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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Data analysis and model building for understanding catchment processes: the case study of the Thur catchment" *by* M. Dal Molin et al.

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Dal Molin et al. investigated, through regression analysis, which indices have explanatory power for streamflow response. Based on the insights gained from the regression analysis, different spatial configurations were implemented in a hydrological model. Although I find the work flow elegant, starting from process-understanding and translating that to the spatial configuration of the model, I have some problems / concerns with the regression set-up.

Major

My main concerns are all related to the regression-part of the study.

1. It is unclear how the indices, on which regression was applied, were selected. There is plenty of literature around on indices and signatures, which could guide indices-selection, but I don't see any justification in the text for the choices made. Check for instance:

Addor et al., A Ranking of Hydrological Signatures Based on Their Predictability in Space, WRR, 2018

Knoben et al., A Quantitative Hydrological Climate Classification Evaluated With Independent Streamflow Data, WRR, 2018

2. Since the choice of the indices is not well justified, I am worried about their mutual correlation. Many indices can describe the same signal. Therefore, please provide the correlation among the indices themselves. This might lead to the insight that you need fewer or different ones.

3. It was rightly mentioned that correlation does not mean causality. It was claimed (not only in the methods, but also in the conclusions) that this study accounts for that by only selecting the indices that have a causal relation, based on expert judgement. I do, however, not recognize the expert judgement in the selection of significant indices, and this actually directly relates to my point 2. Right now, the selection seems to be made based on the mutual correlation of the indices – so the mutual correlation was investigated! – but I don't see any process-reasoning (the expert-judgement) that can justify the selected indices, and that justifies the claim that there is really causality.

4. I disagree with the conclusion of the authors that there is no need to look for non-linearity in the correlation, based on the results in the table. The authors rightly state that only few correlations that are statistically significant based on Spearman are not significant with Pearson, but how do the authors explain the opposite effect? Quite some correlations are significant with Pearson but not with Spearman, is this a Type

2 error in the Pearson test? That could have consequences, for example, aridity was significantly correlated with BFI for Pearson, but not Spearman, and based on 'expert judgement' included in the regression.

5. 1 of the 3 points of the guidelines for modelling based on the regression was not based on the regression at all, namely the conclusion that the presence of snow is relevant. Please include a snow-related indicator in the regression to support this conclusion (based on expert judgement we can expect this, of course).

6. It depends a bit on the definition of model building, but the title and the text might give the impression that the model structure itself was adapted with the insights in the regression, while it was basically the model implementation (accounting for HRU's or not) that was adapted.

Minor

Section 3.2.1, the catchments are sort of grouped based on their stream flow response, but this is not used any further in the analysis. Consider to just briefly describe their response, or to use the grouping later to explain results (in that case, also display the groups in the figure).

In the same section, it seems unnecessarily complicated to use combinations of signatures to determine how flashy catchments are; a flow duration curve can generally provide quite some insight on this already (slope of flow duration curve also frequently used signature)

Provide an overview of the indices and their abbreviations, or include their full name more often in text / tables / figures, because now it requires quite some work from the reader to fully understand all sentences and figures (and a lot of going back to the methods).

A large number of figures is dedicated to showing the signature-values, which is not of direct relevance. I would be interested to see a figure that displays the HRU's.

For the landscape characteristics, it is not mentioned in the methods-section (3.1.1) that you consider fractional area. Please clarify there, as I was wondering how you would apply regression on nominal values, until I found out in the results that you considered frac. area.

The sentence 'optimizing the parameter of the posterior distribution' (l.11, p13) can give the impression that you minimized e.g. variance (describing distribution), please consider reformulation.

Although overall written well and clear, some language editing seems required, for example "The average value oscillates of about ..", (but I'm not a native either).

Overall, I appreciate the intent of the study and the modelling-part seems well designed (except for my question at point 6 which remained unclear), but I do believe the regression-part requires substantial revision, related to the selection of indices (more embedded in literature and account for snow) and to justify the use of the word 'causality'. Given that the work-flow is largely set-up, I think the authors should be able to incorporate this.

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