Reply on review of our manuscript HESS-2015-235 “High-frequency monitoring reveals nutrient sources and transport processes in an agriculture-dominated lowland water system” by Anonymous Reviewer #2

We would like to thank Anonymous Reviewer #2 for his effort for reviewing our manuscript and his valuable comments which we will take in full consideration in improving our paper. Our replies to his comments are listed below.

Major comment

General comment

The study evaluates transport processes of TP and nitrate-N in a highly drained lowland catchment using high resolution data. The main concern I have with the manuscript is that it is highly descriptive and several findings are highly vague. Because only water quality data have been used for evaluating the export mechanisms often a serious proof is missing. The main drawback of the study is the short measurement period which does not allow a clear explanation of the seasonal export behavior of the catchment. It is not clear why a half year period has been selected for the study. The study shows some methodological weaknesses because important constitutes like suspended sediments (e.g. via turbidity) have not been measured. This leaves the conclusions somewhat open. Furthermore the used modelling approaches are highly simple and do not provide deeper insight into transport mechanisms. Reading the manuscript was somewhat exhausting because the ratio of new findings to the length of the text was often small. I would not like to recommend the manuscript for publication in HESS.

We agree with reviewer #2 that the half year period of high frequency data are short for allowing a clear explanation of the hydrogeochemical behavior of the polder. The reason behind this half year period was that we started the measurement in Oct. 2014. At time of writing the manuscript, we only had data for a half year. However, we have continued the monitoring during and after writing of the manuscript until Oct 2015. So, at the moment we have a full year of high-frequency nutrient concentration at the polder outlet and we used this data series in the revised manuscript. Shortly, the NO_3 concentrations dropped to almost zero during April and continued to stay low until intensive precipitation events in August. TP concentrations gradually increased from begin June until the end of August and dropped upon the precipitation event in August.

We also have measured suspended sediment continuously (via turbidity) with an OBS sensor combined with SS measurements on grab samples and Total Reactive P during the period Oct. 2014-Oct. 2015. Our original idea was to write second, more hydraulically oriented paper with a focus on erosion and sedimentation of suspended sediment and P from the channel sediment. We realize that not including this data in our manuscript weakens our environmental interpretation of the nutrient behavior in the system and, therefore, added the suspended sediment data and TRP data to the revised version of the manuscript in support of our findings.

We used transfer function-noise modeling to estimate the impact of rainfall on the nitrate concentration. We do not want to say if TFN modeling is a simple approach or not (there is a long record of scientific publication on development and application of TFN models). We are not convinced that modeling should be complicated by definition. Many model concepts exist that deal with nutrient transport. The most mechanistically oriented models are not necessarily the best ones, because they may underperform when applied at larger spatial scales. To our knowledge TFN models have yet not been applied on high-frequency monitoring data of nutrient concentrations. Although we realize that our model is a first step to, we think that TFN modeling is a promising tool to analyze high frequent water quality datasets.

To make our manuscript more quantitative we calculated the statistics of the changes in TP concentrations and turbidity during the pumping events and calculated the event-driven export-TP flux. For this we used a hydrograph separation method to separate the high-frequency TP concentration data series into short-term TP concentration peaks caused by operation of the pumping station and baseline TP concentration.

Specific comments

Page 8344, line 25: please provide correlation measures on that comparison.

We added the R^2 between high frequency TP data and grab sample TP data (0.982).

Page 8350, line 4: representative for what? Residence time during the period after mid of November? Residence time is the time a water parcel needs to move from rainfall through soil (and groundwater) to the stream or ditch. The fact that nitrate concentration peaks five days after peak discharge does not imply that residence time of water is also five days. That would only be the case if whole nitrate stems from rainfall and nitrate would be a stable tracer. This is of course not the case. The mentioned mean residence time of 5 days is from my point of view unrealistic. The cited literature refers to a residence time of 6.6 days in the main channel and not in the catchment area!

This statement might indeed be somewhat confusing it is. The flow velocities in a polder area are maximised by the capacity of the pumping station. This results in a delay between rainfall and peak concentrations at the pumping station. The 5 days is the average travel time of the water in the field ditches, sub-channels and main channels. Van den Eertwegh (2002) calculated a mean annual residence times of water in the Lage Vaart main channel of 6.6 days. Catchment mean residence times of water vary strongly upon precipitation events and are much shorter after wet conditions (Van der Velde et al., 2012). As a consequence, the residence time in the main channel is shorter during wet conditions. If we only look at the volume of Lage Vaart main channel and pretend that there are no connecting sub-channels between the pumping station and the agricultural area it took already more than one day with continuous pumping to refresh all the water in the channel stretch of approximately 12 km between the pumping station and the beginning of the agricultural area. We added this statement and this reference to the manuscript.

Page 8350, line 17: Are there any measurements on turbidity or sediment concentrations which support this statement? Where should the sediments come from if sediment concentrations can be assumed very low between pumping events? The time series data suggest that the few high DRP concentrations data are mostly associated with high TP concentrations. Therefore high TP concentrations can also be caused by high DRP concentrations. As far as the data suggest PP has not been measured directly during the events and therefor the given explanation that SS are *responsible for high TP concentrations are speculative. It is well known that DRP concentrations are often high in surface runoff and may explain also high TP concentrations, at least during freeze-thaw cycles. Without additional data on turbidity/suspended sediments of direct PP measurements it is not possible to identify sediments as a main source of high TP values.*

See our reply to the general comments. We added our turbidity measurements to the manuscript. These measurements support our statements that the TP concentration peaks are caused by resuspension of the channel bed sediment. We added the following information to the manuscript. Simultaneously to the TP concentration peak we always measured a peak in turbidity. Compared to the concentration before pumping, the maximum TP concentration was on average a factor 1.83 and 1.3 higher during pumping with two pumps and one pump, respectively. This indicates a dependence of the TP concentration on the flow velocity in the main channel (0.125 m/sec with one pump and 0.5 m/sec with two pump). The TRP concentrations also showed an increase in concentration during pumping. To our opinion, it is plausible that the colorimetric measurement of TRP, which takes place in an acidic solution, can be attributed to the dissolution of particulate inorganic P.

Page 8351, line 2: highly speculative, is there any proof on this?

See our reply on the following remark below.

Page 8351, line 3-4: That is not true. Simply high DRP concentrations, which are elevated in surface runoff, can be responsible for high TP concentrations. Here the investigation lacks from missing direct DRP measurements.

Together with the changes in NO_3 and TP concentrations, an increase of the turbidity (from 8 to 57 FTU), a decrease in the TRP concentration (from 0.06 to 0.02 mg P/l) and decrease of the conductivity (from 235 to 122 mS/cm) (Fig. S1) was observed. This strongly points to soil surface runoff and transport of particulate P.

Page 8351, line 10: What does this tell us? It is not surprising that rainfall is related to nitrate concentrations if the soil is saturated (after mid of November). All rainfall is transformed into discharge via transport through the soil and pipe drains. Rainfall becomes not directly discharge but exchanges the soil water rich in nitrate.

As discussed in section 4.1.1 a reduction in NO_3 concentrations coinciding with periods of intensive rainfall is commonly reported in high-frequency monitoring studies in natural catchments and this is attributed to dilution of the surface water by surface run-off. For us, it was somewhat surprising that we didn't see such a dilution phenomenon during rainfall events in our polder catchment. This gives insight in the nutrient transport mechanisms for the agricultural landscape type we studied. It also indicates that soil surface runoff is not an important nutrient transport mechanism in this lowland area. As the relevance of surface runoff on the water quality in such areas is still under debate (e.g. see Van der Salm et al. (2012)) this is valuable information.

Page 8352, line 9: I fully agree. The time series is too short to gain sound information for explaining the export behavior

See our replies before.

Page 8352, line 17: "overestimation" is a wrong term because these loads are calculated with the best available data. Low frequency data underestimate the load!

We considered this during preparation of the manuscript. Originally we used 'overestimation' because load calculation based on grab sampling is common practice and high frequency data provides addition information. We, however, see the point the reviewer makes here and changed this into underestimation of the grab sampling load. This now gives a small change of the percentages when compared to the original manuscript

We extended Fig. 5 to 1 October 2015 and added the cumulative TP load after separation of the short scale TP concentration peaks during pumping events from the high-frequency dataset. We added a short quantitative description of the results.

Page 8353, line 5: Why discussing it here when it will be discussed one more time later on. Likely is not a clear proof.

Agreed, we removed this sentence

Page 8353, line 14: why are the chloride data presented here?

Chloride is a tracer/indicator for the contribution of deep groundwater to the surface water. The Presenting the Chloride data give support to the interpretation of nutrient dynamics.

Page 8353, line 25 and ff: these findings are not surprising and can be therefore shortened

Agreed, we shortened this paragraph.

Page 8354, line 8: is there any evidence on that? Has the cited reference conducted the investigation on the same site than this study?

The cited reference provides evidence for shallow pyrite oxidation and is influence on the SO_4 concentration in intensively drained Holocene marine clay polders with the Netherlands. The Lage Afdeling is typically such a polder. The reference also uses data from this polder.

Page 8357, line 10-15: the restriction of the TFN model to less variable (wet) soil moisture conditions allows better model predictions but reveals less information about transport processes within the catchment since only a relatively short duration of several month is captured. State of the art is continues modelling for at least several years. The presented results are not really new as indicated by the discussion.

To our opinion TFN modeling can become a valuable tool for analyzing high frequency water quality data. We searched in the literature for application of TFN model on high-frequency nutrient concentration but we could not find any.

Page 8358, line 20-25: two different reasons are described for the increase of DRP in autumn (change of loading and in-streams processing). Because of missing detailed studies at least of one of these processes the authors are not able to clarify the importance of both possible processes, the statement therefor keeps vague

It is true that we have no quantitative information to separate the groundwater P load and the P load from remobilization from bed sediments. Both process become dominant for the water quality under dryer conditions and are thus difficult to separate. We do not want to speculate too much on this topic.

Page 8359, line 20-25: because suspended sediment P (PP) measurements are missing it is not clear whether the TP concentrations during pump cycles stem from higher PP or DRP concentrations. If resuspension of sediments is the main reason of increased PP concentrations during events than decreasing PP peak concentrations could be expected with subsequent events because of exhausting sediment deposits in the channel. The data do not show this. Due to missing parallel measurements of DRP and TP a clear statement is not possible.

The following sentence was added to the section 3.3: 'The data shows the highest increase of TP concentrations (0.16-0.60 mg P L^{-1} during pumping with two pumps after longer periods without pumping (21 Oct, 2 Nov, 8 Dec, 20 Feb., 23 June, 25 July and 15 Aug) and decreasing TP peaks were observed with subsequent events (Fig.3)'.

Page 8360, line 1-23: the discussion starts with a statement which should be part of the problem section. The chapter ends up with a conclusion without referring to the study. No results of the study are discussed.

We agree with this comment and we have rewritten this paragraph. The statement is moved to the introduction.

Page 8360, line 27: I do not see that a NO3 peak concentration of 10.4 mgN/L during a given high flow event is a proof of manure input. During January peak concentrations also reached a range between 8 and 9 mgN/l

This is true if we focus on the absolute height of the peaks. However, if we look at the increase in concentration upon the rainfall event this is almost two times higher than the other TP concentration peaks with similar precipitation (Table 2). Moreover, the residuals in the TFN modeling show that this peak and the following peaks on 27 Febr. and 3 March were different from the peaks before half Febr. which also shows the added value of TFN modeling.

Page 8361, line8-25. The discussion poses more questions than reveals explanations of the measured time series. No clear findings can be presented because of missing supplementary measurements despite the concentration measurements at the outlet. Missing PP or turbidity measurements have already been mentioned.

See our reply to the general comment.

Page 8362, line 1-10: if this information is important for the discussion it should be given in the method section.

Agreed, we moved this to the method section.

References

Van den Eertwegh, G. A. P. H.: Water and Nutrient budgets at field and regional scale, travel times of drainage water and nutrient loads to surface water, PhD thesis Wageningen University, 2002.

Van der Salm, C., van den Toorn, A., Chardon, W. J., and Koopmans, G. F.: Water and nutrient transport on a heavy clay soil in a fluvial plain in the Netherlands, Journal of Environmental Quality, 41, 229-241, 2012.

Van der Velde, Y., Torfs, P. J. J. F., van der Zee, S. E. A. T. M., and Uijlenhoet, R.: Quantifying catchment-scale mixing and its effect on time-varying travel time distributions, Water Resources Research, 48, n/a-n/a, 10.1029/2011WR011310, 2012.