Hydrol. Earth Syst. Sci. Discuss., 11, C4897–C4903, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C4897/2014/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Developing a nutrient pollution model to assist policy makers by using a meso-scale Minimum Information Requirement (MIR) approach" *by* R. Adams et al.

Anonymous Referee #3

Received and published: 4 November 2014

The study aims at presenting the use of a modelling tool for meso-scale catchments dedicated to help management plan design for policy makers. The model is lumped in order to be as parsimonious as possible but it is also based on a conceptual understanding of the catchment, thus it is able to account for storm events contrary to classical simplified approaches based on export coefficients. The model includes the simulation of the dynamics of different nutrients: Nitrate and Phosphorus, and different time scales of variability: season and storm events. While Nitrogen is not used to test the implementation of mitigation scenarios because of the very long times associated with deep nitrates fluxes in catchment; it seems that these data could be better val-

orised as an additional constraint for the model. The model is applied on the Frome catchment, Dorset UK where several previous studies provided substantial data sets and knowledge about the catchment behaviour and features. The use of 2 kinds of data sets is interesting combining a long term series and a shorter high frequency series on the water quality. However the author should provide more information on how this combination helped in practice to define the model in the MIR approach or to improve its calibration or the assessment of its performance. This study contains a lot of interesting and original aspects, which are relevant within HESS scope but some discussion about the assumptions about geochemical processes have to be added, some more clarifications about the calibration procedure should be provided, and some additional perspectives with current tools for helping decision makers would be worth to discuss. Therefore I would recommend several modifications before publication. Some questions and suggestions are detailed below.

Criteria:

1.Relevance within HESS: YES

2.Novels concepts, ideas, tools or data: YES, the modelling tool is new, and its application to these data sets too

3.Substantial conclusion: further discussions would be worth (see below)

4.Validity and clarity of methods and assumptions: model calibration description could be improved (and better discussed), and additional discussions are required about the neglecting of geochemical processes

5.Sufficient results: I am convinced that “bad” results are as much important as “good” results in Science. Moreover the poor fits between absolute values of observations and simulations are not so surprising when trying to model stream concentrations. And nevertheless the results are good in term of modelled seasonality and reactivity to storm events therefore these aspects have to be better discussed by providing hypotheses

HESSD

11, C4897–C4903, 2014

about why the model fails when it does.

6.Sufficient description of calculations: could be improved, some parameters are not defined.

7.Clarity of the new/original contribution: YES

8.Title relevance: removing “developing” would be more suitable according to the content of the paper.

9.Abstract concise and complete: YES

10.Overall structure and clarity: YES excepting a minor suggestion (see below)

11.Fluency and precision of language: YES

12.Formulae, symbols, abbreviations and units: to improve, see technical comments

13.Should any parts be clarified /reduced/eliminated: NOTHING RELATED TO A SPECIFIC PART

14.Number and quality of references: YES

15.Supplementary material: NA

Specific comments

Abstract line 1: “water pathways” instead of “runoff pathways”?

Abstract line 5: “hydrological pathways that mobilise nutrients” are the same pathways that mobilise pesticides, soil particles, dissolved organic matter...

P 10367 lines 19-22 are these questions answered in the manuscript? What does “more transparent” mean? More transparent on the model assumption, on the model running process for non-modeller, on the uncertainty on the model outputs?

P 10367 line 24 “this model”: which one?

HESSD

11, C4897–C4903, 2014

P 10637 line 25 Are we sure that the presented model reflect the dominant processes at the end? The authors assume that main drivers are hydrological, this is actually their assumption and should be explicitly written. Soil and hillslope processes describe indeed dominant flow pathways in humid temperate climates: would these assumptions be the same in an arid Mediterranean catchment or in a North American snowmelt driven catchment or in specific geological conditions such as in karst system?

P 10368 line 6 “Aggregation and homogenisation” do not mean that there is no geochemical process. Once again here the authors make an assumption which is that at their scale of interest there is no effect, no control of the spatial variability of land use, and management practices on the nutrient fluxes. This hypothesis should be discussed at the end of the manuscript when interpreting the periods or scenarios the model fails to reproduce.

P 10368 lines 18-22 Looking at the meso-scale, the use of a hillslope model is quite unexpected while the riparian and in stream processes are expected to be taken into account as their relative contribution to Nutrient dynamics is known to increase with the order of the catchment. So far as I understand, the original assumption of the authors is that even at meso-scale catchment, hydrological flow paths are sufficient to represent both the seasonal and the event dynamics of nutrients. But it is not clearly explained and quite in contradiction with what the reader would expect at this point therefore it should be more explicit.

P 10369 line 6 this is a very relevant question but the limit of management interventions which can be designed using such tools have to be precisely defined. Regarding the assumption that the spatial variability is not required in the model the relationship between model outputs and practical management actions is not obvious.

P 10369 line 8 At this stage, we would expect a proper comparison of the MIR approach with and without taking into account geochemical transformations.

P 10369 line 26 please define DSS-based model

HESSD

11, C4897–C4903, 2014

P 10373 The use of a Minimal Information Approach is not clear in the choice of the model which seems to be already chosen at the beginning of the study. Some explanations about the conceptual choice would be nice, e.g. what is the link between WWTP and deep groundwater flow? In many cases, WWTP release the water directly into the hydrographic network and would be associated with rapid surface flows in this case. Is this conceptual model similar in practice with other models? It seems that the partition of Overland flow vs Base flow is similar to what is described in classical physically-based models such as Topmodel for instance, with a second step of partitioning within base flow a subsurface and a deep flow...

Eq. 1: QCSR is not defined, is it?

Figure 1 the conceptual scheme is not clear: e.g. it seems that nutrient outputs only come from DG.

P 10376 lines 4-7 What would be the implications of using the uptake factor if flow is not well simulated? Could this explain the need of a minimal concentration threshold?

Case study description: maybe this section should come before the model description so that it would be easier to understand some choices in the model structure.

P 10379 line 27 Table 2 is cited after table 1.

P 10380 lines 12-14 What the analysis suggests is not so obvious for the reader: some information about the mean duration of the flood would help to justify the choice of the daily time step.

P 10384 lines 3-6 How far have the hydrological parameter been modified? What is used for the depicted results? The use of Nitrate as nutrient associated to deep flow paths and Phosphorus, in particular PP associated rather to surface flow paths could be improved in order to constraint the model, even the hydrological parts if all the parameters were calibrated simultaneously or using maybe specific criteria to assess the performance on base flow and on peak flow associated to storm events.

HESSD

11, C4897–C4903, 2014

Tables 2 and 3: Would it be possible to describe the parameters? It is not clear, especially for the “nutrient” parameters, which one are calibrated and which one are estimated from the text, maybe the table would be the right place to precise.

P 10384-10385 Management Interventions. At the end the model seems more dedicated to help the end-users understanding how their catchment behaves. The concern of policy makers is generally not only to know that nutrient inputs have to be decreased, but rather how, when, and where? If they are, how much will be the cost for farming production or the benefit for other services?

P 10386 lines 12-14 So despite that the MIR procedure would lead to consider only base flow, the knowledge on P transfer lead to keep the additional overland flow component. This illustrates my comment on the use of P and N concentrations as additional variables to constraint the model. Moreover it highlights that something is missing either in the calibration criteria to give some weight to parameter sets able to represent simultaneously flow, NO3 and P dynamics or in the model itself.

P 10387 lines 2-3 miss and over prediction are attributed to the limitations of weather data, could this be related to missing processes in the model?!

P 10387 lines 19 Again there is a little lack of clarity about which parameters are calibrated and which are estimated from previous knowledge, eg. How is determined the contribution of WWTP?

P 10387 line 27 what is the impact of errors on flow simulation on the simulated concentrations? Is there any transformation processes which could explain part of the errors (denitrification, uptake)? Could the use of daily time step explain some discrepancies for storm events simulation (which could last less than one day)?

P 10388 line 14 idem impact of adsorption-desorption processes? How well are peak flows reproduced?

P 10389 line 3 The spikes which are observed but not modelled could be actual events

HESSD

11, C4897–C4903, 2014

that the hydrological model is not able to reproduce (e.g. local runoff on limited saturated areas, or low hortonian runoff in dry season?)

P 10389 line 9 Might the model be too simple to properly simulate the dynamics of Phosphorus?

P 10391 line 4 It is not obvious that it will be possible to catch incidental losses with a low sampling frequency.

P 10391 line 11 Are channel processes really noise? The addition of in stream processes to hillslope processes and then mixing processes make indeed the deconvolution of the stream signal more and more difficult however these processes are real, and they may be relevant in terms of management strategies (actions to increase the natural cleaning capacity for instance)

P 10392 lines 6-10 The authors did not integrate management aiming at reducing nitrate loads in their scenarios which is Ok even it could be clarified in the introduction, and however they implemented a scenario with nitrate concentration decrease in the subsurface flow. This leads to some ambiguities regarding the author's objectives.

P 10392-10393 Discussion about the added value of the use of the CRAFT model comparing to others tools would be worth at this stage.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 10365, 2014.