Review of "PDO-driven interdecadal variability of snowfall over the Karakoram and Western Himalaya" by Bharati, Deb and Hunt
Submitted to WCD

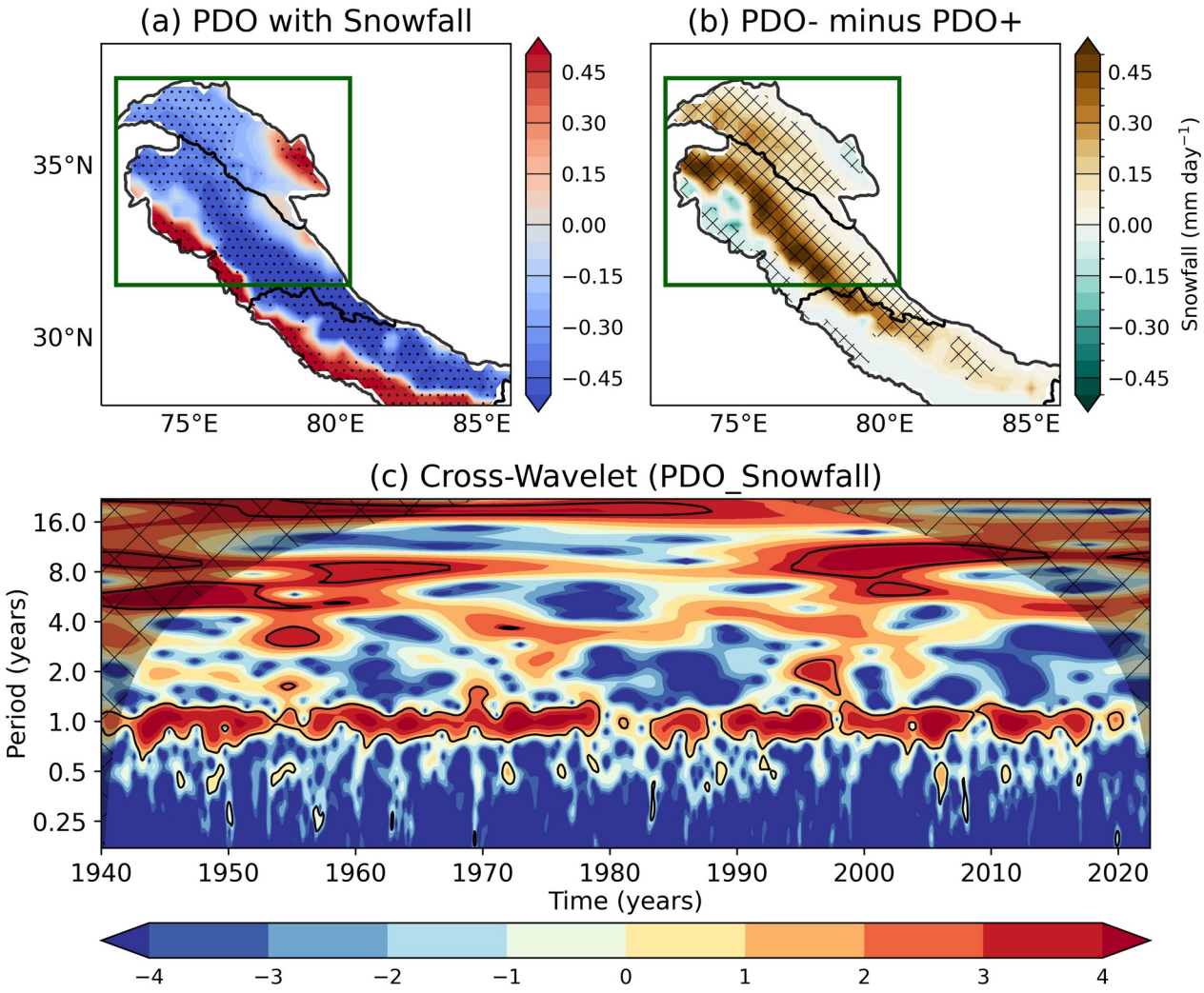
Comments:

1) As noted, it is hard to get good long-term observations of snowfall, but figure 1 shows that the reanalysis datasets consistently overpredict precipitation compared to the satellite datasets (though which is "correct" is not completely clear). In terms of variability then all the datasets (with the possible exception of CMAP) are reasonably well correlated with ERA5 (although this is DJF precipitation averaged over quite a large box so not the toughest of tests). Maybe this correlation is enough if you are looking at how snowfall variability is modulated with the PDO, but it would be good to explicitly discuss this.

Reply: We agree with the challenges associated with long-term snowfall monitoring in this complex topographical area. The reanalysis datasets overestimate precipitation relative to satellite datasets and some rain-gauge datasets; yet, the seasonal variability of precipitation exhibits comparable variability among all datasets in relation to ERA5. Also as the reviewer points out, the correlation coefficients are very high between the reanalysis and most satellite/gauge-based products. The primary reason for choosing ERA5 is its longevity – none of the other datasets have long enough observation periods for a decadal study. Also, although ERA5 precipitation exhibits biases in its quantity throughout this region, it has been used in other studies for analysing seasonal snowfall variability. We have added some text discussing this in our revised manuscript.

2) Why did you choose the green box in figure 2 for averaging over? I can appreciate the simplicity of using a box, but it includes regions where snowfall is negatively correlated with PDO over the mountains, while it is positively correlated with PDO in both the foothills to the SW in the box and in the NE of the box. This potentially complicates the interpretation of later results.

Reply: : Thank you for this useful suggestion. We now use a KH shapefile to select the Karakoram and Western Himalaya regions only. This has improved the strength of correlations presented in the study, and simplified the interpretation of the results. The revised plot in Figure 2 is shown below and discussions have made in the lines from 218 to 226 in revised manuscript.



3) What are the thin black lines on Figure 2 (and other figures). State boundaries? These are helpful in terms of comparing figures, so it would be good to explain and refer to them in the text.

Reply: No, these lines demarcate the traditional boundaries of the Karakoram, Western, and Central Himalaya. We now mentioned this in the revised caption of Figure 2.

4) Is the record of 1940-2022 long enough? The record only contains two periods of negative PDO and one short (11 year) period of positive PDO. Comparing PDO+ and PDO- periods is therefore not very robust. I am not sure what else you can do given data availability (perhaps 20th century reanalysis project?), but at the very least this limitation needs discussing.

Reply: We agree with the reviewer. The lack of availability of long-term observations and reanalysis products of precipitation in the KH region complicates the understanding of low-

frequency precipitation variability in this area. However, the ERA5 dataset is accessible for over 80 years, which can be utilized to illustrate the long-term variability in precipitation within the region influenced by decadal teleconnections. We accept that the occurrences of PDO phase changes are limited in number; however, the duration of these periods is sufficiently extensive to demonstrate the impact of PDO phase changes on precipitation. We also use correlation-based analysis to strengthen our arguments. We have revised the manuscript to discuss this limitation and note that the same type of analysis can be done in future work with the help of longer reanalyses or global modelling experiments.

5) In general, there is some blurring between results and discussion (e.g. lines 234-236 feel more like discussion not results).

Reply: We have moved these sentences to the discussion in our revised version.

6) Figure 2b – what is shown here? If this is a difference in snowfall, then is should presumably be a depth with units of length? Please clarify and include the proper units.

Reply: The unit (mm day^{-1}) has been added in the plot and its caption in figure2b.

7) The interpretation of figure 2c (lines 248-249) talks about the interdecadal variability of KH snowfall depending on the PDO phase. I am not sure you can say that from the results. The power in the 6-15 year band is only significant in the PDO- phase, and so what the results suggest is that in the PDO- phase PDO and KH snowfall are related on a decadal time scale, but again this is only over a 30 / 16 year time frame for the 1st and 2nd PDO- periods and so it is not clear that this is significant.

Reply: The correlation between PDO phase shifts and winter snowfall in the KH has been updated in the revised manuscript following the reviewer's suggestions of the new bounding box (see the comment 2 and plot) This leads to larger areas of significance in the wavelet analysis. The significant power in the 6-15-year range occurred between 1940 and 1970 and again from 1998 to 2015, coinciding with the negative phases of the PDO. An insignificant weak power appeared within the same range from 1971 to 1988, coinciding with the positive phase of the PDO. A long band of strong power exists throughout the 16–20-year range, observed from 1950 to 1990, while a weaker power is shown from 2000 to 2022. This indicates that the low-frequency variability of KH snowfall is influenced by decadal oscillations over various time scales, while the interdecadal variability of KH snowfall is found to influenced by the phase of the PDO.

8) Figure 6b. The additional moisture convergence is relatively modest over the region where there is significant additional snowfall (figure 1b), so how can you be confident this is driving the additional snowfall? Much larger convergence is seen elsewhere.

Reply: Moisture convergence is 16% greater during the negative phase of the PDO compared to the positive phase. This fractional change is statistically significant and leads a fractional increase in snowfall over the KH of similar magnitude. Absolute values of moisture convergence are greater in other areas due to greater atmospheric moisture content.

9) It would be good to mention the uncertainties of the research in the discussion and conclusion, for example the uncertainties in the snowfall datasets and the challenges of a relatively short timeseries for studying decadal oscillations. If models capture the coupling between PDO and snowfall then potentially large ensembles or longer climate simulations could be used to confirm these conclusions? I'm not suggesting you need to do this, but it would be worth mentioning as future work.

Reply: This is a highly beneficial suggestion for the paper. The proposed uncertainties and gaps are indicated as highlighted text in the revised version in lines from 408 to 417.