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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.

Anonymous Referee #1

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The authors propose to infer the structure of a hydrological model based on landscape and process characteristics (signatures) of the catchment. In the first of a two-stage process different landscape and catchment characteristics are compared to different streamflow signatures to identify the most important controls on runoff formation. In the second step this information is used "as an inspiration for model structure design" (p17, l. 32) as the authors put it.

Inferring structure from function (or vice versa) is at the core of hydrological model building and subject to numerous studies. The topic is hence highly relevant for the hydrological community. The manuscript is well structured and well-written. Accordingly

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the manuscript is suitable for a publication on HESS. However, I cannot recommend publishing to current version of the manuscript due to several major points:

1) The purpose of the modelling exercise is not clear. Model requirements for flood forecasting are e.g. totally different from model requirements to simulate climate change. The relevant signatures, temporal and spatial model discretisation, model evaluation metrics and also the degree of model conceptualisation differ accordingly. Please specify more clearly the purpose of you modelling study. Otherwise it is not possible to evaluate the study meaningfully.

2) I consider the selection, evaluation and identification of landscape characteristics as fairly weak due to a number of different reasons:

a. The authors provide no information about why certain characteristics were selected (and why others were not). Catchment characteristics (or signatures) can only provide information on the underlying processes if they have some kind of diagnostic potential or causal relationship. It is clear that these relationships are often unknown and difficult to obtain; nevertheless the selection of appropriate characteristics is vital for the identification of underlying processes and mechanisms. I miss a clear and elaborate description on the selection of catchment descriptors and on their expected diagnostic potential (both in space and time): E.g. why or how can the different land cover ratios or aspects help to derive information on hydrological processes? Are the same characteristics suitable for all catchments (independent of size, altitude, geology)? Please also comment on the importance of the time step e.g. you calculated the flashiness index based on daily streamflow data, although you state that streamflow can change two orders of magnitude in a few hours (p. 3 l. 18). If this is true please explain why you consider a daily-data based flashiness index as a meaningful variable? Please do also explain why you think that “half streamflow period” is a suitable parameter to discriminate to importance of snow. I expect that there are much simpler and more meaningful variables such as temperature and rain, temperature sums or snow data itself to describe the importance of snow. The results in Fig 7-9 also show that streamflow, runoff coefficient and half streamflow period are pretty identically in all cases. Do you consider them being suitable signatures? Please also provide more signature papers in the introduction as the number of up to date references is small.

b. In your study you included several (fairly easy to derive) landscape characteristics that are obviously highly correlated and describe in great detail how you identify and select appropriate ones based on regression and correlation. In my opinion a rather trivial part which does not add any new knowledge to the literature occupies a lot of space. I hence suggest shortening and streamlining the entire section. If you want to derive structure from function than the first goal must be to derive a (comprehensive) matrix of uncorrelated catchment characteristics that have some kind of diagnostic potential. In my opinion this should be the source of the story and not a result.

3) The approach for informing model structure does not appear very elaborate to me. Since this is the core of "model building for understanding catchment process" I particularly miss a clear and elaborate discussion on how the identified landscape characteristics help in the model building process. More specifically:

a. In chapter 3.1.3. you state that the results of the regression analysis were used to build the hydrological model e.g. the subdivision of the catchment in HRUs (p. 7 l. 32). Later, in 4.1.1 you state that subdivisions were defined by gauge locations (p. 11 l 26). I did not find information on how you derived the number of HRUs and the role of catchment characteristics in this context? Chapter 4.1.1. should be more comprehensive in this regard.

b. The argument that "the regression analyses have indicated that precipitation is a dominant control on average streamflow" (p. 12 l. 4) is trivial. I don't think you need this and particular not as a justification for using spatially distribution rainfall as a model input. From your manuscript it appears to me that the spatial discretization of your model was based on the definition of the subcatchments (which are in turn defined by the location of gauges) and according the definition of fields (definition not clear). In consequence I don’t see that landscape characteristics played an important role in this process. Please clarify?

c. You also mention that “the parameters were motivated by the results of the regression analysis” (p. 8 l. 1). Please omit or explain more detailed. A matrix to illustrate the relationships between model parameters and catchment descriptors would be good. I would for instance be interested in how one could use catchment descriptors to derive (or at least constrain) model storage (kFR or kSR) or network lag (trise,IL trise,OL) parameters. Please comment on that

d. Chapter 4.1.5 is difficult to me due to different reasons: i) your analysis does not VERIFY that “models that account for influencing factors . . . lead to an improved representation”. Essentially it only shows that a complicated model (with a larger number of degrees of freedom) outperforms a simpler model (with a smaller amount of degrees of freedom). Please use precise wording. It addresses the question of adequate model complexity. If a lumped representation (M1) is not adequate than also the comparison of M2 to M1 is not adequate. Please explain in more detail why you consider M1 a suitable reference? ii) Please explain why unconsolidated areas receive an individual HRU and why consolidated and alluvial areas can be lumped together (what are your expectation on the underlying processes)? iii) The parameterization of M3 is based on land use, which is not considered to have a causal relationship to the streamflow signatures (Table 2). Please explain why a model which is derived from non-causal properties can be a considered a meaningful reference? Why did you group based on geology and not on elevation, slope or the aridity index which you considered to have a causal relationship? This would maybe be a more appropriate benchmark? iv) Essentially chapter 4.1.5 addresses the questions of optimal degree of model complexity and optimal degree of spatial discretization - which are both very important. However, these aspects are treated together and not separated from each other. Moreover, potential answers to these questions miss a clear link to catchment descriptors. Essentially only differences in geology were considered in the model building. Please clarify to novelties

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of your study more clearly.

e. the wohle structure of the model building story is a bit complicated as aspects are described in chapters 3.1.3, 3.3, 4.1.4 und 4.1.5 which makes it difficult to follow. I suggest combining them into a single chapter. Therein start with the theory e.g. snow is important followed by the surrogate you considered it e.g. half stream flow period. Or geology is important due to. . . –> different HRUs.

4) Several conclusions are not appropriate: e.g. “the proposed approach is useful in the fact that modelling can be used to test specific hypotheses on dominant processes resulting from regression analysis” (p. 19 l. 4). This has not be shown. More over aspects related to the event scale are mentioned in the first three bullet points but not subject to the manuscript. In the third bullet point you state: “Higher proportion of consolidated material has an influence on the baseflow vs quickflow portioning, causing lower baseflow and higher peaks” (p 19 l 14). Does the study provide evidence for this statement or does it support this hypothesis? I expect the latter and missed this statement in the chapter 3.1.1. I suggest re-writing of the entire section and to differentiate concisely between hypothesis, results and conclusions.

5) The model performance evaluation (chapter 4.1.4) is complicated but of minor importance in this context. I suggest shorting the evaluation section and to focus on a single, interpretable metric e.g. the Kling-Gupta Efficiency as the NASH has several limitations and the normalized log-likelihood is difficult to interpret. But this is a minor point and a matter of taste.

Technical corrections (figures and tables only) I only provide technical corrections for the figures and tables as I expect that several parts of the manuscript will be subject to major revisions.

Figure 1: A: I suggest to remove the colour code and to provide notations (abbreviation) in or around the map. This would help improve the readability of the stream network and the location of the gauges. If you want to keep the legend please add catchment

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abbreviations to it, order it according to Fig 2. and use a meaningful colour code (e.g. mean annual precipitation, elevation or geology), B: Try a discrete legend like in atlases, will improve readability. C: Forest and pasture are hardly distinguishable both on my screen and in a printed version.

Figure 2: Please repeat the variables and their abbreviation in the caption such that the figure can be read independent from the text. Maybe add another row and provide grouping indices based on the results in chapter 3.2.1

Figure 3: Please repeat the variables and their abbreviation in the caption such that the figure can be read independent from the text.

Figure 4: Please repeat the variables and their abbreviation in the caption such that the figure can be read independent from the text. Information on the range of the different variables would be pretty helpful as well. If possible include it otherwise please mention the ranges in the text or add the information to table 1.

Figure 5: I'm not sure if this figure is required since B and C show very little variation. The only important message from A is that there a catchments that are stronger controlled by snow than others. I suggest removing it. If you decide to keep it update the colour code according to the suggestion for Figure 1.

Figure 6: I cannot find a description of the symbols and abbreviations in the Appendix. Please specify at least the meaning of the capital letters in the caption (as in 4.1.1) and provide a more comprehensive description in the appendix.

Figure 7: Order according to Figure 2 or 3. Line type and colour code are redundant.

Figure 8, 9, 10: Nice figures! Suggestions: Combine all three figures in one (each model setup as an individual row). This would improve readability. Streamflow, runoff coefficient and half streamflow period have no or little variation (two out of these could be omitted such that all results would fit in one figure). Remove the correlation coefficients due to their distracting nature (correlation (alone) is pretty meaningless in this context). Update colour code according to Fig 1 A.

Table 1: Order according to fig 1 A. Index column is not relevant, omit Code or put it to the very right. Rounding is not yet meaningfully and consistent.

Table 2: This table includes variables with spurious correlations (Brett 2004, Kenny 1982). This includes variables that are considered statistically significant and where causality was assumed e.g. the correlation between aridity index AI and the runoff coefficient RC which are both are derived from precipitation. The same applies for P and RC. Since P and Q are highly correlated and AI is based on P I also wonder about the significance of AI and RC, BFI, FI and HDP. Please clarify. Please also explain why you assumed causality among LP and BFI and among LP and FI? Differences among the geological fractions are small as well. Why do you consider causality in some of the individual relationships and in others not?

Table 3: This analysis also includes variables with spurious correlations. Please comment on that.

Table A1: Please provide a brief explanation on parameters and components. Where does the range of variability come from?

Table A2: Explain component

Literature Brett, M. T. (2004). When is a correlation between non-independent variables “spurious”? Oikos, 105(3), 647–656. Kenney, B. C. (1982). Beware of spurious self-correlations! Water Resources Research, 18(4), 1041–1048. https://doi.org/10.1029/WR018i004p01041

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