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Biogeosciences Discussions

Interactive comment on "Nitrification and inorganic nitrogen distribution in a large perturbed river/estuarine system: the Pearl River Estuary, China" *by* Minhan Dai et al.

Minhan Dai et al.

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General comments

In this paper, the authors studied the spatial and seasonal evolution of dissolved inorganic nitrogen in the three main tributaries of the Pearl River Estuary. On-site incubation was also carried out for determining nitrification rates (ammonium and nitrite oxidation rates) and the relative importance of substrate concentrations, temperature, pH, dissolved oxygen and microbial abundance as controlling factors of nitrification rates. The objective of the paper and the study area are clearly introduced and results clearly presented. Based on their measurements, the authors conclude that temperature is the main controlling factor of nitrification rates in the Pearl River. Despite the

fact that temperature variability could explain seasonal variability of nitrification rates, what is the impact of this factor on the spatial variability? Seasonal and spatial controls of nitrification have to be clearly identified in the conclusion of the paper.

We thank the reviewer for the overall positive comments. As correctly pointed out by the reviewer that our data showed that temperature appeared to control nitrification rates to a large degree yet only clearly at a seasonal time scale. The spatial variability of nitrification rates was obviously controlled by a combination of many other factors, such as nutrient concentrations, nitrifier abundance and DO concentration. We will clarify and add this in both the abstract and the conclusion of the revised MS.

Specific Comments

P 1550, L 19: Why express measured water flow related to the long-term average?The authors could give measured values for the three cruises.

The reason we presented the measured water flow related to the long-term average was to better define the hydrological condition in the context of long-term average. We will also provide the measured values of the three cruises, which were 2068 m3.s-1 in January 2005, 2705 m3.s-1 in March 2006 and 6414 m3.s-1 in August 2005.

P 1552, L10: As pointed by the authors the MPN-Griess technique is known to underestimate bacteria biomass. This underestimation is, as also mentioned in the paper, lower than the one induced by the immonufluorescence technique or MPN-PCR method. Have the authors considered the method proposed by Brion and Billen (1998)?

Yes, we have considered the method proposed by Brion and Billen (1998). Yet we chose to use the classic and relatively straight forward technique of the MPN-Griess method. We have discussed the advantage and disadvantage of a variety of methods in measuring nitrifier abundance.

P 1555, L1: While the increase in wastewater discharge clearly appears on Fig. 2, the

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intensification of chemical oxygen demand discharge (with highest value recorded in 1995) and ammoniacal nitrogen is not so evident.

The reviewer is right about the observation. While we presented Fig 2 aiming to provide a general context of the waste discharge and the general increase trend of ammonia. The not-so-evident increase in chemical oxygen demand discharge might be related to the complex changes of the system, such as the treatment rate of sewage. However, this is beyond the scope of this study.

P 1560, L 4 : What is the difference between the nitrifier number presented Fig 6 and Table 3? As an example, in Table 3 a value of 3500 is referenced for stations 1 and 2 in March 2006 then a value of 3000 is shown Fig. 6. Similarly in August 2005, a nitrifier abundance of 1150 is presented Table 3 and a value lower than 500 is shown Fig. 6.

We apologize that we made a mistake in Fig 6. There are no difference between the nitrifier number presented Fig 6 and Table 3. The nitrifier abundance at Station 1 in March 2006 shown at Fig 6 should be 3500 cell mL-1 while the value at Station 2 for surface and bottom waters in Table 3 should be exchanged. We do appreciate that the reviewer pointed out this mistake and will correct this in the revised MS.

P 1561, L 8: Light and community composition control on nitrification rate are not study in this paper. Could one of these factors be more important than the tested one in the Pearl river estuary?

Light could be an important factor yet in this study area, water is quite turbid especially in the upper estuary. The community composition could also be important in determining the nitrification. However, given the fact that we measured both the bulk nitrification rate and abundance, we reason that the community composition should affect our results and conclusion.

P 1562, L 5: Please could you add temperature data in the manuscript (Table 3 for example)? We will add temperature data in Table 3. As a matter of fact, we presented

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temperature data in Fig. 7 (a).

P 1563, L 25: Despite the distribution pattern in nitrifier abundance was broadly consistent with that of the nitrification rates based on section category (p 1560 L 10), there is no relationship between ammonia oxidation rate and nitrifier number measured in the different section (Fig.6 c-d).

As shown at Fig 6 c-d, the distribution pattern in nitrifier abundance was broadly consistent with that of the nitrification rates based on section category. In general, the ammonia oxidation rate is controlled by nitrifier density to a certain extent, but other factors, such as nutrients concentration, DO concentration may also be responsible. So nitrifier abundance and nitrification rate may not always hold a simple correlation.

P 1565: Is a DO concentration higher than 4 mg L-1 really limiting for nitrification rates in the downstream Humen estuary and in the Yamen estuary? Although most of the higher ammonia oxidation rates were observed to be associated with a DO range of 0.5-2.5 mg L-1(Fig. 7c), we are not certain whether DO concentration higher than 4 mg L-1 really limited the nitrification rates.

P 1566, L 10: This statement contradicts the interpretation made for station 6 p 1596 L 20. In consequence, how can the authors explain the low DO concentration associated to high nitrification rate measured at station 6?

Low DO concentration associated to high nitrification rate was measured at station 6, which is consistent with result that the higher ammonia oxidation rate was associated with a low DO range, since nitrification induced DO consumption.

P 1567, L 10: What is the value of pH during the other cruises? As for temperature, please could you add pH values in the paper?

During our cruise, pH at incubation stations ranged 6.94-7.25, 7.07-7.42, 6.50-7.22 for March 2006, January 2005, August 2005, respectively. We will add these values in our revised MS.

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P 1568, L 16: I agree with the fact that temperature variability could explain seasonal variability of nitrification rates but what about the spatial variability? This has to be mentioned. What is the controlling factor of the spatial variability? Moreover can temperature explain the difference of nitrification rate observed in the Pearl Estuary and in the Scheldt or the Gironde Estuaries as suggested P 1559 L20-30 ?

As explained above, our data showed that temperature appeared to control nitrification rates to a large degree yet only clearly at a seasonal time scale. The spatial variability of nitrification rates was obviously controlled by a combination of many other factors, such as nutrients concentrations, nitrifier abundance and DO concentration. We believe it would be interesting to make a thorough comparison between the Pearl River and other systems such as Scheldt and Gironde yet we would not believe temperature shall sole factor that makes the difference in between.

Interactive comment on Biogeosciences Discuss., 5, 1545, 2008.

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