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Comment

***Interactive comment on*** “Natural and  
human-induced hypoxia and consequences for  
coastal areas: synthesis and future development”  
**by J. Zhang et al.**

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General comments: This is a well-written and well-structured manuscript reviewing our current knowledge on coastal hypoxia. As several reviews on coastal hypoxia have been published already this ms partly re-iterates previous reviews and extend beyond, particularly in synthesising the biogeochemistry. Overall, I enjoyed seeing this bulk of knowledge put together although there was also a large fraction of “dejavu”. However, I think that the feedback mechanisms of the benthic biota on eutrophication could deserve more attention, just as much or perhaps even more than the feedbacks on the climate. Knowing that several of the authors have suggested regime shifts associated

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with events of hypoxia (hysteresis behaviour also proposed by e.g. Diaz and Rosenberg 2008) I was slightly disappointed not to see this addressed. Section 6 describes coastal hypoxia and benthic biota mostly as a one-way relationship, and I would encourage the authors to extend this section to also describe how changes in the benthic community affect the biogeochemistry of the sediments. In general, most of my comments below have arisen from my own curiosity to have more information on specific issues that I would like to see expanded, rather than disagreements with the views and formulations. The authors should be able to address most of these comments, and I therefore recommend publication after minor revision.

Specific comments:

1. Page 11038, l. 10-13: I would suggest making this more specific by adding something like “enhancing emissions of greenhouse gases to the atmosphere”, since “active feedbacks” is too general and doesn’t say much.
2. Page 11045, bottom of page: After reading the last sentence the next question naturally arises: what is needed then to disentangle eutrophication from trends in the physical forcing? This question is answered later on a more general level, but I would suggest that the authors elaborate on this question at this point in the ms.
3. Page 11046 and Figs. 2 and 3: The number of reported coastal hypoxic sites is a pragmatic but not a “true measure” of the hypoxia problem. The authors should acknowledge this, particularly in Fig. 3 where a correlation between fixed nitrogen and hypoxia is implicitly assumed. The number of publications in the ecological literature or natural sciences has probably increased at a similar rate over time, so strictly speaking it is difficult to make explicit inference to hypoxia as an increasing global problem, although I certainly believe it is. Moreover, the number of hypoxic sites in Fig. 3 – where do they come from? Are they the >400 sites from Diaz and Rosenberg (2008) partitioned into decades?
4. Page 11051, l. 12-13: I assume that this 3-times increase is due to further reduction

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processes under anoxic conditions (that are described in the following text). Explaining this at this point in the ms will help the reader better understand the sentence!

5. Page 11054, l. 22: Here, it is suggested that atmospheric feedbacks can have a strong impact on climate change! On page 11058, it is stated that N<sub>2</sub>O is an important contributor to greenhouse gasses, whereas CH<sub>4</sub> is moderate! Then on page 11060, l 5-10 it is stated that the ocean's contribution to CH<sub>4</sub> is insignificant. Knowing that these two atmospheric constituents combined are responsible for ~10% of the overall greenhouse effect, I cannot see that the atmospheric emissions have a strong feedback to climate change, probably a considerable impact, I'd say. Moreover, I would suggest moving section 11 up before section 8 and integrating these two sections better, because many site-specific results are presented in section 11 which are then scaled-up and discussed in both section 8 and 11. Hope this was clear!

6. Another issue that I think the authors should address is the combined effect of low oxygen and high pCO<sub>2</sub> that will further increase the "dead zones", particularly in connection to the OMZ (See Brewer & Peltzer 2009, Science 324: 347-348).

7. Page 11059, l. 28 ff: I don't see the point here. Elevated CH<sub>4</sub> concentrations in the plume of the Yangtze River are most likely due to terrestrial inputs, but where is the link to hypoxia and CH<sub>4</sub> releases from the sediments caused by hypoxia. I would suggest removing sentences until page 11060, l. 5. Moreover, my knowledge of the Chinese geography is limited, so I would suggest restating where Kuroshio is located.

8. Page 11064, l. 16-20: The authors focus only on the potential negative effects of altered wind patterns, but there could be positive effects as well. The authors should acknowledge this and give a more balanced presentation.

Technical corrections:

1. Page 11042-43: Use "Fig. 2a" and "Fig. 2b" in the text to distinguish the panels.
2. Page 11054, l. 27: Should be "through".

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3. Page 11056, l. 22-28: Repeated sentence.

4. Page 11061, l. 14: Should be “advection”.

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Interactive comment on Biogeosciences Discuss., 6, 11035, 2009.

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