

Interactive comment on “Hypoxia disaster in waters adjacent to the Changjiang estuary” by X. F. Luo et al.

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Response to Review

We thank both reviews for very constrictive comments and suggestions. These are very helpful in guiding our revisions of the manuscript. We make the efforts to thoroughly revise in order to improve the presentation of our data analysis and the interpretation of results. A point-to-point response to comments is provided below. Reviewers' comments are in blue font and our response is in black font. Please also note that we have changed the title of this manuscript to “On influencing factors of hypoxia in waters adjacent to the Changjiang estuary” and added an additional author (Youyu Lu).

Reviewer 1: The manuscript is based on a series of hydrological surveys in the region adjacent and offshore the Changjiang estuary, an area that suffers from frequent hy-

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poxia/anoxia episodes. The background information, provided by the Authors in the introductory part of the manuscript, inform the reader that hypoxia development is due to the classical triggering factors: enhanced vertical stratification and organic matter accumulation and remineralisation in the lower water column, but they state that the timing of the hypoxia onset, as well as the location of the main hypoxia center is determined by the interplay between the water masses of the region. The main effort of the paper is about an effort to define the spatial and temporal hydrological characteristics underlying development of the hypoxia events as well as their location. I have to say that despite the rich and extensive dataset assembled, the effort is not successful because of a rather confusing analysis and description of the data collected. Therefore, I do not recommend the publication of the manuscript in its present form. Below I list a series of remarks that hopefully the authors might consider in addressing the very serious major revision that the manuscript needs in order to be considered for publication in future.

R: We thank this reviewer for the very critical review that points out the weakness in our analysis. The detailed comments are fully considered in our revision. (See our response below)

1) The manuscript reads very much as a technical report rather than as a scientific paper. There is a long description of the paper figures that unfortunately does not help very much the reader to understand the following considerations.

R: Following the reviewer's comment, we have made significant effort to simply description of observational data (figures) and enhance the statistical analysis and interpretation.

2) The hydrology of the region (Water mass distribution and pathways of the main currents) is described by means of a qualitative cartoon only. However, in the following the distribution of the observed hydrological properties (T S and DO) is related to specific water masses. Unfortunately the reader not knowledgeable with the oceanographic

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characteristics of the region, cannot fully understand and assess the dependence of the hypoxia onset and location on the basis of the changing hydrology. It is therefore strongly recommended that the authors define more strictly the hydrological properties of the water masses involved in the hypoxia dynamics of the region (the large quantity of data they collected should enable them to provide (for instance) T-S diagram whose analysis can help to define in a quantitative way the interplay among water masses. Also the use of T-DO and/or S-DO diagrams could greatly help the analysis and the considerations about hypoxia timing and location.

R: Following this very insightful and constructive suggestion from the reviewer, we have made substantial effort to improve the analysis of relationship between water mass and DO distributions. First, Figure 1 has been revised to denote the major branches of summer circulation that contribute to the shaping of hydrologic conditions in the region. We divide the focused study area into regions I and II, to simplify the description in the main text and ease the appreciation by a reader who is unfamiliar with the regional oceanography. We further add a new Figure 9 to explore the variations of DO with respect to T, S and N2. These new diagrams show clear grouping of observational data into various major water masses in the study region. Finally, the revised new Figure 10 (formerly Figure 9) illustrates the seasonal evolution of DO in region I corresponding to the changing stratification condition. 3) The figure accompanying the manuscript are very poor and confusing. Again the large quantity of data they collected should deserve a better analysis, based (for instance) on an objective analysis procedure, who would allow the author to define better the location of the water masses. From a formal point of view the combined use of isolines and "colored" dots is adding confusion.

R: The new Figure 9 and revised Figure 10 present objective analyses of the large quantity of data following the suggestion of the reviewer. In Figures 3 and 5-8, the presentation of both color dots and isolines is kept to ease the recognition of spatial gradients by a reader.

4) As it is now it is very much difficult to relate the main conclusion of the paper

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with the data described and analysed earlier. This is due, as stated above, to the poor treatment of the data and the generic analysis procedure.

R: After a major revision following the comments and suggestions, we hope that the connection between our analysis and conclusion is more explicit. We thank the reviewer again for the time spent on our manuscript.

Please also note the supplement to this comment:

<http://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2016-59/nhess-2016-59-AC1-supplement.pdf>

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., doi:10.5194/nhess-2016-59, 2016.

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