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Hydrology and Earth System Sciences Discussions

Interactive comment on "Fuzzy committees of specialised rainfall-runoff models: further enhancements" *by* N. Kayastha et al.

Anonymous Referee #2

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1. General comments

The manuscript presents further developments and an application of the so-called 'fuzzy committee models' approach on three catchments using the HBV hydrological model. Compared to previous studies on similar issue, the originality of the paper lies in the comparative assessment in verification mode of the committee models with multi-objective solutions given by the NSGAII algorithm. The issue is clearly relevant, the methods are original and the objectives of the study clearly stated. Still, there are many methodological details that are missing and to my opinion, the results are not deeply analysed as they could be.

2. Specific comments

I have three main comments on the manuscripts that shall be discussed by the authors

2.1. It does not appear clear to me what scheme is preconized at the end of the paper. A number of weighting and membership functions are tested. Some are prescribed (the weighting function parameters), other are optimized (the membership function parameters). These tests are clearly interesting to study the sensitivity of the approach to these functions but there is a lack of methodological guidelines for further studies. For instance, it is not clear at what stage the membership function parameters are optimized (and how are they optimized?). Are they calibrated after the calibration of the specialised models? On the calibration period? These elements are missing or not clearly stated and the methodological choices could be better justified. A related issue is the possible interactions between the parameters of the membership function and the parameter of the weighting function, a point that is not discussed at all.

2.2. I am concerned by the robustness of the conclusions and the developments of the approach since only three catchments are tested in the paper (and the results on the Alzette catchment are not fully analyzed), with very mixed results. Besides, I wonder if the record periods are long enough to draw robust conclusions, given the number of free parameters of the hydrological model and the additional calibration of the membership function. This may lead to objective functions that are driven by only one flood event and thus potentially less robust inferred parameters. At the view of the surface responses on Figure 4, this probably occurred on the Bagmati catchment. To me, the main advantage of the committee model is to increase the flexibility of a given model structure without increasing its degree of freedom. This advantage is likely decreased in the proposed study by increasing the number of choices in the membership functions and the weighting functions. The few catchments studied reinforce this impression.

2.3. The results section is particularly short. Here are some points that could be discussed in more details (the list is clearly non exhaustive). One may expect a deeper analysis on the comparison of the committee models and the multi-objective calibration framework, e.g. is there a solution from the Pareto front that performed better than the

committee model in verification mode? Table 3 is not really discussed in the paper. It may be interesting to determine if there exists a generic solution for the three catchments concerning the parameters of the weighting and the membership functions. Is there any conceptual reason why these parameters shall differ from one catchment to another? The differences of the ACCO and GA algorithms are not discussed. Consequently, why using two algorithms? The best solutions in calibration of the weighting and membership functions do not provide the best solution in verification. How the authors interpret this?

3. Technical corrections

There are many typos in the text that should be corrected.

p.678,l.11-13 the sentence does not make sense.

Section 2.2 is not very clear from the first reading. The authors should state more clearly that only four configurations of parameters alpha and N are tested in the paper, and refer to Figure 1.

p.680 l.14 viva -> vice ???

Section 2.3: As in section 2.2, state clearly in that paragraph how and at which stage gamma, delta and N are optimized.

p.683 l.17-19: Please, state that this conclusion concerns the calibration mode, even if it is explained in the next paragraph.

p.683-684 l.24-. . .: This result is very interesting and I wonder why it appears only in conclusion and not in the results section.

p.684 l.7: I am not very clear if the committee models approach is beneficial or detrimental for hydrological simulation under Climate Change. Could the authors discuss a little more this topic?

Homogenize significant digits of Table 3 and Table 4.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 675, 2013.