

Interactive comment on “Dissolved inorganic nitrogen in a tropical estuary at Malaysia: transport and transformation” by Shan Jiang et al.

Anonymous Referee #2

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Dissolved inorganic nitrogen in a tropical estuary at Malaysia: transport and transformation. Authors: Shan Jiang et al.

Increasing nitrogen enrichment is one of the main pressures compromising the integrity of coastal ecosystems. Given the rapidly changing tropic coastal areas, the present paper is timely and an important contribution towards a better understanding of a rapidly changing global nitrogen cycle. The authors describe a series of cruises in the Rajang estuary where they investigated the distribution of nitrogen species including $^{15}\text{N}/^{18}\text{O}$ isotopes. By combining observed distribution patterns with a dedicated series of incubations, the authors derive estimates of N transformation, and based on this, an improved estimate of riverine nitrogen loads by the Rajang river to the coastal ocean.

In general, the paper is well written and most results are clearly presented. However,

the discussion is quite long with several unclear sentences: It needs to be more focused and needs attention in terms of clarity. On many instances, claims are made that are not backed up by literature or data. I suggest to focus on a few points for which clear cases are presented.

The title focusses on inorganic nitrogen but dissolved organic nitrogen is playing a crucial role. The authors may consider to leave “inorganic” from the title.

The language was mostly clear, but it is important that the text is corrected by a native speaker. E.g. often articles are missing.

Suggestions and questions Abstract, Line 17 – 19. Split sentence into two: La Niña induced high precipitation and discharge rates, decreased reaction intensities of ammonification and nitrification. Hence similar distribution patterns of DIN species in the estuary were found during both seasons.

Page 5, Line 7: Precision instead of pression

Page 6, line 15. I do not agree that the concept of Apparent Oxygen Utilization can be applied as the river is an open system. Hence, an unknown amount of O₂ is exchanged with the atmosphere. I strongly suggest to use undersaturation instead.

Page 6, line 20/21: In most estuarine literature, this phenomenon is referred to as an estuarine turbidity maximum. I suggest to use that term. What was the SPM concentration in the sea?

Page 6, line 24: the correlation with salinity is not evident from S4. If that correlation is not important, I suggest to delete/reword the sentence or alternatively show whether the correlation is significant.

Page 7, line 1. PN values are given in mg/l, dissolved fractions in mol/l. I suggest to convert the PN also in mol to simplify a comparison.

Page 7, line 4. Leaving out which data? And why? Are they shown somewhere? What

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is the effect on the conclusions?

Page 7, line 13. What was meant: a deviation from linear mixing? Please be more precise (You explained the principle in material and methods). E.g. in addition to offset name it “deviation from conservative mixing” the first time you use the concept. And mention that NO₃ is released.

Page 7, line 19. Conservative mixing instead of . . . distribution?

Page 8, line 1. Porewater samples was limited? Maybe you mean low? How low?

Page 8, line 5 I am not familiar with using “aorta” to refer to river characteristics. Please clarify/use other terms. Page 8, line 30: . . .was comparably “wet” than. . . : unclear sentence: wetter than ?

Page 8. Section 4.1: Do the stable isotopes support that higher N release in Rajang watershed is from fertilizers? This is not apparent from Fig 7. Alternatively, it is related to degradation of peatlands. I suggest the authors to improve their case(s) in Section 4.1. Specifically, it would help if the authors are able to discern between two important sources fertilizer/human sources and N from the oxidation of peat. Can the stable isotopes help? Also, I suggest to discern more clearly between increased loads due to increased runoff (la nina) and due to increased concentrations.

Page 9. Section 4.2: First paragraph How much pore water exchange is necessary to explain the observed increase? Is this realistic or are other processes be involved?

Page 9. Section 4.2: second paragraph You claim that PN is not involved in the transformation processes, but given the high PN concentrations and low DIN concentrations, small changes in PN may have a large impact on DIN. I suggest to do some simple calculations, how much PN has to be reduced to explain the observed DIN changes. See also comment to page 10, line 33ff

Page 9, line 32/33. This sentence reads as if DON in the mixing zone is lower than in the coastal ocean. Please rephrase.

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Page 10. Line 10: . . . their input can be identified. This is just a claim. Please substantiate.

Page 10, line 20 – 28. This part of the discussion could fit better in Part 4.1

Page 10, line 33 ff. Here you claim that PN can play a role, but in 4.1 you claim that PN does not play a role. Please clarify this.

Page 11. Line 6 ff. No supporting parameters like chlorophyll are presented that may clarify changes in PN quality. In this respect, I wonder about whether phytoplankton blooms occur? After all, fresh readily degradable organic matter is needed to create the anoxic microniches needed for denitrification. Please clarify this. Also, can you discern between sediment denitrification and water column denitrification?

Page 25, line 25. This observation reinforced.: please add a citation to back this statement.

Section 4.3 Formula (4) should be transferred to the Material and methods section.

Of course, the total loads are strongly dependent on discharge. For that reason I suggest no to focus on loads but on the concentrations: What are the factors responsible for the observed rather low DIN loads???

Discussion: General Comment In general, the discussion is too long. The points addressed in the discussion are important. But the paper would gain, if the discussion is more focused than at present.

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