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Discussions

Interactive comment on "Reducing structural uncertainty in conceptual hydrological modeling in the semi-arid Andes" *by* P. Hublart et al.

Anonymous Referee #2

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The article addresses the interesting issue of structural uncertainty in conceptual hydrological modelling. The authors test a large number of alternative structures on a catchment in the Andes in Chile and discuss their relative merits in a multi-objective framework.

Overall, I found the article interesting and well written. I think it could make a valuable contribution to HESS provided that a number of points are improved. I have two main concerns. First, the conclusions of this study do not appear so novel compared to existing works based either on multi-hypotheses or multi-objective frameworks. I think the authors should strengthen the last part (discussion/conclusion) of their paper to better demonstrate what was learnt from the quite complex testing scheme they set up and what is new compared to what was already shown in past studies. Second,

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their study would give more general conclusions if tests had been made on more than one catchment. Indeed, the conclusions may strongly depend on the characteristics of the selected catchment. It would be useful to test the approach to at least another catchment, to check whether similar conclusions are reached.

I have also a number of detailed comments below. I think the paper could be reconsidered for publication after major revision.

Detailed comments:

1. There are remaining typos that should be corrected. Consistency between references in the text and the list of references at the end of the manuscript should also be further checked.

2. p.12139,l.25: The authors may find interesting reflections on this issue in the book edited by Wainwright and Mulligan (2004).

3. p.12142,l.1-10: I do not agree that the multi-model approach was mainly focused on small catchments. There are a number of studies in the literature that investigated larger ranges of catchment size. Besides, what makes the application of such approaches to larger catchments essentially different given the lumped approach used? I found that the argument of scale to explain the novelty of the study not really convincing here.

4. Section 2: As explained above, I found that adding another case study (possibly under similar or different conditions) would make conclusions more general. Here the catchment is quite specific in the sense that there seems to be a huge uncertainty in precipitation estimates. Adding another catchment with better known precipitation would provide a comparative reference to balance the results presented here.

5. p.12143,l.26: The location of gauges could be shown in Fig.1.

6. p.12144,l.17-22: I did not understand why the Oudin's PE formula was adjusted to the Penman-Montheith's one. Why not directly using the latter if it is found more

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adapted to the study site?

7. p.12144,l.22-25: This statement is a bit vague. Could the authors give more details on this and explain to which extent the naturalization process may introduce uncertainty in the evaluation of models?

8. p.12146,l.10-15: Is not there any seasonality in these processes?

9. p.12146,l.22: Do the authors mean that the geological boundaries may be different from the topographic ones?

10. p.12151,l.25: Do the authors wish to refer to section 2.3.1 instead?

11. p.12152,l.21: It is unclear how the SCA was modelled given the lumped approach followed here.

12. p.12154,l.4-12: I found this choice questionable. Uncertainty bounds should refer to actual nominal values. For example, if one seeks to build 90% confidence intervals, then one should expect that the uncertainty bands contain 90% of the observations, not the maximum of observations. Does it mean here that the authors wish to build 100% confidence intervals? If one wishes to use other confidence intervals, how the approach should be applied? I understand that the authors rightly distinguish reliability and sharpness as two expected qualities of the uncertainty estimates, but there are many criteria proposed in the literature to evaluate these qualities. Maybe the authors should use the commonly applied criteria to strengthen the evaluation of uncertainty bounds.

13. p.12156,l.14-16: It is a bit difficult to see at first glance the structural differences between these three models. The reader has to reconstruct the structures from table 4 and figure 2. Could the authors help the reader here by detailing these differences?

14. p.12162,l.5-6: Was this actually demonstrated here, given there remain similarly performing structures? Besides, I think the usefulness of multi-model frameworks was already demonstrated by past studies. So maybe this should be seen more like a

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confirmation of existing results.

15. p.12162,l.16-22: Can 9-parameter models be considered as parsimonious? The difference between 9 and 13 parameters is not so large, since many modellers may consider 9-parameter models already overparameterized. Maybe this discussion could further refer to past works discussing parsimony in conceptual modelling.

16. p.12164,l.1: Would groundwater data be actually helpful in the case of this catchment, given the large uncertainties in precipitation estimates?

17. Table 1: I do not understand the first equation for snow, which seems larger than P. Maybe remind option type in the table.

18. Table 2: Where does the range for Kc come from? The ranges given for K3 seem dependent on the option but are the same in the table.

Cited reference

Wainwright, J. and M. Mulligan, Eds. (2004). Environmental modelling - Finding simplicity in complexity. Chichester, John Wiley & Sons, Ltd.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 12137, 2014.