Dear reviewers, dear editor,

We would like to thank the two reviewers for the generally positive feedback and constructive criticism on our manuscript.

Below, you can find all comments made (in black) and our individual responses (in green). We hope our responses and suggested changes satisfy the reviewers.

Kind regards,
Fabian Große

On behalf of all authors

Review #1 by Hagen Radtke

General remarks

The article has a clear scientific objective and is written clearly and concisely. It provides new insights into East China Sea hypoxia.

Unfortunately I see two major shortcomings which need to be clarified before the article can be published. The first one is about the appropriateness of using a simplification of the nutrient tagging method for this application. The second one is about insufficient model validation. If these can be fixed and the authors show that (a) the method is applicable and (b) the model has a sufficient quality in the parameters in question, I would recommend publication of the article.

Choice of the tagging method

I see a serious issue with the applied tagging method.

Problem

Your equation (2) presented in line 101 does not describe the element tracing method described in Menesguen et al. (2006) and Radtke et al. (2012), which deviates from the full equation.

[equations removed]

In your 2017 publication (”A Novel Modeling Approach to Quantify the Influence of Nitrogen Inputs on the Oxygen Dynamics of the North Sea”) you discussed this problem in a specific paragraph, but you do not state this difference here.

The effect is that in the simplified equation, mixing or advection of the tagged element is always driven by a gradient in the total concentration Cx. So your formulation does not allow a diffusion or advection of a tagged element Cx_i against the gradient of the total concentration Cx_i.

This is especially problematic in this application, where you try to investigate how much oceanic N enters the (N-richer) coastal area. Your simplification prevents this transport. In this way, the contribution of a ”local” source is systematically overestimated while that of a remote source is underestimated. I cannot see why it can be ruled out that this methodological error actually determines the result of your study.

Suggested solution

I suggest a simple experiment to quantify the impact of the simplification. In a first step, you initialize three passive tracers with the concentrations of

• p1 = riverine N,
• p2 = non-riverine N and
• p3 = totalN

at some single time step. Then you run the model for a few years and see how these spread. I expect that your numerical scheme will be linear and p1+p2=p3 will be maintained as it should.

Then, initialize two "active elements" (whose spreading is calculated by equation (2)):

• a1 = riverine N,
• a2 = non-riverine N.

with the same initial concentrations, using p3 as their "parent element". Make them practically passive by setting R_p1 = R_p2 = 0. If the simplification error is negligible, a1 should behave very similar to p1 and a2 to p2, and you should end up with very similar ratios of non-riverine N to total N in both methods. Then you could present this as a verification that your simplification error is small.

If my expectation is right and the results will show a significant difference, this would mean that you have to apply the full rather than the simplified method for this application.

Reply: Thank you for spotting this. In fact, the advection term in our Eq. (2) was written incorrectly and confused the differential term describing the change in the concentration at a location with the discretization of the calculation of the advective transports across the grid cell interfaces. We will correct this. Advection of a labelled tracer is calculated correctly, yet your statement on diffusion holds. However, we are aware of this and think it is reasonable to assume that the numerical diffusion from the applied first-order upstream advection scheme (MPDATA) is much larger than the turbulent diffusion, and thus the effect of this simplification is small. Your comment made us aware of a limitation of our treatment of advection (= multiplication of transport flux of unlabelled tracer with relative fraction of labelled tracer). Specifically, this approach only works correctly for advection schemes based on absolute concentrations, as we have used, but would yield incorrect results for gradient based advection schemes. We will include a statement on this in the revised manuscript.

Missing validation

You refer to an existing publication for the model validation. That is not sufficient. Your study relies on the assumption that at least the following is reproduced by the model:

1. lateral nitrogen transport, 2. oxygen consumption rates, 3. hypoxic area extent.

You should then present model validation that proves that the model is capable to do that. I am thinking of

1. DIN observations,
2. benthic chamber lander O2 fluxes or, if not existent, at least primary production rates as a proxy,
3. observational-based estimates of the hypoxic area.

Reply: Publicly available DIN and oxygen observations for the coastal areas of the East China Sea are sparse. However, we will include a validation of the spatial patterns of DIN (or nitrate) based on the available observations. Observation-based estimates of hypoxic area for individual years are also available from the literature and we will include these numbers in the revised manuscript. A qualitative comparison of simulated and observed bottom O_2 concentrations (both spatially and temporally) is provided by Zhang et al. (https://www.biogeosciences-discuss.net/bg-2019-341/; Fig. 3). A brief comparison of simulated sediment O_2 consumption with literature values is provided in the discussion of the companion paper by Zhang et al.

Specific comments

L38: The correct reference for the element tracing method is Menesguen et al. 2006: "A new numerical technique for tracking chemical species in a multisource, coastal ecosystem applied to nitrogen causing Ulva blooms in the Bay of Brest (France)". In the Menesguen and Hoch 1997 paper, a more general method for tracking multiplicative properties of model state variables was described which only later in the later paper was applied for element tracing.

Reply: One could argue that the element tracing is only a special case of the general method described in Ménesguen & Hoch (1997), but we will change the reference as suggested.

Figure 1: I suggest to change the color scale. Firstly, it guides the reader's focus to the location of the shelf edge only and makes it hard to distinguish topographic features in the tracing region. Secondly, a scale like that is typically used the opposite way, having the darkest shades of blue at the deepest locations, I would recommend to stick to this habit to make it more intuitive for the reader.

Reply: We will change the color scale as suggested (i.e., invert it).

L61-62: Instantaneous benthic remineralization is a good choice if (a) sediment biogeochemistry is in a dynamic steady state (carbon accumulation negligible) and (b) the area is so deep that lateral transport of resuspended organic matter does not play a role. Both assumptions seem questionable here, please discuss the possible implications on your model results.

Reply: Indeed, Song et al. (2016; https://doi.org/10.1016/j.dsr2.2015.04.012) determined (based on observations) that on average about 45% of the settled organic matter carbon (~14% of primary production) are permanently buried in the sediments of the East China Sea, although with quite some spatial variability. Consequently, simulated sediment O_2 consumption (SOC) may overestimate the observations, which is indicated when comparing simulated and observation-based SOC rates. This likely affects simulated near-bottom O_2, but we consider it having only a small effect on the relative contributions of individual sources to gross O_2 consumption (GOC), as this limitation equally applies to all labelled N sources.
Sediment resuspension may result in a lower riverine contribution near the river mouths, and higher contributions in more distant areas (vice versa for oceanic contribution). However, except for typhoon events, wind speed (and thus resuspension) is generally lower during summer. Song et al. also state that resuspension may particularly play a role in fall when wind speed starts increasing with the change in the monsoon cycle. We will include this in the discussion of the revised manuscript.

L75: Please specify which rivers you prescribed, maybe by adding them as dots in Figure 1 or by supplying a table with their mouths' coordinates in the online supplement.

Reply: We will provide a Table with the river names and mouth locations in the revised supplement in order to not overload Fig. 1.

L86: Please state earlier than in the ”Discussion” section what motivates the reduced-oxygen scenarios and why you choose a 20% reduction.

Reply: Yes, we will include this in the scenario description in the Methods section of the revised manuscript.

L122: The TN concentrations are actually monthly, daily values are only obtained by interpolation, correct? Please also change the caption of Figure 2.

Reply: Yes, river load concentrations for the Changjiang River are monthly data from Global NEWS. Only freshwater discharge is daily. We will correct this in the caption.

Table 1: How is ”anoxic area” defined?

Reply: Anoxic area is defined as the region experiencing O_2 concentrations of 0. We will add this in the text of the revised manuscript.

Technical corrections

L59: Citation style is wrong here, please use the ”citep” command if the reference can be omitted without changing the meaning of the sentence.

Reply: Will be corrected.

L138: A comma is missing after ”South of 32°N”.

Reply: Will be corrected.

L278: A comma is missing after ”e.g.”

Reply: We consistently use no comma after “e.g.” (like on the Biogeosciences website (https://www.biogeosciences.net/for_authors/manuscript_preparation.html; “English guidelines and house standards”)

Review #2 by anonymous referee

This manuscript quantified the contribution of nitrogen from Changjiang and open ocean (Taiwan Strait and Kuroshio) to the hypoxia formation in the East China Sea and proposed the reduction of nitrogen from river as an efficient way to avoid hypoxia. In general, I can follow this manuscript. However, I also found many points needed to clarify before I can recommend its publication.

General comments

1. Do you include the particle organic nitrogen from rivers? On line 61, you mention only dissolved organic matter (DON) but show TN in Fig. 2. If your TN includes particle organic

nitrogen, how did you determine the proportion of PON, DON and DIN (NO3 and NH4) in your input data of TN?

Reply: Yes, the river forcing includes information for small and large detritus (=PON), DON, NO3 and NH4 (=DIN). However, actual forcing data is only available for NO3 and NH4 (from Global NEWS). For the 3 groups of PON/DON constant concentrations were applied. We will include those in the description of the river forcing in the Methods section.

2. Consumption of oxygen by sediment is an important factor affects formation of hypoxia. What is your sediment condition? There is only one sentence (line 62) saying it but it is not enough.

Reply: The statement on line 62 implies that all organic material that sinks to the seafloor is remineralized immediately, with a fraction of 75% being lost to dinitrogen via benthic denitrification (Fennel et al., 2006; https://doi.org/10.1029/2005GB002456). We will clarify this in the description of the biogeochemical model and include a paragraph on potential implications of this relatively simple approach in the discussion of the revised manuscript.

3. You mention the importance of winds in the interannual variations. However, the change of wind speed in Fig. 5 is very small (<2 m/s?). Would you like to present more evidences for the processes related to winds? For example, you mentioned changes in flow field and turbulence but did not show any figures for these changes.

Reply: In terms of absolute numbers, the year-to-year differences in wind speed are indeed relatively small (1-3 m/s). However, considering the discussed events, e.g. September 2008 vs. 2013 and June 2013, it can be seen that the relative change is quite significant as absolute wind speed does not exceed 4 m/s (during these events). In the supplement (Fig. S2), we provided time series of monthly averaged potential energy anomaly (PEA; a measure for water column stability), which implicitly reflects changes in turbulence as vertical mixing is reduced under more stable conditions. This is discussed on lines 180-188. Along with the PEA time series, we show time series of freshwater thickness associated with the Changjiang River discharge. We use freshwater thickness is a measure of the total amount of Changjiang freshwater in the top 25 m of the water column. Changes in freshwater thickness can only result from changes in lateral transport and in discharge from the Changjiang. However, the discharge is quite similar in the first half of 2008 and 2013 (see Fig. 2), thus differences in freshwater thickness between the two years need to be due to differences in transport of freshwater from the Changjiang to the southern analysis region. In addition, changes in freshwater thickness and PEA (Fig. S2) clearly coincide with anomalous wind events (Fig. 2). We think the effect on stratification/turbulence is sufficiently addressed by the PEA time series. However, we will consider including an example for the effect of wind on surface currents for one or two of the discussed events in the supplement (similar to Fig. S1).
Is this sufficient?

4. You emphasized the importance of Changjiang in this study. However, you actually did not consider the interannual variations in the Taiwan Strait and Kuroshio region because you used a nudging to climatology there. The same thing also occurs for the nitrogen from Yellow Sea. Therefore, your conclusion is not fair.

Reply: It is correct, that we do not fully resolve interannual variations in the nitrogen supply from the oceanic sources due to the nudging of nitrate concentrations to a climatology. However, the nutrient supply from the Kuroshio occurs primarily in the subsurface, with open-ocean subsurface concentrations showing significantly lower absolute values (e.g. Liu et al., 2016; http://dx.doi.org/10.1016/j.jmarsys.2015.05.010) and lower variability than coastal waters with river influence. Therefore, variations in volume transport of Kuroshio intrusions are likely the main cause for interannual variations in nutrient supply from the Kuroshio. These are resolved by the model. Similarly, nitrogen concentrations in Taiwan Strait (e.g. Chen et al., 2004; https://doi.org/10.1016/j.marchem.2004.01.006) are significantly lower than in the river inputs (by factor 10 to 100). Therefore, we consider the effect of interannual variability in nitrogen levels in the oceanic sources small compared to the variability in the river loads. We will include this in the discussion of the revised manuscript.

5. What is background for reduction of O2 in the open ocean by 20%? It is better for you to check the papers for DO change at 137E line for some evidences.

Reply: This 20% reduction corresponds to the reduction in subsurface O_2 levels in the northeast Pacific projected by Earth System models (Bopp et al., 2017). We are particularly interested in potential future changes in the O_2 conditions off the Changjiang. We therefore base our scenario on these future projections rather than observations of past changes. As suggested by reviewer #1, we will clarify this already in the scenario description in the Methods section.

6. I did not find figures showing interannual and seasonal variations in spatial variations of bottom DO concentration from your model. Apparently, they are important to your model validation because you can find some observations showing such figures. Without a serious validation of model results, no people in China can follow your suggestion on reduction of nitrogen input by 50%.

Reply: This model-data comparison is provided in the companion paper of Zhang et al. (https://www.biogeosciences-discuss.net/bg-2019-341/; Fig. 3). We consider it redundant providing the same analysis in this manuscript. However, we will expand the discussion of the revised manuscript with respect to model agreement with observations and explicitly refer to this companion paper at the appropriate locations.
We further like to stress that the 50% reduction scenario is only a single model realization and does not suffice to make actual recommendations. We ran this scenario to obtain first insight into how the system may respond to nitrogen load reductions. The 50% reduction was chosen in analogy to Zhou et al. (2017; https://doi.org/10.1016/j.marchem.2017.07.006) who did not consider sediment O_2 consumption in their model, thus missing relevant parts of the system. This is also discussed on lines 267-270.
From our point of view, the strongest statement with respect to the potential impact of river load reductions on hypoxia reads as follows in the original version of the manuscript:
"Our analysis of the changes in hypoxic area and hypoxic exposure under reduced Changjiang River N loads (see Fig. 6 and Table 1) underlines the high potential of riverine N load reductions to mitigate hypoxia." (lines 263-264)
We will rephrase it to:

"Our analysis of the changes in hypoxic area and hypoxic exposure under reduced Changjiang River N loads (see Fig. 6 and Table 1) **suggests a** high potential of riverine N load reductions to mitigate hypoxia."
As we only ran a single reduction scenario, we are fully aware that we are not in the position of making an actual recommendation and we do not mean to be prescriptive in any way.

Specific comments
Line 29-31: This statement is not correct.

Reply: We will rephrase the last part of the sentence to: "with strong southwestward winds in winter and weak northwestward winds in summer supporting stronger northward water mass transport in summer than in winter."

Line 74: please use full spell for 'FW'.

Reply: "FW" is first introduced on line 20. After that we consistently use "FW" instead of "freshwater", which we would like to keep.

Line 84-86: ". . .the initial and open-boundary O2 concentrations were reduced by 20% throughout the water column in regions deeper than 200 m. . ." How much O2 reduction from the Kuroshio boundary or Taiwan Strait boundary?

Reply: We only reduced the O_2 levels at the open boundaries of the model domain (see Fig. 1 in the manuscript) in regions deeper than 200 m. We will clarify this.

Line 110: ". . .Minjiang, Hanjiang and Oujiang Rivers; grouped into one source. . ." You mean Hanjiang River or Qiantangjiang River? In Figure 1, Hanjiang River is not inside the tracing region. How did you trace the N of it?

Reply: We accidentally put a wrong river name, it has to be "Qiantangjiang River", which will be corrected.

Line 112: What is your evidence for that the tracer cannot reenter the tracing region?

Reply: This is owed to the tracing setup, which does not keep track of the origin of a tracer once it leaves the tracing region. In reality, nutrients could be recirculated into/re-enter the region. We will clarify this.

Line115: "… To spin up the tracing, we first re-ran year 2006 three times. For the first iteration, all N mass already in the system was attributed to the small rivers." What's the purpose of doing this?

Reply: This is done to spin up the model (it is common practice to do so). Note that we do not have information on the actual distributions of nitrogen from the different sources in the region. Therefore, we have to start from an arbitrary distribution for which all nitrogen tracers are attributed to the small rivers (any other of the traced sources would be equally good). We then run the tracing multiple times (3 times in this case) with the same forcing until we achieve a statistical steady state meaning that the distributions of tracers associated with the different sources do not change between December 31 of two subsequent iterations. At this point the model is considered as spun up. This way we make sure that our results are not affected by the arbitrary initial distributions. This is also stated in lines 117-119, but we will try to make this more clear.

Line125: Figure2. In 2009, 2011, 2013, the Changjiang discharge and TN concentration seem to have the similar trend, but 2010 and 2012 the opposite. Why does this happen?

Reply: This is a good question, to which we don't have a sure answer. To some extent, this could be a result of combining information from two different sources (Global NEWS for nitrogen concentrations, Datong gauge measurements for discharge). However, more likely this relates to the strong river floods in 2010 and 2012 (indicated by the much higher discharge peaks in both years compared to the other years). However, we could not find literature explaining this in more detail and it is outside of our field of expertise to answer this question (and outside of the intended scope of this manuscript).

Line 135: do you have any data to verify the GOC given here? Supplement: what is your purpose to show PEA/D not PEA itself?

Reply: We do not have data for GOC but we will include a comparison of simulated sediment O_2 consumption with observation-based estimates in the discussion.
PEA increases with increasing water depth, which would give stronger weight to deeper regions within the analysis regions. To avoid this, we show PEA/D accounting for this spatial variability of water depth.