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Biogeosciences Discussions

Interactive comment on "Tracing the origin of the oxygen-consuming organic matter in the hypoxic zone in a large eutrophic estuary: the lower reach of the Pearl River Estuary, China" *by* Jianzhong Su et al.

Anonymous Referee #2

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This paper determines the origin of the oxygen-consuming organic matter in the hypoxic zone of Pearl River Estuary, China. The approach is the same as that used for the Changjiang Estuary (ES&T 2016), utilizing C-13 value for the increased DIC in the hypoxic zone. I think that the approach is technically valid and provides useful information regarding the cause of hypoxia so as to make right policies for hypoxia remediation. However, authors do not show any advances in data analyses and interpretation, compared with the previous ES&T paper. Authors found that 73% of the oxygen-consuming organic matter is of marine source (the rest portion is terrestrial), which is different from the hypoxia in the Changjiang Estuary (100% marine organic matter contributions). Al-

though authors do suggest some speculations for the cause of this difference, they do not go through thorough data analyses and interpretations about this issue. I suggest following trials to better interpret the data.

(1) Authors may compare all C-13 data for POC and DIC between the two estuaries together in the same plots, so that any systematic difference between the two estuaries can be examined.

(2) When I look at both data sets from the two estuaries, the difference in the contribution of marine organic matter for the two estuaries is within the uncertainties of this approach. Depending on how to omit the outlier in the relationship (Fig. 7 vs. Fig. 6 of ES&T), the proportion of marine organic matter varies significantly. The ES&T paper shows a large scattering for the slope (if hypoxic zone is collected separately) but reduced the error by including the subsurface layer data which is not reasonable. Therefore, I am not convinced with the argument that the sources of organic matter in the hypoxic zone of the two estuaries are different.

(3) I think that most of the oxidation happens in the surface sediment layer. In order to determine the reason for the difference between the two esturaries, authors should show surface sediment C-13 data (any difference between the two estuaries?). Otherwise present any difference in the characteristics of organic matter (C3/C4 plants vs. marine OM) in surface sediments.

(4) Authors state that "We chose the concentrations of Ca2+ as a conservative tracer to validate our model prediction, and the model values were in good accordance with the field-observed values..". In order to choose a conservative tracer, authors may use another conservative element instead of Ca which is not necessarily conservative in the coastal system. I think that Ca should be used to examine the effect of CaCO3 in this system, proving that no addition or removal of DIC associated with CaCO3. This result suggests that the DIC change in the hypoxia is solely owing to the organic matter dissolution.

(5) Although the contribution is relatively small, authors should account for the contribution of DOC in this system.

Minor comments

- Authors do not use correct significant figures for DO and DIC (i.e., 153.1 umol/kg for DO and 1900.7 umol/kg for DIC). Otherwise, do you measure such accurate numbers?

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