

Comments on the 25 July 2007 draft

From RETurner

p. comments

40 line 37, to p. 42, l 16. the second paragraph (p. 41, l 26) is duplicated, with modification, but largely the same material.

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109 line 9; re: diversions can remove 10 to 15% of the nitrogen load. The estimate of 10-15% nitrogen removed by coastal wetland overland flow is from the text in the Mitsch et al. 2001 paper (2<sup>nd</sup> paragraph, Mississippi River diversions). THIS IS AN INCORRECT ESTIMATE.

WHY: See tables 1 and 7 of the reference source (Mitsch et al. 2001). Table 7 and the text has estimates of the amount of the river and the area inundated necessary to achieve a reduction of 50 (low) and 100 (high)  $10^3$  mt. Table 1 has their estimate of the total N load ( $1567 \cdot 10^3$  mt). Dividing the low and high by the total load gives a range of 3 to 6 % removal.

FURTHERMORE: The estimates are wildly high.

WHY: Estimates of wetland for the coast are in Bauman and Turner 1990 (attached). There are 1.2 million ha of wetlands in coastal Louisiana, and the Mitsch et al. assumption is that 1.0 million ha are used for nitrogen removal. 80% of the coastal wetlands are no where near the river; Fig. 6 (current draft) shows large swatches of the coast without a diversion. Therefore, 1 million ha are not available. Further, there is no way that the engineering could spread it out evenly, and – would we want that amount engineering?

225 l 17-19 the model is being criticized for something it does not pretend to do. The model tested assumptions, as per the discussion in the previous sentences. It is used to predict the size of the zone three months ahead of time, and then updated every year for the next year's prediction.

Fyi: the 'year' term can be viewed as a years since a regime shift. The acceptance of a regime shift is in the text of draft report. The rationale is that there is an accumulation of organics each year that have a legacy for the next year – an idea also in the text. The idea of cumulative effects and 'trends' are something embedded in lots of models, including corn production trends. The ideas are discussed in a ms. now in review, and which will be shared with the committee when the review process finds it acceptable for publication (yes, there is a backlog of hypoxic papers at ES&T). These comments apply to p. 58, lines 7-9.

P driving the size of hypoxia: Scavia and Donnelly ms. has a lot of "if" in it. I do not have the latest draft, but the last one we reviewed prior to submission says: "Our analysis suggests that, **if**

phosphorus is limiting, it became so because of the increase in nitrogen loads during the 1970s and 1980s, not only in absolute terms, but also in relation to phosphorus loads.” And “**If** phosphorus loads **alone** were reduced, our analyses suggest reductions of TP of between 40 and 50%.” (emphasis added).

Is it not risky to assume the conclusion that P limits the size of the hypoxic zone based on four samples (May and June)? Limnologists made that mistake when they assumed that results from bottle experiments applied to whole lakes. Schindler showed them how wrong they were. Also, Redfield ratios are best applied to loadings, not in situ concentrations. Redfield ratios were about the ratios in arising from the decomposition of organics, not the uptake from the concentration in situ.

The data driven empirical models do not find that P loading describes the variation in the size of hypoxia. The prediction from some models predicted (true prediction) the size of the hypoxic zone with 99% last year and 93% this year. The Scavia model has an R<sup>2</sup> value of half that *after* a post-model re-calibration of the advection term. It does not predict the size of the hypoxic zone. The model effort has many outstanding attributes, but prediction is not one of them.

p. 2, lines 27-29 becomes, in this context, a much more specific conclusion. I.e.: “Additionally, new information has emerged that more precisely demonstrates the role of phosphorus (in determining the size of the hypoxic zone.” The bioassay experiments do not show a relationship with the size of the hypoxic zone. The models do not predict the size of the hypoxic zone, and the models that can predict the size of the hypoxic zone find that only N is useful, but not P. There is, therefore, (I think) a disconnect between the science available and the conclusion as written.

#### Other

Ammerman 2004 (an abstract whose content is in Sylvan 2006) is still there in the text. They were not all removed – J. Sanders said this was removed. (e.g., p 38, 39, 129; a global search would catch them)

#### Attached:

Mitsch, W. J., J. W. Day, Jr., J. W. Gilliam, P. M. Groffman, D. L. Hey, G. W. Randall and N. Wang. 2001. Reducing nitrogen loading to the Gulf of Mexico from the Mississippi River basin: strategies to counter a persistent ecological problem. **BioScience** 51: 373-388.

Baumann, R. H. and R. E. Turner 1990. Direct impacts of outer continental shelf activities on wetland loss in the central Gulf of Mexico. **Environmental Geology and Water Resources** 15: 189-198.