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Hydrology and Earth System Sciences Discussions

Interactive comment on "Ensemble modelling of nitrogen fluxes: data fusion for a Swedish meso-scale catchment" *by* J.-F. Exbrayat et al.

Anonymous Referee #2

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GENERAL COMMENTS

It is a very interesting paper, well worked and clean presented. There are several works in hydrological literature trying to reduce simulation uncertainty by models combination (especially in real time flood forecasting) and few ones in quality modelling (as it has been well reviewed by authors). I recommend its publication, but some additional explanations should be done in the final version:

1.- The single models and the ensembles have been calibrated in the period Jan 2000 to Dec 2004. However, I don't see any type of model validation (temporal and/or spatial), which is important in general (see the details for example in the DMIP project, which is mentioned in the paper), but crucial when using models without physical mean-

ing. And the linear regressions between models are not. So, two important questions arise immediately:

- Why there is not a temporal validation period, splitting the 5 observed years into 3 years for calibration and 2 for validation?

- The results will be the same in a different year than calibration period?

2.- P5314 L15-19. From my point of view, this is the key part of the paper and I think more explanations are needed. For example:

- I don't fully understand why are you using monthly measurements and daily simulations for regressions

- I must assume the same types of regression are done for MMEs than SMEs, or do not I?

- What are the a priori implications of using constrained or unconstrained?

- Or more general, why are you using linear regressions for ensemble constructions? Remark any precedent in literature if any. Or in other words, justify this type of ensembling.

- Can you explain with more detail Table 2?

3.- P5311 L17. It is not clear if, for the paper case study, there is only hydrological calibration (and N parameters are obtained by Monte Carlo simulations) or there is a two step calibration (including N submodel calibration).

4.- I like very much the Discussion Section, but I find the conclusions are very short. Why not merge both in a single "Discussion and conclusions" section?

MINOR CORRECTIONS/SUGGESTIONS

Unfortunately different people understand differently the scale "mesoscale". Stress in the Introduction that in your case "mesoscale" is basin scale (I think!).

Models combination is more frequent in real time forecasting world. Authors cite Abrahart and See (2002), but there are more interesting works for a literature review in this topic. Just two examples (I have not published any related paper, by the way!):

- Regonda, S. K., B. Rajagopalan, M. Clark, and E. Zagona (2006), A multimodel ensemble forecast framework: Application to spring seasonal flows in the Gunnison River Basin, Water Resour. Res., 42, W09404, doi:10.1029/2005WR004653.

- Ajami, N. K., Q. Duan, and S. Sorooshian (2007), An integrated hydrologic Bayesian multimodel combination framework: Confronting input, parameter, and model structural uncertainty in hydrologic prediction, Water Resour. Res., 43, W01403, doi:10.1029/2005WR004745.

P5306 L25. There is an incompatibility between subcatchments division and HRUs and land uses. If I have understood well, the last are nested. Explain it here.

Table 1 caption. Better “Main model characteristics”

Table 1. Authors have described SWAT explaining it has three different runoff fluxes: surface runoff, lateral flow and baseflow. It will be interesting to have the same equivalent description for the other models, in this table 1 and within the text. In the text, I suggest also to underline the implications of different hydrological modelling in the N modelling.

Table 3 and 4. Are results during the calibration period? Mention it.

Figure 1. Names in the figure are confusing: there are two “Vattholma”, names have different colors without explanation, Uppsala is clearly a city, but the rest, are they cities, basins or stations?

Figure 3. It is not needed “METHODOLOGY” within the figure, because it is already in the caption.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 5299, 2010.