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.

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

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

L133: Since the early 2010s not "2010"

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).

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.

IMS has known uncertainties and this should be addressed.

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.

L244: "Negative" rather than "Negatives"

Figure 1: In my original comment I had requested anomaly difference plots, rather than only a presentation of the anomalies. This point remains important to visualize the differences between ERA5 and ERAI. Showing the anomalies, as currently done, is valuable for the reason the authors' noted in their original response, but visualizing differences 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.

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.

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 different sizes. I maintain that showing the snow cover fraction here would be meaningful and would be a valuable supplementary figure.

The caption notes “Filled” circles, but the circles are empty.

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.

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).

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.

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

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.