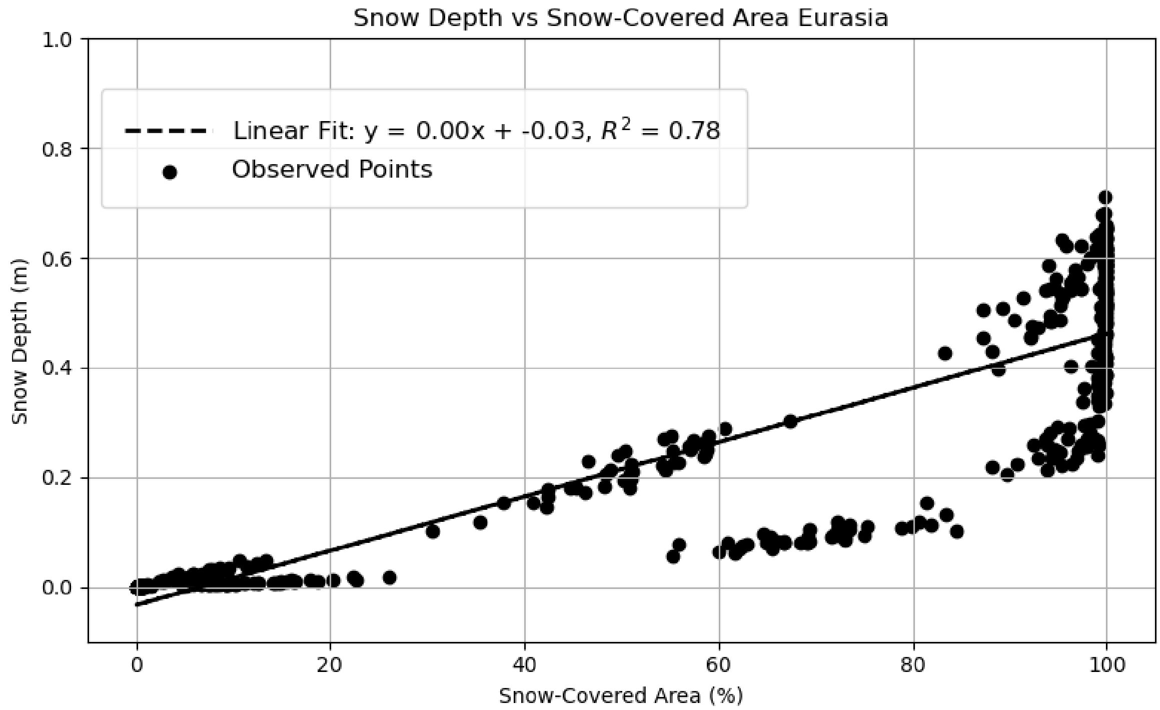
This clear dual-linear structure is present in both regions analyzed — Eurasia and North America (Canada) — and is interpreted as evidence of two distinct snowpack regimes: (i) a shallow-snow regime, where SCF varies primarily due to patchy, residual snow cover; and (ii) a deeper-snow regime, where snow depth increases more rapidly with SCF — that is, the slope of the snow depth–SCF relationship is steeper — indicating the development of a continuous snowpack.

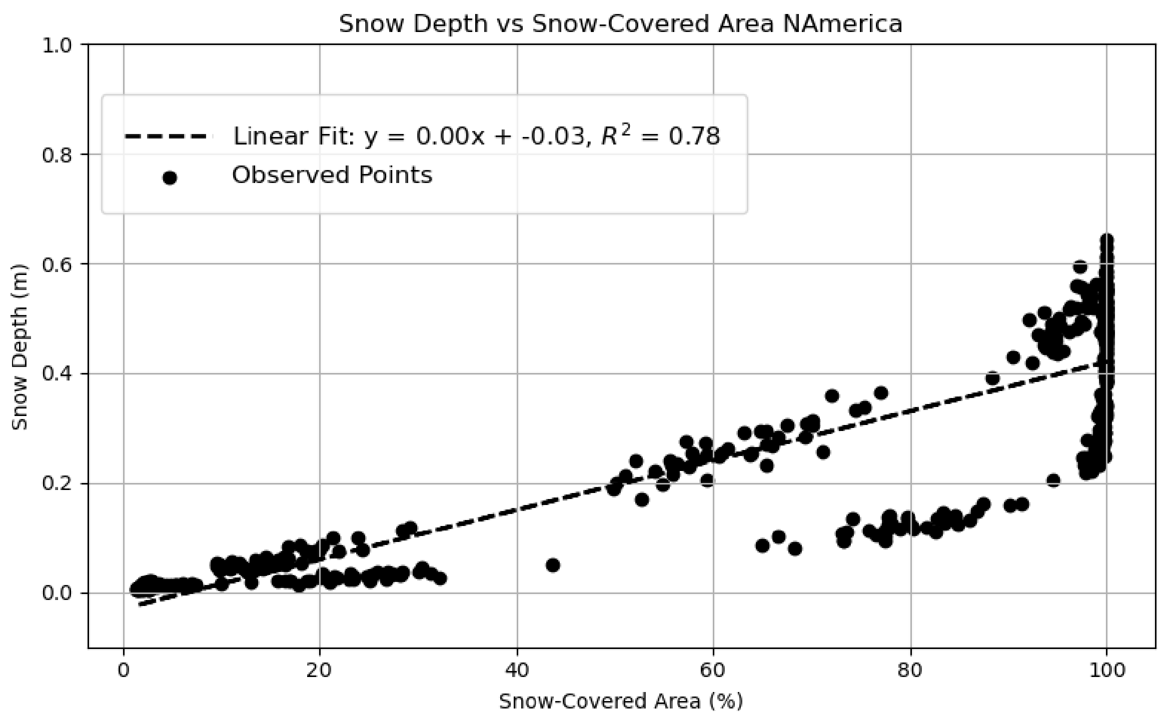
In the shallow-snow regime, the flatter slope suggests greater SCF sensitivity to surface heterogeneity and local bare-ground exposure than to snow depth itself. In contrast, the steeper slope in the deeper-snow regime reflects the influence of a more uniform, depth-driven snow cover.

The coexistence of these two regimes points to a transitional behaviour in snow depletion dynamics, marking a shift from patchy, remnant snow to extensive, continuous snowpack. This analysis highlights that the fraction of bare ground — rather than snow depth alone — is the main driver of SCF reduction during the melt period.

Although no additional figure has been included, this interpretation is now explicitly discussed in the revised text (Section 2.3) to clarify the physical basis of the snow depth-SCF relationship in the model outputs.

We believe this addition provides a physically grounded response to the reviewer's request for detail on how snow cover extent is evaluated in our study.





Reviewer Comment 1.5 — Section 2.5: There is no guarantee that the in-situ snow depth observations, particularly with these observations tending to be in clearings, provide spatial representation allowing a fair comparison against simulation snow depth at a course (0.25-

degree) resolution. There needs to be a qualification of this point. The elevation difference was considered, which is good, but other heterogeneities (e.g., from land cover, aspect, wind redistribution, etc) can play important roles.

Reply: We agree with the reviewer that the in-situ measurements, while mostly taken over open ground in accordance with WMO standards, may not fully capture the variability within a 0.25° grid cell. To clarify this point, we have added the following sentence at the end of the relevant paragraph in Section 2.5: "While such site selection ensures good consistency with the open-field herbaceous configuration of Crocus-ERA5, it does not guarantee full spatial representativeness of a 0.25° grid cell. Sub-grid heterogeneities such as partial forest cover, slope, aspect, or wind-driven redistribution may still locally influence the comparison between observations and simulations."

Reviewer Comment 1.6 — IMS has known uncertainties and this should be addressed.

Reply: We agree with the reviewer and have added a sentence in Section 2.5 acknowledging the known limitations of IMS, including cloud-snow discrimination issues, coarse resolution at high latitudes, and potential misclassification during transition seasons, while also justifying its use in this study.

Reviewer Comment 1.7 — L230: The claim that forests do not have an important impact on interannual variability needs to be supported, proven, or removed. If removed, this limitation needs to be addressed.

Reply: We have rephrased the text to clarify that the conclusion is supported by the multi-product evaluation of Mudryk et al. (2025), which assessed 23 gridded SWE products, including Crocus-ERA5. Their results show that, for products without snow data assimilation and forced by the same meteorological reanalysis, interannual variability is primarily controlled by large-scale temperature and precipitation forcing, rather than by specific land cover representations. In this context, the absence of explicit snow–forest interactions in Crocus-ERA5 mainly affects the mean snow depth, while year-to-year anomalies and trends remain consistent with other top-performing products.

Reviewer Comment 1.8 — L244: "Negative" rather than "Negatives"

Reply: Done.

Reviewer Comment 1.9 — Figure 1: In my original comment I had requested anomaly diference plots, rather than only a presentation of the anomalies. This point remains important to visualize the diRerences between ERA5 and ERAI. Showing the anomalies, as currently done, is valuable for the reason the authors' noted in their original response, but visualizing diRerences is also very important to understand discrepancies between the 2 versions. I disagree with the authors' argument on maintaining the calendar year ordering, as noted in their response, the snow accumulation starts in Oct-Nov (i.e., the start of the water year), so it would make sense to present results in this manner. This note applies also to Figure 7.

Reply: As suggested, we have revised Figure 1 (and Figure 7) to now include plots of the monthly anomaly differences. This representation directly addresses the need to visualize systematic differences between the two reanalyses beyond the individual anomaly patterns. In the new Figure 1 — which shows ERA5 anomalies minus ERAI anomalies — the differences span a much narrower range (–0.02 m to +0.02 m) compared to the original anomalies (–0.1 m to +0.1 m). This required adjusting the color scale to preserve visual contrast and readability.

Regarding the time axis of the climatology, we acknowledge the reviewer's point concerning the snow accumulation season. To maintain consistency with other datasets and figures organized around the calendar year (e.g. Mortimer et al., 2020), we have initially retained the January–December ordering. However, we also recognize that aligning with the hydrological year (October–September) is more appropriate for interpreting snow-related processes, and we have therefore adopted this time structure in the revised figures.

Reviewer Comment 1.10 — More clarification is needed for the presented anomaly calculation. For example, in panel 3 in 1995 there are negative snow anomalies in typical high SWE months (April-June) whereas SWE anomalies are positive in summer months when SWE tends to be minimal (Jul-Sept). In the calculation of the anomalies, are the respective monthly averages subtracted or the total time series average? If the latter, this result would suggest that SWE is relatively higher in summer months which would be perplexing. Thus, I assume the former is the case here which should be articulated.

Reply: We confirm that the monthly anomalies are computed by subtracting the *respective monthly climatologies* (1979–2018) from each dataset. That is, each month is compared to its own long-term monthly mean (e.g., April 1995 is compared to the April climatology), ensuring that the anomalies reflect deviations from typical seasonal behavior. This has been clarified in the revised manuscript. Regarding the example mentioned for 1995 in Panel 3, where *negative anomalies appear in April–June and positive anomalies in July–September*, this pattern is consistent with the method described above and can be explained as follows:

1. The **April–June negative anomalies** indicate that SWE in those months was *lower than the average SWE for those same months over 1979–2018*. This could result from below-average snowfall or earlier-than-usual melting during that year.

2. In contrast, **positive anomalies in July–September**, despite SWE typically being minimal, suggest that a small amount of residual snow remained in certain areas, *more than what is climatologically expected for summer*. In summer, because the climatological snow depth is close to zero, even minimal residual snow in the model may appear as a positive anomaly.

3. These summer anomalies are mainly driven by the model's internal snow processes (e.g., energy balance, compaction, metamorphism), which govern the persistence of snow in isolated regions. In such low-snow periods, small differences in model physics can generate noticeable anomaly signals, though their *absolute magnitudes remain low*.

We have revised the manuscript to clarify both the anomaly calculation method and this type of seasonal interpretation.

Reviewer Comment 1.11 — Figure 2: Snow cover area, which is often represented as snow cover fraction in comparisons, is physically meaningful (e.g., for surface albedo) and allows comparisons across regions of diferent sizes. I maintain that showing the snow cover fraction here would be meaningful and would be a valuable supplementary figure.

Reply: I agree with the reviewer's point about the physical relevance of snow cover fraction (SCF) and its usefulness for interregional comparisons. However, while SCF is a meaningful variable at the grid-cell scale (ranging from 0 to 1), its use for hemispheric-scale anomaly time series is less straightforward. SCF is a fractional quantity defined at each model grid point and is not directly comparable across models or observations unless weighted by grid-cell area. It is difficult to aggregate or interpret SCF meaningfully over time without accounting for differences in grid cell area. By contrast, snow cover extent (SCE) provides an area-integrated metric that aligns more closely with the methodologies used in prior assessments and enables consistent comparisons of interannual and regional variability. Since SCE is derived from SCF, we have therefore retained the SCE-based approach in the main manuscript.

Reviewer Comment 1.12 — Figure 2: ... The caption notes "Filled" circles, but the circles are empty.

Reply: It was a mistake. The caption has been corrected.

Reviewer Comment 1.13 — Figure 2: ... In the original response, the authors' note that the figure format is selected to maintain consistency with prior studies to allow easy comparisons, which makes sense, however these 2 studies do not seem to be cited in this article.

Reply: The diagnostic we used – the Arctic SCE anomaly index – is consistent with the methodology applied in the NOAA *Arctic Report Card 2024* (available at: `https://arctic.noaa.gov/wp-content/uploads/2024/12/ArcticReportCard_full_report2024.pdf`), which was itself inspired by the approach of Callaghan et al. (2011, *Ambio*, `https://link.springer.com/article/10.1007/s13280-011-0212-y`). These studies analyze anomalies in snow cover extent (SCE) and duration (SCD). In the revised manuscript, we have explicitly cited both references to clarify the provenance of this diagnostic. Specifically, we compute standardized monthly SCE anomalies (relative to the 1991-2020 climatology) for the North American and Eurasian sectors ($\geq 60°N$) and apply 5-year running means to highlight long-term seasonal variability.

Reviewer Comment 1.14 — Figure 4: I maintain that showing a difference plot is highly valuable here. If the authors worry that this would add too much complexity to this figure, then the difference can be shown in supplementary. The difference would be highly insightful to present observed vs. modeled differences in snow depth between climatological periods.

There is also a key issue with the current representation in Figure 4. Namely, it seems the goal with this figure is to show whether the model captures the spatial distribution of snow depth relative to observations, but in areas with overlapping circles that are filled with color, there is no way to see the underlying observation color in the map. Therefore, it would be appropriate to present a bias map of the circles (small enough to reduce overlap) colored by the model bias (e.g., using a polar color scheme from red to blue to present underestimates to overestimates).

Reply: We thank the reviewer for these suggestions. Indeed, the visual overlap between the filled observation circles and the underlying model field is a deliberate feature, designed to reveal agreement or disagreement between observed and modelled snow depth through colour correspondence. Observation circles are filled using the same color scale as the background model field: when observations and model values match, the circle blends with the background, while contrasting colors highlight discrepancies. For example, when the model overestimates snow depth relative to the observation (often in mountainous regions), the circle visually interrupts the field, drawing attention to this difference. To improve clarity, we have also added black outlines to all observation circles.

Regarding the suggestion to include a difference plot, we agree that it provides valuable insight. Thus, we have included a plot in the figure showing modeled minus observed climatological snow depth differences. The color scheme is designed to clearly highlight under- and overestimates, allowing readers to better assess spatial biases.

Reviewer Comment 1.15 — Figure 7: I largely disagree with the authors' argument to not show SCF, which I consider potentially more informative than a comparison of anomalies due to the physical meaningfulness of SCF. Furthermore, an anomaly comparison will mask systematic biases which are important to present. If the authors would like to present standardized anomalies, this is reasonable, but the physically meaningful comparison of SCF, which has the ability to provide a presentation of systematic biases, should be included as well (e.g., in supplementary). The SCE comparison is useful but has lower granularity.

The original suggestion of showing the difference panel remains, as this is needed to easily visualize the discrepancies between the model and observations.

Reply: We reiterate our thanks to the reviewer for highlighting the importance of presenting the SCF, which provides a physically meaningful basis for identifying systematic biases. The manuscript already includes maps of SCF differences (Crocus-ERA5 minus IMS estimates, Figure 9), fulfilling the original suggestion to visualize discrepancies between model and observations. These maps show the seasonal mean SCF bias over the Northern Hemisphere and are now explicitly referred to as SCF difference maps (bias) in the revised captions and main text.

In response to the reviewer's comment, we have further clarified the relationship between SCF and SCE. While SCF is highly relevant at the grid-cell scale (ranging from 0 to 1), its aggregation into hemispheric-scale anomaly time series can be prone to errors, because SCF is defined per grid cell and is not directly comparable across models or observations without accounting for grid-cell area. By contrast, SCE provides an area-integrated metric that allows robust comparisons of interannual and regional variability. Importantly, SCE is derived from SCF, and the larger aggregated values are statistically more robust.

Accordingly, the revised manuscript retains anomaly comparisons based on SCE for the monthly mean over the Arctic Northern Hemisphere, while also providing SCF difference maps (bias) to explicitly show physically meaningful deviations, as suggested by the reviewer. This ensures that both the granularity of SCF and the robustness of SCE are clearly communicated.

Reviewer Comment 1.16 — Figure 9: The color bar label should note snow cover biases, rather than snow cover. A polarized color bar here is appropriate.

Reply: We have revised the figure accordingly. Initially, we used a categorical color scale to highlight low SCF values, because of it makes small differences more visible. However, following the reviewer's recommendation, we have revised Figure 9 to use a diverging (polarized) color scheme centered on zero, with the color bar label now explicitly indicating snow cover bias. This improves consistency and makes overestimations as underestimations easier to interpret.

Reviewer Comment 1.17 — In the original response the authors' note that a detailed assessment for key snow-related variables in this data set (e.g., SWE and snow albedo) are beyond the scope of this study. Yet the opening sentence in the abstract is: "This article provides a detailed analysis of the Crocus-ERA5 snow product covering the Northern Hemisphere from 1950 to 2022". This seems contradictory and unsatisfactory.

Reply: We thank the reviewer for highlighting this ambiguity. The first sentence of the abstract has been revised to clearly state that the evaluation in this study focuses on snow depth and snow cover, rather than all variables available in the Crocus-ERA5 dataset. The revised sentence now reads: "This article provides an overview of the daily Crocus-ERA5 snow product covering the Northern Hemisphere from 1950 to 2022. It assesses the product's performance in terms of snow depth and cover compared to in situ observations and satellite data." This wording, we believe, resolves the perceived contradiction and aligns the abstract with the stated scope of the study.

Reviewer 2

General Comment — The authors have successfully addressed my previous comments, leading to a substantial improvement in the manuscript's quality.

However, there are several sections, particularly those providing background information, that lack sufficient citations.

I will highlight specific portions of the text that need attention below, but I recommend the authors thoroughly review the manuscript to ensure all statements are properly cited.

Reply: Thank you for recognizing the improvements made to the manuscript. In response to your comments, we thoroughly reviewed the background and contextual sections to ensure that all relevant statements are now supported by appropriate citations.

We also carefully revised the figures to enhance clarity and align with the feedback provided by Reviewer #1.

Reviewer Comment 2.1 — L24–54: Please add the necessary citations, as this section currently contains none.

Reply: We appreciate the reviewer pointing out the lack of references in this section. We have now revised the text to include appropriate citations, adding both recent studies and key references that provide specific evidence and general background knowledge. These updates ensure that the discussion is properly supported by the relevant literature.

Reviewer Comment 2.2 — L83–105: This section also lacks citations; please address this in the same manner as point #1.

Reply: We have revised the text accordingly. As in the Introduction, this section has been updated to include appropriate citations, drawing on both recent studies and foundational references to support the discussion.

In addition, a paragraph concerning SWE has been added in response to Reviewer #1 and has likewise been complemented with the relevant references.

Reviewer Comment 2.3 — L192: State and define which statistics will be calculated (e.g., R and RMSE).

Reply: We have clarified this point in the revised manuscript. The following statistics are now explicitly stated and defined: correlation coefficient (R, strength of the linear relationship model-obs), bias (model minus observations), centered Root Mean Square Error (CRMSE, deviations after removing the mean bias) and, sample size (N, number of paired values used in the comparison).

Reviewer Comment 2.4 — L449: Since the acronym SWE has already been defined, remove the redundant definition here and simply emphasize its importance to Arctic amplification.

Reply: The redundant definition of SWE has been removed, and the text now simply emphasizes its importance for Arctic amplification.

Editor's comments

Whilst one reviewer considers the manuscript to be greatly improved, the other maintains that several issues raised in their original review remain to be sufficiently addressed. These are largely related to the visualisation of the data.

In addition, both reviewers point out that references of existing literature in the introduction / background section require expanding.

I would therefore be grateful if you could revise the manuscript according to the feedback provided by the reviewers, focusing on these two aspects.

Reply: We thank the editor for summarizing the reviewers' feedback and for highlighting the key aspects requiring further revision. In response, we have carefully revised the manuscript with particular attention to:

- Data visualization – Figures have been updated and refined to improve clarity and accessibility, including the adoption of colorblind-friendly palettes, clearer labeling, and more explicit highlighting of under- and overestimates. These changes address the concerns raised by Reviewer 2 and enhance the overall readability of the results.
- Expanded references in the Introduction and background – We have substantially enriched the literature review, incorporating both recent studies and foundational works to strengthen the context and support of our findings.

We believe these revisions directly address the concerns raised and contribute to a clearer and more robust manuscript.

Summary of manuscript changes

- Added citations throughout the text, particularly in the Introduction and Section 2.
- Revised Figures 1 and 7 to illustrate differences between monthly anomalies.
- Updated Figure 2 for black-and-white readability.
- Modified Figure 4 to include a difference plot (bias).
- Revised Figure 9 to display snow cover fraction biases using a diverging scale.
- Adapted all figures to colorblind-friendly palettes for improved accessibility.

Overall, these revisions strengthen the consistency between the figures and the narrative, and improve both clarity and accessibility of the manuscript.