

From: R. E. Turner

EPA SAB Hypoxia Advisory Panel Public Draft Report – For Wed-Friday meeting 5-24-07

The report has a lot of information in it, and editing is required (as would be the normal circumstance at this point). I wholeheartedly support the conclusion that the Action Plan Goals are both achievable and worth doing. We should start implementing it. A letter of support has been sent separately. So the comments below are more critical in nature, to highlight contradictions or omissions whose attention will strengthen the report, not weaken the conclusions.

Comments by page number

Page Comment

8 Osterman et al. 2005 did not measure hypoxia. They used a proxy (PEM, based on the relative abundance of forams) for relative changes oxygen that was not calibrated. The PEM proxy is not quantitatively related to oxygen concentration,, much less hypoxia I(defined as being  $<2.0 \text{ mg l}^{-1}$ ). Note: the title for the reference is incorrect in the literature cited list.

14 Re: “the spatial distribution of sediment cores...is not sufficient to determine whether increases in the spatial extent of hypoxia have occurred with time.” The results clearly show an increase in organic loading and BSi across the shelf which is coincidental with the foram proxy “A/E” index of Sen Gupta (confirmed by other studies). These are cores from multiple locations, and all show that hypoxia has developed in recent times. Perhaps the author was meaning to say that the expansion of the hypoxic zone after the 1980s is not delineated as a front by using only these sediment data. The cores by themselves show that hypoxia has expanded at many locations on the shelf.

15 Re;” a number of reasonably dated sediment cores have provided a coherent pictures of changes in hypoxia with time.” Here is a conclusion that contradicts what was said (above) on page 14. And again, in the next paragraph “there is some evidence for spatial increases in hypoxic extent in time.”  
Repeated idea: “the spatial and temporal variations observed between dated sediment cores are large.” Of course – hypoxia does not extent to all areas of the shelf simultaneously. The point of this sentence is not clear.

16 RE: Key findings: the introductory paragraph: “the spatial distribution of sediment cores is not sufficient to determine whether increases in the spatial extent of hypoxia have occurred with them.” Same comment as above – contradictory within the text and within this paragraph and, as written, casts doubt that the sediment proxies are saying anything useful.  
This is a sentence taken from one part of the report that was about hypoxia proxies, it is now used to describe all the results from all kinds of proxies and measurements. The

- results of multiple kinds of sediment core measurements made by many people are consistent for all proxies, including pigments, BSi, organic carbon, PEM and AE indices. This is a non-trivial conclusion and is incorrect as written.
- 19 1<sup>st</sup> paragraph. The discussion about stratification is sophomoric and relies on an unpublished dissertation. It is an obvious conclusion made many times before, including in the 1999 Hypoxia report documents.
- 21 the discussion about the water flow down the Atchafalaya causing an increase in the hypoxic zone is a narrow and incomplete view. It neglects to consider the effects of half the water flowing in the Atchafalaya NOT going into the Birds foot delta, where considerable amounts flow to the east. Hypoxia has been observed east of the delta. This may be a net sum game, and actually result in LESS hypoxia. We simply do not know.
- 22 last sentence. This section has lots of trivial conclusions, and this is one of them. Line 1 pg. 25 is another.
- 27 line 31. I don't think that Lohrenz 1992, 1997 made this conclusion, and Ammerman et al. is an abstract about the Sylvan et al. paper – it adds nothing. Sylvan et al. did not state this conclusion so harshly; they couched the result in term so the co-limiting impact of P with N as driven by the high N loading, just as this report says in other places. This is too flamboyant a conclusion as written.
- 28 line 42. Misleading. The cut line says “All experiments” and this is incorrect, as can be seen from the panels shown. Further, the ‘strong’ P limitation is actually much less than the co-limitation of P, N and sometimes Si. The conclusion here, and elsewhere, is internally inconsistent. e.g., p. 29 “the strong P limitation... appears to be a result of the very high rates of N loading. Page 31, line 33 describes the results of the experiment correctly “are P limited and P-N co-limited during the spring” but still leaves how Si additions influenced the bioassay results (March)
- 29 line 42 ditto. And, these generic conclusions are conclusions based 2 experiments? This conclusion seems a bit over-reaching in light of the criticism of bioassays raised on page 30, lines 1-14.
- 32, line 15-18 this has been addressed multiple times. Please acknowledge why this is so important to mention as one of three important findings.
- 35, line 39. Osterman et al. did NOT use a proxy for hypoxia, but for low oxygen. The PEM is not calibrated. They did not demonstrate hypoxia “in the late 1800s”
- 36, line 5-6 “influence of organic matter losses from wetlands .... remains unresolved.” This statement, a topic sentence, is not matched with the discussion on p. 38-39, which concludes “it is unlikely that wetland loss could be a prime source of OM to the hypoxic zone.”

37 and 40 this represents almost exclusively the work of one collaborating science group – which is unfortunate.

41 Key findings..  
line 21 again, the role of wetland OM hedged, whereas it is argued elsewhere that it is clearly unimportant.

45 line 8-18 and afterwards. Here is a place to mention that organic matter stored in the sediments in increasing amounts has caused the hypoxic formation to form more readily and be sustained longer. This system memory (the stored organic carbon) increases the respiratory demand in future years, and explains the higher and higher amounts of oxygen removed for the same amount of stratification or nitrate that is mentioned earlier.

46 lines 7-19 Re: the ‘regime shift’ “has not yet displayed features of a regime shift in biological variables” Is the system not already demonstrating a ‘regime shift’? The benthos are wiped out periodically. The oxygen concentration has gone down regularly and over a wider area, with the loss of benthic organisms, communities, size structure and amount. There is more carbon stored in the sediments. Fisherman have to travel further to make their catches. What is the definition of ‘regime shift’ that does not recognize these biological impacts as “not yet displayed”

50 lines 1-15 This section criticizes a particular model and, I think, is unfairly critical and limiting. Testing hypotheses and detecting system change are useful attributes for models – including this one based on statistical analyses. Statistical models have a long history of being useful for management purposes. There are several places where the Panel recommends the development and application of a diversity of models. This model is one kind of model and certainly not the only model. The Panel is certainly welcome to their opinion, and this statistical model has its limitations. But these limitations are stated in the paper, and all models use empirical relationships and the fit is really good for ecosystem models – much better than the alternatives. This statistical model has a fit of 80%+, whereas one of the other models are 45-55% AFTER adding a post-prediction correction for advective diffusion. The variable “year” is discussed as a surrogate for the accumulating sedimentary carbon storage. The model discussion says to use the model coefficients to track system attributes, by watching coefficient change (they are stable now), an indicator of system behavior and process changes is detected. This is a management purpose – to detect changes in system behavior. Further, it tests hypotheses, which the other models try to do.

74 line 5-8 Soil mineralization is clearly the dominant source of nitrate from IOWA rivers.

76 line 17 WHY are more water quality stations “critically needed”? I believe you, but the case is not made clearly. In fact, the judicious use of data in all of these figures might leave the reader with the impression that we have enough to make the case for the

purposes of this report. Please consider strengthening the rationale for having more monitoring stations.

86 and throughout this section: Graphs 30 and 33 show that the Net N input is stable, and that the Net P input has decreased since the 1980s, which means that the net TN/TP of the inputs has gone up. The Key findings (p. 98, line 23) say that the N and P attentuatoins can be equal, or higher, for P, which would tend to keep the TN/TP ratio in the inputs down stream, or higher. The TN / TP in the river, however, has declined. If the river data are correct (I believe it) and the NANI estimates are correct (they should be), then what has happened to cause this contradiction? So there is a contrast between analyses of one aspect of the system with what actually happens. This seems to be a fundamental teaching opportunity – either to explain how the system ‘works’ or to learn what we don’t know about it, and to make a recommendation about how to find out what is going on.

The discussion could be included with the section on fig. 37 (p. 93).

97 line 31 Turner (1999) did not generalize that river diversions “would remove small amounts of nutrients relative to nutrient input loads.”

113 line 16Re: “there are no experimental data relating phytoplankton responses there to different levels of P.” This statement is incorrect – there are several experimental tests that related P responses to phytoplankton, including the Sylvan et al and the models by Bierman et al., and by Turner et al.

181 Re. Perennials The zeros for perennials should all be positives (+)

182 Re: coastal wetlands:

186 line 8-9. this topic sentence is not supported in the text, and is unnecessary

lines 23-24 this is not a recommendation, and is trivial

lines 25-26 is there any doubt about this conclusions? And science does not confirm, but test

lines 38-44 internally inconsistent logic, per previous comments

189, line 7 Re: 12,700 km<sup>2</sup>. the average from 2000-2006 was 15,888 km<sup>2</sup>. (excluding two years when there were strong storms just before the hypoxia survey)

Line 18,19 same comment. And the predictions in this less critical assessment are not making predictions “beyond the range of supporting data.”

193, lines 17-19 Re: wetland uptake of nutrients. The report would benefit if this section included a look at the whole system for this issue. By omission of relevant alternative explanations, it appears that nitrogen removal is a win-win solution, regardless of how small. Denitrification requires and electron donor – carbon – which will reduce carbon accumulation. Jim Morris has shown that coastal wetland lose organic matter when nutrients are applied. These coastal wetlands require organic matter to sustain themselves – it is not the inorganics that keep them at a favorable elevation. Make whatever your recommendation a balanced examination of the ecosystem.

194-5 lines 44, 1-3 Re: Social benefits: This is an important paragraph, and an embarrassingly short amount of attention is given to this point or to what can be done about it, or what should be done to explore and communicate the embedded issues. And the sentences expresses some doubt that there is a net social benefit, which makes it that much weaker. The conclusion is further overwhelmed by the econometric focus of the report on benefits and costs. Community health, safety, food security, social welfare, environmental health are omitted – as I recall, these non-economic values have a lot to say about human experience in a fundamental way. They are values that make or break many environmental improvement policies in practice.

146, lines 38-39 Re: “Clearly, including perennial crops in a rotation system can dramatically reduce NO<sub>3</sub> leaching.”

Perennial systems would have a dramatic effect if used continuously, too. Perennial systems are the only ‘solution’ mentioned in the text that have a ‘dramatic’ impact. They deserved more than a few words, therefore. They are mentioned here and there throughout the various text pieces, but if you believe how significant they can be in improving water quality, then their use deserves more attention as a stand-alone section.

(the recommendation on page 173, line 33; should stand, and the justification be expanded)

Generic issues:

27, line 31, and lots of places: the term “inshore” is used to describe what appears to be a ‘nearshore’ system. Or, does the EPA report mean to describe estuary – offshore connections? I think not, because it is not a reasonable interpretation that the estuaries have much influence on the offshore zone within the context of this report. This wording can be misleading and add confusion if not changed.

Examples:

p. 28, line 13, ditto

p. 29, line 13, ditto

p. 31, line 32, ditto

p. 32, line 9, ditto

188, line 17, ditto