Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-601-RC2, 2017

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Influence of snow surface processes on soil moisture dynamics and streamflow generation in alpine catchments" *by* Nander Wever et al.

Anonymous Referee #2

Received and published: 19 May 2017

The manuscript mainly presents a comprehensive, multi-objective model validation study of an extension of the complex, physics-based Alpine 3D model. The model's performance to represent streamflow, snow depth, soil moisture and soil temperature/freezing is analyzed for 3 years in a Swiss mountain catchment and its surrounding. This evaluation is a framed by the context of the importance of soil moisture as a pre-disposition for floods. This framing – for my taste – is a bit wanted, and the title is somehow misleading. However, the quality of the model validation study is of a high standard and certainly of high importance. Especially the parallel evaluation of snow, soil and streamflow representation is cool. Although the overall impression of the article is very good, I do have some moderate remarks. The manuscript is certainly within

the scope of HESS, it my knowledge of original content and can be a valuable article after my concerns are addressed.

General comments:

- As mentioned before, I find the title a bit misleading, as it reads as the influence of snow processes on soil moisture and streamflow is analyzed. Instead, the focus is clearly on the model validation to reproduce the linkage between snow, soil and streamflow. I would recommend a rephrasing of the title.

- Alike reviewer 1 (I haven't read his comments until I finished my review), I do not understand the usage of the soil water fluxes for streamflow generation. As reviewer 1 wrote to the point, why should one just take one of the fluxes (-2cm indicating surface runoff, -30 cm indicating interflow, -60 cm indicating baseflow). I also had a look in the cited publication, yet I find the entire concept very unusual and irritating. Because of this (I guess), the interpretation of the influence of soil moisture (Page 10, lines 14 ff) on streamflow is a bit simple. E.g. "neglecting the soil layers almost completely, by routing the 2 cm flux to the runoff model, is reducing the model efficiency". – This is logical as you neglect interflow and baseflow in summer months and interflow widely considered to be the dominant process in alpine catchments. Hence, the concept of this streamflow generation needs to be clarified and its strong limitation in terms of dynamic runoff generation should be discussed. Furthermore, effects of this simplified approach on the interpretation should be discussed.

- The description of the different soil layers is unclear: You introduced increasing soil layer magnitudes (from 2cm to 40 cm) up to a soils depth of 300 cm in the model. However, you take water fluxes from 2, 30 and 60 cm. Moreover you compare these to soil moisture measured at 10, 30, 50, 80, and 120 cm depth. And finally, you take the average of the upper 40 cm (page 10, line 25) as the soil moisture state within the catchment. As these number do not match at a first glance, a clarification is advisable. Maybe a sketch would help.

- In the manuscript, the SNOWPACK and Alpine3D are described as two separate models (e.g. in the model description and partly in the introduction). But as written in the Conclusion, SNOWPACK is a module of Alpine3D and as I understand an integrated part of Alpine3D. This should be clarified throughout the text, especially in the beginning (Aims section)

- As the Dischma catchment is an alpine catchment I assume that skeleton fraction is a major issue, both for measuring the "correct" soil moisture as well as for simulating the soil moist dynamics. Please, clarify how and if the skeleton fraction was considered in the pedo-transfer-function and how it was considered in the selection of the measuring location (and how representative the selection in terms of skeleton fraction is). Moreover, please discuss if the found biases in the soil moisture and soil temperature simulations can be explained by skeleton fraction. Finally (I hope I did not miss it), how do the soil types of all measuring stations represent the soil types in the Dischma catchment.

- The description of the meteorological data is quite long and very detailed. I would suggest to just briefly describe the table 2.

Specific comments:

- Page 1, line 11, and 12: Please clarify the word "including", as you do not combine the three layers.

- Page 2, line 29: "small scale surface processes". Please, specify the scale.

- Page 3 ,line 5: Please, specify the catchment size

- Page 3, line 13 ff & Figure 2: How did you separated snow from rain here.

- Page 3, and Table 1: A comparison to the long term norm period would be interesting

- Page 4 and Figure 1: "Golfplatz" in the Figure versus "Golf course" in the text. "SLF2" site is named "SLF" in the map. How were the boarders of the Dischma catchment

defined (topography based from the model?). I would recommend some light, partly transparent background color for the names, to improve readability. I have to admit, I am not a fan of topographic maps as background, especially if the legend is missing. Any chance to replace it with a more generalized map?

- Page 6, line 5 ff: Are the interpolations done for each time step?

- Page 6, line 14: I do not think that "initialization" is the correct term. Is it not parameterization?

- Page 7, line 21. "sub-catchments" – so is this approach some kind of HRU approach?

- Page 7, line 33: Again, the soil moisture is calculated for the first 40 cm. Can you clarify its relation to the 30 cm stated before and after.

- The definition of a rainfall event is a bit broad. Do you used mowing 12 h sum? What if a rainfall event is ended by falling below the 3mm thresholds criteria, but followed by a >10mm event again. Why do you choose a time window of 12 mm. Did you do any concentration time analysis?

- Page 8, line 14, and Figure 3. A comment on the vegetation growth (?) during summer would be nice.

- Page 9, line 27 ff. In my opinion, the r2 is not the appropriate statistical measure here, as it does not consider any systematic offsets/biases. The application of the RMSE or similar would be more fair. Furthermore, can you set your results in light of other models of soil moistures in alpine terrains? Also to show that your results are pretty good.

- Page, line 10: "however, ..." Isn't this finding clear and logical as you only consider "deeper" water fluxes

I am looking forward to the revised manuscript.