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Hydrology and Earth System Sciences Discussions

Interactive comment on "Data-based mechanistic model of catchment phosphorus load improves predictions of storm transfers and annual loads in surface waters" *by* Mary C. Ockenden et al.

Anonymous Referee #4

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General comments

The manuscript presents data-based models (DBMs) of discharge (Q) and total phosphorus (TP), with rainfall as the driver. For short-term description, rainfall (R) was the input, while for longer periods an effective rainfall intensity accounted for possible non-stationary behaviour. Effective rainfall was derived from R using a modifying factor that was a power function of the most recent discharge. The models were applied to 3 small agricultural catchments in the UK, where an exceptional monitoring programme provided high-resolution time-series of R, Q, and TP. For short term, model calibration was successful only for the most flashy catchment, while slow trends and data gaps

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Discussion paper

prevented calibration for the other two catchments. Long-term calibration was satisfactory in each catchment. Validation was unsuccessful for long-term TP modeling in some cases because the effective rainfall relationship failed to properly adjust R data beyond the training phase. Model calibration/identification yielded the time-constants and relative weight of the ‘fast’ and ‘slow’ response components. The paper is well written and shows an interesting analysis of an extraordinary dataset with sophisticated modeling tools. Besides these merits, the manuscript would clearly benefit from a new title, a somewhat more balanced judgement on data-based models, a clearer model introduction and a better separation from Ockenden et al. (in press).

The title promises improving “predictions of storm transfers and annual loads in surface waters”, yet the outcome was rather “a better understanding of the dominant nutrient transfer modes, which will, in-turn, help in planning appropriate pollution mitigation measures” (last sentence of abstract). I agree more, but not completely, with the abstract: the manuscript is about transfer modes derived from observed data and not better predictions (the predictive power of the models was rather limited). Loads did not get too much emphasis either. Therefore, I suggest to change the title to match the main findings.

According to the authors, a major advantage of the data-based approach is that rather complex models can be used to predict Q and TP without the need to know anything about the major processes – knowledge will be extracted from the data. Indeed, complexity of their model rivals that of certain conceptual models. However, this application wasn’t a clear success. The intro and the conclusions are very optimistic about data-based models. However, such DBMs have the same problems as conceptual models: their elements are so abstract that they can’t be linked to anything observable. This semi-black-box behaviour is nicely demonstrated in the manuscript: a strong ‘slow’ component is present in TP in certain catchments, various possible reasons are listed, but there is no way to know which applies in the specific case (e.g. SRP in baseflow may indeed come from WWTPs or activated deposits, etc, but which case does apply here? [We are not informed whether there are WWTPs in the catchments.]). So while process-based models are typically overparameterised and laden with uncertainty, their less abstract formulation leaves open at least theoretically to gather additional observations to prove or falsify hypotheses. Overall, I think that it should be mentioned that DBMs pay with an extreme data demand for not asking a priori knowledge on the system. Given that unresolvable issues appeared with such an extremely good data coverage, it is somewhat dissonant to recommend DBMs for catchment management when (i) model constituents can't be linked to anything else than the input, and (ii) there are practically no other catchments in the World with a comparable data coverage.

Another issue is related to the model presentation and the relation to the paper by Ockenden et al. (in press). The Methods section provides a very brief overview of the models referring to the companion paper for details and for calibration, so the models were first published and calibrated therein (this can't be verified because the article isn't accessible at the moment, it's still in press). Please highlight the novel parts of this manuscript compared to Ockenden et al. (in press). If the main novelty is the dynamics of TP load, the analysis of the results could be somewhat extended at the cost of details on the fast/slow components of TP (which are presented to the last detail). For example, it would be useful to elaborate more on the TPflux vs Q relationship. Yes, Q was already used to calculate TPflux, but the final correlation is actually determined by the relative variance of Q and TPconc. This would highlight how much delay and nonlinearity (both causing hysteresis) is present and therefore how much we gain by having a nonlinear autoregressive model.

Independent of this, deriving the applied models from the general 2nd order continuous transfer function (TF) model seems to be an unnecessarily complicated choice for several reasons:

- Only those models were accepted, which could be converted to the parallel linear storage format. This is very welcome, because such models are Markovian, i.e. the system's current internal state (or the last state in discrete formulation) and the current inputs completely determine the system's response. This assumption is typically made in most environmental and hydrological models. In contrast, a 2nd order TF model can be non-Markovian too (=a long system history is required to understand the current response, actual state is not a complete descriptor), which would be very hard to justify (which physical/biological/chemical process would lead to such system?).

- Continuity dominates in the model description, while eq. 4 and the aggregation to 30 minutes make it obvious that inputs were treated discretely. Then why bother with the more complex continuous models? These kinds of models are not used very frequently in hydrology/water quality modeling (as opposed to signal processing). Potential readers may easier understand if 2 parallel linear storages or ARX models were mentioned as alternative formulations for the same model.

- If the parallel storage formulation is so important to learn about slow and fast components, why are parameters shown in the general 2nd order form in Table S5?

Smaller issues

Page 2 Line 25: USLE is more semi-empirical than process-based.

Page 4 Equation 1: Use consistent units. If Q had [m3/h], and TPconc [kg/m3], TPload would readily be in [kg/h] without a conversion constant.

Page 5: Were the tau (delta) delay constants calibrated or fixed?

Page 6 L 15: If Re was necessary because the internal state of the catchment affected runoff and TP transport, would it make sense to use Re to the model Q as well?

Page 7 L 22-25: Of course, most pollutants do not follow Q, because they have either limited or temporarily activated sources or they partition between water and sediment. According to your argument on celerity, even non-partitioning conservative pollutants would theoretically show a hysteresis.

Page 9 L 3-4: Converting these constants to half-lives would make them easier to judge. It is somewhat difficult to grasp decay to 1/exp(1).

Page 10 L 18-21: If we don't know the mechanisms responsible for the slow pathway, what kind of measures could be taken?

Page 12 L 24-28: It's true that process-based models make some assumptions that do not always hold, but here it was demonstrated that neither the DBM can always be validated. Time-variable parameters are a useful concept, but seldom implemented.

As fluxes of TP are modelled, Fig S1 should rather show TP fluxes against Q. This could reduce clutter and illustrate how a naive linear model would work. Considering my comment above, it would be useful to move a modified version of this figure into the main text.

A major model-based finding of this study is the demonstration of are the importances of fast and slow pathways of Q and TP in the catchments. This could, at least partially, be derived directly from the data! High baseflow indices and slower recession indicate important slow pathways of Q, high baseline concentration indicates the same for TP. As the applied model doesn't have any mechanistic explanatory power (e.g. identification of reasons for these), how could management benefit from modeling? Please comment on this briefly.

On load figures, load is [kg], but per which time unit?

What are the time units in e.g. Table 2? If the continuous version was used, time has to have a unit. If the discrete version was used, the applied timestep has to be written.

What are the other units in Table 2?

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