SPECIFIC COMMETS #3

This paper has derived several depletion curves and implemented them to upscale a point model to a larger grid. To do this the snow cover fraction and the snow depth are obtained using terrestrial photography using the methods from Pimentel et al.(2015). From that data and with a sigmoid function, 5 depletion curves were derived, one curve for accumulation and four curves for the melting of snow. These depletion curves were then implemented into a point snow model from Herrero et al.(2009), through a decision tree. The model was calibrated with different simulations over 3 years and after that a validation run was done for one year with the optimal simulation. The results show an accurate SCF and snow depth with similar results for both the calibration and validation. There are some errors due to weather phenomena that are not implemented into the model. The use of different depletion curves makes upscaling to larger areas possible.

I think this paper is appropriate for the journal of hydrology and earth system science because the modeling of snow is an important part of the hydrological modeling and it fits well with the scope of the paper. The research is new and innovating. The use of several depletion curves to describe accumulation and melting has never been done before and instead of the WUE like most papers, the snow depth was used for the depletion curves. This derivation of these depletion curves from terrestrial photography is an innovative approach that will be useful in further research.

This paper provides a very good research with solid methods. The methods chosen fit well together and form a consistent research together. They are well implemented from the previous literature and still very well written so that it is understandable how the methods are applied. They are to the point and explained well. Also this paper gives a very structured and good presentation of results. It is really understandable what they have done in the methods and how they came to their results. The results are given for every step of the methods, throughout the process of deriving the curves and implementing the model. This makes the process understandable and repeatable and the results credible. By chopping up the results in reasonable parts, it has a good structure which makes it nice to read. The figures and tables of the results also are understandable and are easy to understand. The introduction gives a good indication of previous methods used and it gives a good overview of history of snow modeling, making the subject more understandable The writing style of the paper is one of the lesser parts of this paper. It has been written like all the background information is already known to the reader and more details need to be implemented for better understanding. Also the discussion could be improved in several ways. Overall, I think that the research done in this paper is really well thought out and very well executed. Therefore I recommend publication for this paper with some moderate revisions following the arguments below.

First I will explain the major arguments that I think must be addressed, afterwards there are my minor comments of how to improve the paper and lastly there is a list of minor revisions.

The introduction starts with the main reason for this research. Thereafter it goes into detail of what is known and unknown and the actual goal of the research. After reading the introduction it was difficult to understand the paper since there is no context. What is missing here is an introduction of the subject of the paper, which is important for people who are no expert in the field of snow modeling. In this paper, there is only one sentence introduction(p1, line 25) and no context given. The reason for initiating this research is too short for people who have not read the background

material. It is unclear from the introduction why the scale issues are still an issue since Bloschl et al is written in 1999 and why this snow distribution modeling is important at all. This change will only affect the introduction of the paper, which can be improved in some ways. One option is a single figure in the introduction that gives the context of the subject by introducing the links between the most used terms. An explanation of the subject is also possible. In Anderton et al,(2004) and Luce et Tarboton,(2004) they first introduce the subject of snow models and explain various terms. Then they take a narrower view towards their problem that they want to solve. To broaden the reason for this research, more detail could be applied. Bloschl et al, 1999 came first with the problem of scale issues, but in this introduction there is not yet an explanation what these scale issues pertain or why they are important in the context of snow modeling so that can be implemented in the introduction.

In the discussion, the results are only compared to one other study that is a previous study using the same method, namely Pimentel et al.(2015). This is also only done in a qualitative setting by mentioning that the results are improved without mentioning what the results were of the previous study(P10, line 17-19). By not comparing the results with other papers using different methods of modeling subgrid variability, the credibility of the results can be questioned. It undermines the feasibility of this research since no quantitative comparisons are done. The addition of numbers will give a better visual presentation and makes comparison much easier for the reader. Also other papers can be mentioned that used other models or methods to derive the depletion curves such as Kolberg et al(2006), Luce et Tarboton, 2004 and also Herrero et al, 2009 (other DC's). This shows how these results compare within the field of snow distribution modeling and if the model is truly a good foundation for further upscaling of models as stated in the conclusion.

One of the goals of this paper is to give an insight in the upscaling from a point model to a 30x30 grid(P6, line 25-26). However, nowhere in the paper it is explained how this upscaling is done in the model. There is a mention that this upscaling is done by implementing the depletion curves into the model (P6, line 25-26), but by indicating that, still no explanation is given for the process of the actual upscaling. By not describing this process, an important part of the methods is left out. The research is not repeatable and less applicable for further research on upscaling. This can be remedied by explaining the process of the upscaling in the methods. This can be done for example by a step by step explanation or a short summary including the formulas used as is done in Luce et al, 1999 and Pimentel et al, 2015.

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 R. Meeusen to our work. Some of the suggestions made by the Reviewer have been addressed in previous comments. Some sentences have been introduced in the Introduction to clarify different aspects (see page 2 lines 1-2, 7-8 and 23 in the revised text); moreover, specific values for the metrics obtained in previous works are added to the discussion (see page 12 lines 1-14 in the revised text); and finally some clarifications are added to the methodology section to better understand how the curves have been incorporated into the model (see page 9 lines 12-16 in the revised text).

The minor comments:

Minor comment 1) The results of the RMSE mentioned in the discussion, conclusion and abstract are the results that were achieved with the calibration of the model. This seems counterintuitive since the validation of the model indicates how well the model actually works, while the calibration gives the optimal values for the model that can be achieved. Therefore the results of the validation period are more important to indicate how well the model works and should be used instead of the calibration results. The argument is given that these results are almost the same, but then please explain on what that is based, because there is still a difference in the number and if the numbers are indeed significantly the same, then still the validation results can be used for the comparison to other papers.

We have revised and changed these values in the abstract according to the Reviewer suggestion (see page 1 line 22 in the revised text).

Minor comment 2) The second paragraph of the discussion (line 8-11) is unclear in what is meant. Figure 9 is a important figure but does not seem understandable now. An explanation is needed of what is exactly visible in the different pictures, and what is different between the pictures with same SCF. In line 8-11 an explanation of weather phenomena are given that are not clearly visible in figure 9. Please rewrite this paragraph and give an explanation of the differences visible, and in weather conditions, in the pictures in figure 9.

Minor comment 3) The third paragraph of the discussion (line 12-16) is unclear in their goal. The arguments given in this paragraph do not seem to relate to the conclusion in the last sentence of this paragraph. Please explain this relation better and why that conclusion can be drawn. Also that conclusion relates to the last sentence in this paper, which is a conclusion that this research provides a basis for extension of snow models to larger areas. However these conclusions do not say the same thing. Please relate the two conclusions better with each other and give argument for these conclusions. Also explain why these conclusions can be drawn from the results because that is now not completely clear in the paper.

We have added more discussion of the results following these two Minors comments 2 and 3 (see page 10 lines 19-34 and page 11, lines 1-14 and lines 24-34 in the revised text)

Minor comment 4) This paper has a good explanation of DCs. The description of the method of acquiring these depletion curves is very accurately described and it is well thought out how they would use the data to derive the depletion curves. Especially page 8 gives a very clear overview of each depletion curve that is used. This part is very nice and well written.

Minor comment 5) In paragraph 3.2 the methods for acquiring the snow depth with the rods is explained. However is unclear to me how the reference snow depth is calculated. Figure 2 does not help in explaining this method. Please give more explanation of the method and explain figure 2 in more detail. Also why the different snow depths are visible at the levels that they are located on.

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).

Minor comment 6) At the very end of section 4.2 on page 8 there is said that a decision tree is implemented into the model, but it is not described how this is done. The decision tree is not even mentioned in the methods. Please describe in the methods how this decision tree is implemented in the model.

Following this, we have introduced some new sentences explaining how the decision tree is used by the model (see page 9 lines 11-16 in the revised text).

Minor comment 7) At the end of page 10, the error sources of the depletion curves are given. Both the first and third error also give a possible explanation for these errors. However the second error does not have any explanation of how this insufficiency of rain-over-snow effects is caused. Please indicate the reason for this error, or indicate that it is unclear how this error is caused.

The model does not include a specific formulation to capture this effect. It could be a future improvement on the model.

Minor revisions:

P1, line 17-18: "The resulting DCs were able to capture certain physical features of the snow, which were used. . ." seems like the physical features were used and included into the model instead of the depletion curves, so the sentence structure could be changed.

P1, line 29-30:" Luce et Tarboton, 1996" is not mentioned as such in the references, should be Tarboton et Luce, 1996 or reference needs to be changed.

P2, line 20: "Korbert" should be "Kolberg".

We apologize for all these typos. We have corrected them in the revised text.

P2, line 28-31: These sentences can be moved to the methods, since they describe the possible shapes for depletion curves.

These sentences describe the general pattern that accumulation/melting cycles follow, rather than the curves used to quantify their evolution, and that is why this is included in the Introduction.

P3, line 3-5: These sentences can be moved to the methods, since this information is missing there, and if it all information on the methods of the rods is put together, the explanation of the rods is more understandable.

These sentences do not aim at describing a technique; they include further usefulness of the terrestrial images, and we have included it to focus on the potential advantages with easy tools.

P4, line 22:"Ying et al" should be "Yin et al".

We apologize for this typo. We have corrected it.

P5, line 6-7: This sentence pertains results and does not have to be mentioned in the methods.

Following this, we have removed these lines from this section.

P5, line 19: "A previous defined function" has not been defined in this paper, so please put it in the paper.

We have rewritten this sentence to avoid confusion (see page 5, line 27, in the revised version and new Figure 2).

P6, line 11: "P" is not mentioned in formula, should be "R" that is mentioned in formula above, or the "R" should be changed into a "P".

We apologize for this typo. We have corrected it.

P7, line 15-16: The paper mentions 18 cycles per year with a duration of 49+108=157 days for each cycle. This seems very illogical since it seems that the cycle duration is too long to fit 18 times in one year. Either a bit more explanation that cycles can overlap is necessary or the sentence structure needs to be changed.

We have rewritten the sentence: The number of cycles and their duration varied considerably over the years, with a mean number of 18± 5 cycles per year and a mean duration of 3 ±1 and 6±5 days for the accumulation and melting phases of each cycle, respectively. On an annual basis, the mean number of days with melting and accumulation dominance was 49 ±14 and 108 ±18 days, respectively (see page 7, lines 21-24, in the revised version).

P7, line 26-28: These sentences can be moved to the methods. The amount of detail given here belongs in the methods, not in the results.

P8, line 28-29: This sentence belongs in the methods, since it is not mentioned there.

P9, line 2-4: These sentences belong in the methods. The decision tree is not mentioned in the methods and this amount of detail should be mentioned in the methods.

In fact, these sentences focus on aspects that were derived after the obtaining of some results or during their analysis, and anticipating them in the methods make it complex to maintain a clear order of reasoning. We have maintained the current version due to this.

P11, line 3: it says "error sources of error" so one error can be left out.

We apologize for this typo. We have corrected it.

P11, line 28: It says "error of less than", but in the rest of the paper these amounts are given as the error, not a smaller value as is insinuated here.

We apologize for this typo. We have corrected it (see page 13 line 27 in the revised text).

P14, line 5-6: I think the wrong reference title is mentioned here, since the paper with that title is from 1997, and does not mention depletion curves, while the paper that is referred too does mention DCs as indicated in the text P2 line 14.

We apologize for this typo. We have corrected it.

P16, table 1: This table can be left out of the paper, since this figure does not have added value to the understandability of the paper.

P17, table 2: This table can be left out of the paper, since this information is not necessary to understand the paper, and is not explained in the text.

Table 2 sums up the changeable characteristics of accumulation/melting cycle on semiarid areas. We have maintained it.

P 20/21, table 5 & 6: these figures can be combined into one figure since table 6 is very small.

Following this, we have combined both tables in a new Table 5.

P 23, fig 2: it is unclear how href and h1 and h2 came to be in this figure, more explanation in the header can give more understanding of what is meant with each parameter visible in the figure.

We have introduced some clarification in section 3.2 and in Figure 2 (see page 5 lines 22-34 in the revised text)

P24, fig 3: The header of the figure could use an explanation about the differences in weather condition between the three different dates, as it is now quite unclear what makes them different since now especially the first and last date seem to indicate the same conditions.

We have introduced more information in the caption of Figure 3 following the suggestion of the Reviewer.

P 25/26 fig 4 & 5: both figures can be combined, the only difference in figure 4 with 5 is that all cycles are mentioned in figure 4 instead of only the cycles used in this paper as in figure 5. Placing two of the same figures in the paper with little difference in information is unnecessary.

We have decided not to joint Figures 4 and 5, since the information that they show it different in each of them.

P26, fig 5: axis of the DC curves are not readable.

P27, fig 6: axis of the DC curves are not readable. P27, fig 6 "More than 30 days with previous snow" is unclear what is meant, so it would be good to reformulate.

We have improved the resolution quality of both Figures.