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Hydrology and Earth System Sciences Discussions

Interactive comment on "A conceptual glacio-hydrological model for high mountainous catchments" *by* B. Schaefli et al.

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The paper presents a glacio-hydrological model and its application to various basins in Switzerland. The paper is very well-written, well structured and easy to read. Especially the calibration procedure seems to have been conducted with much care. I strongly suggest publication, but suggest the authors consider the following comments.

GENERAL COMMENTS

Page 80: threshold temperature

The authors set the threshold temperature to zero and argue against its inclusion in the calibration process. I understand the point that glaciers are special in the sense that overprediction of snowfall can be compensated for by overprediction of melt due to the open-system character. However, this is also true for any other model parame-

ters relating to melt and accumulation, but the authors do not hesitate to calibrate the degree-day factors. Basically, if the assumed 0 degree threshold is “wrong” then the model compensates for this error by tuning the degree-day factors accordingly and the problem the authors want to avoid remains. The only way to get around this problem is to include mass balance measurements and not only runoff data in the calibration process. In addition, if a constant threshold is used, I suggest the authors use a more realistic one than 0 degrees. It is generally known that snow fall tends to occur even at temperatures slightly higher than 0; often models assume a value between 1 and 2 degrees. There is even a detailed study investigating the threshold temperature using all data available in Switzerland (Rohrer and Braun, 1994, Nordic Hydrology, 25). I suggest the authors consult this publication and at least use a more realistic value if they do not include the threshold temperature as calibration parameter.

Page 80, last paragraph of 2.3

This paragraph is a bit confusing and difficult to grasp. I suggest reformulation. Talking about “ice-covered spatial units” one could misinterpret this as “glacier units”, but I guess the authors mean only the units where bare ice is exposed at the surface. In addition the authors totally ignore firn or mess up terms by assuming firn=snow. How do the authors treat firn, i.e. the areas of the glacier where after the melt of the winter’s snow cover firn (=snow that has survived one summer) is at the surface and not ice, basically the area above the long-term equilibrium line or long-term accumulation area. I assume that in these areas the degree-day factor for snow is used ? How is the firn area delineated in the model ?

Page 80, Ice-covered area 2.4 and equation 4

The authors consider 2 linear reservoirs and argue that a third reservoir did not improve the modelling results. However, the authors do not state how they treat the area above the firnline. This basically follows from ignoring it in the melt formulation as outlined above. Which k-value is used for melt produced in the firn area after the snow has

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melted ? The firn area has a strong retardation effect on the water flow through the glacier and maybe a third reservoir (as e.g. used in Hock and Noetzli, 1997, Annals of Glaciology and further shown in Jansson et al, 2003, J. Hydrology) did not improve results here, but the authors may consider to discuss if this a consequence of their treatment of firn (which is unknown to the reader). Especially when using the model for climate scenarios, when under warming conditions the firn area decreases in size the model may fail to consider this change in hydrological conditions from relatively slow average throughflow velocities to very large velocities when the firn has disappeared. In any case the authors need to clearly state how they have treated the firn area in melt modelling and their linear reservoir approach.

DETAILS

Page 88, line 17: Add "snow": "because of higher snow albedo"; otherwise it is unclear what it refers to.

Page 92, line7: "on to" ???

Page 92, line 26: replace "winter" by "mass balance year", because it is the net balance it refers to.

Page 93, line 1-2: I have a hard time believing this argument. The mass balance values given in Table 8 refer to the water equivalent over the entire glacier area. Even if the glacier retreated several tens of meters (more is unrealistic from one year to another) and the model would produce melt in this marginal area while the ice in reality is gone, this would never explain the enormous error in specific mass balance. How does the mass balance - elevation curve look like for that year (as in Figure 7) ? Does it only deviate in the lowest elevation band ?

Tables: I would suggest to combine some of the tables, e.g. 1+2 or 2+4.

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