Biogeosciences Discuss., 6, C2711–C2732, 2009
www.biogeosciences-discuss.net/6/C2711/2009/

Biogeosciences Discussions

Interactive comment on "Coastal hypoxia responses to remediation" *by* W. M. Kemp et al.

W. M. Kemp et al.

jtesta@hpl.umces.edu

Received and published: 27 October 2009

Reply to Comments on Manuscript "Hypoxia Responses to Remediation" by W.M. Kemp et al. by Reviewer #3 Anonymous

This reviewer has provided many useful detailed comments and editorial suggestions, and we have revised our ms accordingly. The primary critique of this manuscript, which is repeated throughout the review, is that the term "remediation" is inappropriate in our title. We agree with this comment, and acknowledge that there are indeed few examples of clear reductions in anthropogenic nutrient loading to hypoxic coastal systems aimed at improving O2 conditions. We also agree that the real purpose and scope of our paper may not have been stated with sufficient clarity. In the revised ms, we have thus modified the title, edited the narrative and clarified goals and objectives. We believe that these changes have greatly improved the ms and established the unique-

ness and importance of our review of theoretical hypoxia response trajectories and mechanistic controls on hypoxia in the context of several case studies that illustrate the range of temporal patterns of coastal hypoxia in relation to changes in nutrient loading, ecological processes, and climatic conditions. It should be noted that we have listed and numbered the reviewer's comments (both general and specific), and we have responded to each of these comments with narrative.

General Comments. (1)The title of this ms is a worthwhile venture, either with theory, mathematical models, conceptual models, and the limited data that are available.

As per later suggestions by this reviewer, we have changed the title of our chapter to reflect more clearly the actual scope of this manuscript.

(2)But, there are few examples of remediation provided. Most of the obvious trajectories for remediation (although the worsening trajectory is not shown) are for organic loading. The more incipient nonpoint source loading is harder to remediate. There are limited examples of real remediation of human-caused nutrient pollution that has led to reduction in hypoxia and other symptoms of nutrient-driven eutrophication.

We agree with the reviewer's comment, and now recognize that the term "remediation" does not really convey the overall scope and goals of this manuscript. We have, thus changed the manuscript's title and added clarifying text to the manuscript. The new title is: "Recovery and recalcitrance of coastal hypoxia in response to nutrients and physical controls"

(3)There is only one example of an economically-driven reduction in nutrients and a real ecosystem response, most obvious in hypoxia reduction, but other aspects of the nw Black Sea shelf are not "recovered." The ms should be clear about the forces and distinctions for the shelf area and the open basin area.

We agree that the term "remediation" may be misleading in this case. Webster's New World Dictionary defines the verb, to remedy, as follows "to cure or heal, as with

medicine; to put back in order (put right); to correct or remove." All of these definitions imply a direct human intervention to restore a thing to its correct or natural state. The case of the Black Sea can be considered an inadvertent, rather than a purposeful, remediation, where the collapse of the Soviet Union ended use of expensive chemical fertilizers, and resulted in unintentional reductions in nutrient loading from the Danube River to the Black Sea.

To eliminate this ambiguity, we have removed the term remediation from our title and from most of the ms narrative. We have also added further clarification that we are referring to the shelf region and not the open basin area.

(4)The descriptions of well-documented coastal hypoxic areas and their nutrient dynamics are good, but are not the focus of this ms, at least not the title. The ms is more of a discussion of nutrients, ecosystem changes, including hypoxia, and little with regard to remediation, or what conceptual models might be application to a remediation that might eventually be accomplished.

Again, we understand the reviewer's concern and confusion revolving around the term remediation. We believe that this misunderstanding has been eliminated in the revised ms. We have also added clearer reference to Section 4 in our discussion of case studies to relate observed trends in hypoxia to theoretical trajectories.

(5)The general descriptive material in general and the case studies that do not show any decrease in nutrient or organic loading or attempts at remediation should not be part of the ms. The Chesapeake Bay is relevant because remediation has taken place, but with limited success. The details of formation of hypoxia there are not as important as the responses to nutrient reductions, and those aspects of the ecosystem that have not responded.

Again, the reviewer's perception that the manuscript was intended to focus narrowly on "remediation" has led to this apparent misunderstanding. One part of the manuscript's case-study discussions is intended to focus on a few coastal systems exhibiting strong

non-linear temporal trends in hypoxia in relation to time-courses in nutrient loading. We wanted to bring this interesting phenomenon to the attention of the BG readership, and the NGOM is a very important example of this poorly understood behavior.

(6)The idea of multiple stressors is a good one to bring into the discussion of expected outcomes of remediation. More on time lags, aquatic or terrestrial or both, would be useful. I recommend stick to ecosystems that have seen reductions, either managed, remediated, or accidental, and further examine, try to reconstruct the trajectories in both directions, and consider other aspects of ecosystem structure and trophic interactions in addition to improved oxygen conditions.

We agree that it is important to focus on more than one controlling factor, particularly in the cases of NGOM and CB where highly non-linear trajectories of hypoxia per N loading have been shown. We maintained the case studies of systems without clear remediation (consistent with the revised title of the manuscript) – and most all of the case studies emphasize the role of multiple stressors. We will maintain our considerations of how changes in ecosystem structure might have/will affect oxygen conditions, but discussion of the responses of ecosystem structure to improved or maintained hypoxic conditions is beyond the scope of this manuscript.

(7)The Conclusion brings in many new ideas that could be more fully explored in the text, than examples of increasing hypoxia with increasing nutrient loads.

We agree that we found surprisingly few case studies in published literature where parallel time-series data are presented describing trends in O2, nutrient loading and other potent controls. We believe that there are probably many other cases of hypoxic systems where requisite time-series data exist but have not been made public and or published in the peer-reviewed literature. One of our purposes, which is now much clearer in the revised manuscript, is to emphasize the need to examine these buried data sets to quantify hypoxia response trajectories in relation to changing loading rates, climatic conditions, and ecological processes. We have clarified the text of the con-

cluding section, where we make reference to previous sections of the manuscript that express these ideas.

(8)The ms needs to focus on the topic of response of ecosystems, particularly in hypoxia, to reductions in organic or nutrient loading, or the stated focus of the ms needs to change. Much material is unnecessary or too detailed.

Again, we agree and have clarified the document's scope and goals in the revised ms.

Detailed comments (9)My first response to this title, was "what remediation?" from the case studies, Patuxent River, yes some localized success; northwestern shelf of the Black Sea an economic/ ecological experiment, certainly no "remediation"; Baltic Sea, little remediation, some point source reductions, but shifts in nutrient ratios, how to succeed on the large scale?; remediation, but what is the success story overall for all the effort?; northern Gulf of Mexico, "what remediation?" (Page 1, Line 1)

Obviously, the reviewer is keenly aware of the paucity of examples of instances where nutrient input management has resulted in demonstrable decreases in coastal hypoxia. The revised ms clarifies these points.

(10)I will continue, but a more appropriate title might be "What is, or might be, the course of ecosystem response for hypoxic areas to nutrient reductions?" (Page 1, Line 1)

Yes, this change has been made - please see our Reply to Comment #2 on Page 1.

(11)Most of the locations of authors I have seen in this series spells out the US state name (Page 1, Line 9,17)

OKâĂŤwe have corrected this.

(12)I am not sure that this is a broad assumption. I think the literature is full of "no response" examples, or not returning to previous state. (Page 1, Line 26) (Page 1, Line 1)

Until very recently, most engineers, managers, and scientists working on coastal hypoxia, had been operating under the assumption that reducing nutrient loading would mitigate the hypoxia problem. The massive nutrient management efforts throughout Denmark, for example, represent action on this widely held notion (e.g., Carstensen et al. 2006, L&O). To satisfy the reviewer's concern, however, we have reworded this sentence, "Although strategies to reduce hypoxia by reducing nutrient loading imply a broad assumption that reductions. . . ."

(13)I would delete this "many." Too many "many"s in this sentence. (Page 1, Line 25)

OK, done.

(14)Again, I'm not sure many expected a linear backward response. Better to place in context of ecological theory on hysteresis, etc. (Page 1, Line 29)

It was not our intention to imply linear backward responses, but we do hold that most managers anticipated relatively simple cause-effect relations. See our response to comment 2-1, above.

(15)this is good news. Are there examples from the literature where the system has not responded to nutrient load or organic carbon load remediations? (Page 1, Line 35)

Yes, although there are few examples overall, many suggest slow and complex responses. Therefore, we highlighted some of these systems in our manuscript.

(16)There are many documented regime shifts WITH increased nutrient loading. I look forward to reading about regime shifts post nutrient abatement. (Page 1, Line 37)

This is a similar point to our reply our comment #14 and #15, and our response is the same.

(17)An important aspect may be some theoretical discussion of what might be expected in a system that has experience a regime shift with increased nutrients, and IF nutrient reductions were successful, how might the ecosystem respond. Purely conceptual at

this point. (Page 2, Line 42)

We agree with the reviewer's suggestion, and we have edited our narrative in several places in the revised ms to address this question.

(18)Reconsider abstract with revision of paper, if warranted. (Page 21, Line 47)

OK.

(19)need to follow citation examples for BG (Page 2, Line 52)

Thank you, we have resolved this issue.

(20)suggest Diaz and Rosenberg 2008 Science. Also refer to Gilbert et al. this volume (Page 2, Line 52)

OK – we have added these citations.

(21)see Rabalais et al. and Gilbert et al. who stick to the SCOR WG 129 of 30% saturation, = 2 mg/L, 63 uM, etc. (Page 2, Line 54)

We, obviously, agree that the % saturation for a fixed concentration changes regionally and seasonally with O2 solubility. The revised ms has removed this problem.

(22)In other cases, low oxygen excludes many organism including predators of stressed benthos. (Page 2, Line 60)

OK.

(23)1994) (Page 3, Line 70)

We have added the ending parenthesis. Thanks.

(24)Not sure we said that respiration was strongest, because the bottom water does not necessarily change as much as the surface waters, and respiration rates are O2 concentration dependent. (Page 3, Line 83)

We disagree with the comment that respiration rates are O2 dependent – although this

is true in the case of extremely low O2 concentrations (e.g., Sampou and Kemp 1994 MEPS), a large fraction of most estuarine waters are above such low concentrations. That being said, we should have cited a different paper to support these statements. Plankton and bacterial respiration are definitely highest in summer in most systems, and benthic respiration also increases with temperature as long as O2 in the overlying water exceeds 2 mg/l (e.g., Hopkinson and Smith In: del Giorgio and Williams. Oxford).

(25)tidal and coastal waters (Page 3, Line 86)

OK, thanks; we have added "coastal" to clarify this point.

(26)Again, not sure this is a broad assumption (Page 4, Line 101)

Again, this is a semantic disagreement. Obviously, the fact that water quality agencies have committed to reducing inputs of nutrients and labile organics to coastal waters worldwide suggest a faith that these expensive actions will effect reductions in hypoxia and other major consequences of eutrophication. Efforts to reduce nutrient loading from the Mississippi River watershed to decrease NGOM hypoxia are based on this assumption that nutrient remediation will reduce hypoxia, maybe not immediately and with a linear trajectory, but that the response will be large and effective.

(27)not all of the 5 case histories have experienced nutrient loading reductions. e.g. Baltic, Mississippi (Page 4, Line 112)

As stated in our responses to comment #2, #14-16, etc., these case-study discussions are also intended to focus on coastal systems exhibiting strong non-linear temporal trends in hypoxia in relation to time-courses in nutrient loading. The revised narrative now makes this much clearer.

(28)or lack thereof. This would be as interesting as trajectories that did respond to nutrient remediation. (Page 4, Line 113)

We fully agree that there is as much to be learned from examples with delayed and otherwise complex responses as with those that show simple linear responses. The

trick is to have enough examples to infer information about patterns and controlling factors.

(29)Not usually a nutrient issue, although could be associated. Also, response would be much different for the hypoxia in a permanent anoxic basin to a coastal system. Maybe you will show that. Therefore, not a focus for this paper. (Page 4, Line 129)

Yes, permanent hypoxic systems are not a focus of this paper; however, we feel strongly that it is useful to include these in a broad analysis of the topology of hypoxic conditions.

(30)also may or may not be affected by nutrient pollution, could be natural. (Page 4, Line 130)

We are not aware of any pristine systems that exhibit diel hypoxia. The pattern seems to be simply that diel hypoxia happens in shallow systems (with substantial water residence time) where very high ecosystem production (P) and respiration rates (R) create large swings in O2 concentrations between dawn and dusk. Shallow depth is important because high areal rates of P and R translate into large daily swings in volumetric concentrations of O2 during low wind conditions that limit air-sea exchanges of O2. In any case, this section is about a topology of hypoxic systems rather than about human impacts on O2.

(31)suggest....river discharge (Page 5, Line 136)

OK, river discharge may be a better term.

(32)...activities that lead to increased nutrient loads. (Page 5, Line 151)

OK. The revised ms replaces the phrase “human activities” with the expression, "anthropogenic nutrient enrichment."

(33)through aerobic respiration of accumulated carbon (Page 5, Line 160) In revision, we have added this suggested wording.

(34)fresh water (Page 5, Line 161)

OK, revised as suggested.

(35)LIS also has a strong oceanic advection component (Page 6, Line 164)

Although we agree with this statement, it is not really relevant to the point we are making here.

(36)or offshore (Page 6, Line 172)

OK, point added to revised ms.

(37)(Rabalais et al., 2004; Wiseman et al. 2004) (Page 6, Line 173)

OK, we have cited one of these suggested references in the revised ms.

(38)This paragraph contains interesting information but little directed to the focus of the ms, responses of hypoxic waters to nutrient reductions. Suggest delete. (Page 6, Line 177)

This paragraph is integral for this section which provide a brief review of the balance between physical and ecological controls on hypoxia. We feel that it is thus important to retain this paragraph. We have reduced its size to some extent in the revision.

(39)Relevant paragraph and relevant to focus of paper. (Page 7, Line 216)

We agree.

(40)suggest cite Rabalais et al. 2009. (Page 7, Line 220)

We have added reference to this paper in the revised ms.

(41)Nice information, but not really tied to the idea of the paper. suggest reduce to 1 or 2 sentences. (Page 8, Line 234)

The idea for Sections 2-4 of our paper is to provide technical background information that is relevant to understanding trajectories of hypoxia response to nutrient loading as

modulated by physical and ecological processes. This particular section is important, for example, because it gives background needed to explain the regime shifts in hypoxia per nutrient loading in Chesapeake Bay and Long Island Sound. Thus, we feel strongly that details provided here must be retained. The revised ms clarifies this point.

(42)Section 3 can be reduced considerably, because this is a synthesis of processes and not directly applicable or tied in with the theoretical or real responses to nutrient reductions. Plus, other ms in the volume can be cited, Levin et al., 2009; Middelburg and Levin, 2009; Rabalais et al., 2009). Where a particular process is a key element in a reversal of trends or regime shift, then that (those) process should be considered at that point in the ms. Discussion of tidal submerged vegetation is not relevant at all.(Page 8, Line 236)

As indicated in our response to the previous comment, Section 3 is fundamentally important in the structure of this ms because it provides background for our analysis of case studies. We felt that this structure creates a balance that avoids repeating the same ideas and interpretations among several case study narratives. It also helps the reader to get a better appreciation of the range of controlling factors that we are considering in our interpretations of time-series patterns and responses to nutrient loading. Again, in the revised ms we have clarified this structure of the document and strengthened the link between background information (Sections 2-4) and case study discussions (Sections 5-6).

(43)Now, we're getting to the focus of this paper. (Page 12, Line 359)

OK, but as we explain above the preceding sections are also very important for the points that we are making in this ms.

(44)delete (Page 12, Line 364)

We have greatly reduced this narrative in the revised ms.

(45)What does this paper add to the information in Duarte et al. 2009? (Page 12, Line

376)

The Duarte et al. 2009 paper does not address questions of hypoxia, and provides very little theoretical background. Our ms certainly builds on Duarte et al. 2009 to broaden how hypoxia response trajectories are even more complex than those relating phytoplankton to nutrient loading. (Duarte, C.M., Conley, D.J., Carstensen, J., and Sanchez-Camacho, J.: Return to "Neverland": Shifting baselines affect eutrophication restoration targets, Estuaries Coasts, 32, 29-36, 2009.)

(46)These panels suggest a return to the original state. The response may be hysteritic AND not return to the original state but to a new equilibrium. This is not in the figure and is a distinct possibility. (Page 13, Line 389)

This is a very good point. We have edited the diagram and narrative to reflect these ideas.

(47)A reference to this statement? or is this conceptual? (Page 13, Line 392)

We added a citation to Carpenter and Lathrop 2008. Probabilistic estimate of a threshold for eutrophication. Ecosystems. 11: 601–613.

(48)possibility that complete recovery may not occur, even if all other conditions remain stable, which they will not. (Page 13, Line 396)

This is only true if the internal properties of the system change during the intervening time. We have clarified this point in revision.

(49)add Mee papers here because it shows a response to lowered nutrient loads, but NOT remediation. (Page 13, Line 406)

Although we agree that the Black Sea recovery was caused by inadvertent rather than purposeful remediation, we are not referring to the Black Sea at this point, and the reference to Diaz and Rosenburg is appropriate.

(50)(Turner et al., 2005, 2006) (Page 14, Line 414)

OK, we have added the suggested reference to Turner et al. 2006.

(51)These are examples of nutrients or BOD loadings decreasing and oxygen conditions improving, but conditions that led to this eventual decline are not known, or not shown, and were not necessarily linear. So even though the response appears linear, the initial trajectory may not have been linear and may have taken a different course to impaired state. (Page 14, Line 434)

Table 2 contains information on case studies where significant decreases in hypoxia in relation to decreases in nutrient (or BOD) loading or where non-linear trajectories of hypoxia responses to changes in nutrient loading have been reported in the published literature. In the revised manuscript we have added clarity to how we define the response trajectories.

(52)Example, in case, of knowing the "improving" trajectory, but not necessarily "impairment" trajectory. Unfortunately the data sets do not provide all that we would like to see. (Page 15, Line 452)

We agree with the reviewer that it is difficult to sort out mechanistic explanations for the observed time-series trends without a full complement of accompanying data, which rarely exist. We have edited the narrative in the revised ms to acknowledge this point.

(53)A better measure would be total N, or all the species of N, because declining DIN may indicate more uptake and conversion to organic forms of N. (Page 16, Line 474)

Although we agree with the reviewer that TN would probably be a more dependable index of nutrient loading, TN time-series were not available in the source paper (Soetaert et al. 2006) for this discussion. Data on spatial distributions of Chla were, however, given for four time periods (but not for a continuous time-series), and these data reveal a clear trend of declining Chla that parallels the time-series decline in DIN. We, thus, concluded that DIN was a reliable proxy for TN loading in this analysis. The revised text clarifies this point.

(54)Care to speculate here or elsewhere, the differential removal of some nutrients or species of nutrients versus others and how this might affect phytoplankton community composition, abundance and subsequent ecosystem responses that may or may not relieve hypoxia? (Page 16, Line 502)

In the revised manuscript, we have added some speculation on anticipated response trajectories in this discussion. We feel that such discussion helps to tie observations to theory.

(55)Can you provide some citations for systems where there was no improvement in hypoxia with decreased nutrient loads[?] Could be just as informative. (Page 17, Line 505)

Although there are a few recent papers that discuss the absence of chl-a responses to reductions in nutrient loading (e.g., Duarte et al. 2008), we have found few published papers that document the absence of a response in hypoxia to reductions in nutrient loading. We do reference at least one such paper (Testa et al. 2008 Estuar and Coast), refer to such a scenario in western Long Island Sound, and discuss many examples where hypoxia has worsened with no change in nutrient loading.

(56)Also there may be a shift in phytoplankton community composition and relative biomass of taxa both in the initial "change trajectory" versus the "return" trajectory. E.g., Seto Inland Sea, not a hypoxia issue, but certainly a phytoplankton, zooplankton, fish issue. Also the increase and shifts in nutrient ratios in the Miss R influenced northern Gulf of Mexico have been documented (Rabalais et al., 1996; Dortch et al. 2001) or conceptualized (Turner, 2001). (Page 17, Line 510)

The reviewer notes a broad and important topic that is, however, outside the scope of our paper. It may be more important in relation to the ecological consequences of hypoxia rather than relative to important controls on hypoxia.

(57)Turner, 2001, also Turner et al. 1998, 2008 Turner, R. E., N. N. Rabalais and D.

Justić. 2008. Gulf of Mexico hypoxia: alternate states and a legacy. Environmental Science and Technology 42: 2323-2327. Turner, R. E., N. Qureshi, N. N. Rabalais, Q. Dortch, D. Justić, R. F. Shaw and J. Cope. 1998. Fluctuating silicate:nitrate ratios and coastal plankton food webs. Proceedings National Academy of Science, USA 95:13048-13051.

We have cited the suggested references in this section.

(58)There is NO or LITTLE remediation in some of these systems, and in others a result of economic conditions, no effort at remediation. Suggest rethinking the context of the paper with regard to "remediation." (Page 17, Line 523)

As indicated in our responses to earlier comments, we changed the title of our ms, dropping the term remediation and we have clearly explained its scope and focus in the revised ms.

(59)MAJOR COMMENT: This section title is not what the prior paragraph said would be treated in the following section, i.e., responses to "remediation." This section is the "meat" of this ms, and should stick to the subject. This ms has become more of a discussion of nutrients, ecosystem changes, and little with regard to remediation, or what conceptual models might be applicable to a remediation that might eventually be accomplished. I think this paper would be better served by using examples of where true remediation has or has not changed ecosystem state, OR a combination of remediation and other reasons for nutrient reductions, and leave out or only be conceptual about what might happen in ecosystems that have obviously changed in response to nutrient loads, but are receiving no remediation. I.e., a discussion of nutrient relationship in nGoMx does not add to this paper on responses, because there has been no remediation and no responses to the no remediation. Hate to lose the world's best example of coastal hypoxia but the system does not fit the concept of what this paper addresses. Also, this comment is relevant to the Baltic, a great system to study with regard to how it got to the condition it has, but there is no remediation or response to

remediation to be discussed. (Page 17, Line 525)

The reviewer is correct in the suggestion that we are here including case studies that have not undergone remediation, per se. As indicated in many of our earlier responses to the reviewer comments, the revised ms has clarified its scope and focus and removed the term remediation. We have chosen to include a separate section that describes a few case studies where complex non-linear trajectories of hypoxia response to changes in nutrient loading have been documented. We focus here on these examples to illustrate a range of consequences that derive from multiple interacting stressors. We include the well documented patterns of hypoxia response to changes in nutrient loading (both increases & decreases) in NGOM because these data and interpretations raise a range of important questions that help to improve overall understanding of what to expect for hypoxia responses to future nutrient loading remediation.

(60)There are no P data for this figure, are any data available? (Page 18, Line 543)

Data are available for phosphorus; and we have added these data to Figure 6.

(61)there are no hypoxic volume data in fig. 7, are any available? (Page 18, Line 550)

Hypoxia volume-days (volume integrated over time) are presented in Fig. 7g.

(62)Figure 7 is referenced before Fig. 6. Need to put in order. Also, there are no hypoxia volume data for Patuxent in either. (Page 18, Line 555)

We appreciate the reviewer's point on our incorrect sequencing of Figs. 6 and 7. In the revised ms, Fig. 6 and the associated change-point analysis is discussed prior the Patuxent estuary case study discussion and associated reference to Fig. 7.

(63)While this is my favorite example of varying responses of components of the ecosystem to reduced nutrient loading, it is not in response to remediation. Reconsider the title. (Page 19, Line 572)

As indicated in many previous responses to the reviewer's comments, the revised ms

has a new title and it no longer uses the term remediation in relation to this case study.

(64)disappearance, not a removal (Page 19, Line 583)

OK, this has been done.

(65)yes, changes, but NOT remediation. Rethink title (Page 19, Line 584)

OK, this has been done.

(66)also P loads and P fertilizer use (Page 19, Line 591)

We have revised the x-y plot in Fig. 8b with separate trajectories for hypoxia vs. N and P fertilizer use. Although previous studies (Mee 2006) show that trends in N and P fertilizer use are generally parallel, different trends in N and P loading might be expected. Various analyses suggest appears that the relative importance of N vs. P limitation for phytoplankton growth varies in time and space on the NW Black Sea shelf (e.g., Ragueneau et al. 2002. ECSS, T. Oguz, personal comm.). The revised text discusses these relevant points, including N and P fertilizer application as proxies for N and P loading, variations in N vs. P limited algal growth, and estimates interannual variations in hypoxia extent.

(67)The ms should be clear about the forces and distinctions for the shelf area and the open basin area. (Page 19, Line 591)

This is clarified in the revised ms.

(68)also P loads (Page 19, Line 594)

This is clarified in revised ms.

(69)A nice description of nutrient, hypoxia interactions and time courses, but there really is no remediation here and no post remediation trajectory. Hate to lose another good hypoxia area, but does not address title. (Page 20, Line 613)

As with NGOM, we include the Baltic Sea in this analysis because of its complex non-

linear hypoxia response to changes in nutrient loading and other physical and ecological factors. This approach and scope is clarified in the revised manuscript.

(70)There are definitely remediation efforts in the Chesapeake Bay and there are documented shifts in ecosystem response with increased loads, however there are no data to illustrate "response to remediation." This is a nice summary of what causes hypoxia in CB and how the system has changed, but has no information on responses to remediation, or even conceptual suggestions as to what might occur. (Page 22, Line 659)

In fact, this discussion points out that despite the leveling and modest decline in nitrogen input concentration (and flow-corrected N loading), relative volume of hypoxic water (volume per N-load) continues to increase. This is totally within the scope of the original and especially the revised ms. However, in the revision we have added a brief speculative discussion on anticipated hypoxia responses to continued decreases in N and P loading.

(71)nice to see the nGoMx highlighted, but this section does not address focus of the paper. (Page 25, Line 755)

We have already responded to this point in the reviewer's comment on page 13.

(72)(Scavia et al. 2003; Turner et al. 2006) (Page 25, Line 766)

We have added these reference citations.

(73)sorry, but there is an order of magnitude calculation error in this paper. (Page 26, Line 791)

It is inappropriate for us to debate here the veracity of a published paper. In our revised ms, we have avoided this potential controversy by removing reference to the magnitude of marsh loss as a source of organic carbon to fuel hypoxia, but maintain our argument that the timing does not jive with the documented timing of hypoxia development in NGOM.

(74)The conclusion brings in much new information that is not supported by the text. The implications for management are useful, but could have been suggested without the examples in this ms. (Page 27, Line 842)

We disagree with the reviewer's statement. This discussion is fully supported by the preceding material in this manuscript, including consideration of theoretical trajectories of hypoxia response to changes in nutrient loading, differences in response to organic vs inorganic nutrients, differences in responses of shallow and deep systems, effects of physical processes modulating hypoxia responses, and more.

(75)yes, but what is this paper supposed to be about (Page 28, Line 850)

Again, the revised ms clarifies the scope of our ms, and this is clearly reflected in the original and particularly the revised discussion.

(76)well described, but not the focus of the paper (Page 28, Line 852)

Again the scope has been clearly stated in the revised ms.

(77)There is a large leap here from preceding sentence and this one. (Page 28, Line 856)

This apparent "leap" is probably attributable to the fact that the focus on physical features is only implied but not explicit in the topic sentence. This is clarified in the revision.

(78)shifts in nutrient ratios, phytoplankton composition and trophic structure (Turner et al.1998) (Page 28, Line 858)

These ideas are not relevant to the topic of this sentence. We are referring to the fact that changes in strength of grazing control by filter feeding benthos in shallow systems (or by filter feeding fish in deeper systems) represents a potential source of threshold or regime shift behaviors strongly modulating hypoxia response to nutrient loading.

(79)There is little evidence in this ms about thresholds, time lags, return to historical condition. Even the nw shelf of the Black Sea has not "recovered" with respect to other

aspects of the ecosystem. (Page 28, Line 859)

The reviewer seems to be referring to recovery in a more complete sense than we intended. We are focusing on reductions in hypoxic waters rather than recovery of other symptoms of eutrophication. In the revised ms we have added further clarification on these points.

(80)should be system specific. (Page 28, Line 871)

OK, thanks for noting this editorial error. It has been corrected in the revised ms.

(81)there has been NO remediation or reduction in nutrient loads to the nGoMx (Page 29, Line 889)

True, but it remains an excellent case study illustrating non-linear relationships to changing nutrient loading that arise from other interacting factors. This kind of analysis will be useful to improve understanding of controlling factors and forecasting likely responses of NGOM hypoxia to nutrient remediation when it happens.

(82)they may be expected to occur, but there are no data on recovery trajectories. (Page 29, Line 890)

We agree that there are, unfortunately, few data sets with parallel time-series of hypoxia and declining nutrient loading that illustrate hypoxia recovery trajectories. In general, this deficiency is attributable largely to the very limited number of clear reductions in nutrient loading combined with consistent monitoring of hypoxia responses. There is, however, much to be learned from the information that is available for a few hypoxic systems that have undergone reductions in nutrient loading.

(83)limited data for responses to remediation (Page 30, Line 911)

OK, but now is the appropriate time to start the process of analyzing, synthesizing and interpreting the data that do exist to expand our understanding of the controlling processes.

(84)suspect oceanic influence may only affect the lower CB, and in nGoMx the deeper slope water does not appear to impinge on the inner shelf. (Page 30, Line 914)

We agree that this sentence may be misleading, and we have edited it for improved clarity.

(85)reconsider either title or reduce the examples to those for which remediation (or just nutrient reductions due to economic collapse) has occurred and hypoxia has responded or not. (Page 30, Line 919)

Again, we have taken this suggestion to heart and changed the manuscript's title and scope.

(86)Time lags would have been a good subject for this paper. both in aquatic environments and in the landscape (Page 30, Line 928)

That would be interesting but is beyond the scope of this paper.

(87)Most of these are ecosystems with responses to INCREASED nutrient loads, NOT remediation (Page 47, Line 1596)

We disagree with the reviewer here. Aside from two systems in this table (which we noted in the 5th column had no nutrient reductions), nutrient and/or organic matter loading has declined in all these systems, consistent with the Supplementary Table from Diaz and Rosenberg 2008 (among other places). We have, however, completely revised Table 2 to make it more consistent with the scope of our ms and to add clarity and rigor to the information included.

(88)Do not think that this figure is necessary. (No page now)

We agree with the reviewer on this, and have removed the figure in the revised ms.

(89)better to identify the sources with the data rather than in the text (No page needed)

We agree and have made this change in the revised ms.

Interactive comment on Biogeosciences Discuss., 6, 6889, 2009.