Reviewer comments are in **black**.

Initial author responses during the discussion period are in **blue**. Line numbers correspond to original paper submission.

Additional author responses are in **green**. Line numbers correspond to the revised version of the paper.

Reviewer 1:

Switanek et al. provide an analysis of the multivariate dependence of relative snow depth anomalies over the Austrian and Swiss Alps to temperature and precipitation anomalies. Besides showing past trends of relative snow depth trends, they use the estimated sensitivities to predict snow depth and compare it to a degree-day snow model. The multivariate approach is interesting and has a lot of potential for understanding past changes and predicting future changes. However, some major reservations need to be addressed or discussed first. Finally, it is unclear what the paper is mainly about. I tended to follow what was written in the title. But there are also other elements within that need to be linked to the research aims (a lot of trend analysis of relative changes and comparison to a degree-day model).

The paper's structure is somewhat unfamiliar, because it does not follow the standard approach of intro, methods, results, discussion, but instead guides the reader through a research journey with a lot of motivation used, e.g., in the methods description. Personally, I enjoyed reading it. But, a major drawback is that methods are sometimes difficult to find, since they are spread out. Furthermore important elements are missing, the research questions/aims and the discussion. I honestly don't know, if I should recommend a standard paper structure or not, but definitely the missing components need to be added.

The authors would like to thank the reviewer for his time and effort in providing useful feedback concerning our paper. The reviewer has made a comment about the structure of our paper. We would like to be clear that our paper does contain the standard sections mentioned by the reviewer (i.e., intro, methods, results, discussion/conclusion). The last paragraph in our Introduction outlines the primary focus of the paper. The reviewer has questioned: What is the paper mainly about? Our main goal of the paper is to use observational records to show the sensitivity of snow depth to temperature and precipitation anomalies at different elevations. And furthermore, we show that these empirical-statistical relationships are quite robust over longer periods of time, and as a result we can use historical sensitivities to make surprisingly skillful forecasts of "future" snow depth. One could use a physically-based model to investigate these sensitivities, but they might not align with the observational records themselves. Therefore, we use the observational data itself to inform us and produce a data-driven model to better quantify these sensitivities. While that is the main focus of the paper, we do also provide some additional trend analysis in order to provide the specific context for the data we used in our study. The second additional component of the paper is the comparison of the forecasts from our proposed methodology to an existing model, SNOWGRID-CL. The authors find these additions to be strengths, rather than a distraction, from the paper. However, if it is seen as beneficial to the paper to remove anything relating to the observed historical trends, we could proceed in that direction.

In an effort to improve the structure of the paper, we have removed the content related to trend analysis from the Results section of the paper. This has been moved to section 3.1.1 along with the addition of Appendix A.

Major points

1. I would expect temperature and precipitation to have different effects in the accumulation and ablation phases of the snow cover. But in your model, using seasonal averages, accumulation and ablation are treated together. Did you perform tests for differences in sensitivities between start and end of the snow season?

   This is a good observation of the reviewer. While the authors agree that greater model complexity has the potential to further improve forecasts, that is precisely what we are trying to avoid in this paper. The main objective of the paper is to show how effective a simplified data-driven, empirical-statistical model performs in making forecasts of long-term changes to snow depth. We consider some of these simplifications (e.g., seasonal averages of our predictors, or using a type of localized linear regression model) to be a strength. This allows researchers and other end-users to very easily visualize how different combinations of changes in precipitation and temperature would be projected to translate into changes in snow depth. As we state at L369: “The SnowSens model is not to be seen as a replacement for physically-based models such as the SNOWGRID-CL.” We show how large simplifications can still provide very useful and skillful forecasts, most especially concerning long-term trends averaged over elevation bands.

2. One major drawback of your method is the strong need for extrapolation of the sensitivities in “unknown” climatological terrain. In my opinion, the

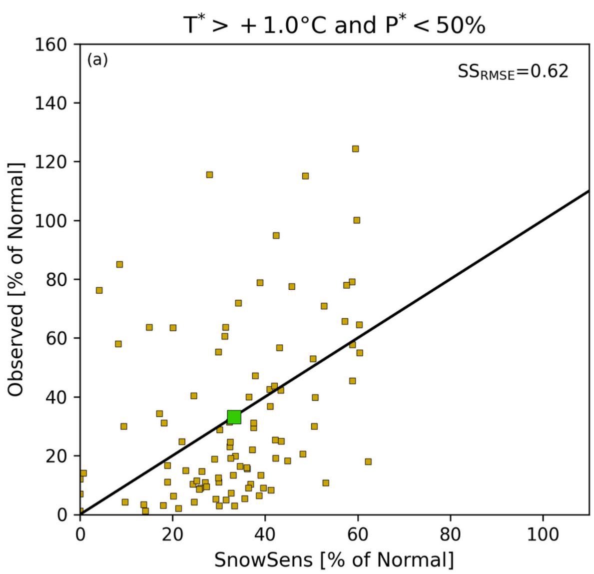
chosen approach using local linear regression produces unrealistic values, especially at the boundaries and beyond the training domain (Fig 5a-d). Moreover, it smoothes out a lot of local effects (Fig 5 comparing the different columns); this might be a reason why SnowSens does not capture interannual variability. I don’t know a simple remedy to this, but at least this needs to be discussed.

Thank you for this comment, and the authors appreciate your skepticism. It is true that we use extrapolation in our methodology. To be clear, the sensitivities, shown in Figure 5, are constructed only using data over the calibration period 1902-1971. This same period is also used to calibrate our SnowSens model. Then, forecasts of snow depth are evaluated over the validation period 1972-2021. Therefore, if the model was systematically producing unrealistic values, then that would adversely affect our skill measures. We do not find this to be the case. As stated in the paper, we find the trends of the band-averaged forecasts to track very well with observations over the 1972-2021 validation period (see Figure 11).

And yes, we have already pointed out (L341) that the SnowSens model does underestimate the observed interannual variability for any given individual station. Perhaps the authors can do a better job stressing in our revised version of the paper the most appropriate application of our proposed methodology. In our revision, we would more strongly recommend that a user of our methodology should not place too much weight on the forecasts for any one station or any one point location, but rather should focus more on band-averaged forecasts. For the paper, we wanted to be transparent about how the skill of the SnowSens model compares to something like the SNOWGRID-CL model. Therefore, we initially show the interannual skill at the station level.

Here would be a good place to discuss the extrapolation that we use in our model. Later, the reviewer has this comment when discussing L210: “Personally, I would not trust the values far beyond (>1degC, 50% prec) what one sees in Fig 5e-h.” In Figure 1, seen below in this response to the reviewer, we have plotted the cases which fulfilled these criteria. Figure 1a shows the 95 cases where the average seasonal temperature in the validation period was greater than 1.0degC and less than 50% of normal precipitation. One can see that there is not perfect agreement between the individual forecasts and observations. That would be true for any snow model. Though, the error of the SnowSens forecasts are less than half of the climatological forecasts (indicated by RMSE_SS>0.50). The average of the forecasts and observations over these cases are the same; they are both 33% of normal. Figure 1b increases the sample size by using a threshold of less than 75% of normal precipitation. This gives us 988 cases. Again, the average forecast error is less than half of climatological forecasts. The average of the forecasts and observations over these cases are 42% and 41%, respectively. So, while we are extrapolating to "unknown" climatological terrain, we find the model is quite capable of performing well in that new terrain, especially when aggregating over a number of cases.

We have added text, reflecting the above content, in the revised version of the paper between L353-363. Also, a new Figure 10 has been added to the paper.



Paper Figure 10: Figure 10a shows the 95 cases where the average seasonal temperature in the 1972-2021 validation period was greater than 1.0degC and less than 50% of normal precipitation. Figure 10b increases the sample size by using a threshold of less than 75% of normal precipitation. This gives us 988 cases. The larger squares are the forecasted and observed averages over these cases. The skill scores, for these two different criteria, are shown in the top right of the subplots.

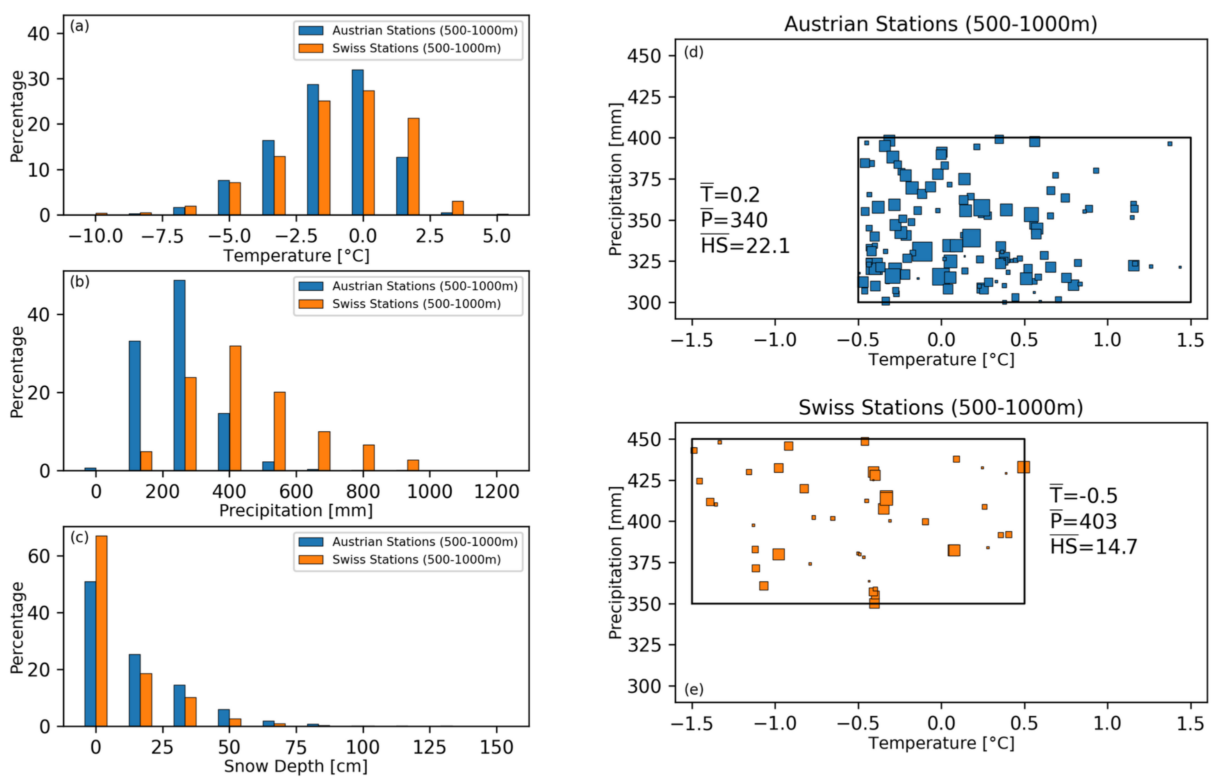
3. I understand the choice of elevation bands, but in a changing climate context, I could also imagine a lot of potential for statistical methods to learn across elevation, at least what concerns temperature, given its strong dependence with elevation. However, this probably requires going away from anomalies to absolute temperature and snow depth values. Did you test the multivariate dependency also for "raw", ie., absolute values of

temp, precip and HS? Would it work? Also without subdividing by elevation?

We would like to thank the reviewer for bringing up this point. In the Conclusions, we state at L374: “If these sensitivities continue to remain persistent into the future, then this modeling approach can be expected to yield skillful forecasts for the next 50 years.” We only used data from 1902-1971 to forecast snow depths for the period 1972-2021. These forecasts were shown to be skillful. As a result, one can logically conclude that the sensitivities over the last 120 years have been reasonably stationary. Given this information, we then propose that these methods could produce skillful forecasts over the next 50 years. In contrast, we are not proposing that the historical sensitivities be applied for the next 200, or 500, or 1000 years. A user should periodically update the sensitivities, in addition to testing their effectiveness in a cross-validated framework, prior to making another long-range forecast. For example, people in the year 2050 should not solely rely on data from 1902-1971 or 1902-2021 to say something about the future of snow depths. They can, and should, also incorporate data over the more recent period 2022-2049.

The reviewer asked about if “raw” or absolute values can effectively be used. In our study, we found that constructing the SnowSens model using absolute values across either, 1) elevation bands or, 2) all of the stations, produced forecasts that performed substantially worse than the normalized version of the model (the forecasts from the absolute model also performed worse than climatology). We can show why normalization is a critically important step when using our proposed methodological approach. First, take a look at Figures 2a-2c here in this response to the reviewer. The bar plots show the distribution of values for absolute temperature, precipitation, and snow depth for the Austrian and Swiss stations between 500-1000 meters. The average station height of the Austrian stations used is 745m, while it is 742m for the Swiss stations. So, they are not much different in elevation between the two regions. One can observe that the Swiss stations are generally warmer and wetter than their Austrian counterparts. At the same time, the Swiss stations have lower seasonal averages of snow depth. Let’s take a further subset of these Austrian and Swiss data points over this 500-1000m elevation band. Those observed data points of the subsets of data are shown as the scatter plots in Figures 2d-2e. A Student’s t-test shows that the means (for temperature, precipitation, and snow depth) of the subset of Austrian data points (Figure 2d) are all statistically significantly different than the subset of Swiss data points (Figure 2e). Looking closely, we find that while this subset of historical observations in Austria has a greater absolute temperature and less absolute precipitation than the Swiss subset, the Austrian stations have significantly more absolute snow depth than the Swiss stations. As we decrease temperature and increase precipitation, we should expect snow depth to increase. However, this is exactly the opposite of what the absolute data is telling us. By simply using the absolute data alone here, we get the wrong signal. This is an example of a regional or spatial climatological difference that we can address through normalization. After normalizing the data, we are able to leverage information across a larger region.

We have added text, reflecting the above content, in the revised version of the paper between L192-207. This text is accompanied by the addition of a new Figure 5 in the paper.



Paper Figure 5: Figures 5a-5c are bar plots that show the distribution of absolute temperature, precipitation, and snow depth for the Austrian and Swiss stations between 500-1000 meters over the historical period 1902-1971. The average station height of the Austrian stations is 745m, while it is 742m for the Swiss stations. The percentages of the blue and orange bars in each subplot (5a-5c) will sum to 100%. The bar plots are comprised of 1,755 observed data points for Austria and 558 data points for Switzerland. A subset of these Austrian and

Swiss data points are shown as the scatter plots in Figures 5d and 5e, respectively. The size of the squares reflect the values of absolute snow depth. So the larger the snow depth, the larger the square. The subset of Austrian data points have a greater absolute mean temperature, less absolute mean precipitation, and greater mean absolute snow depth.

Minor points:

- L1: What climatic cycles do you? Maybe better rephrase, since climatic cycle can mean something like the Milankovitch cycles.

  Yes, we can make this clearer. Though, Milankovitch cycles operate between tens of thousands and hundreds of thousands of years. Since our paper focuses on a time horizon on the order of ~50 years, it would be unlikely that a reader would be confusing these two.

  We have added "annual" into our abstract at L1.

- L40: Might be worth mentioning doi:10.1002/joc.8002 who also attempted something similar for snowfall

  We can do that.

  This is added at L40.

- L49-54: this belongs into methods. Please provide here a more conceptual statement how you go beyond the state-of-the-art and what your research questions, aims, or hypotheses (choose one) are.

  We respectfully disagree with the reviewer here. It is standard practice to provide a brief outline of what is done in the Introduction of the paper. We do provide one research aim. See our initial response above. The trend analysis provides relevant context for our work, while the forecast comparison to an existing model provides a necessary level of legitimacy of our newly proposed methodology.

  As we said above, we have moved the trend analysis out of the Results section in an effort to improve the structure of the paper.

- L99: so y_clim,i should not be a time series but a fixed value for every station, right? Maybe state it explicitly. Also your RMSEs are then the average over all stations?

  y_clim at station, i, can be thought of as a single value or an time series array where all values are the same. We will make this more clear. And yes, as the equations 1 and 2 indicate, the averages are performed over all of the stations.

We attempt to make this clearer at L101.

- Sec 3 is more than just methods, it contains a lot of background information and motivation

  See above. It is primarily trend analysis that is the additional component of the paper. We found this to be a valuable addition to provide the necessary context with the specific data that we are using. However, if it would improve the paper to remove this content, we can consider doing this.

  See above.

- L126: Not sure I agree that Nov-Mar performance should equal to Nov-May. See also Major point 1.

  We respectfully disagree with the reviewer here. One can easily compute Nov-Mar and Nov-May seasonal averages of snow depth. We have done that, and their similarity can be observed in Figure 2b. This does not mean that the April-May average cannot also have its own variability, it is just that the April-May contributions to Nov-May average snow depth are obscured, to a large extent, by the larger contributions from Nov-Mar. Also, keep in mind that we are showing and comparing the similarity of the normalized quantities, and not their absolute quantities. The normalized quantities Nov-Mar (normalized with respect to Nov-Mar, station-by-station) are very strongly related to Nov-May (normalized with respect to Nov-May, station-by-station). Put another way, when the Nov-Mar average snow depth, for a particular station, was about 20% above average (or 120% of normal), then we can expect that the Nov-May will also be quite close to 20% above average. We compute a mean absolute error between the two normalized seasonal averages of 3.0%. So, on average, a Nov-May percentage anomaly will vary about 3.0% above or below a Nov-Mar percentage anomaly. The Nov-Mar anomalies explain 99% of the variance of the Nov-May anomalies.

  We changed the sentence starting at L128 to be clear that we are referring to anomalous values of the seasonal averages. In that case, as stated in the paragraph above, the anomalous quantities from the two seasons are extremely close to one another.

- Sec 3.2. is unclear. Please describe better how you performed the interpolation. Eg, “function of the inverse distance”? “adjusted to match”? Also not clear if your interpolation takes into account the effect of elevation? The five nearest stations might not be equally representative in that regard.

We say at L142 that we use inverse distance weighting. It is true that elevation can influence the absolute values of these meteorological quantities. However, since we use normalized temperature and precipitation anomalies, it doesn't particularly matter to us or our model what the absolute values of these predictors are. That said, if one were to produce and use "better" predictor data along with our methodology, this should only improve our model performance.

We have tried to improve that section. We have changed some of the text between L163-171.

- Related: Why did you not use LAPrec or the gridded HISTALP to extract this information? They use homogenized input, but at least for LAPrec, the spatialization is much more complex and takes topography well into account.

  We made a choice of the data to use for our study and to construct our sensitivity maps. While beyond the scope of our paper, it could be useful for a future study to compare the influence that different data sets have on the results.

- L150ff: Seems like research questions to me, not methods.

  We are providing local context, in this section, for the methods that are being presented.

- L156: which correlation coefficient (Pearson, Spearman)?

  Good point. We use Pearson correlation. We will make that clearer in the revised version of the paper.

  We now state in the paper that Pearson correlation coefficients are used.

- Fig 4: Please do not use rainbow scales, since the changing colors introduce artificial visual breaks. Use a continuous scale such as viridis, scico (https://www.fabiocrameri.ch/colourmaps/), or similar. Moreover, figure looks quite overplotted, maybe it could help to sub-divide by elevation bins? Ok, I see this comes as Fig5. So maybe in Fig4 you could focus on a few single stations instead or omit?

  Thank you for the good suggestions. We can think about how to improve the visibility of these figures.

  We have changed the colormaps of Figures 6 and 7. For Figure 6, we still want to show all of the values in the record. Then, in Figure 7, we break this up by the different elevation bands.

- L209: how did you define “nearest quartile” in 2d?

  Thank you for pointing this out. We will have to make it clearer what we have done there. We use a Euclidean distance measure which essentially equates the distances of a 10% precipitation anomaly with a 0.2decC temperature anomaly. So, a data point that had the coordinates of (0.4decC warmer, 0% of normal precip) with respect to a point of interest, and another data point with coordinates (0.0decC, 20% of normal precip), would be treated as the same distance. We did not find the model to be overly sensitive to providing more or less weight to the temperature or precipitation axes.

  The text between L256-260 have been added to better describe how we define the nearest quartile.

- L210: Why did you not use the actual values for your localized linear regression instead of the bins? In that way, you can maximize the information better, and also include information beyond empty bins (< 50 values). Moreover, in statistics, extrapolating beyond the range of training data is controversial. Personally, I would not trust the values far beyond (>1degC, 50% prec) what one sees in Fig 5e-h. Finally, since you want to get 2d-surfaces, GAMs (generalized additive models) seem like a prime tool to be used (with a 2d tensor product smooth); it would not require to bin your data, and would also work in 3d with elevation as third predictor.

  See above our answer to major point 2. While the forecasts are not perfect, the authors find that the model performs quite well in the climatological region that you propose. With respect to GAMs and a tensor product: As we have said above, our current aim is to show how something quite simple can still perform quite skillfully. However, as we have also pointed out, increasing model complexity has the potential to further improve on the methods proposed here.

- L242 Please explain, why the bias correction is needed.

  Without bias correction, the SNOWGRID-CL model (which is the one we compare ours against) performs about as well as climatology. This is due to the bias of the SNOWGRID-CL model (see Table 2 in the paper). For example, SNOWGRID-CL might track the interannual variability fairly well for a station, but its forecast average might be twice as large as the observed average. Calculating the error on the uncorrected forecasts will show that the model is not skillful, while the skill of the SNOWGRID-CL model dramatically improves with bias correction.

We had already stated in the initial version of the paper, “Prior to any bias correction, the SNOWGRID-CL predictions perform about as well or worse than climatology.” This can be found in the revised version at L311.

- Sec 4.1. Why this? Not related to the main paper goal, I guess? Also there are some methodological concerns, and missing descriptions: related to data coverage, usage of linear regression for multiple stations (not recommended, because of their correlation, better to a regional/elevation series first), why the arbitrary split in two periods given the know non-linearity of change (papers by Marty and co.).

  At L251, we discuss a couple of caveats to the calculation of the trends. The authors disagree that the beginning and middle of last century are two completely arbitrary starting points.

- L307: What test did you use to assess this significance of skill?

  We used bootstrapping to test for statistical significance. We will be sure to add that into the revised version of the paper.

  We have added this. See L322 and L346.

- Fig10 a) and b) scales do not match but should? a) has -0.4 to 0.4 and b) has -0.2 to 0.6

  There is one station that was cut off from Figure 10b that corresponds to the red station in Figure 10a. We did this simply to improve the visibility of Figure 10b.

  We changed the look of this figure (now Figure 11) to show all of the data points.

- L341: Does this also hold for the single series? Would be interesting to see some single stations time series and not only regional averages.

  We need to make more clear where and when our proposed model is most appropriate. For transparency, we compare the year-to-year forecasts, at the station level, to those of SNOWGRID-CL for the Austrian region. However, we propose a user exercise caution in interpreting the forecasts of any one station or point. See above. Rather, we recommend interpreting the results over band-averages.

  Yes, there is skill for nearly all of the individual stations, which can be seen in Figures 11a and 11b. You can see all of the modeled versus observed anomalous seasonal snow depths in the validation period in the new Figure 9. A scatter plot allows us to plot all of the modeled versus observed values, instead of just one or two time series from a station or two.

- L350: Very interesting application of your method. However, 3.2degC is beyond your training range for that elevation range, so the accuracy is highly questionable. Especially, since your numbers are very different compared to previous studies (a comparison with existing literature would be very useful, there are a lot of studies using regional climate models, or snow models forced with climate models).

  If you look closely at Figure 5i, it is around temperatures above 3.5degC and below normal precipitation that the model predicts zero precipitation for the elevation band 0-500 meters. While it is true that these criteria are beyond the training range of the data, we find that the model performs quite well in these cases in the validation period. There are 32 instances that fulfill these criteria in the period 1972-2021. As indicated by Figure 5i, the predicted values for these 32 cases is always 0% of normal. The observed values for these 32 cases range between 0%-25% of normal, with a mean of 8% of normal. This translates to an RMSE_SS is equal to 0.89, which means that the error associated with the model is nine times less than climatology. So, while a number of the observed values in these cases are not exactly zero, they are quite close to it.

  While we did include the new content regarding new climatological terrain (as stated above), we chose not to include this example in the revised version of the paper.

- Discussion of results missing.

  We need to make more clear where and when our proposed model is most appropriate. Thank you for this point. We will see where we could expand on our discussion.

  We made some changes to the paragraph between L402-414 in an attempt to clarify what we see as the most appropriate application of our proposed model.

Reviewer 2:

Switanek et al examine the dependence of snow depth (SD) on temperature (T), precipitation (P), and elevation (E) in the Austrian and Swiss Alps. By using historical data from weather stations, they build a statistical model (SnowSens) to estimate seasonal SD based on these predictors. The statistical model is trained with data from 1901-1970/71, then evaluated over 1971/72-2021. The model performance is compared with that of the physics-based model SNOWGRID-CL for a subset of weather stations. Finally, the statistical model is used to estimate SD over the entire domain and some conclusions are drawn on future changes of SD at specified elevation bands.

The authors claim that SnowSens is used to “forecast snow depth” (SD), although SD estimates are produced with contemporaneous observed T and P. The model is, as presented, an emulator of SD driven by P and T, and not a forecasting tool. This and other major concerns listed below diminish the significance of this work, and should be clearly addressed before the paper can be considered for publication:

The authors would like to thank the reviewer for their time and effort in providing useful feedback concerning our paper. The reviewer has made a valid point about “forecasting.” The authors will change the terminology used in the paper to reflect how the performance of the model is being evaluated.

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.

Major

1. SnowSens is not a forecasting tool. SnowSens forecasts could be produced if SD lagged T and P, or T and P were themselves forecast, which is not the case in this study. The authors call “forecasts” what seem to be out-of-sample estimates of SD used to validate their model. Therefore, the authors should give a more clear explanation of how their model should be applied. Is this statistical model expected to outperform more advanced state-of-the-art physics-based models? Or, is it more a diagnostic and analysis tool? Perhaps the authors should emphasize applications such as that discussed in L343-355 and Fig. 12, with estimations of future SD based on projected T and P.

Thank you for this comment. The authors agree that we need to do a better job in explaining how our model should be applied. The main objective of the paper is to show how effective a simplified data-driven, empirical-statistical model performs in making “forecasts” of long-term changes to snow depth. This allows researchers and other end-users to very easily visualize how different combinations of changes in precipitation and temperature would be projected to translate into changes in snow depth. As we state at L369: “The SnowSens model is not to be seen as a replacement for physically-based models such as the SNOWGRID-CL.” We show how large simplifications can still provide very useful and skillful forecasts, most especially concerning long-term trends averaged over elevation bands or new climatological terrain (as Reviewer 1 referred to it).

We do agree that the model values used for validation in the paper are not exactly “forecasts.” This is because, as the reviewer has pointed out, we use seasonal temperature (T) and precipitation (P) to say something about contemporaneous or concurrent snow depth (SD) anomalies. However, we are very much proposing that the model and/or the sensitivity plots be used to make actual forecasts of SD given projected future ranges of T and P. We have used a perfect prognosis approach to quantify the uncertainty of one part of the modeling chain as it concerns seasonal snow depth. We have asked the question: Given specific known values of average seasonal temperature and precipitation at the various stations in the region, how accurately can we “forecast” the values of SD? We have answered this in the paper, and our associated skill measures tell us this. As with any model, though, there can be uncertainty that is added anywhere along the modeling chain. If observed future values of T and P differ from what were forecast by CMIP6 for example, then this adds to the uncertainty and will ultimately degrade the quality of the forecasts of any snow model that is used. The whole point of the validation of our model is to give users the confidence to apply it in a true forecasting framework. So, consider an example where we know the average CMIP6 seasonal forecasts of T and P over a period of time such as 2031-2070. Given those conditions, with those specific forecasts of T and P, we can make an actual forecast of SD over that same period of time. We will clarify these points in the revised version of the paper. As a comparison, the SNOWGRID-CL model is also being run with concurrent data. The SNOWGRID-CL model also makes “forecasts” or estimates of SD, for a particular day, given T and P from the same day.

To further illustrate our approach and applicability, consider an illustrative example where some climate model can make forecasts out for the next six months. One might want to observe whether that model is capable of capturing, in a forecast framework, the precipitation patterns associated with ENSO. Initially, the model developers might run the model with reanalysis data in order to observe how well the “known” set of conditions can be used as model input to simulate a “known” set of precipitation observations. If, over many cases, the precipitation patterns do not align very well with ENSO patterns, then perhaps that model should not be trusted in a new and unknown case. On the other hand, if the model is found to perform well, then the model can be useful to provide a set of actual precipitation forecasts which have been conditioned on a forecast of ENSO (e.g., the NINO3.4 region will be 1.5°C above average over the next 3 months). Our approach is similar. We show, over a set of "known" cases, that the model is quite capable of quantifying how different T and P anomalies translate to anomalies of SD. Now, with a skillful model one can make actual forecasts of SD given different a range of future values of T and P. We will make this clearer in the revised version of the paper.

See our comment in green above.

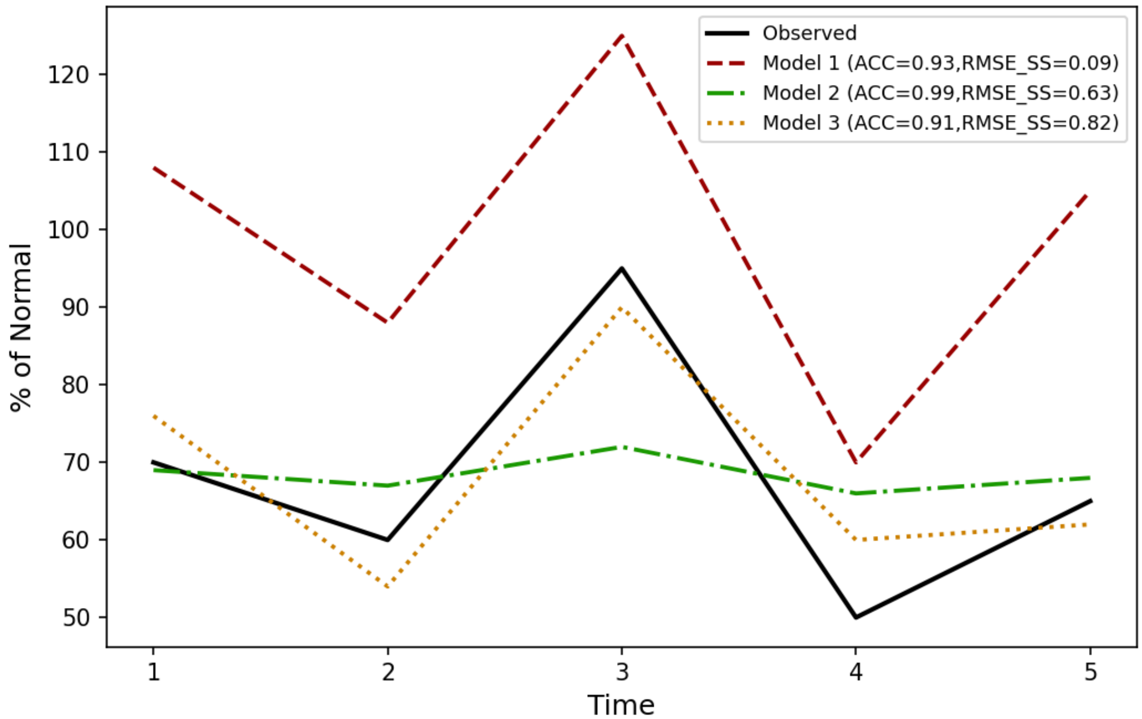
2. The statistical model seems to work best at larger scales (e.g., averages over elevation bands), but it may fail at representing e.g., interannual variability at smaller scales, where processes such as orographic precipitation as well as blowing and sublimation of snow can greatly affect the snowpack. Can the authors comment on this?

As stated in the paper and above in this response, "The SnowSens model is not to be seen as a replacement for physically-based models such as the SNOWGRID-CL." More advanced state-of-the-art physics-based models have their place, and we are not trying to replace them. While the authors agree that greater model complexity has the potential to further improve forecasts, that is precisely what we are trying to avoid in this paper. We consider some of the simplifications that we use (e.g., seasonal averages of our predictors, or using a type of localized linear regression model) to be a strength. We have already pointed out (L341) that the SnowSens model does underestimate the observed interannual variability for any given individual station. The authors will do a better job stressing in our revised version of the paper the most appropriate application of our proposed methodology. In our revision, we would more strongly recommend that a user of our methodology should not place too much weight on the forecasts for any one station or any one point location, but rather should focus more on band-averaged values. For the paper, we wanted to be transparent about how the skill of the SnowSens model compares to something like the SNOWGRID-CL model. Therefore, we initially show the interannual skill at the station level.

We have tried to be as clear as possible in the revised version of the paper about when and where there is skill in the SnowSens model. Related to a comment of the reviewer below, there is definitely skill in the SnowSens model at the interannual time scale, or when using seasonal values. While the model is clearly skillful at the interannual and station scale, we still find that the bias-corrected SNOWGRID-CL model is more skillful at this scale. This is clearly stated in the paper. New text discussing the interannual (or year-to-year) variability can be found between L342-352, along with the new Figure 9.

3. L322. Related to the previous comment: to provide a comprehensive assessment of the modeled SD “year-to-year variability”, it would be beneficial to include results of the anomaly correlation coefficient (ACC) of estimated and observed SD. Given the results in Fig. 10 and the comment in L307-308, ACC for the estimated SD at weather stations may be low. If so, the authors should clearly and explicitly address this shortcoming of their method. I would be curious to know whether (and how) the authors plan to overcome this.

We thank the reviewer for this comment. As stated above, our methodology is not designed to better capture the year-to-year variability at individual stations in comparison to a more state-of-the-art physics-based model. Given what we have already said about the applicability of our model, we do not advise placing too much weight on RMSE_SS values or anomaly correlation coefficients at individual stations or point locations. We typically avoided using something like ACC (with the exception of Figure 11) because it is not well suited in evaluating skill over trending time series. Additionally, ACC does not show or reflect whether any biases are present within a model with respect to observed time series. In contrast, a skill score such as RMSE_SS evaluates how well modeled values match the observed values (a close match will better minimize the errors between the model and observations), which includes information related to the observed trend, along with whether bias exists in either the mean or the variance. In Figure 1, which can bee seen below in this document, there are a set of 5 synthetic “observed” values which are all below normal (i.e., solid black line). The values of Model 1 (dashed red line) have a high ACC, but have large error residuals from the observations because it is not capturing the systematic mean change, which could result from an underlying trend. Model 2 (dashed-dotted green line) has the highest ACC, but is dramatically underestimating the observed variability. Model 3 (dotted orange line) has the lowest ACC, but the highest RMSE_SS. This tells us that, on average, the squared differences between observations and Model 3 are closer to one another than between observations and Model 1 or Model 2.



Response Figure 1: Four synthetic time series are plotted in order to illustrate how effectively different skill metrics evaluate the performance of a model.

We discuss the year-to-year skill of the SnowSens model both above and below in this document. We find that the SnowSens model is skillful in predicting year-to-year snow depths at the station level. However, we list in the paragraph above (in this document) the benefits to using the RMSE skill score instead of anomaly correlations.

4. Based on Fig. 9, SnowSens tends to underestimate SD more than SnowGrid-CL does, particularly for high SD. This suggests that SnowSens may not work well at estimating high snow accumulations and more generally in cases of extreme snowfalls. Can the authors comment on whether/how their model could/would handle extreme events?

This point is also concerning year-to-year variability. See our comments above to the last two points. The SnowSens model is not making any attempt to forecast extremes, and that is outside the scope of our current study. We are proposing that the SnowSens model be used to make forecasts of expected changes to band-averaged seasonal SD given future projections of T and P. As stated above, we will make this clearer in our revised version of the paper.

Again, see the content from our prior responses to the reviewer.

5. L309-316. Are the values reported in Table 2 for bias corrected SNOWGRID-CL? Please clarify. If not, please provide the bias corrected values as well.

Thank you for this comment. We will make this clearer in the revised version of the paper. The first skill metrics listed for SNOWGRID-CL (which is the first row in Table 2, and it is labeled: SNOWGRID-CL RMSE SS (HS)) are for the absolute or raw modeled values. This tells us that the absolute values of the SNOWGRID-CL model are not particularly skillful, or much closer to observations than climatology. This is due to the fact that the SNOWGRID-CL model contains substantial mean biases. After correcting for these systematic mean biases, the skill can be seen to improve (i.e., this is the third row in Table 2, which is labeled: SNOWGRID-CL RMSE SS (HS^*)).

The skill of the SNOWGRID-CL model, using both the absolute values and the bias-corrected values, are included in the paper and in Table 2.

6. How sensitive is the statistical model to the bin size discussed in L194-L204. Is it robust to changing bin sizes?

While the performance of the SnowSens model can change as a function of bin sizes, we do not observe the performance of the model to be overly sensitive to what we consider reasonable choices of bin sizes, given the ranges of the data and the sample sizes. When using the bin sizes from the paper (window with sizes, 0.8degC T and 40% P), the RMSE_SS across all modeled versus observed values is 0.26 (row 4 in Table 2) and RMSE_SS is 0.19 for its ability to capture the SD changes from one period of time to another for all of the stations (row 7 in Table 2). If we use a window size of 1.0degC T and 50% P, then these skill scores are 0.26 and 0.20, respectively. And, if we use a window size of 0.6degC T and 30% P, then these skill scores are 0.26 and 0.17, respectively. While we present a working and skillful model, a user may deem it appropriate in their application to use a different bin size than what we have put forward in the paper.

We added some text regarding the bin size in the paper at L243: “We did experiment using different bin sizes, though we found that the choice of bin size does not strongly affect model performance (please refer to our comments in the paper discussion at https://doi.org/10.5194/egusphere-2024-1172-AC2).”

7. Following on the previous comment, have the authors considered quantifying the uncertainty of their statistical model?

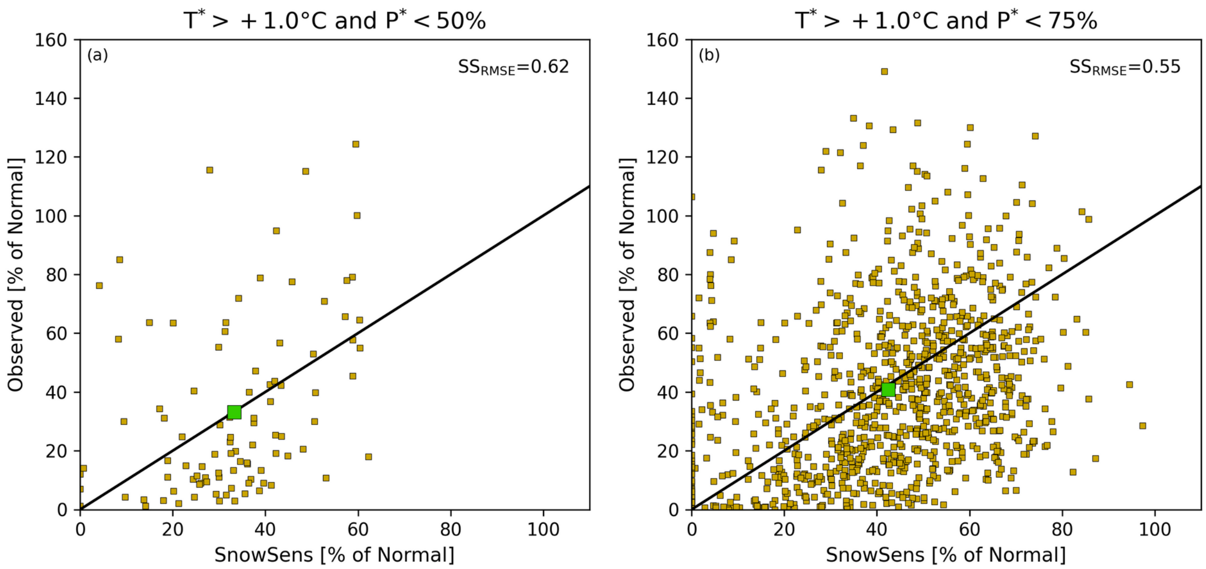
We thank the reviewer for this question. While we appreciate the motivation to quantify all of the uncertainty associated with our model, this is beyond the scope of our study. We have presented a methodology with which we have evaluated the performance of modeled values with respect to observed values. The skill scores that we have listed in the paper do provide a quantification relating to how “certain,” or how much variability, there exists in the observations about the modeled values. However, future work can focus on better quantifying the uncertainty of the model as a function of different elevations, T anomalies, and P anomalies.

8. I may have missed it, but how the authors extrapolate T, P, and SD in Fig. 5e,f,g,h to create the maps in Fig. 5i,j,k,l beyond the range of observed values? For example, in Fig. 5i for the 0-500 m band, how is the map created for temperature anomalies greater than 3° C or precipitation more than twice the normal values? It seems unlikely that the model will perform well out of the observed range.

Here, we include some of our responses to Reviewer 1, who also raised a couple of questions about extrapolating to new values. We are planning to integrate much of the following content into the revised version of the paper.

It is true that we use extrapolation in our model. The reviewer has this comment when discussing L210: “Personally, I would not trust the values far beyond (>1degC, 50% prec) what one sees in Fig 5e-h.” In Figure 2, seen below in this response to the reviewer, we have plotted the cases which fulfilled these criteria. Figure 2a shows the 95 cases where the average seasonal temperature in the validation period was greater than 1.0degC and less than 50% of normal precipitation. One can see that there is not perfect agreement between the individual forecasts and observations. That would be true for any snow model. Though, the error of the SnowSens forecasts are less than half of the climatological forecasts (indicated by RMSE_SS>0.50). The average of the forecasts and observations over these cases are the same; they are both 33% of normal. Figure 2b increases the sample size by using a threshold of less than 75% of normal precipitation. This gives us 988 cases. Again, the average forecast error is less than half of climatological forecasts. The average of the forecasts and observations over these cases are 42% and 41%, respectively. So, while we are extrapolating to “unknown” climatological terrain, we find the model is quite capable of performing well in that new terrain, especially when aggregating over a number of cases.

And here is another example of extrapolating to large temperature anomalies. If you look closely at Figure 5i (in the paper), it is around temperatures above 3.5degC and below normal precipitation that the model predicts zero precipitation for the elevation band 0-500 meters. While it is true that these criteria are beyond the training range of the data, we find that the model performs quite well in these cases in the validation period. There are 32 instances that fulfill these criteria in the period 1972-2021. As indicated by Figure 5i, the predicted values for these 32 cases is always 0% of normal. The observed values for these 32 cases range between 0%-25% of normal, with a mean of 8% of normal. This translates to an RMSE_SS is equal to 0.89, which means that the error associated with the model is nine times less than climatology. So, while a number of the observed values in these cases are not exactly zero, they are quite close to it.



Paper Figure 10: Figure 10a shows the 95 cases where the average seasonal temperature in the 1972-2021 validation period was greater than 1.0degC and less than 50% of normal precipitation. Figure 10b increases the sample size by using a threshold of less than 75% of normal precipitation. This gives us 988 cases. The larger squares are the "forecasted" or simulated and observed averages over these cases. The skill scores, for these two different criteria, are shown in the top right of the subplots.

We have added text, reflecting the above content, in the revised version of the paper between L353-363. A new Figure 10 has also been added to the paper.

9. Table 3 states that the result are statistically significant at $p< 0.05$. What statistical test is used to establish this?

We used bootstrapping to test for statistical significance. We will be sure to add that description into the revised version of the paper.

We have added this. See L322 and L346.

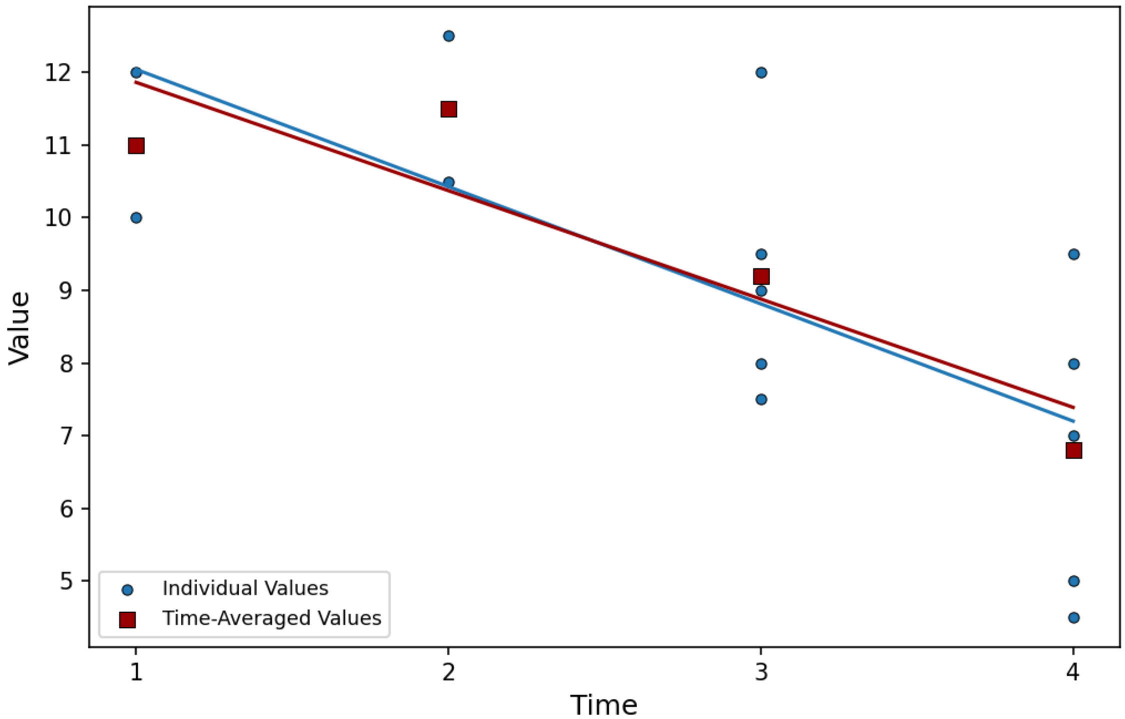
10. L249-259. In describing Figs. 6 and 7, the authors make good points regarding the nature of SD and how those fitted straight lines could be

misleading. Another point is that the sample size may be different each year (e.g., there may have been considerably less stations at the start of the recording period compared to present time as is clearly the case for Fig. 6d, making the trend largely uncertain). Can the authors comment on this and provide a measure of uncertainty associated to these straight lines?

Thank you for this comment. We have already accounted for the fact that each year can have a different number of measurements. This is done by fitting a least-squares regression line to all of the scatter points in Figures 6 and 7 in the paper (independently for each elevation band), instead of a least-squares fit to the regionally-averaged SD. Yes, if a particular year only had five measurements and another year had 100, then getting regional averages would apply equal weighting to the both of these years even though they have dramatically different data coverage. In Figure 3, below in this document, we have provided a simplified example using synthetic data. In this example there are two individual data points for times 1 and 2, while there are five individual data points for times 3 and 4. One can fit a regression line to the individual blue points, and this is shown as the blue line. This is what we have done in the paper. In contrast, one could average the values at each time and then fit the regression to those time-averaged values (shown as the red line, this could be thought of as the average of all of the available stations for a given season). Because we are not using a very large number of points in this example, the blue and red lines are not all that different. We are only illustrating that they are different, and it does matter what you are using to fit your regression.

The reviewers comment on the uncertainty of the trend is a good one. In the revised version of the paper, we will be sure to add confidence intervals to the plotted trend lines.

We have added some additional description and confidence intervals concerning the plotted trends. See the caption for what is now the new Figure 3 in the paper.



Response Figure 2: Synthetic data which has two individual data points (blue circles) for times 1 and 2, and five individual data points (also blue circles) for times 3 and 4. The red squares are averages of the blue circles at each time step. The blue regression line is fitted to the individual blue points, while the red regression line is fitted to the red points.

Minor

1. L1 Delete “incredibly” and “climatic and”

We can remove “incredibly.” However, we see snow depth as something that not only results from a particular climatic regime, but it also influences the climate. Consider the influence of albedo on the energy present on the ground or in the atmosphere, and how that changes as a function of SD.

We have removed “incredibly.”

2. L32-39. altitude → elevation

Thank you. We can be more consistent with the wording.

We now use “elevation” throughout the paper.

3. L47. “However, these studies suffer ... strong dependence of snow depth on elevation”. Please clarify.

We are primarily referring to some prior work conflating the absolute changes in snow depth and the fact that higher elevations climatologically have greater depths of snow to begin with. At L36, we write: “However, this altitudinal dependence of the snow depth trends is conflated to some degree with the fact that stations at higher elevations also typically receive more snow.”

See our response in the paragraph above, and we have changed the wording here and the new content starting at L46 reads: “However, these studies also experience to some extent a conflation between snow depth quantities and elevation. Furthermore, the statistical relationships shown are often correlations, which do not capture how much snow depth would change, for example, as a function of air temperature.”

4. L50. “This allows us to remove the influence of elevation ...”. Please clarify. “Remove” from what? The dependencies established in this study are strongly affected by elevation.

We remove the influence of elevation on the absolute values of SD. Even after removing this influence, there is still an influence of elevation in a relative sense. We will clarify this point in the revised version of the paper.

We have changed our text to now read: “This step removes regional and elevation-dependent climatological differences, thereby allowing us to better quantify anomalous or relative changes.”

5. L68-71. Please clarify what homogenization means in this context and why one or the other choice is not expected to change the results.

Homogenization in this context is referring to the possible shifting up or down different parts of the snow depth time series at certain stations. This can happen if they were found to experience a noticeable break (or shift) in the time series due to something like moving the station up or down in elevation. We state at L69, “Through personal communication, the authors of a recent homogenization study in the Alps (i.e., Resch et al. (2022)) have indicated that there are not any systematic changes in snow depth one way or the other as a result of the homogenization procedure (Marcolini et al., 2019; Buchmann et al., 2022).” For that reason, we do not expect the results in our paper to differ all that much from some potential future study that chooses to use homogenized data.

6. L96. Specify what those time series are? Seasonal averages at various years?

That is a good point. We will make this more clear that we are using time series of seasonal averages.

We have added this in. See L96.

7. L121. Delete “in a given month at a given station”. Unless I’ve misunderstood the statement, it refers to the snow depth coverage of the 291 stations for all the Januaries during 1901-2020.

We will remove that text.

We have done that.

8. L125-127. If so, why not simply use the November-April or the November-May season as in previous studies?

This is explained in the paper at L116, “During warmer months, and especially with stations at lower elevations, an observable amount of precipitation will not always translate to a measured snow depth. This would result in trying to fit a predictor time series (i.e., precipitation), which does vary, with a predictand time series that does not (i.e., snow depth). Therefore, we would like to minimize the number of cases where there is zero measured snow depth.” And since many more stations can have zero recorded snow depth in the months of April and May, we chose not to include those months in our model fit. That way, we can have consistency in the lengths of our seasons for both the predictors, T and P, and our predictand, SD.

9. L134. precipitaion → precipitation

We will fix that.

We have done that.

10. L137. “homogenized stations”? It seems the authors provide a method to homogenize the data, but precipitation and mean temperature are taken over all “available” stations?

We use temperature and precipitation station data that has already been homogenized by the data provider. We do not apply any homogenization ourselves in the paper.

11. L141-144. This is not clear. In particular, how is the first of the “two time series” computed? Is the second time series an actual time series or an average value over the training period? And, how is the “first time series” adjusted? Do you mean it is super-imposed to the average temperature computed in (2)?

We will rewrite these sentences in an effort to make it more clear to the reader.

We have tried to improve that section. See L161-172.

12. L154. Delete “the similarly”

We will fix the wording there.

We have done that.

13. Figure 3 shows correlations between SD and T or P, and their dependence with elevation. Given that T and P are not independent variables, perhaps it would be more illustrative to show partial correlations e.g., between SD and T while controlling for P, and between SD and P while controlling for T. In a way, those partial correlations are related to the partial derivatives over the surface shown in Fig. 5.

While our model is not very complex, we do want to layer some of the methodological concepts, piece by piece. We first transform the data into anomalies with respect to average T, P, or SD. Then, we show, like others have before, the correlations between SD and T along with SD and P. This initially presents “forecasting” SD as a one-dimensional problem as either a function of T or P. As you say, using either T or P, independently, only gives partial information about what we can expect with SD. That is why we then introduce Figure 4, and show the problem as two-dimensional. Then, in Figure 5, we break up the data into the elevation bands, and now we present SD forecasting as a three-dimensional problem.

14. L165-169. Unlike P and SD, Eq. 4 shows T “anomalies” relative to the climatology over the training period. These anomalies are not normalized. Why are they called “normalized” temperatures? If there is a need to refer to “normalized” T, P and SD with one term, then perhaps use “reduced”, or simply normalize the temperature anomalies with a relevant scaling factor common across stations and years.

In order to avoid confusion, the authors changed our terminology in the revised version of the paper. We will present these values simply as anomalies, either as percentages or degrees C from normal.

We have done that.

15. L165-174 Define $T_{x,t}$, $P_{x,t}$ and $HS_{x,t}$. In particular, is $P_{x,t}$ the accumulated or averaged precipitation over November-March at station x and year t?

Yes, $P_{x,t}$ is the accumulated precipitation over November-March at station x and year t. At L137, we state, “First, we obtain November-March sums of precipitation and averages of mean temperatures at all of homogenized stations over the years 1901/02-2020/21.” So, we use sums or accumulations of precipitation. Though the results would not change if one were to instead use average monthly precipitation. This would simply scale the precipitation accumulations by a common factor.

16. L177-178. The larger squares are hard to see in the figure. And, what “black lines”?

The larger squares are made using black lines. We will work to make this figure easier to see and interpret.

See the revised Figure 6.

17. L179-182 “One can observe… two-dimensional plane (not shown)… in the lower-right”. This is not clear. What 2D planes?

Consider the example that we have outlined. We have two predictors, T and P, which are being used to fit some model that can be used to “forecast” SD. With a multiple linear regression fit, then the value of SD depends on both T and P. When one predictor is used, the fit is a line. When two predictors are used, the fit is a plane or a surface. That is what we are referring to. We will improve our description of this in the revised version of the paper.

We have removed that wording, and have now added this sentence at L221: “As one would expect, the average snow depth anomalies increase as the temperature anomaly decreases and the precipitation anomaly increases.”

18. L176-190 This paragraph seems to be a motivation to include an SD dependence not only on T and P, but also on elevation. If so, the explanation could be simplified and made clearer, and previous work explicitly addressing this could be cited, e.g., Moran-Tejeda 2013 [doi:10.1002/grl.50463], Sospedra-Alfonso et al 2015 [doi:10.1002/2015GL063898], Scalzitti et al 2016 [doi:10.1002/2016GL068798].

We thank the reviewer in providing us with some relevant citations. We will look if we can make the explanation clearer.

We have added the citations to the Introduction. Also, see L219-233.

19. L207. valus → values

We will change that.

We have done that.

20. L232 and L234. Consider deleting “real-valued” and use only “absolute” value, or “full” value.

Thank you. We can consider that suggestion.

We have removed “real-valued.”

21. L241. This is confusing. How are HS MOD 1962−1971 ,x,t and HS OBS 1962−1971 ,x,t in Eq. 8 defined? Do they depend on t? And, is the numerator in Eq. 8 missing an ∗?

Thank you for bringing this to our attention. We will work to make this clearer.

We did remove the “t” subscript. Thank you for catching that. However, the rest of the equation is correct.

22. L271. The comparison is for the last 30-year averages relative to averages over a 40-year period. Why not 30 years for consistency? And, are the dots in the figure averages at all available stations? Sampling errors seem to impact more lower than higher elevations.

We chose to use a longer prior period (the 40-year period) in order to increase the robustness of the measured changes. We could use the most recent 30-year period compared to the 30-year period before, though this will be more subject to sampling variability than using a longer prior period. Though, if it is seen to make more sense to use 30 years for both periods, we can proceed in that direction.

We have chosen to stick with a longer prior period because of what we have said above concerning the robustness of that approach.

Yes, the dots are the percentage anomalies at all available stations. And no, these are not sampling errors that we observe at the lower elevations. This is instead an indication that the snow depth at lower elevations exhibits both greater variability and skewness than the snow depth at higher elevations. It also reflects the zero-bounded nature of something like snow depth or precipitation. The average seasonal snow depth for a station below 500 meters might be something quite close to zero, like 2cm. Some season might be 1cm (or 50% of normal), while another season might be 10cm (or 500% of normal). One can observe the same phenomenon for summer precipitation across the state of California. The average daily precipitation amounts are very small, where the daily averages in July are typically less than 1mm, for example. Then, in the rare events where something like 10mm of rain falls at a station, that would be categorized as an event that is greater than 1000% of normal.

23. L293. As mentioned above, I wouldn’t call this “forecast skill”, as these are not actual forecasts. Perhaps refer to it as a measure of model “accuracy” or “performance”?

We have discussed this above.

See our responses above.

24. L341-342. In the panels of Fig. 11, the authors give the correlation coefficients computed for the elevation bands and validation period. These correlations are largely driven by the decreasing trend (particularly at lower elevations). Could the authors add the correlations for the detrended time series?

We respectfully disagree with the reviewer. The trends are not responsible for the level of correlations that we report in Figure 11. The correlations that we reported in Figure 11 between the modeled and observed band averages in the validation period are 0.89, 0.84, 0.81, and 0.75, respectively for the four elevation bands. If we detrend both the modeled and observed time series for each of the bands, then we obtain correlations of 0.89, 0.83, 0.79, and 0.74. They are very close to the correlations which contain trends. We can write this in the revised version of the paper.

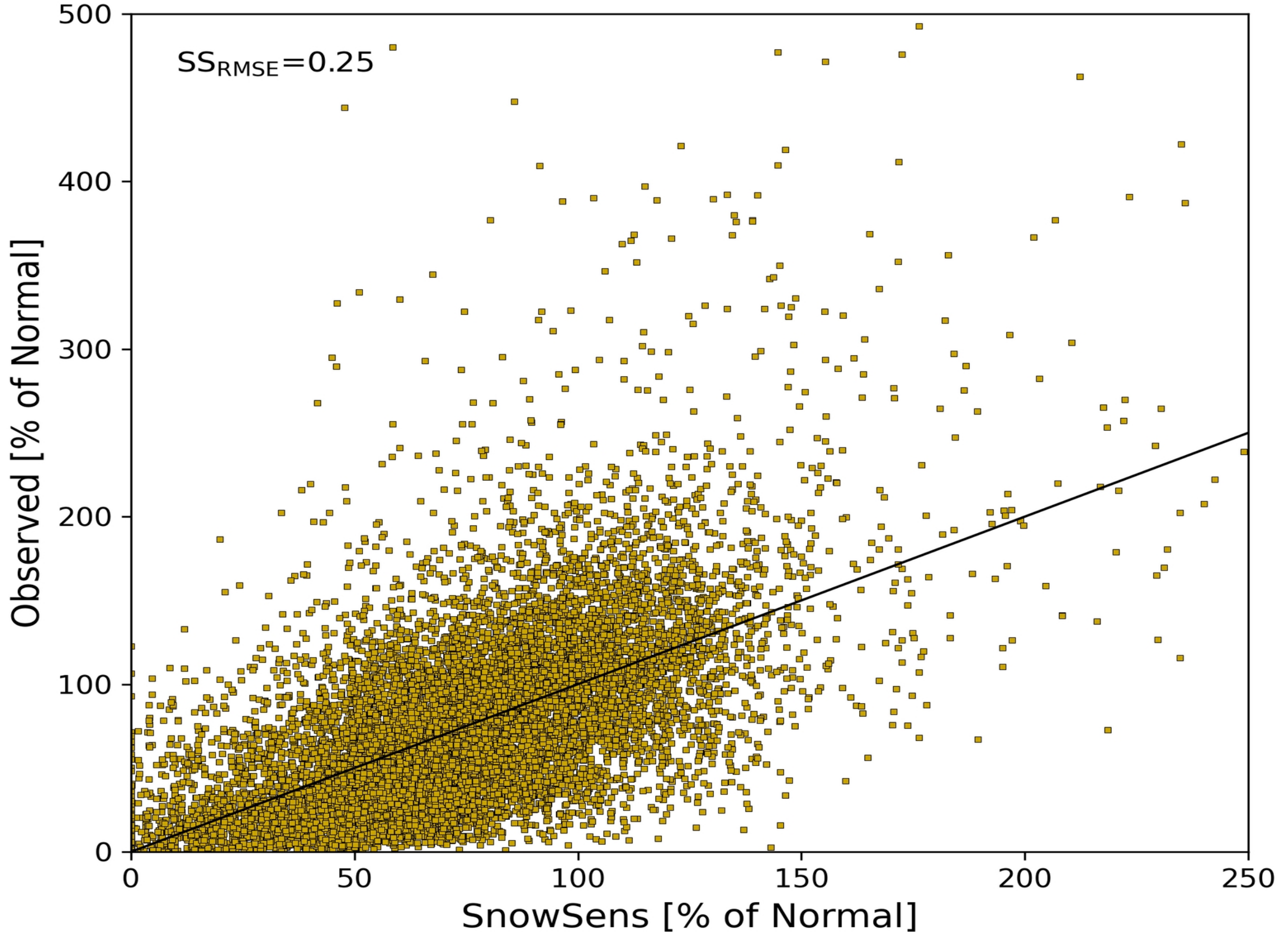
We provide the detrended correlations in the caption for what is now Figure 12.

25. L367-368 That SnowSens can“skillfully forecast year-to-year variability of snow depth” seems an overstatement, particularly when ACC at the level station were not provided or discussed.

We respectfully disagree with the reviewer. We do not consider that particular text to be an overstatement. The SnowSens model performs statistically significantly better than climatology, which is the baseline measure of skill. The ACC between the SnowSens modeled anomalies versus the observed anomalies is 0.593. This is the result of taking the ACC of all of the modeled vs observed pairings, or the yellow points, in Figure 9 from the paper. We have simplified the viewing of these SnowSens modeled vs observed values, and that is provided as Figure 4 in this document (see below). The number of modeled/observed pairings in the validation period is 10,985. Given that the stations are not independent of one another, the effective sample size would be smaller than 11,064. While the effective sample size might be on the order of three times smaller, we can go to an extreme to clarify our point. Let's instead reduce our sample size by a factor of 100. An ACC of 0.593, with a sample size of 110, has p-value of <1e-13. So, this can be interpreted to mean that there is less than 1 in a trillion chance, on average, that one could achieve the level of skill exhibited by our model, using randomly generated "forecasts." We also produced 10,000 randomly generated simulations, and the maximum ACC value we found in those 10,000 simulations was 0.037. We would argue that it is not in any way an overstatement to say that the SnowSens model has skill when evaluating the performance of year-to-year values at the station level.

As we just pointed out above, Figure 9 shows all of the pairings of modeled values vs observations for the validation period across the four elevation bands. Figure 10 and Table 2 also provide the skill at the station level. We discussed above why we used RMSE_SS instead of ACC.

See our response above. We have included this content above between L342-352, along with the new Figure 9.



Paper Figure 9: All of the modeled values and observed values in the validation period at the station level. There are 11,064 observed cases. If we had 100% data coverage for the observations over the last 50 seasons, then there would be 14,550 cases (50*291). The Pearson correlation coefficient between the modeled and observed values in this figure is 0.61. If one computed the correlation, station-by-station, then the average correlation across the stations is 0.66, with a minimum of 0.29 and a maximum of 0.87.

26. L378 Delete “of the world”

We can do that.

We have done that.