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Comment

Interactive comment on “Extreme dissolved organic nitrogen fluxes in the human impacted Pamba River, Kerala, India” by S. Elizabeth David and T. C. Jennerjahn

Anonymous Referee #1

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Review of “Extreme dissolved organic nitrogen fluxes in the human impacted. . .”

This manuscript presents data on DON concentrations and estimated fluxes/yields from a southwest Indian catchment with a very particular anthropogenic disturbance – in addition to the high population density the catchment holds one of the most visited pilgrim sites, with high associated N delivery expected to be linked to this. While the manuscript has some potentially interesting data, I have some concerns on the data itself, and the manuscript requires a lot of clarification before its conclusions and interpretation can be properly evaluated.

*The number of samples collected is rather low to quantify fluxes and yields for DON

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(monthly sampling over 9 months). I would be useful to show any relationships between discharge and DON concentrations, or at least provide the full data so that the reader can reconstruct these.

* Please provide some additional methodological details, e.g. (i) which standards were used to analyse TDN ? (ii) how were DIN samples taken and preserved ? (iii) the detection limits mentioned for TDN analyses seem very high compared to what is typically reported in the literature for these types of instruments, the authors mention a detection limit of 0.29 to 0.32 mg N L⁻¹ for their instruments (equivalent to ~20 to 23 μM – which would be a better unit to use given that this is used throughout the rest of the ms to report concentrations). Stubbins and Dittmar (2012, L&O Methods 10, 347–35) for example report detection limits between 0.6 and 11 μM , Shimadzu itself suggests detection limits to be around 4 μM .

* DIN analyses are mentioned in the Methods section, but data are not shown in detail – reference is made to a paper under revision but we have no access to this at the moment. It would be good to describe the DIN data in more detail in this paper.

*While we have no objective reason to question the data, the concentrations and fluxes are obviously extremely high compared to data from other systems globally. A number of questions and suggestions in this context: -The fluxes/yields reported here (90-1976 kg N ha⁻¹ y⁻¹, i.e. 9000-197600 kg N km⁻² y⁻¹) are so much higher than in other systems that it's hard not to raise an eyebrow. The authors have a few strong arguments to suggest the system here is indeed exceptional (e.g. calculations based on number of pilgrims) but these estimates, which don't take possible loss terms and N processing into account, still fall short at reaching the N yield observed. For comparison, the NEWS model output for calibration and validation catchments (Harrison et al. 2005) report DON yields up to ~1000 kg N km⁻² y⁻¹, global output up to ~2200 kg N km⁻² y⁻¹; and the data compilation by Alvarez-Cobelas et al. (2008, reference below) has empirical data that do not go higher than ~500 kg N km⁻² y⁻¹. The fact that the authors here find a DON yield of approximately 9000 kg N km⁻² y⁻¹ for the upper catchment

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(section I), which is reportedly pristine with a population density < 1 inhabitant per km^2 , is therefore suspect. The authors should convince us their numbers are correct and if so, provide a hypothesis on why their DON yields from the pristine upper catchment would be so much higher than that reported in even highly anthropogenic catchments elsewhere. Alvarez-Cobelas et al. (2008) Export of nitrogen from catchments: A world-wide analysis. *Environmental Pollution* 156 (2008) 261-269.

- I can only assume that the authors have also measured concentrations of dissolved organic carbon (DOC). These would be very useful to have a little more insight as to what the major form of DON might be (low DOC/DON ratios expected if e.g. labile compounds such as AA or urea contribute substantially); and as a quality check to constrain the DON concentrations. DOC:DON ratios have a global average of around 20, but can vary substantially. Even if we assume a relatively low DOC:DON ratio of ~ 10 (as observed e.g. by Seitzinger et al. 2002 in agriculturally dominated systems), the DON data reported here would translate into huge DOC concentrations unseen elsewhere. DOC analyses are thus a good check to confirm the validity of the data.

* The Discussion section mentions a few other parameters such as TSS, BOD, pH which are not described in the Methods section and it's unclear where these data come from.

* page 16149 top lines: It is mentioned here that pH decreased from 7.3 ± 1 to 7.1 ± 0 , then increased to 7.1 ± 1 – Given the errors reported I doubt we can classify these as a clear or significant decrease and increase.

* It is often hard to follow the arguments and trace the numbers being cited. For example, on page 16149 you mention a DON flux of 606 kg (organic N) ha^{-1} , based on the number of pilgrims and the average organic N content in human waste. Does this number only refer to the 10 km^2 area where the pilgrimage is concentrated in, or is this extrapolated over the segment, or the entire catchment? Is this per day, per year? Why is this number different from the 271 kg DON ha^{-1} y^{-1} mentioned in Table 3?

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A little explanation is provided on page 16155: “As discussed before a DON input of 606 kg ha⁻¹ was calculated from the 10 km² temple area and when normalized to the whole catchment, the input from the pilgrim activity amounted to 271 kg ha⁻¹ yr⁻¹.”, but I don’t see the link between the numbers 606 kg ha⁻¹ for a 10 km² area, how does this convert to 271 kg ha⁻¹ yr⁻¹ for a catchment of 2235 km² ?

* The total potential DON input is calculated as 514 kg N ha⁻¹ yr⁻¹, and the range mentioned is 477-752 kg N ha⁻¹ yr⁻¹. While Table 3 lists the numbers for individual components that add up to 514 kg N ha⁻¹ yr⁻¹, it’s not clear how the estimated range was calculated.

P16138 L 14: rephrase this sentence, “inadequate sewage treatment” is not a land use practice.

P16150 line 12: ambiguous to where these numbers refer to (your study or those of the study in Spain). Also on line 23 of the same page “application rate was about 8-fold higher”: ambiguous in which of the two studies it was higher.

Interactive comment on Biogeosciences Discuss., 10, 16137, 2013.

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