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Hydrology and Earth System Sciences
Discussions

# *Interactive comment on* "How can expert knowledge increase the realism of conceptual hydrological models? A case study in the Swiss Pre-Alps" *by* Manuel Antonetti and Massimiliano Zappa

**Anonymous Referee #1**

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The manuscript discusses the role of expert knowledge in hydrological modelling, contrasts several perspectives described as "modeller" and "experimentalist", and provides a simple case study illustrating the discussion.

I generally enjoyed reading the manuscript, it is written in an easy-to-follow style, and does not oversell its results. The case study is presented in a fairly succinct way. Overall I believe the authors arguments are valid and of interest to the community, even if they are not necessarily new. However there are several important instances with questionable gaps in the logic and some critical parts of the method description



seem to be missing.

An immediate question I had when reading the manuscript is that the term "expert knowledge" does not appear to be defined / explained. Should it be contrasted to some some other kind of "general" knowledge (or "alternative" knowledge?). As a result it is unclear what type of information/data do the authors consider to be "expert knowledge", and which they do not, and the discussion that follows then becomes a bit too vague. For example, is the classification into top-down vs bottom-up considered expert knowledge? I would have argued this would nowdays represent "general" knowledge rather than "expert" knowledge, and so forth.

The case study description should also more clearly categorise and describe the types of "expert knowledge" used. Some of the expert knowledge seems to be just a particular data set (which is in principle accessible to "non-expert" modellers also). Perhaps I missed something here, in which case a clarification in the manuscript is still needed to prevent such confusion for other readers.

The manuscript makes an overly general statement is when suggesting "modellers, or 'dry' hydrologists" tend to develop theories at the catchment scale. Surely this would not apply to hydrologists that use "physically-based" models, which are classic bottom-up approaches. And the contrast to "experimentalists, or 'wet' hydrologists" is itself questionable - surely some of them work at the small scale and others extrapolate to the larger scale. Some additional supporting references here are needed. Or maybe clarify that "modellers" here mean "conceptual" modellers (?)

The ANOVA analysis used to estimate dominant source of uncertainty is a worthwhile undertaking, but the description here lacks essential detail. For example, how are the individual terms in equation 6 calculated? Clearly some assumptions need to be made here, eg, about the errors in the rainfall inputs (eg, Renard et al WRR2011), about the errors in the maps, and so on - how are the assumptions made and how are they checked in this study? Otherwise the conclusions of this analysis would not really be

justified.

Pages 12 and 16: "Model realism" ... Of course every scientist and engineer would want their model to be "realistic", and there have been many opinion papers on this in hydrology advocating improving the "realism", whatever that means beyond ultimately matching some performance criteria. In this manuscript this issue is raised on Page 12, but then after a fairly basic modelling attempt produced results deemed "unrealistic" (page 13, lines 10-20), any improvement was deemed "beyond the scope". So I am not sure the manuscript and its case study in their current form can convincingly call on other modellers to achieve such "realism".

Other

1. Page 2 Line 5: What does the "It" refer to? "Expert knowledge"? Or "linking observations and laws"? The latter would make far more sense, but is not apparent from the way the sentence is written.

2. Page 2 Line 24 "Wet and dry" - this usage here is confusing and unnecessary. Taken literally "dry hydrologists" might be mistaken for hydrologists working in dry (arid/semi-arid) catchments. I think it is fun to refer to experimentalists and modellers as wet and dry, but I think once the point is made to continue using these terms is not necessary and can lead to confusion.

3. Page 3 line 19 "too simplistic" - I think this requires some statement of the purpose - too simplistic to achieve what?

4. Pages 3-4: Fenicia et al WRR2016 is an example where bottom-up ("distributed" and scaled up) and "top-down" (conceptual) approaches (as per definitions given earlier) are combined, applied and several hypotheses about process representations and hydrological controls were tested.

5. Page 5 line 25: This text correctly refers to the distinction between top-down and bottom-up approaches not being sharp - this aspect was overlooked in the earlier



pages when discussing these two approaches, which lead to some too-strong statements being made.

6. Page 6 Line 25: "parameter allocation" sounds a bit awkward? Wouldn't "estimation" be a better choice here, covering all options (though measurement, calibration, etc)?

7. Page 6 line 31: give a reference for this "so-called hydrological approach", and explain how it is used in this work

8. Page 7 line 22: "storage parameter" might read better here than "storage constant"?

9. Eqn 1: numbering each of these 2 equations individually would be helpful

10. Page 8 line 20: what is a "modelling chain combination"? please briefly define/explain what this means

11. Page 9 eqn 5 - clarify that "KGE applied to runoff time series" or similar, for clarity

12. Page 9 line 18-23: Please define these P-factors and R-factors by equations, at least to the same standard as the KGE in eqn 1. Then all metrics become properly documented and easily reproducible without guessing how exactly they are calculated.

13. Section 4.5 It is good that the study is noting its limitations. I would also suggest the overall calibration approach used here is quite simplistic - it is quite possible a more detailed application would've produced quite different results. Worth also noting the use of a single case study makes it impossible to know if similar conclusions would hold elsewhere. This does not invalidate the results, just should be noted as one of the limitations.

14. Section 5 Conclusions - to make sense of this, need to list the actual "expert knowledge" used here. This relates to a major concerned raised earlier that this concept is not sufficiently clearly delineated here.

15. Fig 1 - I found this figure rather confusing - somehow physically based models became closer to experimentalists approaches than conceptual models? I would not

have said that, these kinds of models are certainly not developed by "field" "wet" hydrologists ... I think they do not fit within this "planar" dichotomous figure, they almost form another dimension of their own.

16. Fig 4 - worth providing references to several earlier studies where such "distributed model structures" were used.

17. Fig 6 - what are the benchmark options for the process maps, parameterizations, and parameter values?

18. Figure 7 - not sure I am seeing in the figure how you arrived at the last sentence in the caption.

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