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Interactive comment

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."

Summary and recommendations

To perform hydrological modelling which is GIS-based subgrid variability is important to be taken into account. Snow coverage in Mediterranean mountainous areas is highly variable. One crucial factor in the spatiotemporal distribution of snow is micro-topography. Due to the heterogeneous snow distribution it is hard to calculate a snow depletion curve for semiarid regions which is representative for the whole area. Do define depletion curves snow cover fraction data over the whole study area is needed. Terrestrial photography was used in this research to derive snow depth and snow cover fraction at subgrid scales. In this study a four year series (the hydrological years 2009-2013) of terrestrial photography images were taken in a study area in the Sierra Nevada Mountains in Spain. The images showed a high variability in snow accumulation-melting cycles. Based on these terrestrial photography images one curve was defined for the accumulation phase of the snow cycle and four curves were derived which are representative for the melting phases. All these four cycles are representative for different kinds of melting phases. Next a sigmoid flexible function was used to parameterize the snow depletion curves. The parameterized depletion curves were implemented in the point physical snow model described in Herrero et al. (2009) so that snow cover fraction becomes a new state variable. Simulation were performed with this model to check what the optimal model parameters were. The simulation with the best parameters shows that snow depth and snow cover fraction are simulated with a high level of accuracy. Although snow cover fraction can be overestimated in short snow accumulation-melting cycles and snow depth simulation results show different results than in reality for certain states the simulation is still accurate enough. Overall it is concluded that implementation of depletion curves that are derived from terrestrial photography improves the performance of the snow model.

In my opinion the manuscript fits perfectly to the scope of Hydrology and Earth System Sciences. The research topic is very inspiring since it is a well performed research on a small scale study area. The topic is very interesting and the research continues on

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previous research in the Sierra Nevada Mountains. They usage of depletion curves to improve the snow model is good since they are developed to characterize the decrease in snow cover fraction when snow melts (Luce and Tarboton, 2004; Rango and Martinec, 1982) and snow cover fraction is one of the important variables that is being studied in this research. Next to this I like the writing style of sections 4.2 and 4.3 very much. The results are discussed stepwise and in good detail. These sections are clearly written and they are well supported with figures and tables. The part of the discussion were the success of the depletion curves derived by the terrestrial photography is discussed is pleasant and clear to read. The manuscript is worth publishing after some moderate revisions. I have three main comments which I will discuss in detail in the main comment section. First of all the overall message why it is so important to fulfil this research is missing. Secondly a comparison with existing literature is missing in the discussion. The conclusion that the implementation of the depletion curves in the snow model improves it performance misses comparison with quantitative results from previous studies. Next to this I also think that the methodology of the incorporation of the depletion curves in the snow model misses some explanation.

Main comments

The overall message why it is so important to fulfill this research is missing. In general the research is a good research which is performed with a lot of high quality techniques. These techniques are all very well integrated. However the importance of the research is not well described in the introduction. Only after reading the last paragraph of the conclusion I could make a suggestion of the importance of the study. Even though it is important to note down the importance of this research in the scientific world. By doing this the readers can be more convinced about the importance of this research. I would suggest to point out in the introduction what the advantages are of the extension of point physical snow models with depletion curves. Why is it so important to know how snow cover fraction changes on such a small scale? For example Anderton et al. (2004) tells that snow models are important in hydrological modelling and spatial variability has large influence on the prediction of surface runoff in a catchment. Another example which explains the importance of the understanding of snow cover fraction processes is that snowmelt which leads to surface runoff can only occur where the surface is covered with snow (Luce and Tarboton. 2004). I recommend to include sentences like this in the introduction and add at the end of the introduction a small paragraph where the author explains to the reader that he hopes that his results will lead to a more improved prediction of the snow cover fraction to improve the prediction of surface runoff in hydrological models.

My second main comment is about the fact that the discussion is missing comparison with previous research. The description of possible errors sources for the simulation of the model is not supported with any existing literature. It would be nice to see that the conclusions made in this paragraph are supported with previously written scientific work. Another example is that it is concluded that the results of this research are better than the results of Pimentel et al.(2015). Quantitative numbers of the accuracy of both snow model performances are not given which makes this statement hard to believe. Not only would it be better to discuss this with quantitatively results but believability of the conclusion can also increase if this research is compared to more than just one study were the performance of snow modelling is tested.

The explanation of the implementation of depletion curves in the point physical snow model is not clear enough. Right now it is impossible for other researchers to repeat this research. It is only described that the point physical is expanded by incorporating the depletion curves obtained from the terrestrial photography. However it is not explained how this is done, only that by doing this snow cover fraction will become a new state variable. Now readers can put question marks to the methodology. An unclear methodology can question the credibility of the results for the simulation of the snow model. I would suggest to explain how the depletion curves can be included in the model. Luce and Tarboton (2004) and Luce and Tarboton (1999) show that this can be done with only one sentence; by telling that the equations for the mass balance will only be applied for the snow covered fraction. By adding a sentence like this it is clear how the implementation of the depletion curves lead to snow cover fraction as a new state variable . Next to this it was also discussed in Herrero et al. (2009) that the importance of the time step of the algorithm that solves the mass and energy fluxes is important. By including a sentence were you refer that the same algorithms as in Herrero et al. (2009) are used or an explanation why a different time step is used this problem can be solved.

Minor comments

Minor comment 1: In the text the five curves are most of the time all called depletion curves (for example p8, line 5), although this is not true for curve 0 since it represent the accumulation phase. Try to call only the four melting curves depletion curves and rather call curve 0 the accumulation curve.

Minor comment 2: The terms micro-topography or microtopography are both used in the manuscript I would suggest to use only one of them. On page 11 in line 17 it is the only time that is has been written as microtopography.

Minor comment 3: The same as above try to stay consistent with the use of accumulation/melting cycles and accumulation-melting cycles.

Minor comment 4: The reference of Luce et al. (1999) in the reference list is not published in 1999 but in 1997. This paper is used as a reference to show that in the past depletion curves have been implemented in snow physical models, but in Luce et al. (1997) depletion curves are not mentioned at all. After some research I found the paper "Sub-grid parameterization of snow distribution for an energy and mass balance snow cover model" by Luce et al (1999). This paper is published in 1999 and in this research implementation of depletion curves in snow models is discussed. I would recommend to take a look at this and include this paper in your reference list and leave out the paper by Luce et al. (1997) since it is not used anywhere else in the manuscript.

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Minor comment 5: There are too many tables and figures. In the list of minor issues I have noted recommendations to combine some tables and figures. I made also a suggestion to take out one table.

List of minor issues

P1, line 30: A used reference is Luce and Tarboton (1996). In the reference list there is no reference from Luce and Tarboton from this year. So either the year number on page 1 is not correct or the reference is missing in the list.

P2, line 20: The reference Kolbert et al. (2006) should be Kolberg et al. (2006).

P3, lines 3-5: I suggest to not include this in the introduction since it is detailed methodology. The first introduction of the rods should be mentioned in the study site and available data section.

P4, line 22: There is a typo in the reference Ying et al. (2003). It should be Yin et al (2003).

P5, lines 6-7: This should not be included in the methodology section. The fact that 5 parameterizations are selected is a result. Therefore I suggest to remove these lines from this section.

P5, line 19: The previous defined linear equation is not defined earlier in the text. This arises question of the credibility of your methodology section. Provide in section 3.2 also a description of this equation that is used.

P6, lines 8 and 11: In equation 3 precipitation is defined with R, however in the text it is described that P defines the precipitation.

P6, line 16: Include a reference that support why W can be disregarded. In Pimentel et al. (2016) it is stated that the rapid snow metamorphosis is observed but also in this paper a proper reference is missing. I would recommend to refer to the observations or give another proper reference.

P7, line 2: The year of the reference Cline (1999) mismatches the year in the reference list.

P7, lines 15-16: It is stated that each year has a mean number of 18$\pm$ 5 cycles a year and a mean duration of 49$\pm$14 days for the accumulation phase and 108 $\pm$18 days for the melting phase. This tells me that one cycle has a mean duration of 49+108 = 157 days. But 18 times 157 days will exceed the amount of days in one year. This is very confusing.

Table 1: This table can be left out. I did not have the idea that this gives important information needed for the research. Rather describe more of the meteorological conditions in the text.

Table 3 and 4: Add table together since they both give information about the fitted parameters for the depletion curves.

Table 5 and 6: Figure 6 is a very small table which continues on Table 5 since it shows the errors of the calibration parameters used in simulation 7. I would recommend to add Table 6 with Table 5.

Figure 3: This figure is used to show that terrestrial photography can be used to determine accurate depletion curves even when the atmospheric conditions are different. Include in the caption which atmospheric conditions were present for which picture. This makes the argument more powerful.

Figure 5: The quality of this figure is not good. The upper part with the accumulation-melting cycles is still readable, however a higher resolution would give a more neat figure. The quality of the depletion curve patterns in part b is very bad. Since I can see the shape of the accumulation curve and the depletion curves the fact that the 5 curves are very different was still clear to me. But I am not able to read the axes and titles. Due to this detailed discussion of the results cannot be followed.

Figure 4 and Figure 5: Since part a of Figure 5 and Figure 4 both show the accumulation-melting cycles for the calibration periods I would suggest to combine these two figures. In Figure 4 the lines of the snow depth for the cycles that are used for the calibration process can be made bold. I would recommend to add the depletion curves of part b of

Figure 5 underneath the calibration and validation figures.

Figure 6: The same accounts as for Figure 5; the depletion curves figures have a low resolution, which makes it impossible to read the figures properly.

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