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Interactive comment on "A reactive nitrogen budget of the Bohai Sea based on an isotope mass balance model" *by* Shichao Tian et al.

Anonymous Referee #1

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The manuscript of Tian et al. presents a budget of nitrogen from the Bohai Sea in China and also measurements along two transects in the bay which are impacted by large river inflows. Samples for nitrate and POM analyses had been collected during spring and summer seasons and stable isotopes been measured. Fluxes in and out of the bay were modelled. Moreover, a budget using the LOICZ approach was constructed using measurements but mainly data from other studies. The budget balances sources and sinks of nitrate and is supported by stable isotope data of nitrate. As a central conclusion the overwhelming role of nitrification as the major source of nitrate is presented. Overall the topic is of interest and fits well to the scope of the journal Biogeosciences.

Unfortunately, there are several major problems with this manuscript affecting the combination of measurements with the budget, the equations, and the conclusion. The most significant error is the definition of equations (1) and (2). Sources and sinks are listed and supposed to be balanced. However, the source terms list nitrification and the loss terms list sedimentation. Both of these do not fulfill the criteria of a source or sink, respectively.

Nitrification is neither a source nor a sink for nitrate but simply a microbial process that converts ammonium via nitrite to nitrate. Nitrification does not generate new DIN for a system simply because the substrate of the nitrification process is ammonium and comes from internal turnover processes of organic matter. The LOICZ report (no 5 LOICZ BIOGEOCHEMICAL Modelling Guidelines, 1996) states "The important point to note with this reaction (nitrification) is that carbon and phosphorus are not directly involved in the net reaction. Again this makes the point that the relationship between NO3 and NH4 may be considered an "internal cycle" which need not be dealt with directly."

Sedimentation is also a problematic variable for a balanced budget because it is not a removal process for nitrate. Organic material in sediments is prone to remineralization and resuspension. Only if that organic matter is permanently buried it can be considered a long-term sink. The finding that nitrification in the Bohai Bay is a very active process – generating large quantities of nitrate - is not a surprise but rather typical for coastal eutrophied systems.

Another problem I have with this manuscript is that it actually consists of two independent stories; one is the field study of nitrogen compounds and stable isotopes in the Bohai Sea and the other the budget. Both are rather disconnected although the authors try to include some measurements in the budget. But data from only two seasons can hardly be used for a budget averaging annual mean fluxes. The field data are rather distracting from the main scope because they suggest that part of the budget is based on measurements although, most data are taken from other publications. Of course the authors write very clearly where the numbers for the budget are from and which underlying assumptions were applied to derive mean values. Nevertheless, the field data and budget remain two different stories. The results of the HAMSON model were not used at all and in the discussion the field data are only briefly mentioned.

The final major concern is the lacking error estimate of the budget. All field data are subject to some degree of major or minor inaccuracy, which is not analyzed and not included in the budget calculations. Point 4.3.3 is insufficient and only addresses single sources. What's needed is an error propagation estimate.

In summary, it is very sad to state that this study is neither conclusive and correctly done nor represents scientific progress – although a lot of effort has been invested by the authors. May be the field data can be presented elsewhere and the HAMSON model data used to constrain the "metabolism" of the bay in spring and summer. The budget may be corrected amended with an error estimated and submitted elsewhere. A clear research question then needs to be defined.

In case the authors decide to revise and refocus the manuscript, there are a smaller issues that would need consideration:

The introduction would benefit from a clearer focus and research hypothesis instead of an aim. In its present form the paragraphs read a bit isolated from each other.

Line 33 Reactive nitrogen is different to fixed nitrogen. While the first term summarises all bioavailable forms of nitrogen the latter is dedicated to diazotrophs.

Line 52 what is a "dramatic" increase of N/P ratios?

Line 61 it should be avoided to merge the process of nitrification into budget considerations

Line 71ff If a microbial process like nitrification is a major scope of a study it should have been measured during the field work. Including these rates could improve the study significantly.

Line 103 are the detection limits indeed as reported? They seem high to me.

Line 133 the model has a depth resolution of 1.5m, the field data seem to have a spacing of 5-10m. This mismatch should be solved as the model validation can hardly be done with the data gathered.

Line 145 The authors may not use two seasons only to extrapolate to an entire year. Here the data of other studies could be used to generate a full annual data coverage.

Fig 2 and 4 have blanks. How are does ODV generate these? Are the gradients of riparian data too large?

Line 166 Here I do not agree. The nutrient concentrations in spring are highly variable from 15-5 micromol L-1.

Line 168 Fig 4 and 5 do not present any phosphate concentrations – the reference to the figures is not correct

Line 170ff average concentrations of all stations and depth have been calculated. Although I understand why this is done it makes of course no sense when a thermal stratification, a clear river plume and other features exist. Rephrasing and explaining this would help.

Line 230 Sv unit should use superscript

Line 244 there is a typo r=. . .

Line 251 trace amounts are usually much lower than 0.5 micromol per liter which is the detection limit given.

Line 270ff the assumption of similar nitrate fluxes in rivers without data based on the regional vicinity seems doubtful to me. Is the land use similar too?

Point 4.2.2 this paragraph tries to explain away all uncertainties and assumptions but the potential error is likely very high. As said above – this and the other fluxes need to be treated using error estimates.

Point 4.2.3 was indeed the atmospheric deposition of entire China used? There should be tremendous differences across the country. May be I am misunderstanding something, but it seems that regional deposition data should be used. And again the uncertainty in the estimate needs to be included.

Line 326 unit

Page 18 and 19 the concerns explained above would need consideration to construct the budget differently

Line 363 is not a hypothesis but a well known fact

Line 456 delta as Greek letter. What about sediment resuspension and transport? Wouldn't that also blur any isotope signature?

Point 5 the conclusion would need a revision

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