

## ***Interactive comment on “Model-based analysis of nutrient retention and management for a lowland river” by D. Kneis et al.***

**D. Kneis et al.**

Received and published: 10 May 2006

In reply to the comments of Referee #3:

We like to thank you for your critical and constructive review of our manuscript. The comments certainly helped a lot to improve the quality of the revised version. The following notes describe how we dealt with each of your comments:

Notes referring to the "specific comments" (no. 1-3):

1) We agree with the referee that the assumption made with respect to the release of phosphorus from sediments (constant release within the simulation period) is critical. This is due to the fact that a continuous P export necessarily causes the pool of phosphorus stored in the sediment to decline and - after a certain time - also the P release must ease off. In our opinion, it depends on the time frame of the prediction whether this needs to be reflected by the model. According to our quantitative knowl-

edge on the sediment P store at the time when the first manuscript was submitted, the annual observed P export from the Havel Lakes is small compared to the amount of P accumulated in sediments. Also, the expected change in the sediments P:Fe ratio, a possible indicator for the potential of P release, was expected to be small within the chosen simulation period (until 2015). However, in the meantime we analyzed a greater number of sediment core samples. In fact, at some locations of the investigated lakes, the sediment P pool turned out to be significantly smaller compared to our previous estimation. Because of that, the modeling approach necessarily had to be modified, to reflect the decay of the sediment P store and its effect on P release. Consequently, we had to repeat all simulations with a significantly enhanced version of the simulation tool TRAM. Apart from other major improvements, the new model includes the sediment compartment that was missed in the prior version. The rate of P release is now linked to the decline in the estimated P excess of the sediments. Nevertheless, also the enhanced model doesn't attempt to fully describe the aquatic P cycle. With respect to the rather complex system of coupled shallow lakes being investigated ongoing research - especially on the properties of sedimentary P fractions - is required, before a more detailed P model can be build.

2) Considering the changes that were made to the model concept, we believe that it now suits much better for simulating P concentrations of the Havel River within the focused time period. Due to the use of the enhanced simulation concept (which is now presented in detail in the text), we believe that the lack in novelty which has been identified in the earlier version of the manuscript is compensated. We also emphasize that it was the first time that the presented simulation program was applied.

3) The modeling methods were described only very shortly in the first version of the manuscript. As requested, a much more detailed description of the features and implicit assumptions of the water quality model was added to the Methods section of the revised manuscript. In agreement with the expectation of the referee, we feel that the paper became more self-contained due to this.

Notes referring to the "technical comments" (no. 1-14):

1-7) The listed grammatical errors were resolved.

8) The reason for the chosen simulation time period (until 2015) is now mentioned in the text. It is the time period which is set by the European Water Framework Directive to establish the "good ecological status" in all but so called "heavily modified" water bodies all over the EU.

9) The criticized paragraph was simplified as it was hard to understand. What we originally tried to express is this: If settling of organic P is lower than on average, the net P release from the sediment might be higher than on average. This is due to the fact that what we call "net P release" is a balance:  $\text{net P release} = \text{P release from long term sediment P store} + \text{recycling of freshly settled organic P} - \text{settling of organic P}$ . Unfortunately, reality is more complicated, because often a considerable fraction of stored P in sediments is bound to redox-sensitive hydroxides (mainly amorphous iron-III hydroxides). Considering that the redox potential is controlled by the mineralization activity (consumption of electron acceptors like oxygen and nitrate) it turns out that the prediction from the above balance equation may be wrong because it doesn't account for a feedback mechanism: If settling of organic matter (includes P) is low, degradation activity at the top sediment will also be low. Thus, the redox potential is kept higher and less iron-III is reduced to iron-II (the latter may precipitate as sulfide). Finally, if less iron-III is reduced, more of the sediments P binding capacity is preserved and P release from the long term P store (gross release) could be lower. In fact, this is one of the more difficult topics of limnology. The discussion above should have made clear that gross P release and settling of organic P are not independent. However, a quantitative description of this relationship is very difficult and there are reasons to expect both, a positive as well as an inverse dependency of gross P release on sedimentation rates. The missing explanation, why a distinction into "net P export/release" and "gross P export/release" makes sense (see the balance equation above), is now given in the text.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

10) Bad English was fixed.

11) Three measures are accounted for in the scenarios of reduced P emissions (added to the text): Firstly, the completion of sewage treatment plants which are currently under construction was considered. Secondly, it was assumed that the wastewater of those households which are currently unconnected to a sewer system is treated in small scale plants. Furthermore, an upgrade in P elimination efficiency in wastewater treatment to the highest technical standard was assumed.

12) It was criticized that the P export rate from sediments was assumed to be constant within the simulated time period. As mentioned above (specific comments #1), this was rectified in the new version of the model. The rate of P release from sediments is now a function of the sediments P excess, i.e. the release rate is assumed to decrease proportional to the decline in the sediment P excess. We agree with the referee that, in general, the sediments act like a buffer for P. However, the mechanisms behind this behavior are very complex and can hardly be described by e.g. a simple sorption model. In case of the system we investigated, biologically driven processes, i.e. settling and recycling of organic P are likely to contribute significantly to the observed buffering effect. Unfortunately (see comment on (9)), these mechanisms resist simple quantitative descriptions. Nevertheless, because of the spatial scale of the river-lake system and the fact, that data on sediment chemistry had to be obtained by ourselves, we were bound to use a simple model approach. Consequently, possible feedbacks between internal and external loading are not included also in the revised model. This is now clearly expressed and consequences are discussed in the revised text. The possible impact of neglected feedback is however demonstrated by a sensitivity analysis (internal P loading was assumed to increase proportional do decreased external loading). We have to mention that ongoing field research as well as the test of alternative model formulations is required to achieve a satisfying, more complete description of the P cycle in the Havel River.

13) The criticized assumption of constant P release from sediments is no longer part

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)

of the model (see notes on specific comments #1 & #12).

14) We agree with the referee that in-river nitrogen retention should better not be considered as "for free". Indeed, the problems arising from algal blooms due to elevated nutrient input may result in costs. However, it will be rather difficult to assess the costs on a monetary basis (perhaps this is possible when problems with the production of drinking water (bank filtration) arise or restrictions with respect to the recreational use are necessary). We just eliminated the term "for free" from the text.

The addressed relation between denitrification and cyanobacteria is certainly the result of a misunderstanding. In fact, we mentioned in the manuscript that N-fixation (not denitrification) may be conducted by blue-green algae at low N:P ratios. With respect to N accumulation in sediments we need to point out the following: Unlike phosphorus, nitrogen is almost exclusively found in organic binding forms (though some ammonium sorption to mineral surfaces is, like in soils, possible). In consequence, the N which is released in the degradation of organic matter can either undergo denitrification (after prior nitrification) or it may diffuse back into the water column. However, unlike P, nitrogen is hardly sorbed to mineral surfaces and it does not form insoluble precipitates with inorganic compounds like Ca, Fe, or other metals, like P does. Thus, depending on the sediments P sorption capacity, P is usually accumulated in much higher quantities than N.

---

Interactive comment on Hydrology and Earth System Sciences Discussions, 2, 2549, 2005.

[Full Screen / Esc](#)[Print Version](#)[Interactive Discussion](#)[Discussion Paper](#)