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November 2, 2007

M. Granger Morgan, Ph.D., Chair
Chartered Science Advisory Board (1400F)
U.S. Environmental Protection Agency
1200 Pennsylvania Avenue, NW
Washington, DC 20460

RE: Hypoxia in the Northern Gulf of Mexico: An Update by the EPA Science Advisory Board

Dear Dr. Morgan:

Per my letter of September 27, 2007, the College of Agriculture and Life Sciences of Iowa State University (ISU) convened a task force of research scientists on agricultural nonpoint source landscapes to review the draft report of the Science Advisory Board concerning Hypoxia in the Gulf of Mexico. At that time, the ISU task force's comments were summarized into ten major issue areas and were submitted by the September 28 deadline.

The September 27 document included an offer to make available specific comments by individual members of our task force. Enclosed here are those comments as a supplemental piece for the original document. These specific comments by individual task force members bring forward more fully their perspectives on information contained in the draft report. The comments enclosed in many cases reference page and line numbers to support the summarized issues and for consideration by the Science Advisory Board in considering revisions to the draft. Some comments may overlap among the reviewers and some are posed as questions.

We appreciate the SAB's decision to reconvene the panel to consider both the previously submitted September 27th letter and now these attached supplemental comments.

Thank you again for the opportunity for input into the draft report.

Sincerely,

Wendy Wintersteen, Ph.D., Dean
College of Agriculture and Life Sciences
Iowa State University

Attachment

IOWA STATE UNIVERSITY
College of Agriculture and Life Sciences

Supplemental Comments of Iowa State University Task Force on the "Science Advisory Board (SAB) Hypoxia Panel Draft Advisory Report" dated August 30, 2007

Submitted on November 2, 2007, by the College of Agriculture and Life Sciences, Iowa State University, Ames, Iowa 50011

Listed below are more specific comments by eight individual members (idenfiied by last name) of a task force convened at Iowa State University to review the draft report of the EPA Science Advisory Board concerning Hypoxia in the Northern Gulf of Mexico. Several of the comments reference specific pages, line numbers and figures or tables from the draft report.

Baker comments

Page	Line	Comment
i	42	The self-proclaimed need to discuss policy options is subject to question.
ii	9	More than "conservation" is needed.
2	1-2	Not sure what this is saying; more details are needed.
2	9-11	If "it is even more important to proceed in a directionally correct fashion to manage factors affecting hypoxia than to wait for greater precision in the setting the goal for the size of the zone," the past and still current goal is already too specific in both the value for the area (i.e., 5,000 sq km) and the deadline (i.e., by 2015).
2	11-13	While this very general statement can be true, the choice of "management plans" and methods of implementing them will no doubt involve substantial costs; reference to this fact should be made relative to the care needed in planning to do this.
2	26	Per the models, how much and how soon will a 45-55% reduction in N "reduce the size of the hypoxic zone"?
2	41	"Mixing" of what and how is that important?
3	11	Has not annual total N flux also decreased (see p. 221; l. 16)?
3	25-33	The implications of a "regime shift" could be severe; how reliable are the "indications" (pp. 50-52) that this has occurred and how reliable is the statement that "today the system is more sensitive to inputs of nutrient than in the past"?
4	12-19	Much more emphasis is needed on monitoring (to cover the costs of not only for the three bullets, but also on current management practices within the MARB).
5	30	What are the "dual nutrient control practices" that need be examined?

Page	Line	Comment *(Baker, continued)*
6	6	Here and elsewhere (e.g., p. 121), "adaptive management" is oversold — it is the use of common sense and logic that is already practiced, as well as considering the economic implications of new management actions before they are undertaken.
6	13	Is it realistic to think a "point of no return" is even in the foreseeable future? If it is and happened, then no amount of nutrient reduction would help?
7	10	Is this sentence implying that wetlands remove significant amounts of P?
7	13	In many parts of the Corn Belt, "measures to improve drainage water management" involve an unproven technology, certainly on the watershed scale; therefore, to say that they are "urgently needed" is an overstatement.
7	17	It needs to be emphasized that none of these "opportunities," including eliminating fall fertilization, comes without cost.
8	4	Should the word "direction" be deleted?
8	33	If there is an "abundance of information," why do we still need more research (and "adaptive management")?
53	4-5	The statement "increases in N loads have clearly been occurring throughout the past decades" doesn't agree with other statements (e.g., p. 3, l. 11; p. 221, l.16).
56	35-36	What are the "similar levels"?
68-74	Figs. 17-23 (excluding Fig. 22)	Can the variability in the graphed values (or confidence intervals) be shown to illustrate whether any apparent trends are likely to be real?
84	10	An even stronger case should be made for "more robust monitoring programs."
126	8-9	Here again, reference to an "increase in N loading" in "the last 30-to-50 years" does not agree with other statements (e.g., p. 3, l. 11; p. 221, l. 16).
126	23	The new model and paper cited need to be made available for evaluation.
128	3	Is this the only justification for a "40-50% reduction in the five-year running average of P loading"? (p. 126, l. 41-42)
155	19	Would not a "decreased tile depth" increase the loss of soluble P (with less interaction of drainage water with P-deficient subsoils)?
156	3-6	Increased surface runoff volume also would transport more soluble N and P.
158	1	How much is "less"?
160	12	For clarity, "Total N" needs to be defined in terms of its components.
161	23-25	How long is "long-term" relative to the time for "new sediment and soil" to fill up the wetland?
162	17	How can wetlands restored for N removal "be managed for P retention as well"?
163	13-17	It needs to be clear up front in this sentence that these results do not apply to tile-drained areas; consider placing the next sentence in front of this one.
163	34-37	In citing small-area studies such as these, care must be taken to consider the drainage area to buffer area and the resultant effects on the hydrology (relative to a watershed-scale application of this practice).
163	41	Define "cascading."
167	20	Correct conversion for 150 kg/ha is 134 lb/ac. The 223 lb N/ac seems high. Iowa's recommendation is from 150 to 200 lb/ac for continuous corn.

Page	Line	Comment *(Baker, continued)*
167	45	Although both legumes, soybeans should not be "equated" with alfalfa because with soybeans, much more N is removed with harvest than is fixed, even though both can use residual soil N.
168	4	"April" should be changed to May or even June.
169	12	Define the "AFO paradigm."
170	7-15	In the Corn Belt, as opposed to other areas, separation of grain production from animal production is a matter of miles not regions; use of the term "nutrient excesses" is inappropriate for the data referenced.
173	25	See previous comment relative to "regional nutrient transfer."
173	34-35	Provide details relative to manure "increasing vegetative cover."
173	42	Manure management plans in the Corn Belt already are often required to consider both N and P.
174	2-3	While conservation tillage may increase infiltration and the volume of subsurface drainage, reductions in nitrate concentrations usually offset that to the point that N leaching losses are less than or equal to those for moldboard plowing.
174	Table 14	The number of significant figures for most values should be reduced by at least one.
176	9	In the Corn Belt, manure management issues are dominated by inadequate knowledge of the available nutrient content of each manure load and physical application problems, all of which reduce the ability to add manure uniformly to the soil at the desired rate so that a producer can confidently take credit for the manure nutrients applied.
178	22-23	Part of the timing issue also is a placement/location issue. If the N present in the soil is not where active roots are located, uptake will be limited (e.g., a problem can arise when N is side-dressed into dry surface soils).
179	3	Without additional information, the value "273,000 tons of actual N" has no relevance.
179	Fig. 47	These fall-applied N data should be plotted as a percentage of total N applied; looking at areas with "some amount of N in the fall" skews the results because of the small amounts of N applied with fall-applied P.
181	2	Is this implying that tile drainage is affecting soil temperatures?
181	5	Iowa State University does not "recommend" fall N fertilization, but provides guidelines as to what should be done if a producer decides to fall apply.
186	19-29	This needs to be rewritten to remove the "X's" and replace them with the rates.
187	6-7	This sentence could be rewritten to: The N mass balance approach to determining long-term changes in SOC and SOM is irrefutable, but presents numerous experimental challenges.
187	7-8	This sentence could be rewritten to: While there is no mechanism for lower fertilizer N applications to directly stimulate increased SOM mineralization, the amount of crop residue available to replace the removal of the mineralized N with grain is dependent on the fertilizer N applied.
189	39-41	This is another case of an unproven technology that requires more development and verification before promoting wide-scale adoption (noted on p.190, l. 12-14).

Page	Line	Comment *(Baker, continued)*
196	1	Actually, more definitive information is needed for a comparison of fall versus spring application as well as comparisons of fall and spring with a spring-split application — limited data and simple "logic" are inadequate.
196	15	What are "reasonable costs"?
197	13-21	If NOx emissions are reduced, but the N is released as ammonia, does that solve the emission problem?
198	2-4	Will not the supply of methane limit the options?
201	14	"263%" — by when?
201	30	Are there not technological challenges for cellulosic ethanol, too?
202	43	Because of erosion, CRP will not be "low nutrient loss land" relative to P and sediment-N if converted to corn.
203	3-4	Relative to the N mass balance, continuous corn will deplete soil organic matter slower that the corn-soybean rotation.
204	1	What and where is "idle land"?
204	28	Why are only "dairy cattle" being considered?
205	4-6	What does this current report conclude about the importance of reduction in N application rates as a tool to reduce N loss from corn?
205	13-14	This statement implies that over-application of N is still a big issue. In order to really know where things are now, better current-practice monitoring data are needed — this includes use of both fertilizers and manures.
205	19	"Increases losses" relative to what?
214	24-28	Part of the "large variability in reduction efficiencies" relative to edge-of-field losses is the variability in soils and, particularly, in weather (place-to-place and year-to-year). This problem becomes even greater when projections to the watershed-scale are made.
216	16-18	Of the practices listed in Table 18, none give a "synergistic" effect; in fact, none give an additive effect but rather a multiplicative effect, where if one practice reduced nitrate leaching by 40% and another by 20%, the overall reduction would not be 60%, but 52%, i.e., [100-((1.0 – 0.2) x (1.0 – 0.4) x 100)].
216	21-25	Both issues — increased nitrate leaching with reduced tillage and manure management based just on N — are over-emphasized.
218	1	"Discontinuing fall N application for corn" has the potential to reduce nitrate leaching losses (and possibly increase yields due to reduced losses including those from denitrification). However, care must be taken when implementing this recommendation because of increased costs and safety concerns.
218	1	The timing of spring drainage release required for crop production may not work well with the desire to reduce nitrate leaching in the April-June period for hypoxia control.
222	8	Should there not be a call for better point-source data generation?
222	18-19	Is January the correct month to begin this period of "high flows"?
222	22	What is the definition of "small"?
222	29	As noted earlier, if the goal cannot be "refined," why have one like the current one at all?

Page	Line	Comment *(Baker, continued)*
223	41-43	Why is the very important variable listed in addition to nutrients, i.e. "discharge" or the volume of fresh water flow, not even addressed (p. 224, l. 1-2)?
225	9	The need for additional/better monitoring can not be emphasized enough.

Duffy comments

Overall, I thought that the draft report was a good one and covered a significant amount of material in a fairly straightforward fashion. I think the group is to be commended for trying to present a balanced approach.

One of my major concerns was the lack of discussion on costs of the proposed options, especially the ones they recommended. Tables 18 and 19 (pages 215 and 216) present a list of management options and anticipated benefits. I would have liked to see a similar table with costs of the suggestions listed. This could have been simply a high-, medium- and low-cost division. In addition, who bears the costs would be very enlightening, especially as one considers the recommendations put forward.

The report just simply seems to skip around the whole issue of costs, especially public versus private, and whether or not a cost for a certain practice could be justified based on the benefits. For example, in the Executive Summary (page 7, lines 36 to 38) there is discussion of costs being borne "largely by residents in the region." But is it the residents or just the farmers?

Such discussions may have been beyond the scope of the Report, but to me it is hard to evaluate the proposed alternatives without some idea of the costs and their distribution.

I also think the report was remiss in not addressing the changing structure of agriculture that will influence many of their recommendations and the discussions. The CAFO issue is a prime example. But so too is the discussion on shifting fertilizer from the fall to the spring. The costs of such changes could be considerable when you consider that in Iowa 11 percent of farms have 42 percent of the harvested acres. The desirability of such division can be debated, but the fact remains that simply saying shifting fertilizer from fall to spring overlooks an important feature of Iowa agriculture.

If we move all the fertilizer to the spring, several important questions are overlooked. For example, do we have the infrastructure? Do we have the machinery and equipment? We might be able to shift fertilizer application to a regional-type arrangement similar to wheat harvest, but that needs to be researched. What will be the impacts on yields due to delayed plantings if we have to wait for fertilizer applications? There are many other potential pitfalls to this approach that need to be considered before such a recommendation would be put in place.

The discussion on changing cropping patterns also is lacking in that it doesn't go far enough to discuss where farmers would find a market for the products produced.

It will be hard for Iowa to complain too much about animal manure rules, however, when you look at Table 13. Perhaps what is needed is simply more of the permits. I don't know, but this might be something to think about in this regard. That said, I found the manure section to be one of the weaker ones in the report. Several statements and comments aren't clear to me. For example, saying sound feed decisions "lie with the integrator in the CAFO industry rather than the individual farmer." Who are the integrators? Also, "wider adoption of manure transportation that links producers with buyers will greatly enhance the sustainability of AFO's." There is a limit how far manure can be transported. There are a host of issues rather than such a simplistic comment.

The report mentions the "conflicts between certain aspects of current agricultural and energy policies on the one hand and the goals of hypoxia reduction and improving water quality on the other." These conflicts are a key point. Even though there is discussion of the conflict in Section 4.5.9, putting the above quote in the conclusions of the Executive Summary does not do enough to highlight the problems being created.

Finally, it seems to me that putting such specific targets as 45 percent reductions when by their own admission it won't be obtained might be counterproductive. If you have an unrealistic target, then you have automatically set yourself up for failure. Perhaps we should just strife for the "directionally correct" approaches for now.

Helmers comments

- While the concept of adaptive management may be straightforward, application to systems which are so dynamic and where there may be significant time lags from practice implementation to when results are apparent may prove to be difficult. Consideration should be given for discussion of the limitations and challenges of an adaptive management strategy for nonpoint source pollution.

- Discussion of conservation buffers should appropriately recognize that performance can vary significantly depending on factors such as presence of concentrated flow, area of buffer receiving flow and maintenance of buffer relative to vegetation and short-circuiting of flow. There is a need for evaluation of buffers under these "real-field" conditions. Few studies have evaluated buffer performance under common field conditions. As with other practices, to be most effective for water quality improvement buffers should be sited where they will receive flow.

- Page 168, lines 1-2: Discussion of soybeans as scavenger intimates that one would expect to see significantly lower N-loss in the soybean year; but through much of the Upper Midwest, in a corn-soybean rotation we see similar N-loss in the corn and soybean year.

- Page 202, line 38: Review the 33% increase in annual N loss statement.

- Page 212. Ranges in Table 17 for potential impacts of spring versus fall application and recommended versus above-recommended rates seem larger than noted in the narrative.

- In Table 17, ranges for riparian buffer performance seem larger than in the text. Range of 12 to 90% sediment and nutrient reduction are reported on page 164.

- Is the potential time lag from practice implementation to when water quality improvement effects may be detectable adequately discussed?

Jaynes comments

This draft report is greatly improved over earlier drafts. It is a much more comprehensive review of the science and potential solutions than the earlier Integrated Assessment. The scientists involved should be congratulated on their efforts and diligence in delivering this report in the very short time allotted.

Page 6, line 13: The panel indicates that hypoxia in the NGOM may soon enter a "point of no return," where "even larger nutrient reductions are required to reduce the area of hypoxia." These statements appear to be in conflict as "state of no return" implies just that; that no amount of action will reduce hypoxia, while "even larger … reductions" implies that hypoxia could still be reduced but with much greater effort. In fact, the panel does not establish either of these points within the science assessment of the report. We urge the panel to more carefully phrase their finding, unless phrases such as "point of no return" be taken out of context and used by those without a clear understanding of the causes and uncertainties of Gulf hypoxia.

Page 39, line 45. Delete this paragraph as it repeats previous paragraph.

Page 60, line 45 and caption Fig. 10: The Jaynes analysis was for row crops and not specific to corn and soybeans, although those would be the dominant row crop in much of the Midwest.

Page 3, line 15; Pages 76-77: Much is made of the fact that the Upper MS and OH river basins contribute the majority of the nutrient fluxes (80% for total N) while representing only 32% of the MARB drainage area. This comparison, while true, is misleading because the upper MS and OH river basins also contribute 71% of the total flow of the MARB and its flow that in great part determines flux, not area drained. Viewed in this context, the OH river basin's contribution to nutrients is proportional to its contribution of flow (Table 3). The Upper MS contributes about 150-250% more nutrients than flow, the Missouri basin contributes about 200% more TKN and total P than flow, as does the Lower MS for total P.

Page 84, line 1: It would potentially be of great benefit if the panel were to elaborate on why the nitrate flux from the MO river basin has decreased so dramatically since the 1990s. Was the decrease just from manipulations of the river's flow regime or have land-use practices within the basin driven the decreases? This "success" could have profound implications for the other MARB basins.

Page 86 table, bullet 2: Total P flux increased markedly from the Ohio River basin, but it was not apparently due primarily to an increase in flow (Fig. 25) as stated here.

Page 92, lines 10-24: While we agree that N balance computations are very helpful for identifying basins out of nutrient balance, these computations are of limited benefit for estimating N losses to streams. For example, NANI ignores effects of cover crops on N losses, but field studies show that cover crops greatly reduce N losses to tile drainage (apparently by building SOM). Conversely, NANI doesn't consider the priming effect observed when small amounts of added N fertilizer can cause proportionately greater N losses to tile drainage (for example, see Cookson et al, Nutr. Cycl. Agroeco., 56:99-107).

Page 102, lines 1-10: While the panel is correct that the IPCC estimates that 0.75% of the N leached to rivers is converted to N_2O, the IPCC considered denitrification in rivers to represent only part of the fate of N leached from soils. The IPCC considered leached N to cycle through groundwater and agricultural drainage, through rivers and then through estuaries on its path to the sea. Considering all of the N_2O losses of the N leached from soil, the IPCC estimated a mean of 2.5% of the N leached lost as N_2O, far exceeding the 0.75% just for riverine losses. Thus, the panel's conclusion on line 10 is correct, but less N_2O is produced if N is denitrified in the soil versus leached and denitrified in drainage, rivers and estuaries.

Pages 141-144: The discussion on landscape design is interesting, but contributes little to the analysis of the causes and remedies for Gulf hypoxia. The section could be deleted.

Page 155 and Page 185. The Cooke et al., 2007; Karlen et al., 2005; and Mosier et al., 1998, citations are missing from the references.

Pages 159-160: Captions for Figs. 41 and 42 list the equations incorrectly. The -0.33 and 0.67 should be raised as exponents in the two figures, respectively.

Page 182, lines 13-22. Switching from fall to spring N application may more reasonably be estimated to reduce nitrate losses to streams by 5-20%, not the 15-30% used here (see Randall and Vetsch, 2004, from the report's reference list, for example). In addition, the estimated N loss from tile drainage of 20-40 kg/ha is high compared to many observations (see Jaynes et al., 1999, under the report's reference list, for example). An estimate closer to 10-30 kg/ha would appear more representative of the Corn Belt on average. Using these more realistic values would greatly decrease the estimated reduction of N delivered to the MARB from this switch in N fertilizer practice.

Page 185, line 25. Actually, the study by Karlen et al., 2005, is the same study, but different paper, as reported by Jaynes et al., 2004.

Page 189, line 14: While we agree that more research is needed to examine the effect of N fertilizer rate near the optimum rate on SON and SOC and that most soil carbon studies have found little effect of near optimum N rate on SOC, the panel has ignored two important points. First, the N imbalances computed by Jaynes et al., 2001, are of a magnitude to be below the

accuracy of measurement for most SOC change studies. Second, other N mass balance studies also have concluded that loss of SON is a very likely explanation of a N mass imbalance (David et al., 2001, and Fig. 26 of the panel report, for example).

Page 195: The panel has identified many promising management options for reducing nutrient losses to surface water. However, most are still experimental or not fully tested. There is little data available that quantifies the water quality impact of any of these options at a meaningful water-quality scale such as a local watershed. In addition, it has not been shown how many of these practices can be fully incorporated into existing production agricultural systems. All of these practices need to be fully vetted at the watershed scale before major investment of public or private funds.

Page 201, line 32: This statement is misleading. It is not just the transportation costs of feedstocks that are slowing cellulosic ethanol production, but also fermentation inefficiencies with current conversion processes.

Pages 170-171, Figs. 43-44: These figures appear to be inaccurate and misleading. They appear to be based on Fig. B-1 and B-2 of the original USDA-NRCS, 2003, report, in which case they should be labeled as "recoverable manure nitrogen" and "recoverable manure phosphorus," respectively. These are not "excess manure N and P" as labeled in Figs. 43 and 44. Recoverable nutrients are those that can be incorporated into a farm nutrient management plan to meet the nutrient requirements for that farm. Excess implies that the nutrients available exceed the nutrient requirements of the farm and are thus susceptible to loss to surface waters.

Page 212, Table 17: Dinnes, 2004, in a survey of Midwestern studies gave a range for N reductions when switching from fall N fertilizer application to spring pre-plant of -10 – 30% with a mean of 15%. These estimates are well below the range listed here.

Page 217: Using interseeding of leguminous cover or relay cropping with corn is not a good example to use, as neither is currently feasible due to cold temperatures in the northern half of the Corn Belt — source of much of the N to the NGOM.

<u>Mallarino comments</u>

1. This very complex issue spans many disciplines. Although the science bases for many of the processes are not all too clear yet, there is a need to establish some general recommendations for possible immediate or future actions. Therefore, the panel should be commended for obviously dedicating a great deal of time and effort to this report.

2. The stated assertion that this report does not imply a mandate or immediate action by EPA, although strictly true, obviously will not hold in practice because, without doubt, this report will be used as the basis for regulation and litigation in the future. Therefore, EPA should have allowed more time for discussion, input and comments.

3. Current knowledge of the processes involved with the release and transport of several forms of nutrients from fields to streams and further to the Gulf clearly indicates that the target size reduction for the hypoxic zone and the time frame to achieve it is not reasonable. Especially for the case of P, many more years, perhaps spanning up to two decades, will be needed to be able to achieve even a fraction of the proposed target. This, of course, is good reason to start doing something different soon. However, much is being done already (for example, requirement or voluntary adoption of nutrient or manure management plans; environmental P assessment tools such as the P index; etc.). More rapid progress may be achievable if the area of row crops or cropland pasture is drastically reduced, but current economic and sociological indices suggest this will not be achievable without extreme economic or regulatory policy measures that are not viable in our political system.

4. The emphasis on changing the timing of N and P application (manure or fertilizer) to reduce nutrient loss is much exaggerated. Research cited in the report and many other studies clearly indicate that for N, the rate of application is by far more important and that for P, both the rate and method of application are much more important.

5. Although reductions of nutrient application rates are explicitly or implicitly proposed, there is not enough discussion of information related to effects of such reductions on crop yield and farm economy, nor how a reduction can be reasonably achieved. The report does refer to some new technologies that can be used to improve assessments of nutrient availability and nutrient application to fields, which is good. However, overall the report still gives the idea, mainly for people without a technical background, that the main reason for high nutrient export is over-application of N and P and that rate reductions "across the board" is the right thing to do and that it will go a long way at solving the problem. Research suggests this may not be the case. So the report should discuss and summarize this better into the final recommendations.

6. The report does not appropriately address the impacts of nutrient application reductions in long-term soil productivity and quality. For example, many studies have shown direct or indirect effects of nitrogen and manure application at increasing dry matter production, soil organic carbon and soil physical properties. Deceivingly small changes in crop productivity and soil organic matter can have a large impact on nutrient stability in soil and its release.

7. The case for impacts of cover crops in a major portion of the Northern Corn Belt and current knowledge about impacts is oversold. The climate and production systems in the Northern Corn Belt will not allow for a significant adoption of cover crops for row-crop systems unless there are significant direct payments and subsidies to farmers to alleviate economic losses. Furthermore, much research still is needed to have a clear understanding of the actual impacts of cover crops at reducing nutrient loss from fields in the northern Mississippi watershed, especially of nitrate.

8. The role of subsurface P application methods and tillage system on P loss from fields needs much further discussion and attention. The report does well in discussing the

potential of new precision agriculture technologies, such as variable-rate technology, but is incredibly weak at discussing impacts of tillage system and subsurface application methods on P loss with surface runoff. The report provides no support for the very wide (8 to 92%) expected impact on total P loss (Table 17). In fact, I found no discussion at all. By the way, this table is wrongly referred to as Table 16 on Page 224. The panel should have recognized that the role of the P application method is doubtful and could even be neutral when compared with surface application under row crops with aggressive tillage systems. However, much research has been published in science journals or conference proceedings indicating the benefit of subsurface P banding to reduce P loss with surface runoff in no-till and pasture systems. This is an important point because increased adoption of no-till in the Northern Corn Belt is an effective way of reducing export of sediment and particulate P, but this brings along the problem of P application and its effects on P loss with surface runoff. Because much research in the region shows no clear effects of subsurface P banding at improving crop yield, adoption of banding for environmental reasons with no-till may be an issue to be specifically addressed when developing policies and incentives to farmers.

9. Although much is said about — and based on — modeling, when addressing P issues little or nothing is said about the importance of distinguishing between particulate and dissolved P loss with erosion and surface runoff and how management practices affect one or the other. This is very important because some management practices the report refers to can have little or very large impact on one P form or the other. It is obvious, for example, that practices that reduce soil loss will significantly reduce particulate P export from fields, but they may or may not have a similar effect on dissolved P loss. Then, what P form is delivered near or far from the source is another important issue when we talk of water bodies near the source or as far way as the Gulf. Perhaps the panelists assumed this is well-known. But administrators and policy-makers who will read this report ought to understand that, for example, P rate reductions may have little or no impact in conditions with large soil erosion, but would have a very large impact in no-till or pasture. Therefore, this issue is very important when planning at the watershed level in terms of what practice does what when used in combination with others. Incidentally, the report lightly uses the word "synergism" between practices, when in fact there should be multiplicative or additive effects, depending on what is being considered.

<u>Cruse comments</u>

1. The detailed explanation of adaptive management is unwarranted. This section should be removed.

2. Explanation of different computer models is unnecessary and can be eliminated from the text.

3. Socio-economic barriers are identified as factors affecting nutrient management. However, socio-economic barriers are not mentioned in the cropping systems section.

Land ownership and rental arrangements play a large role in identifying what cropping systems can even be tried on a parcel of land.

4. Because large quantities of nitrogen are tied up in SOM compared to fertilizer or dry deposition N, slight changes in SOM can cause large changes in nitrate available for movement to the hypoxia zone. Different approaches were discussed to try to understand N sources — fertilizer vs. SOM. However, discussion of soil carbon modeling studies and their impacts relative to N release from SOM decomposition is missing and should be included.

5. The report focuses almost exclusively on market-driven choices, i.e., corn and soybean price will dictate what happens on the landscape. The resource advantage of crop rotations in general and of perennials for ethanol production is recognized, but choice not to move this direction is almost entirely explained in terms of dollars/bushel. Another key player is land ownership. Moving to a multiple year rotation or implementing perennial production is nearly impossible when renting on a year-to-year basis — the majority of harvested lands are rented in the central U.S. Even if financial incentives were in place for the "farmer" to move towards rotations or perennials, they may not cause change due to ownership issues. This becomes very complex; building incentives to create change when your target audience (management decision-makers) could be renters, maybe a landlord 500 miles away or a land management company that receives a cut of gross returns. I increasingly do not know what a farmer is and even more important, I am not sure what policy could cause change in behavior across the increasingly wide range of land management decision-makers. It is true that price (especially corn and soybean price) will dictate which crop is produced. Corn and soybeans are annuals and annual leases make annual crop choice decisions clear. Beyond that, creating behavioral change with policy becomes less clear.

6. The cellulosic discussion considers only fermentation conversion. Thermochemical opportunities could dramatically change transport and storage issues. This form of conversion is getting much more attention lately as it appears to be more carbon friendly and people friendly, and it seems considerably more distributed system friendly than the biological conversion. If distributed, a portion of the value added could stay close to the farmgate as opposed to being transferred to Wall Street. This gets somewhat speculative, but paints a different picture as it relates to what might be produced and where, and how it might impact hypoxia.

<u>Sawyer comments</u>

1. One wonders if any reduction in the size of the Gulf hypoxic zone is achievable, even with a 45% reduction in N and P load as suggested in the SAB report, since a >20% reduction in total-N and nitrate-N delivery to the Northern Gulf of Mexico since the mid-1990s has not decreased its size. There certainly could be a long lag-time effect, but seven years should have produced some change? Not only is this important for changes to

the hypoxic zone, but also to success with implementation of practices in the Mississippi River Basin, even the choice of "directional correct" practices.

2. Page 78, Table 5: For the Upper Mississippi sub-basin, the nitrate-N yield is 7.1 kg N/ha/yr, and TKN is 2.7 kg N/ha/yr. These are not large numbers; in fact, quite small for N. Can these be reduced significantly with revised in-field or other practices?

3. Page 78, Table 5: For the Upper Mississippi sub-basin, the total P yield is 0.8 kg P/ha/yr. This is a very small number. Can this be reduced significantly with revised in-field or other practices?

4. Page 81, lines 15-17: Has fall application of anhydrous ammonia increased since the 1980s? Sales data are not a good indication of that, as well as fraction of ammonia or total N applied in the fall.

5. Page 81, lines 23-26. Are there data that show that an increase in winter temperatures has resulted in faster nitrification? Is the warmer temperatures relevant if soils are less than 32F?

6. Page 92 and elsewhere: Are soil N resources being depleted with agronomic rates (or less) of N application? And, is this important for influencing Gulf hypoxia?

7. Page 126, line 23: Reductions in N are proposed (probably need to be) of 40 to 45% or more, but no data are presented to document those numbers.

8. Page 127, lines 16-28: Nitrate-N and TKN have declined more than water flux to the Gulf (total N reduction of 21%). However, spring N flux has not decreased more than the decreased water flow. What implications does this mean for the hypoxic zone and N management strategies to reduce N during the springtime flow? And is the spring water flow and nitrate-N and TKN flux most important for affecting the size of the hypoxic zone?

9. Page 167, line 45 - Page 168 line 2: Soybean does utilize soil nitrate preferentially, but the statement about "scavenge" residual inorganic-N needs clarification as nitrate loss in tile flow is approximately the same in the soybean year as the corn year. Being an annual crop, soybean has as much to do with nitrate losses as corn in the corn-soybean rotation. Alfalfa, being a perennial, can better reduce nitrate in soil and loss during the spring.

10. Page 178, lines 13-15: Does spring or sidedress N reduce N loss to water systems?

11. Page 178, lines 18-20 (and elsewhere): Will plant N stress monitoring improve N use efficiency, maintain corn yields and improve N use precision?

12. Page 178, lines 25-27 (and elsewhere): Use of N fertilizer sales data does not reflect appropriately the N application timing for fall versus spring/sidedress.

13. Page 180, line 13 - Page 181, line 4: Are tile-drained portions receiving higher proportions of fall applied N? And for the reasons stated?

14. Page 182, lines 1-16: Is the change from fall to spring a 0-15% reduction in tile-flow nitrate-N, instead of 15-30% as indicated?

15. Page 182, lines 30-34: Is the change in tile-flow nitrate with use of a nitrification inhibitor in the fall 0-11% or 11% as indicated?

16. Page 182, lines 41-43: Has this occurred for the Illinois Lake Springfield watershed where N-Serve with fall ammonia was a targeted practice?

17. Page 183, lines 1-6: How much research is needed to come up with more localized N rate recommendations/management?

18. Page 185, lines 41-43: Are there any data to support the effect of controlled release N products on nitrate-N loss in tile-flow or watersheds? Iowa research to date has not shown a N-rate change with use of controlled release N fertilizer versus noncontrolled release.

19. Page 190, lines 9-12: The Jaynes et al. (2006) paper measured tile-flow nitrate with in-season (late sidedress to tall corn) N management/application; it went up, not down?

20. Page 202, lines 13-39: Data on the effect of CC versus SC for tile-flow nitrate need to be revisited again, along with a comparison of the percent change between SC and CC from Matt Helmers' regressions for the Gilmore City data. Is any difference just the rate difference and not also more frequent application? According to the Gilmore City nitrate-N regression equations, at 175 lb N/acre for CC the tile-flow nitrate-N is 13.9 ppm and at 125 lb N/acre for SC is 11.4 ppm (mid-range N agronomic application rates for the rotations). Based on the SC tile-flow concentration, the increase with CC is 22%.

21. Page 211, lines 2-26: Are the ranges in the Dinnes (2004) and other references too wide to be of value here?

22. Page 212, Table 17: The effects of practices and the values for reduction efficiencies need to be checked, and maybe against the Heartland water quality publications. Also, urease inhibitors are listed in the first row of "nutrient-use efficiency" practices. Is there any data that suggest use of an urease inhibitor will lower nitrate in-tile flow? The argument is for keeping more of the applied N in the soil for crop use (increase efficiency), but wouldn't more N in the soil result in more N loss?

23. Page 217, Key Findings section: Will cover or relay crops decrease fertilizer N requirements, reduce profile nitrate-N in the spring, mine excess P and produce the water quality gains suggested?

24. Page 218, Key Findings section: Are perennials a realistic method of reducing losses, most effective in-field practice? Probably yes, but realistic (same for cover crops)? What about small grain systems? And should fall N application be discontinued (does this mean all fertilizer N and manure)?

Isenhart comments

General issues:

1. The SAB panel was tasked to focus its evaluation on three issues and questions (characterization of hypoxia, characterization of nutrient fate, transport and sources, and scientific goals and management options). Within these charges, they were asked to discuss options for reducing hypoxia in terms of cost, feasibility and social welfare. In the cover letter, the panel states that "to address this question, the SAB found it necessary to discuss policy options, in particular, those policies that create economic incentives."

- In public comments, several stakeholder groups have objected to this extension of scope and asked the SAB to focus its efforts on the reassessment of the original hypoxia science, to remove the economic policy recommendations and to leave all other policy recommendations to the Mississippi River/Gulf of Mexico Watershed Nutrient Task Force.
- Most of the policies discussed are federal initiatives, while at the same time we are advocating watershed-based, innovative approaches. Such discussion also would be speculative, given the current climate of agricultural and energy policies.

2. It is stated several times throughout the report (page 1, line 42, and page 8, line 30) that "since publication of the Integrated Assessment, scientific understanding of the causes of hypoxia has grown while actions to control hypoxia have lagged."

- This comment lacks objectivity and does not recognize the substantial conservation efforts of the Basin states and should be removed or reworded.

3. Page 6, line 13: Reference to "a point of no return" is speculative and should be removed.

Specific comments to characterization of hypoxia:

1. Page 34, line 16: "The strong P limitation during this period appears to be the result of the very high rates of N loading that have increased more rapidly than P loading over recent history (the past 50 years)."

- Chapter 3, Nutrient Fate, Transport and Sources focuses on N loadings and provides little evidence for an historical increase in P loading. Data collection to address this knowledge gap should be a key outcome of any additional monitoring recommendations.

2. A key difference in the science between the Integrated Assessment and this report is the introduction of phosphorus as the potential nutrient limiting primary productivity in some locations of the NGOM and during some times of the year. Because this question is so important for Gulf ecology and land management within the catchment, a focus should be placed on studying these biological processes in the context of the physical system, currents, mixing, residence time, etc. (page 46). Concomitantly, Basin states should adopt the "dual-removal" strategies as per the key findings and recommendations on page 55.