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Hydrology and Earth System Sciences
Discussions

Interactive comment on "A conceptual, distributed snow redistribution model" *by* S. Frey and H. Holzmann

Anonymous Referee #2

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General comments: This paper deals with the redistribution of snow in order to remedy the weakness of many conceptual rainfall-runoff (RR) models that snow tends to accumulate over the seasons, generating "snow towers", perhaps glaciers? The authors have decided on a solution to the problem of redistributing the snow from high altitudes to lower- lying areas in order for it to melt there due to the higher temperatures at these lower elevations. The results show that the modelled snow reservoirs show a much more reasonable behaviour, do not accumulate over the seasons, and that runoff due to snow and glacier melt is much higher. No significant improvement using the new approach are seen, however, when comparing against MODIS derived snow covered area. This is explained by residuals of snow after redistribution due to vegetation. Overall, small improvements are found for runoff simulations.

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Interactive Discussion
Discussion Paper

The paper addresses an important problem, but I am not (yet) convinced that it presents the best solution to the problem. It is fairly obvious that transporting snow from elevations that have little melt to elevations with substantial melt will work, but addressing the problem of too much snow by just moving it appears as too simplistic. The “snow towers “ in RR models is a very common problem, and, at the catchment scale, the usual way to address this has been to look at the input: too heavy precipitation gradient with altitude and/or too negative temperature gradient. In addition, the spatial frequency distribution of snow may be very influential for the dynamics of the snow reservoir.

I think the authors need to review the problem (of snow towers/ accumulation of snow over several seasons) properly. This includes reviewing the different reasons for the problem and show how other authors have solved the problem. The solutions proposed in this paper should logically emerge as a potentially better choice than the reviewed approaches. Include this in the introduction

The issue of spatial scale is very important in a paper like this. The authors state that “no model for redistributing snow on a 1X1 km grid sixe exists” (p.611. l.16). There may be very good reasons for why it is so. Redistribution by wind is considered an important process for the spatial distribution of snow on rather modest spatial scales (up to some 100 meters ?). My feeling is that this is not yet a closed issue, but the authors need to discuss scales (quantitatively, not ”small” and “large”) and present a review on what is considered the important processes for the spatial distribution of snow at what scales. The Liston (2004) paper may serve as a starting point. After such a review I am not at all certain that the proposed method is a natural choice on a 1X1 km grid. “Scale” is often mentioned in the paper, but seldom quantified. Include this in the introduction.

The presented model is very parameter-rich. I believe I counted some 10 calibration parameters just in the snow module. With such possibilities for equifinality problems and compensating parameters, how do the parameter uncertainty influence the validation of the method? Discuss this.

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I think it is strange that there were no differences in simulated snow covered area (SCA) by the two models, and that model A did not compare better with the MODIS scenes. If you remove a lot of snow then you would expect areas to become snowfree earlier (even though some snow is, initially, retained by vegetation).

Language: The paper needs the attention of an English-speaking person. Section 6.3 is especially poorly formulated, but poor formulations are found throughout the text. Do not forget the figure captions!

Detailed comments. p.611, l 5. It says humidity, you mean turbulent fluxes?

p.611, l 22. High variability especially on high-resolution scales, less variability on small scales (see Melvold and Skaugen, 2013, Annals of Glaciology)

p.611, l 24. Quantify the scales in section 2

p.612, l 16. Shading and long wave radiation are not opposite entities? You have long wave radiation as long as you have a temperature above zero (Kelvin)

p.613, l 9...this study. . ., yours or That of Kling et al.

p.613, l 13. Sub-grid, what scale (quantified) is that?

p.613, l 16. The five classes are not clear, neither from the text nor from the figure. In addition what is the size of the cell?

p.615, l 16. Where is the “settling constant” defined?

p.616, l 12. Snow pack instead of snow cover. The less dense snow pack, the higher the portion available for redistribution.. . .. . .

p.615, l 15. Is not 0.45 extremely dense snow?. Perhaps 0.3 or so is better? What does literature say?

p.617, l 1. S_SWE_A

p.618, l 4. ..wind speed or -direction

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p.618, l 23. The figure has mm, not m3/s

p.619, l 18. Better perceptibility?, rephrase

p.6120, l 3-5. rephrase

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 609, 2015.