

Interactive comment on “Subgrid parameterization of snow distribution at a Mediterranean site using terrestrial photography” by Rafael Pimentel et al.

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“Note to the editor and authors: As part of an introductory course to the Master programme Earth & Environment at Wageningen University, students get the assignment to review a scientific paper. Since several years, students have been reviewing papers that are in open online discussion for HESS, and they have been asked to submit their reports to the discussion in order to help the review process. While these reports are written as official reviews, they were not requested for by the editor, and we leave it up to the editor and authors to use these reports to their advantage. While several students were asked to review the same paper, this was not done to provide the authors with much extra work. We hope that these reports will positively contribute to the scientific discussion and to the quality of papers published in HESS. This report was supervised by dr. Ryan Teuling.”

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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

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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

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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.

Specific Comments

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

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.

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)".

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.

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

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

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.

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

Page 7, line 21: Did excluding the cycles with short duration and cycles where 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.

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.

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

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

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

References

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Herrero, J., Polo, M.J., Moñino, A. and Losada, M.A.: An energy balance snowmelt model in a Mediterranean site, *J.Hydrol.*, 371, 98-107, 2009

Kolberg, S. A., and Gottschalk, L.: Updating of snow depletion curve with remote sensing data, *Hydrol. Process.*, 20, 2363-2380, 2006.

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