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Biogeosciences Discussions

Interactive comment on "Dynamics and distribution of natural and human-caused coastal hypoxia" *by* N. N. Rabalais et al.

Anonymous Referee #2

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The causes and dynamics of hypoxia in marine ecosystems have been the focus of accelerated marine research interest in recent years and I have been particularly looking forward to the outcomes of the SCOR working group effort on this topic. Given the expertise assembled for this effort, expectations for novel, synthetic insights are high. There have of course been a number of extensive reviews on coastal hypoxia (many by the authors themselves) and a key challenge for the authors is to provide a review that offers insights that are novel given the volume of review literature already published. To this extent, I found the consideration of hypoxia across the spectrum of natural to human-induced to be a most compelling framework for capturing new insights. Much has been written about natural or human-induced hypoxic systems, but the emphasis has been to consider the two classes of systems in isolation. As the authors note in the conclusion, the range of dominant time scales of variability afforded by physically and biogeochemically divergent systems provide a rich context for understanding hypoxia dynamics currently and into the future. I couldn't agree more and the compilation and discussion of the dynamics that underlie hypoxia formation across upwelling shelves, enclosed basins, OMZs, and estuaries can alone represent a valuable contribution. This review is certainly considerable in scope and provides a largely well-written and authoritative source for introducing the diversity of systems that are impacted by hypoxia.

While I am supportive of the publication of this review, I do wish to suggest changes that can strengthen its value. In particular, while the diverse time scales of variability exhibited by hypoxic systems was highlighted as a particularly productive framework for interrogating hypoxia dynamics, I found little explicit use of this framework for review of the various systems considered. My reservation here is that this review runs the risk of being less defined by elements of conceptual synthesis than by the reiteration of already published reviews as well as case studies that have been largely well described elsewhere. Description of case studies is of course central to reviews but there can be a missed opportunity for real synthesis here. Otherwise, I do struggle with the fact that large sections of this manuscript do appear to be a review of already published reviews. My recommendation would be to organize the case studies more explicitly around key frameworks (be it dynamics in terms of scales of variability, causalities, and/or sensitivity to climate...etc.) that the authors have themselves highlighted so that commonalities and differences across the broad spectrum of systems can be examined in the clearest and most succinct terms.

My specific comments are below: p.9374, l.1: states that OMZ boundaries are controlled by natural cycles, but citations noted later such as Stramma et al. (2008) and Mataer et al. (2000)...etc and figure 8 speak to the possible role of climate change on the scope of oceanic OMZs.

p.9374, l11: We often associate oceanic OMZs with upwelling and causally link upwelling induced export production to the formation of OMZs but when one looks at the distribution of OMZs, the presence of OMZ across the north pacific (extending to the western boundary currents) would suggest that large reaches of OMZs cannot be explained by upwelling-export production-respiration coupling. It is likely that the persistence of hypoxia in the ocean interior simply reflect the long residence time of those isopycnals within unventilated depths.

p.9375, l.25: That hypoxia lags fertilizer increases by ca. 10yrs is an interesting idea, but it is not clear to me how well supported this is. In particular, the idea that it takes a decade for excess organic matter from primary production to buildup sufficiently to cause hypoxia seems to be at variance with correlations between interannual differences in spring-time nutrient loading and hypoxia area in the northern Gulf of Mexico (and similar relationships elsewhere). Maybe there is some threshold dynamics that is implicit in this lagged response idea, but I would like to see more support presented.

p.9376: Given the extensive scope of the Humboldt current low oxygen zone, and the many excellent work that have described the dynamics of hypoxia in those systems in terms of climate forcing, biogeochemical cycling, fisheries effects…etc. I find the paucity of information surprising and unbalanced relative to the other case studies.

p.9376, l. 7: The billfish habitat compression idea is quite intriguing. Can references be provided? I for one would like to follow-up with a reading of that literature.

p.9376, l. 25: replace “sinks to a pycnocline” with “sinks to below the pycnocline”

p.9377, l.5: anoxia is described for the California current as well elsewhere in the paper

p. 9377, l. 14: Elsewhere in the text, the point that climate change can affect hypoxia in upwelling systems is advanced (also see Andy Bakun’s hypotheses on this topic). I’m looking for a consistent treatment as to whether hypoxia in these systems are “not aggravated by human activity” or not.

p. 9378, l. 13: I am aware of the proposed linkage between global warming and upwelling wind stress but am not aware one between global warming and jet stream

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dynamics. Can the authors elaborate?

p. 9382, l.18: “but the oceanic supply is probably more uniform over the long term” I’m flagging this because I find the discussions and ideas about climate sensitivity with respect to hypoxia to be particularly interesting and novel. However, see two points being advanced at the same time in the paper. One is that oceanic dynamics are relatively static and insensitive to changes and the second is that in fact anthropogenic climate change can have important alterations on oceanic and coastal processes. If we set aside climate change modeling projections, given our understanding of the influence of climate cycles such as ENSO, PDO, NAO, NPGO on ocean physics and biogeochemistry, conjectures about low frequency uniformity in oceanic inputs or dynamics are probably not well supported or at least requires more in depth explorations.

p. 9383, last line: The statement about feedbacks is a bit vague. What aspects of negative feedbacks are being referred to (enhancement of denitrification?)? What of the positive feedback on sediment P-flux that is discussed later?

p.9391, l. 12: I am very curious about the processes that makes hypoxia in a system more susceptible to N-loading. Can the authors elaborate what the proposed mechanism(s) maybe as it would be a very important element of hypoxia dynamics? Alternatively, could the decoupling between N-loading and hypoxia be interpreted not as enhance susceptibility to N-loading but rather as changes in physical forcing or shifts to P or Si limitation?

With respect to the figures, 25 figures seem a bit excessive and not all figures are essential for illustrating the points raised by the text. I would recommend a judicious pruning.

Interactive comment on Biogeosciences Discuss., 6, 9359, 2009.