Review Comments of Dr. Walter Dodds, Division of Biology, Kansas State University, 9-10-07

Review of EPA-Science Advisory Board Report on Gulf Hypoxia

The review generally addresses the original charge questions asked by the EPA. It is very thorough, and deals with many of the issues that will be required to control the size of the zone of hypoxia. The Advisory Board is to be commended for such a comprehensive and broad scope report dealing with the many potential issues involved with this complex problem. I have only a few major concerns.

My first concern is the rationale for linking the size of the hypoxic zone target reductions to the amount of total N and P reduction. The models for N have been published elsewhere but should be reproduced in the report along with an error analysis indicating the likelihood of obtaining the stated reduction goals. Straight regression models for TN and TP resulted in r^2 values of 0.27 and 0.60 respectively (Turner et al. 2006). This indicates substantial unaccounted for variance. The mechanistic models do better for total N loading (Scavia et al. 2003). Still, given the high variance, the potential for error should be calculated. Unrealistic expectations that a 45% decrease in TN load will decrease the hypoxic zone to $1/4^{th}$ the size could ultimately lead to disappointment. Clear explanation of why reduction in the size of the zone is not strictly proportional to reduced nutrient loads would be helpful to the case for load reductions of this magnitude.

Related to the above concern is the method used to set the TP load reduction. Limited reasoning is given for reducing the total loading of TP 45%. Given that TN and TP loading values are very close to the Redfield ratio recently, equal percentage reductions could be warranted, but this point should be made clearly. Better yet, unpublished materials by Scavia should be presented (maybe they will be out in time for the final report?). Given that models of phytoplankton production predict interactions of N and P that increase yield, if it is easier to lower one or the other more than 45%, the recommendation maybe should be phrased as “at least 45%”. If total N and total P data are available historically, then maybe the ratio of loading that was present before the hypoxic zone should be the target unless mechanistic models suggest otherwise. The case for a stoichiometric approach has been made strongly with regard to this issue (Dodds, W. K. 2006. Nutrients and the "Dead Zone": Ecological stoichiometry and depressed dissolved oxygen in the northern Gulf of Mexico. Frontiers in Ecology and the Environment 4:211-217). DIN:SRP data available historically could be used to estimate TN:TP (albeit with a good bit of error). Please note that the plots in the Dodds reference were not corrected by the journal as per request and a correction was published online for members or is available at
http://www.k-state.edu/doddslab/journalarts/dodds%20free%202006.pdf

I am not thrilled by the statement on page 36 that N and P limitations tend to be confirmed by SRP:DIN. The relative degree of N and P limitation can be calculated from

bioassays, and the data can be used to create ratios of N and P limitation. This approach to calculating degree of N limitation from a complete bioassay design has been published (Dodds, W. K., E. Martí, J. L. Tank. J. Pontius, S. K. Hamilton, N. B. Grimm, W. B. Bowden, W. H. McDowell, B. J. Peterson, H. M. Valett, J. R. Webster and S. Gregory. 2004. Carbon and nitrogen stoichiometry and nitrogen cycling rates in streams. Oecologia 140:458-467). I agree with the related statements that a more complete mechanistic understanding of how N and P is transported into the NGOM is necessary as are more detailed bioassays.

My third concern involves a control option that I did not see discussed. Restoring the length of stream channels to historic levels could potentially have a positive impact. The report acknowledges the importance of in-stream nitrogen removal as determined by the SPARROW and RIV-N model (albeit with large ranges). The effect of removal is a function of length of stream channel. Streams have a sinuosity of roughly 1.6, so channelization could decrease stream length by roughly 1/3. This could substantially influence the amount of removal and restoring natural stream geomorphology could assist with removal (Bernot, M. J. and W. K. Dodds. 2005. Nitrogen retention, removal, and saturation in lotic ecosystems. Ecosystems 8:442-453). This restoration of stream length could also improve phosphorus retention.

Page 40 repeats an entire paragraph from the previous page.

Some of the specific research recommendations could be construed as self serving for members of the panel and I hope that research funding in the recommended areas is ultimately open competition and subject to peer review (e.g. EPA STAR grants). The report could even suggest this to maximize the perception of unbiased participation.