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Hydrology and Earth System Sciences Discussions

Interactive comment on "Impact of phosphorus control measures on in-river phosphorus retention associated with point source pollution" *by* B. O. L. Demars et al.

A. Montanari (Editor)

alberto.montanari@unibo.it

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The paper deals with a very interesting issue. The control of phosphorus fluxes in natural streams is a topical problem and this paper gives a significant contribution to the subject. I have two concerns on this study, that I think should be addressed before the paper is considered for publication.

- The authors present a simple model for the simulation of total phosphorus loads. It is not clear whether or not this approach was already used and tested in previous studies. I think the authors should put in clearer evidence what are the original parts of the proposed model. In section 3 more reference to previous works should be given. If new approaches are proposed, a short discussion should be

provided about alternative (and perhaps physically-based) solutions and a justification for the use of simple regressions (equations from 3 to 8) should be given.

- Simple model are in general preferred in view of their robustness. The necessity to develop a tool that can be profitably used in real world applications is frequently the reason that suggests us to avoid the use of complex physically based models. In fact, these latter might be difficult to parameterise reliably, because measurements of their internal state variables are often lacking in real world applications. Therefore, the use of simple models, like the one that is adopted here, should be justified by fully proving their performances and capability to well work in real applications. This could be done in the case of the present work, because an extended record of observed variables is available. In section 4.1 the authors only provide an indication of the model performances in terms of explained variance. It seems that they refer to the performances in reproducing the total phosphorus loads during calibration. I would suggest that the authors provide a detailed indication of the goodness of the regression equations (3) and (4). The reader needs to know at least the R^2 coefficients of these regressions. Finally, I wonder why the authors do not perform a split sample exercise in order to fully validate their model. I agree that the final values of model parameters should be estimated by using the whole data set. However, a preliminary split sample exercise would allow one to obtain an indication of model performances in real world applications. Therefore I would suggest to devote an additional section of the paper to present the results of a model application where the available data set is divided in two samples: the first (longer) one can be used to parameterise the model; the second one can be used to emulate a practical implementation of the model where unknown total phosphorus loads are simulated. Of course we expect that in validation the model explains a reduced percentage of the variance of the observed data with respect to the calibration phase and it would be interesting to see whether the results are still satisfactory.

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Interactive Comment

I have also some minor comments.

In section 4.1, the authors provide the best estimates for parameters *a*, *b*, *c* and *d*, along with their confidence bands. What is the confidence level? May be 95%?

Section 4.1: the number of digits provided for the estimates of *a*, *b*, *c* and *d* should be homogeneous.

At page 12 the authors state that the flow duration curve is estimated using a long term data set. Do the authors refer to the period 1990-2001?

Section 4.2: I do not understand the last sentence (... the daily errors were simply added). Does this mean that the monthly errors were derived by summing up the daily errors? Does this solution lead to overestimate the total monthly error?

Section 4.3: it would be interesting to know whether the critical discharges above which net mobilisation occurs are different in a statistically significant sense before and after phosphorus control measures. It seems that the critical discharge increases, after phosphorus control, in the first scenarios and decreases in the second (industrial) scenario. Is there an interpretation for this opposite tendency?

I hope the above comments can help the authors to revise their paper, that could be a interesting contribution for readers of HESS.

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 37, 2005.