Hydrol. Earth Syst. Sci. Discuss., 7, C2936–C2939, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C2936/2010/

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 #1

Received and published: 14 October 2010

This paper is an interesting contribution in the field of biogeochemical modelling at the catchment scale. It is appealing to see attempts to bring some light into the issue beyond the Manichaean arguments about model capabilities, usually only a reflection of modelers' preferences (frequently acquired just by contingency). I liked a lot the starting point of the paper, because it explicitly accounts for one of the most basic (and sometimes forgotten) limitation of a mathematical model: it is just maths!!, including lots of assumptions that may or may not be appropriate. With no doubt, ensemble modelling is one of the best shortcuts to cope with model's incompleteness also in catchment biogeochemistry, as is the case in some other areas where modelling is the core methodology (e.g. climate prediction). However, there are some assumptions and reasoning in the paper that in my opinion deserve discussion, because they are

central for the conclusions reached. I'm particularly worried about the modelling target (nutrient load), because this is at odds with one of the basic objectives of the paper (to focus on N dynamics). Also, although the presentation is good in general terms, the explanation of some of the methodological aspects could be improved.

1. Why are you using nutrient loads as modelling target if your aim is "to focus the study on the stochastic uncertainty linked to the nitrogen algorithms only"? I think nutrient concentration is a best option because:

a. If streamflow and nutrient concentration are related, then is quite easy to fit the nitrogen modules against a corrected nitrogen concentration trace. I mean, if you have previously fit the streamflow modules, you can calculate the mismatch between modelled and observed streamflow. Then, you can correct your observed nutrient concentration by this mismatch considering the empirical relationship between nutrient concentration and streamflow. Fitting the model against this corrected nutrient concentration avoids biases in the nutrient parameters determined by poor fit to streamflow. b. If streamflow and nutrient concentration are not related at all, then you can assume that streamflow is not dramatically affecting your nutrient dynamics (or at least it is not the main driver). Then, work with raw nutrient concentrations.

I think this is quite important in a paper where different models showing different fits to streamflow are detected. I think in your case the importance of loads for management or other considerations are of secondary meaning, because you present here a pure technical issue.

2. I was a bit confused by the way you used the term "uncertainty" in the paper. In your work you only used ensemble modelling to improve prediction, but in many places you talk about "prediction uncertainty" and the like. Although in the last sentences of the paper you put clear what you missed, I think you must be cautious when talking about uncertainty in your paper, because your numerical experiment is related to uncertainty only in a theoretical sense. In my opinion, your results are only about prediction (fit).

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Please, put clear this in the text.

3. There is reasoning in the discussion that clearly violates your own statements about the limitations of your work. You stated that because model results themselves were not a target, individual model efficiency may not be maximized. However, in the discussion you used particular model results to raise some conclusions (page 5318, lines 20 to 24). In this paragraph you argue that since improved water description did not result in better nitrogen dynamics description when comparing LASCAM and LASCAM-S, then you concluded that improved water description does not necessarily provide better nutrient export prediction.

A part from being at odds with your own statements about your work limitations (if you acknowledge that the calibration of individual models may not be totally efficient then you must be cautious when comparing performance, as you wisely stated first. I do not understand why you did not follow your own wise recommendations later), in my opinion your reasoning is totally wrong. First, it is nonsense to say that improved streamflow description does not lead to improved nutrient flux calculation. If nothing else changes, by definition improved water routing description leads to better flux prediction. It is just maths. Second, in your case you are comparing two models that are identical but in some of the water routing routines. That is, the formulation of nutrient dynamics is the same. Then, if the model that performs better with hydrology is performing worse for the nutrients, this only means that the Monte Carlo scheme you applied to find the best parameterizations are not optimal. I mean, probably 40000 realizations were not enough to catch the global minimum for these two models. This poses in doubt your statements in lines 26-29. Then, you cannot compare model fit to raise those conclusions, because your models can be used as heuristic tools only in case of fit to an almost global minimum.

4. This leads me to a rather philosophical question. In the discussion, you seem to advocate in favour of applying models in an ensemble fashion without regard of the internal conceptualization inside the models (page 5320, lines 1 to 5). However, I wonder

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which the value of models is if not as heuristic tools (see papers by Naomi Oreskes). If you forget the conceptualizations, are not we loosing all the science behind? Do you mean that ensemble modelling is better that model design in terms of knowledge generation?

5. With no doubt, you need to explain better contents in section 2.3.2. I still do not understand why you have 55 MME predictions and not 75. The whole section is hard to follow.

I have some other minor comments:

ïĆğ In the Introduction, the scale of your work is not clearly stated. You start with broad statements about global biogeochemistry, but your work is about the catchment scale. ïĆğ I do not think you must work with loads, but if you want to defend this you must explain how did you calculate them (page 5306). ïĆğ Page 5316, line 8. Figure 4????

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