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Interactive comment on "Uncertainty assessment of a dominant-process catchment model of dissolved phosphorus transfer" *by* R. Dupas et al.

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Dear Editor and Referees,

We appreciated the constructive criticisms of the two referees Tobias Krüger and Paul Whitehead. We have addressed each of their concerns as outlined below.

RC1

One methodological inconsistency, however, is the translation of probability density functions (from regressions) into LOA and triangular weighting functions (sections 2.3.1-2.3.2, P11-13). [. . .]

1. We agree that a triangular measure might not always be the best approximation to the normal distribution. Hence we will reweight the contributions using the statistical

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deviation weights (truncated to 90 %).

Another aspect that is not entirely clear is the reasoning behind the 2-day aggregation of SRP loads (P12, L29-P13, L7) [. . .]

2. I understand the concern: when aggregating the data at a coarser resolution than that of the simulation, constrains on the model become loose and there is a risk of accepting models that would not have been accepted otherwise. Here is the reason for two-day aggregation: in the study catchment, storm events usually affect SRP concentrations during around 24h. A rainfall at the beginning of the day will lead to high load during the first day and "almost baseflow load" during the second day. If the rainfall occurs at the end of the day, then the high load will occur during the next day and the mean load of the first day will be "almost baseflow load". With the daily information we use in this modelling study, the model does not know what time the rainfall occurred hence the 2-day aggregation, which represents the load during the storm event (plus some "baseflow load" before or after the storm event).

We will amend the manuscript: "A 2-day aggregation was necessary here because increased SRP load as a response to each storm event could occur either mainly during the day of the rainfall (if the rainfall occurred early in the morning) or mainly during the day following the rainfall (if the rainfall occurred late in the evening), and with the daily resolution of the input data and model simulation, the information about the timing of the rainfall event was not available to the model."

I'm also missing all the earlier GLUE LOA applications in the field of water quality modelling [. . .]

3. Some of the references suggested will be added.

Specific points

P2, L5-7: It would be good to also refer to ecological impacts here.

4. The manuscript will be amended as suggested "serious hazard to ecosystems and

humans”

P2, L13-15: The P fractionation in transit (e.g. resorption) would be important, too.

5. The manuscript will be amended as suggested “as well as the potential P resorption during transit”

P3, L31-P4, L1: Here you could cite Krueger et al. (2012) where we dealt with evaluation data uncertainty (suspended solids, TP) explicitly in a GLUE limits of acceptability framework, albeit with even simpler models at finer time scales.

6. This reference will be added where suggested.

P4, L2-5: The grab sample uncertainty discussion could be usefully enhanced by referencing McMillan et al. (2012) where we discuss these issues at length by synthesizing a large body of work. P4, L3-5: Grab samples also represent a snapshot at a given point in the stream (e.g.Rode & Suhr, 2007).

7. The manuscript will be amended as suggested. “Grab sample data represent a snapshot of the concentration at a given time of the day, which can differ from the flow weighted mean daily concentration (McMillan et al. 2012), and a specific point in the stream cross-section, which can differ from the cross section mean concentration (Rode and Suhr, 2007).”

P5, L12 and elsewhere: Please specify what +/- represents – one standard deviation?

8. We will add “$\pm$ stand deviations”.

P7, L13f: I think there should be “model” at the end of the title.

9. “model” will be added at the end of the title.

P10, L4-17: Here or elsewhere it would be good to note that no long-term depletion of soil P pools was modelled, i.e. effectively assuming steady state. This would also be an interesting point for discussion.

10. The manuscript will be amended as suggested in the “Methods” section. “No long-term depletion of the different P pools was modelled, because P export from the catchment was small compared to the size of soil and sub-soil P pools.” The fact that long term evolution of soil P content was not modelled by TNT2-P (and our recommendation not to try and couple TNT2-P with a long term soil P content model but instead generate new map with a long term model and use these maps as input data to TNT2-P) is already part of the discussion.

P11, L4-16: Here especially it would be good to cite other GLUE LOA studies in water quality modelling, see above.

11. Here we are only citing general papers (including early papers) about the concepts and the philosophy of GLUE. References about applications to different field (including water quality) will be cited earlier in the manuscript (see comment 3).

P12, L20-22: Does this imply that no intercept was fitted in the regression equation? Would be good to clarify either choice.

12. There is an intercept. The fact that we assumed no bias due to sampling time does not mean that other sources of variability between the two datasets could not create a bias (for example the different storage time). To clarify this point, we will add the equation of the regression “y=a *x +b” P14, L7-9: Why were the weights summed (average) and not multiplied in keeping with the LOA concept? Krueger et al. (2012) discuss this.

13. I agree and I will multiply the weights in the revised manuscript.

P16, L23-25: No. What you must say is that you cannot reject this set of processes as a hypothesis of dominant control given the available evidence! There is no confirmation here, only a failed rejection.

14. I agree. The manuscript will be amended as suggested “The fairly good performance of TNT2-P at simulating SRP loads provides further support that the hydrolog-

ical and biogeochemical processes included into the model are dominant controlling factors in the Kervidy-Naizin catchment (i.e. the modelling hypotheses could not be rejected based on this study)."

Section 4.2, P17-19: Here I'm missing a discussion of the neglect of farm management practices in the model – which are vitally important if the model is to eventually have any bearing on catchment management.

15. I disagree. The fact that the calendar of agricultural practices is not correlated with variability in stream SRP concentration is one of our starting hypotheses (section 2.1.3, page 6 line 25). Agricultural practices have an effect on long term change in soil P Olsen content. In the discussion paragraph 4.2, page 18 lines 18-25, we discuss whether a model to simulate long term change in soil P Olsen content should be coupled with TNT2-P or if such a model should be run separately.

P18, L5-8: If you want to make this point then you should also discuss what benefit the finer resolution of the SW-GW interactions brings given that the subsequent P processes are much coarser (e.g. there are no hyporheic zone P reactions).

16. I agree that some of the models mentioned here include routines to simulate stream P processing (including hyporheic processes). However our statement was not about surface water – groundwater interaction, it was about soil - groundwater interaction, which is represented in a more explicit way in TNT2-P compared to semi distributed models. To improve clarity, I will add "extend of the riparian wetland area".

Fig. 7: The 1st storm in (a) is not easy to see (lines too close together) – consider different x-scale or 2 panels or else. In (b) the mix of lines and vertical lines with triangles to represent the LOA at the different resolutions (storms vs. baseflow) is confusing. Best would be to evaluate the model during the storms at the same resolution as during baseflow (daily, see above). If you can convince that this is not necessary then think of a different representation, maybe only the vertical lines but making the triangles smaller.

17. About time resolution of model evaluation see response 2. The stream was dry before the first storm of the year (and after the storm) hence the line = 0. Because several people do not like the triangles, I will remove them.

Fig. 8: Can't distinguish the lines and the LOA to get a sense of model fit. Consider scaling x differently, see above, and making model lines smaller and in same colour. Emphasise LOA lines (perhaps move in front of model lines).

18. I agree it is difficult to read both the uncertainty intervals and the results of model runs in this figure. For this reason I will remove the uncertainty interval from the "model runs" figure (8) and place it next to the "acceptability intervals" figure (7). Please note: the "model runs" figure shows only 50 runs, hence this illustration does not aim to be used for counting how many models fit. You can get a better sense model fit with figure 9 and with the number of models fulfilling the selection criteria as described in the text.

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