

Point-to-point Reply and Rebuttal to Anonymous Referee #2

This paper presents a nice example of the propagation of dissolved inorganic nitrogen (DIN) concentration and the interaction between the different forms. There are 20 stations with measurements of nitrate, ammonium, nitrite and dissolved oxygen in the Danshui river (Taiwan). The authors are trying to explain the pathway of nitrogen from upstream to downstream. They also couple these observations with anthropogenic factors like population density, percentage building area, land-use (agricultural and natural) and water flows. The presentation quality is fair. There are 6 tables and 7 figures, which give a nice presentation of the results and data used. In general i can follow the way the conclusions are obtained, but some conclusions are in my opinion 'a step too far'. The text needs major improvement to clarify or be more precise and less speculative.

In this revised manuscript, we removed the NANI relevant discussion which is an example of "a step too far" in the previous article. Furthermore, we extended our data from a single year to four years, added new plots of C-Q relation into Results, gave examples of how Q controls the cumulative DIN export, and elaborated the stoichiometric relation between DO and DIN in Discussions to make our every conclusion more straightforward. We appreciated reviewer's comments which indeed improve the scientific level of this manuscript.

General remarks

This study is performed on a river basin of 2726 km². This area is comparable with one grid cell of 0.5 * 0.5 degree. I believe that global models, like for example Global NEWS, can only be used at a regional scale like continents and a number of rivers which have an area of at least 10000 km². Using results of a global model on rivers with a basin area like the Danshui River is not within the scope of global models.

Reviewer is correct. However, Danshui River is an example for Oceania rivers where nutrient export should not be neglected and our analysis reveals that the N export from small mountainous rivers is more sensitive to population density change when comparing with that derived from the global model (Table 6). Our report enriches the global river database benefiting our understanding on the nutrient export from small watersheds which there're ~10,000 small rivers drain ~20% of global land surface area.

The idea that this river could be an estimator for Oceania is doubtful. This needs to have more explanation and underlying facts.

In the revised manuscript, we have added more specific description in the Introduction to identify which Oceania countries we are referring to and our similarities. Besides, in the second paragraph of Section 4.2, we summarize some basic basin characteristics, including watershed area, river length, maximum relief, and population density, for the selected Oceania rivers in Philippines, Indonesia, and Papua New Guinea (please also refer to Figure 6). There are totally 92 selected rivers draining ~1.4x10⁶ km² surface land area, about ~45% of the Oceania. The entire Danshui watershed has

2697km² in watershed area, 125km in flow length, 3529m in maximum relief, and ~2m in runoff. The Danshui watershed and its subwatersheds have similar features with the Oceania rivers, implying Danshui River could be an estimator for Oceania.

Method RC is influencing the results. It needs a second thought whether this approach is needed here.

As our responses to the Reviewer#1, using any estimate described in our study will not influence our story since the coefficient of variation among the four estimates is small (See Table 4).

The measurements are more than 10 years old. Are there no additional measurements (for example from the EPA) to show some trends? I miss the supporting information with all the measurement data and background parameters used. I think this is essential.

In this revised version, we extended our data from a single year to 3-4 years. We stopped our sampling in the upstream after 2004. Therefore, we do not basin-wide data to support our study afterwards. As mentioned above, the purpose of our application is to examine the runoff control on DIN “yield or export” rather than concentration. From Figure 4, it is also found that the variation of concentration is less than the one of runoff, indicating the hydrological control on DIN export here. However, we have to admit the DIN concentration in the downstream (from EPA) has an overall decreasing trend in the recent years because of the new establishment of WWTP. We are now initiating a new manuscript on this issue. On the other hand, the 2002-2005 data should be more representative to retrieve the real impacts of human activities.

We’ve added the supporting information and background parameters, i.e. landuse and census data, in the section of Materials and Methods.

Technical remarks

pp 2498-2499: “have emitted doubled”. I don’t understand this.

Since we rewrote the Abstract, the sentence no longer existed.

Pp 2499, 11: Give a more precise definition of Oceania.

In the third paragraph of Introduction, we specifically identified which Oceania countries we’re referring to in the manuscript.

Pp 2499, 16: change yet into they

Since we rewrote the Introduction, the sentence no longer existed.

Pp 2499, 25-26: Why the reason “and has relatively well resources for river studies”. Remove.

Removed.

Pp 2500, 4: city into City

Changed.

Pp 2500, 5-6: The findings rivers (references). I don't have seen any information to confirm this sentence. Remove.

Removed.

Pp 2500, 9-11: "most of global models may have very likely underestimated DIN export in the Oceania" This is too general formulated. Specify this or remove this highlight.

Reviewer is right. I shouldn't have had this statement in the Introduction. Hence, we've removed it in the revised version.

Pp 2500, 9 – 13. It seems to me, that this is a result. So move to results or discussion section.

We rewrote the Introduction to make the paragraph suitable in this section.

Paragraph 2.2. It is not clear to me, when the measurements are taken. In table 2 there are 8 or 9 measurements. But here (line 21) is mentioned "monthly" and "the same as EPA" and in 2003. So 8 or 9 can not cover the year 2003. In combination with pp. 2499 lines 16-19 about the typhoons, i have concerns about the coverage over the year 2003. Could you please clarify this?

Thanks for your careful reviewing. We rewrote the section of "water sampling and chemistry" to make it more clear. Basically, water samples were collected at different frequency yet covered both dry (Nov.-Mar.) and wet season (Apr.-Oct.). We added one more paragraph in Section 4.1 to address the influence of typhoon on DIN export from the experience in the neighborhood watershed. The year of 2003 is a relatively dried year without strong typhoon's invasion. We did not take any typhoon samples either in 2003. Hence, we illustrate the DIN yield in 2003, actually showing a lower boundary of DIN export of Danshui River.

Pp 2500-2501, 26-1: Community development ...coal mining. Nice to know but irrelevant. Remove.

Removed.

PP 2502, 1-12: Is temperature or land-use a factor in converting precipitation into discharge? This paragraph is very general description. It is an important step. Please make it more specific. In flux calculation you realize that there uncertainties involved. But for discharge, there are also uncertainties. Give also some information about this.

We removed the rainfall information which is relatively irrelevant in this study and to make Figure 1 clearer. Discharge, in a certain degree, could represent the rainfall pattern. Besides, observed

discharge data were used in this study to inherently consider the influences of temperature or land-use factors on discharge. The area proportion method to estimate ungauged discharge may not be so accurate but the bias should be minimized in our study watershed where longest water travelling time is less than 6 hours (much less in the channel) and the land-use pattern change among the adjacent watersheds is ~10%.

PP2502, 1-12: Table 1 is missing the upstream area for each station. Add it.

The column of “Area” of “Watershed characteristics” represents the upstream area for each station.

Pp 2502, 26 Change nitrate into DIN.

Changed and moved the entire section to the Supplementary Information.

Pp 2502-2504, paragraph flux calculations. Make this paragraph shorter. Move equations and further explanation to supporting information.

As suggested by the reviewer, we moved the descriptions of flux calculation methods to the Supplementary Information.

Pp 2503, 15: Change “water” in “DIN”?

Changed.

Pp 2503, 17 : Change weighted into weighed

We’ve look it up some references. It should be “weighted” and remains.

pp 2504, 5-13: Text does not match with formula $C=aQ^b$. Make consistent with formula 4. I do not understand the RC method. First it appears that it is used to make discharge on a daily bases, but in eq. 4 it gives an DIN flux. It is used on small mountainous rivers. Is that the case here? What is the value of a and b? This method does not make any sense. This formula is generally used to estimate discharge, but here it looks that DIN is generated.

We’ve modified our equation and now it reads “ $FLUX = m \sum_{j=1}^T (Q_j C_j) = m \sum_{j=1}^T [Q_j (a Q_j^b)] = m \sum_{j=1}^T a Q_j^{b+1}$ ”.

The formula $C=aQ^b$ is now in Eq. 4. The reviewer is correct that this formula is generally used to estimate discharge by replacing Q_j with water level. However, this formula is also used to estimate fluvial constituents’ concentration and mass movement a lot, not only for small mountainous rivers but large plain rivers (e.g., River Severn in Wales, England (Cooper and Watts, 2002) and Loire in France (Moatar and Meybeck, 2005)). In these cases, Q is known from the observation, and a straight line is fitted to the logarithms of C and Q to get a and b . Ferguson (1987) and Preston et al. (1988) listed several common methods in flux estimation, and rating curve is one of them.

We've also modified this paragraph to clarify the rating curve method.

<Reference>

Moatar F, Meybeck M. 2005. Compared performances of different algorithms for estimating annual nutrient loads discharged by the eutrophic river Loire. *Hydrological Processes* 19: 429-444.

Cooper DM, Watts CD. 2002, A comparison of river load estimation techniques-application to dissolved organic carbon. *Environmetrics* 13: 733-750.

Ferguson RI. 1987. Accuracy and precision of methods for estimating river loads. *Earth Surface Processes and Landforms* 12: 95-104.

Preston SD, Bierman JVJ, Silliman SE. 1988. An evaluation of methods for the estimation of tributary mass loads. *Water Resources Research* 25(6): 1379 - 1389.

Pp 2504, 24: 1593 - 2569 mm is not consistent with 2500-4000 mm (pp 2500, 18)?

Thanks for the kindly reminding. Since we removed the results about rainfall, we've removed this paragraph and kept the long-term mean annual rainfall, i.e. 2500-4000 mm, as the general description of Danshui watershed.

pp 2504-2505, 18 -3. To me, this is not a result. Move to section 2.

Removed as mentioned above.

pp2505, 4: The discharge in terms of runoff is very confusing.

Since we rewrote parts of the Results, the sentence no longer existed.

Pp2505, 6: What is rho?

Pearson Correlation Coefficient.

Pp 2505 and further. Change runoff depth into runoff.

Changed.

Pp2505, 4-7. I don't understand why S05, S07 and S12 are removed. Is this due the downscaling of discharge to a daily bases? How is it possible to have more discharge than precipitation? Could you explain this more?

Observed daily discharge was used in this study. Thus, the concern of downscaling doesn't exist. In this revised manuscript, we focus on DIN flux inferred from the observed discharge and DIN concentration, and hence removed rainfall relevant paragraph. However, we still explain here how it is possible to have more discharge than precipitation. It occurs when rain gauges within the watershed could not accurately reflect the precipitation in the watershed. Either inadequate numbers of rain gauges or inappropriate locations of rain gauges could result in poor estimation of watershed

precipitation. We haven't done any in-depth investigation on this issue which is not the focus in this study. Discharge, measured in the river with high confidence, is more important in this study.

Pp2505, 20: except D03. I think it would help when the load (kg N) is also presented in table 2. This could clarify this. Or not?

Since we included more data in the revised manuscript, we rewrote parts of the Results. However, we do not think the load in the river can directly explain the dilution or enhancement of DIN concentration in wet season. In wet season of Taiwan, the DIN load is always larger than in dry season, even if the DIN concentration in wet season is lower (i.e., dilution) while more water discharges to the stream. Dilution occurs whenever the net increase in water delivery to the stream is greater than the increase in DIN delivery. On the contrary, enhancement occurs. Regardless of dilution or enhancement in wet season, the load increases in wet season. Therefore, we didn't present the load information in Table 2.

Pp 2505, 17: I miss in section 2 the description of the population numbers and population density. Add it.

Added in the new Section 2.2.

Pp2505, 17: unit of population is different than in abstract. Make consistent.

We've made it consistent. Now it reads people/km², abbreviated as pl/km².

Pp 2507. The dilution part and higher concentration in wet season, could be proven with a load. I hope that adding the load would clarify these issues.

As mentioned above, at a single site higher DIN load is always the case in wet season, even if the DIN concentration is lower in wet season. It is not a direct way to explain the dilution or the enhancement. Therefore, we did not add the load information in these paragraphs.

Pp2508, 20: Landuse. What is the source of this? What is meant by bare land? Agricultural land? Explain this. But not here, but in section 2 (data and methods).

Added in the new Section 2.2. Bare land usually covers landslide and outcrop.

Pp2508, 21: remove and in "highly and positively".

Removed.

Pp2508, 26: It is not population density that regulates DIN but the activities related to population....

Since we rewrote parts of the Results, the sentence no longer existed.

pp2511, 13: “A modeling work” Which?

To be more specific, now the sentence reads “. The Ecopath with Ecosim software system was used to construct a mass-balanced trophic model for the Danshui estuary, also suggesting the estuary is a heterotrophic ecosystem”.

Pp2511, 27 change do into to

Changed.

From paragraph 4.3 including 4.4 and 4.5 i am very surprised that these paragraphs are placed here. It looks there is a description of a model development. But there are no sign in this paper, that this is the case. Besides that, these steps is “a step too far”. I have not seen any connection between this river and the whole of Oceania. This river is too small to compare this work with some global models. It does not make any sense at all. Going from river measurements to budgets is very speculative. There are no other sources used to provide more basis for this story. So i think skipping these paragraphs and rewriting the conclusion is an important improvement for this paper. I have stopped here to give detailed comments.

As mentioned above, we have reorganized the entire manuscript based on the reviewer’s comments. We believe that manuscript is now more straightforward and well linked to the inferences.