

## Transcript of Panelists' Discussion and Q & A and Audience Q & A

### Session 1: Characterization and Long-Term Trends of Hypoxia in the Northern Gulf of Mexico

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**Panelists:** William Battaglin, Piers Chapman, Robert Twilley, Tom Cronin, Craig Stow

**Alan Lewitus:** We will begin with Craig Stow. Do you have anything you want to ask?

**Craig Stow:** Part of our charge here is to identify the relative certainties and uncertainties in the body of evidence that we have been presented with. Nancy showed quite a bit of long-term correlative evidence showing a relationship between lots of different indicators and the relative size of the hypoxic zone. A general question I would like to pose to the co-authors is: How comfortable are they with these relationships—these sort of correlative relationships—as indicative of a cause and effect relationship, and what sort of information would be helpful to make that cause and effect relationship a little firmer? I am posing this generally as a kickoff for discussion.

**Donald F. Boesch:** In that we have a written draft of a paper and in it... I assume that your question has to do with the long-term history of it?

**Stow:** Yes.

**Boesch:** In the paper we try to look at the evidence that has emerged—the new information since the assessment that was done in 1999. And we look at the lines of evidence from five different standpoints.

One is the mining of older data sets. Nancy showed some of that in your own work, for example, that looked at the sea map data, and Gene's work that looked at the loop monitoring data—and to see whether they showed similar history. The short answer to that is they *did*.

The second line of evidence has to do with the results that Nancy showed on statistical analysis that Gene and co-authors have done. I will certainly let him speak on that.

The third line of evidence that you will hear more about has to do with the empirical modeling that Vic Bierman, Dubravko Justic, and Donald Scavia did, which try to go back in time with the history of measured loadings: what would be the rise of hypoxia over time? That was also another line of evidence, which basically gave the same result in that this phenomenon really became very prominent on the shelf in the late 1960s to early 1970s time period.

The fourth line of evidence has to do with the new additional information on the sediment record that was not available in 1999, that includes more of Foram (Foraminifera) work

that our colleague here [referring to Barun K. Sen Gupta] has done but also, Lisa E. Osterman from USGS, and the new pigment work that is available since then, again with similar results in terms of the chronology.

The last issue has to do with, what we call in the paper, the coherence with events elsewhere. Nancy just showed that one slide that showed you these phenomena happened widely in the 1960s and 1980s, depending on the coastal system. In some shallow systems, they resulted in sea grass losses and harmful algae blooms, and in other deep stratified systems they resulted in hypoxia. So one would ask the question with the record of increased nutrient loading—nitrogen and nitrate loading in particular—to the gulf, you would be surprised if it did not happen in that time frame, given the documented evidence of it elsewhere in the world.

One of the best documented cases (and information is only recently emerging because of the political history) is the northwestern Black Sea where the Soviets had actually monitored the distribution of hydrographic properties on the northwest shelf back in time and were able to document this same sort of phenomena happening and growing in that same time period.

The thing that I think there is probably the greatest amount of uncertainty about, simply because we are unable to have the hypoxia<sup>1</sup> records all over the shelf in terms of sediment history (because the shelf where hypoxia occurs is for the most part non-depositional, it is periodically erosional, it does not leave that record), is the extent of the size of hypoxia. So I think that we have good information that would suggest that it developed and grew in this time frame. We have good information to suggest by reason and a little bit of evidence that it grew in size as well.

**Lewitus:** Anyone want to continue on that?

**Turner:** Sure. Well stated. And what we have uncertainty about is what is going to happen to the driving forces. We do not know if the percentage of urea fertilizer is going keep changing, it has been going up. We do not know if the farming practices are going to go to more perennial. We do not know several things about climate and discharge relationships. We do not know how the soil system is going to behave under different cropping patterns. The explanations by correlation, for example, are describing something that has happened by definition, and we do not know for sure how this inventive human society is going to change (in positive and negative ways) the driving forces. So that is a major uncertainty. And I just want to back up. We have most of our data from the eastern side of the shelf. We have some lesser amount of data for the west of the Atchafalaya Delta. And what we do have fits with the story, and it has lags as we would expect for a system moving westward expanding. But we have less data for over there. Those are too uncertain—things I would like to have build up a little bit.

**Robert R. Twilley:** I have a question. I guess I will sort of preface this by—I would like to focus from the characterization, in a restoration context, it is all about goals and targets

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<sup>1</sup> Could have been “proxy.” This was hard to decipher from the tape recording.

and things like that, and a subsequent and adaptive management view is looking at assumptions and defining relating to what you are trying to accomplish. So, given that, I have a couple questions. I will start with this one: What I looked at, when I read the document and looked through the material was the consistency and the assumptions related to system response. And it was very interesting to me that in some of these indicators of system change, either model output, historical sediment documentation, or actual observations in the field looking at historical nitrate loading, that you almost get decadal differences in resolution of when hypoxia really started to occur. And you actually find very different comments related to when the events started to occur. I would like to get some resolution on this, because the key is that 1960 is a major point relative to the time frame when the systems started to degrade, and that extends up to 1980. So that 20-year record is a record that is really of interest. You get a lot of different perspectives depending on what metric or method you are using. During 1960 to 1985, there is a huge change in nitrate loading. That is when the real slope really started to go up.

So, here is my question: The sediment record refers to 1960 as the real key change in the system. That is the onset. The nitrate loading it sort of associates with, follows that a little bit—the nitrate loading takes off, I think, in the 1970s and some of the model results show that hypoxia historically really started to be present in 1975 and even 1980. So that is a 1960 to 1975 difference in context of when the system really changed. Is there some resolution, explanation, of why these do not focus more on a clearer time frame?

**Turner:** Part of the answer is that the data for these are not for the whole shelf where the size of the hypoxia is for the whole shelf. So when you measure size of the area coverage of the shelf, you are not covering it with the same sediment record data for the whole shelf. You are getting parts of it. Another is that when you date sediments, it may say it is 1960 on the graph, but it is actually recognizing some integrated benthic turbation layer that might be a few centimeters thick actually, which could represent 10 years without any problem. So the resolution of sedimentary record is a smeared record. Within a decade—it is very rare that you get a layered system like the Chesapeake Bay—they do get layers there. We do not have tree rings. We do not have depositions that are light and then heavy, and light and then heavy, every year. So there is a smeared record as well.

**Twilley:** And the thing that is interesting about this is that...

**Turner:** And the third thing is that organisms do not respond all the time to a step function. So, you might have an appearance on a ratio when it starts to change and these are ratios, which means logarithmic not a linear ratio, embodiment of some change, and so they are not responding just... Okay I am going to stop, today. So it is a record also that has some gradual transitions to it.

**Twilley:** So the model results pretty much define, I think, 1980 as when hypoxia really started to occur on the shelf. In your paper, Gene, around 1975, 1980 and the question is, if, depending on when the onset of the system change occurred historically, could that be used to actually look at nutrient loading rates and the concentration at that time to really

define targets and goals? That is my question. Can a historical context be used, and is the resolution good enough to help focus goals and targets?

**Nancy Rabalais:** One of the issues is that, like Gene said, the time is smeared somewhat, and there is not a step function in response so that it is the averaging over say a 3-year or 5-year running average that tells you something about the system. Because there are nutrients still remaining in the soil, there are nutrients remaining in the sediments offshore. The system is not a “do something and respond immediately” sort of system. So, as far as the trends show changes over a longer period of time, that incorporates a lot of the natural variability in the system. And so, if you look at it in a broader sense, on a 5-, 10-year period, then yes, you can target some goals. But if you try to pick a specific year in the response for that year, I do not think it is going to be very productive.

**Boesch:** Robert, it bears some analysis. I think your question, your point, is very important, that if we could better understand the timing of the change, we would have empirical evidence of what the loadings were that caused the change.

**Twilley:** Exactly, that is my point.

**Boesch:** I think that it is important to remember several things. First of all, this system, like all other systems that have been studied with sediment records, were altered by, had a human effect on their metabolism before the 1960s increase in manufactured fertilizer, simply by killing the soil and changing the hydrology and the flux of materials to the coast. So there was some effect before then.

My recollection is, from looking at the charts, that if you would ask that question and put a suite of model estimates and the paleo-data, and so on, what they would show is that, in the period of the mid 1970s to about 1980, is when things kind of tipped. If that is the case and if you look at the nitrate loadings then, you would draw the conclusion that the reduction of nitrate loadings to get us back to that point would probably be about 50 percent, something of that order, which is not that different than what the empirical models are showing.

**Twilley:** OK.

**Turner:** One other thing is that if you had a tipping point, as you said, that is because of the precursor conditions, part of which are the nutrient loadings that preceded it, which leave organics in the sediment, which have a respiratory demand. So if that respiratory demand is building up over 20 years prior to that, it is contributing to what happens at that tipping point. You are removing oxygen because at a certain level, depending on the previous years' depositions and events, all the carbon previously...So having an individual year or period is dependent upon what happened all the years beforehand, which were building up with our nitrogen. So this means that simple linear relationships will not necessarily work. You have a memory to the system, and you have lags because of that memory. That also means that on a restoration goal, which was brought up, you

also have to consider these flags of the memory in the system as to how long you are going to get a response. It is not fair to say that if you cut off 50 percent of the loading right today, say in discharge, you would have 50 percent reduction in size. The system has a memory to it. And part of that is stored in the sedimentary record, which is a respiratory demand.

**Michael Murrell:** Can I follow up on that point before we move from it? Do we have the information necessary to sort out the contemporaneous demand that is the water column biochemical oxygen demand (BOD) from the sediment oxygen demand? Can we separate those at this point?

**Rabalais:** Yes. Well, there are... we are getting into biological processes, Mike, which we have not heard a lot of the information yet. Typical systems would be a third of the respiration in the sediments and two thirds would be in the water column. And that fits with some of the oxygen isotope data that Justic, Fry, and Quinones-Rivera have, but some of it does not. That is still not completely known and the proportions of those respiratory... the relative proportions will change through the season as well.

**Unknown speaker:** As well as the place on the shelf.

**Rabalais:** As well as the place on the shelf.

**Michael Murrell:** I might add that since June of 2003, in the research that we have been doing at EPA, that is exactly what we are looking at. One of the things we are looking at (not put it together yet) is the relative magnitude of water column versus sediment oxygen demand and those sorts of rough estimates; depending of course what the bottom water oxygen concentrations are. If they are low, there is not a big oxygen demand in the lower water column and in the sediment.

**Barun K. Sen Gupta:** May I add a minor point? Some of the Foram indices would be affected before the dissolved oxygen reaches the 2 mg/L level. So you will see those tipping points come before the 1970s. We are using multiple indices here, and the tipping points will be different.

**William A. Battaglin:** I got one other more technical basis rule number one. You made a comment, Nancy, that the area of basal hypoxia did not necessarily correlate that strongly or perfectly with the volume of the hypoxic zone. Could you make a further comment about that? We are tying a lot of our effort to that area estimate and yet is that the best estimate of hypoxia or the most accurate estimate of the extent that you expect, or are there better measures that are a little more cumbersome to use but maybe we should be thinking about them?

**Rabalais:** That is a good question. The volume data just are not as tight as the surface area. But the surface area just does not always tell you the amount of oxygen deficiency, which is what you would relate to the carbon loading and the nutrients that fuel that carbon loading. So volume is definitely another measure. It just has not been done

consistently yet. We do have the data. We are just not that happy with our volume estimates right now.

**Boesch:** I think one of the reasons they focused on area is that those have been kind of the usual metrics, annual metrics. But in addition to that, people are concerned about benthic resources, you know—shrimp fisheries and that sort of thing, so it has some relevance in that. I would predict that as this evolves, it may involve something like it did in the Chesapeake, when the assessment gradually came to the realization that you would probably never get rid of hypoxia. There are some areas that are really resistant. They are going to be the first places to become hypoxic. And if you reduce the nutrient loadings, you are still not going to improve them very much. So, in that case, it may well be that you would zone the shelf and say these are the areas that we know that we are going to have recurrent hypoxia. These are the other areas that we will not, that we do not want it. Then you can develop models to help you understand what kind of nutrient management strategies get you to that spatial set of goals in a more sophisticated way.

**Piers Chapman:** This is one of the areas where the three dimensional models are really going to help us. This is the ROMS (Regional Ocean Modeling System) models and things that Rob Hetland, for instance, is using at Texas A&M where you can actually apply different forcing functions and see how both the area of the hypoxia and its volume change over time.

**Rabalais:** I know the Chesapeake Bay people do very good volume estimates, but they also have a more confined system. It is a little easier to know and define the size of the hypoxic areas. We have a fairly unbounded offshore system, and the hypoxia does not just neatly go to a closure because sometimes it dips over the shelf. So it is a more unwieldy volume calculation than an estuary, you know, a bounded estuary.

**Battaglin:** Following that then... Because that is really important, again, going back to goals-assessment. If this is going to be done every 5 years, the question is going to come up, Donald, that you asked earlier. And that is that this whole program is going to be looking at performance measures to actually gage actions up in the watershed related to system response, which is why I am pushing that. And so the metric by which you use to define degradation and which is going to be the performance measure, and by which you are going to establish effectiveness related to whatever is implemented in the river basin is important. The *area of extent* as the goal verses the *volume of the hypoxia* verses the *monitoring*, which we have not gotten to yet, and the *strategies for monitoring* are critical. And so is area the most appropriate metric to use?

**Boesch:** I would think it is a fine metric to start with. I think that in an adaptive way you can become more sophisticated with your tools and so on. When the Chesapeake Bay restoration and the nutrient goals were started, the first commitment was in 1987 and that was for 40 percent reduction of both nitrogen and phosphorous. Twenty years later we had more refined models, and it came out that actually the best estimate to reach the areal-specific goals that I just described, you would probably need a 56 percent reduction of nitrogen. Now if you waited until you did all that refinement before taking action, it

was in the range of 40–50 percent just as I think we are presently there with the crude models that we have, that of 30–45 percent (that is the range in direction). I think you can refine it as you go forward in a sophisticated management sense. But in the end, does it really matter because of the high variability of the natural system? You are not going to be able to control what it is in any given year anyway. So, I think as a present goal objective, it is the right direction; it is about the right amount. I think that as we go forward, we will be using these models to get more sophisticated indicators, not necessarily to know exactly whether the benchmarks have been met but to understand how the system is responding... Are we getting the right results, in a qualitative sense and a quantitative sense?

**Lewitus:** Any more questions?

**Chapman:** The point is that even if there is no hypoxia, you are still going to have less than saturated oxygen on that shelf. So, I do not know what a normal value is, probably somewhere around 3–4 ml/L which translates to about 6–8 mg/L, something like that. That would be zero hypoxia. So I think that, at some point, we are going to have to start bringing in these different measures and say, okay, how are we improving the total oxygen concentration over the shelf? If we do that then if you just... whether you then... actually want to quantify in terms of the area that is effected or the volume of water that is effected or take out the working concentration units you can get three different things. I guess that is something we need to worry about here as to what is the best way to measure it.

**Lewitus:** Let me move to...

**Rabalais:** Can I just make one comment? The unit or the metric does not change the fact that the system has definitely changed.

**Battaglin:** Agreed. I am not arguing that. I am arguing... We are here to discuss assessment. We are here looking at the program every 5 years and performance measures and that whole idea. That is all I am asking. I am not arguing the degree of degradation. What I am looking for is a metric that quantifies that and can be put in the context of an assessment. It is not only the metric, but *when* the metric is taken, *when* you take the pulse of the system. So my question here is, right now the characterization is pretty much based on a fixed..not a fixed time...but it pretty much ended in July...the last 2 weeks of July? (This was described very well in the paper.) And as we saw today, there is a huge uncertainty about events prior to when the measurement is taken as to what you will see in terms of characterization. Given that, and the new model refinements, I am curious what your response would be to either normalizing the assessment of the system's degradation after a series of pre-condition...situation, like a *project conditions prior to the survey* or using modeling to define when you think hypoxia, onset of hypoxia, would occur through the models and then run your monitoring programs to test it. I am just curious what the thinking would be... Or have a combination of the two. Keep your fix because that is a beautiful long-term record. You do not want to mess that up. But I am curious relative to assessment. If it is one major survey, you do have a timed series at one

station which is another issue is, how do you extrapolate that over the area? But what I am...question...begging...you know, sort of, getting your response, is characterization—with the noise—to really articulate in assessing effectiveness of change. That is a real problem.

**Rabalais:** For the simple metric, the size—with enough years' worth of data—there is a very tight correlation, even with the natural variability. We have seen with the additional data that, as I showed, if you are out there within 1 to 2 weeks, you are going to get the same area. If you are out there a month before or a month after, you are going to have something different. That depends on the currents and tropical storm activity. If we had gone out and measured hypoxia in 1993 during the peak flood period of the river while that tropical storm was in Campeche, there would have been no low oxygen. It is the luck of the draw right now as to when you go. But there is enough consistency in that period of time that you can keep to that metric. We definitely know... I mean, we have the models that show the changes in carbon, the changes in net production and the changes in respiration. We have the whole time sequence worked out. But, we do not have the unknown shift in winds that would change the currents that would move it closer to shore, closer to offshore. We do not have the crystal ball that is going to tell us whether a hurricane is going to pop up in the middle of the whole very well-defined seasonal cycle of carbon production and accumulation in oxygen depletion. If you could get rid of the physical part, you could very easily predict when you could go out and measure, say, the peak of the low oxygen, because we know how the strength of the stratification changes over time. We know how the oxygen consumption rates change over time. We could probably very easily predict when the best times are to go out, to get a consistent metric if they were not part of the system that was totally out of our control. I can see the goal to be more specific about knowing exactly when to go, but I think the consistency over 20-something years now, shows that it is a fairly robust measure.

**Boesch:** That is part of the reasoning why the goal would be, the Action Plan goal is a multi-year, 3-year, 5-year running average. Better to kind of sort, then you can iron them out.

**Turner:** Was the question, is there a two member choice that is that either you use existing long term records, which are imperfect, or you use a model to decide when you make the assessment? Those are a very different choices.

**Twilley:** It was the strategy, again, of 10 years or 20 years from now, you will be looking at this data set to evaluate effectiveness. So my question was now that you have the lessons learned, and you really understand the system, is this a point of time to start thinking related to how you are monitoring the characterization program to be used to continue the long-term perspective is there a strategy there? Now that you know the effects of storms, you know the effect of physics, you know the effect of discharge, you have all these correlations to actually start testing these assumptions in the context of your characterization and push the envelope even farther. I know the modeling (session) is on the last day, which I am sorry, I think modeling should be on the first day. Because that (modeling) sets the context how you ask questions and what you do. But then

everyone follows along with that, but I think in the modeling context, then that really defines the questions and in fact then you get into a model testing mode in your characterization program, and you actually chose a time to go out and, okay, the onset should be at this level based on our models and run your characterization program there. I am looking 20 years from now, how this program is going to assess the effectiveness of nutrients reduction up in the basin. Given the variability of this system, it is going to be tough...

**Turner:** In fact it is that is involved with the programs now, because that is why we have moorings in a particular place. The model is not going to drive everything, but it becomes a tool, and that is in fact what we are doing now.

**Twilley:** That is my recommendation, using it as a tool, in the context of testing it.

**Turner:** Well, I think you will find that the models are being used now. They are being developed, and that is the reason why this is a social issue. People know what the modelers are coming out with. We do talk to each other and some of us do not talk to everybody, some people are in the periphery, but the modelers are involved with this, but it is not the determinant. I would be a little afraid if the model became the determinant about how we sample, because models are based upon sampling and you have to.

**Twilley:** You define your observation and then you go out there and...

**Turner:** I am saying use both of them, but not exclusively.

**Boesch:** It is an excellent point, Robert, I think you are right. We have a better way to understand the metrics. But, you know, talk to me when you get a 10 percent reduction in the nitrate loading in the river. Then we have a challenge.

**Battaglin:** I have a question for you. Related to the data, is the current frequency of information coming from there in terms of the flux of nutrients adequate for your modeling efforts, or is seasonal data good enough, monthly data good enough, or do you need higher-resolution information to have better models?

**Turner:** The USGS should have their budget doubled for this. They've had a major...*[laughter]* It is right on the edge of not having enough. It is crazy. I mean you are left sometimes with 8 samples in a year for St. Francisville or 12, and this is a monthly sample. We are doing it weekly or biweekly in Baton Rouge, and, fortunately, things are working out. But you look at the variability from day to night sometimes or from week to week, it is crazy. This is a very rich country with a major river in it, and we have a few stations down here that are being covered It is terrible.

**Chapman:** When you get out on the shelf it is worse. Because essentially there is one station that you sample continuously and two minor stations that you sample monthly or bimonthly. We tried to put a station up that would also run continuously. That got wiped out by the hurricane as well. There are not even any weather buoys off Louisiana.

**Turner:** There are 15 states signed off on this pack. Fifteen states are signed off, and we are looking at the major driver being the loading at St. Francisville. And it is based on 12 measurements a year? It is out of proportion, it is not even one for every state.

**Sen Gupta:** To introduce one word of optimism here. Does it count that the historical data at least does not contradict the model?

**Greene:** Let us give the steering committee an opportunity to ask any questions they might have. I know there are a few....I will start off then. Maybe we read this enough, but I just wanted to ask it one more time. In terms of the measures of how well we are doing in the future, is it possible to know what the uncertainty is around the areal extent estimates that you make every year during the same index period? Is it 15,000 square kilometers plus or minus 30 percent, or I know there is tripulation.

**Rabalais:** You cannot have those data unless you do a cruise every week, to know what the change is from week to week.

**Greene:** It is possible from some of the back-to-back surveys that you showed there may be more of this too to take a stab at estimating what the uncertainty might be say on a biweekly cruise, bay cruise basis.

**Rabalais:** Not from the existing data. I would not say. No. Because there is different coverage of different areas, they are not even covering the same area.

**Greene:** But you did show, Nancy, one slide where there were three different groups cruising and occupying the same stations, I think, in a much narrower, maybe a narrower sampling regime, rather than a shelf life estimates. Let me put it this way: is that important to know as we move into the future on how well we are doing through the Action Plan? Is it important for us to know what the certainty is in that estimate?

**Turner:** It is our job to reduce uncertainty. So we need to work on those problems. And we have to distinguish between *within* sample variability and *among* samples. You would have to do it, as you said, through repeated measurement. There are ways to put bounds around some of these measurements, because you know from mooring data there is certain variability from day to day. So, what is that? We know among mooring data there is certain variability. You can scope out what it might be. But unless you actually go out, one day the whole coast, the next day the whole coast, the next day the whole coast, it is tricky. It is expensive. Well maybe NOAA is going to pay for this, are you? But we have predictions that are down to 80 percent. I mean that is pretty darn good. But by a lot of other measures, if you reduce the variability from year to year down to some simple-minded predictions using simply minded tools down to 80 percent of variability, that is pretty good relative to other human endeavors. You cannot even get the federal budget down to that 6 months ahead of time.

**Rob Magnien:** This whole issue of monitoring both in the river and out in the gulf has been pretty actively discussed among the agencies the last couple of years, so any specific guidance that come out of this symposium would be very helpful to us. At a minimum, underscoring the need for it, and we will work out the details later. But if there is a specific guidance for either one, in terms of uncertainty estimate, or linking the needs of the river input monitoring with the needs of the temporal and spatial scales, of offshore monitoring, that would be very helpful. And give us a little more ammunition to work with in justifying those obviously needed expenditures.

**Dugan Sabins:** I will add that, of course, our agency is very interested in water quality in the Mississippi River. We have been trying to push for increased monitoring for years. Unfortunately it is budgets and priorities up and down the river, and we are probably the only state that actively monitors the Mississippi. We have various reasons to do so, but we are trying to work through the system with a lot of other organizations, one of which is composed of lower river states, to get greater monitoring, because I am sure, it would help us to know above St. Francisville. And what we already know from USGS data is, we see a pretty consistent concentration flow for the major nitrogen species and phosphorus, that it is pretty consistent. We had a station for many years at Lake Providence, and statistically you would run all the correlations you wanted and there would be one-on-one correlation between Lake Providence and St. Francisville. In fact, you could go on down to the lower river and it is remarkably consistent. We work with some of the dischargers—in particular with the phosphate fertilizer facilities—and once we got their discharge under control and reduced 90 or more percent, the phosphorus was virtually unremarkable in change down the river. But we still have three stations, and I keep wanting to make sure, maybe Jane I need to get with Charlie Demus, and our local USGS folks, and maybe we can at least put a stronger monitoring package together for our lower river to look at the different stations. Now, unfortunately, we do not do as detailed a series of analyses as USGS does, which is probably one of the reasons your system is more expensive and always ends up on the cutting block. We do nitrate, nitrite, total kjeldahl nitrogen (TKN), total phosphorus, total nitrogen. So, I think we can put that to work in our benefit and maybe between the state of Louisiana and USGS, we can at least work closer to get the lower river adequately monitored. We need to understand, Nancy, from what you are needing from the lower river, how much more monitoring we need and what type of monitoring. We have a historical database too, that we can tap into. Some of the stations are no longer collected, but in the case of St. Francisville, Donaldsonville, and Plackman area, we have maybe 25 years or more with the data that we can bring to bear. So, I am agreeing on the monitoring. It is a frustrating thing. And the hurricanes do not knock out the river stations, at least. But I understand the problem we have in keeping these stations.

**Boesch:** I think that the issue...In response to your question, Rob, from my perspective, I do not speak for this whole group. These people are trying to keep together their long-term monitoring in the gulf, which is important. But essential to this is better monitoring in the watershed and the river, and it is not just the St. Francisville, or the lower river load estimations, *rim estimates* in the Chesapeake Bay parlance, but it is a network throughout the whole watershed. So if you do take action, you have a better way of understanding the

effectiveness of that action. You have a better statistical basis of detecting trends that is going to be the important thing, as you design that. I think it is absolutely essential, and I would agree with Eugene, that it is criminal that we are going the wrong way in terms of national investment on this.

**Magnien:** Right, I think there is. I was thinking about the flow about the symposium. There is a really good opportunity to do a wrap, what we know, what additional uncertainties there are, and maybe that is a good place to really put down these specifics I was talking about before. I think the upper basin monitoring issues will probably be more prominent in some of the other symposia. But we should nail down the river mouth issues and the needs for the offshore models in this symposium.

**Chapman:** And do not forget the Atchafalaya, because the actual constituents coming down the Atchafalaya are different from what comes down the lower Mississippi because of the dilution by the rain<sup>2</sup>. And the volume hitting the shelf is approximately equal between the Southwest Pass and the Atchafalaya.

**John Wilson:** I had a question. We were talking a lot about the variability of uncertainty, but there have also been some comments made about the long-term trends being really well established. I wanted to ask the panel how confident they are in terms of basically the estimates of onset and duration. So how comfortable are we, knowing that what we are seeing as a long-term pattern is a pretty accurate description, and given that, have we seen any change in the last decade or so with the advent of significant agriculture practices or other things up the watershed? Has there been any change in that overall?

**Boesch:** What significant alterations or practices? I think that the biggest question mark in my mind is—I cannot speak for my colleagues here and Eugene’s analysis tries to deal with it—but in respect to this year factor, which is sort of a quantified term that embodies a whole bunch of things, is that with a leveling off of nitrate concentrations in the river, more or less, variability in loads, as opposed to by year-to-year variations, we still seem to have had in the 1990s and early 2000s a secular trend and increase in the volume of hypoxia, area of hypoxia. That is an interesting question, and I do not have a ready answer for it.

**Chapman:** The only thing I would say is that the 1990s was generally a wet decade, so you are going to get more runoff, which would create more stratification. That will bring more nitrogen down as well. But within 1 year, the actual peaks vary randomly, and if you can tell me what the runoff is going to be 9 months in advance or what the wind system is going to do 9 months in advance, then maybe in 5 years’ time, with the model development, we will be able to tell you what the hypoxia going to be. But at the moment, it is just a random number generated.

**Sabins:** That is a good question. Can I ask if Gene or NOAA was going to make an estimate prediction for this year?

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<sup>2</sup> Possibly “rim”. Hard to decipher from tape recording.

**Rabalais:** We have already had a couple of peaks in river discharge, and we have had an all-time low in river discharge this year already. Oxygen is going down as is typical for this time of year but not as early as typical for this time of year. It is hard to say because we have not seen the flow for the whole year yet, or the hurricane frequency or severity.

**Sabins:** Yes, that is a good point. I think we have not been within 10 feet of flood stage in Baton Rouge, the whole year yet. Pretty remarkable. That is a sign for crawfish.

**Magnien:** I would like to move on to the audience questions. We had only two questions from one person, both of which were addressed to a certain extent by discussions already.

**Lewitus:** I promised something from Tom Cronin, another panelist. However, what he is addressing really fits more into session 6 because he is talking about the importance of climate change and long-term changes in precipitation with respect to future planning. I think that belongs in the forecasting model issue. I know they are going to address that pretty well. So if that is OK we will just hold that off. *[Note: See addendum for remarks submitted by Tom Cronin.]*

**Magnien:** So I will read these couple of questions. If you feel you have already addressed them, these were submitted before the discussion, we can move on. Then, hopefully, there will actually be some time for an open mic discussion for other questions that have come up. So, the first question was: The point was made or raised about area versus the volume of hypoxia region. What is the relationship between the volume of hypoxia and flux?

**Twilley:** Positive.

**Rabalais:** It is positive and it is not as strong as area.

**Magnien:** Again, that was discussed, so we can leave it at that I guess. What is the lag time used during modeling efforts? Is it necessary to invoke a 1-year lag and how does that play into the correlation between “N, load, and areal extent of hypoxia?”

**Rabalais:** I think we answered that but ... Just like there are running averages in loads and changes through time, there is hysteresis both in the land system and the offshore sediment system. So accumulating data over time gives you a more consistent or a stronger relationship over that period of time. And, also, I had another thought, if you... I cannot remember what I was thinking. It also depends what question you are asking. If you are asking loads versus areas, it is very different question versus load and net primary production with distance from the river or something like that. So, it depends on what question you are asking as well.

**Magnien:** Don and I know that in the Chesapeake work, this has been a big issue: the lag time, and movement through ground water, and such. I imagine it is only greater in a basin of this size. There is a linking up of the watershed with the aquifer.

**Boesch:** To a certain degree not, because we have these systems, these drainage systems that Jim Baker talked about and so there is a lot of flux of nitrogen quickly in the system but, he is not here is he, our friends from...Gregory McIsaac and company...actually tried to incorporate this in a watershed basis with the models. What was the finding...like 2-year period...or something like that. He tried to do the best fit in terms of the input of nutrients and the output adding into that water flow and found that there is a lag effect in terms of the budget of nitrogen input in terms of when you see it. And you can fit it pretty well. It does not mean that all of it does not come out, but within 2 years. The flow and rainfall, as the first panelist (people from the upper basin) mentioned has a very important effect because it flushes out a lot of the accumulated materials; so that is one aspect. The other one that we really do not know much about, Gene alluded to in his comments, is the residual deposits on the shelf. The residence time on the shelf is less than a year of water. So it is gone in a season. It is not going to come back and stay dissolved in the water column. But the sediments, of course, and the benthic deposits stay around a lot longer. And it is not only an issue of organic matter but, I am increasingly believing, that it is really an issue to understand phosphorus...which I am increasingly convinced is basically from benthic sources, regenerated. It is a very open dynamic shelf a lot of resuspension. As soon as you have oxygen stress, the sediments just flux phosphorous like crazy and it is probably a lot. That may be a long-term effect that we would have to deal with over time either through burial of off-shelf advection of that particulate matter.

**Lewitus:** Other questions, audience? Just go to the mic.

### Audience Q & A

**Dave Hollinder:** I have my cards but obviously did not pass them forward. Sorry. I am David Hollinder from the College of Marine Science in the University of South Florida. I am representing not only our group, but also a research group with the USGS. And the question is, with the recognition that low oxygen events of an equivalent magnitude have occurred repeatedly prior to anthropogenic influences, how are these events incorporated into a long-term revised research action plan? What are the mechanisms responsible for these historical events, and how do they compare to the contemporary hypoxic events?

**Rabalais:** I am not aware of any historic extensive areas of hypoxia that have been shown in the paleo records.

**Hollinder:** I had a recent *Geology* article and another article that is co-authored by I think your husband, Eugene Turner.

**Rabalais:** The *Pseudononion atlanticum*, *Epistominella vitrea*, and *Buliminella morgani* (PEB) index shows conditions of lower oxygen, and many of those are in deeper waters on the shelf. It does not necessarily indicate hypoxia, because there are different foraminiferal species. I will let Barun address that.

**Sen Gupta:** Yes, the PEB index deals with three species whose proportions apparently go up if there is oxygen or possibly some other stress in the environment. The species are

not increasing in their populations. The index goes up because something is going down. And it is not necessarily the same species that are going down. If you look over an extended period of time—100–200 years—you have to ask what has gone down during those periods that shot the PEB index up, and we have not gone into these questions yet. Although I do see in terms of the last 50 years' record, a correlation between the PEB index and other foreign indices that have been tested against organic carbonate sediments or actually measured oxygen values. I do see a correlation between such indices and PEB indices for the last 50 years. I do not know—maybe Lisa does—at this point, what the periodic increases in PEB index in times past mean.

**Hollinder:** I will just state that their magnitude (regardless of what the causes for their increase) in events before 1900 are certainly of equivalent size or of equivalent magnitude. And interestingly their duration is not short lived. Their duration is certainly on the order of decades, which is sort of what one sees in the most recent record. I just think, I am just putting out the point that maybe the historical record of the low oxygen events identify unique or not unique mechanisms and processes. And I think to put your head in the sand about the concepts that the historical events may be quite significant in understanding the entire system is quite relevant.

**Boesch:** I am not an expert in this field, but I did look at the upper basin workshop that was held. There was a presentation on this. And there was a graph that showed that PEB indices down depth with cores down into the 17<sup>th</sup>, or 16<sup>th</sup> century, I believe it was. If you looked at that, there are two ways to look at that: you had this little peak up there that also had this same PEB index. But it was one measurement. And if you looked at the top of the curve they were all consistently at this high level. Now it is reasonable to think that this big river has always been efluxing lots of material including fresh water, which causes intense stratification for a very long time. So to think that hypoxic events never occurred over time, there is no reason to think that, simply because of the dynamics of the river. I think the difference in interpretation is that there is nothing in that record which is the same in terms of the consistency of year after year trends like this; accompanied by all these other indicators—geochemical, biogenic, and silicic—that show high levels of production and that is at all precedented in that record.

**Turner:** As a co-author on these papers, I want to make sure you hear this. It is low oxygen that is correlated with this index. It is not hypoxia. Hypoxia is 2 mg or less. Low oxygen is an undefined term. It just means it is lower in saturation. So you got caught up in the press at one point that this low oxygen meant hypoxia. That is not the case. It is an unquantified relationship between the PEB index and what the oxygen value is, unlike the *Ammonia parkersoniana* and *Elphidium discoideale* (AE) index. David, that does not mean it will not work out. And I am not saying, “Do not use these indices.” I am just saying be careful about the interpretation, as Don was saying.

**Hollinder:** It does show ecological responses.

**Turner:** Sure. And those were consistent with the period where there was a massive flood on the Mississippi River. And there were two standard deviations outside the average.

**Hollinder:** Of course, there has been more since the publication came out, and there is repeated reference now of long cores, piston cores that identify multiple events, not singular events, that can be related to in shore and offshore systems. So it is not only in the deep water system, it is also in the shallow water system.

**Lewitus:** Is your hypothesis—based on what I know about it—that prior to the 1950s the causes of hypoxia were different than after the 1950s, based on isotopes?

**Hollinder:** Well, what we are recognizing is that there is a significant microbial contribution in, not only the contemporary events, but also in the pre-anthropogenic low oxygen events, what we are interpreting as low oxygen rather than hypoxia. We have recognized, indeed, that in the most recent time of course, it is a marine-derived organic matter that seems to be perpetuating the respiration. In the historical perspective, indeed some of the isotopic indicators suggest it is more of a terrestrially derived source. But there is a very, very effective microbial recycling process that is going on that we can also see in the most recent record. So again, using pigments has an advantage and a limitation based on their decomposition. Other types of organic molecules have a longer lived geologic perspective that allows us to interpret perhaps mechanisms and processes in a different way.

**Boesch:** You are saying that those earlier events were driven by organic carbon sources?

**Hollinder:** Yes. In terms of, they are organic carbon sources, but we do not necessarily see them being marine derived, which is interesting.

**Boesch:** Right. Well, that is entirely consistent with what these people are describing as the trend. In that we have gone into a system that is now nutrient rich in which there is increased amount of in-situ allochthonous production of organic carbon due to the fact... *autochthonous*, right, due to the fact that there are more nutrients stimulating the production. And it is not inconsistent. Hans Paerl is here. He can tell you about in a much tiny little system of the Neuse River how the flood events there can drive the system over the brink with respect to hypoxia driven by organic inputs from the watershed that are flushed out in these storms. You know, it is entirely reasonable that the same thing happened on a bigger scale for the Mississippi Basin in the past.

**Hollinder:** There is a significant amount of microbial interactions and that is a very common feature to both contemporary hypoxia and pre-anthropogenic low oxygen effects.

**Sen Gupta:** I need to add just one point about the various fossil indices that you were using. For instance, we are looking at preserved record in sediments of some species whose ecology we are not very sure about because not too many live experiments have

been done, and some of them have been designed faultily. Not to do with anyone present here. So the point is, we need to determine by some cleverly-designed field experiments what drives any particular index. You can look at any Foram species—I think Lisa would agree with me—and you will see some fluctuations in the study of the record which can have multiple interpretations. And the most convenient interpretation would be to use the data we have in hand. But the fact is that many of these indices have not been tested against independent parameters that may be present in the sediments. So I throw that point before we go too deep into what fluctuation in any particular index means in the historical record.

**Brian Fry:** I am Brian Fry from LSU [Louisiana State University] and I have a question for Nancy, and maybe the panel. Where are you in thinking about the Afchafalaya versus the Birdsfoot Delta, that Piers brought that up? Is there a difference nutrient flux that actually enters the coastal ocean where you define it, perhaps at 5 meters or something like that? And if there were, does that set up an interesting spatial experiment with one part of hypoxia closer to where it split into another part, though I am sure you have thought about this, but perhaps it is worth exposing the thinking of today.

**Rabalais:** Well, there are differences in proportions and kinds of nutrients. It changes by seasons, and it changes by year. And that is exactly why we have put the transect off the Atchafalaya so we could look at the system response. And those data are not real best enough to say exactly what that is and Gene is going to have a lot of information on the two rivers. There is both a physical difference in the delta regions and the hydrography and the light fields. There are all kinds of differences. It made for a very good proposal to the National Science Foundation (NSF), but we have not gotten the work done yet.

**Fry:** It is just interesting when you see 5 years from now (I am glad you did that. So, congratulations!) it may be very pressing. You know, where, looking to establish a monitoring plan that lets us tell the difference between the different kinds of loadings that are entering the coastal zone, and perhaps the other large scale experiment going on is taking advantage of, in a spatial sense not just this historical sense.

**Rabalais:** I agree.

**Lewitus:** A question? Written question in the back? You want to say it or do you want us to read it for you?

**Howard Marshall:** I will read it.

**Lewitus:** Keep in mind that we have about 2 minutes.

**Marshall:** My card is empty, but I will read the question anyway. This is a comment and sympathy to Dr. Twilley and a couple of others about monitoring in the lower Mississippi River and dike. It is a crime that you do not have more USGS stations in the Mississippi River. Because the station at St. Francisville is above the Mississippi River industrial corridor, it does not pick up the discharges in the state of Louisiana. And it misses,

according to the paper that will be presented tomorrow, 25 percent of the phosphorous that is added to the river within the border of the state of Louisiana. Anyway, the station that should be really discussed in more detail is Belle Chasse, below New Orleans, which was mostly discontinued a number of years ago. That station should be reestablished immediately, and the data should be taken in accordance with the procedures described by the USGS, which is depth-integrated samples, not surface samples. And they should be taken, not monthly, but every week. And if you can get \$50 million given to Louisiana for other purposes, it is likely you could get that much for one little old station below New Orleans. Further, there were a couple of words that have not been mentioned here, especially the federal Clean Water Act. And if you are ever going to have any real success in this endeavor, you are going to have to get down to a couple of sections in the federal Clean Water Act: section 302 and section 402. You need to be very familiar with those because you are going to have to use them in any kind of implementing, any kind of successful program that you have. So I would recommend that at future symposiums like this, you have at least one person on the panel giving a presentation who has some experience in implementing the enforcement provisions of the federal Clean Water Act, and in development of standards on the federal Clean Water Act. Because I believe that there are no nutrients standards for the lower Mississippi River, and there are no nutrient standards for the Atchafalaya River. There are no nutrient standards for the Gulf of Mexico. And I do not believe that there have been any Total Maximum Daily Loads (TMDL)s done on the section 204. That was my comment. Establish the station at Belle Chasse forthwith.

**Battaglin:** I will respond to that. In fact, I believe we just did reestablish the station at Belle Chasse, starting this month. It was discontinued because occasionally it sits upstream of the salt water intrusion line on the river so it makes it difficult for us to make a lot of the other measurements besides nutrients because salt water comes up that far of the river some times of the year. That was originally why it was discontinued, but I believe that it is going to be reestablished, not at a very high frequency—certainly not at weekly sampling right now. We will be doing depth-integrated sampling there, a monthly schedule. Again, I believe that they are starting this month. With regard to some of the other sites, we are also thinking about moving our sampling site on the Atchafalaya. I believe we have established some temporary sites, and we are checking it out. But we are moving to the lower Atchafalaya, so we will no longer be measuring at the beginning of the Atchafalaya, assuming that is what is going out the end. We will be looking at Wax Lake and some other site down there. So we will be looking closer to the outlet of the Atchafalaya. Almost all the studies that have come out have shown that there is really very little nutrient change between St. Francisville and Belle Chasse. Every time, we have done the studies, whether we sampled it in the Lagrangian sense or looked at long-term data analysis, the amount...And we assumed that there are inputs between St. Francisville and Belle Chasse. But they never really show up in terms of data. There are either losses in that system that compensate for the inputs, or the inputs are inconsequential to the amount of nutrients that are already in the river, at that point, so they do not really concentrate very much.

**Lewitus:** Thank you, Bill. I am going to have to wrap this up. Hey, how about a hand for everyone up here.

## Addendum: Remarks Submitted by Tom Cronin

Comments on Rabalais et al. Session 1.

It's hard to criticize such a well-written, comprehensive report that covers almost all bases vis-à-vis Gulf hypoxia and the processes controlling it and has the literature to back it up. But I will anyway, in a positive way, in hopes of solving the problem.

I will stick to a couple of major points and stay brief so these can be handed out in my absence and I'll list a couple of specific points. I'll put in bold my main question to help with the discussion. I enjoyed reading this report and wish I could be at the workshop.

Shelf hypoxia: The topic of continental shelf hypoxia in and of itself is of great scientific interest, not merely as an environmental or economic issue. **So what are the oceanographic, bathymetric, climatic, geological (as well as biogeochemical) controls in oxygen dynamics on continental shelves on passive margins?** That is, where is there "natural" oxygen depletion along inner and middle shelf regions, such as occurs in deeper water oxygen minimum zones of continental slopes, where full anoxia occurs in upwelling regions. Though Diaz' 2001 review of global coastal hypoxic systems is useful, and Rowe's summary of intrinsic (not river-borne nutrients) informative, I still don't see in depth analysis of the physical boundary conditions and forcings in the Gulf or along other coastal zones. The Black Sea is given as an situation similar to the GofM, and perhaps it is the best one, but gee, it's an awfully different type of system than the northern GofM and can we justify a comparison simply because fertilizers were shut off when the Soviet Union collapsed, as though this would also happen if the Mississippi watershed agri-industry stopped!?

Climate change versus variability: The report cites **pervasive evidence that climatological forcing strongly influences the development of hypoxia**. A corollary is that future **management and/or restoration efforts must take both climatic (seasonal and longer term) factors and extreme event-driven (hurricanes, storms) factors into account or the potential exists that unattainable targets will be set** if precipitation or oceanographic factors change or increase in variability. Many climatologists and climate modelers discuss in a literature too large and dispersed for the authors to consider here that not only will there be changes in mean climatic conditions in the future (see below), but in fact that they are already occurring. Though the report says there are no studies linking Mississippi discharge to "secular climate change". I am not sure (a) if this mean there is no link, or (b) what they mean by secular change? Human-induced, CO-2 related "global warming"? Or natural change? There are several timescales pertinent here and different causes of climate should be explicit. **One ramification of human influence on climate, taking the example of ENSO patterns of the past 30 years, is that the tendency of increasing hypoxia since the 60s may itself be in part an artifact of climatic patterns in the watershed and Gulf altered by human activity**. Unpopular as this concept might be in Washington (I understand the uncertainties and complexities), it nonetheless must be considered when viewing any of the 20<sup>th</sup> century time-series data presented in the report.

Moving forward: Here's a subtle point. As well-written, balanced and objective as this report is, the prose still gives the impression that there is a need to demonstrate (to somebody?) the basic premise that nutrient loading is fueling GofM hypoxia. One example is on page 20 where the Justic study is cited as showing "only" 20-25% of increased nitrate might be due to greater discharge from the 60s to the 90's. That is still a large percentage, and potentially even greater in the future, given difficulties reducing nutrient loads through management and climate scenarios. Another place is interpretation of paleo-DO proxies (like Chen's) where downcore diagenesis, spatially limited core locations, uncertainty in chronologies, etc tell us we still have much to do about understanding natural DO variability prior to the 60's.

**Sure there are uncertainties, but perhaps the workshop can move beyond any need to defend the solid evidence for cultural eutrophication and ask the question how issues of precipitation-discharge variability have been or will be factored into planning targets.** The paragraph on page 20 on future climate/precipitation changes, discussed in Justic's papers, seem to be made almost as an aside, **when it could be the most important aspect of the entire hypoxia issue.** In fact, Justic et al is a great paper, but in it, the authors refer to the 1988 and 1993 ENSO-forced extremes in discharge (and hypoxia) as "anomalous". This is deceptive. ENSO has been around for at least the last half of the Holocene and it is not anomalous – it is natural internal climate variability having complex relationships with both higher (Madden-Julian Oscillation) and lower frequency (PDO) processes and, as the authors know, well-known teleconnections to the Gulf. Late 20<sup>th</sup> century behavior may itself be anomalous due to human influence.

Regardless, in my opinion, as long as the decision-making community treats such variability as anomalous or of secondary importance, it is doubtful targets can be set and reached. Regional scale climate and oceanographic modeling, coupled with better understanding of impacts of large climate events of the past are quite important avenues for research.

Minor points:

Page 8-9 climatic factors. While floods of 1993, hurricanes etc can be viewed as "climatic" they are really just extreme weather events. Seasonal, annual and multi-year droughts are more related to climatic forcing, ie. Changes in mean conditions.

Page 19. It states the river discharge data are available back to 1820 but Goolsby cites data back to 1890s, and doesn't use the older ACOE data that Turner/Rabalais use. Regardless one wonders if the quality of the data are comparable in 19<sup>th</sup> vs 20<sup>th</sup> centuries. Early- mid 19<sup>th</sup> century was part of the Little Ice Age, and the 20<sup>th</sup> century is not, and lots of climatic implications go with this.

Oceanographic variability related to surface and shallow circulation processes, wind-forcing, etc is not really addressed.

Turner et al. 2005 hindcasting. Is this valid?

**Transcript of Panelists' Discussion and Q & A and Audience Q & A**

**Session 2: Causes of Hypoxia I: Characterization of Nutrient and Organic Matter Loads**

**Authors:** Eugene Turner, Richard Alexander, Tom Bianchi, Robert Howarth, Greg McIsaac  
**Panelists:** Miguel Goni, John Lehrter

*Loss of tape for approximately less than 1 minute.*

**Unknown speaker:** ...and then in the surface column there is going to be more light penetration as well, so it would have some effect (*Unknown speaker interjects: "I'd like to get"*) but of course dropped off in the '50s before the hypoxia really started to take off.

**Thomas Bianchi:** I mean another part of enhanced light is more phytoplankton inputs. I mean we have a paper in press now where we have looked at phytoplankton in the river, and it is mostly diatom-dominated. And it is much higher than you would imagine. Sometimes the chlorophyll numbers rival what you see on the coast. We also have surface sediment data just off the mouth of the river, where you see this decoupling between what is in the water column and what you see in surface sediments. I think a lot of that labile material is freshwater phytoplankton coming in. So that is another factor, I think, that affects sort of the overall quality of organic matter that you would be getting from the river.

*(Woman: Off mike, too low to hear.)*

**Bianchi:** That is right. Those stations are right there, so its impact on hypoxia is still a question. Certainly the quality of particulate material is falling out very early, but if you have a lot of phytoplankton in the water column, you have a lot of DOM that could come from that as they go through this osmotic change. So while they are dropping out, there may be a release. And some of the work that we have looked at also suggests that the DOM in the river, not just the POM, looks much more labile and phytoplankton-like based on biomarkers and C<sup>13</sup> and MR. So there's a dissolved component to that phytoplankton pool that is likely to be more mobile and move around as well. But Nancy is right. I agree that the particulate parts are going to fall out pretty quick.

**John Lehrter:** I wanted to ask about the changes in the TN and TP ratios. I am just curious. Most of the analyses have been based on annual averages of the TN and the TP. How do seasonal changes, especially in the springtime ...Is the seasonal TN to TP... Is it generally much higher than the annual?

**Turner:** It is highest in the winter and lowest in the end of the summer at the seasonal, and then it goes back up again. I showed some graphs. I think one of them... I would have to pull it up to show it. Well, it is going along. It would be highest in

December/January/February and then lowest in July. So it is a transition in between. And I showed some figures for the ratios. It is around 20–30 certainly, in April/May/June, which is the key setup time for driving the summer loading.

**Lehrter:** These are based on St. Francisville data? Is that correct?

**Turner:** Yes.

**Lehrter:** I guess some of Tom Bianchi's work has shown there is sediment trapping in the lower river and the suspension as the river flow increases. How does that accumulation and then release of sediment in the lower river come into play in the use of these ratios?

**Turner:** Well, I think that zone is a really interesting place to examine... We do not know much about it. However, it has to get out, one way or another. There is no long-term storage, even for a matter of weeks, except on the bottom, and I do not know how long that storage would be there. So it has to get out. It is just too big a river. I do not know what the transitions are, but there is no great gas effusion.

**Miguel Goni:** Just to follow up on this, you mentioned that the discharge has not changed over the last 200 years, I think, just as the records show. (*Turner interjects: "190 years."*) How about the seasonality of the discharge, with all the dams?

**Turner:** The data, I don't know. I was wondering if someone from USGS knows that. Anybody? Here we go.

**Richard Alexander:** Well, most of the flow there in the Mississippi, as Gene was showing, is coming out from the Ohio. So the Ohio system sort of dominates. I do not think there have been substantial changes as a result of the shifts in the Missouri that are evident in the lower Mississippi portion of the river. But any fluctuations that are occurring with respect to climate change and impact on the Ohio would be more evident down there. But I am not aware of any significant, appreciable changes that have occurred in seasonality distribution.

**Turner:** Does anybody in the audience know the answer to that? Ah, here we go.

Audience member: I have some figures that suggest recent changes in seasonality since the ...early 1980s....(*too low to hear*)

**Turner:** 1880 or 1990?

Audience member: (*too low to hear*)

**Turner:** (*Confirming*) 1980s

Audience member: (*too low to hear*)

**Turner:** For those that could not hear, the answer was there are some seasonal shifts since the 1980s in the distribution of water coming down the Mississippi River. Right? Is that correct? There are data to look into that question.

**Lehrter:** My question sort of follows up on that in that the April/May/June time period has been highlighted as the critical time period, and there has been some discussion about climate and climate variability. While the jury is still out on what that is going to mean in terms of precipitation in the watershed, the evidence is certainly stronger that there is going to be an increase in temperature in the watershed, and that will probably result in earlier planting and a move to earlier farming, as well as changes in farming. Can you comment on what you think that might be—an effect, if any, on the change in timing of the delivery of the nutrients, as well as the increase in surface temperature of the Gulf of Mexico because of the increasing air temperatures?... And how would that change affect the hypoxic situation?

**Turner:** This is speculation, but you are supposed to get more hurricanes in a warmer Gulf of Mexico. Everybody here knows that. It depends how much you are going to advance the crop because there actually is a pretty big temperature differential between here and Iowa and Minnesota. So you would have to advance the planting season and cropping patterns quite a bit, in fact.

**Bianchi?:** The other thing is, of course, the enhanced soil mineralization effects and potential release of DOM. I mean, it is primarily a high-particulate-load river, not really so much high in DOM relative to other systems. But I think that would change as well, which could affect loading of DOM, as I said before, to the coast.

**Lehrter:** Do you think there is a potential for there to be a worse situation because it is getting the hypoxic zone a chance to set up earlier? The load is essentially coming down the river a month earlier, or do you think that might not be a problem?

**Turner:** There are some people that have looked into this, but as I recall, reading this a while ago, the main issue is going to be change in discharge; stratification issues. And that could have a big effect on hypoxia. And the models all in their own way are reasonable conclusions, but they are different. You know, there are three or four or five models that are predicting the ...*(loss of tape, approximately less than one minute)*....several estimations because not all that is labile and not all that makes its way out of the estuary. It seems likely that if you multiply it by 100 you would still have only 10 grams carbon (C) per meter square, and that is pretty relatively insignificant

**Unknown speaker:** But that is on a daily basis?

**Turner:** That is a daily. Right. I mean, 300 grams C is pretty good. It is a pretty high amount of quantifying production.

**Unknown speaker:** That is on an annual basis?

**Turner:** On an annual basis. Right.

*[Question off mike? Too low to hear.]*

**Turner:** No. It is possible, but it is not probable. I do not know... You guys? They cannot defend a number they did not come up with.

**Thomas S. Bianchi:** That is the first time I have seen those numbers so... I would have to think about those numbers a little bit more.

**William A. Battaglin:** The pattern of occurrence of hypoxia seems to be really strongly influenced by extreme climatic events. In particular, floods seem to have perhaps a multiyear effect and certainly cause large, expansive hypoxia zones. But perhaps more dramatic are droughts. The only time we have made it under the 5,000 is during extreme droughts. The system seems to react very quickly to droughts, making me suspect that perhaps there is much more of a threshold-like amount of delivery versus a linear. All models are sort of linearly. Predicting increases in flux will result in increases in size. But the evidence from the data suggests that there is more of a threshold where, when you get above a certain amount, you have widespread hypoxia, and you make it bigger or smaller depending upon the overall amount. But below a certain amount, you seem to get almost none. Is that purely because of the stratification issue, or is that because of delivery of a nutrient?

**Turner:** It is not a loading issue. It is for this panel. I am saying that I think we need to draw on some people from the other session, or another session, as well.

**Battaglin:** I asked this question once.

**Turner:** So you got to answer any question. But I think some of the modelers would be able to help on that, in that some of these predictions are within 60 or 70 percent of the variabilities captured by the linearity. That says that the step functions maybe are not quite that important. And we are probably going to have a pretty low discharge year. So the more years we collect, we can test that out. But in 1988 it was definitely a drought year, and there was no low oxygen. But of course that was a combination of both little loading as well as little stratification. So, we may have another one this year.

**Lehrter:** To follow up on that question, what percentage of the variability is explained just by discharge alone?

**Turner:** I cannot answer that right off the top of my head. But the loading, the discharge varies about, on the average 10 year, 100 percent from one year to the next. And the nitrogen (N) loading has varied more than that. It is the two of them together. So maybe you would have an eightfold, I suppose, multiplied together. One can compensate for the other but not entirely.

**Lehrter:** I guess from the first session, and from some of your thoughts, too, it appears that N has not changed over the past decade or so. Is that true? It has kind of flatlined?

**Turner:** It is within its natural variability. There is not a big shift. It has continued the same patterns in terms of loading. Right.

**Lehrter:** So in that case...

**Turner:** There is a seasonal signal and a \_\_\_\_ signal (*too low to hear*)

**Lehrter:** If that is the case, it would seem that the variability in the river flow would be a strong descriptor of the size of hypoxia. I am just wondering.

**Turner:** The two of them have not changed. Yes, right. The two of them together have not... This year we will ..., but right. They have not changed much together.

**Rob Magnien:** Just a comment here. These are obviously very good questions. Hold on to them because I think some of the other sessions and authors may be in a better position to answer some of these, when we get to physical processes and the modeling,

**Bianchi:** It is difficult to separate the load because the freshwater discharge and the concentration are the load. So separating the fact that the concentrations have gone up over the past 50 years at the same time the discharge has gone up. It is important to tease those two apart.

**Turner:** Right.

**Battaglin:** Toward point number four here. And again, I would have asked this of Nancy if I had time this morning. She mentioned in her talk and since you did not mention it in your talk, I assume that you also agree with that. You are saying that the ground water discharge is not an important source of nutrients to the system in the Gulf of Mexico. By that I assume you were talking about direct ground water discharge to the Gulf of Mexico and not ground water in the upper basin. Do you guys have information on that? And I want you to not think about the upper basin. The upper basin ground water system is the reservoir of N that this whole system is battling against. Unfortunately, it is a reservoir of N that has a very long retention time, and it could be an important issue.

**Turner:** Well, I am not a ground water scientist, but a couple of people made these radon measurements on either side and cannot find evidence. Not that it does not exist, but they cannot any find evidence of ground water seeping, say, in the upper 20 meters. It is not coming out, so you would expect this diagnostic. That is not true? No? You are saying there are? I thought you were shaking your head no.

**Amy Parker:** You are right.

**Turner:** Okay. Eye opener. So there does not seem to be evidence that you might find other places for ground water coming out on this part of the coast. Florida, yes, but not here. But I stand corrected by my esteemed colleague...

**Alexander:** I think it is clearly a riverine-dominated system. I mean the Mississippi is huge. It plays a prominent role here in terms of the delivery of water flux, as well as nutrients. Clearly the source contribution that has been documented fairly well with respect to N in the water flux leaving the Mississippi River argues that a lot of that material is coming from a distant source, not a nearby source. So a combination of those factors would argue that the river itself, the surface water, plays a prominent role here.

**Goni:** I guess we are ready to get some questions from the audience.

**Turner:** I guess I would make one summary additional point. That is that there is a variability in the natural system, and it is driven by land use patterns. Those are much harder to figure out the drivers of, to change management. So the hypoxia issue, in terms of loading alone, is decades of management, or interest, or however you want to term it. If there is going to be some intended action to get an intended response, this involves land use, which is really not describing it very well. But it is describing the whole socioeconomic system. And if there was one clear thing I got out of this morning, it was that perennial crops do the job. But why are people using perennial crops? Because they need the economic incentive or they need the alternatives shown to them or they need other trade routes to make sure that the crops have a whole marketing system. If it is land use, land use, and land use, we need to find ways to do it, and that is one clear technical means to do it. Whether it is going to work or not is not science, it seems to me.

**Magnien:** Are there any more questions from the panel? Any more comments that the authors would like to make? Okay, why don't we go to any questions that the steering committee might have now.

**Len Bahr:** Gene, it is no mystery to me ... You and I have known one another for a long time and have long disagreed about river diversions, not as a means to solve hypoxia but for a whole suite of reasons. It seems to me that diverting the river on a massive scale is the appropriate thing to do, and I do not think that to date there has been a really good look at the potential for N reduction as a combination of burial, uptake, and denitrification. We could argue about that forever. But one of the things you specifically talked about was the Atchafalaya River and its potential. There are a number of proposals, at least a couple active—Andy Nyman and some other people are looking seriously at the potential of the Atchafalaya Basin to remove a fair amount of N. Based on what you said, I do not disagree with your data, but I think it needs to be pointed out that the vast majority of the river is channelized. The water never gets into the swamp. So it is not really fair to talk about Simsport versus Morgan City N change at this point, I do not think, on a normal flow year, because the water does not get into the areas where it could be nitrified or taken up by coastal forests ... or anything else or ... (*thought trails off*)

**Turner:** Well, without going into the why you would do those things, my point is that these are decades-long planning processes, and right now they are not contributing to the reduction goal for 2015. I mean, it is a small amount and it is not going to change. Whatever programs anyone wants to come up with on that scale, these are decades-long issues. We have a goal here that is dated 2015. Right? Or some date that is a decade or less. So, we need to go ahead with...things. You can keep those in mind and discuss the merits of them, but in terms of reaching a reduction in the size of the hypoxic zone, we have to deal with the existing systems we have, whether the others are done or not.

**Lehrter:** I would agree. I would answer, though, that the Davis Pond and Canarvon are not good models for how long it takes to get a diverted project authorized and built, in my judgment, because Katrina and Rita really turned things around. At least they should have, and I think they did. In terms of the urgency of saving what we can of our coast, there is a new proposal out that would divert about a third of the Atchafalaya River water into the Terrebonne marshes, for example, to the east around Morgan City. It is a very exciting proposal that is getting some attention now. And that project could be fast-tracked and come online...within 10 years, I would say. I may be optimistic. But I agree that the 15-year horizon target for hypoxia reduction is the real issue.

**Greene(?):** Any other steering committee questions?

**Dugan Sabins:** I guess I will just add the topic of nutrient loading, and removal will get prime billing at the next symposium in June. So I want everybody to come out to that one. We will definitely continue this approach. Some of the ideas that Len talked about, some of the ideas that are ongoing with the coastal restoration program, and some water quality programs that we are involved in will be discussed at that symposium. It is appropriate that the rest of the title is *Data, Trends, and Opportunities*. And I think we will. What Gene says is true, our goal is 2015, and insofar as we can use some of these as tools to eventually affect hypoxia, whether we reach it in time for 2015 or not, they are important concepts that we want to pursue. That is what we will be talking about at the June symposium while, of course, we work with our colleagues. I have been talking with Dennis [McKenna] and Dean Lemke and James Baker from upriver. We all work together to address problems on the entire Mississippi River. Also, I think we need to add that the U.S. Geological Survey (USGS) is helping us take the lead on our last of the reassessment symposiums, which will hopefully deal more directly with the monitoring gaps that we have for status and trends on the river. This is tentatively planned for later on in the summer. So we have opportunities to try to take the unanswered questions from these workshops and symposium and move them forward to these last two.

**Greene:** I just wanted to follow up on one point. When you were discussing the TN-to-TP ratios in the river and the recent trends, I assume that that was St. Francisville data with the recent trends over the last, say, decade, moving toward more N limitations in that part of the river. It is curious, and I am wondering whether you or anybody else has gone beyond St. Francisville to look at where that transition from an N-limited river to a P-limited river may occur in the basin.

**Alexander:** I do not think so. I mean, we have data at Belle Chasse historically. Obviously, that site was shut down in the mid-1990s, but as Bill Battaglin pointed out, it is about to start up again. So we would have new information there. But in general, the historical record does not show any appreciable differences between St. Francisville and Belle Chasse. It is certainly within the margin of error in terms of the load estimates. Certainly for that reach of the river, there is not much difference. But below, you are dealing with a tidally influenced area, and I am not sure what the extent of monitoring has been below Belle Chasse.

**Turner:** Maybe that is not your question, but we studied it at Venice. There is some data from Venice, and they did not show that much difference either. There is a question of whether that is contaminated by saltwater mixing. But it did not show much of a difference either.

**Greene:** If you go up the basin, up the river...

**Turner:** Where is the loading coming from?

**Greene:** My question is, I guess, can you account for the recent downward trend in the TN-to-TP ratios in the river? I do not recall whether you gave an explanation for that downward trend, but do you have any guesses or further analyses? It seems to me that that is an important trend that we have not accounted for and that I certainly was not aware of.

**Alexander:** It comes back to the question, I guess, of separating flow from anthropogenic sources in the basin. But pretty clearly, at least over the period of the early 1990s through 2000s, 2003, 2004, there is evidence for close agreement about what has happened in flow and N during that period. So there is some flow-controlling aspect to what is going on with the N in terms of what Gene was showing over that time period. But it would be of interest, I think, to look more closely at the fertilizer record or what has happened with respect to other anthropogenic sources within the watershed over that time period—to look more closely to see what other, potential, more direct, sources are and what impact they might have.

**Turner:** I cannot answer this quantitatively, but if you look over 20 or 30 years of record where corn or soy beans are being planted; if you look at the whole country, they were more on the periphery and they have been concentrated into the center of the upper Midwest. That is where the highest N loading is. So it is not that the cropping system has not stayed the same in all these basins. And if there is one place to look where you might have higher N than phosphorus (P) loading, it would be where you have corn and soy beans that have been concentrated in those upper states. So that is probably where the major change has happened. Because we used to have five or eight crops and now you have one or two. That is all there is in the county—just soy beans and corn—and they are very intensive users of fertilizer. And it is not being grown somewhere else.

**Greene:** I will go ahead and ask the one question I have so far from the audience, which is related to what we were just talking about. The question is, “What is the annual change in nitrate-to-phosphate ratio during the year in the lower river, and does this have any impact on what we consider to be the limiting nutrient in the area of high productivity in the gulf?” This may be going beyond a little into the next session, but...

**Turner:** You can plot that out, but there is a huge range of those ratios. I might have one here. Throughout the year the ratios change a lot. In April it has been getting closer and closer to being more N. You know, 16-20...

**Greene:** In the?

**Turner:** In the spring.

**Greene:** In the river or the gulf?

**Turner:** In the river. I am trying to stay in shore. Is that the question?

**Greene:** What I am getting at is, can we relate, say, the monthly changes or annual changes in nitrate-to-phosphate ratios in the river, to what we see in surface waters in the gulf? I think that is the real question.

**Turner:** That would be a dicey thing because of the limited data set offshore. However, when we did try to predict the size of the hypoxic zone on the basis of these loadings, the ratios were not important. So that is the N number. You are talking about something in-between and the water in the size of the hypoxic zone, which is why I think that question is being answered, perhaps mistakenly so. So the ratios were not that important for the data set we had, which is the past 25 years.

**Greene:** The whole N-to-P issue, I think, is an important one. We need to do a better job in the reassessment in the revisions to the Action Plan. And I am wondering... Nancy [Rabalais] showed in her presentation this morning, pretty continuous records of, and long-term records for, surface water nitrate. I do not know, Nancy, if you showed the same for phosphate, but I know there is quite an extensive data set for surface water phosphate at some of the stations. If you looked—and maybe we can do this post-meeting—at the N-to-P ratios in the river and tried to correlate that with N-to-P ratios for surface water in the gulf, can we make some inferences about critical periods where either N or P may be the key nutrient regulating productivity in the gulf?

**Turner:** I have tried to do that. Maybe it was not adequate. I did try to do that, and I could not get too far because it is confounded by ... You dilute what is in the river with ocean water, so you are dealing with different salinities and then you have to figure out how to compensate for that. And then there is uptake, which may change because your sampling data point was in a storm or wave conditions. It is a mishmash. It is very hard to compensate for these other factors, and I could not get anywhere. That does not mean that it does not exist. I just could not get anywhere with it. The best we did, was I got some

data off Grand Isle once with, I think, TP or TN of the river. I had a slight hint that they were related, once you normalize it for salinity making some outrageous assumptions. But it just did not go too far. It was not rigorous enough to do anything more with.

**Lewitus:** Rick stole some of my thunder, but he did a better job than I was going to do on it. So, I just want to get straight on something on discharge. You showed over the very long term that there is no variability. But then I think you made the point that if you break it into shorter terms, you will see...*(thought trails off)* So anyway, if you break it from the 1950s on, is there an increase in discharge? And does that follow the hypoxic zone area increase as well?

**Alexander:** Well, the Goolsby report was pretty specific in terms of stating that over the 1960s through the '80s, there was about a 30 percent increase in flow that occurred. So, yes, there have been increases in flow, certainly over that time period. At the same time, there have been other analyses that Nancy and Gene pointed to earlier today. Some subsequent analysis has been done (by Simon Donner, others, and Justic) that has pointed out that much of the change that occurred in nitrate at least over that time period has been due to anthropogenic sources and not due to flow. About 20–25 percent of the changes in the nitrate have been due to flow. There have been coincident changes that have occurred over the long-term period with respect to nitrate as well as flow. Certainly, that can be said. But, there are more, over short-term periods. Certainly during the latter part of the record there have been other changes that have occurred in flow, including some declines during the later part of the 1990s and 2000s. But those have been rather small, and I do not know what you might want to say about how that correlates with the hypoxic zone.

**Turner:** I would like to go back to this part about discharges. One of the graphs did not work on here. The '50s were really pretty low discharge years—the '50s and early '60s. Then recently we have had wetter years. So we have been on part of the cycle that has been going up and down, up and down. And where the records started, as you said, in the '50s was one of the lower points. That would have been obvious from one of these graphs I have with me. So, as I recall, there is another system in which you can always claim success based on weather patterns if you start in the right part of the cycle, right? So we have to keep that in mind.

*Coffee Break*

**Magnien:** Please, everybody, asking a question, and responding, get close to the mics. It has been a problem with everyone being able to hear.

**Alan Lewitus:** I have been told to say my name because my back is to the audience. Alan Lewitus. And the question is to the coauthors. It appears that there is a peak in Mississippi River N loading in the early 1980s. Do you have any insight into what caused this pattern?

**Alexander:** Yes. It is one that has been talked about quite a bit, and that is the correspondence with fertilizer use during that period. There is a strong correspondence between the record of fertilizer use and N during that period. So, that is at least one often-cited correspondence that is referred to.

**Lewitus:** Okay. This is just in general, I guess. Is there any increase in phosphate from St. Francisville to Belle Chasse? There is a 25 percent increase. Is that important?

**Alexander:** It is not there. Yes, there has been quite a bit of discussion of this over the past couple of years. Much of it involved EPA's draft report that was put out. It has been pretty thoroughly reviewed. Some of the reviewer responses dealt with that question as well. One of the reviewers, in fact, did a fairly nice summary of the monitoring data. They did a comparison between the two locations, and, as far as the P record is concerned, for TP there are very few differences between the two locations. There do appear to be some differences with respect to the ortho-phosphate between the two locations, and that has been reported on. But in general, the comparison that was done in terms of looking at the ortho-phosphate and the TP between the two locations, particularly if you look at an additional source, the USGS records at Belle Chasse, there are very few differences between the two locations. I think one important thing to note is there is the difference in the sampling methods that were used. With the USGS methods being depth- and width-integrated techniques as compared with what I believe are grab samples for the other data that were referred to that were showing differences between the two locations. So when the depth- and width-integrated information is looked at between Belle Chasse and St. Francisville, there are relatively small differences between the two locations. Certainly, it falls within the margin of error, in terms of the estimates of loads, which are typically about 5–10 percent on an annual basis. So there are relatively few differences between the two locations. I know there has been some reference to the contributions that point sources make there in the lower portion of the river. NECT released a report in 2000 that was done for the Louisiana Department of the Environment, I believe, that looked at toxic release inventory information for both the industrial and municipal facilities for the lower basin. It suggested that they might have amounted to about 15 percent of the loadings at Belle Chasse and St. Francisville. So it is a fairly small percentage, and that would be a maximum that they would represent. I am not so sure where the 25 percent or the 28 percent figure is coming from because it does not appear that, at least from that information, the contributions in the lower part of the gulf could account for much of a difference between the two locations.

**Turner:** With respect to that, it is not 1980 but the 1980s hump. Gregory McIsaac's model captured that really well. I was just looking at the graph on this to make sure I was right. And it is the confluence of cropping patterns, fertilizer use, and discharge that reached, together, a higher point than the average on the period. So that would explain that high and other lows because they stick together.

**Rick Greene:** This is Rick Greene and I will read one of the audience questions. This is regarding the Baton Rouge data set on the river. It says that the dissolved organic nitrogen (DON)-to-dissolved organic phosphorus (DOP) appears during high flow to

peak as well and such that organic loads are very significant in the spring. And the question is: “Is this not a significant finding, and how would this affect processes in the gulf... So, are the DON and DOP concentrations elevated in the springtime during high-flow years, during peak flows, such that you are getting high organics delivered to the gulf during this springtime period? It that significant or not?”

**Turner:** This is about organic N and organic P?

**Greene:** DON and DOP.

**Turner:** DON. Well, most of the N is not dissolved organic nitrogen. It is *nitrate*. I mean, better than 90 percent (*Someone whispers: “70 percent”*) (*momentary loss of tape*) ...is you know dissolved. It is not the organic fraction, and it does go up in the spring coincidental with land use practices. And the discharge is sizeable, so the loading is a multiplication of the two. So I do not quite understand the question or how DON is...I do not know that it is higher in the spring. If someone... (*end of comment*).

**Bianchi:** We have seen some relationships with DON and phytoplankton pool in the river. So again it goes back to what I was saying before about that labile pool being interactive with that phytoplankton in the river itself.

**Turner:** The DON is high...

**Bianchi:** When you have high phytoplankton.

**Turner:** Which is in the summer in August and July, not in the spring. The turbidity drops with low discharge, and that is not in the spring.

**Bianchi:** Yes, but again, when you follow the sort of the traditional hydrograph, which looks very smooth, it is not. I mean there are these smaller events that you do see these kinds of responses that fall into a smaller category of dividing it into the traditional seasons. So you do get these bumps on rising and falling hydrographs that you can get these responses to phytoplankton and some of the dissolved N. So what I am saying is that, overall, you may have spring being dominant. But you can have these other relationships that occur during summer, during these other phases, where you can find this relationship between organic N and phytoplankton, even though it may not be the peak period.

**Turner:** Within the river you are saying?

**Bianchi:** Yes.

**Turner:** And are you saying they are using the DON or producing it?

**Bianchi:** I do not know. I am just saying that it appears that there seems to be some sort of relationship with the presence of the phytoplankton. But we have not done any up-to-date measurements. We also looked at amino acids as part of that pool.

**Greene:** Just to follow up on that, is it still uncertain the extent to which organic loads, organic N and P loads, delivered to the gulf in the springtime contribute to hypoxia in the summertime?

**Bianchi:** I can only speculate. I would think not very much compared to the dissolved inorganic nitrogen (DIN) pool, just based on the percent numbers. But there are some very interesting patterns going on on smaller-event scales. That is my only point. But to the larger hypoxia picture, probably not very much.

**Greene:** Another question from the audience. The question is: “Would the combination of nutrient concentrations, N and P, organic and inorganic forms and their stable isotope ratios be a better determinant of nutrient sources most important to sustaining hypoxia than just nutrient concentrations?” Part B to this question is: “What might the disadvantages be to using stable isotopes to determine the sources most important for fueling hypoxia?”

**Turner:** So...where is Brian Fry? If Brian Fry were allowed to talk...*(laughter)* I do not think that is your question. The isotope issues have been looked at to try to separate out sources in the river, and it is apparently a lot more... it is not straightforward.

**Bianchi:** You are talking about P, N, or is it C as well because it is a big problem with the C. You can mix C<sup>4</sup> plants, C<sup>3</sup> plants, phytoplankton and come up with lots of numbers. We have been trying other mechanisms—sulfur and things like that—but your question was focused on N and P, was it not, not C?

**Greene:** Yes, the audience question was really on N, inorganic and organic forms of N and P.

**Turner:** The nuances of this, Miguel Goni and Brian Fry would be real good people to go to for whoever had that question.

**Battaglin:** We have done a little bit of N isotope work in the lower Mississippi, and what we find is that the signals of the nitrate that enter the river, say, in the upper Mississippi and the Ohio, pretty much do not change all the way down. So there is very little evidence from an isotopic standpoint of de-nitrification going on in that stretch between the confluence of the Ohio and the upper Mississippi, and St. Francisville. The isotopic signature itself is pretty intermediate between a fertilizer signal and a soil N signal, which is what you would expect. So, it does not really help to identify a whole lot about the sources. It indicates that the source—the nitrate that is introduced in the upper watershed—pretty much remains the nitrate that leaves the river.

**Brian Fry:** Can I comment? I am Brian Fry. We have been doing [work] at Louisiana State University (LSU). I have been there a few years, and we have been studying the effect of the Mississippi River on many deltaic systems in the Birchwood area and also in some cores offshore that Gene Turner has provided us. Generally, there is an interesting signal that is associated with the river—very useful. It is a high  $N^{15}$  signal, elevated  $N^{15}$  values of biota, sediments, and plants. It is not huge. It is the river nitrate average of about plus 8 per milliliter (mL) on the scale, and we can see in some of the offshore cores...I keep telling Gene we have to write this up, probably. The  $N^{15}$  values increase a little bit. They consist of about 1–2 per mL. It is not a huge signal, but you can see it since about 1950 or 1930 that kind... So, consistent with these other paleo indices.

And it is a very valuable additional tracer in the  $N^{15}$ . I think we are still working on aspects of it for the offshore site. I had a post-doc, Bjorn Bissel<sup>3</sup>, who just recently moved on, and he measured the particular organic N over a couple of these shelf-like cruises. But he has not put that data together. It is like Tom Bianchi was saying: There is a lot of variability, but it averages out into the sediments. So on average, there is higher  $N^{15}$  building up in the sediments, which is a record for the river influence.

**Lewitus:** Question from the audience: “Six percent N removal in the Atchafalaya is significant for the goal of 30 percent reduction. Can wetlands management be ignored? Is it not easier to manage the Atchafalaya than agricultural practices?”

**Turner:** Given the variability in the data, 6 percent is statistically noise. So it is not a strong signal, and there is actually more nitrate coming out. If you accept, you would have to accept that there is more nitrate coming out even though...total nitrate might be down, there is more nitrate is coming out. Which I think also is statistical noise; 4 percent is in the middle range of data. So, if you accept one, you have to accept the other. I am more worried about nitrate than I am about organic N at this point. But regardless, maybe there are things we could do with the Atchafalaya Basin that are acceptable without having to do unacceptable things. Like plumb the whole basin, put flood walls up to hold the water in, or maybe there are other things to do. So, I am not saying “Take it off the table.” I am just trying to scale it. Again, for the purposes of this meeting, if it is the 2015 plan, in decades, it is a pretty large area. And multi-decades worth of planning would go into it before you would try some meager thing to be done in there, I think. They had trouble enough just breaking a couple of levees in it. It has taken 20 years to put some breaks in these levees that people have been complaining about for decades. I do not see how it would take any less time to get the whole basin organized. There are too many landowners, timber crops, and vacation homes and stuff. But do not take it off the table.

**Greene:** This is a question from the audience. “N-to-P loadings and concentrations you presented are annual means and thus do not necessarily reflect the ratios during spring bloom periods, which may be significantly greater than 20 or maybe more like 100. Have these spring N-to-P ratios changed over the years, and can N really be reduced realistically during this period to limit or control productivity?”

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<sup>3</sup> Please verify name

**Turner:** I know my talk went fairly fast... That was on a panel of four; there were annual loads on a frequency plot on one side, and the spring loads were on the other. And it does not show, in fact demonstrate, for this particular length of record, that they have gone down as the annual ones. Maybe the cycles have gone ... but on the average, they have gone down. In the springtime, it has followed that pattern. It is a little lower ratio than it is, just like it has for the annual averages. The springtime ratios have gone down as well. So, yes, there is a lot of variation month to month, but within those months, it has also gone down, as well as the annual average.

And the other question was about managing ratios in some way? That is, what we are trying to do, and they are trying to do this in smaller areas in the Baltic with sewage treatment plants? I know a couple of places. So, we are just trying to scale up to something like 40 percent of the United States. The ratios are helpful to learn about how the system works, but they do not necessarily tell us about how it will work. The ratio is not necessarily...It could be interpreted as well as N limitation as well as approaching toward balanced growth. Right? It is closer to the Redfield ratio. It is erratic, and it is not necessarily always N-limited. It is just maybe a situation in which you are not limiting one or the other. So it may be tweaking one or the other. If you can do that on a scale of a continent, it could also have an effect on future growth. But that is a list of assumptions you would have to follow through offshore. But, just hypothetically, you could say why not make it N-limited or why not make it P-limited, or more than it is now? And one of the answers is you have a lot of funny organisms that like those situations. You get powerful algal blooms; you get a lot of strange organisms that have not had that before. That is something to be discussed.

**Greene:** One final question, just to bring some closure to the nutrient sources issue that I didn't see discussed in the presentation: "Is there any new information that you are aware of on atmospheric N inputs, particularly in the gulf? And if so, what is the relative contribution to the loadings of other sources?"

**Turner:** As far as I can tell, there is no change to the original assessment, which is that it is a minor component. The river is an overwhelming source. There are some monitoring stations out. I did not look these up to see if they have changed much, but if they had doubled, I feel like we would have known about it. If atmospheric sources had doubled, somebody would have picked up on that. No, I am not aware of any changes from the original assessment.

**Alexander:** I think, in general, the previous assessments stand. Probably the highest level that was cited was about 18–20 percent from the SPARROW model. I do not think those have changed significantly, but it will be worth looking a bit more closely at the atmospheric trends.

**Turner:** You are talking over land or over coastal waters?

**Alexander:** No, from land.

**Turner:** From the watershed?

**Magnien:** Have you exhausted the written questions then? If there are a couple of focused, brief questions that still remain in the audience, we can give it a shot. Do you have one?

**Cliff Snyder:** I am with the Potash and Phosphate Institute. I would like to address this question to Dr. Turner, please. In the paper that you published earlier this spring, in the *Marine Pollution Bulletin*, I believe, where you showed that the May discharge is highly correlated with the annual size of the hypoxic zone. I am wondering if that April–May or May–June discharge—if it is so critically important to the high size of the hypoxic zone annually—if there is a way to divert more of the Mississippi River flow through the Atchafalaya Basin during that seasonal period, and reduce nitrate discharge to the gulf. And I bring up a paper that was recently published by Yu Xu<sup>4</sup> (if I pronounced the name incorrectly, I apologize), a scientist with LSU, to show there was a 27 percent reduction in nitrate in the water received at the upper end of the Atchafalaya and that discharged to the gulf. So, he attributed a 26 percent de-nitrification of that load through the Atchafalaya Basin before it was discharged to the gulf, indicating that that wetland might be a significant tool to help minimize the impact of the Mississippi River flow if more of it might be diverted through the Atchafalaya during that critical May period. The question is, have you seen Dr. Xu’s paper, and would you think that the Atchafalaya Basin might be an effective means to reduce more of that nitrate load to the gulf than what we are currently doing?

**Turner:** I have not seen the paper. I do not know if it is a modeling paper or data-rich or whatever. If it is being taken up, you could discuss it. It is used for that when the floods come up. It is used as a spillway, so there are other considerations of adding water through the basin. As I recall the stage discharge curve at Morgan City, for example, is going up every year because the basin itself is filling in, year after year, as the water goes through. And the transects across the basin show the same thing. So people have to take that into account. I am not saying do not do it. It is supposed to be a flood-control relief basin for New Orleans, so some other people might have an interest in not having that happen. So I guess I will try to grasp the point would be that ... if at the right time of year you diverted water through the Atchafalaya, you might reduce the ratios in a way that could shrink the size of the hypoxic zone. Done strategically, you know.

**Don Boesch:** I am going to make sort of an observation and then ask you to comment on it, rather than phrasing it as a question. This has to do with the wetland issue, as well. The various calculations about the material coming from degrading, deteriorating wetlands sort of assume that they deteriorate rapidly from the edges and all material is flushed out, when in fact the wetland deterioration that is going on here is pretty well rotting in place. It is falling apart from within, and things degrade so that a lot of the oxidation of that fixed C takes place in the coastal system.

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<sup>4</sup> Please verify author.

Now if we have big events like Katrina, it is going to be interesting to see if this big flush of organic matter coming out of it causes areas where there are hot spots—the deposition of that material that causes oxygen reduction. It could be, but I would argue that it is probably in the extreme events. With respect to the wetlands and nutrients, N specifically, or both elements' removal, I think that the important thing to keep in mind in the 2000 *Integrated Assessment* was that the recommendation is not that all of these goals be met by reducing agricultural losses, or fertilizer use or anything, but that it would take an integrated approach. We would require getting bits and pieces of the goals from wetland restoration in the basin, wetland restoration in terms of how you make smart decisions, and redistribution of the material through the delta system. So, you know, 5 percent does not sound like much, but if your goal is 30 percent and you can get 5 percent here and 8 percent here, 6 percent here, that is how you make the goal. So I think it is important to remember that integrated assessment recommendation is not just directed toward agricultural sources alone, but toward renewing and rehabilitating the sinks of nutrients within the system.

**Turner:** According to the assessment, let us assume that N is actually not removed and not flushed back out as some other form. To get 4 percent, you would have to get 20 percent of the river, as opposed to less than 1 percent now, spread over 50 percent of all the wetlands. I would argue that, in terms of the 2015 goal of reducing it and the money involved and all the rest, this is not, politely, a scenario to happen. I agree that everything should be put in place ...I do not see that we are going to get 20 percent, *5 percent*, of the river out in the next 20 years. Maybe that is just my myopia, but much less than 20 percent. And if all the calculations were correct and there were no other factors to consider from social, ecologic, economic, hurricanes, and so on. It just seems unrealistic to get 20 percent of the river out when we cannot get 1 percent out now. We are still learning about these diversions, which is not necessary to go into here. But I do not think anyone would claim that we have 100 percent knowledge of the effects. Getting back to the 2015, we are not on track if we go too far in that direction.

**Boesch:** What about my first observation—about the deteriorating wetlands as a source of C or nutrients? Even your liberal calculations are unrealistic because that material is not exported. It is basically mostly oxidized in place.

**Turner:** Sure. I mean we have the same conclusion on that.

**Bianchi:** Are you saying that the dissolved oxygen carbon (DOC) is oxidized from leaching and degrading marsh material?

**Boesch:** Yes, I would think that most of the labile DOC that results from the deteriorating wetland is in fact metabolized within that system. It is not exported, unmodified from a plant, out to the Gulf of Mexico. There are lots of opportunities for bacterial/metabolic breakdown of that material in the estuary and in the wetlands itself. Any assumption based on assuming that total C inventory of an acre, or a unit, or square meter of wetlands and exporting that to the gulf neglects the fact that the breakdown of organic matter largely takes place in the real world in situ.

**Goni:** Can I make a comment about your unusual events? I was part of a cruise that cored almost 70 places around this area after Katrina. One of the objectives was to look for wetland C being deposited in post Katrina, and in none of the cores did we see any evidence for that.

**Loutis:** Okay, let us take one final question.

**Reide Corbett:** I have one informational plug and then one question. Regarding ground water flux onto the shelf, we just completed a 2-year study using two different approaches—a geochemical approach and a modeling approach—that did show just that, that there was no terrestrial ground water flux on to the shelf itself. If anything, you have very minor contributions from deep convection cells. Again, no significant source of ground water. As far as the question I had, Gene, you gave a figure for C from the river to the hypoxic zone. It was 300 grams of C per square meter per day. And the question I had is, first, is that considered a large flux? In comparison to your wetland, I guess it is. But just in general, is that considered a large flux? How much of that is labile? What portion would be DOC, and of that how much is labile? So how does this play into the whole hypoxic picture?

**Turner:** You know, when I did that calculation I knew...*(thought trails off into laughter)* Don wrote a really nice review of this, and I would really prefer that he answer it, but... It is a large number without consideration of whether there are losses before it is consumed, that it is used...

**Don Boesch:** ...and panel four is going to address that subject.

**Turner:** And panel four is going to address that subject. *(Laughter)*

**Unknown speaker 2:** You will answer all that.

**Unknown speaker 3:** That is a good answer.

**Turner:** So, stay tuned.

**Bianchi:** That number does seem high to me. In fact, we were talking about that during the break. So, I would like to see the calculation.

**Goni:** It seems high, but if you see how much sediment deposits annually over the shelf and you multiply by the organic C content of the sediment (I just did the calculation here), it is not that high.

**Bianchi:** Relative to that region?

**Goni:** Right.

**Bianchi:** But is it all going to the hypoxic region?

**Goni:** Well, but even within the sediment... the shelf area of the Atchafalaya, depending upon the accumulation rates that you take as average, they accumulate a half centimeter per year, you multiply over the C content of that sediment, you actually come up with rates... They are not 300, but they are not that different from that measurement, that number that you just calculated.

**Bianchi:** And another interesting part of the story there is that the burial rates really do not jive with the input rates of the C. So it is going somewhere else. It is either more efficiently being re-mineralized or being transported...

**Magnien :** That is the final word? Okay, I have a couple of important announcements. But first I would like to thank Gene, his coauthors, and the panelists for a very excellent session.

*(Applause)*

**Transcript of Panelists' Discussion and Q & A and Audience Q & A**

**Session 3: Causes of Hypoxia II: Influence of Physical Oceanographic Processes on the Distribution and Extent of the Hypoxic Zone**

**Authors:** Steve DiMarco, Rob Hetland, Stephan Howden, Steve Murray, Nan Walker, William Wiseman

**Panelists:** Bill Boicourt, Steve Lentz

**Rob Hetland:** In terms of these hydrographics cruises and mooring surveys, we typically think of physical systems in terms of means and statistics, but the satellite imagery really brings out the events. What do you think is the relative importance of long-term statistics and means versus the events in setting stratification and controlling the structure of hypoxia?

**Steve DiMarco:** It depends on which process you are talking about. I think there are different processes that will have different effects.

**Rob Hetland:** Stratification first of all and then hypoxia.

**DiMarco:** The different processes are what I am thinking about. If an episodic hurricane comes through, obviously that is going to change things considerably. It will break down the stratification, and it will scrub away the hypoxia. There are, of course, multiple reasons—wind stress, direct wind driving, and also wind waves. If you have something not as strong, such as an eddy, that is an episodic feature, would it scrub away the hypoxia? Probably not. But will it draw off fresh water that can create the stratification? Probably so. So there are different time scales associated with many of these different processes that are event-driven.

**Hetland:** What is your gut feeling? Do you think we have any hope of being able to model hypoxia given that these events can be significant? Hurricanes are hard to predict.

**DiMarco:** If you have a crystal ball and can look 9 months in advance and you know that there is a wind event that could become strong enough to break down all the hypoxia, then you could pretty much have a good idea and a good prediction. But barring that, you do not. The best you can do, which is the way I would do it, is to try to understand the system as well as possible and then use models. Maybe you could use some Monte Carlo models, or maybe you have a model that has all the physics, dynamics, and biology you can throw into it, and then you play the game. We are at the point now that we probably have good wind fields for 20 years. You play all those wind fields over top of it, and you get the statistics of how big the hypoxia is based on those wind fields. And maybe you can have some error bars on it. I have no idea. That might be a start.

**Lentz:** A related question: Are there any climatology trends in the frequency of tropical storms, for example?

**DiMarco:** We looked at this for the Minerals Management Service (MMS) study a few years ago, and it showed there are about 2½ storms per year that make it into the Gulf of Mexico. However, it is less than 2½ that will actually make it onto the shelf. Obviously, in the last couple years we have had some significant changes in the hurricane patterns. Whether that is part of a decadal cycle or whether it is part of climate change we will have to find out.

**Bill Boicourt:** I want to say that I enjoyed that survey of the processes and the climatology. I was actually struck by a comment yesterday by an esteemed colleague. He was sitting up here (he happened to be a marsh ecologist), and he seemed to demonstrate an unwavering and fundamental trust of models. *[Laughter]* I happen to share his unwavering and fundamental trust of models, so I can ask this question. I wonder whether in fact we are ready to look at the cross-shelf structure. You referred to some of the processes, and I think they are fairly excited that they look like cross-shelf-related processes. Are we ready to resolve those processes in the models? Also, are we to the point where we go changing the distributary pattern to ameliorate hypoxia? Are we ready to test those scenarios with modeling?

**DiMarco:** Those are excellent questions. I think we are probably at the point now with the resolution of these models—circulation models—that we can start looking at the transport cross shelf. Rob is funded to do this right now and has a couple of geochemists, biologists, and modelers working with him.

**Boicourt:** Should I ask that question of Rob, too?

**Hetland:** Okay. Pretend I am over there. I think the question comes down to what scales you are looking at. One of the issues with diverting the distributaries is figuring out what the processes are. What are the biological/chemical processes in the wetlands themselves? A shelf model is not going to answer that question for you. So you have to match the model to the question you are asking, in terms of the cross-shelf processes on the shelf. I think that the model is pretty much there. It largely depends on the forcing of the boundary conditions that you want to give it. Although we know the winds pretty well and we know the river discharge pretty well, the big unknown is the offshore forcing. It is difficult to put that in with, say, satellite data. I do not think anybody has really solved that problem for a shelf-scale model. The gulf-wide models that do loop current predictions that drive these offshore currents look pretty good but are statistical because they do not have any data simulation, *or* they use data simulation and they under-predict eddies or they miss them. So I think we can get the wind-driven part right. That is pretty easy. The offshore part, say deeper than 50 meters, is a bigger problem, but it is probably solvable.

**Boicourt:** My understanding from yesterday is that the marshes and the biogeochemistry are irrelevant to the whole hypoxia problem. The question is if we are really ready to just do where the fresh water is going—where the stratification is. Can we do that?

**DiMarco:** Sure.

**Boicourt:** Good. Right answer. I am impressed. I have seen some of Rob's modeling and some other models down here, and I think we really are.

**Nan Walker:** I was impressed by some of the data that I looked at recently on tropical storms and hurricanes that are shown there. One expects that there is a direct effect of winds on de-stratification of the water column, hypoxia, and bottom oxygen concentrations; however, that is not what the data supported. The data, at least a limited amount of the data that are available, showed that there are much more complex processes going on, which involve currents on the shelf. You could not look at the winds and infer the currents there. You really need the data that show you what the currents are. I think that a lot of money has been put into modeling without having good measurements to validate the models. I have seen and worked with a number of modelers, and I think modeling is great. But I really think that we need to put some effort into obtaining more measurements so that models have something to be validated against. Because, as Steve showed in his field data, there is a tremendous amount of current structure on the shelf. There are probably shelf waves there all the time. In fact, I think that you picked them up in most of your cruise data. We see them in the imagery. I am actually quite surprised that we are able to view them. We have viewed them in sea surface temperature imagery, which is just a skin temperature, and a lot of the processes are actually going on closer to the bottom. So there are just a whole lot of processes that we really do not know enough about. I think we do need to make an investment in more measurement. We only have one new mooring going in, I think. Nancy has one long-term mooring. I really do not think it is enough.

**DiMarco:** That is a great point because far be it from me to support you modelers. However, you do not need less data to support or validate a good model. You need more data. It is very easy to invalidate a bad model. You need only a few points to do that. However, you need a lot of points to validate a good model. You need more data.

**Hetland:** Can I be on the other side of the table for one second? The point about model validation is that you have to make sure you validate a model with respect to a particular question you are asking. Obviously, we are not using climate models to study hypoxia, although those climate models are validated. It is just not the right tool. To validate a model you need to do the same things you would do if you had no model. You need to measure the system, study it, and understand it. The model lets you extrapolate that understanding; however, it does not give you the understanding.

**DiMarco:** The model is only as good as the question you are asking. If you ask the wrong question of the right model, you are not going to get a satisfactory answer.

**Lentz:** I guess I am not as confident as my colleagues are about models. In particular, I think we are talking about buoyant plumes, which are an especially difficult thing to model because they are thin and depend critically on vertical mixing—something we do

not do really well. Plume dynamics is where I more or less agree with the need for a lot of observations to validate the model.

**Boicourt:** That is a good point. Now that we have all agreed that models are failures, *[Laughter]* are we ready to try to use the models to do some of the things that are fairly important? Gene Turner talked about some of these important time scales. Robert Twilley asked for some kind of indices here and response times. Are we ready to tease out some processes like lags, response scales, and memory from the models? What condition are the water quality or the biogeochemical models built on top of these hydrodynamic models?

**DiMarco:** Right now, in terms of our study, we just have that simple model mind. We are working on putting the nutrients/phytoplankton/zooplankton (NPZ) type and even a sediment module inside that. Already we can start teasing out some of these features. For example, say we can have the river run like it was running a hundred years ago with a lot less river discharge out of the Atchafalaya. Then run the 1993 winds over top of it and see if you get the same structure, the same hypoxic area. Maybe the river jumped its banks in some alternate universe, and it is all coming out of the Atchafalaya. We talked about 20 percent more coming down through the Atchafalaya on some floods as a floodplain. You can do that, too. Throw it in and make it all come out the Atchafalaya and see how that affects the system. Maybe you get that big brown curl all the way to Brownsville that we saw from one of those slides yesterday. So I think we are *[doing]* all kinds of scenario testing. But you have to be careful with that because you will get any answer you want with those models.

**Rick Greene:** I am going to jump in here. Being neither a modeler nor a physical oceanographer (and I suspect the same for a lot of the audience), I wonder if you guys could summarize for us what you believe are the most significant findings over the past 5 years in our understanding of the physics of the Northern Gulf. Put it in terms that I can understand. *[Laughter]*

**DiMarco:** That is a heavy question.

**Greene:** Or your own terms.

**DiMarco:** In terms of the physics in the Northern Gulf and significant findings, the Louisiana–Texas Shelf Physical Oceanography (LATEX) Program was essentially designed to look at the circulation patterns, and I think we have nailed that down pretty well. The low-frequency circulation is pretty well known. Finding these shelf waves, however, was a complete surprise in 2004 when we were out there. We were just doing stations and trying to stick to our plan, which usually goes out the window as soon as I hit the sea buoy. We try to stick to it and we try to make these measurements. Then when we looked at the data, that wave just popped up. I immediately called Bill Wiseman, one of the people who have supported that system for 30 years. I said, “Have you ever done a station line along the 20-meter isobath?” And the answer was “No.” I said, “I think I have seen a shelf wave there.” And he said something like “I have never seen a shelf

wave there.” The LATEX program was out at the 50-meter isobath, which is way far to see these types of structures. So seeing that and seeing how it was actively affecting the bottom oxygen layer was, I think, a very substantial find. The other thing that I think is significant is the work by Rabalais. She talks about the fingering of the density surface coming out. But sitting there on station for 24 hours, and seeing how stable it was was new. Watching it along that vector surface was important. So I think a lot of the physics of this system is now known. There are some interesting things like the wave, but really the new findings are how it is related to biogeochemistry.

**Walker:** I have just recently become involved in the hypoxia problem, so I do not want to comment of the physics of the shelf too much. It is a very complex shelf with the stratification. It was mentioned that both the salinity and temperature affect the stratification from spring to summer. All the different physical forces are encouraging the creation of a hypoxic water mass, I think. You could look at that. We have all the fresh water coming in causing increased stratification, and the temperatures are rising. Last summer, before Katrina hit the coast, the temperature along the shelf was 33 degrees Celsius. That has to be encouraging in a hypoxic system. Also, the winds decrease over time, except for these sporadic events, and the hurricanes disrupt the hypoxia for only a couple of days. So I think the more measurements we make and the more attention we pay to anything, the more complex the situation becomes. I think the satellite data have increased our knowledge of these fluxes on and off shelf. I think there must be some way to quantify those, and by those we can move in the direction of remote sensing people working with modelers, for example.

**Hetland:** I am going to be on this side of the table again. I have a question. Most of the LATEX ... there is a LATEX study showing surface currents and mostly focusing on the along-shore currents. There are two other important types of circulation, particularly for hypoxia. What are the bottom currents doing and what about the upwelling circulation—the cross-shore circulation down deep?

**DiMarco:** Generally, the currents decrease with depth across the shelf, and you do get some non-zero means from the record lengths. Whether they are statistically different from zero (it is probably not statistically different from zero), they have high variability, on the order of 5 or 10 centimeters per second standard deviations, similar to what the surface has. A lot of the Acoustic Doppler Current Profiler (ADCP) data showed that the LATEX had only two or three current meters associated with them. One was right at the surface; one was right at the bottom. And if it was deep enough, we put one in the middle. That usually shows a two-layer system and that there was some communication between the top and the bottom but not always. So it was the baroclinic type. What was your second question?

**Hetland:** How about the upline circulation? Did you do any estimates of net upwelling?

**DiMarco:** No. There are three components to LATEX—LATEX A, B, and C. We did “A” with the current meter and the hydrography shelf-wide. LATEX “B” was LSU’s component looking at the plume. They did look at upwelling, downwelling, and favorable

winds and also looked at the structure of the plume. A lot of that is still locked up in report.

**Hetland:** But the plume is surface water. What about that bringing deep water up? No one looked at that, I guess.

**DiMarco:** No one has looked at that.

**Hetland:** Could you look at that with the LATEX data set?

**DiMarco:** You could. Absolutely. Because we had 200-meter isobaths, current meters, plus the hydrography, which went all the way to the 500- to 1,000-meter isobaths. You can definitely get the offshore circulation if there is upwelling. Typically, you do not see upwelling far up on the shore. I have never seen any indication, where we have been for our hypoxia program, of nutrient-rich water coming up and producing on the shelf. I have not seen that. I think it is because where we are the shelf is too far away from the shelf break. It is a wide shelf. But you may see that. In the Northeast Gulf you see that. You do see upwelling.

**Boicourt:** Steve, you reviewed how much water is going west versus east out here. I am wondering (in my Corps of Engineers hat) about ameliorating the problems of hypoxia. I can envision a situation where I treat the distributary as a set of historical data, and I love to build big dams and gates. I am thinking of running the distributary in a real-time adaptive system. Can I send all this water in the right time off the Mississippi and solve Louisiana's problems?

**DiMarco:** I have thought about that a little bit. The Southwest Pass, where most of the river water comes out, now is pretty dry. I do not think anybody is here that can comment on it. But I will know if anything comes out of the South Pass because it spits right over the shelf and goes real deep real quick. The Southwest Pass comes out in the middle of the shelf. So that water becomes more available to the shelf. If you spit everything out, South Pass becomes more available to offshore circulation and maybe does not get entrained into the shelf's circulation. It does not provide the stratification or the nutrients to fuel the hypoxia. However, what is that going to do to harmful algal blooms off the coast of Florida?

**Boicourt:** That is far off field. We do not care about Florida. *[Laughter]*

**Lentz:** Regarding the shelf waves you saw, have you looked at the relationship to bathymetry? The fact that they were stationary makes me wonder.

**DiMarco:** We have run some of the theoretical codes for inputting the density structure. The big load code is being run by one of Rob's and my graduate students. We do get dispersion graphs that are consistent with what the observations are—wavelengths and maybe periods, heavy periods. I think we may get periods. Rob is shaking his head, "No." To get an arrested wave, you have to bend those dispersion solutions. If there is a mean

circulation or a mean current that those waves are superimposed on, you might be able to bend them and get an arrested solution. We are thinking that maybe we actually might have had a mean current at that point. We were bucking into a strong, fairly persistent down-coast wind when we were heading out for our cruise. It was dropping and smashing into that current the whole way, and then the wind shifted once we arrived on station. Maybe the residual of that current is enough to make that wave stand. I do not know. We do not have the time series. That is the real problem—why we lost that mooring. We lost a lot of moorings. It is a bad environment. Shrimpers!

**Alan Lewitus:** I think we will take a break. It is about 5 minutes early, but then we will reconvene for audience questions at 10:25.

*[Break]*

**Lewitus:** We talked about this at the break, and I think this might be worth clarifying. It is the implication of this standing wave in terms of hypoxia. You know that it is going to promote or suppress or both. Can you elaborate on that please?

**DiMarco:** Sure. That is a good question because we are still trying to tease out all the physics associated with that wave. It would have been a lot easier if we had had the time series measurements from a mooring that was right in the middle of that, but unfortunately we lost that mooring. Based on the ADCP data that we took during that cruise, we were getting onshore and offshore flows associated with the crests and the troughs of those. What the actual net effect on the oxygen was is unclear right now. My feeling is probably a net drawdown—I mean a net influx of oxygen—that you are actually putting into that system, although it is at a rate greater than what you are containing in the bottom oxygen low values. But we are still trying to tease that out. If it does not matter, it does not matter if there is not net oxygen flux. Just looking at the data, my feeling is that the data support that it is probably a net influx of oxygen into the benthos.

**Lewitus:** You talked about the three hypothetical mechanisms for hypoxia formation. I know they are hypotheses, but can you speculate as to where each one of those might be important and your rationale behind those?

**DiMarco:** First of all, the blue region is probably the far greatest in terms of areal coverage. It is where the plume spreads out the most—where the nutrients associated with the plume are diminished. It is where the spreading is—where the production has already occurred. There is low nutrient concentration, so that is area “C.” It covers most of the shelf. West of Terrebonne, off the shoal, and off the Atchafalaya. Zone “B,” which is the green zone, is where the photosynthesis is occurring. It is where the particulate material is already flocculated out. Our feeling is that it is typically in the Louisiana Bight type region right off where the Mississippi River Delta is. That is where the nose of the delta comes out. Where it is off the Atchafalaya I am not really sure. We cannot get the boat close enough to find it. We may have been on the fringe. I used the gyre. We were only getting into about 10 meters of water. We tried to get into 8 meters a couple of times, but

it really makes the captain nervous. Zone “A” is, essentially, right where you saw the brown water in those satellite images. It is where the brown plume is very identifiable. It moves with the winds and that is close to the river.

**Lewitus:** You showed us that bi-layer of hypoxia and also the point about hypoxia forming east of the plume. What implications do those have for perhaps our underestimating the extent of or the magnitude of hypoxia?

**DiMarco:** In terms of west of the plume, one of the things that concerns me, in doing all this, is how do you get the carbon to the bottom if there are no nutrients at the surface? I think that is one mechanism to do that. You are getting the nutrients from the bottom that are being re-mineralized from the benthic decay inshore, and that was being advected and producing along those density surfaces. Ultimately, that production is going to sink to the bottom.

How often that occurs, I am not sure. We saw it once. We saw indications of it. However, nothing that strong in any of the cruises that we have had. But in the historical data (Nancy’s data) you see indications of that. You do not want to get too distracted east of the delta, but I think it is certainly part of the system. It should at least be considered, and I think it would be an oversight by this whole symposium if you did not at least mention there is hypoxia there. When we did the Northeastern Gulf of Mexico Physical Oceanography (NEGOM) Program cruises (nine cruises from 1997 to 2000), we saw very little low dissolved oxygen on the Mississippi–Alabama shelf. But in subsequent years it was more rainy. Those were during drought years. But during the wet years we are seeing more stratification on that. The physical processes and a lot of the biological processes are very similar. Under the right conditions you could probably get hypoxia over on the east side, too.

**Lewitus:** Thank you. That is it for my questions right now.

**Amy Parker:** That was excellent, Steve. Given all of these factors that can cause the formation of the blooms, what recommendations would you be prepared to give *now* to reduce the size of the hypoxic zone by 2015?

**DiMarco:** That is a real question?

**Parker:** That is a real question.

**DiMarco:** I think the physical processes that are occurring there are destined to occur there in 2015. So I think it is hard to tell. We are trying to tease out what the effects of the physics are on the whole system. I am not prepared to say that reducing nutrient loading is a bad idea. I think that having all that nitrate come down the river is probably a bad idea. What I did not show, and what was intimated in looking at the model, was that it gets very good reproduction of the hypoxic area west of Terrebonne. It does a really terrible job in the Louisiana Bight, where the physical processes are not nearly as important, in my feeling. It is the biological processes that are driving that system. So if

you do not do anything, you are going to continue to have what you have. If you reduce, you might have the chance of reducing the hypoxic area. You may reduce it in the Louisiana Bight, while you might do nothing to it off the Atchafalaya, though. It is hard to tell.

**Greene:** Let me just follow up on that. The Zone “C” that you are describing, which is largely controlled more by the physics than by the biology or chemistry, would appear to be less a target for nutrient control of hypoxia. Can you give us some relative magnitude of that zone year by year or the approximate size of that zone relative to Zones “A” and “B”?

**DiMarco:** The best indication is looking at Nancy’s plot of frequency of occurrence. Remember that 75 percent of the time it was typically off the Terrebonne and the Mississippi ... or the Louisiana Bight, and a little off the Atchafalaya. Then you had a large, less gray area that was 50 percent of the time. If you looked at that, you saw that the area that is covered 50 percent of the time or 25 percent of the time was much larger on the west side than it was on the east side. To me that is where the interest is. If it is always going hypoxic on the east side, then you know something about the system. But why is it not going hypoxic 75 percent of the time? That is what is driving the area. It is where it is not going hypoxic. It is larger.

**Parker:** I have one other question. Given the information they gave us yesterday about the biological processes and the relative unimportance of the Atchafalaya River to those biological processes, and that the Atchafalaya appears to be much more physically driven than biologically driven (whereas in the Bight it is not as biologically driven), is there a way we could then manage the resource based on the physical processes to reduce the size of the hypoxic zone by managing the Atchafalaya River and its distributary function better? Anybody that has a comment, go right ahead for it.

**Hetland:** I think that the problem is that Zone “C” is controlled largely by stratification. So whatever you do to the Atchafalaya, you are still going to be putting that fresh water out on the shelf. Some of these things that we do not understand about that zone are where the time scales are, particularly with the supply of organic material to feed benthic respiration. It is not particularly high in that part of the shelf. The benthic respiration you find there is typical of other regions that are not hypoxic, and the key element is the stratification that caps it off.

So I guess the key question to ask is this: What if we could reduce the nitrogen (N) coming down the river by 50 percent a day and maybe see in 5 years from now that we are still getting hypoxia in that region because it still has a long memory? We do not know. Well, how are you going to explain that to all the people who spent all that money to reduce all that N? If we can understand how that system works—in particular, regarding the times scales of biological/chemical processes that occur there—we can be prepared for the system not responding right away to changes we make in it. Did that answer your question?

**Parker:** This is the challenge we are presented with. You all are the experts, so tell us what you think. That is what we are asking. Yes.

**John Wilson:** I really appreciated the presentation. It was great. And I also appreciated Rick's comment asking to put it in terms others would understand. What I would like to ask is a couple questions to underline a couple issues that have come up here periodically in the reassessment. We have seen a lot of evidence about significant changes both in loading and in the size of the zone, starting in the '60s really up through now in terms of size. Are there any physical oceanographic issues that have any bearing on that change over the last half of the last century? Has anything changed that would tend to drive that? The second question is to ask that you underline your comments about the role of deep water upwelling of nutrients and whether in fact that is in any significant way a driver of the size or presence of the zone.

**DiMarco:** I guess I will take the second part first. It is a wide shelf, so the nutricline is fairly deep off the deeper part of the Gulf of Mexico. So it is actually going to take a very large event to get nutrients that are down below the photic zone up into the shelf in coastal waters. My feeling is, at least on the Texas–Louisiana shelf, that it is not like we are off Chile, where we have a very steep slope and a very narrow shelf. It is a very gradual slope and shelf. It is much harder to get the nutrient up. So I do not think that is really a player. I have not looked at that data in a while, but I do not remember a lot of upwelling of nutrient from the deep benthos, or the benthic part of the deep gulf, to the surface.

**Hetland:** But you might get upwelling from re-mineralized nutrients in the shallower part of the shelf. That could be significant, right?

**DiMarco:** That is exactly right. That was my next point about this. We are seeing with every cruise that there are lots and lots of nutrients associated with the hypoxic zone. So those nutrients could become available to produce, and we do see that often.

**Boicourt:** I would like to go back to the flow alteration question—if there was something we could do altering the position, or the distributary proportion of the Atchafalaya to the Mississippi. That is fundamentally a combination ... inherently an interdisciplinary question. I have been privy to seeing some of Rob Hetland's model outputs, and in my opinion, the very best way to approach that subject is to do some very simple, full three-dimensional numerical modeling. Get it right. Then do some very simple biochemical kinds of assessments and then do some what-ifs. I am very impressed with these, and I suggest that all of you stick around to hear the presentation. The kinds of assessments he will explain tomorrow are absolutely the right way to go. So I think the question is a very valid one. I am hearing flow alteration talk a lot, and I think that this is the kind of assessment that would really help that question. I hope I have not set Rob up too much.

**Wilson:** I just wanted to ask again the first part of my question, which was if there are any physical oceanographic changes in the last century that would correlate with the increase in size or presence of the zone.

**DiMarco:** For the physical oceanography, the good data are only in the last half-century on that shelf. You cannot really talk about centennial-type scale variability, based on the observations of currents or hydrography. That said, it looks like the pattern is reasonably stable and it is a fairly deterministic system. It looks like every summer you get the reversal of winds. Whether that is going to change due to climate change, global warming, or whatever is not known. I think we have to see that the substantial change was that the ratio of the Atchafalaya to the total flow coming down the river is a huge change. It was doubled in 75 years. More fresh water from the Atchafalaya on the shelf and was confined and stayed there. It is much harder to get that water (Atchafalaya) off the shelf than it is [to get] the Southwest Pass water [off the shelf].

**Boicourt:** Yes, but in terms of the hypoxia, Steve, you said that the physics did not do it. Is that correct? The physics was not the cause of the hypoxia.

**DiMarco:** Yes, the physics is necessary. It is not sufficient.

**Boicourt:** Thank you.

**Greene:** Alan, let us move into audience questions.

**Alan Lewitus:** Is it necessary to quantify the model at the weather band in order to address the management objective of the 5-year mean and, if so, why?

**DiMarco:** Well, the model is driven by the weather band. The weather band is 2 to 10 days in our shelf, and that is where you see a peak with our winds associated with the frontal passages. So just by running the model with realistic winds (the way Rob does the actual wind fields) is addressing that question. We are running it for the weather band patterns and also for the seasonal patterns of the winds.

**Hetland:** That is an excellent way to define how you are going to validate the model. So in answer to your question, you do not need to get the weather band variability right exactly. You need to get the effect of the weather band winds on the mixing and the advection on the plume correct. You do not need to get the details right, though. It is like getting the eddies wrong but the eddy flux right. Does that make sense to any one else?

**Lewitus:** Probably not. Try again.

**Hetland:** If you are going to model turbulence, you are not going to get every tiny little eddy right, but what you want to do is get the net effect of all the eddies correctly modeled, simulated, and parameterized. It is the same thing here. You do not need to get the details of the weather band right necessarily, as long as you get them statistically right.

**Audience member:** But from a management position?

**Hetland:** So the answer is no. Technically, you do need to get the weather band right in order to get the mixing right and to get all the processes right. Ultimately, you need to have weather band variability in the model. You cannot have a seasonal model or an annual model. You need to know what is happening on those scales at some level.

**Lewitus:** We saw a regression of hypoxia (area in question marks). How do you explain the departure from the original regression line in more recent years where much more hypoxia now occurs for a given river flow rate? Second part: What physical or ecological processes dominate? Third, what thresholds and feedback mechanisms might be important?

**DiMarco:** A lot of those points that were off that line were because there were not substantial other processes around that were scrubbing away at the hypoxic zone. I do not remember exactly which years they were, but it is in the data. In the data those years you had a larger hypoxic area than what you would have predicted with just a linear regression. It is probably due to the fact there was not a frontal passage or there was maybe a frontal passage or a storm or something—whatever processes were occurring.

**Lewitus:** This is for Nan Walker. Is it possible to monitor salinity distributions from satellites, and would this salinity information be a helpful way to monitor aspects of bottom-water hypoxia?

**Walker:** There is an effort by NASA to actually launch a salinity sensor in the next few years. I am afraid the spatial resolution of the sensor will probably not be good enough to monitor salinity on the shelf. Right now you can image salinity with microwave sensors, and the footprint or the pixel size of those is about 25 kilometers on a side. So it is very, very coarse. It has to do with the amount of radiation that is detectable in those wavelengths. You can monitor salinity with an aircraft pretty easily; however, that gets expensive. The best way to do it is still with inside two sensors. And yes, if you can monitor the salinity stratification, then you know you should have some idea of how bad the hypoxia will be on the shelf. But then you need more than just a surface value. You need some vertical information.

**Lewitus:** Okay. Another question. If you go beyond the science presented here to look for solutions, are there any sorts of engineered structures that could help to break up stratification? An example could be diffusers, vertical mixers, windmills. Given the large scale of the physical phenomena, would these sorts of devices have any potential to make a difference or just be a localized drop in the bucket?

**DiMarco:** The latter!

**Boicourt:** I often get phone calls like that. We get calls on the Chesapeake Bay all the time asking those kinds of questions. There are engineering firms proposing that. They even did some pilot programs. A drop in the bucket is the most accurate assessment.

**DiMarco:** I have seen people who want to shut down hurricanes by pumping cold water to the surface. It would take a million pumps to do it. It would be the same thing: it is just unattainable.

**Lewitus:** Okay, we have time for only one more question. The physics of the system is not going to change. The physical models can lend insight but do not say much about management. Should we not place greater emphasis on biogeochemical models that will provide insight into how much load should be reduced? Insight into the N cycle will provide more management information than insight into physics.

**Hetland:** Yes, of course. Obviously, that is where you need to put your effort. There is no question. However, you have to put those models into context, and that is the physical model. The physical model is just the environment in which you place these more biogeochemical models.

**DiMarco:** Good answer.

**Greene:** Given the physical models that we have, could any of you comment on how complex we need to make the biogeochemical models that will couple to that to “get it right” in terms of predicting hypoxia?

**Hetland:** I think that is completely unknown right now. We do not know what the dominant balances are. There is no nutrient budget for the shelf. It is not clear how much recycling of nutrients there is between the benthos upwelling into the surface layer and then falling back down. You have to be ready to do more complicated models until you can prove that you do not need them. And I think you need to have a lot of biogeochemical measurements to understand rates and processes that are important in the system in order to begin to create those models. The biogeochemical modeling is not like the physical modeling. We have the advantage that we know...we have a set of equations that we can start with. In the biogeochemical modeling there is way more complexity. And it is not clear what you need to include and what you do not need to include.

**Greene:** That was intended as a lead-in to the next session.

**Boicourt:** I want to point out that the statement I made about *your* modeling is that if in fact we are expecting the biogeochemical model to simulate reality in a three-dimensional structure and highly accurate simulation of tracking observations, that is correct. But if in fact we are trying to get insights so where we can set up hypotheses and test some basic rates and structures, I think the kind of model you are doing right now is right on. And that is quite simple, compared to these very complex models. If it is not oversimplified and they still have to put a lot of thought into them, I think they really are right on.

**Greene:** Just one follow-up question. You made some comments earlier about how accurate or how much detail we need to get it right in terms of the weather bands, but to what extent are there great uncertainties that we need to flesh out in the circulation models that will move us forward over the next 5 to 10 years in helping to resolve the questions related to hypoxia?

**Hetland:** There are a lot of unknowns in the model in terms of internal model physics. The biggest unknown is the mixing, which is obviously important for hypoxia. In my experience, boundary conditions are another very big unknown and in this case, the biggest unknown is what is going on offshore. Offshore forcing... obviously the deep gulf currents are energetic and influence the shelf circulation for about 50 meters. That is an important thing to address.

**Greene:** Are there physical barriers that limit the depth to which hypoxia could occur?

**Hetland:** The deep side rather than the shelf...?

**DiMarco:** There is a very large reservoir of oxygenated water at the shelf break, so that comes in and just erodes through advection and mixing, lateral mixing processes. It erodes away at the shoreward boundary. It depends on how strong that pycnocline is. I think I heard yesterday about 60 meters is about as deep as you are going to find it, and typically 30 meters is about as deep as you will find it. That is very consistent with what we have been finding, and it is usually because we have that seaward reservoir of oxygen to pull from.

**Hetland:** It could also just be the width of the plume—the width of the stratified area on the shelf. And it could be the energetic currents from offshore. That is it: we do not know. That is a good question.

**Greene:** Well, with that I would like to thank the authors and the panelists for a very good session.

*[Applause]*

**Transcript of Panelists' Discussion and Q & A and Audience Q & A**

**Session 4: Causes of Hypoxia III: Influence of Water Column Processes on Oxygen Dynamics Including Chemical and Biological Nutrient Transformations Leading to Hypoxia**

**Authors:** Michael Dagg, Jim Ammerman, Rainer Amon, Wayne Gardner, Rebecca Green, Steve Lohrenz

**Panelists:** Ron Benner, Quay Dortch, Frank Jochem, Hans Paerl, Rodney Powell

**Ron Benner:** All right, I think we have to jump into this. Right after this talk will be lunch and then the discussion. The next talk is by Mike Dagg. His coauthors are Jim Ammerman, Rainer Amon, Wayne Gardner, Rebecca Green, and Steve Lohrenz. The panel chair is Ron Benner, and the other panelists are Quay Dortch, Frank Jochem, and Hans Paerl. Rodney Powell could not make it.

**Alan Lewitus:** One point I want to make is let us try to focus the question-and-answer period more on what we know. There are a lot of uncertainties, and it is important to get across what the research gaps are. Absolutely. But we would like to focus on things we know—things that we can perhaps come to a consensus on or at least come up with some good alternatives to feed to the management process. So that is what we want to focus on here. Why don't we start with the panelists? Ron, do you want to start out?

**Benner:** First of all, I think that presentation was excellent for reviewing major biogeochemical processes that are important in, and the oxygen consumption in, the hypoxic zone. My first question relates to question number one up on the board in front of you—the relative importance of terrestrial versus riverine-produced organic carbon. Based on a lot of the information presented today and a little of what was presented yesterday, the indication is that the organic material from the river is not very important for fueling hypoxia. But I think we need to understand that the organic material is (if we think of the major elements) carbon (C), nitrogen (N), and phosphorus (P). Most of the discussion so far has been “What happens to the C?” and “How reactive is it?” There was some information that there is remineralization in the plume for dissolved organic matter from the river, and photochemical processes play an important role. But that is not going to affect oxygen consumption in the deeper waters where it is hypoxic. But what about the N and P that are released from organic matter that is cycling in the plume? How important might that N and P be in helping to fuel primary production? So, N and P might be the link from the organic matter that contributes to primary production in potentially what you would call autochthonous material that sinks into the hypoxic zone.

**Wayne S. Gardner:** I think that is a very good point about the N coming in or P driving the organic material. I think that the labile organic material that is formed is going to take up oxygen, but, as Mike said, when it is in the surface it does not essentially settle. That is, until it settles, it really does not have a role in the hypoxia. One important point I thought regarding that is that you have this material formed within. As it keeps recycling in the surface and moves away toward the shallow areas and then sinks out, it is going to make a

big difference in hypoxia in those regions. So the idea is that materials cycle and stay in the surface, and then as the currents move, part of those particles settle to the bottom and take up oxygen and so forth. That would include both the stuff freed from the organic matter that you mentioned and also the material from the recycling above and the nitrate coming in and being taken up by phytoplankton and recycling to ammonia, which in and of itself will take up oxygen. As the organic material settles down and is re-mineralized, it takes oxygen out of the hypoxic layer, so they are all kind of interconnected there.

**Michael J. Dagg:** Also, I think yesterday we saw that about 20 percent on average of the total N is organic coming in the river, so to the extent that organic N fuels phytoplankton production, it would enhance the entire system by, say, 20 percent.

**James W. Ammerman:** Let me make a comment about dissolved organic phosphorus (DOP). The measurements are very limited. We're generating some more right now. The DOP coming out of the river is probably half a micro-mole or less. Maybe there is a little more in the river, but the measurements are very limited. Clearly, from the enzyme measurements the organisms are capable of using some of this DOP. You do see it depleted offshore or to 0.2 micro-mole or perhaps a little less. So it could be a significant source, but it probably is still—given that inorganic phosphate coming in the river can be up to 5 or 6 micro-moles—a pretty small source.

**Steven E. Lohrenz:** I would like to add, along those lines, that there are other sources of the organic material that is coming down the river that would contribute to, I think, the delivery of C to the bottom water. And there are (and you might show the data, but I do not know that you really focused on this very much) certain times of the year that if you compare the vertical flux to the primary productivity, it actually exceeds the primary production. I guess the point I am trying to make is that there are allochthonous sources that can potentially be very important in terms of the overall flux of organic C to the bottom waters. Maybe the next question is this: To what extent does that actually contribute to bottom respiration—to oxygen demand in the bottom waters—particularly if it is sort of pulsed at a specific time of year?

**Benner:** I think that the dissolved phase coming in from the river is only through this linkage—through N and P—contributing to oxygen consumption in the hypoxic zone. There seems to be a consensus on that. Regarding your suggestion that maybe there is some particulate material of riverine origin that is sinking, I thought it was telling in the previous presentation by DiMarco that in the hypoxic layer there are a lot of suspended particles. I'm wondering if you have any suggestions as to what the source of that particulate material might be and if there is any information on that or on its reactivity?

**Gardner:** I would think a lot of that would be resuspended, but that is just my guess. And that would include organic material and really fine clays.

**Dagg:** Well, the main sinking particles are phytoplankton, large phytoplankton, fecal pellets, and aggregated stuff from the water column. Those are the main sources of the materials you are talking about. The balance between those three I do not know.

**Quay Dortch:** My turn next? When we first started, we heard a couple of talks from the upstream end about that, and one of the questions they posed to us is this: Is it N or is it P? Before Alan made his remarks, I was going to ask the question that Mike had said—that we need more research on which is the nutrient, the limiting nutrient, and what the critical gaps were. But that question was taken off the table, so I think I will ask a little bit more controversial question. In my own research, I have gone from thinking it was entirely N limitation to becoming interested in silicate and, more recently, in phosphate limitation. I realize that we have two issues here: one is how the nutrient availability affects production, primary production: then, how that nutrient availability may affect the C flux. So I guess what I am going to do is ask the panel members instead—the ones that feel they have the expertise on this: Is there a consensus we can reach on the question of N versus P? I have my preconceived ideas of what the answer is, but I'll let them speak for themselves.

**Ammerman:** Well, I guess I'll give my phosphorous speech here. If the Mississippi Basin and the Gulf coast were a pristine system, I think it would be like any other coastal marine system—largely N-limited. The river would obviously have an impact, particularly in high-flow periods, but given the N loading to the system, I think what we have seen is that it is pushed at certain times and places. It has pushed the coastal system here into P limitation. Now this is something that has been predicted. It is being seen around the world with increasing N eutrofication of coastal regions. Particularly in places where P has been controlled or decreased, this is even more evident. And so what you are seeing now is an apparent P limitation of phytoplankton growth in the spring and early summer (that time period in particular) and secondly in the region just west of the plume of the river we've talked about in Steve DiMarco's parlance, it would be the "green zone;" in some of the other discussions, it would be the "mid-salinity/high-productivity zone." We do not know the importance of the P limitation to hypoxia. What we do know is that the P limitation is most apparent during the high-productivity period of the year and also in the area where we know a lot of productivity occurs. So potentially it could be an important producer of C that could then lead to hypoxia. There are a bunch of steps in between that we do not understand well. That is how I would summarize the P story now.

As Mike showed in his talk, it varies from year to year. Some years the P limitation is stronger; it seems to be a higher-flow year. 2001 and 2002 were particularly good examples. In the example he showed for May 2001 from our data, basically that same picture was evident in July 2001 and July 2002. So it seems that it is important, most important, in a high-flow year, perhaps when the high flow is early in the season. So you get those nutrients out on the shelf at the beginning of the growing season. In 2004 there was more limited evidence for P limitation, and it was still present. But the flow was considerably reduced, and the higher flow that year was May and June actually! So it was unusually late. We have evidence of some degree of P limitation going back to the early '90s with the Nutrient Enhanced Coastal Ocean Productivity (NECOP) project. So it is nothing new, but it seems to be accented in high-flow years, particularly those years in which the high flow is early.

**Dortch:** I doubt the consensus. I think this consensus would be something much broader.

**Dagg:** I will repeat what we talked about at the break, Quay, and that is it seems to me that all three—N, P and silica—are important in limiting under different circumstances. I think we could put together a picture that describes those circumstances pretty well with the data we have, but it needs to be done and it has not been done. We have what Jim just said—indications that P-limitation is more widespread during high discharge and so on, and when does silica come in. I think there is enough data and knowledge around to put that picture together into a nice description or a conceptual model of it, but it has not been done and it should be done.

**Gardner:** One thing I would like to add about silica. I do not know if it is limiting or not. It becomes so, but the fact that it changes the benthic-pelagic couplings so much is a very important aspect.

**Lohrenz:** Yes, I would agree with Wayne on that. I think we can reach a consensus on the fact that all three nutrients have a role in controlling productivity or aspects of community structure in different conditions, seasons, and salinity regions and so on. But the question remains: How does that influence the delivery of organic matter to the bottom water? In other words, if in fact during these high-flow periods the mid-salinity region, where there is the highest productivity, if that is indeed a major source of organic matter that fuels hypoxia throughout the summer season, then it is certainly going to be key in managing that problem. On the other hand, if there is a lower level of input that goes on during the summer when N becomes perhaps the more critical limiting nutrient, then that is obviously going to be an important management consideration.

**Frank Jochem:** So now that we have talked about the production, of course, to make the hypoxia, it is of less importance maybe where the production is, but how do we get the production down to where we make the hypoxia—to where the music plays? We know that particularly over the summer, despite the nutrient loading, there is the shift from diatom to more non-diatom phytoplankton and to smaller forms. There is a substantial part of phytoplankton, that is very small, that we know is not raised by nocopophax<sup>5</sup> and that does not sediment on its own. So this aspect about the lowerdicene<sup>6</sup> was very interesting to me. I think we definitely should explore more how much of a vertical transport that could provide, especially when we consider Rebecca's paper or estimate about only 26 percent. I mean, you could say that is a significant contribution. I put it on the other hand: Oh, it is only 26 percent that would fuel the mid-water or the benthic C demands. Where does the rest come from? We looked a lot at the micro-zooplankton on the phytoplankton, which again we say protistan produce fecal pellets that are prone to sinking. I do not know if the lowerdicene<sup>7</sup> can explain all the vertical transport of C that we need to explain the C demands. So my question is this: Are we missing or have we not looked enough in the protist-copepod link? Would looking into that help us in explaining the fulfilling of the C demands?

**Dagg:** Of course, copepods feed primarily on larger particles, which include the protozoans (as you point out), and their fecal pellets from that diet will undoubtedly contribute to organic

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<sup>5</sup> Verbatim phonetics from tape recording. Please verify the term.

<sup>6</sup> Verbatim phonetics from tape recording. Please verify the term.

<sup>7</sup> Verbatim phonetics from tape recording. Please verify the term.

matter flux to the bottom. The magnitude of that I do not know. And the other methods for transporting small cells to the bottom also need to be looked at more—the aggregation processes that lead to bigger, more rapidly sinking particles, which could transport groups of small particles down. There is a great need for more direct measurements of vertical flux and for analysis of the composition of that material.

**Lohrenz:** I just want to say one other thing about vertical flux and its relationship to productivity, and again this is hugely speculative because there is so little data. If you look at the seasonal pattern and relationship between primary production and the vertical flux, you find that in the spring-winter periods the flux tends to represent a higher portion typically of productivity, which is not atypical of other systems. Then in the summer you find higher productivity in some cases and yet lower flux and a much lower percentage of flux during the warmer months, which actually also coincides with what we often see as a shift toward smaller phytoplankton. I am sure, Wayne that you could comment on the fact that that is so likely a period of tighter coupling between the grazers and the primary producers. So maybe that is a period when there is a more efficient recycling less export. I do not know if that bears on your question or not, Frank.

**Dortch:** But I would also note that Nancy in her talk showed some core data with zeaxanthin—huge increases in zeaxanthin in the cores, at the surface of the cores—suggesting that picocyanic bacteria (and I guess there are a few others that have a small amount of zeaxanthin) are getting to the bottom and there has been a huge increase. Just looking at our data, it looks like a 6- to 10-fold increase in the flux of little organisms to the bottom. So somehow even the little ones are getting down.

**Hans W. Paerl:** I would hate to argue against my favorite organisms, but I think we need to be pretty careful about those data because the influence of diagenetic processes needs to be taken into consideration in those cores and that was one of the questions I was going to ask. To what extent is that curve that we are seeing in the sediments actually diagenesis versus historic accumulation? That is just, I guess, a point of concern.

**Donald F. Boesch:** It still finds its way to the surface.

**Paerl:** Sure, but it could be that there was just as much coming down 50 years ago. It has just been decomposed.

**Boesch:** My point was then it does get down. Small.

**Jochem:** I guess my point was that it gets down, but I think we do not really know and understand the mechanisms by which it comes down.

**Dagg:** But I think we can agree that it does not get down as individual cells, sinking by themselves. They do not sink fast. That is all.

**Rebecca E. Green:** Yes, actually I wanted to say something about this. For the nitrogen/phytoplankton/zooplankton (NPZ) modeling work that we did in the plume, there

were a lot of especially intermediate to high salinities, a lot of organic C transfer from the bacteria and small cells through the micro-zooplankton. From a modeling perspective, it is in order to match the other processes in terms of how the micro-grazers are feeding. It is difficult to match the model results with the actual measurements. We found ways to make it work, but there is a lot of organic C transfer through that micro-zooplankton component. And they do reach high biomasses in the plume. So knowing what is happening with those micro-zooplankton is clearing an important link for understanding how organic C is getting to the bottom, especially the transfer of those small cells through the food ladder to the bottom. There are still some questions in terms of how the micro grazers are feeding that need to be answered in order to understand that transfer.

**Paerl:** Quay and Ron already asked my question, so I am going to put a manager's hat on now and think about this: Okay, we have maybe 23 percent of the tweakable production in the system that is fueled by nutrients happening at a time when there is a huge excess of nutrients apparently in the system, particularly N. The question is: Where does that leave managers in terms of their being able to explain how we manage nutrients in order to be able to control that again, assuming that that is the most manageable component of new production in the system, particularly since the springtime. I was given a transparency that you did not show in your talk, that indicated that the N-to-P ratio at times was exceeding 100, during the spring, over total P. Is that right? At times of the maximum production?

**Dagg:** Well, you showed a May figure on the shelf that exceeds a thousand.

**Paerl:** Okay. Well, anyway I guess I would like to get some response from you. If I am a manager sitting here to figure out whether we are doing the right thing, whether we should put things on hold, where do we go?

**Dagg:** I am not going to answer that, [*laughter*] but I will clarify something, and that is that the 23 percent number from the model was for the Mississippi plume. As Steve DiMarco clearly showed this morning, or nicely showed this morning, about half of the total freshwater input to the Louisiana shelf probably comes from the Atchafalaya. That 23 percent number that we provided does not include the Atchafalaya. So if it is half the freshwater, it is going to be a significant addition—maybe more, maybe less, depending on the processes and recycling, but it is certainly a lot of material. So with that maybe I'll partially answer your question and say clearly the river inputs are extremely significant and that may still be the main avenue to pursue. I do know I am not going to talk about the management issues in that sense. But we cannot just say, "Oh, the river inputs aren't important" because they certainly are the dominant input of nutrients, even though perhaps it is not as big at the Mississippi as we would have anticipated.

**Gardner:** Well, also, as I tried to say before, the ultimate effect of this N or P, whatever, coming in does not always happen right near where it came in. But if it stays in the surface waters, ultimately it is going to have the same effect as it drops out. As it spreads over the more shallow waters, it will probably have a greater effect than it would over the narrower ones. So it is still the nutrient input that has to be driving all these things that take out oxygen ultimately.

**Ammerman:** I will risk a comment on that. Clearly N is the main issue here. It always has been. It is the issue in most coastal environments. Nonetheless, the consensus again in most estuarine and river-impacted coastal environments is that both N and P should be considered in terms of nutrient control. Now clearly the N reductions required in the system to achieve our goals for hypoxia are going to have to be larger than we originally thought they were. The one-to-one correspondence does not happen. That is pretty clear. And certainly when you are talking about point source issues, P is easier and cheaper to control anyway. I think the issue is, in most of these systems, consensus over time, the Baltic etcetera., both N and P certainly need to be looked at. The issue is when you look at agriculture, as we heard the other day, the conditions for major N and P runoff from agricultural fields are rather different so control aspects would be different. That would be an important issue. Again, I would say that the overriding issue is N, but because of the nitrogen input P is now something that should be considered as well.

**Green:** Actually, that is a good question, and it is something we were talking about after lunch. I think that in terms of that 23 percent contribution there are clearly different areas along the shelf that are going to have different microbial cycling of organic C and different grazing rates. The region where we put together this budget is an area where we know or we have a pretty good idea what those rates are, whereas off the Atchafalaya not as many measurements have been made of the bacterial grazing and phytoplankton communities.

So we put together a budget for this area which is, you know, the Mississippi River turbidity plume essentially. It can be constrained by the suspended sediments. We are saying that that area contributes this percentage. We know a lot about the microbial web and grazing there. It would be important to put together similar budgets for other regions of the coast, whether from the wetlands or the Atchafalaya area, to know what they contribute.

Certainly one big difference in terms of the physical oceanography of the other two systems is that dilution plays a very important role, where the river plume enters from the Southwest Pass. That water is pretty quickly diluted and has a short residence time. Whereas let us say 50 percent of this water is coming in through the Atchafalaya Bay. That water can spend a lot longer time there and on the shelf and potentially contribute more productivity. That could be an important part of the budget or the coastal wetlands. But presumably that N either now or originally locked up in the wetlands has come down the river at some point. So it seems like it is still a question of managing N or possibly another limiting nutrient.

**Boesch:** Alan, could I ask you to put the slide up? I want to use the pictures. It is a complex issue, the question. [*Speaking to graphic on slide*] Mike showed this. It is Rebecca Green et al.'s paper. He showed a nice colored version (I only had a black-and-white) of the analysis they did, which was a very courageous analysis bringing together a lot of data over a long period of time, to understand the plankton dynamics and biogenic chemistry, particularly C in the plume, the Southwest Pass plume, the first thing you will notice, if you look at the pictures, is there is not a lot of coincidence of plume and hypoxia. So the question is, how do you know how much of that, production, all of the sedimentation in that plume is credited toward reducing hypoxia?

**Green:** That is right, and we just looked at the months from March through June.

**Boesch:** Right.

**Green:** Right, because it is assumed that in the summer time...

**Boesch:** So I am on this panel of both authors and panelists—this hegemony of plankton scholars. I am a benthic guy by training. And I immediately get “Where is the bottom?” In the figure on the right, it shows you where the bottom is—that indeed most of the area under the study in the plume area lies over water that is 50–200 or more meters deep. So if indeed, as Mike summarized, the settling rates on the order of large particles—pico-plankton aggregates and so on, on the order of tens of meters a day—it is implausible to me to get that organic matter into the hypoxic zone for a variety of reasons. One is that once it seeps down past 30–40 meters you have to get a mechanism to get that deep, dense water up on the shelf. In the physical oceanography data, the current data, which are actually pretty scant for this area, you do have that eddy flow back. But for subsurface there is really no evidence on cross-shelf transport that you have any transport mechanism, anything like that. So I would think that the 23 percent, assuming that all the other parts of the analysis are the same assumptions, has got to be far too high. It has to be a fraction of that. Just look at it. How much of that production is going to end up in the hypoxic zone, given the fact that once it has settled down in a matter of a couple of days it is not going to find its way back up there?

Now you could also argue that if it depletes the oxygen in the subpycnocline, and that water with reduced oxygen is also important as the source of the hypoxic area. That is also a factor, but still you do not have a physical mechanism to get either depleted oxygen water or C onto the shelf anything like 100 percent and probably far, far less than that. So I would think that what we are looking at here with the studies of the southwest plume is more the muzzle fire at the end of the gun, rather than the bullet, with respect to hypoxia. The bullet is what happens to the nutrients that escape the system. They are (and this is now, I think, well understood in terms of the basic physics of freshwater conservation) entrained into the coastal boundary lane, the Louisiana coastal boundary current, which flows to the west and reverses in summer. So it is that mechanism that moves water. And the nutrients which are in that sense, particularly for P, depleted largely through N, have to be largely be re-mineralized to support the production in the coastal boundary downcurrent.

Now if you go to the next one, Alan. This was an aggregate figure that I think was shown earlier. Steve DiMarco might have shown it. It was in the pre-print of his presentation. This is a section 92 degrees off the Atchafalaya, so there is an Atchafalaya influence, but these are very typical sections from the shallow water offshore. You see it off Terrebonne Bay and so on all the time. The first thing you want to notice is that we do ourselves a disservice as oceanographers when we do vertical plots because these plots are exaggerated a thousand times,  $10^3$  times. So think of this as not something like that but something that is really thin. Okay? So then you think about where the sources of nutrients are, where the processes are, and you have to think of the vertical dimension when you are dealing with meters as opposed to 1,000 meters, 10,000 meters, 100,000 meters in a horizontal direction.

Mike made the point that re-mineralization recycling within that is not happening. I wonder about that because first of all even though part of the graph is kind of shorn off, on the oxygen you will notice higher nitrate levels on the bottom, as you might expect, and then it looks like it gives out. Well, the reason of course is the hypoxic zone. It is ammonia, not N, there. So it would be interesting to see what the ammonia is there. You have high regeneration, high concentrations below the seabed. If you could see the other end of the salinity end, that stratification runs into the bottom more or less about 10 meters. So you have this area of really intense breakdown ability to remobilize, mix things up, from surface to bottom. It is not a lake. It is a dynamic system, as we heard Steve talk about—a lot of wind forcing, a lot of sediment resuspension and so on in the system. I think the first order is that you have to understand that process of the recycling and re-availability and re-mineralization of nutrients as a source of the production that drives hypoxia. Because you will notice that especially if you think thin, recast it as a thousand times wider than it is deep here. You will see that the production that supports hypoxia really comes from the top, vertically, rather than several hundred kilometers away. Why do we know that? We know it for a number of reasons. One is that you will see that again and again, when you have hurricanes break things up or fronts, these things re-set up. So there has to be a local organic source to supply it.

So, if you show the last slide. Mike mentioned this, but he did not show the slide and it is not a very clear one. I just had to take it off a PDF. This is the Chen et al. paper. It is very characteristic. You see it on all the satellite images, confused a bit because you have got suspended sediments there as well—obvious in shore, coastal boundary layer, high chlorophyll level. That is the source of C that causes hypoxia. The Atchafalaya River mouth enters into this obviously—a major thing. And as it goes downcurrent either to the west, which is the area that we remember is just physically dominated, there is no biology going on. Right? See it? Or from the Mississippi Bight, the Louisiana Bight, off to the west, it is those processes that are taking place in the coastal boundary layer that affect hypoxia. That is very important to understanding the nutrient relationships because it is all re-mobilized, reworked nutrients one way or another. It may not be from the contemporary sources that year, particularly for P, which is conserved. It is not de-nitrified into the system. So I think we have to understand, in terms of the nutrient forces, a much more dynamic view of this, in which we are not distracted anymore by the Southwest Pass plume. I do not think it has any significant thing to do with hypoxia creation other than determining how much of the nutrients N and P escape the plume and get entrained in the coastal boundary. So, I would like your comments about this.

**Dagg:** A couple of things, and maybe Steve DiMarco can clarify this a little better. The coastal boundary layer is a well-mixed layer. That is fine. It is shallow and it is frequently well mixed. The narrow layer along the coast, near the coast, is in most systems tidally mixed, but here it is tidally and wind mixed. As a consequence, any nutrients in that coastal boundary layer will be in the lighted zone a lot and there will be a lot of productivity there. There is no doubt about that. The issue is: How does that productivity contribute to hypoxia? If it is transported out into stratified waters and sinks or gets somehow below the pycnocline outside that boundary layer, it can consume oxygen and contribute to hypoxia, at which point it no longer becomes recyclable back in the coastal boundary layer unless it is transported

back in there somehow. And that is what I talked about up-loading which is something we do not know much about. That is my first point. The second one is not in any way disagreeing with your comment about the Southwest Pass input, assuming that the nutrients from the Southwest Pass are the source of the coastal boundary layer chlorophyll and productivity. The calculations we made are still valid. We assumed all the productivity from Southwest Pass contributes to hypoxia. We did not try to deal with how it got there. Your point is that it is just separated.

**Boesch:** What you are saying is that it is the nutrients that are escaping?

**Dagg:** Right.

**Boesch:** It is not the organic.

**Dagg:** It is the whole nutrient load from the river that can cause the productivity that contributes to hypoxia, one time. Once it is in the hypoxic layer, it does not get back to more productivity until there is a method for that.

**Boesch:** Right.

**Dagg:** It certainly gets back in the fall when there is an over turn and the pycnocline breaks down. It gets back if there is upwelling (which is something I mentioned in the presentation) but we do not know much about that process. It gets back, as we saw through Nancy's and Steve's maps of hypoxia. It clearly —there is exchange across the pycnocline vertically. The issue there is if there is nutrient exchange up, then there is oxygen exchange down, and both of those factors have to be calculated properly in calculating oxygen demands. So you did not ask me a question. I am going to end with a comment, too. And Rebecca might want to add a few more. I do not know.

**Lohrenz:** I was just going to say that the only scenario that I can envision—and maybe someone has more insight than I do—but where the sort of mechanism that you are talking about, Don, would actually work is if there was a de-coupling between the re-mineralization of N occurred, whereby N was released more rapidly from settling material than was able to be remobilized and made available again for productivity and C continued to settle. I do not have information that that is the case.

**Dagg:** But the C:N ratios in the plume in the surface waters and subsurface waters are approximately the same, based on your sediment traps. So it does not seem to be. There is no evidence for that de-coupling.

**Lohrenz:** I make one more comment, and Rebecca can correct me if I am wrong. The way that the plume was defined for the model is based on a suspended sediment signature. You know, that means that that signature is still in the surface. It represents the surface sort of manifestation of the plume. The flow during that time of year does tend towards the coast, does carry that material in towards the coast. Otherwise, I think what you are saying is

correct, and it is certainly the productivity. That certainly is what Rebecca's model would support—that it is not enough by itself.

**Rainer M.W. Amon:** I think that is one other pathway especially on the nutrient conditions than off the ratio. If you have very high N-to-P ratios, your production of DOM could go up quite a bit, and then the DOM could be transported on to the shelf, entering the microbial loop and micro zooplankton flux to the bottom later on. And we do not have a good understanding of that at this point in time. But you have very awkward ratios, which probably cause changes in the overall biogeochemistry of organic matter. Going from more particulate to potentially more dissolved organic material.

**Boesch:** I think the point is, I certainly agreed, in fact Steve mentioned that we do not know much about the processes that relate to the vertical mixing. The other thing I think it is important to remember is that this is not a lake. This is an open Gulf of Mexico. There are a lot of bed-scale dynamics that cause breakdown of stratification near the coast. That pycnocline runs into the bottom at some point as it runs into the coast. That is a very dynamic area that could be responsible for re-injecting. I do not know the rates, but I am saying, first order, if you want to look at the cause of production that causes hypoxia, it is from above rather than 200 kilometers away.

**Green:** One thing I would say about that in terms of the region we looked at in the model, once you are outside that zone, nitrate is very low. So we constrained the model to basically an area of the plume where the nitrate gets down to very low levels by the edge of that region. So we have looked at an area where a lot of nitrate is being taken up into very high rates of productivity. The question then would be where is all the N coming from if it is happening in the surface waters? Clearly in this region we have looked at, a lot of the N is being removed. So, if you think it is not, if this whole area is not making it along down shelf then where is the nitrogen coming from?

**Boesch:** In Jim's work, in the periods where there was phosphorus (P) limitation, it shows that the P runs out and there is N. It seems to be if you contour it, it seems to be continuing from that into the coastal, down coastal boundary. Is that right, Jim?

**Ammerman:** Yes. In the figure that Mike showed of May 2001, which was high flow, there were 10 micro-moles of nitrate along the coast almost all the way to the Atchafalaya. But it is not always quite that high.

**Green:** Yes. That was unusual, was it not, Jim?

**Ammerman:** 2002 looked similar, but certainly 2000 and other lower-flow years would not look like that.

**Benner:** I would like to revisit something that I brought up in the very beginning. I think it needs some reconsideration. We are talking about regenerated nutrients, and regenerated nutrients are obviously important in fueling it. The question is: What was the original source of those regenerated nutrients? Again, you might say that for N discharge the organic N is 20

percent, so that is 20 percent of the total N that is coming from the river in organic form, and that is regenerated and that contributes to production just as nitrate does. Also, in these coastal areas (which has been pointed out in Mike's talk and in the talk yesterday), there is a lot of coastal erosion; there is a lot of organic material that is not entering directly from the river. That is typical of coastlines in general, but you have a lot of marsh-derived organic material. The carbon (C) itself might not be contributing to hypoxia through oxidation of the C, but the N and the P that are associated with that material also are regenerated, and that is another source of the N and P that fuel the primary production. If you implement a plan where you are going to reduce nitrate in the river, then you have to understand that as you reduce the nitrate, as we have heard before, it is not going to be a one-to-one correspondence with hypoxia. As you reduce nitrate, the relative importance of the organic forms of N and P increases. You gave an estimate, Mike, of how much C for that coastal erosion. You gave it as per square meter per day, and it was around 1 gram.

**Dagg:** Right.

**Benner:** Do you have an idea what the C:N of that is and how much nitrogen that might be?

**Dagg:** No, and I want to emphasize that those are what we call "back of the envelope" calculations. We assume 100 percent of that is labile. It is a very approximate calculation. The point being, however, just to emphasize it, is that the number spread over the entire hypoxic region is on the same order as phytoplankton production, dissolved organic carbon (DOC) production, by marine processes. It is not a trivial number, and it needs to be looked at more carefully. I do not know the ratio of C, N, and P in wetland organic matter. I do not know at all what that is.

**Boesch:** A reference was made earlier to the Georgia and South Carolina coast in terms of the contribution of marsh wetland organics to offshore. Although there is an increasingly large volume of water in the bays, it is a relatively micro-tidal system. There is not a lot of tidal prism to these estuaries to try on a regular basis and so you have to ask the question: Why would an estuarine ecosystem be sending out labile organic C?

**Benner:** I do not understand, because if it is labial it gets recycled again—it gets regenerated again—and the water does not stay there. Once it is a regenerated dissolved nutrient, it is in the water and that water is [*crosstalk*]

**Boesch:** The nutrients, not the organic.

**Benner:** Nutrients.

**Boesch:** Well, his calculation was based on the organic.

**Benner:** Right, and I am saying that with that C go N and P, and they also are regenerated, and that is significant.

**Boesch:** Those are also things, of course, in that wetland. In estuarine systems there are processes called sedimentation and so on in the bays that can conserve P, but there is also a lot of de-nitrification that takes place and any N that is liberated. I am just saying that all of those things have to be brought into the calculation to make it more realistic than the “back of the envelope calculation.”

**Dagg:** I agree fully we need to look at this much more carefully. I wanted to make one point of clarification, though, and that is the area north of the Gulf of Mexico is a micro-tidal area. We have been looking at this now carefully at Monkton. The marsh does flood frequently. Now it does not flood very high like a hurricane, but water does go on the marsh with the tidal cycle for several days each normal tidal cycle. Water will go on the marsh and drain off. More important, as you know, is that most of the water-height around here is wind-driven. You do not need a north wind in the winter to push the water out of the marsh and then have it flood back on. A setup occurs quite commonly in the summer in which you get one to two feet of water on the marsh whenever you get a little strong south wind for a couple of days. Then as soon as that relaxes, that water runs out. I think it is well shown that traditional nutrients are not important in that exchange. Nitrate and phosphate do not really do well in that hypothesis. But organics have not been looked at very carefully. That is the point.

**Boesch:** Let us compare it to the Carolina–Georgia situation—the whole volume of the estuary virtually changes twice a day.

**Dagg:** Right. It is a different process. And here it is not a daily cycle; it is an event cycle, an event-scale process that can lead to significant flushing and relaxing of the marshes on specific event occurrences.

**Paerl:** Just one quick follow-up comment to Don’s comment about what appears to be the disconnection of the Southwest Pass plume and where the hypoxia occurs. That may well be true for C. But that plume is also acting as a nutrient filter for nutrients that are going through it that may be impacting, in a soluble state, production downstream. So we should not ignore it. I think this is all part of the whole issue—looking at the entire system where the one plume may be modifying the nutrients. They ultimately come and trigger the C or the production that may play a more intimate role with respect to the hypoxia downstream. We do have a good example of that actually in our mini Gulf of Mexico in North Carolina. We can see that managing P really tightly over the ’60s and ’70s in this river estuary has moved the chlorophyll max downstream because it removed the filter to allow the nutrients to be taken up farther upstream. So now all we have done is kind of move the problem down into the lower Neuse River and the Pamlico Sound. There are some examples that clearly show there is a connection there. Even though the C may not be connected, the nutrient flux is connected. Yes, that is the one right there.

**Alan Lewitus:** Are there any other questions/comments?

**Boesch:** I would like to add one more comment because I said all the dynamic processes on the coast boundary layer yet need to be studied. I am the first to argue for that. But the stubborn fact remains that there is no evidence of a secular increase of P loading in the river

other than is driven by the increase of flow at the time at which there was a threefold increase of nitrate loading coincident with hypoxia. That has got to tell us something.

**Ammerman:** The P record in the river is limited and poor. What it shows is perhaps limited increase over time, but that is probably at best. The reason that P is an issue at all is because of all the N loading. But there are certainly riverine sources of P that could likely be dealt with that may help the issue. It would be much nicer if we had a much longer, better record of P in the river. The best we could say is that there is a slight increase over time. But clearly there has to be some runoff coming from agriculture and other processes.

**Lohrenz:** I will just make one more comment regarding the correlations over time—the period that you were talking about, Don, where we saw the increase of N also coincide with an increase in discharge. So that is another correlate in all of this and one that I think we have to be careful of. There may be a contributing factor, as we have heard.

**Boesch:** There is, as we discussed yesterday, what is it: 80-20? What is the set theory— 20 percent flow-related and 70 percent increase load-related? There have been several of these.

**Lohrenz:** Well, I guess my point is that there is an impact on the physics, as well as a result of the increased freshwater discharge. As we have heard from various people, that cannot be ignored as a contributing variable to all this.

**Boesch:** Absolutely not.

**Lewitus:** Yes, it is a very important discussion. It went long and that is okay. We are not done yet. Steering committee, please?

**Rick Greene:** I just want to follow up a little bit on the N-to-P issue again — I am not sure if there has been any new analysis done regarding the long-term record of phytoplankton— either size distributions or species composition— that may coincide with the shift in whether it is N- or P-limited. I would like anybody to comment on that. One would expect to see perhaps a shift in either one of those two parameters. Second, either I missed it or it was not thoroughly discussed: Do we actually know where the zones of high productivity are—not high biomass, but high productivity—across that part of the world?

**Ammerman:** I cannot address the phytoplankton composition issue. What I can say is this: We have evidence for P limitation dating back to 1990. It is limited; it is only certain seasons. The seasonal pattern, however, seems to be the same as we are seeing now: spring and early summer, changing to N limitation in the fall. I think Steve could address the productivity issue. I do not know about the phytoplankton composition.

**Lohrenz:** One part of your question was: What is the relationship between biomass and productivity? You are talking about chlorophyll biomass, in particular? Did I get that right?

**Greene:** Well, we have seen a lot of maps on chlorophyll biomass and where those high biomass areas are. If I missed it, I apologize. But are there similar data sets for productivity rates and, if so, where are those high rates of productivity in the surface waters?

**Lohrenz:** It is a pretty good relationship. There is a very strong correlation between surface chlorophyll and primary production, at least in the immediate plume area. As you get either farther offshore or farther away from the strongly freshwater-influenced regions, it may not be quite as tight.

**Paerl:** Steve, a question for that. I know that spatially we have a good correlation between chlorophyll and productivity, but I think temporally the highest productivity is not linked with the diatom spring bloom but it is actually occurring in the summer, when the standing stalks are actually lower. So I think this is something that we should consider as well. Spatially, yes, but temporally we might not have the highest production when we have the highest—when we have this diatom spring bloom, which is the most prone to sedimentation. So that is why I came back. We are still looking for what gets the stuff down there because the productivity is highest when the organisms are not likely to sediment.

**Lohrenz:** Yes, that is a really good point. The biomass-specific rates are much higher in the warmer periods so, yes, I agree with that.

**Greene:** So it is at that time of high biomass-specific productivity during the summer where it is really—is that the source? It is occurring under largely N-regulated times of the year. Is there a consensus that that is the dominate source of organic matter reaching the bottom and contributing to hypoxia, throughout a very broad length of the coast?

**Lohrenz:** I do not know that I would say that is a consensus. Certainly, that argument has been made and there are calculations, although I could not give you any details on that. So it would suggest that there is enough productivity or at least a fraction of productivity to support the rates of respiration. But I do not actually have that data. That is something I have heard from Nancy and other folks.

**Dortch:** Another way of looking at that—and this is complicated, so I may get lost in it almost—is the diagram that showed that somebody referred to as an unusual time, where there were high nutrients all over the shelf in the springtime. I am not sure that that is as unusual as some of the discussion has led us to believe. One of the problems here is that everyone is looking at little bits of the data, and we have never put all the data for all the cruises together (and some of these are funded by very disparate sources and people have not been working together). If we were to put it together, I think we would see that there are probably more times in the spring when there are high nutrients over large parts of the shelf. So if there were periods where you had high production and high vertical flux that set up the conditions for hypoxia by a lot of sedimentation to the bottom and then there is summertime, the flux from that material in the summer may not be as high, even though the productivity is high. But it is enough to keep the hypoxia going as opposed to actually getting it to set up. Purely speculative. We do not have enough data to answer that question, although some of the nutrient data that I have seen from the sea transect argue that every spring you have high

nutrients on the sea transect, which is right through the core of that hypoxic region. That argues that you get high nutrients out there every spring. It may be that we just do not have enough shelf cruises to go out there all the time. So, one of the problems is a lack of data in critical periods. Now there are more cruises, so perhaps we will capture more of those in the upcoming years. I certainly hope so.

But nobody answered your phytoplankton question with respect to phosphate. Obviously, I am sitting on a lot of phytoplankton data. We are working on that right now. From a historical point of view, I do not think we are ever going to get a really good answer to that question because I cannot think of any proxies that would give us good answers to that, historical in the sense of the last 50 or more years. I think in terms of silicate, though, the potential changes in species structure as a result of the changes in silicate are much, much greater and are more likely to have an impact not so much on production but on the flux to the bottom in the hypoxia. We are still working up some of that data.

**Ammerman:** Let me add one comment, Rick, quickly. The conditions that Mike showed for May also in terms of high N-to-P ratios and everything that went with it pertained also to July of 2001 and July of 2002. The cruises were a week or two before Nancy's annual survey. So there was still very strong indication of P-limitation in mid-July.

**Greene:** I do not think that the high nitrate concentrations that they showed are a common event. I agree there is a lot of variability there, but we have not seen 10 micromolar down that far west on a number of our cruises. But it is just variability I am sure. I want to follow up on Quay. There was a point made yesterday regarding the changing elemental ratios and the suggestion that we might be entering a phase more conducive to harmful algal blooms, more flagellate-dominated systems. I am just wondering. I am not aware of any in situ production of large-scale blooms in that system, other than those being transported in. Have we seen anything recently that would lead us to believe that there has been a change in more flagellate-dominated systems?

**Dortch:** I would not say so. Of course, one of the problems is that one of the volatile<sup>8</sup> species is a diatom that is the dominate species out there much of the time. It is one that can live with very little silica. So, I think what has happened here is it has tended to go toward a system—because it is at one right now—of lightly silicified diatoms and little tiny centric diatoms. So the core data that looks at the diatom species other than the *Pseudo-nitzschia* shows a bit of an increase over time in little centric diatoms, which is similar to what has happened in the Chesapeake Bay. This is a sign to me, possibly along with the *Pseudo-nitzschia* data, of increasing silica limitation. Because those species and *Skeletonemia*, the other dominant species out there, have enormous plasticity in terms of their silica requirement and can almost disappear, have no silica test almost, under low silica conditions. We have not yet reached the point where it would slip over into being predominated by flagellates. But it could, I think.

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<sup>8</sup> Verbatim phonetics from tape recording. Please verify the term.

**Amy Parker:** So given this long discussion (which was very interesting, by the way; in fact, I think it is some of the most interesting data I have seen on hypoxia in ages), Jim suggested that we could go forward possibly with production of P and get us to the place that we would like to be where we reduce the size of the hypoxic zone. But given the uncertainty about where this productivity is occurring and how it moves to drive the hypoxic zone, would that really be a good course of action for managing the system?

**Ammerman:** Well, that is your job, not mine. Some people have used this raising of the P issue to question whether N control is needed here. Clearly that is a silly question, from my point of view. But clearly you deal with P at the same time whenever possible. Whether you make separate investments to control P alone is a more difficult question that I am not prepared to answer. But any time you can deal with both nutrients at once, you obviously should. As I said, virtually every other system has some similarity to this. This is what is clear. Certainly N should remain the priority. Again, further investments in P control I am not sure are warranted. But certainly when you can deal with both, you ought to.

**Boesch:** I also think there is some experience elsewhere, including my friend next to me here, that suggests that there is a potential risk in doing that—of controlling P and not N. If you looked at my argument about the Southwest Pass plume sitting over deep water, you could make the argument that you want to maximize the production there. It is P-limited in source inshore. That was what was happening in the Neuse River when they controlled it for P. It produced the blue-green plumes in the upper end of the estuary, but then exported more N. The hypoxia area actually grew. So if the goal is to reduce the volume of hypoxia, you may actually get the opposite results by controlling just P. You may actually export more N and therefore extend the size of the hypoxic zone.

**Paerl:** I do not think any of us are advocating a single nutrient reduction and letting the other one go. In fact, I would point out that we can learn a lot from European waters such as the Baltic, the Adriatic, and the North Sea. All of those have been most successful by really going after both nutrients.

**Boesch:** Yes.

**Parker:** And if there is confusion out there, the Task Force's current plan is to do a nutrient strategy.

**Paerl:** I just want to underscore that none of us are saying forget about N because that is not the topic of this discussion.

**Boesch:** That is the plan? You have come to that conclusion already? I mean seriously.

**Parker:** We have already come to that conclusion.

**Greene:** We have a number of questions that came from the audience, and I think we have sufficiently addressed all of them. I do not know, Alan, how we are doing on time here? Does

someone feel like his or her questions were not sufficiently addressed? Do you want to open it up for a few minutes, or do we need to be moving on?

Lewitus: No, I think we are okay. We are scheduled for a break right now, but if there are any burning questions, I think we can handle a couple. Can you use the mic, Peter?

**Peter Eldridge:** This is Peter Eldridge, EPA. I do have to admit to a little confusion on one of the issues in the interchange between Ron Benner and I could not quite catch who the other person was regarding the export of DOM from the marsh area. There seems to be that scaling between the export rate and the lability of that organic material that will result, which will let us determine exactly how much is going out. I wanted to direct the question to Ron, who is on the wrong table I suspect, for that. I know you did that work on the Oasis Bay, where you actually did look at the lability issues that are in some of the marshes—whether that was alone or with one of your graduate students. I was wondering if you have more information on that that would help us determine what the lability issues are so that we can come up with the flushing rate when you know how much might actually end up being exported out into the hypoxic zone.

**Benner:** Peter, I really do not have direct information on the lability, but I will address the question by an indirect approach. One of the things that I work on is the contributions of terrestrial materials to the oceans. When we look out in the open Gulf of Mexico, we see very little of the terrestrial material that is discharged annually to the Gulf out in the open Gulf. The only conclusion you can reach from that is that it gets re-mineralized somewhere along the line. It appears that that re-mineralization is occurring in the ocean margins. Part of it is through exposure to sunlight and photochemical transformations, which are very important in softening up material that might be relatively resistant to microbial degradation. But photochemical transformations, first of all, mineralize and re-mineralize nutrients. But they also make the remaining material, the organic material, much more available to the microbial community.

**Mary Booth:** I have two quick questions. First, is there any evidence for a late limitation of C fixation in the plume itself when it is really turbid? The second question is: I think that evidence that was presented earlier on hysteresis effect where reductions in nitrate loading might not actually be that efficacious if there is a lot of recycling. It is very interesting, but there is this correlation, a pretty tight correlation, between runoff and the volume of N delivered to the Gulf and the size of the hypoxic zone. That Gene Turner paper that came out this year does show that nice correlation. So how do you reconcile that relationship? The nutrient has to come originally from somewhere. And it seems to me that yes, there has to be some re-mineralization and recycling, but with every such transformation there is going to be an increasing probability of those nutrients disappearing below the zone where new C can be fixed. I am just wondering how strong the evidence is for re-mineralization really kind of prolonging the hypoxic effect even with major reductions of the nutrient loading at the source.

**Lohrenz:** I will take the late limitation part of that question. There is pretty clear evidence that in the high turbidity in the river plume itself and in the very low salinity regions of the

plume, the attenuation of light is such that productivity is constrained. It is a very abrupt transition, probably associated largely with the deposition of sediment in the plume as it opens up into the open Gulf. You see a very good relationship with the increased light penetration and depletion of nutrients and elevation of productivity and biomass. I think I will let somebody else address the other part of that question.

**Gardner:** Again it seems reasonable to me, if you have the nutrients coming in, they get recycled in the top. They are not causing hypoxia at that stage. But at some stage they are going to reach the lower depths, and that is where you will get your correlation with hypoxia, I think. So, what the recycling does is keep them in the water, but ultimately they will drop out in some form and cause a sediment oxygen demand.

**Greene:** We are going to have to cut it off for now. We are behind, so let us meet back at 2:45 for the next session. I need a break! Let us thank the coauthors, the author, and the panelists.

*[Applause]*

## Transcript of Panelists' Discussion and Q & A and Audience Q & A

### **Session 5: Causes of Hypoxia IV: Benthic Processes Influencing Oxygen and Nutrient Dynamics**

**Authors:** John Morse, Jeff Cornwell, Peter Eldridge, Beth Mullenbach, Gil Rowe  
**Panelists:** Reide Corbett, Brian Fry, Mike Kemp

**Rob Magnien:** The panelists for this session are Reide Corbett, Brian Fry, and Mike Murrell, and the chair of the panel is Mike Kemp. I would like to reiterate that through our science reassessment for the Gulf of Mexico, we want to figure out how we can narrow uncertainties in our knowledge and come to consensus on issues. We want to probe questions and turn over important rocks. We really want to identify how much we know and understand after the 6 or 7 years since the last integrated assessment.

**Mike Kemp:** It strikes me that the key questions that have already been posed are still on the table. I am accustomed to the situation in the Chesapeake Bay, where there are good data on benthic processes and the relationship to water column activities. What fraction of the total, integrated ecosystem respiration is associated with the benthos? What fraction of the total ammonium regeneration or phosphate regeneration is associated with the benthos? These questions have come up indirectly in the discussions all day.

**Peter Eldridge:** I will start with the oxygen because that is the only one we can really answer at this point. We know what the model rates of ammonia flux, dissolved inorganic carbon (DIC) flux, and oxygen flux are, but we do not know what the water column rates are very well. I was looking at the huge spectrum of values that Quay Dortch and others had found. In one of our graphics, we were able to show that at the initiation of hypoxia, close to 60 percent of the oxygen metabolism was occurring in the sediments. Later, in the midst of hypoxia, we were getting closer to 35 to 40 percent from the bottom.

Part of the reason the oxygen demand is low is that there is very little oxygen concentration and, therefore, very little oxygen to be taken up. That is more so in the sediments than in the water column. Sediments can be very important at the initiation of the bloom, because of the high oxygen demand by the sediments, and important at the end of the bloom, because of the inventory of reduced products in the sediments. This produces a lag effect. We did produce graphics showing the amount of ammonia and nitrate fluxes in and out of the sediment, but I do not have any comparable measurements at this time for the water column below the pycnocline.

**Kemp:** I had forgotten about this until after I arrived here, but we did an analysis of the relative roles of benthic versus plankton processes, as well as physical versus biological processes, in regulating the seasonal variation in respiration. We had a figure that related the relative contribution of the benthic processes to the overall bottom layer respiration (to the height of the bottom layer) because that is really the key master variable. I dug up some early '90s data on water column respiration from Gene Turner, and then benthic

respiration from Robert Tool. The data from this region fit the overall relationship, but I cannot remember exactly what they were.

**John Morse:** One important thing to consider is to try to divide this work into three. The sediments in this work are very different. The mobile muds are in one area, and then you get to the far west, where you can hardly get a good core; it is almost non-depositional. So the answer to your question may depend on where you are. In much of this area, although the bottom is an important contributor, the actual rates that you calculate are not exceptionally high. Delaware has really high bottom rates, while *these* are not particularly different.

Some time ago, a student of mine and I did a study of sulfate reduction rates across the entire gulf transects shelf of the deep. The sulfate reduction is often a major chunk of the organic matter oxidation that goes on. We found that 80 percent of it for the entire Gulf of Mexico occurred up there in this Mississippi Bight area. It is very concentrated in that area's fluid mud. After that is the shelf, and that is not an exceptional zone at all.

**Kemp:** One big difference is that the height of the bottom hypoxic layer in Delaware is large compared to the height of the bottom hypoxic layer here in the Gulf of Mexico. Therefore, the potential role of benthic processes—even though the absolute values may be lower—is still potentially very large.

**Eldridge:** Yesterday Steve DiMarco showed the huge variations in the depth of the pycnocline, even with just 20 meters of water. The proportion of uptake by benthos in the deep water below the pycnocline water can vary depending on what the physics are in those situations. So this is a more idealized situation at buoy C-6.

**Tom Bianchi:** I was very surprised by the limited amount of benthic work that has been done in this region, as compared to the Chesapeake and Baltic. In the tidal effect region in the lower river, as opposed to the shelf, you get huge storage events. You get a buildup in pore water during these storage phases, and we really do not know how much that affects these ratios.

**Kemp:** Is sulfide involved?

**Bianchi:** We have not looked at sulfide, but certainly some of the reduced metals and ammonium. That is another area that we need to address. Sulfur cycling, nitrogen (N) cycling, and even some of the phosphorus (P) work have been very sparse relative to what most people who review our proposals seem to think is out there. They look at this as a region that has had a lot of research done.

**Morse:** You almost never see detectable levels of dissolved sulfide in these pore waters because there is so much iron. It keeps the sulfide down, it helps preserve it, and it is in this area where you get rapid deposition. The highest sulfide retention I have ever seen is where you have mainly oxic overlying waters. Twenty-five percent of the sulfide

produced is stored, but it is stored in the solid phase. Even though you do not see much dissolved, you do see good concentrations of solid-phase iron sulfide.

**Reide Corbett:** I would like to follow up on this idea of mobile muds. Especially near Southwest Pass, this system is not steady state by any means. Your model is simulating down to about 20 centimeters. I would suggest that you are re-suspending and redistributing much deeper than that, at least on a seasonal basis. I think the idea that you cannot move material farther away from Southwest Pass is not the case, and that has been demonstrated.

**Brian Fry:** There is also a mobile mud layer (*Reide Corbett interjects: mud belt*) downstream of the Atchafalaya River and the estuary that might be interesting for remineralization.

**Corbett:** How does that play into the modeling efforts that you are doing? I know that you have an advective term within both your pore water and your sediment transport. The sediment is an advective term for accumulation where your pore water ... (I am not sure if that is pore water in or pore water out.) How and on what time scales can you incorporate redistribution of much of the seafloor on at least seasonal time scales, not even including hurricanes or other major storms?

**Morse:** I do not know of anybody that has tried to do diagenetic modeling on mobile muds. What we do see in our model is that the pore water systems are sufficiently reactive so that on the time scale that you are dealing with, they can reset the pore water. The bigger problem is with the solid phase. When we run these models, it is important to remember that typically there are 10 to 20 thousand times more in the solid phase than in the dissolved phase. So these diagenetic models that model primarily off the concentrations in the dissolved phase can reset themselves fairly quickly. In our experiments we found that this could be a few days to less than a month. It looks like the diagenetic model works, but it is false in the sense that if you re-suspend all that, you get flat profiles and then start all over. I think that is a new place to go, and it will require cross communications. Diagenetic modelers have since the beginning, hated an 'unsteady state' of any sort.

**Corbett:** You have shown that before hypoxia sets in, maybe 60 percent of the oxygen consumption is benthic. When the seafloor is remobilized—especially around Southwest Pass during these winter frontal passages (December through May)—the sediment has been documented moving to the west. This is the time prior to hypoxia setting in. How important do you think that is in potentially enhancing that 60 percent to even more?

**Eldridge:** In addition to the mobile muds, we have floc layers that may be very important. They are part of the re-suspension process. They may be made of colloidal and other suspended materials, or they may be very reactive layers that are not considered when we deal with just bottom water and sediment diagenesis.

There is that benthic boundary layer area that has not been looked at in detail. That is another gap we would like to fill. Some of those floc layers are only a few centimeters, and at other times they may be a meter. That can make a huge difference in the metabolisms that occur near the bottom. Drs. Hetland and DiMarco have talked about this.

**Bianchi:** Some of the earlier discussions about terrestrial organic matter breakdown come into play with mobile muds. These muds are dynamic and possibly take very refractory material—whether it is coming from marshes or from the river—and combine it with the very productive phytoplankton inputs. You can get something generally referred to as co-metabolism, which we have looked at and appears to be happening out there. My student who is in the audience, Laura Wysaki, will be publishing something on this. Not only do these muds move around, but they also take some of these terrestrial signatures. Ron talked about water column issues. I am talking about what is coming in with the sediments. And it may enhance their ability to be used. If you take very refractory things and mix them with labile components, you can possibly enhance oxygen drawdown. Mobile muds as it relates to terrestrial sources is something that hasn't been studied very well.

**Corbett:** Another thing that varies quite widely in the rivers is sediment supply. Regardless of terrestrial, refractory, whatever, and in the models that you have, you do have the ability to adjust the accumulation rate. So how would benthic re-mineralization change as a function of sediment supply?

**Eldridge:** If you have a lot of terrestrial material that is relatively unreactive, refractory in nature, you may end up with a high percent organic matter but not necessarily with high fluxes of oxygen, high oxygen demand, or other diagenetic reactions. You end up with a lot of burial in the sediments. If, however, the material coming up the river is more reactive, then you can get very high rates of oxygen demand in those sediments. Generally, we have seen terrestrial particulate material coming down the river that is not very reactive, but there may be episodes that are reactive, that we do not know about.

**Corbett:** What if you decrease supply so you potentially have a longer residence time of the material near the surface? Could you potentially increase oxygen demand?

**Morse:** There are a number of parameters being used and a wealth of papers out by good people on factors concerning organic matter preservation and sediments. Since the early '70s, people have looked at the percent of organic carbon (C) versus sedimentation rate from the standpoint that the longer things are exposed to the oxygen, the more completely the organic matter will be burned up.

Complicating that, in hypoxic areas, there has been a heated debate for decades about whether organic matter can be created as far or as quickly anaerobically as aerobically. The experiments show that if you take fresh phytoplankton, you get the same rate under anaerobic and aerobic conditions. If you take aged organic matter, the results are very different. In our models, for organic matter concentration we put one fraction called

“labile” and another that is “non-labile.” We probably should change that for hypoxic conditions because things that are labile under oxic conditions (if the organic matter is somewhat worked over) becomes fairly non-labile under anoxic conditions. If most of the organic matter is fresh plankton bloom that has settled to the bottom, it would not be much different whether it is being worked on oxidically at the surface or buried in the sediment and being worked on by fluid muds or sulfate reducers. On the other hand, if it is old organic matter re-suspended from terrestrial organic matter, it may make a very large difference.

**Bianchi:** Another factor is the macro-faunal effect, which is long-term change due to damming and reduced sediment loads over time. I am not aware of a lot of preliminary macro-faunal data to compare it to earlier from the '20s or '30s. As we move out from the inner shelf toward the can, we are starting to see some late-stage, deeper, burrowing kinds of organisms, not just these shallow or selected species. I would speculate that some of that may be a result of this reduced loading because of the long-term sedimentation rates we are actually getting from these cast-in cores and some of these other deeper cores. If you look at the timing of when you started to see the reduced loads, you know part of the story.

Another interesting component is actually bringing macro-fauna back. Baltic researchers have looked at these flashy effects of things coming in and out. I think we are in a gradual change now of bringing more organisms back. Before, we said that there are not a lot of organisms out there, primarily because of the oxygen. I would argue that a good deal of it is also because benthic organisms find it hard to survive in mobile mud. Also, they find it difficult to keep up with high sedimentation rates, unlike these fast-burrowing *Calianaci* species<sup>9</sup>. This may be in transition over decadal periods now.

**Fry:** Is the carryover of deposited organic matter from one year to the next a factor? Or is the matter labile enough over a season that it is mostly degraded?

**Eldridge:** That certainly can make a large difference. On coastal shelves, sediments typically are not disturbed much (unless you get a hurricane) and are slowly buried. If irrigation processes change over time, this organic matter can be remobilized even if they don't change in the depth profile and are exposed to more oxygen or other electron receptors that are more energetic than, say, sulfide. Whether an infaunal organism survives or not, can make a big difference on how materials buried 10 or 20 centimeters, into the sediment may become oxidized.

**Kemp:** Sediment 10 to 20 centimeters deep is pretty old stuff...it depends on where you are. That is the answer to all of these questions.

**Eldridge:** It does depend on where you are. There are two things going on here—irrigation processes and mixing process by the infaunal mixers. Material is being mixed

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<sup>9</sup> Please verify term.

around and, depending on the biology, material can be mixed up or not. Those mixing rates make a big difference in terms of sediment movement.

**Morse:** What benthic critters are living there if you do not have any of the larger bioturbators that really mix the organic matter down? You are restricted to biofauna and small nematodes, and so forth. It becomes a different game than you would have in normal oxic sediments.

**Bianchi:** As you move from sites in the near deposition (assuming it was deposited by the river) area right outside Southwest Pass, and you move west to Terrebonne and look within the top 10 centimeters where you have this ephemeral or mobile mud material, our pigment decay rates indicate that stuff is pretty much gone within a year. As you move west, you start to pick up more of a degraded signal. So, that zone is original parent material unless you get new impulses.

**Eldridge:** To clarify, the advection rate that we are using in depositional rate calculations is approximately 0.5 centimeter per year. The mixing of the bioturbators causes a lot more exposure to deeper material.

**Kemp:** Except you do not have those bioturbators there.

**Eldridge:** You do not necessarily have them. If you look at Diaz and Rosenthal's work and some of the work of Roberto Marinelli, there is a big difference between hypoxia and the oxygen concentrations that those organisms experience. Although they would be dead if the water is anoxic, some can still live and function under hypoxia conditions.

**Kemp:** Tom, can you comment on that? Because I am hearing a different story.

**Bianchi:** I disagree with that to a certain extent. I agree that the whole anoxic/hypoxic effect on a benthic organism is fairly straightforward. In the zone where these mobile muds are moving around, I do not think it is just the oxygen concentrations that is determining who is there. In all the years we have been collecting box cores, I think we have seen only very small polychaetes.

**Kemp:** Is there any way to tease out the relative roles of the hypoxia versus the mobile mud environment?

**Bianchi:** That is another area that has not been extensively worked up.

**Mike Murrell:** I do not have a question about the diagenetic model mentioned earlier. Regarding the slide about the sensitivity analysis, I am concerned about the usefulness of that as a tool during hypoxic conditions. It seems like there are a lot of variables that are not well constrained. Do those measurements just need to be made many times, or can you tweak the model to get around some of those constraints and uncertainties?

**Morse:** We were surprised by how robust the model was under normal oxic conditions and how sensitive it became under the hypoxic conditions. Think of the hypoxic

conditions as meta-stable conditions: the system wants to flop either monoxic or back to oxix. It does not take much push either way. One of the most sensitive parameters is the bio-irrigation rate. This really affects the flux rate and how much organic matter is oxidized and how much ammonia is generated. The question becomes: How do you get those bio-irrigation rates under hypoxic conditions? I think the answer is that it is extremely difficult. A number of years ago, Gil Rowe and I published a paper on the topic. We designed and built a chemostat, where we could keep oxygen constant in the benthic chamber. When you get to low oxygen, it changes very rapidly, so every time you perturb the system, you get a bad number. You almost have to go to something fairly exotic like divers putting chambers down that are chemostats so you can maintain the oxygen at that hypoxic level and measure fluxes under that overlying condition. Then take your cores and measure the parameters and force it to go together. Because the pieces have to fit, you can find the bio-irrigation rate. It is very difficult. Maybe some of *Jurgensen's*<sup>10</sup> people over at Max Planck, who seem to be able to do everything, have tried it.

**Murrell:** So that would be able to get that kind of measurement from one site? I am thinking that this is a predictive tool or a tool to extrapolate.

**Eldridge:** We are getting educated here today about mobile muds. Sounds like many bets are off when you start looking at these mobile muds. There may have to be a program to look specifically at how these sediments react with movement of mobile muds under hypoxic, anoxic, and normoxic conditions.

**Kemp:** Peter and John, concerning the use of this vertically fine-scale diagenetic model in the context of this sort of regional-scale question...there are other ways to get similar calculations, such as the *DOM Datoros*<sup>11</sup> scheme for using a two-layer model and identifying parameters for a lot of the processes. Alternatively, we can get to the point where there is a biogeochemical model coupled to the physical-circulation transport-model processes in the overlying water... We might as well try to incorporate sedimentology as well because it seems critical... What type of sediment-diagenetic model do you think would be appropriate? Do you think this is the right track?

**Eldridge:** We will have to go back to earlier in the day. The model you use is dependent on the question you have. For example, George Jackson and I developed an inverse model that was essentially a diagenetic model for the Santa Monica Basin. Even though we were able to use various electron acceptors quite well, if we started ignoring the physics with our biological models, we had problems. There are instances in which they can improve your understanding of the system. In highly irrigated muds the physics that we use in typical diagenetic models do not work, so you are forced to do another type of modeling. That is where you would go to an inverse model or one of the other tutorial types of models. In this circumstance the model seems to work fairly well, even if it is because the pore waters are being reset rather rapidly or we have just happened not to hit

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<sup>10</sup> Please verify author.

<sup>11</sup> Or "Don Detorro's". Please verify term or author.

muds that are fluidized. We may find that when we get into those circumstances, our model is not going to be that useful.

**Kemp:** What about the computational burden? Has this been done at the scale of a 500-meter by 500-meter grid or over a 150-kilometer by 50-kilometer region?

**Eldridge:** We found a really good way of dealing with the computational question. We give it the answer before we run the model. (*Kemp interjects: Run it backward.*) You take the result from a different site that is similar, stuff it into the model, and it finds the solution for your site. Running the model used to take 4 hours. Now we can run the model in a minute or so by providing it with initial conditions that are near to the solution. You can take these results from almost any other model (e.g., Laguna Madre), put in that vector, and it will reduce the computational time significantly.

**Fry:** I am curious about how you would check your models. You brought up skepticism about the patchiness and difficulty of the fieldwork. Are there areas maybe to the west and the two areas of intense hypoxia, of sediments that have more stability, where you would have some geochemical parameters that would be relatively robust, allowing you to check your models and large-scale grid computations?

**Morse:** In the intermediate area, our two sites were both the most exciting and worst places to sample. We were somewhat pleased that we were able to predict site 3 (data, measurements) from site 2 by adjusting the amount of organic matter and so forth. But I think you gain confidence as to what you need by going to other sites and working in the fluidized mud area. Areas of rapid accumulation toward the river are the more difficult ones to do. Our modeling goals were more general, but if our model is aimed only at predicting oxygen and ammonia fluxes, it may be possible to significantly simplify it. One thing we can do with our sensitivity test is to identify things that we really do not have to measure. If we really are not interested in what is happening to the manganese system, we can take it out of the model. This model is very modularized that way for different conditions. When we built this model, we were not necessarily thinking of optimizing it with the objectives of this meeting but more generally proving the model.

**Fry:** If we thought the hypoxic areas were particularly severe one year, would you be able to predict that for a season? Could you say, “Yes, we predicted that from the model and went out on a cruise and found that”?

**Morse:** With this model we can easily increase the organic matter and labile organic matter flux, and change the length of time or severity of the hypoxia.

**Eldridge:** You have to make assumptions about how infauna might respond. You can run the model accepting those assumptions. It is important to have other types of data that you use along with your modeling to validate the model and make sure you understand it, such as benthic fluxes and other types of things. If you can independently get the model to successfully predict a sulfate reduction rate, for instance, you can feel good about it. I expect it would not do that in a fluidized mud situation.

**Brian Reide:** Yesterday I heard that benthic respiration accounted for one-third of oxygen consumption versus water column respiration, which accounted for two-thirds of oxygen consumption. Today it seems like you are suggesting that in your study area, 60 percent was benthic prior to hypoxia and then maybe it moved toward one-third/two-thirds. If you move into an area that did have more of a dynamic system as far as re-suspension, mixing, re-distribution of sediments, would you expect to see an increase in the oxygen consumption associated with the benthic environment? At what scale would you expect that increase, and how might you take your model to try and evaluate that? I think that to make this model worthwhile on the shelf, it needs to be expanded into these areas that are a little more dynamic.

**Morse:** I think we would need to obtain two pieces of hard-to-get data: how much material is re-suspended for how long, and at what depth interval in the sediment. Then, during that period, what is the oxygen demand of the re-suspended material that you can get experimentally? We could go out and get a core in one of your areas, shake up a gram per liter, and see how much the oxygen consumption goes up. We do not have that number at all. I cannot find it from anybody, but it would seem to me that it would be readily attainable. You have to say how much re-suspension and for how long. That varies from year to year with physical oceanography, weather, and the like. We do not have that number, and you probably need to get it from different areas of the hypoxic region where you think re-suspension may be important. You need to know what you are going to re-suspend and then do an experiment and see how it reacts with the water.

**Eldridge:** As an example of how important that might be, in some of the prior talks you saw some indications of how long hypoxia takes to reestablish itself—maybe just days, not months or seasons. The only mechanism by which I could see that happen is that re-suspension is increasing metabolism considerably.

**Corbett:** Is the back-of-the-envelope type of calculation good enough for the amount of material re-suspended on a seasonal basis? I imagine we have some information on the material that is being re-suspended.

**Morse:** Well, I think you need to do some experiments. Like the pyrite in the sediment. Forty percent of it is oxidized in the first few hours, and the next 60 percent takes 3 months. You have to get some sediment and do some re-suspension oxygen consumption experiments. The rate of oxygen consumption will change because you will find that the acid-volatile sulfites will go in the first hour and then it will slow down. You will get an initial pulse of very reactive things, and then you will get slower reactions asymptoting out. It is very difficult a priori to draw that curve for a given sediment, but it is an easy experiment to do. And then you have that equation.

**Fry:** What is the relative importance of benthic and epibenthic primary production and oxygen production? How do these processes influence the duration and extent of the hypoxic zone? Does anybody work on this? We found some data in a paper from Quay Dortch and a little bit of information from Gil Rowe, but not much.

**Eldridge:** I have seen a lot of work done on that on the Atlantic coast by Jim Eckbend and Roberto Marinelli. They found at times that the quantity of benthic photosynthesis at the benthic-water interface could be similar to what they are getting in the photic zone by phytoplankton. I have no idea what it is like down here, but it obviously can have huge effects on the benthic processes. (*Morse: Depends on light penetration, too.*) There is light penetration data available.

**Murrell:** We were out last week. A large area of the western shelf is euphotic on the bottom, and the water column is clear enough to support ... production on the bottom. I do not think anybody has systematically looked at the light distribution on the bottom.

**Eldridge:** To parameterize these models reasonably, you have to include that.

**Kemp:** In the big hypoxic zone does light reach the bottom?

**Murrell:** It did last week. *April 17-21; 2006*

**Bianchi:** I would think that in this particular region (west, Atchafalaya-west), it is probably not very significant.

**Magnien:** Let us move the questioning to the steering committee. I do have one question that spans a couple of the sessions today. In the water column presentation, we discussed regeneration of the hypoxic zone during summer, when there is a considerable amount of nitrate in bottom waters relative to surface waters. I am not sure I understand where the nitrate is coming from. Is it being advected from one of the plumes, and we are just seeing the vertical pattern because the surface water concentrations are being drawn down by producers? Or is it being regenerated in the water column or sediment and being nitrified? That does not seem to be supported by the benthic data I saw. In fact, nitrate is being drawn in and I did not see much ammonium coming out, so where is that nitrate maxima in bottom waters during summer? Does anyone have any insight? Is it possible it is somehow being derived in the lower part of the water column or benthic interface?

**Eldridge:** With the benthos, even though you saw nitrate being taken up, the ammonia going out is very quickly nitrified.

**Magnien:** I did not see much ammonia going out, did I? (*Crosstalk All: Yes*) Okay, then I missed that. That is what I would have expected. (*Eldridge: There are lateral sources also.*)

**Kemp:** Nitrate was not going in though. That was a pool of nitrate that was stuck in the lower layer. It may have been from a lateral source, but it was not produced. If it was from the plume, it got there through a funny mechanism because it was overlying that layer. No nitrate/low nitrate in your presentation seemed anomalous.

**Magnien:** But that is a pattern—at least the nitrate maxima in bottom waters.

**Eldridge:** You tend to have more nitrate in the bottom waters because most of the photosynthesis is occurring in the near surface waters.

**Magnien:** As we discussed earlier, that is important because that has the potential in a dynamic system like this to fuel continued production during the summer.

**Eldridge:** There is a source of ammonia from the benthos that is nitrified. Then you have lateral sources of ammonia coming in from off the shelf or elsewhere, so there are a lot of different sources of nitrate. But it is utilized, so you do not see much of it in surface water because of all the ongoing photosynthesis there.

**Rick Greene:** I want to follow up on a couple of things we have discussed over the past two days about the memory of the system. I think Reide hit on that a little bit. When you did the time series analyses of your model output, most of the fluxes and the parameters were very responsive to changes in the oxygen conditions. I do not remember exactly what the time scale of that was, but I presume that it was days to weeks. I wonder if you could comment again on the suggestion that the sediments have memory that may persist from year to year. I understand that following a hurricane or major storm, hypoxic conditions can reestablish themselves very quickly. Are there any data over the annual cycle? What is your best guesstimate?

**Morse:** You have to think of the question in two ways. If you talk about pore waters, most of the pore water changes and responses can occur in a few days to a couple of weeks. They are very responsive. If you look at the solid-phase responses, those are months to years, depending on what is there (and if we are not having re-suspension). That is because so much more of the carbon is solid carbon rather than dissolved organic carbon, and so much more of the sulfur is solid sulfide, not dissolved sulfide, and so on. They are there at very low concentrations, but they are a very small part of the pool. Diagenetic models are almost all largely based on pore water profiles, which are very quick to respond, but they are not very sensitive to solid-phase concentrations. The activity of a solid phase is the same whether there is a little bit or a lot of it, so the models do not handle that well. In general, the solid phases have a much longer time frame—months to years—and it may be true that the models that are driven largely by controlling the solid-phase concentrations off the pore water fluxes have a soft spot.

**Eldridge:** Grain size is very important. Sand is hell to work with. Pore waters come rapidly out of sand. The effect of pressure waves (pumping) caused by waves going up and down can essentially evacuate the pore waters from the upper layers of the sediments rapidly. That is not going to happen if you have sand/clay mixtures, so the make-up of those sediments can be very important for sediment memory—how that reduced inventory is going to leak out of the sediments after a hypoxic event. Would you agree?

**Jeff Cornwell:** Based on lots of sulfite<sup>12</sup> reduction data, ammonium flux data, we do not really agree. I think the perception in the Chesapeake Bay is that most of what we have for the spring bloom is burned off that summer. In terms of a memory, the main driving

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<sup>12</sup> Could be “sulfide”

force is going to be labile organic matter that is consumed rapidly. At greater depth, you do see modest rates of metabolism, but in terms of the bulk rate of metabolism on an areal basis, most of that is consumed rapidly. We have warm bottom water temperatures, and the memory effect is pretty small. Now if you are looking at other things in the sediments, it might be different. It can take quite a while to change the nature of pyrite and other parameters. You know the microbial side of this: if it is labile, it is going to be used fast. Is that the same perception you have, Mike? We see inter-annual variability in fluxes based on how much nitrate comes into the Chesapeake Bay and how much organic matter is produced in the spring bloom. Nothing is smoothing that out in any way from year to year. It is very responsive.

**Morse:** Do you think that is happening in this floc layer rather than really getting into the sediments?

**Cornwell:** No question. We have extremely high rates in the top few millimeters that are driving all that.

**Bianchi:** Reide, do you think that the movement of the mobile muds exposes other, deeper areas after you get burn-off within? If you think of this system as potentially having a drawdown of oxygen and its being a long-term repository of C accumulation, regardless of what has been burned off and transported, if you peel layers off it, you are still exposing new sources of C that were buried.

**Corbett:** In most years, you are still accumulating sediments on a much longer time scale. You are depositing much more than you are accumulating. On annual time scales, I think you are still accumulating sediments and not necessarily exposing the older sediments, except during major storms. (*Bianchi interjects: That is what I am getting at.*) You do have massive redistribution of sediments that does expose a lot of that old material. But that said, I think a lot of what is coming down the river is not necessarily labile, so what is important is the re-suspension and redistribution on seasonal time scales. This leads the memory effect because you are re-exposing some of that less-labile material back into an oxygenated environment, allowing for that memory effect to start breaking down this less-labile material.

**Alan Lewitus:** I am interested in the relative influences of the physical turbulence and re-suspension versus bioturbation or bio-irrigation. Could you just address that? It seems like a major turbulent event like a storm would really overwhelm any of the biological disturbances, so are you talking about special circumstances where bioturbation would be important? Or maybe address that seasonally as well. Talk about the relative importance of these two in driving hypoxia.

**Bianchi:** If you are talking about just within the hypoxic region, I do not think the microfauna play a big role. But there are other areas—if you start going mid-shelf or even out to the slope toward the canyon—where you start picking up more significant communities. But that is not relevant to what you are asking. For within the hypoxic

zone, I do not see it as a big factor. I do not know how you may want to respond to that with your model.

**Eldridge:** The rates we used for bioturbation were relatively low. We tried lots of different irrigation rates and found that the model was very sensitive to them. In John's conversations with Gil Rowe, he suggested that the infauna, at least under hypoxia situations, never actually go away. So, even modest rates of bio-irrigation would probably have significant impact on the sediments. Now all bets may be off when you get into fluidized mud, which we have not really dealt with in our modeling. But at least in the more consolidated sediments, that would seem to be the case.

**Morse:** I think Gil Rowe told me that he and Don Harper discussed this. They would agree with Tom that it is not a very highly developed, rich benthic community. Out there it is more what we called "the pioneering community" in our model—small organisms, probably not living very deep, but still influential. When we get to the re-suspension story, we need a lot more data on how much material is getting suspended and how often in relation to the hypoxic events. Rob Hetland thinks that they are certainly connected. There is a paucity of data here. There are three regions. In one region it may be important, while in another region it may not. One thing I have learned is that it is dangerous to start making generalities about the hypoxia here because the area is very heterogeneous.

**Amy Parker:** How much of the sediments are really mobilized? How much unconsolidated mud is there because that really speaks to the reactivity of the biogeochemical processes in the mud itself. Your maps showed extensive areas of it. How deep is that—a centimeter or 2, or 10 to 20? Are we talking about the flocculated layer as well as the mobilized mud sediments? That is the biggest question here.

**Corbett:** The data that we have are primarily around Southwest Pass and within 100 kilometers west of Southwest Pass. That mobile mud layer ranges from to very little in fairly shallow water to upwards of 20 to 30 centimeters. Within 30 kilometers of Southwest Pass, you have a major deposition area associated with the river that falls off pretty quickly away from that 30-kilometer point. Even 100 kilometers west of Southwest Pass, you still have at least 5 to 10 centimeters of a mobile unit. In this case, "mobile" means an ephemeral, seasonal-type storage.

**Parker:** What are the particle sizes of those mobile muds? Are they fine, silty particles or a silt/sand mix? That makes a big difference in what kind of mechanical activity occurs to break down those recalcitrant C compounds coming out of the river systems. That is a huge unknown if we are looking at the effects on hypoxia.

**Corbett:** I would agree. It is all very fine silt.

**Eldridge:** You are hitting on an area of interest that stems back to some of the early work on the Amazon River's fluid muds. Bob Aller and other people are working in other

river-dominated margins like Papua, New Guinea, and other places like that. The biogeochemistry of these muds is a big question.

**Parker:** If we are looking at oxygen consumption, the biogeochemical cycling offers a huge area of reactivity that could cause an enormous oxygen demand from just those ephemeral mobilized muds.

[The floor was opened to questions from the audience.]

**Audience 1:** Is there in situ production of organic matter in the sediment? I do not mean necessarily benthic photo-autotrophy. How about chemoautotrophy? And there is a series of follow-up questions: How are the diagenetic reactions coupled to chemo-autotrophy production such as iron magnesium oxidation, reduction, electron shuttling, and sulfide-oxidation?

**Eldridge:** All those are in there. We used Philip Van Caplan's scheme, which I think was a 1997 paper. It includes all the diagenetic processes in terms of the mineralization of the organic matter, and all the processes—chemo, litho, and autotrophic processes—required to take those reduced products back to their oxidized states.

**Audience 1:** It is a loaded question. We have done a lot of molecular organic geochemistry there and have found significant amounts of chemo-autotrophic biomass.

**Eldridge:** Yes.

**Audience 1:** Significant—oftentimes more than the photo-autotrophic biomass.

**Eldridge:** We would be interested in talking to you because some of the rates we used are from the literature. We did not have any rates that we could use directly from this area.

**Audience 1:** That does not preclude that the chemo-autotrophic reactions can be happening in these fine-grain muds as well. Also, what is the role of chemo-autotrophy and remobilizing/sequestering of C, N, or P in the sediments in the benthic carbon realm?

**Eldridge:** Yes, although chemo-litho-autotrophic processes are not very efficient in terms of producing more biomass unless you get to a vent situation.

**Audience 1:** That is not necessarily true if you have oxidation of sulfides. Using CO<sub>2</sub> you can easily generate it, and there is an enormous amount of remobilized N in there as well. The P is a difficult question, but I think you could generate carbon biomass.

**Eldridge:** We would be interested in seeing your results.

**Don Boesch:** On the issue of fluid mud, I am going to ask the same question I asked the last panel. John, on the slide you showed (I guess it was Reide's) of the fluid mud patch,

its movement was in relatively deep water. How much of that fluid mud gets into the 30-meter hypoxic zone as opposed to being advected down the shelf at deeper depths?

**Corbett:** Just for clarification, I would call it mobile mud, not fluid mud, because it is more of an ephemeral deposit. I think most of this ephemeral deposit probably starts at about the 30-meter contour and moves into much deeper water—80 meters or so. You typically would not get it above the 25-meter isobaths. That said, I think the re-suspension that occurs during the winter months (anywhere from December through May) can move that material farther afield towards the west, potentially off shelf. I am not certain if it gets into less than 30 meters of water. You do not have much long-term accumulation in that depth of water. But I do not see why you could not have it being deposited there for short periods, depending on the wave climate associated with the redistribution that occurs every winter.

**Bianchi:** There is another interesting benthic process that Larry Maher and a few of us are studying. Mike Dagg was talking about it in terms of inputs of additional DOC. There may be some of these muds—not necessarily mobile muds, but fine particles—that make it from the river into these areas that are less than 20 meters. You have more light and potentially more re-suspension and desorption processes occurring. Larry is looking at this sort of shallow zone as an area where some of this fine riverine particle material is stored, churned up, and exposed to light. This is occurring in between the wetlands and the hypoxic zone. There may be another bleed of DOM from these particles. That is another potentially interesting benthic process that really was not mentioned but is in the early stages of study.

**Miguel Goni:** There are a few papers. Reide Allison's 2000 paper, for example, has talked about fluid muds. Gale Kinko's definition of fluid muds is a solution that has more than 10 grams per liter. These are not the same mobile muds. These are actually hyper-thick fluids hugging the bathymetry that move independent of currents because they are gravity-driven. I think Gale has another paper coming out in *Continental Shelf Research* that shows that there is cross-shelf transport of these fluid mud layers that are independent of current because they move along, across isobaths. This was all done on the Atchafalaya shelf, where you have deposition in the inner region and re-suspension, which leads to fluid mud formation (fluid mud, not mobilized mud). Then it is transported downhill. There are other ways of moving materials into the deeper zones where hypoxia occurs that are related to physical processes. I do not think we have talked about this in the past two days.

**Bianchi:** I think that is a good point. It may not make a difference to many of you out there whether it is called mobile mud or fluid mud, but that is the transition you get. You get more mobile muds by the Mississippi, and you get more well-studied, fluidized mud that hug the coast, some of which goes off. I think that is a good point that needs to be raised as you go farther west.

**Magnien:** Tomorrow we are going to spend the whole day talking about modeling approaches. We will try to synthesize all these processes you have heard over the past

couple of days. In the afternoon, we will hear the panel chairs summarize the questions raised in their sessions.

**Transcript of Panelists' Discussion and Q & A and Audience Q & A**

**Session 6: Modeling Applications**

**Authors:** Dubravko Justic, Vic Bierman, Don Scavia, Rob Hetland

**Panelists:** Carl Cerco, Changsheng Chen, Jim Hagy, Russ Kreis, Kyeong Park

**Rick Greene:** If you would take your seats please. I just wanted to make one general comment, because it was not said earlier in the presentation. If you do not know the modeling effort that was conducted during the first national assessment used the Bierman et al. construct that was presented earlier. I would like to just go straight to the panel and ask for any general comments, or if none exist, we will go straight into questions.

**Russell G. Kreis:** The panel would like to thank Dr. Justic, his co-authors, Dr. Hetland, for the fine presentations on modeling. They have also produced a good draft write-up for us to work with. Now as it relates to the load reductions, and this is following onto what Dr. Greene had to say, that doctors Justic, Bierman, Scavia in many regards have shouldered the greatest part of the modeling load for the gulf proper. Dr. Scavia sent his regrets earlier (with his new responsibilities). They have been the primary knowledge base and corporate memory for the gulf modeling. They should be acknowledged for that. We have seen some other advancements in modeling as well. I think we should point out that the entire meeting has talked about a number of models—many of them directed toward the models in the Gulf Hypoxia Action Plan, and we have seen some advancements, particularly I think in Dr. Hetland's work and Steve DiMarco's presentation and others in terms of models. I think both the panel and the co-authors would agree that the model synthesis is in fact the synthesis of all the research, all the monitoring, all the interpretation by many people and that this has all come together in the modeling. And so the intimate linkage between monitoring, research, and modeling should be recognized. With that, and the acknowledgement of some of the real leaders in this area, that we should probably go to the first questions. And I wonder if anybody would like to take the lead on that... Dr. Cerco?

**Carl F. Cerco:** Nobody ever calls me doctor. In virtually all the models we have seen, there has been no coupling at all between processes in the water column and processes in the sediments. For example, one coupling might be a link between primary production and sediment oxygen demand. Another thing I would love to know would be some linkage between processes in the water column and potential denitrification in the bottom sediments. So a two-part question—first, absent these processes, what are the implications for the modeling that has taken place so far? And second, if such a coupling could be accomplished in the future, what might be the implications for future modeling?

**Dubravko Justic:** Well there is coupling—I will let Vic address the simulation program—through the net primary productivity. There is a flux of carbon that is a function of net primary productivity that sinks to the bottom and then fuels hypoxia. So there is the coupling in a very simple way, of course... In my view, models are

approximations of reality. You try to capture essential things, but the coupling is there, at least in my models. Maybe he can comment on what is in his.

**Victor J. Bierman:** Good question Carl. The model that we developed includes mass balance of the state variables only in the water column. It does not mass balance quantities between the water column and sediments. The sediment in our model is an external forcing function. We specify, for example, sediment oxygen demand, and we specify the nutrient fluxes from sediment to water column. So we have interaction, but it is not fully coupled. And of course, we do not have denitrification of sediments. I do not think any of the three models that are out there... We do consider sediments in indirect ways, although not coupled the way Carl suggested they should be, and I concur with that. None of the three models can directly address the question, what is the relative importance of sediment oxygen demand, versus respiration in a water column, with respect to controlling dissolved oxygen (DO) levels in the subpycnocline? Our model, with its externally specified sediment oxygen demand, which was based on data taken by Gil Rowe, computed that, averaged over the zone of hypoxia, that the sediment oxygen demand is about 22 percent of the total oxygen demand (most of the demands in the water column, 22 percent from sediment). But as I said, that is not from a fully coupled model. That is from a model with an externally specified boundary condition.

I do not think we got all the parts of Carl's question. The second part was, what would it take?

**Cerco:** What might be the implications if these processes were dynamically linked in the future? What might be the implication for future models?

**Bierman:** Well, two things. It may improve the accuracy of the models in representing present conditions... The question of lags came up. The sediments are a loaded issue, because (1) how much do they contribute to current DO depletion, but (2) in the event of load reduction, how much memory is there? How much of a lag might there be, if any, between nutrient load reductions and reduction of primary production in the water column, and exertion of biochemical oxygen demand (BOD) on the bottom waters? We cannot address the question of time lag at all, unless we incorporate the sediment compartment and the principle controlling processes.

**Kreis:** So maybe as a follow up to Carl's question. Dr. Justic presented the big scary models; Dr Hetland spoke about them also. There could be 2 questions. Have we enough modeling? Should we stop? Or, what does the future modeling framework look like in terms of reducing uncertainties, being able to answer other questions, should there be an integrated atmospheric hydrodynamic and sediment transport and fate diagenetic model with a water quality model? Do you envision that type of thing as moving forward, or should modeling stop?

**Justic:** As Dr. Cerco pointed out, no, I do not think the modeling should stop. On that chart that I showed, I said I hoped to see before I retire. And I do not see why that cannot be developed. Of course, the choice that has to be made is, out of the models that are out

there, which one do you choose? And I would not like to make a recommendation of how to do it. I always have in my mind... ways to test things in a simpler level. We have very elegant box model simulations that can reveal inherent uncertainties. They go back to the types of data that are collected. And you will simply see that in terms of if you want to calibrate a phytoplankton production model, you are dealing with half situation constants, maximum growth rates, and Michaelis-Menton kinetics. And if those things are not measured at all in the region that you are studying, it is naïve to think that just by moving to three-dimensional models that will somehow resolve those things. They will not. So in my mind, uncertainties that exist at the box model level should be solved before we move to fully coupled three-dimensional models. That does not mean of course that you will wait 20 years, but somehow you need to link those activities so that there is benefit for this larger modeling effort. So yes, that is not *future*, I would call it a present, if you like. Those things are going on. We will see linked models. We will see them not only because we *like* them, but we will see them because the development of sensors and observation systems that will give us data more frequently, more rapidly, would force model development. So the fact that we will have systems that will give us serious data in real time would force real-time model development. And once we have a model with those capabilities, then we will begin to deal with real-time forecasting. So we would not only predict hypoxia once in a year, but also 3 months down the road, or seasonally. There might be interesting questions like what they do with sea nettle in the Chesapeake Bay. If I go to the beach tomorrow, is the bar<sup>13</sup> going to be there or not? So we are talking about a 24- to 72-hour forecast. Maybe it would be useful for oil people. Maybe it would be useful for fisherman. You do not go and try to catch something in the center of the hypoxia patch. So yes, we will see those things no matter what we decide. They will come.

**Bierman:** When you ask a modeler if there should be more models, of course there should be more models, for many reasons. There are two purposes for using models. One should be to gain understanding of how a system works—environmental processes. I firmly believe that unless you first use a model to understand a system, you have no business using it for management purposes. This is because you do not fully understand all the assumptions, the complications, the potential pitfalls, and you do not know what caveats to put on it. Second, as expensive as models can seem to be, especially the mega-models, the scary models, they are still roughly an order of magnitude cheaper than acquiring data. If you are going to spend money to acquire data, it seems to me to be extremely cost-effective to conduct the modeling.

I think Dr. Justic did an excellent job of making the case in his presentation for the benefits to be gained by multiple, parallel, simultaneous modeling efforts. And I do agree, by the way. I do not think it is a disagreement with Dr. Justic. But I do not think it should be linear. I think that there should be multiple parallel research pathways, because I think that there can be great synergy by interactions and collaborations among modeling investigators who are pursuing models with different spatial-temporal scales, different levels of process resolution, and different conceptual frameworks.

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<sup>13</sup> Or “bug”? Phonetics from tape recording, please verify term.

I will finish this by harkening back to an example that is now 30 years old. I had several former lives—one of them, I worked for EPA in the Great Lakes program back in the 1970s. The 1978 water quality agreement, which put phosphorous loading caps on all the Great Lakes, was the first example where models had been used enough to make a large-scale management decision of that nature. We did not use one model. We used five models ranging in complexity from the simple Volinvitor loading plot model, to the fairly complex multinutrient-multispecies phytoplankton model. And there were several models in between. That effort was extremely successful, and in my opinion, the use of multiple models created a weight-of-evidence case that greatly strengthened those management decisions. I strongly believe in multiple models.

**Rob Hetland:** It has been awhile, could you repeat the question?

**Kreis:** Should modeling stop? Or should there be a future to integrating the different media, the different concepts, maybe multiple lines of evidence, and weight of evidence of different models in the future?

**Justic:** No, to the first one-third of your question.

**Hetland:** Well it depends on what question you are asking. There has been a lot of discussion here about what we want to predict. I have seen talks that seem to suggest that what we want to predict is the hypoxic area. That would require one set of models. And I think those models are actually the existing models. The statistical models are not so bad at that.

What about the reduction in the hypoxic area? That is another question. I would argue that we do not understand the processes well enough to answer it just yet.

And what about the nutrient ratio question and speciation and the potential for harmful algal blooms? That is a third question, and that would require a third set of models, of which we have seen none today or throughout the whole meeting.

It also depends on whether you want to use the models for prediction, for management questions, or to understand processes to make sure that your mental conceptual model of what is going on out there is complete. If it is the second thing, if you want to look at processes (if you want to understand if you have all the issues covered)—like the flux of carbon to the benthos from the water column, the issue of denitrification, the issue of mobile muds—are these things important for any of those three things I just listed? Maybe so, maybe not. Then I would argue that you would actually want to start out with the complicated models and simplify them, rather than start with the simplified models and complicate them. Because, if you start it simple and you try to build complexity, you are going to build complexity in the way you think it should be built. And you might be missing the most important thing and never know it just because you never added it to your model.

**James D. Hagy:** I would like to go ahead and ask a question. Rob, we heard you mentioned that in the western side of the shelf, that with the unremarkable background sediment oxygen demand, we might be seeing a large area of hypoxia that would be principally caused by physical factors. And given that the largest areas of hypoxia occur when hypoxia develops out there, it raises the question of whether this area is amenable to fixing hypoxia through nutrient reductions at all. Not that I necessarily think that is the case. I am interested. Is this result, or any other new modeling result that we have, sufficient to suggest that we might wait or reevaluate our plan to reduce nitrogen loading as the main initial course of action to fix hypoxia?

**Hetland:** Let me first say, I am glad you understood the talk. That was my point, and you got it. So let me answer the last part first. So, is this a reason to wait? No, it is not. What is your best guess, if you want to reduce the hypoxic area? If this is your goal, what is your best guess as to what to do? Everybody else, all over the world, has said reduce the nutrient loading. And by how much? I am going to bet that 30 percent is a whole lot closer to the answer than zero is. That is just your first best guess, right? So no, it does not mean not do anything. I think what it does mean is, if this area is less sensitive, it has a longer memory. Or maybe it stays hypoxic. You have to be prepared when you implement your management plan for something not to happen out there—for there *still* to be hypoxia. And it would be nice if you could expect it and say, “When we reduce nitrogen by 30 percent or by 50 percent, this is what is going to happen.” And we know that ahead of time so people do not get mad and say, “We spent billions of dollars reducing the nitrogen and nothing happened.” So, it is being prepared for what might happen when you implement your management plan. This goes to my point too. If your question is, “What happens when you reduce the nitrogen or the phosphorous coming down the river?” it is a different question than predicting the hypoxic area.

**Hagy:** Coming from the Chesapeake Bay way of thinking about things—one of the things that I see as most important, or as a very likely avenue of change, is that the sediment oxygen demand would in fact change as nutrient loading is reduced, and that would be one of the most important things. We know that some of the models that have been done before cannot capture that. But Rob, you said that the sediment oxygen demand that we see on the western shelf was the same as you have off Texas, was unremarkable. Do you think that those rates might actually go down? What would we have to add to our models to address that?

**Hetland:** You are confusing me with a biogeochemist. I do not know. My colleagues have told me that this oxygen flux is unremarkable; you should ask John Morse or Gil Rowe or Piers Chapman. In terms of modeling it... If I get the equation, I can model it. That is not the issue. In the future work, I have an excellent biogeochemical modeler. If we understand what the response might be, we can model it. We can even model different hypotheses.

**Justic:** Let me address that issue. There are two aspects here. One, obviously if you are going to put the emphasis on sediments, the question is, “Is there a sediment memory?” Not only a memory that would last a couple weeks, but one that would last from one year

into another. I am still looking for scientific evidence that that is the case. So before we start developing models, we should include substantial sediment subroutines. I would like to see firm evidence that in fact there is a memory and that organic material deposited this spring will last longer than the end of summer. Working with Nancy and other people, I have not seen evidence that that is the case.

There was only one instance, which was the flood of 1993, where we felt that there was a carry on—that there was an influence in 1994. That 1994 data were suggesting that there was a surplus of organic material from the previous year. So that is the question that needs to be answered, and depending on the answer, modelers will do what needs to be done. Again, it is a very interesting question. Do you have a rapid deposition that gets burned in a couple of months? There are studies by Smethachick<sup>14</sup> and others. They say that the diatom blooms that are deposited within a few days are burned in 20–30 days, and they carry 80 percent of the annual primary production. So is that the case? Or is it the memory that will last, and you build organic carbon in the sediment? So the watershed issue comes up with McIsacc's approach—that there is a multi-annual signal. So that is one point that I wanted to make.

The second is historical context. In the available oxygen records and areal maps, I have absolutely not seen anything suggesting that the area around the Atchafalaya Delta was historically different than the rest of the shelf, and that that region was more prone to hypoxia before (which one would expect to be the case if, in fact, it is preconditioned to hypoxia based on certification). The only one possible reason that something has changed, and that came from talking to Don Boesch, would be if something happened with the Atchafalaya deposits (movement of stuff around the delta, the shallow delta with a lot of reworking), which again we have no firm data on that. We have no idea why if there is something different in the Atchafalaya recently, what that might be. What I am trying to say is, let us talk about the last 35 years. Let us try to see what has changed and what has not. And then you can talk about wetlands, fluid muds, watershed, atmospheric, all kinds of things.

**Cerco:** I wanted to make a comment yesterday and fortunately I got stopped before I put my foot in my mouth. But now I am up here, so I am going to make a comment. Then I am going to pass the microphone to Dr. Chen. I think there has been far too much emphasis on looking at a hotspot, whether it is modeled or observed, and asking what is going on at the hotspot. So for example, what rains down from the surface to the bottom that creates demand at that hotspot? And in fact, what goes on at that hotspot is the accumulation of everything that has gone on effectively upstream of the hotspot. What is the Streeter-Phelps model that Scavia did? It is something that shows us that things accumulate as the water moves. So, just because you see a red thing on a model prediction that is to the west of the Atchafalaya does not mean that the process that created that is upstairs from here. It means what you see at that spot is the accumulation of parcels of water traveling all the way from the mouth of the Mississippi, and what gets there is the sum of it. So, I want to emphasize that, and I hate to do it, but that has been shown at the Chesapeake Bay. The place where the hypoxia is worst is not worst because

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<sup>14</sup> Verbatim phonetics from tape recording. Please verify author

of what rains down from up top, it is because that is at the head of the trench and it has accumulated anoxia all the way from the mouth of the bay up. We should not look too strongly at the red place on the map and try to say what is going on at that red place. You have to look everywhere.

**Hetland:** The Louisiana-Texas shelf is totally different from an estuary, and in fact, you do not get a whole lot of lateral advection. The processes are largely vertical, and I think that my hotspot model showed that. If you put a circle of hypoxic water near the source, it does not bleed out onto the rest of the shelf. The mean currents are very small down there, especially in the bottom waters. It works in estuaries, it works in rivers, but it is not the right conceptual model for the shelf. It is vertical.

**Cerco:** The fact that the currents are slow is great, because it means you have a much longer time for oxygen demand to accumulate to get to that place.

**Hetland:** If you have very weak currents—the mean currents there are half a centimeter-a-second—it is half-a-kilometer a day. It would take about a year to walk across the hypoxic zone. It just cannot be important.

**Kyeong Park:** I find that the plankton issue is pretty good—I think this is the first step we should do. Everyone has been arguing for 10 years on modeling. You can always argue, say which model is better. We should have a multiple models. But I think the work that has been done, very simple model as the first step, is really good. I would like to use Rob's model to do some process study. I think it is really exciting. You people do excellent work. So the question is, you already have a lot of data, so we see a lot of results. But we did not see how the timing of the hypoxia occurred. From 1993 to 1999 you have a lot of data already, right? You can probably do some hindcast modeling to study what is the physical, biological process, then control the timing of when hypoxia occurs. That is the first question. The second question is, "If hypoxia occurred, how long can it last?" You can use a very simple physical-biological model to address this question. You have probably already done this, but I do not know. I just ask do you have a plan or have you already done that?

**Justic:** Yes there are data out there, absolutely, since 1985. But if you look carefully, you ask what kind of data. There is one, only one shelf-wide cruise that Nancy is doing the last week of July. In recent years, there have been more cruises because EPA's group is going out, the Texas group is going out, and Nancy is going out. So you get more coverage. But traditionally, you had one areal map for one year. And for a conceptual model, if I ran 55 years and took one snapshot in a year, it might be useful. But for a fully coupled 3-D model that runs in 15-second intervals, you will agree that that might not be enough for calibration. That might be one of the reasons, or excuses, why more modeling was not done traditionally. But I would say for recent years when there were more cruises, the hindcasting and tuning the model to be observed might be the way to go.

**Bierman:** I am going to jump in there as well. I think that is a good point about the data. There are issues both with models and data. This gets partially at the answer to question

one: We have been, in my opinion, on the modeling side, lacking integration of the physics and the water quality. And we do not have an internally consistent physical, chemical, biological representation of the hypoxic zone at the spatial and temporal scales at which it really occurs. For example, the model that we developed in 1994 is three dimensional in space, it is time variable, but we ran it to steady state. So, it gives us space, but it is a snapshot in time. So we captured no dynamics. The model that Dubravko developed does exactly the opposite. It does an excellent job of capturing monthly dynamics, but at a single point in space and at a single location. Scavia's model does something else—it looks at the hypoxic area. The model that Rob Hetland showed us—an excellent start at representing the complex 3-D, 4-D if we include time, circulation aspects on the shelf. But it lacks the representations of biology that these other models have.

The point is that the pieces are there, but they have not been fully integrated into a platform that can represent the spatial, temporal, and process dynamics of hypoxia at the scales at which it actually occurs.

**Hetland:** I have more biology in my model than you give me credit for. I have started work on an NPZD type model that is nitrogen, plankton, zooplankton, detritis but I am not going to show anybody yet, it is just not ready to be released. But that is the first thing. We have money to do this, and that is the first thing we are going to do. We are poised, we have the physical model going, we have the biological model ready to plug into it, we just have to type “Go,” preferably with somebody who has a clue of how to set the biological model parameters correctly—and we have that too, now.

**Park:** Before I start asking my questions, which are more like comments, I would like to state for the record very clearly that I agree with you that the modeling work should not stop. The modeling work should be funded forever. And I also agree with you on the concept of multiple models. We really need to have multiple models to verify against each other. And I think we all agree that all the modeling work that has been done so far was great in terms of advancing our understanding of the underlying processes. And also, as you just pointed out, that we all agree that it is about time to integrate the different kinds of models like the hydrodynamic model, water column-biogeochemical models, sediment-diagenesis model, and possibly the sediment-transport model.

In integrating these models, I think that we really need to have some discussion about the structure of the models. I am not talking about to select any specific model. I am talking about the general structure of the model. For example, with the hydrodynamic model—what would be the appropriate modeling domain? How far do you go offshore and how far do you get into the land? Do you include the Mississippi River? Up to where? And also, what kind of vertical layers? What resolution would be appropriate, considering the understanding that we have and the data available? And then for the water column-biogeochemical models—how many state variables do you think will be necessary? This will be certainly guided by the available data, and the computation time. Also how high a trophic level would you like?

If the modeler who is going to do this job, is going to determine these things by him/herself, then down the road, some people might raise some questions against it (in about 2 or 3 years after the study). So I think it would be nice and desirable to have some talk before we initiate this integration (i.e., on the structure of the model).

In a similar context, I think it would be nice to have a chance to talk about the target processes. We all know that no model can simulate the prototype behavior exactly as it happens, so we really need to have some target processes to get some credibility about our models. For physical transport, for example, what kind of processes do you want your model to be able to simulate for you to feel comfortable about your model? Create a kind of a shopping list, and hopefully if you can attach some quantity/range to each of the target processes, that would be even better. But even without that, the list of target process would be quite important. I want to ask what you think about these two points.

**Justic:** Do you want to address the grid issue and vertical layers that are required for the shelf?

**Hetland:** Yes, okay. I picked my grid and I picked my vertical layers. I think that is a reasonable choice. It is a balance, of course, between what you think is a reasonable choice and computer resources. Every time you double the resolution of the model—this is an interesting fact I like to drag out—you increase the computation expense by a factor of ten. And that is because you have four times as many cells; it takes twice as long to run so that is a pretty big deal. I would like to be able to go up into the estuaries and up the rivers but I cannot because of the resolution issues.

**Park:** That is exactly what I am getting at. You will probably need to make some calls, but you do not like to take the full responsibility of the calls so you like to seek some consensus from the community that has been focusing on this work. As a modeler, if I am given the whole freedom of choosing to making calls over everything, I would like that. But at the same time, not everyone will be happy about the results that I am going to produce.

**Justic:** If I can add a comment about directions... Historically if you see where the large models were developed, you could see that they were typically not developed by university researchers. And there is a reason for this. The funding cycles are usually 3 years and they are too short. By the time you get money, you have to write the final report. So it is too short to develop a significant and large-scale model. Therefore, models were developed within...

**Hetland** [*interjecting*]: Well, wait that is not true, because I think, FE-COM<sup>15</sup> was developed by ...

**Justic** [*interjecting*]: Yeah but, Chen gets a million dollars funding from the state...[*laughter*] ...and they just bought him two supercomputers for his lab...

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<sup>15</sup> Verbatim phonetics from tape recording. Please verify model name

**Changsheng Chen:** I have a comment here from a modeling structure. We talk about models. We talk about a converging of the models, so I agree with Rob's comments. So it really depends on what kind of things you have. You double the resolution, you increase time and increase efforts. Modeling is very hard work. A lot of field people think okay, modeling is very easy just like a blackbox, you turn it on, you can run a model. It is not so easy. Sometimes as a modeler, I feel the time spent is much more than your field measurements people. So it is really hard work, but we do see modeling as very important. So we cannot stop the program. I think there should be more funding to the model because the model really can be the tool to advise the field measurements. So resolution is an issue, we know about this, but we probably need another workshop to talk about technique. So the model structure right now, I think, all the model they have the same dynamics no matter if you use the *wroungs*, *pound*, *recon*<sup>16</sup> the dynamics are the same. The only difference is, it depends on what case you use. So then the biological model, a lot of people are talking about we need to add in the suspended model, we need to add the biosediment model. But we have to be careful. You add more to the model, you add more degrees of freedom, and the uncertainty becomes huge. Right now like the nitrogen/phytoplankton/zooplankton (NPZ) model, for example, we have seven parameters. Even for the seven parameters, we do not know how to fund. Most are for literature, for some other places. So now you run for balance model, you have 40 to 60 parameters. So in this manner uncertainty comes in the model. If you add more to the model, you get more and more uncertainty. So I thought as a good way we should go back to the simple one. Start like very basic, like the old model. So add the terms by terms one by one, like I think Rob is doing, a very simple one, and see what happens. We do not expect everything to be right, but we will just see what happens. Also I think Dr. Justic's model, you know surely, like a prediction they are pretty good, we know we make this, but we can make this tool for managers to use. You look at the model right now. If you look at the hydrodynamic model, you probably can take the model the next 10—you really can do the forecasting. But 10 years we cannot wait. We better have some simple models that we can at least make forecasting that management can use. That is what I thought; it is not an easy job.

**Justic:** Yes I fully agree with your comments. What I was getting at is that, if we are going to support a large scale modeling effort and development of not only the model, but the modeling system, we need to have a mechanism in place that would support that effort not over the typical funding cycle, 2 or 3 years, but much longer than that. And then you would integrate groups and have the system in place, which would essentially force people to work together. And I thought that the original nutrient enhanced coastal ocean productivity (NECOP) modeling effort and the whole NECOP program was quite good at it. You even had a data manager for the entire program, which is quite rare today.

**Bierman:** Right I concur.

**Kreis:** Go ahead Dr. Bierman.

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<sup>16</sup> Verbatim phonetics from tape recording, unable to decipher meaning.

**Bierman:** Thank you Dr. Kreis. Just we do not talk like this in the bar to each other, Carl, Russ and I. I would like to offer some comments about what is really needed, in terms of what are the minimum, what are the most important pieces we need? If the question is dissolved oxygen in the subpycnocline waters, I think we should look at the primary driver for that and work backwards. I think we need to ask what controls the supply rate of organic carbon loading to the bottom waters and then what controls the consumption rate. What controls the supply and consumption rates of dissolved oxygen (DO) in the bottom waters? Well, the carbon has to come from somewhere, so that takes us up to primary production. We have to produce the carbon, because I think it is pretty clear that the hypoxia does not occur by land-side loadings. It occurs because we grow too much carbon. For primary production, we need to look at nitrogen, phosphorous, silicon and the internal cycling. But I think it is important to look at the fate pathways for the organic carbon that we produce. In simplified terms three things can happen. You can send organic carbon up the trophic transfer, you can send it up the food chain, or you can physically settle it to the bottom, and while it is settling it can decay. Okay. Let us leave the sediment aside for a moment. So I think we have to grow the carbon, as a function of nutrients, light, temperature, and then we have to decide how to split those pathways. And then in terms of physics, I think it is pretty clear. We have to be able to represent stratification at the scale of the hypoxic zone, and we have to be able to represent the transport and fate of nutrients from the two principal sources: the river discharges. And we need to make sure they get to the right places. Okay. Now the only thing I have left out is sediment. And that is a question. I do not think anyone knows just the relative importance of sediment either to present conditions or to imposing time lags in response to changes and loadings. It probably should be investigated, I think I agree with Dr. Justic, we ought to take a real hard look at the data and try to make some determination as to how important it is and how much effort to put into it. But basically, in my opinion, those are the minimum elements we need to take the next step. And I do not think it has to be a mega-model. I do not think it involves too much of a bigger step than where we are right now. If we just take the pieces that we have and put them into a unified framework...

**Kreis:** I want to bring up... this is for the steering committee as it relates to these discussions. There is a recent significant document published in 2004. Many in this room were involved with that. Rob have you recovered from this? Yes? No? You really have not? It has been 2 years, and so if I could ask or let the co-authors and the panel and the steering committee know that this is a significant document. Many of these comments that have been brought up have been partially addressed in here—in terms of modeling frameworks, in terms of recommendations for these considerations, some of the limitations, and so on. For the steering committee, this is one of our primary reference documents. Dr. Greene, could we have the list of questions? I am going to jump to question one... and in this document as well. There is... a lot of the existing uncertainties, shortcomings, recommendations, for modeling, monitoring, and research summarizes them and together with the presentation by Dr. Justic, more uncertainties, and so on. There are other publications that deal with this. That that might be a starting place for answering question number one. Dr. Hagy is going to go right for the jugular

here we have to get to, if we back up on to one of the other questions here, maybe four or so, Dr. Hagy.

**Hagy:** So I guess if I am going for the jugular, I am going with the butter knife. I think that the presentations so far have been pretty honest about what the models can and cannot do. So we can really skip the step of saying, “But what about this and what about that?” But I know that there is a lot of interest in the question of nitrogen, phosphorus, and dual nutrient strategies. I know Carl and I both our eyes were caught by the graphic from the 1994 model that showed phosphorus and nitrogen reduction effects which, although the nitrogen effects appeared to be larger, they seemed to be almost on a par. And so I have a two part question. First of all, at this stage do we still think—and I guess I can address this to Vic—do you think that phosphorus reductions would have such a large effect as that graphic suggests? And then, in terms of a target process for modeling, what things would we need to do and what kind of model would we need to develop to be able to address more directly the question of nitrogen verses phosphorus impacts on hypoxia?

**Bierman:** Do I want to take that? Good question. First of all the caveats on that. There are two results actually I want to talk about. One is the graphic that shows improvements in dissolved oxygen in responses to reductions of both N and P, but the responses to N reduction were larger. The other graphic, you might remember, showed P is relatively more limiting than N to surface primary production within the first 50 miles of the delta, and then progressively, it flips to N and becomes progressively more N-limiting farther away. There is an apparent contradiction, and I will come back to that. It is not a contradiction at all. But I think there should be many caveats hung on those results before we take them more seriously than they deserve. One is, it is a steady-state model, two, the stoichiometry in that model is fixed.

The carbon-nitrogen-phosphorus (CNP) relationships for phytoplankton, the N:C ratio, the P:C ratio—they are not constant. Built into that model is the assumption that they are at the redfield ratio. Myself, Carl, Dr. Cerco actually, in the past have worked with variable stoichiometry models, we know that they are not constants. Also, I should point out that I said that we should focus on the supply rate of organic carbon to DO... When it comes to primary production, we need to focus on the supply rate of available nutrients, in this case, dissolved inorganic phosphorus (DIP) or soluble reactive phosphorus (SRP) and dissolved inorganic nitrogen (DIN) to the water column. And they are not just coming from the Mississippi Atchafalaya River. You have land-side loadings for most river sources.

That model has a huge open boundary. I had to specify (based on available data), open boundary conditions, so they come into play and then we have a third source that is internal recycling of both phosphorus and nitrogen. So the nutrient dynamics there were extremely complex. They were complex in the model, and the model is an extreme simplification of reality and again it is steady-state. So I think the results are informative.

The results are the relative P-N limitation are very consistent with Jim Ammerman's results back in NECOP. I think those results are reasonable and they make sense, they are consistent with what we know. The results indicating that the system is more responsive to N and P—very consistent with the large body of modeling that has been done in the Chesapeake Bay. (The same question arose there.) Simulations were conducted—P alone, N alone, both together—and basically the same results. There are responses to P reduction, responses to N reduction, but responses to N were greater. EPA chose to adopt a dual nutrient strategy. I think these results make sense, but I think we need... they are far from proven. I think we need a lot more experimental work process work. And we really need to look at these processes dynamically over the season, because again, I will come back to it, the results I showed were for one steady-state snapshot in July. I do not know what the dynamics would have been in the spring. I do not know what they would have been later on in the fall. My model does not even touch that.

So I think we really need the capability to conduct dynamic simulations on a seasonal scale at the spatial scale of hypoxia.

**Greene:** Let me just follow up on that and be a little more explicit. Since we have the bulk of the modeling expertise in the room, I will really put you on the spot. We heard Tuesday about some uncertainties in the areal extent estimates that we have, which are the target in the Action Plan. We heard yesterday the need for the logical benefits of controlling both nitrogen and phosphorus. And yet most of the current modeling estimates really look at the range of nitrogen load reductions that might give you the target areal extent or some increase in bottom water, but they do not really address the variability in the size estimates. That is one issue I would like you to comment on. The other is, are the current models that are available that are giving us and directing us to a target nitrogen load-reduction suitable for estimating a target phosphorus load-reduction? Or can we do that back of the envelope by just...How much phosphorus do we need to get rid of, to get us back into a reasonable N:P ratio. (That was using the steak knife.)

**Justic:** Well, I think, obviously, one can argue that not only three nutrients, nitrogen, phosphorus and silicon, but many other things are important. People can bring the issue of other factors, probably not iron, but there are certainly other elements that should be modeled. At the time when I started this effort, the consensus was it was nitrogen and only nitrogen. Phosphorus was not even discussed remotely as an important factor. We were thinking about silicon, mostly based on Quay's work and then Gene Turner's, indicating that silicon would be an important thing as you go into shifting silicon to nitrogen ratios. And then this latest round of discussions and peer reviews brought the awareness about phosphorus. In a spatial sense, if you design a shelf-wide model, then of course if there are regions that are phosphorus limited—and that clearly shows in the data—then of course you need to have a model that includes phosphorus.

The question for me would be if I want to do something more, with my box model. And if it happens to be in the region where there is no phosphorus limitation whatsoever, or at least the data does not seem to support that idea, should I include phosphorus or not? Maybe not in that case, but I think we need to go back maybe to the drawing board and

think about why there are issues with nitrogen, phosphorus, and silicon. And we are learning as we go. I mentioned in my talk, that lightly-silicified diatoms are not sinkers, and then I immediately was corrected by Quay, who told me in fact, they are sinkers, and they sink quite rapidly. The silicon limitation and the shift to lightly-silicified diatoms might actually enhance vertical flux. This whole decadal trend towards greater *Pseudo-nitzschia* abandons<sup>17</sup> might have actually promoted hypoxia, so it is not as simple as nitrogen verses phosphorus. The whole community has shifted, and that is why I think that modelers and field researchers really need to work together rather than to visit each other occasionally once every 2 years. But of course, if you want to model areal extent, and you have indications that something is limited, you need to have it in the model.

**Bierman:** To try and answer to that, our model has phosphorus but I would not advocate its use in the present model, in its present form, to try to develop a target loading objective. That does not mean I do not support a dual nutrient strategy. I am just saying that, in addition to all the caveats I mentioned before on the model, one of the things that these models does is make certain assumptions about availability. I assume that the only phosphorus form that is available is orthophosphate. And the forms of nitrogen that are available are ammonia and the sum of  $\text{NO}_3$  and  $\text{NO}_2$ . That is not completely realistic—there are a lot of questions about enzymes that can kick into play and convert certain dissolved organic phosphorus (DOP) forms to available forms; there are some questions about dissolved organic nitrogen (DON) availability. I think that there is just far too much uncertainty. I think that one has to go beyond these models.

There is a huge body of research out there about coastal eutrophication, and again the weight-of-evidence—I love that phrase—modeling work, experimental work, points to the importance of nitrogen. The case for nitrogen control is very strong, and I think Jim Ammerman made some good points yesterday.

If you enrich a system so much in nitrogen, of course, you can actually force a system into phosphorus limitation. But that does not mean that you fixed the problem by reducing the phosphorus. It is the nitrogen enrichment that caused the problem in the first place. So you really have to be careful. I am not sure what the phosphorus target should look like, if there should be one, and I would certainly not advocate my model in its present form be used for that. I just think that there are too many uncertainties.

**Hetland:** Our model, my biological model right now does not have phosphorus, it will though by request. I do not know what is going to happen with that. But I think I am going to agree with what has been said and say that maybe this is not a question for the model. There is plenty of observational evidence, and maybe we should start to look at what the data say and maybe your ratio (nutrient ratio) for a dual nutrient strategy would be the way to go. Adding another nutrient to a biological model increases the complexity by more than a factor of two. So, you would not believe your model results anyway unless you had a firm observational ground-truthing.

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<sup>17</sup> Verbatim phonetics from tape recording. Please verify term.

**Hagy:** I would like to say something about that. You know I think certainly we need good field observations about the relative roles of N and P, but I think it absolutely *is* a modeling question. It is not that we are really so concerned about whether the phytoplankton is N-limited or P-limited. What we are really concerned about is whether limitation by one or both of these nutrients affects the end impact in terms of hypoxia, wouldn't you agree?

**Hetland:** Well, I think that would be an easier question to answer perhaps. The nutrient limiting question is probably the easiest. Whatever nutrient is limiting, then phytoplankton stop growing there. I think the big question I have seen develop in this meeting—and I am not a biologist either, I am not a chemist or a biologist—is the question of speciation. You have different things growing under different nutrient conditions, and that is difficult for the model. Usually we have a box for phytoplankton, and that box has certain rate fluxes associated with the stuff that is growing there at the time. Well what happens if that box changes its character because it is not diatoms anymore but it is something else? How do you model aberrations? I do not know. So I think that is the part of the problem that needs observational support—what are the changes in the community structure? I do not think that the models right now can answer that question correctly.

**Cerco:** I am going to put my foot in my mouth again. One, just based on what we have seen in the data, you cannot ignore P. You just cannot. If you are going to model eutrophication in this system, you have to have P. Now you can be enormously complex with your P models or you can be simplistic, but you cannot ignore it. And the other issue is, in terms of eutrophication management, N and P have been in these models for 30 years. There is actually a small subset of what I think of as oceanographic models, that only consider nitrogen. I mean phosphorus and nitrogen are your standard, basic, 30-year-old eutrophication model, and you just cannot ignore it.

**Bierman:** I wanted to go back Jim and respond to a point you made and then offer another reason why I concur with Carl. When we talk about nitrogen and phosphorus, we have to be careful about what process we are talking about. And the point is that, “Which of the two nutrients is relatively more limiting to primary production?” is a very different question than “To which of those two nutrients is dissolved oxygen more responsive?”

The primary production and DO dynamics in the subpycnocline waters are really two different sets of dynamics and external forcings. The driver for primary production really is the supply rate. DIN:DIP ratios give you a snapshot of external concentrations at a given point in time or space. It is extremely difficult to draw inferences about what that means at the spatial and temporal scales of primary production, much less hypoxia.

But the real driver there is: what are the supply rates of available nutrients to the water column wherever they come from—the water side, the boundary conditions, internal recycling? Let us go down to the bottom, to the organic carbon. The oxygen really depends on the supply rate of organic carbon and the consumption rate of organic carbon. Primary production can come into play, we saw that in one of the presentations.

Dubravko, as evident in your model, you found that you could not make things work unless you attributed about 15 percent of the production to the bottom. In our 1994 model, if you go back and look at the *Estuaries* paper, what happens is that the sub-pycnocline waters are about 20 meters deep near the delta; the bathymetry changes—it is only about 6 meters deep in the region west of the Atchafalaya. At least inside our model, the 1 percent light-depth was below the pycnocline. We were producing dissolved oxygen in the subpycnocline, so that played into it. I am trying to say... Oh and then we have the sediment oxygen demand, the way that plays into it.

I was going to address an apparent inconsistency in some slides that Dubravko showed. The results from our model showed that DO improves. The improvement of DO is greater for equal reductions of nitrogen than for phosphorus, yet at the same time, it showed that phosphorus was relatively more limiting to primary production—a whole different set of processes, within the first 50 miles of the delta. There is not an inconsistency there because we are looking at different things. Another result from that same model, which was not shown, is that down at the bottom, if we look at the nutrient limitation in the bottom waters, our model computes at least, nitrogen limitation everywhere right across the shelf. So, in top and bottom segments, you do not even have the same dynamics. So this is extremely complicated.

It is all part of the reason why I would be... we have that large body of literature out there supporting the importance of nitrogen. I do not see that quite that same body of evidence, I am not confident enough in the model to say that it should be used for phosphorus.

Now, Carl mentioned that phosphorus and nitrogen have been in these models for years. And that is certainly true. But let us remember that one of the uses of these models is management scenarios. There are very few management scenarios that control only nitrogen and not phosphorus, and vice versa. If that is going to be one of the intended uses, it just makes sense to include them both from that standpoint. Maybe one is relatively more controlling than the other. But if you are going to change two at a time and your model only has one, it might give you results that are not accurate in terms of that scenario.

**Kreis:** Given that discussion, I want to ask Rick's question again. This is in light of the reassessment. Do you believe that, based on the series of publications on the presentation today, the loading target in the reassessment should be revised or altered?

**Justic:** Whether the target should be revised, is not up to us. It is up to the people who we recommend whether it is 5,000 square meters or something else. But what immediately came out after we started running those additional simulations was the issue of climate change or climate variability...and that is an important issue. So it depends on what happens in the future. Targets will have to be readjusted as in adaptive management strategies.

Again there are all kinds of scenarios, and we do not know which one will play out. So, that is one aspect of it. I can only talk about what the models show. Of course these are

models, these are not real things that will happen. These are forecasts, and I expressed my opinion about that. They seemed to suggest that 30 percent would not be enough (talking about nitrogen), so since 60 percent was what happened between 1954 and the present... No... actually if you would *reduce* it by 60 percent now you would go back to the 1954 conditions. Of course nobody would do that. So somewhere between 30 and what was done is the answer. Whether it was 45, 40, 43, 47, 50, we can argue about that to death. But I would say it is anywhere between 30 and a high number. My take on that.

**Bierman:** I concur

**Greene:** Let me go back to what I asked earlier and that is, recognizing that the 5,000 square kilometers was really a policy decision, as, from the science end of it, is there a better end point that we might consider rather than *size of the zone* that would be easier perhaps to model?

**Hetland:** So your question is about a metric, which metric to choose.

**Justic:** Well, I think in the discussion with Nancy—she is the one who knows the most about hypoxia—one thing that came out if you talk about the total oxygen deficit on the shelf, the total nitrogen, if we assume that N fuels hypoxia, produces total oxygen deficit through the conversion to the carbon pool. So it appeared that having measures of total oxygen deficit on the shelf would be a very useful one to model and that would provide an integrated metabolic end product of everything that happened. And whether you would express that, you can go and express it as a volume of hypoxia, which some other people have done. But I would—rather than volume of hypoxia—really want to see integrated oxygen deficit for the entire hypoxic zone. Give me tons of oxygen, millions of tons, or whatever units, that I am missing with respect to the saturation bed. That is the type of data that I would like to have. And it is not that difficult if you have a very good program. You integrate...a good server program can do it, and there are better packages that can do it.

**Bierman:** I concur with Dr. Justic, if one looks to the future. If one is asked, well this is beyond this present assessment, but if you are looking for metrics, DO by itself is not what we are trying to protect. We are trying to protect living resources. And, really ultimately we need... Your question was what other metrics would there be, and you said that would be easy to model. I will answer the question in two parts. I would like to think eventually the science would evolve to the point where we would include living resources and the impacts of multiple stressors on living resources, with DO being a stressor. And I am thinking of bottom-dwelling organisms, the benthic, macrobenthic community, and so on, things that we do not want to die if we go anoxic. They are part of the benthic food chain. I would like to think that some day they would be included explicitly in models. But the answer to the second part of your question is, no, they are not easy.

**Hetland:** Ok let me nip this in the bud, Alan I am not going to put fish in my model.

**Hagy:** Just to amplify Vic's comment a little bit. My understanding is in the Chesapeake, people got sick of hearing about *hypoxic volume* days which was this large, seemingly meaningless number. And as we started to get—I will never use the right term and Amy is not here to help me—numbers, targets of what we want the oxygen to be, in different places, at different times of the year, whether they are criteria or... I can never remember that. We have, or *they* have started to evaluate, I am not part of the we any more, the extent to which the conditions that are observed satisfy these identified living requirements at the different times and places of the year. And I imagine that maybe at some point down the road we will have a better sense of what things are occupying the shelf and what their requirements are. And we might tailor the target to and measure our attainment in terms of how well we are supporting the fisheries and the other biota out on the shelf that we are interested in protecting.

**Amy Parker:** Just to sort of second, what Jim was talking about.... He confuses criteria and standards. And what we are trying to get is a water quality standards in place that will improve the hypoxic zone throughout the whole basin. We will have standards that we will eventually work toward that, but the water quality within the arena, within a particular watershed is also quite important. But I would respectfully disagree with Dr. Kreis, I believe that there is a huge body of literature on food web-modeling, and I would say the Dr. Justic's response about what we need to do to better integrate models is true. And I would say that we absolutely need to include food web-modeling if we are going to try and identify the resources that we need to manage, to best protect the living resources within the water. So I would say that better integration of models including those that model the living resources in the food web would be a part of that model integration that Dr. Justic is referring to.

**Justic:** I agree.

**Kreis:** I do not think I said *no* food web models, did I say that?

**Parker:** You said we do not model living resources.

**Cerco:** Did I say that?

**Parker:** That is what I wrote down.

**Greene:** On the agenda was a break that has passed already but we decided to just continue on. So that is what we are doing in case there are any questions. If you need a break, just feel free. I think we can maybe start integrating some of the audience questions that may have been turned in. We would like to open it up to an open-mike as well. But let's start with the questions. Start with the written questions that were submitted from the audience. Let's reconvene quickly within ten..

**Alan Lewitus:** Alright, I have a question or two of my own first. Okay? Because I had a couple of conversations with people on this topic, so it has to do with models. And with respect to models, is there a consensus that river flow and nutrient loading can be

discriminated? And is there a consensus in the relationship of each of these parameters to inter-annual trends in the hypoxic zone extent? Third, is nutrient load or nutrient concentration a more appropriate variable to relate to a hypoxic zone extent?

**Hetland:** I think the question is instead, can you separate out stratification from the nutrient loading? That you can in a big 3-D model. I think you might be able to do it in the river. But in the river they are coupled very tightly, and the nutrient load is more important than the nutrient concentration for sure. Do you agree?

**Bierman:** Dr. Justic knows far more about those relationships than I do. I think he should take the question.

**Justic:** You can run a model in which you will change, as I did, the flux of water and keep concentrations the same. Or you can play with the concentration. You can adjust it, and then you get some results and some insights. But in the real system they are coupled, you cannot de-couple them. You can design a laboratory experiment to do that, but if you go out on the shelf it will be freshwater and nutrients always together. So, how you measure those things is a challenge. In the modeling sense you can do it.

**Bierman:** I would like to respond to, there were several questions there, can I touch on the load-verses-concentration one? Well, they are both important, but they are very different things. First of all, when we talk about which nutrient is relatively more limiting to primary production that is a concentration-based question. When we talk about hypoxia, the driver there is quantity or mass of organic carbon that you deliver to the subpycnocline water. That has very little to do with concentration. It has to do with the mass of carbon that you generate, which in turn links to the supply rate. It is the mass of the nutrients that you supply. The instantaneous concentration through the Michaelis-Menton kinetics is one of the factors determining the instantaneous primary production rate. But by itself it does not determine how much carbon gets produced.

I also think we ought to realize some of the numbers we are talking about. Phytoplankton are extremely efficient extractors of dissolved available nutrients from the water column. They can draw down dissolved inorganic nitrogen on the order of 10 to 20 micrograms or a micromole, if I did that right. Phosphorus: they can deplete the soluble reactive phosphorus (SRP) down to a microgram or less, which I think is less than a tenth of a micromole.

In fact, with many of these models, we do not really use Michaelis-Menton coefficients as calibration factors. We set them at extremely low values, which is consistent with what the phytoplankton really do, and then we pay much more attention to stoichiometric factors like P:C, N:C ratios.

And that gets to the issue of supply. So which is more important? They are both essential parts of the dynamics. But if you are really concerned about hypoxia, you are much more concerned about load and how much the mass of nutrients you are loading into the

system, not what the instantaneous point in space or point in time concentrations happen to be.

**Cerco:** This is not really a modeling question at all, but I really wonder when you start talking about the future. I am just going to play devil's advocate. Imagine the situation, the flow in the Mississippi River goes up by 20 percent, but the nitrogen applied to the watershed stays constant, so does the load to the gulf stay constant? Does the concentration go down, so you get the same load? Does the load go up because the nitrate concentration in the ground water is constant? I have no idea. It is really something for the next session perhaps, but I would love to think about that. How are they correlated? And will they both go up or will they compensate for each other?

**Justic:** Again, I can only talk about what was published and what people have done and pointed out in the McIsaac paper and previously the analysis done by Don Goolsby. And what they have shown, clearly, is that there is a memory in the *watershed*. So, while we see an obvious lack of memory or not an apparent presence of memory in our coastal sediments, there is fairly good memory in the watershed. I think they did two analyses, 2–3 years and then 6–9 years. So, up to 9 years, you can see the effects of storage in the underground water and that will then be flushed out from the system, depending on the ratio of drought verses flood years. And we did a little analysis (it was published in 2003) in *Estuaries*, when we played with droughts and floods, we saw there is clearly non-linearity there. The thing gets stored in the watershed during drought years and then gets flushed during flood years. So the question is not only whether the flow of the Mississippi River will change in terms of an average, but whether the proportion of normal years verses dry years verses flood years would change. And most climate models disagree about the trends. But all the models agree that there would be an increase in the variability of the future climatic events, which would point out that we would have a greater variability in the fluxes, which would then, of course, mean that they are going to affect the gulf.

**Lewitus:** Okay. These are written questions for anyone. Do you see any utility to a short-term hypoxia forecast? For example, the dead zone in 2006 will be  $x$ -square kilometers? And if so, is this even possible to do within a reasonable degree of certainty, given the uncertainty and wind or current forcing-factors?

**Hetland:** It depends on how far in advance you want to know, what sorts of predictions you are going to use and how well you want to know the answer. Is that vague enough?

**Justic:** There are models out there, there is a paper just out by Turner et al. in *Marine Pollution* in 2006, where one of those simple statistical models is discussed. And I think Scavia was involved, and I was involved. We have these annual contests that Nancy started, to predict the zone before it happens and then we see how good you were. The fact is, if there is a disturbance, if there is a tropical thing in the gulf, all the models are wrong because you simply get a reoxygenated water column. But if there is a type of historical framework for which the models were developed—meaning that, yes, you will get a stable water column and nothing would be different from the previous years—to

which the models were calibrated, you will get about as much as an  $r^2$  were allowed. So if an  $r^2$  for when the model was developed was 0.6 or 0.7, you will get up to 0.7 and your point may get close to the line.

**Hetland:** In order to get to 0.6 or 0.7, you would need to know the river discharge. If you could predict the river discharge for the next year, you would get about 60 percent of the variance right.

**Justic:** Yes but, those models—the predictions are run about 6 to 8 weeks before the next cruise. So the idea is that you use the lag that we all know exists between the occurrence of hypoxia and the peak in river run. They are for surface productivity on the order of 1 month, and for hypoxia on the order of 2 months. So, if you integrate May and June data and you do it in May, or if you do it in June when USGS already gives you the data, you have a short window before you complete it. You have exactly a 20-day window before you complete the analysis, Nancy goes on the cruise, then you use integrated May-June [data], then you predict and see what happens when Nancy comes back. This is how it is done, and it would be great if you could run three dimensional models over that period of time by, of course, taking whatever data they can give and then predicting the rest and then seeing how those models do.

**Hagy:** Part of the question, as I heard was, is there a reason to do this? And, there may be, at least I can think of two possible reasons. One might be if some kind of economic activity might be changed given an eye to what might happen a few months in advance. I do not know whether that is true or not, but I do know that as I mentioned, there have been forecasts issued before, and there are some new forecasts that are being issued on the Chesapeake about a couple of different response variables. And I do not know all the reasons why they do that, but part of it seems to be just to illustrate that we have an understanding about how the system works. And to set the expectations of the public in-line somewhat in advance of when these things actually come in, so that everyone is on the same page about what happened and why.

**Lewitus:** Okay, another question. This is for the authors. In the past years we have put great store in a single value for each year; for example, in doing correlations with flow, nitrate flux, and so on. Can modeling help us understand or estimate the uncertainty in this value? Or more importantly, help us develop the time spatial scales in which to make the measurement or even develop a better quantifier for the problem of hypoxia?

**Hetland:** Absolutely.

**Justic:** Well, it depends on how you define uncertainty. What you can do, you take your model and you vary parameters, you vary input conditions, and then, of course, you will get different results. But what those things—which in the modeling terminology is technically called *sensitivity analysis*—will tell you is not necessarily how a real system will behave, but how your model behaves. And they tell you about the robustness of your model more than they tell you about the robustness or responses of a real system. So I would say, yes. But we have to be very careful because I often see people confusing

uncertainty with the simple sensitivity issues that relate to the robustness of the model and why and how the model was developed. That is a technical issue but a very important one.

**Bierman:** I would like to follow up on that just briefly. These models have inherent uncertainty in their conceptual frameworks, spatial-temporal scales, and process resolution. However, in addition to that, they all require inputs—flows, temperature, sunlight, and weather, so that the result that comes out the back end are due in part to inherent uncertainties in the model framework, because it is a simplistic representation of nature, which is very complex. But it can also represent uncertainties or mis-specification in model inputs. You see, before you can use a model to do a prediction, you have got to predict the inputs. You have to predict the weather basically. And one of the things that Don Scavia did with his model is, he did a Monte Carlo analysis of his representation of the physics and produced results: uncertainty bounds about his estimates of hypoxia in a given year. That uncertainty has to do in part with how he represented the weather. But it also has to do in part with inherent uncertainties in the model. One can always look at the inputs, it is a lot easier to look at the uncertainty on the input side. You can look at the range of precipitation, range of flow, range of weather, do a Monte Carlo analysis, and ask, if the uncertainty in inputs is such-and-such, how does that translate to uncertainty in outputs? You can always do that. But the harder question is, how uncertain or inaccurate is the actual inherent framework itself? This, in my opinion, feeds back to why there should be more than one model, and coming at it from different conceptual frameworks, different assumptions, and a lot of collaborative work between people pursuing different kinds of modeling pathways.

**Justic:** Yes, I can only add that those Alas-Go<sup>18</sup> run models, ultimately, those are systems of differential equations. And those equations have to be solved somehow. They are solved through a process of numerical integration, which is not an analytical solution, it is not fully accurate. So all those algorithms have certain error and sometimes just by switching between the two different types of algorithms, you get a pretty large error that is as much as the error in your data. So there are different sources of errors in the model, and I guess we need to recognize which sources they are and how big they can be before we start to fix something on the input side, which might be a minor part of the variability.

**Cerco:** Generally speaking, the ability to quantify uncertainty in the model is inversely proportional with the complexity of the model. If you have a simple Streeter-Phelps Equation, which runs instantly on a PC, you can do a thousand Monte Carlo simulations with no trouble. If you have a model that takes an hour, or 2 hours, or overnight to run, you are not going to do a thousand models. You just are not going to do it. So oddly enough, you know people who do very simplistic models, and I am not degrading, I am just saying simple models for statisticians who love uncertainty analysis. But as you start getting into these very big, complex, coupled, hydrodynamic water quality models, trying to derive uncertainty from those models is infeasible, in my opinion.

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<sup>18</sup> Verbatim phonetics from tape recording. Please verify term.

**Hetland:** There are tools that you can use to, first of all, analyze the uncertainty in your model. You can use a lot of the inverse methods that have been developed recently to start to look at the error covariance in your model due to boundary conditions, due to model forcing. You can also just do model data comparisons, and see where your model is getting the answer right and where it is getting it wrong.

So I think my colleagues are painting too bleak of a picture and that it is possible to separate the wheat from the chaff, identify the robust parts of the model, and understand what is going on in terms of the development of hypoxia, the time scales, and the space scales. I do not think that is an unrealistic goal... I do not think that it is out of the reach of models, and I do not think that it is going to be overwhelmed by model error.

**Lewitus:** This one is for Rob and it is a challenge. In your talk, how do you reconcile your statement of low nutrients, low chlorophyll not knowing what contributes carbon to the area outside of the Mississippi River Delta Bight by with (1) the data shown in session one that shows the high spring nitrate across the area to the west, the high chlorophyll biomass 1 month later and subsequent low oxygen, (2) also shown in monthly models of discharge high nutrient, high chlorophyll biomass and high flux at the bottom with the Justic model, and (3) similar relationships downcurrent of the Atchafalaya nutrient core for biomass with low oxygen with similar time lags.

**Hetland:** That is a mouthfull. Could you repeat the question? I may ask you to repeat parts of it. Well, there probably is, there has to be some source of carbon to the sediments out there. I did not mean to say that there is no production out there at all, it is just less than you get to the east—that is clear from the satellite images. I do not know. I am not going to try to reconcile it. All I am saying is that my model, with bottom respiration only, that was not particularly high, produced hypoxia in a region west of Terrebonne Bay. And I am not addressing how the carbon gets there; that is the next step. That is the next 3 years of research. But I think that is a fact, and, as a fact, it is something we need to think about.

Maybe the increased carbon fluxes increase the barrel rates, maybe re-suspension moves sediments from near shore, where productivity is high, to offshore, where productivity is less high. Another interesting result I am going to tell you about—one result from my NPZD model, although surface production was patchy—so you had strong patches of nitrogen near the two river sources, strong patches of plankton beyond that, and strong patches of zooplankton beyond that, kind of like you would expect—the flux of detritus was pretty uniform across the shelf. So there may be processes that will smear out localized patches in production across the shelf to form a diffused carbon pool. But I don't think it is related to the low bottom respiration calculation.

**Lewitus:** I think that is it. There are a couple of other questions and ones pretty lengthy about physical circulation. I do not think I could do it justice to it, so who ever wrote that in, you want to go ahead? In fact if you want open the floor.

**Greene:** Yes why don't we go ahead and open it up, I know some people are dying to ask questions, but go to the mic and please state your name.

**Dan Wilkay** (Texas A&M University): At the beginning of this session I heard something that was contradictory to me, but maybe it is just because I was not able to get my mind around it. Dubravko, you said something to the effect that you have to consider the available data prior to structuring your model. As a first step you need to do that. And then Vic you said something to the effect, and only 2 minutes later, which is why the contradiction popped out to me. That you have to let the model guide future investigations because the model then can identify important aspects or features of your system where you can direct your research. So, to me is this contradictory. Are these two different philosophies or to ask the question more to the point how limited will a model be as a guide for future research if that model is built or is constrained by past research?

**Justic:** Well, regardless of what I said let me state how I think the model should be done. You build the model based on the understanding of the signs about a certain process and principle that you want to model. And of course, you start with a conceptual model. You draw a little chart. You put the arrows where you think the arrows should be like I gave the example of the hypoxia model. And then you try to scale down from hundreds of species and hundreds of processes to those essential few that you think are important. So it is a highly subjective process that, of course, is not perfect. And Vic and I, if you ask us to do something we will do two different things (unless we cheat and look at each other's chart.)

So that is how things are done and then of course my personal opinion is that models should be very heavily tested and related to the available data. So then once I draw the chart and the diagram, then I would like to see what is around before I start running it. That, of course, does not mean that if there are absolutely no data on net primary productivity out there that I would develop a model *without* such a thing. I would not do that because it is *essential*—a component of any oxygen model. But in my mind, there are things that you simply have to measure.

Modeling, in my opinion, is only a tool. It is a tool as much as any other scientific tools that we have out there, field research, statistics, or you name it. Very often people have unrealistic expectations that models somehow will tell you things about the future or tell you things about the present that you can as easily go out there and measure. So stated very simply, if you ask me what the oxygen concentration is today in a pond outside this building, let's go and measure it. It is much more straightforward than running a model to see what it is. Of course, there are things that cannot be done in any other way except using models. That is the exploration beyond reality where I think the models are the only scientific tools that can do that. And in that case I would say absolutely. But I want to be very careful in terms of data, because I started as a biologist looking through a microscope and I saw hundreds of species, not one.

**Bierman:** I will try to give an answer that is supportive of what Dr. Justic said, but I guess I view it in different terms. I do not think data and models belong in boxes. They

certainly do not belong in different boxes. I think data and models are both imperfect representations of complex natural systems. You can have tens of millions of dollars and go out and take as many point-in-time and point-in-space measurements as you want, and all you have are data. You do not necessarily have knowledge or understanding. Models to me are not end-user tools. They are not just end user products. They can be used as tools, but to me what is more valuable than the tool is the modeling process. And I characterize the modeling process as ongoing interaction between data and model, using one to understand the other, in a continuous cycle, to improve our understanding. That is how I view it, so I do not see it as a matter of contradiction. I see it as that they are just intimately connected.

**Wilkay:** What I see is in the theoretical literature, some of it blue water, but a lot of limnological research tends to embrace the complexity that you see under a microscope. And they use very different modeling approaches and none of those talked about here. And similarly, if you go to a conference dominated by limnologists they do not embrace the modeling approaches that we saw talked about the past couple of days. And it is curious to me that there are very different approaches to... should I say...very different ways to try to skin this cat. And so, I guess the quote that you put up by Levin is very appropriate that we do need to look at the intersection of these independent lines.

**Nancy Rabalais** (Louisiana Universities Marine Consortium): My question was the one that Alan was not quite sure he could translate properly about circulation, so I will try here. The idea is to address the system as a coupled advective and vertical system because the analysis that Bill Wiseman has done from a year's worth of Acoustic Doppler Current Profiler (ADCP) data shows that the along-shore component is very strong, especially in the spring, even down to the bottom when there are nutrients and a lot of biomass being formed. The data also shows that the cross-shelf portion is much smaller and that the system with his empirical, orthogonal, function is primarily unidirectional for most of the year, but is bidirectional especially during the summer. So my question is, rather than looking at the system as a vertical system, would you agree or disagree that we should be looking at the system as both a coupled advective system on the surface with different rates at different times of the year with bottom currents that are at different rates at different times of the year as well as the flux of material over that same area?

**Justic:** Dr. Cerco will answer that question *[laughter]*.

**Cerco:** No hey, that is for the modelers.

**Hetland:** I have a sneaky suspicion that that question was for me so I will. Well, that is why you drag out the hydrodynamic models, because there is a lot going on. There are lateral motions in the coastal boundary layer. Interestingly, you do not necessarily always see hypoxia in the near-shore regions where it is well mixed, where you do get those coastal boundary currents. The coastal boundary currents are the strongest in the winter when the winds are down-welling and they are weaker in the summer when you get the pooling of the freshwater off the Louisiana Shelf

**Rabalais:** The results that I referred to were from a twenty-meter column. Rob started talking about the inshore portion of the Louisiana coastal current... The data that Bill Wiseman has published are from a 20-meter water column, well offshore of that area.

**Hetland:** Can you describe them more? I do not have that data in my head right now.

**Rabalais:** Well, I thought I did, but he shows much stronger along-shore movements of water and associated properties than cross-shelf. He also shows much higher currents on the surface than on the bottom. But he does show significant currents on the bottom at certain times of the year. And he shows a change from unidirectional flow to bidirectional flow particularly during the summer period. He also show periods of very high shear from the surface to the bottom. And he also shows some periods of very strong reversals of currents—both in the surface and the bottom

**Hetland:** So what are the means, the mean in the summer is of particular interest, what are the means in the bottom during the summer?

**Rabalais:** Well I guess what I was saying is that the currents *all year* and delivery of nutrients and chlorophyll and accumulated carbon *all year* lead to the development and then the maintenance of hypoxia. So in the summer the currents on the bottom can be anything from less than 7 to 20 or 30 centimeters per second. And in the surface they can still be anywhere from 50 to 80 centimeters per second.

**Hetland:** That is smokin'! That is really fast, but those are not the mean currents.

**Rabalais:** Those have been averaged ADCP currents from the bottom to the surface.

**Hetland:** But those mean temporally averaged, those are not averaged...

**Rabalais:** They are instantaneous. It is a time-series from the middle of March through the middle of November.

**Hetland:** I would be shocked if anybody measured 80 centimeters-a-second currents at the surface in any sort of seasonal mean on the Louisiana Shelf. That is really fast.

**Rabalais:** Well I said 50 to 80.

**Hetland:** I would be surprised if it was over 20.

**Rabalais:** Okay. Alright, I will send you the paper.

**Hetland:** The shelf can be very energetic, there are very strong inertial motions out on the shelf and there are strong long-shore currents in the summer during frontal passages. But the means are not necessarily that big. I think it is unresolved how much is due to—wind things are advected laterally—wind vertical.... Certainly in terms of sinking rates....

**Hagy:** Rob just to clarify, when you say *mean current* you are talking about... (**Hetland:** the temporal average)...say, if you averaged an hourly current ADCP measurement over a period of several weeks to a month or even 2 months, what the mean would be at that time scale. You are saying that would be slower than what you might measure for a mean current within a 10-minute period?

**Hetland:** Yes... whenever you calculate means and variances, you have sort of an averaging window. A relevant averaging window for thinking about these coastal circulation issues could be a week or something like that, or a season. And the stuff that happens on a timescale shorter than that averaging window is the variance, and that can be significant whereas the mean can be fairly small.

**Park:** I have been maintaining a mooring station at about the 20-meter depth in the Alabama coast over about a year. And I am talking about the Lopez field signal here from the ADCP. There are some times the Lopez field signal was as strong as about 40 centimeters per second. So I do not know how it was derived or caused, but the system here is very dynamic. As Nancy just pointed out, I have observed almost all the things she has described. You know sometimes it is a one layer system, sometimes as many as three layers in the vertical. So it varies in every way possible.

**Hetland:** Yes I think that is true. So how long... (**Greene:** You can follow up at lunch) Yes. (**Greene:** Let's go to the next question.)

**Rob Benner** (University of South Carolina): This is really more of a comment than a question, but it came up in the discussion today and it has come up before. This question of, is there a memory, or would there be carbon oxidation either in the water column or in sediments if that carbon was not supplied in the previous few weeks or even in the previous few months? And normally oxidation and production are tightly coupled processes. But it has been known for a very, very, very long time, with... I can give you a very definitive answer to, "Is there a memory?" And the answer is "Yes." And that is why geochemists like Bob Burner 30 years ago, developed multiple decay constants for organic matter—whether it is organic matter in the sediments or organic matter in the dissolved phase in the water column. It all does not degrade at the same rate. And so you have from a modeling perspective, a relatively easy way to deal with this. And I do not know how you modeled this. I assume you are using something like an exponential decay constant, then you can use multiple exponential decay constants. I would say at a minimum you need to use two. And so that reservoir... Of course, if you put fresh organic material that is very labile in, that goes at a very high rate that has a decay constant that is very different from this background level. But that is always the case. But if you go back to the scenario of, you capped off Rob, the water in the western part, and you saw hypoxia develop... The key question I would have is how long was that capped, as far as oxygen exchange? How long did that take and what volume of water are we talking about here so you could calculate using a known volume and an oxygen removal? And a known time... how much oxygen consumption was there over a period of time in that water?

**Hetland:** So, what is the question?

**Benner:** I turned the comment into a question, the comment was about the memory. But the question is really, basically what time period did it take for that draw down to reach hypoxic conditions in that simulation that you presented?

**Hetland:** Well it was a couple of weeks to a month or so, about what you would observe out there. It was not necessarily a smooth process either because there were several of small reoxygenation events where the shelf gets hit by a slightly higher wind and the oxygen goes up a little bit, but not too much. So it is not because ... This simple biological parameterization is sitting in a pretty complex physical flow field. The time scales are not going to be purely governed by the benthic respiration rate. It is also going to have to do with the motion of the water above it and the motion of the bottom boundary layer above it. That said, it took about a month for hypoxia to develop, after the stratification started, maybe a little less.

**Cerco:** Here comes the foot in the mouth one more time. We used the 3-G model in Chesapeake Bay for the sediments. And one thing is that the majority of phytoplankton is G-1, which is labile and rapid, but in fact there is a G-2 component. And that particularly relates to lows from the land. Both models and analysis of the data show that the system response time is 2 to 3 years. I mean, given the G-2 component of the load, if somebody were, either in model world to shut down the load, or else in the real world if you have a drought or a big flood that brings in a lot of organic matter, it seems that 2 to 3 years is the residence time for nitrogen. Phosphorus is a different issue. But that seems to be the scale. And I would expect something like that in this system also.

**Mary Booth:** (Environmental Working Group): Hi. First, thank you for a really excellent session this morning. This has been very informative. I had a comment and then a question about the N:P ratios in the models. I think from a modeling standpoint talking about ratios is a lot of fun and interesting and kind of endlessly productive. But that in light of the fact that as someone pointed out, these models are supposed to be helping, or hopefully will help with management decisions, I think that too much focusing on ratios may be a distraction from the main issue. And we really need to just talk about reducing nutrient loads in general.

Maybe in a system like the Gulf of Mexico, which there is an awful lot we do not know about it, I am very concerned that the assumption that primary productivity is controlled by N:P ratios may overlook the fact that... As a soil ecologist I know that there is an almost infinite diversity of microbial life strategies out there, and it is not this simple stoichiometric relationship, if we can just drive down P, that N would not matter any more. So that is one point, but then also my question is, to what degree can the gulf be considered an open system for P? Say that P were controlled better than we controlled N? I mean is there enough delivery of P from other sources in circulation to compensate? We heard some interesting work yesterday about the tying up of P with iron oxides and that that should not be biologically available. And that would certainly seem to downplay

the role that re-suspended sediments could play in delivering P to an otherwise P-limited system. But I am just curious what the consensus is right now about... Can you consider the system open to P from sources other than terrestrial delivery?

**Bierman:** I guess I will take a cut at it because I have the N and I have the P in my model. Just to be clear, it is not quite correct to state that the primary production is controlled by the N:P ratio, primary production in the model is controlled by potential multiple limiting factors—the first limiter is temperature, the second is the light field or underwater light attenuation. The primary production is extremely sensitive to the degree of attenuation as you go deeper in the water column. Then the third thing the model does, is it computes the Michaelis function for phosphorus and the Michaelis function for nitrogen as though they were independent. It selects the more controlling of the two, the more limiting of the two. Then primary production really is a function of the max growth rate, the temperature reduction factor, the light reduction factor, and the nutrient reduction factor. So that is just to get that straight.

Control of primary production (relative control by N verses P) depends on, from the Michaelis equations, the instantaneous concentration. But more broadly, it depends on the relative supply rates, and we have three sources of supply of available nutrients to the water column: (1) land-side loadings, which are delivered at the Mississippi and the Atchafalaya, (2) the open boundary conditions, you make an excellent point about boundary. If you look at it, you recall the grid that Dr. Justic put up, our model has 21 boxes, it is three dimensional, but it only represents the Louisiana-Texas Shelf, which is a really small part of the gulf. We have this big open boundary and we have to assume or assign water exchanges, dispersions, and we have to assign boundary conditions and the values that get assigned to those boundary conditions for nutrients for example, based on data but relatively sparse data, so the nutrients can come in from the boundary or they can leave the model domain and go out. So there is that two-way exchange. And then finally you have (3) nutrient recycle. Total phosphorus and total oxygen do not drive phytokinetics. So you really have all three of those sources: land, ocean boundary, and internal recycle for both phosphorus and nitrogen. So that is all represented, and we assumed this is a fixed stoichiometry model. We assume basically the redfield ratio for the relationships among carbon, nitrogen, and phosphorus. I guess that explains in a little more detail what is inside the model. But I am not sure that I have answered your question. I think you asked about the boundary, yes the boundary is extremely important. It is not just about land-side loadings, and that is one reason why to do this right, in my opinion, we need to nest the shelf inside a larger Gulf of Mexico circulation model. Maybe not at the same spatial or temporal scale, but this is not just a little patch that sits there and is driven by what comes down the Mississippi–Atchafalaya Rivers. It is extremely interactive with the rest of the shelf.

**Greene:** If there are no other questions we will wrap it up and go for lunch. We are running a little bit behind. If we could reconvene at 1:40? We'll get started then for the afternoon session. I would like to thank the authors and the panel for a terrific session.  
*[Applause]*