SPECIFIC COMMETS #2

Summary

Using the combination of terrestrial photography (TP) and snow depletion curves this paper aims for a new way to describe subgrid variability of snow distribution in the Sierra Nevada. Because of the changeable climate conditions in this area, multiple accumulation and melting cycles occur throughout the snow season. Using data for snow cover fraction (SCF) and snow depth (h) from a previous four-year study (Pimentel et al., 2015), five different depletion curves (one for accumulation, four for melting) were parametrized using a flexible sigmoid function adopted from Yin et al. (2003). Subsequently, these depletion curves were implemented into a point snow model developed by Herrero et al. (2009) with the use of a decision tree. Using three years for calibration and one year for validation, simulations of the SCF and snow depth were found to be generally accurate compared to the observed values. Although the simulations in both calibration and validation periods showed an overestimation of the SCF and a mismatch in snow depth values in some states, the use of TP allowed for errors in the simulations to be related to potential error sources in measurements.

Recommendations

This paper is well written and structured which allows for an easy understanding of the methods used and results obtained. The introduction gives a clear overview of previous research in this field and the authors do a good job in explaining the novelty of the approach. I think the paper fits the scope of HESS particularly well because of the multidisciplinary approach, which is shown by combining the products of previous studies in order to derive new knowledge and conclusions. The use of terrestrial photography shows that cost effective methods can be integrated with a modelling approach. Although I feel this paper is almost ready for publication I think the discussion in particular needs more elaboration in order to illustrate that the methods used and results obtained are an addition to the field of snow distribution modelling.

General Comments

The main problem I have with this paper is that, although the approach is novel, this is not emphasized enough in the discussion. The introduction outlines flaws in the methods of previous attempts to capture subgrid variability and argues the paper's novel approach could be an improvement. The performance of the produced model was compared to the field observations to check its' general accuracy, but this is however not enough back up the claim of novelty in the introduction. The whole study is based on the expansion of the model which was developed by Herrero et al. (2009), but no comparison of the supposedly improved model is offered with the original model. There is a mention that the inclusion of the 5-curve set of DC's improved the model (page 10, line 17), this however remains quite vague while I think this should be one of the main results of the study. What would help is including a section where the new model is directly compared with the previous model by simulating the same period with both models. This quantification also allows the reader to decide for themselves if the increased accuracy is justified by the increase in effort in respect to future research.

Additionally, I think there should also be a comparison with different sets of DC's. The text mentions that the DC's are clustered if parameter values are within 10% of each other (page 5, line 4). While there is merit in clustering the DC's like this, no argumentation is given for this value. A larger value would obviously lead to less depletion curves and different simulation results. How would including only one depletion curve for accumulation and one for melting affect the model results? Increasing the number of parameters and complexity of a model will almost always improve the results but to what extend will this affect the application of this method in other areas. I think it would be useful to include a section on this in the discussion.

The introduction also mentions the problem of defining a DC for a whole watershed because of the large spatial and temporal variability in snow distribution (page 2, line 23). The authors state that using a distributed application of DC's could be used to capture this variability. However, the control area used in this study was relatively small (30mx30m) and while the conclusion proposes that this method provides a foundation for the extension of snow point models, the discussion didn't contain information about the usefulness of this method on larger scale. Could this method be applied in large areas such as a whole watershed? For example, Luce et al. (2004) defined depletion curves for the Upper Sheep Creek basin which is approximately 26 ha in size. They did this for one accumulation-melting cycle which is more common for higher latitudes, as was mentioned in the introduction. In contrast to your findings, they found little variability between years. Would this also be the case for areas such as the sierra Nevada, where multiple accumulation-melting cycles occur throughout the year. I also wonder how the terrestrial photography method could be applied in larger areas. Was the high resolution essential for the results? Would remote sensing also have worked in this situation as was done in Kolberg et al. (2006)? I think it would be useful if these upscaling issues were mentioned in the discussion. In this way, the discussion would be more in line with what the introduction stated the approach and goals of this research would be.

First, we would like to thank Dr. Ryan Teuling for the selection of this paper as part of the introductory course of Master Programme Earth & Environment at Wageningen University. We also thank the comments and suggestion made by the student E. de Badts to our work. All Referees and Reviewers in fact point out some of the points that the Reviewer mentioned in his remarks: the emphasis in the discussion comparing the results with existing literature, the explanation of how the curves are incorporated to the model, and the applicability of results beyond the local scale. These comments have been addressed in our previous answers and some changes have been introduced in the text. Please, see the different comments to the other Refeeres and Reviewers for further details.

Specific Comments

I think the title should mention the use of snow depletion curves as it is a major part of this study.

We have decided not to change the title of the paper, since our goals focus on the subgrid scale effects, and the introduction of this tool in the title could induce some misunderstanding related with its traditional use over different scales.

The RMSE of the simulations is mentioned in the abstract but these are the RMSE values for the calibration period. I think showing the RMSE values that were found during the validation of the model would be more representative of the general accuracy.

We have changed these values according with the Reviewer suggestion.

Page 2, line 12: “(Mark and Dozier, 1992)” should be “(Marks and Dozier, 1992)”.

Page 2, line 20: “Kolbert et al. (2006)” should be “Kolberg et al. (2006)”.

We apologize for these typos. We have corrected them.

Page 4, line 15: The explanation of what exactly constitutes an accumulation melting cycle remains vague to me. Does a cycle mean the time between the accumulation of snow from a certain level and when it returns to that same level due to melting? Is it possible for multiple cycles to be ongoing at the same time? Please elaborate.

Following this and other previous comments, we have changed the definition of accumulation/melting cycle into “…to the time period between the beginning of a snowfall event and the end of the complete ablation of the snow or the occurrence of a new snowfall event (see page 4 lines 21-22 in the revised text)

Page 4, line 22: “Ying et al. (2003)” should be “Yin. et al. (2003)”.

We apologize for this typo. We have corrected it.

Page 5, line 4: How did you decide upon using 10% difference for clustering the curves? There should be an argumentation for this.

We chose a threshold for this procedure large enough to obtain more than 2 clusters. 10% was selected as standard difference or error threshold usually found in literature in many applications.

Page 5, line 17: The explanation of how snow depth was obtained from the photos with the clustering algorithm needs more elaboration. Was it necessary to use two snow rods if the reference snow depth was used to calculate the actual average snow depth? What was the previously defined linear equation mentioned in line 19?

Figure 2 implies the relation between the average snow depth and the reference snow depth is linear, which it is not. The figure could be clearer on explaining the relation between the reference and average snow depth.

We have introduced some new sentences in section 3.2. to clarify the snow depth measurement using the poles installed in the study area. We have also changed Figure 2 (see page 5 lines 22-34 in the revised text)

Page 6, line 11: In equation 3, R is used to indicate the precipitation while the text says P.

We apologize for this typo. We have corrected it.

Page 7, line 21: Did excluding the cycles with short duration and cycles were the area was completely covered leave you with 16 cycles? Could use a better explanation on the method of choosing the cycles that were eventually used for calibration. It does seem from figure 5a that cycles were only used for calibration if the SCF dropped to 0. This was however not mentioned in the text.

Yes, after the application of these two criteria we only considered 16 over 53 cycles. The cycles that the Reviewer mentioned are included in the disregarded ones.

Table 2: In the 6th and 7th row of the table: instead of showing the mean duration of accumulation and melting cycles for the whole study period, the means of all columns are summed as was done for the 5th row.

We have changed the denomination of these two rows: annual number of days with snow accumulation/accumulation (see Table 2 in the revised version)

Figure 5b: The axes on the graphs of the curves are not clear.

We have replaced them by a higher resolution version in the revised manuscript.

Table 4: The values of h(e) in the table don't correspond with the values in the text at page 8, line 21.

We apologize for this typo. We have corrected it, the correct one are those written in Table 4

Table 3: I think it would be practical if the table was sorted according to the curve types

We were thinking about this option, but we have finally decided to maintain the table in its current configuration to focus on the variability of cycles found during each analyzed year.