Second Review for the paper "Snow depth sensitivity to mean temperature, precipitation, and elevation in the Austrian and Swiss Alps" by Switanek et al

While I appreciate the authors' effort to address the issues raised in the previous review, I found some answers unsatisfactory. The following comments still require, in my opinion, the authors' attention.

Major

1. The authors state: "We have changed "forecast" to "predict" throughout the paper. We have also added the paragraph between L278-288. This discusses how we are making the predictions in our paper, but how we envision forecasts can be made using a range of future projections of temperature and precipitation."

   I find the explanation in L278-288 cumbersome and should be simplified. As pointed out in the first review, the authors provide estimates of snow depth anomalies based on anomalies of contemporaneous temperature and precipitation. This makes SnowSens a statistical model or emulator, but not a prediction tool. As per the AMS Glossary of Meteorology: `https://glossarytest.ametsoc.net/wiki/Predictability`, predictability is "the extent to which future states of a system may be predicted based on knowledge of current and past states of the system". I do not deny that the emulator could be driven by future meteorology (obtained from, say, a climate model) to estimate future snow depth, but this is, in my opinion, secondary to this paper and doesn't make SnowSens a forecasting or prediction tool. While the authors should point out those potential applications in the paper, I recommend, for accuracy and clarity, that the authors frame their work as the development of a statistical tool to *estimate* snow depth from meteorological and elevation data.

2. Regarding the authors reply to my previous comment: "The statistical model seems to work best at larger scales (...), but it may fail at representing e.g., interannual variability at smaller scales, where processes such as orographic precipitation as well as blowing and sublimation of snow can greatly affect the snowpack."

   The authors state: L343-344 "... we want to be clear that the SnowSens model still exhibits substantial skill for the year-to-year seasonal predictions at the station scale", and then refer to Fig. 9 to support their claim showing "all seasonal SnowSens predictions versus observations over the validation period using all of the stations in the study domain" pooled in a scatter plot. I contend that Fig. 9 does not support the claim, as it doesn't look at the temporal skill of individual stations, but all times and stations combined. For instance, what would be the result of showing the anomaly correlation coefficient of the (perhaps detrended) snow depth estimates over the validation period at each station individually? I do not suggest to do this, since the emphasis of the paper is on larger scales, but the authors should avoid such statements and, at the very least, recognize in the paper the challenges and limitations of using their tool at station/regional scales, where blowing and sublimation

of snow can greatly affect the snowpack, and temperature and precipitation alone may not suffice as predictors (e.g., Sexstone et al 2018 https://doi.org/10.1002/2017WR021172).

3. Regarding the question about extrapolation of T, P, and SD in (old) Fig. 5e,f,g,h to create the maps in (old) Fig. 5i,j,k,l beyond the range of observed values. In their response, the authors refer to L353-363 and Fig. 10 in the new MS. I am not convinced that this figure shows the "effectiveness of the SnowSens model in its ability to predict in new climatological terrain". Here again the authors pool several stations (and years?) in a scatter plot to support their claim. The authors state L362-363 "while we are extrapolating to "unknown" climatological terrain, we find the model is quite capable of performing well in that new terrain." I'm not convinced that the dispersed cloud of points in Fig. 10 justifies such a strong claim. I think the authors should, at the very least, mention that extrapolation from the trained ranges should be taken with caution.

Minor

1. What "annual climatic cycles"? And, consider deleting "annual" as the statement applies to any time scale, e.g., consider instead "snow depth is an important component of the hydrological cycle and the climate".

2. L158: "precipitaion → precipitation". This was mentioned in the previous report (old L134) and wasn't corrected.

3. L208-L218 Define the symbols $T_{x,t}$, $P_{x,t}$, and $HS_{x,t}$. This was pointed out in the previous report (old 165-174) and it wasn't made clear in the MS.

4. Caption to Figure 6 "anomlies → anomalies". Also in L219. Please check text throughout.

5. For the answer to item #22 in the previous review referring to comparisons over periods of different lengths. The authors state: "We chose to use a longer prior period (the 40-year period) in order to increase the robustness of the measured changes...". What robustness? For consistency, it is preferable to compare the statistics over two periods of equal length, particularly when the data have an underlying trend and no statistical significance is provided. If the periods need to be different, please justify.

6. L42-45 The authors state "There have been several prior studies that have linked changes in snow depth, at different elevations, across the Alps to changes in air temperature and precipitation", and then provide several references. Although the references are pertinent, note that they are not all specific to the Alps.