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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.

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Thank you all for your comments and we feel that our paper will be greatly improved by your informed statements.

General response to all

We agree to change the title to include the CRAFT model name

The onus is on us as the authors to make our choice of model processes and how the model was applied and calibrated to be clearer. Hence we will modify the introduction to stress this. Our attempt to model flow, N, TP and SRP simultaneously is ambitious

and we were keen to show the reality of how difficult this task is. Hence we consider the degree of fit we have to the observed data is still good enough that it can act as the basis of a management/decision making tool.

We feel that R1 and R2 and to a lesser degree R3 have perceived that the model did not 'perform' well in terms of reproducing concentration (especially TP). However we were not trying to get a good fit or to over calibrate and optimise the model. We feel this 'curve fitting' exercise is inappropriate to the Meso-scale and to times series of nutrient data with only outlet data is available (weekly or monthly data). We spent some time showing sub-daily concentration data to show that the pattern is very random and that achieving a general fit to storm events, background and to the seasonal shift is possible. We were also explicit about the error in our scatter plots (Fig 5). We agree that we must stress that our calibration is based on our expert judgement and that we are trying to reflect an overall conceptual pattern of runoff based on the observed data and our knowledge gained from research studies. Detailed 'curve fitting' may make the results appear better (which we could have done) but this is not helpful to the policy maker as they want to know that we have not tailored the model to fit the data. We feel that simulating hillslope farming effects at the meso-scale is of value to future management and to the WFD.

We need more than an export coefficient approach as it shows why and when exports differ from event to event. A spatial model is not justifiable as this has too many parameter (a point made by all the referees). We could add more processes (for example: river and riparian processes) but the data does not justify it hence these processes were not included which is the MIR modelling philosophy. So we agree that we do need to clarify this.

Some more specific points were raised by the reviewers and this is our response:

1. The MIR approach, which has led to the development of the CRAFT, needs to be described more clearly. It is important to show how the process of developing a MIR

HESSD

11, C5061–C5065, 2014

model is systematic and all 3 reviewers felt that it could be improved in the paper. We structured this paper such that the final MIR model was shown but omitted much detail in terms of eliminating more complex model structures etc. This was mentioned by all reviewers, for example R3 noted that the model structure appeared to be “chosen at the beginning of the study”. Hence we will add a section covering the provenance of the MIR model using more references to earlier work and versions of the model.

2. In more detail, R1 commented on the absence of a riparian process component in the CRAFT, which would simulate sources of sinks of nutrients into the channel system. We argue that although we are aware of such processes that our model can reproduce the typical weekly or monthly time series of nutrient concentrations collected at catchment outlets. Another approach would be to develop a riparian component and then discard it later if it failed to improve on the model results (and add to the number of parameters). We settled on a three-compartment hillslope model, R3 found this surprising but did not consider it to be unacceptable (we argue that the results in terms of predicting flows (Q) validates the viability of this model structure). The paper would be extremely long if all options were discussed explicitly but we will add a paragraph to clarify the model development process. Hence we will acknowledge that these processes are important and explain why we have omitted them.

3. We agree with R2 that the Case Study (currently Sec 2.2) should probably come before the description of the final MIR model (CRAFT) structure (2.1). The current structure was designed to place the description of the MIR upfront, but since its structure is tailored somewhat to fit the catchment characteristics it may make more sense for it to be reversed. Again, we need to improve the description of the CRAFT model as all three reviewers have felt that it lacked enough detail as it stands. Perhaps we focussed too much on the observed data (Sec 2.2) and we will add new text. It is also important to stress that much development of the MIR concept was carried outlined in other papers where earlier versions of this model has previously been published. We also will attempt to explain why using the CRAFT model over more complex tools will

HESSD

11, C5061–C5065, 2014

add value to studies of nutrient management and mitigation (in response to R2 and R3).

4. We showed two observed datasets collected at the mouth of the Frome, one sub-daily one weekly. The use of the sub-daily data was intended to highlight that (particularly for P) there is a lot of "noise" and non-behavioural patterns observed in the time series data that cannot be modelled using any process-based tool or spatially distributed model. The reviewers (R1 and R2) have not commented on this part of the paper. R3 however has made the point that showing "bad" results is sometimes necessary even if these results (when assessed using standard curve fitting methods) would fail most criteria except the ability to reproduce the mean concentration. These data can also reveal a lot of information about what is happening at the meso-scale, e.g. uncontrolled and random spikes in nutrient concentrations due to local farming activities or wastewater treatment processes. Regarding R2's comments about model errors and not being able to reproduce the observed spikes in concentrations (Cs), it is important to note that in terms of loads (Q x C) these errors are quite small unless the spikes are associated with storm events with high Qs. As we have stated above this is not a paper about getting a 'good fit' it is about a model appropriate to this scale and this type of data.

5. A more detailed discussion of in-stream processes, particularly relating to P, identifiable from the hi-res dataset can be found in Bowes et al. (2009b). Plotting C vs. Q did not highlight any strong correlation between P (in both SRP, TP and PP forms) and Q that would be expected if runoff events were a major driver of elevated Cs. The Frome catchment itself is dominated by subsurface flow so modelling flow at a daily time step is unlikely to miss many sub-daily storm events. The processes may be there but they are not observable at this scale.

6. At least two of the reviewers (R1 & R2) have criticised the model performance in terms of curve fitting (e.g. producing high values of evaluation metrics such as R2 or Nash & Sutcliffe efficiency (NSE)) C data although R3 thought that it was acceptable

HESSD

11, C5061–C5065, 2014

in the context of “bad” data (discussed above). We need to stress once again that the point of the modelling exercise has been intended to show how critical processes can be represented by the CRAFT MIR model as stated above. We have shown that poor fit (e.g. in a scatter plot) due to the timing of our recession period but the overall pattern is still visually acceptable.

7. A mitigation “scenario” was then used in the paper to demonstrate how the observed loads can be reduced. R3 commented that we did not reduce nitrate loads, however this is not true as the nitrate loads from the SS component were reduced (See Fig 7 for a clear depiction of the decrease in loads). R3 has commented that the “relationship between model outputs and practical management options is not obvious”. We would argue that the model needs to be used in conjunction with terrain analysis to identify “hotspots” of pollution generation in the catchment, where management actions could reduce loads as shown by the model results. We agree that this step is probably missing in the current paper and future work will attempt to link the different components together holistically. We will address the more detailed points and edits to tables and figures when we have had a reply from the editor.

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