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Discussions
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Interactive comment on "Should altitudinal gradients of temperature and precipitation inputs be inferred from key parameters in snow-hydrological models?" *by* Denis Ruelland

Anonymous Referee #2

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The article analyzes the sensitivity of a snow accounting procedure and hydrological modeling results to the evaluation of temperature and precipitation in space and time in mountainous catchments. The study is based on a set of 20 catchments in the French Alps and two hydrological models. The author evaluates the interplay between the lapse rate, snow routine and hydrological model parameters.

I found this is a clear and interesting paper. I have a few suggestions for improvement detailed below, some of which are quite major and requiring new calculations. I suggest considering the paper for possible publication in HESS after major revision.

Detailed comments

Printer-friendly version

Discussion paper

1. I found that the literature review could have been more exhaustive, to better stress the originality of the work compared to existing studies on similar or close topics. Some recent works could be discussed, for example the work by Le Moine et al. (2015) on the link between snow and hydrological sub-models in model parameterization, some studies on using snow data to calibrate hydrological models (Besic et al. 2014, Henn et al. 2016, Riboust et al. 2019), some studies with physical approaches to estimate lapse rates (Rahman et al. 2014, Zhang et al. 2015, Naseer et al. 2019). The review could also be extended on how gauge undercatch factors are estimated. The author should further discuss to which extent the proposed approach is original compared to these past findings.

2. Section 2.1: It would be useful to add a figure showing the distributions of mean precipitation and temperature over the set of gauges, to give an idea of the variability across the study domain.

3. Section 2.2: Reference could be given to the work by Leleu et al. (2014).

4. Table 1: Please explain the meaning of abbreviations in the last column. Is this information useful here?

5. Section 3.3: The author calculates the efficiency criteria on precipitation values. However, the criteria may be strongly influenced by a few large rainfall events, which may not be representative of the average characteristics of precipitations. It may be useful to consider computing the efficiency criteria on transformed precipitation (e.g. root square transformation) to avoid putting too much weight on outlier values. Would this change something in results?

6. L261: The name “RMSE” given to the normalized RMSE is a bit confusing. The author may choose another name, e.g. NRMSE.

7. Section 4.1: Some modifications in this snow module were recently proposed by Riboust et al. (2019), to account for snow-covered area. This should be shortly com-

mented, to better explain how the proposed approach compares to this existing work.

8. Fig.3: Maybe add the meaning of the key variables (at least inputs/output) in the figure caption. If UZL is the threshold for the upper output, maybe the arrow should stop at the level of this output.

9. L376-378: This is a point I did not understand in the proposed methodology. By introducing this criterion WB in the objective function, the author forces the model to close the water balance in the sense of Budyko. This is quite successful when looking at results shown in Fig. 6, since no data lies outside the boundaries of balance closure in the plot. However, I do not understand the physical rationale behind putting this constraint. There are many catchments where the water balance cannot be closed in the Budyko sense for good reasons, mainly because of underground water exchanges. The author artificially constrains the models using WB. I think a more classical bias criterion would be better to consider instead.

10. Table 4: There is a strong drop in the NSE criterion for temperature when going from monthly to daily time steps for IDW and ORK. How this drop can be explained?

11. L472-476: I think this result is the consequence of using WB in the objective function. As mentioned above, this constraint is artificial and potentially counterproductive for the efficiency of the model.

12. L510-516: I find this a bit contradictory with the WB constraint. If the author makes the hypothesis that underground water exchanges between catchments may play a key role, why does the author constrain water balance not to account for such exchanges in the optimization phase?

13. Fig. 8 is interesting. However there are some cases which reveal that the optimum is probably outside the preset parameter range. This is typically the case for Test#1 for parameters X1 to X3. Therefore the ranges should be extended.

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