Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-37-RC3, 2016

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Assessing the benefit of snow data assimilation for runoff modelling in alpine catchments" *by* Nena Griessinger et al.

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Received and published: 28 April 2016

The article addresses a highly relevant topic that is the added value of implementing external snow models and snow data in a hydrological model. In the present study, three different snow models of increasing complexity are attached to a HBV model and tested in 20 different mesoscale catchment within Switzerland. The catchment cover all altitudes present in the Alps. For all catchments the model performance of reproducing the runoff with in the snow melt seasons was assessed and served as the basis to judge the added value of the snow models. The authors found that the implementation of a snow model that additionally assimilates observed SWE data improves the runoff considerably, especially in high altitudes and in snow-rich years. The article is very well structured and written, concise and comprehensive at the time. The article is to my knowledge of original content and suits well in the scope of the journal. I still need

to point out some more general concerns and a couple of minor comments. After a revision of the manuscript that take into account this concerns and comments, I would recommend publication:

superior comments/concern: a) The first concern addresses the interpretation of the results. What is exactly the added value of the assimilated data set. Is it a more sophisticated and correct snow melt model or is it rather the added indirect information of precipitation amounts fallen in high altitudes where the meteorological station network is not present. My interpretation would be the latter, as the differences between model M1 and M2 (e.g. assimilation) are considerable for the highest altitudes. I would appreciate a discussion on this question. b) A follow up on this issue. The SLF station data are known to overestimate the SWE amounts. How was this issue addressed in the study and if not what are the consequences for your model as you may have calibrated your model against "differently wrong" data. c) The LOO validation produces by nature highly variable performance values. I find it difficult to estimate differences between the models based on medians of boxplot. I would rather use a significance test. I recommend to show validation boxplots side by side and add notches to them. d) I found examples on the model performance given in Figure 3 and 4 show some room for improvements. Especially in Figure 3 it seems as the threshold for snowmelt was calibrated incorrectly. Is this threshold predefined by the external snowmodel? And if so, doesn't this mean that the snow model itself needs to be updated and calibrated against discharge? And I wonder what the upper benchmark model would look like.

special comments/questions: Page 1 Line 1: Abstract: The first sentence is somehow isolated from the rest of the text. I recommend to delete this sentence P2 L1: and the erroneous precipitation input data at higher altitudes? P3 L 32 "rain input" : which precipitation data set drives the snow model? Also the RHiresD? P4 L1 ff: Is it correct that all model combinations HBV+M1-M3 as well as upper and lower benchmark models are calibrated? This is somehow suggested by Figure 5. In the calibration section I understood that a calibration was done for M3, upper and lower benchmark.

P5 L 2-3 what do you mean by “optimal interpolation approach”. What magnitude of summed corrections can be found? P5 L12: . . ... , but the RHiresD precipitation data set. Correct? P7 L12ff and Figure4: However, the differences between M1-M3 are rather small for the snowmelt season as also indicated by the differences in NSE P7 L27: I wonder if the differences of the LOO validation are significant given the relatively large spread. (see general comments) P7 L31 and Figure 5: - The benchmark lines are only the median of their respective boxplots? What is the spread of benchmark models? - The only difference between the benchmark model and M3 is a predefined DDF in M3 (cp.P5, L17-18)? Or are there further differences? If not, it is unexpected to see M3 to reach higher performance values then the upper benchmark. - Why is the performance of the benchmark model so weak in comparison to the other models especially in the lowest catchment class where snow does not really play a role? P8 L17: Please specify snow-rich: extreme snow years do not necessary result in an increased flood risks. To my understanding, largest snow melt contribution to runoff is expected if snow-covered area is largest and snow depth is widely insignificant (if SWE is above a certain minimum). P8 L30: in snow rich years the extent of snow in the lowlands is presumable larger then in snow-poor years. Accordingly, I also expected an effect of snow-rich years in the lowlands? Can you comment on this? P9 Conclusion: see superior comments Figure 1: The blue lines on black are nearly invisible. Please change colors. Figure 2: Instead of showing one specific year, I would rather see a mean snow melt sum. In addition, maps showing differences between the models would increase readability. Figure 3: Please indicate which model version is represented by the red dashed line. Figure 4: Please add upper benchmark model Table 1: Instead of numbers I would prefer to see the names of the catchments

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