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Gulf of Mexico Hypoxia Working Group
National Centers for Coastal Ocean Science
WS 13446 SSMC4
1305 East-West Highway
Silver Spring, MD 20910

A significant number of the American Farm Bureau Federation's five million members live and farm within the Mississippi River Basin. We appreciate the opportunity to respond to the six topical scientific reports requested by the National Science and Technology Council's Committee on Environment and Natural Resources (CENR) for an assessment on the causes of hypoxia in the Gulf of Mexico. Any policies promulgated, as a result of this report will have an immediate, and possibly profound, impact on the majority of our members.

Attached are a number of specific responses related to the six topic reports. First, we will make some overall comments about the CENR reports and the recommendations submitted. Then we will go back and go over the reports starting with Topic 1.

While there has been a significant amount of work by the scientists who prepared these reports, the reports themselves raise as many questions as they provide answers. The reports contain contradictory statements, and above all, a fundamental intellectual and scientific bias of proposing reductions in nitrogen even after a complete and utter failure of Topic 2 to discover any benefit at all of doing so.

The first and perhaps overriding issue is the impact of the hypoxic zone on the fisheries of the Gulf of Mexico. According to Topic 2:

1) There is no discernable, measurable, or documentable "detrimental ecological and economic effects" to the Gulf environment or its fisheries from hypoxia. (Topic 2, p. 52);

2) Even during the recent unprecedented extremes in hypoxic conditions (1993-97), Louisiana's coastal fisheries flourished, maintaining "energy flow to productive fisheries (crabs and shrimps) that depend on the bottom." (Topic 2, p. 9);

3) Such ecosystem disturbances in the shallow continental shelf as can be documented are as likely to be "due to the other sources of stress that disturbs sediments such as trawling." (Topic 2, p. 50).

These quotes reflect the very challenging aspect of the Gulf of Mexico hypoxia issue. The question remains whether the hypoxic zone will ever have any measurable impact on the people and the economies of the areas in and surrounding the Gulf of Mexico? The indication from this exhaustive research is that it has not had an impact in the past which implies it most likely will not have an impact in the foreseeable future. While some would stop at this point and suggest it is not necessary to pursue this issue further, we believe that there are other important questions that need to be addressed.

Turning next to Topic 1. There are several important issues that need more research and explanation. For example, Carey notes that there was an upward shift in the mean water flow into the Gulf of Mexico in 1967. From 1967 to 1995, the mean flow discharge that's measured at Vicksburg was 11% greater than the mean flow prior to that time. (Carey et al. 1999, page 33-Figure 18). Furthermore, Topic 3 states that streamflow in the Mississippi River increased about 30% during 1970-83. There are a number of possible physical reasons for the increase in the Mississippi River discharge. They range from an increase in the precipitation in the watershed to changes in the river flow as a result of engineering changes along the river. Irrespective of the cause, there is widespread evidence that the flow has increased. The greater the volume of fresh water, the larger the area that is likely to become stratified and thus subject to hypoxic conditions.

On the other hand, the nitrate concentration levels in the water have stabilized, if not actually declined in recent years. **To the extent that the nitrate flux has increased in recent years, it is due solely to the increase in the flow of water to the Gulf of Mexico.**

Another important issue is the question of which nutrients play a role in the hypoxia zone. In the Topic 1 report, the authors focus almost exclusively on the role of nitrogen. This overlooks another important nutrient source, organic carbon. Organic carbon is necessary for the onset of hypoxia in the bottom waters and sediments. Respiration of this carbon reduces oxygen concentrations to the levels that are hypoxic. It is difficult to determine whether it is nutrients and the eutrophication paradigm, river-borne organic carbon, or more likely a combination of both marine and terrestrial organic carbon, that fuel the decline in bottom water oxygen concentration. (See review of this issue which follows on pages 2-25.)

Unfortunately, the authors of Topic 1 have focused almost exclusively on the nitrogen issue. Other researchers are of the opinion that determining the source of organic carbon fueling hypoxia is critical for management decisions related to the hypoxic zone. This issue must be thoroughly researched and reviewed prior to taking any precipitous action regarding nitrogen.

Topic 1

The Topic 1 report is a significant contribution to the body of knowledge regarding hypoxia on the Louisiana/Texas continental shelf. In this review we will provide an overview of the Topic 1 report and discuss the strengths and weaknesses of the data. Alternate interpretations of the data, especially as regards the role of terrestrial organic carbon, will draw heavily from a recently published report on the issue at the University of Alabama (Carey et al. 1999).

Statement of the Problem

World-wide, nutrient over-enrichment (eutrophication) is a critical management issue in coastal environments as a result of the negative effect that it has on water quality parameters such as bottom water oxygen concentrations and light attenuation, as well as commercial fishery harvest and the recreational and aesthetic value of these ecosystems. Changes in the magnitude and timing of nutrient loading, primarily nitrogen (N), caused by watershed land-use alterations and combustion of fossil fuels are likely to have a significant impact on coastal ecosystem health as a result of their effects on biogeochemical processes and food web dynamics in these ecosystems (Rabalais et al. 1996; Vitousek et al. 1997).

The hypoxia issue in the northern Gulf of Mexico and hence management issues concerning nutrient runoff are rooted in the "eutrophication paradigm". The paradigm is a simple sequence of processes that occur in aquatic and marine ecosystems that are experiencing elevated rates of nutrient, nitrogen or phosphorus, loading. Incoming nutrient enhances rates of primary production, which is equivalent to organic carbon production. The organic carbon then sinks from the surface to the bottom where it is decomposed primarily by bacteria. The bacteria utilize oxygen during the decomposition process and if the water column is stratified, i.e. no mixing of oxygen from the atmosphere to the bottom waters, this microbial respiration may reduce oxygen concentrations to levels of hypoxia or anoxia (zero oxygen concentration). **An important point is that hypoxia cannot occur without stratification, regardless of the nutrient load.**

In the northern Gulf of Mexico the Mississippi/Atchafalaya River system delivers an average of 580 km^3 of freshwater per year to the northern Gulf of Mexico. This freshwater disperses over the denser Gulf of Mexico salt water causing water-column stratification. During the late fall, winter, and early spring the frequent passage of storms impart sufficient energy to disrupt this stratification and mix the water column. During the summer, however, storms are infrequent and stratification may persist for months at a time. During this period of stratification, hypoxic bottom waters form as a result of microbial respiration of organic matter and lack of surface and bottom water mixing. Once the passage of strong storms resume, stratification is disrupted and hypoxic conditions cease. Further exacerbating the duration of stratification is the low tidal mixing energy in the Gulf of Mexico. Thus, physical factors make this region highly susceptible to water-column stratification and subsequently the onset and maintenance of hypoxia.

The other ingredient needed for the onset of hypoxia is a sufficient amount of organic carbon in the bottom waters and sediments so that respiration of this carbon reduces oxygen concentrations to levels of hypoxia. In the eutrophication paradigm, this excess of organic carbon is derived from primary production sinking from the surface waters. This is based on work primarily in lakes and coastal regions receiving high nutrient loads, but not receiving high rates of organic carbon loading from the watershed. The Mississippi River, however, carries a large load of both nutrients and organic carbon that are input directly to the region where hypoxia forms. **Thus, it is difficult to determine whether it is nutrients and the eutrophication paradigm, river-borne organic carbon, or more likely a combination of both marine and terrestrial organic carbon that fuel the decline in bottom water oxygen concentration.**

This question would be much easier to answer if long-term Mississippi River nutrient and organic flux data, long-term records of hypoxia, and a better understanding of the mechanisms regulating the vertical flux of both marine and terrestrial organic carbon to bottom waters were in place. As determining the source of organic carbon fueling hypoxia is critical for management decisions we will focus our remarks on this issue for the majority of the review of Topic 1.

Review Comments

Page. xv. The report indicates that nitrate concentrations have doubled or tripled since the 1950s to 1960s. The authors admit that nitrate concentrations have stabilized but that trends are masked because of high variability. Carey et al. (1999) determined that there has not been a statistically significant increase in nitrate concentration during the period from 1979-1995. Table 5 from Carey et al. (1999) shows that almost all stations with data over this period show no trend in nitrate concentration (i.e.

accept null hypothesis of no change). This is an important result as it is argued by the CENR authors that the size of the hypoxia region has increased over this time period.

Page xv. The authors state that a stoichiometric nutrient ratio approaching the Redfield ratio has increased offshore primary production. This is not necessarily the case as production is defined as biomass/time. Thus, increased production occurs by increasing nutrient concentrations (i.e. increasing the mass of nutrient) not necessarily by approaching the Redfield ratio. However, a changing Si:N ratio will affect diatom production.

Page xvi. The authors state that "Although the Mississippi and Atchafalya Rivers discharge organic matter to the shelf, the principal source of carbon reaching the bottom waters in the northern Gulf influenced by the river effluent and characterized by hypoxia is from *in situ* phytoplankton production." This point is debatable (see the attached excerpt from Carey et al. 1999)

Page xvi. The authors state that there is no evidence for century long changes in freshwater discharge. We do not understand why they state this as they later discuss how the Atchafalaya River has increased from 15% of the total flow in 1900 to 30% of the total flow in the present. Further, Carey et al. (1999) determined that total discharge from the river system has increased by 11% since 1967. The increase in streamflow was also noted in the Topic 3 report. "Streamflow in the Mississippi River also increased about 30% during 1970-83 as a result of increased precipitation. The increase in nitrate flux to the Gulf is attributed to both an increase in nitrate concentration and an increase in streamflow." (Topic 3, page 13)

The authors state that "The maps of mid-summer extent of hypoxia provide a benchmark for year-to-year comparisons, albeit they are minimal estimates of the total amount of seabed subjected to hypoxia and not necessarily representative of conditions through the summer."(Page 22) This point is very important for 2 reasons, first, the temporal resolution of these data is poor and more monitoring cruises are needed to temporally describe the extent of hypoxia and, second, these data maps are insufficient to adequately describe a year-to-year trend.

Page 32. There is a strong statistical correlation between river flow and the area of mid-summer hypoxia for the years prior to the flood of 1993. After the flood this relationship does not hold and the authors suggest that organic carbon burial in the sediments during the flood of 1993 contributed to the extensive area of hypoxia seen in 1994. The authors do not indicate whether they believe this carbon is of marine or terrestrial origin. However, as organic carbon of marine origin (phytoplankton) is generally a very labile source of carbon one would expect that this carbon would be respired quickly by the microbes. If this is indeed the case, then terrestrial organic carbon would be the primary form stored in the sediments. Further, the flood of 1993 carried large loads of both nutrients and organic carbon. USGS data from Alton/Grafton, Missouri from 1993 show that **total organic carbon flux at this station was 7x higher than the average for the previous five years** (Carey et al. 1999). Respiration of this terrestrial organic carbon in addition to marine organic carbon produced in 1994 may have contributed to the more extensive hypoxia seen in 1994 under lower flow conditions. These results indicate the difficulty in determining cause and effect relationships for hypoxia in this system.

Page 35-43. The authors sum up this section well with the closing sentence on p.43. As oxygen dynamics are a function of both physical and biological processes, more integrated studies of the type the authors have been involved in the past (e.g. NECOP and LATEX) are needed. More work is needed to describe inter-annual variation in the development of the secondary pycnocline, which is thought to control the morphology of the hypoxia region (Wiseman et al. 1997). Without an

understanding of this variation it will be extremely difficult to predict the magnitude of hypoxia based on river discharge or nutrient and organic carbon loading.

Page 45. Peak flow generally occurs in March, April, and May. So the delivery of solutes and particulates associated with peak flow coincides with warming of surface waters and concomitant increases in microbial activity associated with increasing temperature. Trefry et al (1994) documented that 80% of the annual total organic carbon flux occurs between December and June. These data along with N flux data, discussed on page 55 of Topic 1, indicate that both organic carbon flux and N flux are primarily a function of discharge. **Stratification, which is the ultimate driver of hypoxia, is also a function of discharge. As area of hypoxia, nutrient concentrations, and organic carbon concentrations all covary with discharge it is difficult establish statistical correlations among these variables.**

Page 51. The authors report that average nitrate concentrations have doubled from the time period 1955-1970 to 1980-1996. The authors say most of this change occurred between 1970-1983 and the mean annual N flux has not changed since the early 1980's. However, the authors chose to ignore that the N flux is declining as illustrated in Figure 2 and Figure 3.

Page 53. Table 1 in the report documents a significant increase in dissolved inorganic nitrogen concentration from the early 1960s to the middle 1980s.

Page 63. "Although the Mississippi River discharges organic matter and decomposition of some of this organic matter could consume oxygen in the coastal ecosystem, the principal source of organic matter reaching he bottom waters of the northern Gulf of Mexico influenced by the Mississippi River effluent and characterized by hypoxia is from *in situ* phytoplankton production (Rabalais et al. 1992b, Turner and Rabalais 1994a, Eadie et al. 1994, Justic' et al. 1996, 1997)." See the attached excerpt from Carey et al. (1999) for a rebuttal to this argument.

Page 63-64. Regarding the last two sentences on the bottom and continuing to the top of p. 64: We do not understand this argument nor do we agree with it if they mean what they appear to say. They say that the C:N ratio of marine phytoplankton is higher than the C:N ratio of Mississippi River flux and thus sedimenting phytoplankton cells would contribute much more carbon to hypoxic zones than riverine organic matter. However, this is not a correct comparison as they compare the C:N ratio of phytoplankton (9.5-9.9:1) to the total-C:total-N ratio of the river which includes both the dissolved and particulate fractions. If you look only at the particulate organic matter fraction in the river (which is a more appropriate comparison to the phytoplankton C:N) the C:N ratio averages 8.5:1 (Trefry et al. 1994). Further, sedimenting phytoplankton cells are a small fraction of the vertical organic carbon flux (Qureshi 1995, Figure 55 on page 82 of the report). Even so, the C:N ratio of the organic matter is not an appropriate number for justifying the importance of marine vs. terrestrial organic carbon vertical flux.

Location	Regression	# of Observations	Degrees of freedom	r	T1	Accept or Reject Null Hypothesis	Years analyzed	Average conc. mg/L
Mississippi River								
Clinton, Iowa (1986)	NO3 vs time	46	45	0.0254	-0.1886	accept	Sept 79 to Sept 86	1.241
Clinton, Iowa (1991 on)	NO3 vs time	76	75	0.2044	-1.8079	accept	Apr 91 to Sept 93	2.263
Royalton	NO3 vs time	89	87	0.1007	0.9444	accept	Sept 79 to Mar 95	0.200
Minnesota R.	NO3 vs time	161	159	0.2167	2.7849	Reject	Jan 73 to Mar 95	
St. Paul	NO3 vs time							0.917
Nininger	NO3 vs time	69	67	0.2540	2.1492	Reject	Nov 79 to Mar 95	2.459
Winona	NO3 vs time	45	43	0.1727	1.1496	accept	Jan 73 to Aug 86	1.441
Wisconsin R.	NO3 vs time	111	109	0.1526	1.6116			
Clinton	NO3 vs time					Reject	Sept 79 to Sept 93	1.921
Keokuk	NO3 vs time	47	45	0.0133	0.0892	accept	Oct 79 to Aug 88	2.359
Illinois R.	NO3 vs time							
Alton/Grafton	NO3 vs time	129	128	0.0312	-0.3516	accept	Sept 79 to Sept 94	2.676
Missouri R.	NO3 vs time							
Ohio River	NO3 vs time							
Memphis	NO3 vs time	79	78	0.0016	-0.0136	accept	Oct 79 to Aug 94	1.520
Arkansas City	NO3 vs time	74	72	0.0432	-0.3689	accept	Nov 74 to Feb 92	1.366
Vicksburg	NO3 vs time	85	84	0.0846	-0.7732	accept	Oct 79 to Aug 94	1.480
Belle Chasse	NO3 vs time	122	121	0.0189	0.2074	accept	Sept 79 to June 95	1.420
Illinois River								
Valley City, Ill	NO3 vs time	104	102	0.1082	1.0994	accept	Sept 79 to Sept 93	3.837
Wisconsin River								
Muscoda, Wisc	NO3 vs time	111	109	0.1526	1.6116	accept	Sept 79 to May 95	0.525
Minnesota River								
Jordan, Minn	NO3 vs time	50	48	0.3312	-2.4321	Reject	Jan 73 to Aug 77	0.917
Ohio River								
Wheeling, W. Va.	NO3 vs time	103	102	0.0113	0.1135	accept	Aug 79 to Aug 95	0.923
Warsaw, Ky	NO3 vs time	43	42	0.0554	0.3555	accept	Sept 79 to Aug 88	1.166
Grand Chain, Ill	NO3 vs time	170	169	0.0881	-1.1461	accept	Sept 79 to June 95	1.002

Table 5. Results of trend analysis of dissolved nitrate in the Mississippi River and its tributaries. Data from USGS.

Page 63. "The amount of organic loading in the Mississippi River is not large enough to account for the observed decline in oxygen over such a large area and volume (Turner and Allen 1982b).". We agree with this statement. Surely, oxygen depletion is due to microbial respiration of both phytoplankton organic carbon and riverine organic carbon. There is too much uncertainty about the ultimate fate of terrestrial organic carbon to discount it completely.

Page 71. The authors state that there is no clear relationship between bottom water chlorophyll concentration (surrogate for phytoplankton concentration) and hypoxia. This result again suggests the difficulty in determining cause and effect relationships for hypoxia.

Page 78. Second paragraph. The authors state that the spatial and temporal switching of the primary nutrient (N, P, or Si) limiting phytoplankton production is poorly understood. This will make it extremely difficult to implement effective TMDL management plans aimed at reducing phytoplankton production rates. This is an area of research that requires a lot more work.

Page 82. Figure 55 shows the total organic carbon flux to depth split into three fractions: fecal pellets, phytoplankton, and other material. While zooplankton fecal pellets (which may or may not contain a large fraction of phytoplankton carbon depending on location and time of year) make up 55% of the vertical organic carbon flux to the bottom, it is also apparent from figure 55 that the other material is the dominant fraction collected in the surface sediment traps and is often the dominant fraction in the bottom traps. These results also show that often the other material is the dominant fraction in the surface traps but is greatly reduced in the bottom traps suggesting that this material is rapidly respired as it sinks through the water column and thus makes a significant contribution to oxygen utilization rates in the water column. The authors state that this other material consists of molts, dead zooplankton, marine snow (particle aggregates colonized by bacteria), and particles with adsorbed organic carbon but do not discuss the percentage contribution of each fraction within the other material category.

Page 86. Section 6.8. We think this is one of the strongest portions of the report and contains good theory and convincing data that changing Si:N affects the vertical transport of phytoplankton organic carbon to depth.

Page 91. We do not follow their calculations at the top of this page. They calculate that if N fluxes were 50% lower then this flux would support a daily production rate of 0.29 g C m^{-2} d^{-1}. They then say that this translates to an annual production rate that would not exceed 25 g C m^{-2} yr^{-1}. We do not know how they calculated this number as converting this number to a yearly rate yields 0.29 x 365 = 105.85 g C m^{-2} yr^{-1}. They then use an annual production value of 35 g C m^{-2} yr^{-1} to make further calculations. We are confused about where they got this number as well.

Page 104. Figure 66. The $\delta^{13}C$ data used to determine the marine or terrestrial signal of sediment organic carbon should be reevaluated in light of the recent work by Goñi et al. (1997). See attached excerpt from Carey et al. (1999).

Page 112. Figure 72. This figure showing geological (glauconite abundance) and biological (diversity indices) data in relation to fertilizer use from 1900-1990 suggests that hypoxia has increased in duration and intensity as fertilizer use increased throughout the century. However, a correlation in these numbers does not necessarily mean there is causation.

Excerpts from:

Carey, A.E., J.R. Pennock, J.C. Lehrter, W.B. Lyons, and J.-C. Bonzongo (1999). The Role of the Mississippi River in Gulf of Mexico Hypoxia. Final Technical Report. Environmental Institute Publication Number 70. Prepared for the Fertilizer Institute by The University of Alabama, Tuscaloosa, Alabama. 119 pp.

Q: What are the patterns for inorganic nutrient ratios and why are these ratios important?

A: The ratios between the major ions (nitrogen, silicate and phosphorus) have been shown to have significant influence on phytoplankton species composition in aquatic systems. Turner and Rabalais have shown that the nitrogen:silicate ratio in the lower Mississippi River has approached 1:1 in recent years and suggested that this might have a major impact on phytoplankton species composition and hypoxia.

Some studies (Dortch and Whitledge, 1992; Justic *et al.*, 1995a) suggested silicate limitation of phytoplankton production at salinities less than 30% on the Louisiana shelf. These studies were based on stoichiometric relationships of Si:N:P. Phytoplankton cells should have Si:N:P ratios close to the Redfield ratio of Si:N:P of 16:16:1. If the Si:N ratio drops below 1:1 (16:16) diatom production will become Si limited because the ratio of cellular demand for Si is greater than that available in the water column. This analysis assumes that all other macro- and micronutrients required for growth are available in excess. Generally, it is difficult to assess nutrient limitation based solely on Si:N:P ratios because standing stock nutrient concentrations do not adequately represent uptake and regeneration rates. For example, Smith and Hitchcock (1994) determined the turnover time of the inorganic dissolved P pool in the Mississippi River plume surface waters during a summer high flow event was less than 10 minutes. Furthermore, Si is utilized only by diatoms and thus does not limit production by the non-diatom phytoplankton community.

Silicate is important however in determining the species composition of the phytoplankton assemblage (i.e., if silicate is present, diatoms are present). Corollary evidence for potential silicate limitation is that NO_3 concentrations in the Mississippi River have doubled while silicate (Si) concentrations decreased by half during the past century (Turner and Rabalais, 1991). As a result of these changes, the Si:N ratio in the Mississippi River changed from 4:1 to 1:1. This change resulted in the current Si:N:P ratio equal to the Redfield ratio of 16:16:1 (Figure 12). The trend of nutrient concentrations moving towards the Redfield ratio indicates that the Northern Gulf of Mexico phytoplankton are growing under ideal nutrient concentrations and thus their production has increased over time (Justic *et al.*, 1995a; Rabalais *et al.*, 1996).

Q: Have changes in phytoplankton species composition occurred and is this significant to the hypoxia issue?

A: There is some evidence that phytoplankton species composition has changed in favor of lightly silicified and non-diatom species during certain periods of the year. Some authors have argued that this shift may increase the input of phytoplankton-derived organic matter to the sediments on the Louisiana Shelf. However, lightly silicified diatoms and flagellates would probably be less likely to sink than more heavily silicified diatoms. Similarly, flagellates are less preferred by grazers and thus would be less likely to be incorporated in fecal pellet flux (a major source of organic input to the sediments on the shelf).

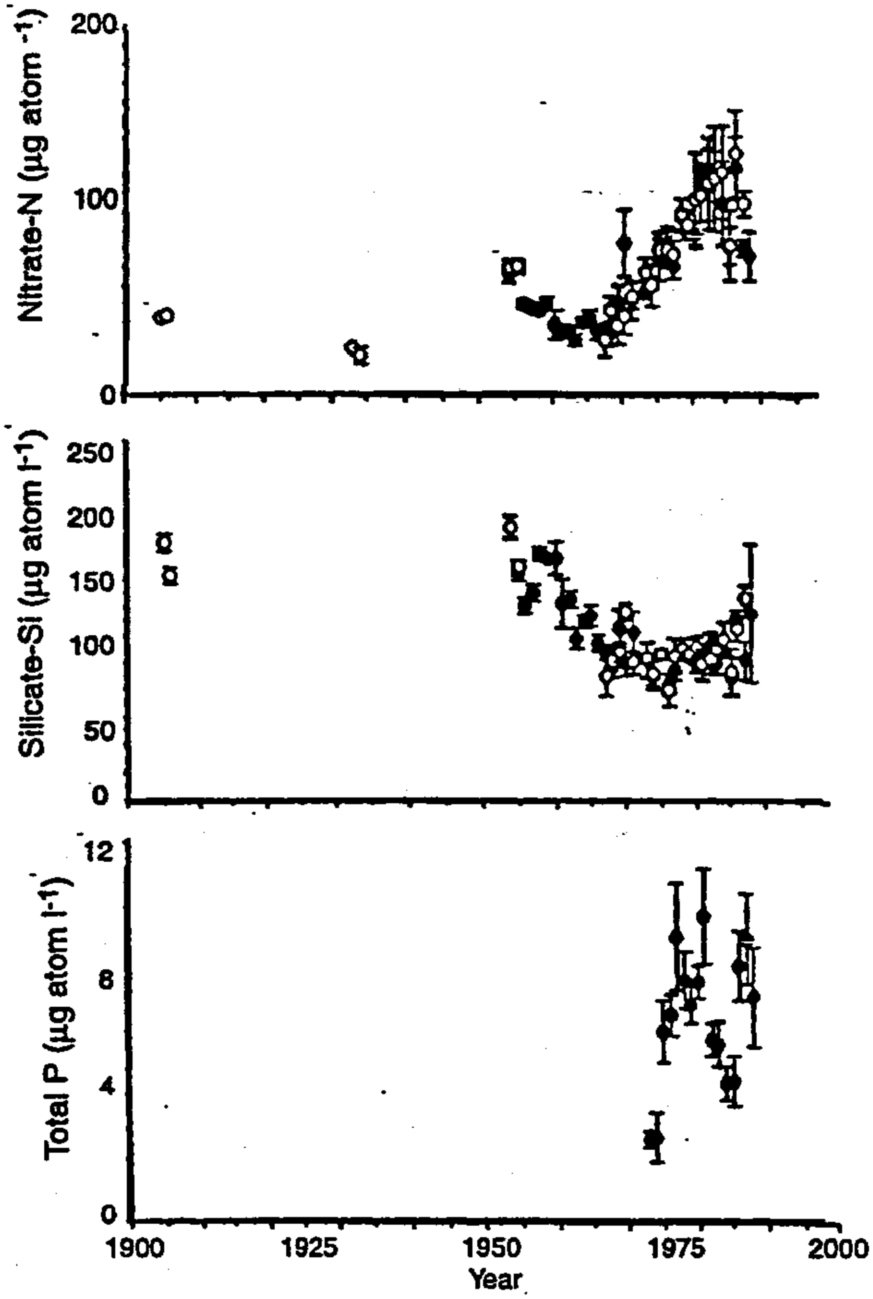


Figure 12. Nitrate, silicate and phosphorus concentrations measured at New Orleans (open circles) and St. Francisville (shaded circles). +/-1 standard error bars are indicated for the annual average of monthly means. Figure adapted from Turner and Rabalais (1991)

Another possible consequence of the change in nutrient stoichiometry and possible silicate limitation is that more non-diatom blooms may occur. This could lead to an increase in the number of non diatom harmful algal blooms (Justic *et al.*, 1995b). Further, Rabalais *et al.* (1996) postulated that the change in

Si:N has altered the species composition of the phytoplankton assemblage. In such a scenario, a shift from palatable diatoms to less palatable flagellates could exacerbate organic carbon sedimentation in the form of phytoplankton cells. The increased magnitude of phytoplankton cell sedimentation would result from decreased grazing by the zooplankton community (Officer and Ryther, 1980). Thus, a shift from diatoms to flagellates would exacerbate hypoxia with more organic carbon input to depth.

Direct comparisons of published phytoplankton species surveys from the past with those of the present are difficult as a result of different sampling techniques, sample locations, and seasons sampled. Rabalais *et al.* (1996) grouped phytoplankton species data from 1955-57, close to the delta (Simmons and Thomas, 1962) along with 1972-73 data from 80 km west of the delta (Fucik, 1974; Ward *et al.*, 1979) and compared these data to 1990-93 data collected near the delta and south of Terrebonne Bay in 20m deep waters. Rabalais *et al.* (1996) acknowledged that early phytoplankton species studies probably did not include small phytoflagellates and cyanobacteria. Thus, no data are available to determine if small phytoplankton cells, which typically dominate current samples, have increased in numbers as a result of silicate limitation. Rabalais *et al.* (1996) reported a decrease in the presence and dominance of heavily silicified diatoms and an increase in the presence and dominance of lightly silicified diatoms. Rabalais *et al.* (1996) also found that some harmful algal species were present which in the past either had been absent or were present in much smaller populations. *Pseudonitzchia pseudodelicatissima* (a sometimes-toxic form of *Nitzchia pungens*) causes amnesic shellfish poisoning. Its concentration has increased from the 1950's to the present, with some bloom concentrations exceeding 1×10^6 cells L^{-1} (Rabalais *et al.*, 1996). *Dinophysis caudata* (a dinoflagellate which may cause diarrhetic shellfish poisoning) was not present in the early studies but has recently been found at concentrations sometimes exceeding 1×10^5 cells L^{-1} (Rabalais *et al.*, 1996).

The discussed shift in phytoplankton species assemblage since the 1950's could affect hypoxia in two different ways. First, the shift from heavily silicified diatoms to more lightly silicified diatoms may have decreased the sedimentation of diatom cells (Dortch *et al.*, 1992b). Second, the shift from palatable diatoms to unpalatable flagellates may have increased the flux of organic material through sinking of ungrazed phytoplankton cells (Officer and Ryther, 1980). This hypothesis, however, precludes the increased awareness of the microbial loop and the importance of microzooplankton in the removal of phytoplankton cells.

Organic Matter Vertical Flux and the Fate of Phytoplankton

Q: What is the role of fecal pellet flux in the deposition of phytoplankton carbon on the sediments of the Louisiana shelf?

A: The packaging of phytoplankton into zooplankton fecal pellets is an important mechanism by which phytoplankton carbon can be transported to the sediments. Sediment traps on the Louisiana shelf often show fecal pellets to be a high percentage of the total material collected in these traps.

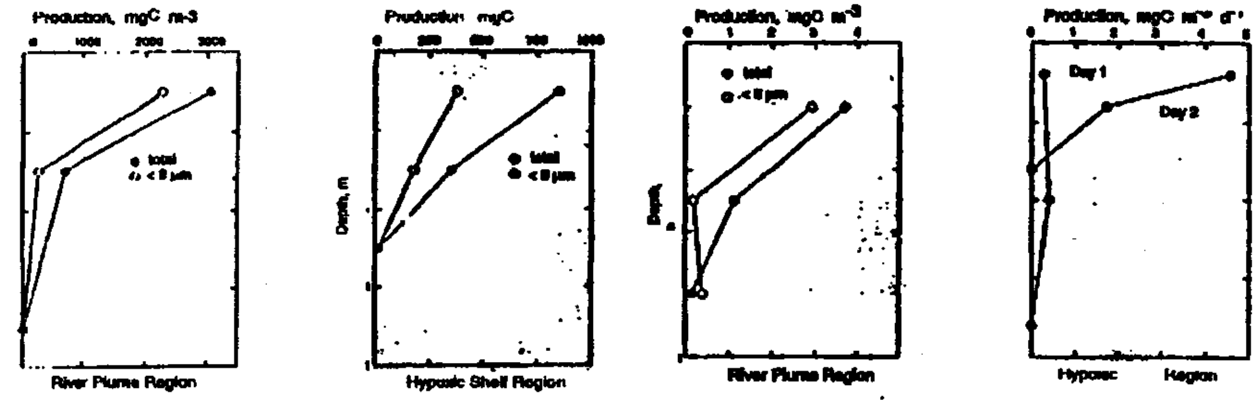
As discussed above, the onset of hypoxia is dependent on both stratification and the presence of organic matter to fuel respiration. Research in the Northern Gulf of Mexico has implicated the vertical flux of organic matter derived from phytoplankton production as the primary source of organic matter fueling respiration below the pycnocline (Turner and Rabalais, 1991; Rabalais *et al.*, 1996). The mechanisms they proposed for vertical transport of phytoplankton-derived organic matter were the direct sinking of phytoplankton cells or indirect sinking of organic matter of phytoplankton origin as zooplankton fecal pellets.

The distinction between these two types of phytoplankton sinking is important. The sedimentation rate of phytoplankton cells could be altered by changes in the phytoplankton species assemblage caused by shifts in nutrient stoichiometries and concentrations. For diatoms, sedimentation is a significant loss term, especially for large colonial diatoms like *Thalassiosira rotula* and *Skeletonema costatum*. These two species had sinking rates of greater than 20 m day^{-1} and 4-13 m day^{-1}, respectively (Fahnenstiel *et al.*, 1995). Sedimentation of diatoms was also much higher in the spring when maximal sedimentation represented a 200% loss compared to the summer when sedimentation represented a 20% loss (Fahnenstiel *et al.*, 1995). Sedimentation can represent a large loss term for diatoms but for non-diatoms this loss term is less than 1% (Fahnenstiel *et al.*, 1995). For the phytoplankton community as a whole, sedimentation is not a significant loss term. Redalje *et al.* (1994) determined that the less than 8 µm portion of the phytoplankton community (non-diatoms) accounts for most of the total primary production (Figure 28). In addition, sinking of phytoplankton cells composed only a small percent of the total organic carbon flux to depth, averaging 4% of the vertical TOC flux (Qureshi, 1995).

Qureshi's (1995) sediment trap data indicate that zooplankton fecal pellet production by the macrozooplankton (zooplankton >200 µm) is the primary mechanism for the flux of organic matter to depth. Zooplankton fecal pellet sedimentation accounts for an average of 55% of the TOC flux but at times fecal pellet organic carbon flux is greater than 95% of the TOC flux (Figure 29). Turner and Rabalais (1991) and Rabalais *et al.* (1996) hypothesized that vertical flux of phytoplankton-derived organic material was the cause of hypoxia and that the mechanism for this flux is zooplankton fecal pellet production by the macrozooplankton community.

However, this hypothesis is problematic because macrozooplankton are not the dominant herbivores, neither in the Mississippi River plume (Dagg, 1995) nor away from the plume (Fahnenstiel *et al.* 1995). Measured macrozooplankton community ingestion rates in the Mississippi River plume in September 1991 accounted for 14-62% of the daily phytoplankton production while in May 1992, macrozooplankton community ingestion rates accounted for only 4-5% of the daily phytoplankton production (Dagg, 1995). Alternatively, Fahnenstiel *et al.* (1995) found that the primary fate of small, non-diatom phytoplankton (responsible for the majority of phytoplankton production) is to be grazed upon by the microzooplankton community (zooplankton <200 µm). Microzooplankton grazing accounted for an average of 82% of the production of the non-diatoms.

Dagg (1995) suggested that macrozooplankton do not become important herbivores until they are able to respond numerically to increased phytoplankton concentrations. Both the microzooplankton grazing rates obtained by Fahnenstiel (1992; 1995) and the observation that



(a) US CMS NECO March 990

(b) USM CMS COP March 99

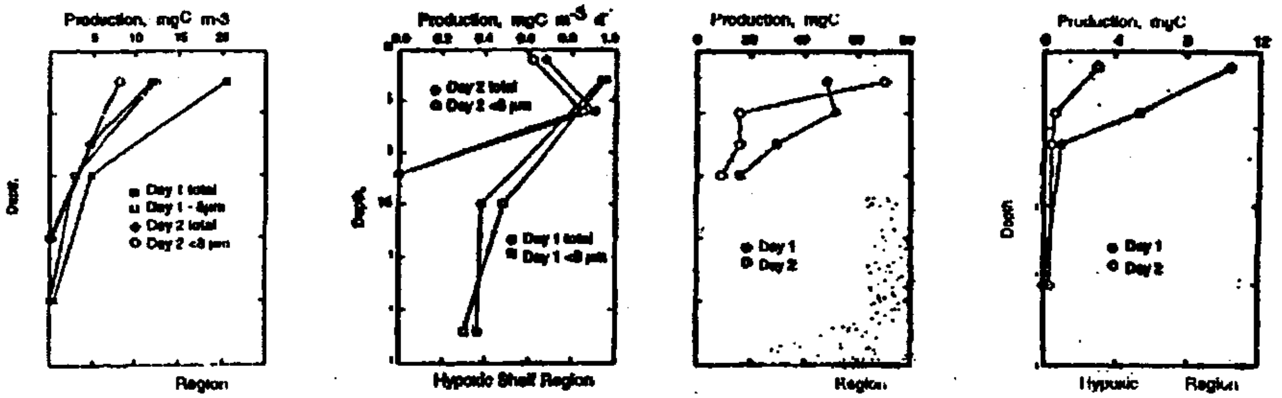


Figure 28. Size-fractioned phytplankton production for phytoplankton < 8µm and phytoplankton > 8µm in size.
Figure adapted from Redalje et al. (1994)

microzooplankton often grow at rates equal to or greater than phytoplankton and thus can numerically respond in kind to increases in phytoplankton abundance (Banse, 1982) suggest that the macrozooplankton are not the dominant herbivores on the Louisiana-Texas Continental Shelf. This suggests the possibility that the primary source of carbon contributing to macrozooplankton fecal pellet deposition may not be directly of phytoplankton origin.

It is likely that the macrozooplankton community subsist primarily on microzooplankton (Gifford and Dagg, 1991). In addition, particles in the Mississippi River plume may serve as a minor carbon source. These particles have been found in fecal pellets from copepods collected near the Mississippi River plume (Turner, 1984). If, as indicated, macrozooplankton are not the dominant herbivores, then the primary mechanism for vertical carbon flux in the plume is not the transfer of nutrients to phytoplankton to zooplankton to organic flux. Herbivory by the microzooplankton community and the transfer of organic carbon through microbial communities to higher trophic levels have not been considered.

Also at times the flux of zooplankton fecal pellets and phytoplankton cells constitutes an insignificant contribution to the vertical TOC flux (Figure 29). Qureshi's data showed that direct flux of terrestrial organic carbon (sinking of terrestrial particulate organic carbon) to depth varies seasonally; at times it represents a significant fraction of the organic carbon flux to the benthos. Trefry *et al.*'s (1994) results (Figure 30) showed that at depth particulate terrestrial organic carbon concentrations from the river can be similar to particulate marine organic carbon concentrations. Although the type of non-fecal pellet/non-phytoplankton organic carbon in the sediment traps is not described, a possible source for the organic carbon's origin is from the river. With the above data in mind it is difficult to accept the hypothesis that nutrient enriched phytoplankton production is the primary mechanism fueling the observed variability in bottom water oxygen concentrations.

Q: Is phytoplankton carbon the major proportion of fecal pellet carbon?

A: No, our calculations suggest that phytoplankton carbon constitutes less than half of the fecal pellet carbon produced.

An analysis of data concerning organic carbon flux to depth and food web dynamics on the Louisiana-Texas Shelf suggests that phytoplankton organic carbon supplies less than half of the organic carbon deposited by fecal pellets (Appendix 3). Though these are admittedly crude calculations, they lead to an important question: What is the source of the missing carbon and why has it been overlooked? Terrigenous organic carbon has previously been considered too recalcitrant to be of use as an energy source. However, Ittekott's (1988) and Amon and Benner's (1996) work suggest that organic carbon from rivers can be more labile than previously thought and bacteria can readily utilize this carbon for energy. Further, terrigenous organic carbon as an organic carbon source for the Northern Gulf of Mexico food chain invokes the microbial food web as an important aspect of energy transfer to higher trophic levels.

Even though the concepts of energy transfer to higher trophic levels by the microbial food web have been in place for 25 years (Pomeroy, 1974), the importance of energy transfer from organic carbon to bacteria to higher trophic levels is not easily quantified in marine ecosystems. Recent studies by limnologists (Laybourn-Parry *et al.*, 1994) in Loch Ness indicate the microbial food web can subsist almost entirely on allochthonous carbon sources (carbon produced outside of the system). Studies like this and evidence that zooplankton can exploit detrital organic carbon (Bowen, 1984), bacteria (Porter *et al.*, 1983) and protozoans (Gifford and Dagg, 1991) as carbon sources point to the quantitative significance of terrigenous organic carbon and the microbial food web as an energy conduit to higher trophic levels in ecosystems with high external organic carbon loading rates. Based on these considerations and our calculations (Appendix 3), terrigenous organic carbon in the plume and on the shelf may be a significant source of carbon input into the coastal food chain via the transfer of organic carbon through the microbial loop to higher trophic levels.

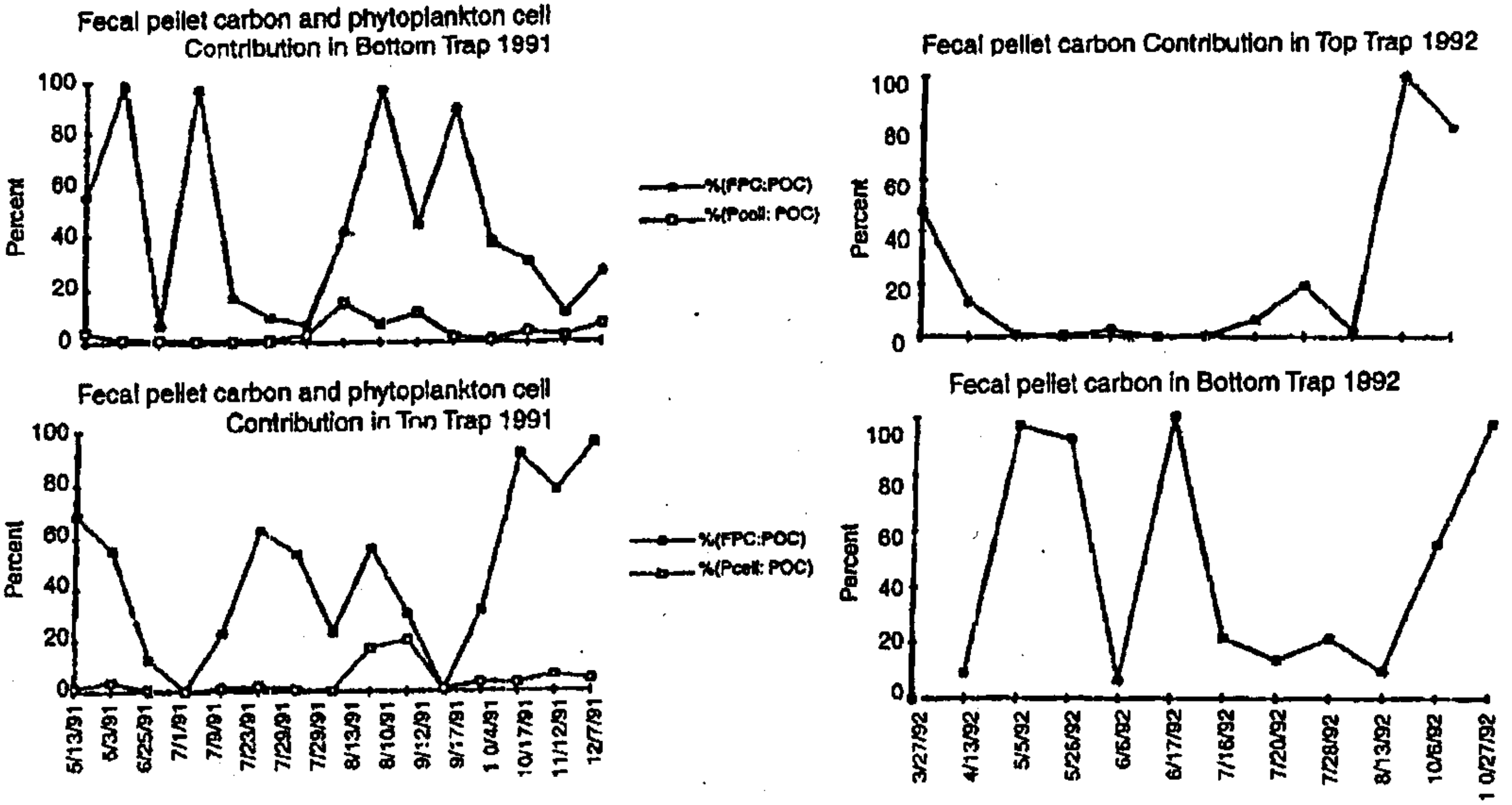


Figure 29. Fecal pellet carbon flux percent contribution to total POC flux % (FPC:POC) and phytoplankton carbon flux percent contribution to total POC flux % (P_{cell}:POC). Organic carbon flux was determined from sedimentation to the surface (5-6 m depth) and bottom (15 m depth) water sediment traps at stations C6B which is located about 20 km south of Terrebonne Bay. Figure adapted from Qureshi (1995).

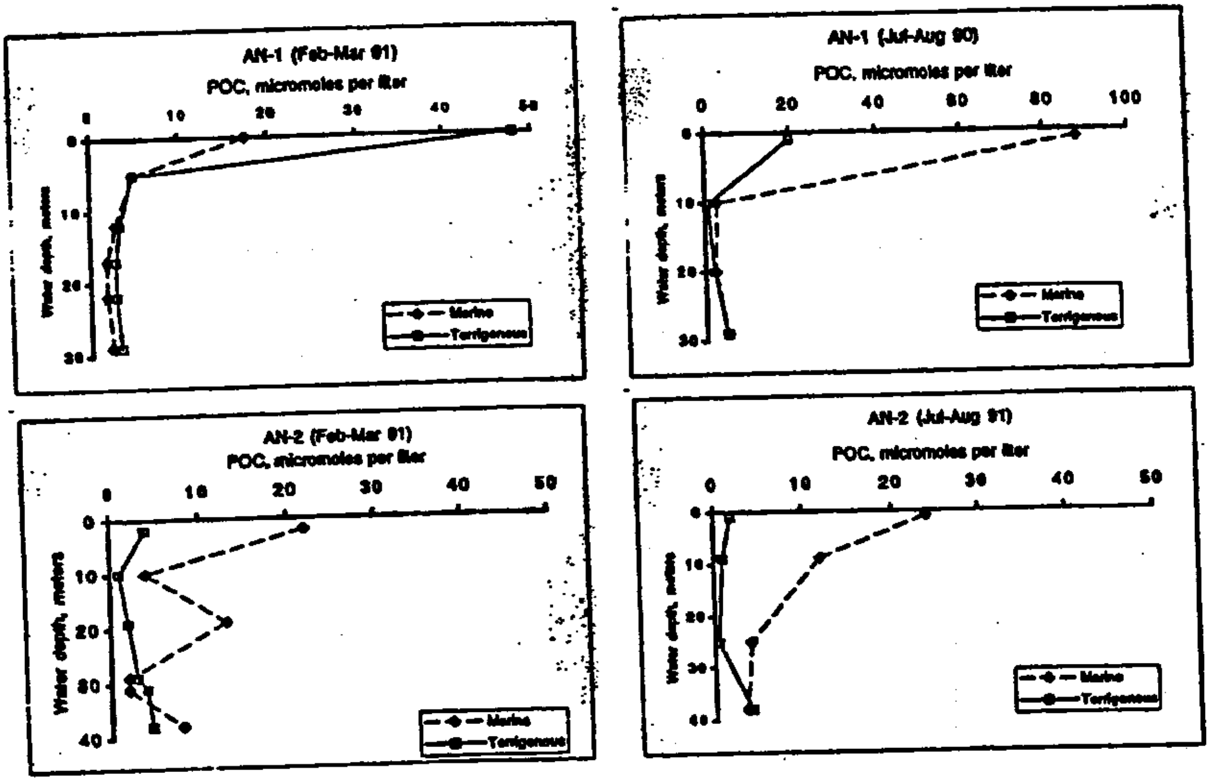


Figure 30. Marine and terrigeous particulate organic carbon concentrations measured by Trefrey *et al.* (1994). Station AN-1 is just west of Southwest Pass. Station AN-2 is approximately 20 km west of AN-1.

Q: How does organic carbon transport in the river affect hypoxia in the Gulf?

A: There is a significant amount of evidence that there is a large export of organic carbon from terrigenous sources to the Louisiana Shelf, particularly during major flooding events. Recent papers contradict earlier stable isotope analyses and suggest that a larger portion of the organic carbon in the sediments is from terrestrial sources than previously thought. Insufficient information is available to assess the ultimate sink for this material.

It is easy to understand why the terrigenous organic carbon has been overlooked as a source of organic carbon to the Northern Gulf of Mexico ecosystem. One major gap in determining the role of terrestrial organic carbon in Gulf hypoxia is that it has been unclear exactly how much organic carbon actually is introduced onto the continental margin from the river and what percentage of organic carbon on the shelf was actually allochthonous. Early investigations of organic carbon transport in the Mississippi River included work performed by Riley (1937). Ratios of organic matter to chlorophyll suggested to Riley that the river carried "large quantities of organic detritus." More recent studies, before and after the 1993 flood, suggested that riverine sedimentary organic matter can be transported long distances downstream during such events (Barber and Writer, 1998).

Trefry *et al.* (1994) estimated the total organic carbon (TOC) flux from the Mississippi River and Atchafalaya River to be 5.76×10^{12} g yr^{-1}. Of this total, they reported 66% of the TOC flux was particulate organic carbon (POC) and 34% was dissolved organic carbon (DOC). Trefry *et al.* (1994)

estimated that 80% of the annual TOC flux from the Mississippi/ Atchafalaya River System to the Northern Gulf of Mexico occurred from December to June. They also showed that the POC flux was linearly related to the total suspended matter of the lower Mississippi River, indicating that POC flux was linearly related with Mississippi River discharge. The %POC in Mississippi River particles also varied with total suspended matter. At total suspended matter concentrations greater than 50 mg L^{-1} the fraction as POC averaged 1.8% but when the total suspended matter was below 50 mg L^{-1}, the fraction of POC increased dramatically to as high as 7.7% (Trefry *et al.*, 1994).

Ludwig *et al.* (1996) suggested that 5-10x10^{12} g of terrestrial organic carbon (dissolved plus particulate) is introduced into the Gulf of Mexico annually in the region that encompasses the Mississippi delta. These data are similar to those of Trefry *et al.* (1994) discussed above. Using the mean instantaneous flux of organic carbon of 102 kg per second calculated from USGS data at St. Francisville, Louisiana over the period 1973 through 1995, we have determined a mean flux of 2x10^{12} grams of carbon per year. Clearly, all three calculations yield fluxes of 10^{12} to 10^{13} grams carbon per year. In fact, additional calculations (using USGS data) show that the total organic carbon flux at Alton/Grafton, Missouri in 1993 was 85x10^{12} g, approximately 7x greater than that of the average of the 5 previous years. Although these data are well upstream from the Gulf of Mexico, they might suggest that during flood years even higher fluxes of organic carbon from the river onto the shelf could occur.

Carbon isotopic studies of Gulf of Mexico continental shelf sediments, up until 1997, had concluded that 50% or more of surficial organic matter derived from marine or phytodetrital sources (Hedges and Parker, 1976; Gearing *et al.*, 1977; Eadie *et al.*, 1994). However, this idea came into doubt when Goñi *et al.* (1997) pointed out that the ^{14}C "ages" of surface (0-2 cm) sediments are very old (~2600 to 6800 years).

This recent work (Goñi *et al.*, 1997) on stable carbon isotopes (δ–^{13}C) in organic compounds of the continental shelf sediments has distinguished between organic carbon whose source was terrestrial detritus and that of marine production. These authors concluded that many previous studies have underestimated the amount of organic matter in marine sediments, which originated as terrestrial plant detritus, particularly from C$_4$ grasses which have stable isotope signatures similar to that of marine organic carbon. Goñi *et al.*'s compound-specific stable isotopic analyses of Gulf of Mexico sediments suggest that organic matter in nearshore Gulf sediments originates from C$_3$ plants from forests and swamps, but that the offshore compounds derive from grasses (C$_4$ plants) originating from the Mississippi River basin. They state that "quantitative assessments of terrigenous organic matter inputs using bulk δ–^{13}C$_{oc}$ are difficult to accomplish and may have led to an underestimation of allochthonous contributions." In fact, no simple mixing model, using only one terrestrial and one marine endmember, can explain the bulk δ-^{13}C data (Ruttenberg and Goñi, 1997). In addition, the organic carbon in suspended matter in the Lower Mississippi and Atchafalaya Rivers yielded δ-^{13}C values of the –19% to –24% observed in surface sediment on the shelf (Onstad *et al.*, in review, as quoted by Goñi *et al.*, 1998). Ruttenberg and Goñi (1997) also observed that at stations in shallow water depths on the shelf (<600 m) have C:P and C:N ratios higher than Redfield ratios, which indicates a strong terrestrial signal (Ruttenberg and Goñi, 1997). Using Gulf of Mexico shelf and slope sediment cores collected in 1987, Goñi *et al.* (1998) have recently shown the importance of land-derived C$_4$ plant (corn and grasses) organic matter in these sediments. They also conclude that this material is old, highly degraded and has been cycled through the soil several times.

Goñi *et al.* (1998) considered the possibility of marsh-derived organic matter as a source of the terrigenous organic matter they observe in Gulf of Mexico sediments. They find this explanation unlikely because of the subsidence of the deltaic plain and surrounding lands which is occurring. This means that the delta should be a sink, not a source, of sediments. In addition, marsh sediments would be younger in ^{14}C-age than the old sediments they observe on the shelf and slope.

The ultimate fate of riverine-borne terrigenous component is unknown. However, the work of Keil *et al.* (1997) has shown that much of the POC entering the delta regions of large rivers like the Amazon is not buried there, but is "lost." Whether this lost POC (~70% in the case of the Amazon) is decomposed directly or is transported seaward in a dissolved form, is unclear In either case, mineralization (i.e., decomposition and the concomitant consumption of some electron donor, like O_2) must occur in relatively short times (Keil *et al.*, 1997). Sediment cores do, however, indicate an increase of organic matter accumulation from about 1925 to the present (Eadie *et al.*, 1994). This increased preservation may be due to increased burial rates (Eadie *et al.*, 1994). This needs to be reconciled with the observed decrease of the riverine input of particulate matter and allochthonous organic matter documented over this time period (Meade and Milliman, 1983; Mayer *et al.*, 1998).

How important could this allochthonous carbon flux be for benthic metabolism and O_2 consumption in the nearshore and shelf region of the Gulf of Mexico? To answer this question, we have performed a rather simple-minded calculation. We have used Dortch *et al.*'s (1994) total benthic respiration rate from the hypoxic zone obtained in 1991 (range of 160 to 800 mg O_2 m^{-2} d^{-1}). We have taken the estimated area of the hypoxic zone for that year and multiplied the equivalent amount of carbon respired by the measured benthic respiration rate (Dortch *et al.*, 1994) cited above, to produce an estimate of total benthic respiration in the hypoxic zone. This yields a carbon respiration or organic carbon consumption of 2.4 to 11.8×10^{10} moles of carbon annually. This value is quite similar both to that of Trefry *et al.* (1994) and the mean USGS flux discussed above, even if we assume that only 35% (Ittekott, 1988) of the total organic carbon is labile or oxidizable (17×10^{10} moles C and 9.2×10^{10} moles C, respectively). The respiration of terrestrially-produced organic carbon is a complex phenomenon which varies both with flow and source (Sun *et al.*, 1997; Hopkinson *et al.*, 1998). Goñi *et al.* (1998) pointed out that much of the organic matter found in the surface sediments of the shelf is highly degraded and refractory, and thus is unavailable for respiration. However, even if only 10% of the total organic carbon flux could be metabolized within the bottom waters or at the sediment-water interface, this could account for a significant portion of O_2 loss.

Recent work by Mayer *et al.* (1998) has indicated that although the dissolved inorganic nitrogen (DIN) concentrations of the Mississippi River have increased over time from 1950-52 until 1973-82, the amount of "labile" or bioavailable particulate nitrogen (LPN) has decreased. They attributed the increase of DIN to fertilizer usage increase, but they attributed the LPN decrease to decreased particle fluxes resulting from engineered structures along the river (Mayer *et al.*, 1998). If one argues, as Mayer and colleagues have, that this LPN can be rapidly solubilized and utilized by coastal phytoplankton, the actual bioavailable N introduced to the shelf from the Mississippi River may have actually decreased from 1950-52 to 1973-82 (Figure 31). Clearly,

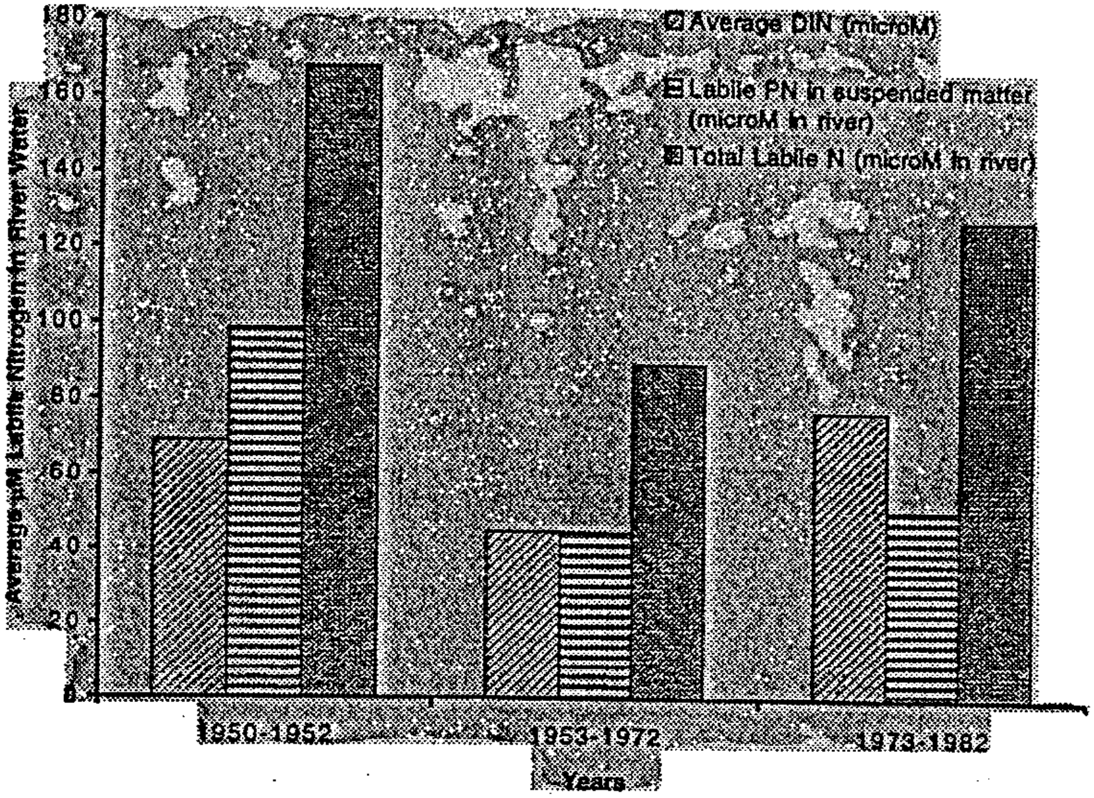


Figure 31. Average Mississippi River concentrations of inorganic nitrogen, particulate nitrogen and labile organic nitrogen. Data from Mayer *et al*. (1998).

labile N is "lost" from particulate material in the river either before or after deposition, but before burial on the shelf (Mayer *et al.*, 1998). This loss of particulate N may support the notion that a percentage of the terrestrial organic matter is labile and rapidly lost from the riverine and shelf system via respiration.

Smith and Hollibaugh (1993) suggest net heterotrophy occurs in the coastal ocean, based on terrigenous inputs of carbon. The amount of carbon input into the Northern Gulf of Mexico by the Mississippi/Atchafalaya River System suggests this fraction could contribute significantly to coastal ecosystem metabolism. This is similar to the conclusion reached by Trefry *et al* (1994). Thus, this source of organic carbon, plus the measurements indicating high ammonium regeneration rates (Dortch *et al.*, 1992a), and the bacterial utilization of a large portion of phytoplankton produced carbon (Benner *et al.*, 1992) suggest the Louisiana-Texas Continental Shelf may be net heterotrophic.

Sediment Evidence for Increased Phytoplankton Production and Hypoxia

Q: What do sediment data tell us about long-term patterns of hypoxia on the Louisiana shelf?

A: Nelsen et al. examined several sediment cores from the hypoxia region for the mineral glauconite (a mineral that is formed under reducing conditions caused by hypoxia/anoxia). These data indicate that there has been an increase in the duration of hypoxia in recent years.

As the research into areas of hypoxia and carbon flux on the Louisiana-Texas Continental Shelf only go back to the 1970's, it is difficult to determine the long term trends in riverine loading, phytoplankton production, and temporal and spatial extents of hypoxia. In order to assess changes in these parameters, researchers have used sedimentary evidence to infer the past state of the Louisiana-Texas Continental Shelf ecosystem. The main thrusts of this research have been to provide corollary evidence that 1) primary production has increased and 2) hypoxic events have increased in duration and severity.

Historical evidence for increased phytoplankton production is from studies of sediment biogenic silica concentrations (Turner and Rabalais, 1994) and sediment organic carbon concentrations and isotope abundance (Eadie *et al.*, 1994). Turner and Rabalais (1994) found sediment biogenic silica from diatom frustules had a constant concentration from 1900 to 1960 but increased dramatically from 1960 to the present. They concluded that diatom production in the plume and diatom surface to bottom flux beneath the Mississippi River plume have increased during this century. However, the possibility that dissolution of biogenic silica is at least partly responsible for these differences cannot be overlooked.

Eadie *et al.* (1994) found the δ–^{13}C of the sediment organic carbon became lighter with increasing age of the sediments. The δ–^{13}C of terrigenous POC in the Mississippi River is –25.5‰ and the δ–^{13}C of phytoplankton POC is –19.5‰. Thus, this trend indicates that the contribution of phytoplankton-derived organic matter in the sediments has increased during the past century. However, as mentioned above, this trend could also be influenced by the contribution of POC from terrestrial C_4 plants (Goñi *et al.*, 1997).

Historical evidence for an increase in the frequency of hypoxia is provided by Nelsen *et al.* (1994) and Sen Gupta *et al.* (1996). Nelsen *et al.* (1994) found that the mineral glauconite increased in abundance from the past to the present. This mineral is known to form under reducing conditions such as found in the hypoxic regions of the Northern Gulf of Mexico. The sediment core in this study that showed increased glauconite abundance is located in the Mississippi River Bight, an area known to experience persistent seasonal hypoxia (Rabalais *et al.*, 1994; Rabalais *et al.*, 1996). Glauconite's presence downcore indicates that hypoxia has occurred historically. The increase in glauconite upcore was interpreted by Nelsen *et al.* (1994) to represent an increase in the frequency and duration of hypoxic bottom water at this location. Cores taken from outside known hypoxic areas do not have glauconite present.

Nelsen *et al.* (1994) and Sen Gupta *et al.* (1996) also examined the composition of benthic foraminifera tests in the sediment cores. Benthic foraminifera can be identified by species by the morphology of their tests (shells). Benthic foraminifera species diversity decreased upcore in a core taken from an area known to experience seasonal hypoxia presently (Nelsen *et al.* 1994). There was also a shift of dominant species upcore. Species unable to tolerate low bottom water oxygen concentrations were more abundant downcore while upcore the dominant species were species known to tolerate low oxygen concentrations (Nelsen *et al.*, 1994; Sen Gupta *et al.*, 1996). Benthic foraminifera in cores taken outside of the region of hypoxia did not have shifts in species diversity (Nelsen *et al.*, 1994). This type of evidence does indicate that hypoxia has increased in duration but does not explain the cause of this hypoxia.

Appendix 3. Analysis of Fecal Pellet Carbon Origin Based on Food Web Dynamics

If we suppose that phytoplankton production on the Louisiana-Texas Continental Shelf is 290 g C m^{-2} yr^{-1} (Sklar and Turner, 1981) and also that 82% of that amount of production is ingested by the microzooplankton community (Fahnenstiel *et al.*, 1992; 1995) then

$$290 \text{ g C m}^{-2} \text{ x } 0.82 = 238 \text{ g C m}^{-2} \text{ yr}^{-1}$$

passes through the microzooplankton community.

If the microzooplankton community has a growth gross efficiency of 30% (Straile, 1997), then the microzooplankton community retains

$$238 \text{ g C m}^{-2} \text{ yr}^{-1} \text{ x } 0.30 = 71 \text{ g C m}^{-2} \text{ yr}^{-1}$$

which is of phytoplankton origin. The remainder of the organic carbon ingested by the microzooplankton is respired as CO_2 or is released as POC and DOC. The POC released by the microzooplankton will not be a major source of organic carbon flux because these units are not packaged into a pellet like the POC released by the macrozooplankton.

Based on Qureshi's (1995) phytoplankton flux measurements and Sklar and Turner's (1981) annual shelf phytoplankton production estimate, approximately 4% of the annual phytoplankton production is lost to sedimentation. This leaves 14% [100 - ($82_{\text{microzooplankton}}$ + $4_{\text{sedimentation}}$)]of the 290 g C m^{-2} yr^{-1} annual phytoplankton production available for consumption by the macrozooplankton.

If all of this remaining 14% of phytoplankton production is available for consumption by the macrozooplankton, then

$$290 \text{ g C m}^{-2} \text{ yr}^{-1} \text{ x } 0.14 = 41 \text{ g C m}^{-2} \text{ yr}^{-1}$$

of phytoplankton carbon is ingested by the macrozooplankton.

Zooplankton are known to have decreasing assimilation efficiencies with increasing food concentration (Landry *et al.*, 1984; Straile, 1997). If we take the low value of the range of zooplankton assimilation efficiencies determined by Landry *et al.* (1984), then approximately 70% of the ingested carbon is assimilated and the remaining 30% is excreted as fecal pellets. If the Louisiana continental shelf macrozooplankton consume all of the microzooplankton and all the remaining 14% of the phytoplankton, then the macrozooplankton community ingests

$$71 \text{ g C m}^{-2} + 41 \text{ g C m}^{-2} \text{ yr}^{-1} = 112 \text{ g C m}^{-2} \text{ yr}^{-1}$$

Of that 112 g C m^{-2} yr^{-1}, 70% is assimilated and 30% or

$$112 \text{ g C m}^{-2} \text{ yr}^{-1} \text{ x } 0.30 = 34 \text{ g C m}^{-2} \text{ yr}^{-1}$$

goes to fecal pellet production.

During four cruises between July 1990 and May 1992, Redalje *et al.* (1994) found that the average TOC flux was 1.015 g C m^{-2} d^{-1} in the plume and 0.296 g C m^{-2} d^{-1} on the shelf (Table 7). These daily flux rates, when converted to yearly flux rates, yield an annual plume TOC flux rate of 370 g C m^{-2} yr^{-1} and an annual shelf TOC flux rate of 108 g C m^{2} yr^{-1}.

Qureshi estimated that fecal pellet flux constituted 55% of the TOC flux. Thus, the fecal pellet flux in the plume is

Cruise	Plume Region	Shelf Region
July-August 1990		
POC flux	0.29 (0.02; n=3)	0.18 (0.01; n=4)
PON flux	0.06 (0.003; n=6)	0.03 (0.02; n=8)
March 1991		
POC flux	0.95 (0.01; n=3)	0.32 (9.02; n=3)
PON flux	0.16 (0.009; n=6)	0.05 (0.002; n=6)
Sept 1991		
POC flux	0.69 (0.02; n=12)	0.19 (0.01; n=6)
PON flux	0.12 (0.003; n=12)	0.03 (0.001; n=6)
May 1992		
POC flux	1.80 (0.04; n=8)	0.40 (0.02; n=10)
PON flux	0.27 (0.008; n=16)	0.07 (0.004; n=10)

Table 7. Vertical fluxes of POC and PON out of the photic zone. Units are grams of C or grams of N per square meter per day.
Values in parentheses indicate the standard error and the number of replicates.
Data from Redalje (1994).

$$370 \text{ g C m}^{-2} \text{ yr}^{-1} \times 0.55 = 204 \text{ g C m}^{-2} \text{ yr}^{-1}$$

and the fecal pellet flux on the shelf is

$$109 \text{ g C m}^{-2} \text{ yr}^{-1} \times 0.55 = 59 \text{ g C m}^{-2} \text{ yr}^{-1}$$

The fecal pellet flux of 34 g C m^{-2} yr^{-1} derived from phytoplankton carbon is much less than the total fecal pellet carbon flux.

For mass balance, the total fecal pellet organic carbon and the remaining fecal pellet carbon flux must be derived from terrestrial organic carbon delivered by the river and other sources of marine organic carbon.

Although this modeling exercise is based on parameters estimated from a small number of observations, the results suggest allochthonous organic carbon in the fecal pellet flux and the direct flux of particles of terrestrial origin are both important components of total POC flux to bottom waters.

References Cited

Amon, R. M., and R. Benner. 1996. Bacterial utilization of different size classes of dissolved organic matter. Limnol. Oceanogr. 41:41-51.

Banse, K. 1982. Cell volumes, maximal growth rates of unicellular algae and ciliates, and the role of ciliates in the marine pelagial. Limnology and Oceanography. 27:1059-1071.

Barber, L. B. and J.H. Writer. 1998. Impact of the 1993 flood on the distribution of organic contaminants in bed sediments of the upper Mississippi River. Environ. Sci. Technol. 32:2077-2083.

Benner, R., G. Chin-leo, W. Gardner, B. Eadie, and J. Cotner. 1992. The fates and effects of riverine and shelf-derived DOM on Mississippi River plume/Gulf shelf processes, p. 84-94, In: Proceedings, Nutrient Enhanced Coastal Ocean Productivity Workshop. Publ. No. TAMU-SG-92-109, Texas Sea Grant College Program, Texas A&M University, College Station, Texas.

Bowen, S. H. 1984. Evidence of a detritus food chain based on consumption of organic precipitates. Bull. Mar. Sci. 35:440-448.

Dagg, M. J. 1995. Copepod grazing and the fate of phytoplankton in the northern Gulf of Mexico. Contin. Shelf Res.

Dortch, Q., A. Bode, R. R. Twilley. 1992a. Nitrogen uptake and regeneration in surface waters of the Louisiana continental shelf influenced by the Mississippi River, p. 52-56. In: Proceedings, Nutrient Enhanced Coastal Ocean Productivity Workshop. Publ. No. TAMU-SG-92-109, Texas Sea Grant College Program, Texas A&M University, College Station, Texas.

Dortch, Q., D. Milsted, N. N. Rabalais, S. E. Lohrenz, D. G. Redalje, M. J. Dagg, R. E. Turner, and T. E. Whitledge. 1992b. Role of silicate availability in phytoplankton species composition and fate of carbon, p.76-83, In: Proceedings, Nutrient Enhanced Coastal Ocean Productivity Workshop. Publ. No. TAMU-SG-92-109, Texas Sea Grant College Program, Texas A&M University, College Station, Texas

Dortch, Q., and T. E. Whitledge. 1992. Does nitrogen or silicon limit phytoplankton production in the Mississippi River plume and nearby regions? Contin. Shelf Res. 12:1293-1309.

Eadie, B. J., B. A. McKee, M. B. Lansing, J. A. Robbins, S. Metz and J. H. Trefry. 1994. Records of nutrient-enhanced coastal ocean productivity in sediments from the Louisiana continental shelf. Estuaries. 17:754-765.

Fahnenstiel, G. L., M. H. Marcovitz, M. J. McCormick, D. G. Redalje, S. E. Lohrenz, H. J. Carrick, and M. J. Dagg. 1992. High growth and microzooplankton-grazing loss rates for phytoplankton populations from the Mississippi River plume, p. 111-116, In: Proceedings, Nutrient Enhanced Coastal Ocean Productivity Workshop. Publ. No. TAMU-SG-92-109, Texas Sea Grant College Program, Texas A&M University, College Station, Texas

Fahnenstiel, G. L., M. J. McCormick, G. A. Lang, D. G. Redalje, S. E. Lohrenz, M. Markowitz, B. Wagoner, and H. J. Carrick. 1995. Taxon-specific growth and loss rates for dominant phytoplankton populations from the northern Gulf of Mexico. Mar. Ecol. Prog. Ser. 117:229-239.

Fucik, K. W. 1974. The effect of petroleum operations on the phytoplankton ecology of the Louisiana coastal waters. M. S. thesis. Texas A&M University, College Station, Texas.

Gearing, P., F. E. Plucker, and P. L. Parker. 1977. Organic carbon stable isotope ratios of continental sediments. Mar. Chem. 5:251-266.

Gifford, D. J., and M. J. Dagg. 1991. The microzooplankton-mesozooplankton link: consumption of planktonic protozoa by the calanoid copepods *Acartia tonsa* Dana and *Neocalanus plumchrus* Marukawa. Mar. Microb. Food Webs 5:161-177.

Goñi, M. A., K. C. Ruttenberg, and T. I. Eglinton. 1997. Sources and contributions of terrigenous organic carbon to surface sediments in the Gulf of Mexico. Nature. 389:275-278.

Goñi, M. A., K. C. Ruttenberg, and T. I. Eglinton. 1998. A reassessment of the sources and importance of land-derived organic matter in surface sediments from the Gulf of Mexico. Geochim. Cosmochim. Acta. 62(18):3055-3075.

Hedges, J. I. and P. L. Parker. 1976. Land-derived organic matter in surface sediments from the Gulf of Mexico. Geochim. Cosmochim. Acta. 40:1019-1029.

Hopkinson, C. S., I. Buffam, J. Hobbie, J. Vallino, M. Perdue, B. Eversmeyer, F. Prahl, J. Covert, R. Hodson, M. A. Moran, E. Smith, J. Baross, B. Crump, S. Findlay and K. Foreman. 1998. Terrestrial inputs of organic matter to coastal ecosystems: An intercomparison of chemical characteristics and bioavailability, Biogeochem. 43:211-234.

Ittekkot. V. 1988. Global trends in the nature of organic matter in river suspensions. Nature 332:436-438.

Justic, D., N. N., Rabalais, and R. E. Turner. 1995a. Stoichiometric nutrient balance and origin of coastal eutrophication. Mar. Pollution Bull. 30:41-46.

Justic, D., N. N. Rabalais, R. E. Turner, and Q. Dortch. 1995b. Changes in nutrient structure of river-dominated coastal waters: Stoichiometric nutrient balance and its consequences. Est. Coast. Shelf Sci. 40:339-356.

Justic, D., N. N. Rabalais, R. E. Turner, and W. J. Wiseman. 1993. Seasonal coupling between riverborne nutrients, net productivity, and hypoxia. Mar. Pollution Bull. 26:184-189.

Keil, R. G., L. M. Mayer, P. D. Quay, J. E. Richey and J. I. Hedges. 1997. Loss of organic matter from riverine particles in deltas. Geochim. Cosmochim. Acta. 61:1507-1511.

Landry, M. R., R. P. Hassett, V. L. Fagerness, J. Downs, and C. J. Lorenzen. 1984. Effect of food acclimation on assimilation efficiency of *Calanus pacificus*. Limonl. Oceanger. 29:361-364.

Laybourn-Parry, J., M. Walton, J. Young, R. I. Jones, and A. Shine. 1994. Protozooplankton and bacterioplankton in a large oligotrophic lake-Loch Ness, Scotland. J. Plankton Res. 16:1655-1670.

Ludwig, W., P. Amiotte-Suchet, and J.-L. Probst. 1996. River discharges of carbon to the world's oceans: determining local inputs of alkalinity and of dissolved and particulate organic carbon. C. R. Acad. Sci. Paris, t. 323, series IIa, p. 1007-1114.

Mayer, L. M., R. G. Keil, S. A. Macko, S. B. Joye, K. C. Ruttenberg and R. C. Aller. 1998. Importance of suspended particulates in riverine delivery of bioavailable nitrogen to coastal zones. Global Biogeochem. Cycles. 12(4):573-579.

Meade, R. H. and J. D. Milliman. 1983. World-wide delivery of river sediment to the oceans. J. Geol. 91:1-21.

Nelsen, T. A., P. Blackwelder, T. Hood, B. McKee, N., Romer, C. Alvarez-Zarikian, and S. Metz. 1994. Time-based correlation of biogenic, lithogenic and authigenic sediment components with anthropogenic inputs in the Gulf of Mexico NECOP study area. Estuaries. 17:873-885.

Officer, C. B., and J. H. Ryther, 1980, The possible importance of silicon in marine eutrophication, Mar. Ecol. Progr. Ser. 3, 83-91.

Pomeroy, L. R. 1974. The ocean's food web, a changing paradigm. BioScience. 24:499-504.

Porter, K. G., Y. S. Feig, and E. F. Vetter. 1983. Morphology, flow regimes and filtering rates of *Daphnia, Ceriodaphnia* and *Bosmina* fed on natural bacteria. Oecologia (Berlin). 58:156-163.

Qureshi, N. A. 1995. The role of fecal pellets in the flux of carbon to the sea floor on a river influenced continental shelf subject to hypoxia. Ph.D. Dissertation, Louisiana State University, Baton Rouge, Louisiana.

Rabalais, N. N., W. J. Wiseman, Jr., and R. E. Turner. 1994. Comparison of continuous records of near-bottom dissolved oxygen from the hypoxia zone along the Louisiana coast. Estuaries. 17:850-861.

Rabalais, N. N., W. J. Wiseman, Jr., R. E. Turner, D. Justic, B. K. Sen-Gupta, and Q. Dortch. 1996. Nutrient changes in the Mississippi River and system responses on the adjacent continental shelf. Estuaries 19:386-407.

Redalje, D. G., S. E. Lohrenz, and G. L. Fahnenstiel. 1994. The relationship between primary production and the vertical export of particulate organic matter in a river-impacted coastal ecosystem. Estuaries. 17:829-838.

Riley, G. C. 1937. The significance of the Mississippi River drainage for biological conditions in the northern Gulf of Mexico. J. Mar. Res. 1(1):60-74.

Ruttenberg, K. C. and M. A. Goñi , 1997. Phosphorus distribution, C:N:P ratios, and $\delta^{13}C$ in arctic, temperate and tropical coastal sediments: Tools for characterizing bulk sedimentary organic matter. Marine Geology, 139:123-145.

Sklar, F. H., and R. E. Turner. 1981. Characteristics of phytoplankton production off Barataria Bay in an area influenced by the Mississippi River. Contrib. Mar. Sci. 24:93-106.

Smith, S. M., and G. L. Hitchcock. 1994. Nutrient enrichments and phytoplankton growth in the surface waters of the Louisiana Bight. Estuaries. 17:740-753.

Smith, S. V., and J. T. Hollibaugh. 1993. Coastal metabolism and the oceanic carbon balance. Rev. Geophys. 31:75-89.

Straile, D. 1997. Gross growth efficiencies of protozoan and metazoan zooplankton and their dependence on food concentration, predator-prey weight ratio, and taxonomic group. Limnol. Oceanogr. 42:1375-1385.

Sun, L., E. M. Perdue, J. L. Meyer and J. Weis. 1997. Use of elemental composition to predict bioavailability of dissolved organic matter in a Georgia river. Limnol. Oceanogr. 42(4):714-721.

Trefry, J. H., S. Metz, T. A. Nelsen, R. P. Trocine, and B. J. Eadie. 1994. Transport of particulate organic carbon by the Mississippi River and its fate in the Gulf of Mexico. Estuaries 17:839-849.

Turner, R. E. 1984. Zooplankton feeding ecology: contents of fecal pellets of the copepods *Eucalanus pilaetus* and *Paracalanus quasimodo* from continental shelf waters of the Gulf of Mexico. Mar. Ecol. Prog. Ser. 15:27-46.

Turner, R. E., and N. N. Rabalais. 1991. Changes in Mississippi River water quality this century: Implications for coastal foodwebs. BioScience. 41:140-147.

Turner, R. E., and N. N. Rabalais. 1994. Coastal eutrophication near the Mississippi river delta. Nature. 368:619-621.

Vitousek, P. M., J. D. Aber, R. W. Howarth, G. E. Likens, P. A. Matson, D. W. Schindler, W. H. Schlesinger and D. G. Tilman. 1997. Human alteration of the global nitrogen cycle: Sources and consequences. Ecol. Application. 7:737-750.

Ward, C. H., M. E. Bender, and D. J. Reish (eds.). 1979. The Offshore Ecology Investigation. Effects of oil drilling and production in a coastal environment. Rice University Studies 65:1-589.

THE NEXT STEP TO PURSUE ON THE ORGANIC CARBON ISSUE

There is a company called Hydroqual that has developed a carbon model that could probably be utilized to explore the organic carbon issue in detail. We recommend that the CENR approach Hydroqual to explore the use of their model regarding this issue. The unknowns pertaining to organic carbon are 1) the role of terrestrial carbon as energy in the trophic food web (this relates directly to determining the source of carbon in zooplankton fecal pellets), 2) the lability of terrestrial carbon (i.e. how much is being respired by the microbes vs. how much is buried in the sediments), 3) determining seasonal variation in the isotopic signal of terrestrial carbon so that you can point to the fallacy of using a simple two-member mixing model to determine the contribution of terrestrial vs marine carbon, and 4) determining the oxygen utilization rates of particulate terrestrial organic carbon and dissolved organic carbon derived from the breakdown of the particulate fraction. None of these unknowns will be easy to measure and it will not be inexpensive to do this work.

Topic 2

Moving briefly on to Topic 2, one quick point needs to be made with regard to the comparison of the hypoxic zone in the Gulf of Mexico with other parts of the world. "So, in this respect the northern Gulf of Mexico is not unique. Interestingly, the degree of obvious ecological and economic effects related to the hypoxia varies from system to system. The most serious ecological and economic effects of the combined problems of eutrophication and hypoxia are seen in the Black Sea and Baltic Sea where demersal trawl fisheries have either been eliminated or severely stressed (Mee 1992, Elmgren 1984). A comparison of effects from three similar coastal hypoxic zones within the Gulf indicates that, at least for now significant declines in fishery production attributable to hypoxia have not been documented for the Gulf of Mexico (Table 3.1)." (Page 26 – Topic 2)

What the authors failed to acknowledge is that the Gulf of Mexico is an open, highly dynamic system compared to other water bodies such as the Black Sea and the Baltic Sea. Specifically, the circulation of the Baltic Sea is very slow, it takes over 10 years to renew the waters in this body. Moreover, these bodies are surrounded by heavily populated areas and industrial systems that have severely polluted their waters. In contrast, the Gulf of Mexico is a very open and dynamic system. The waters flow in a counterclockwise within the Gulf, with the Atlantic Ocean constantly recharging or mixing the area. While the Gulf of Mexico hypoxic zone stays in the same general area along the Coast, it does move and change in formation from year-to-year. Moreover, the hypoxic zone and the Gulf of Mexico is temporal, lasting not much more than 90 days in any given year.

With this in mind, it is difficult to understand why researchers seem to imply by comparison that the fisheries experience in the Black Sea and Baltic Sea will eventually occur in the Gulf of Mexico. They simply are not comparable. There has not been a measurable decline in the Gulf of Mexico fishery production in the past and there is unlikely to be a measurable decline in the Gulf of Mexico fisheries in the future.

Contradictions in Basic Hypoxia Theory

Page xvii of Topic 2 states that, "Models that link Mississippi River discharge with Gulf of Mexico hypoxia demonstrate a worsening hypoxia in bottom waters with increased freshwater discharge...." However, on page xv of Topic 1 the authors state that the long-term effects in the offshore ecosystem are not the result of the amount of freshwater discharge or alterations in its delivery. Since even the authors of the reports are not in agreement about the fundamental rudiments of how hypoxic conditions arise, it raises a serious question of how can there be clear direction of what, if anything, needs to be done.

Topic 3

Total Nitrogen Declining in Latter Half of Century. Topic 3 states that nitrate concentration in the Mississippi River has increased 2- to 5- fold in the last century. However, while Topic 3 notes that total nitrogen concentration was about 2.26 mg/L for 1980-96, it fails to note that data from Leighton (1907) could be interpreted to indicate that the concentration of total nitrogen in the Mississippi River has gone down in the last century from 2.5 to 3.0 mg/L in the early 1900's and its high point of the 3.0 mg/L of the 1950s.

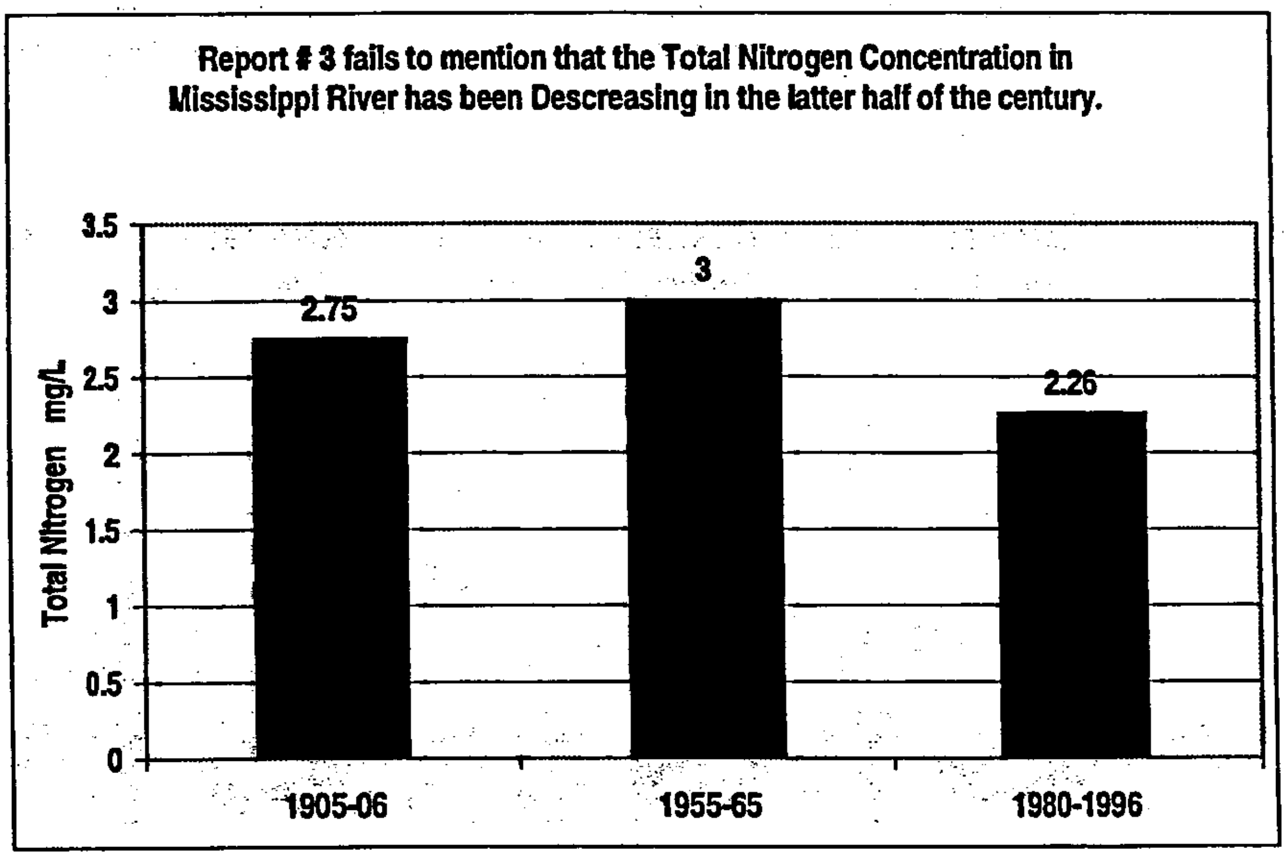

Report # 3 fails to mention that the Total Nitrogen Concentration in Mississippi River has been Descreasing in the latter half of the century.

Figure 1.

Nitrate vs Total Nitrogen. Topic 3 states that flux of total nitrogen from the Mississippi-Atchafalaya River Basin to the Gulf is about 1.6 million metric tons per year, while Topic 1 states that the yearly flux is 1.6×10^6 t/yr nitrate. There is a huge difference between "nitrate" and "total nitrogen." This inconsistent use of terminology between reports is confusing at best, and, at worst, will result in policy being set totally differently depending upon which terminology is used. These discrepancies should have been caught and corrected in the peer review process.

Nitrate-N Flux Trending Down Despite What Topic 3 Says. Topic 3 states that nitrate flux has averaged nearly 1 million metric tons per year since 1980. On page 30-31 the report further states that "There is no statistically significant trend, upward or downward in nitrate flux since 1980, even if the flood year of 1993 is removed." While mathematically true, the authors chose to ignore that things changed in 1983. Furthermore, they ignored all the data that existed before 1980 and chose not to use the more robust statistical analysis that they could have used with the 43 years of data that were available to them. Statistical textbooks state that 30 data points are needed to do proper trend analysis; why then, did the authors of Topic 3 choose to only use 17 data points to make their statement of "no statistically significant trend"? Our analysis of the 43 years of available data shows that there was a statistically significant trend since 1983 and that trend was downward at the rate of 17,000 metric tons per year, Figure 2. The r-square for this analysis is 0.82.

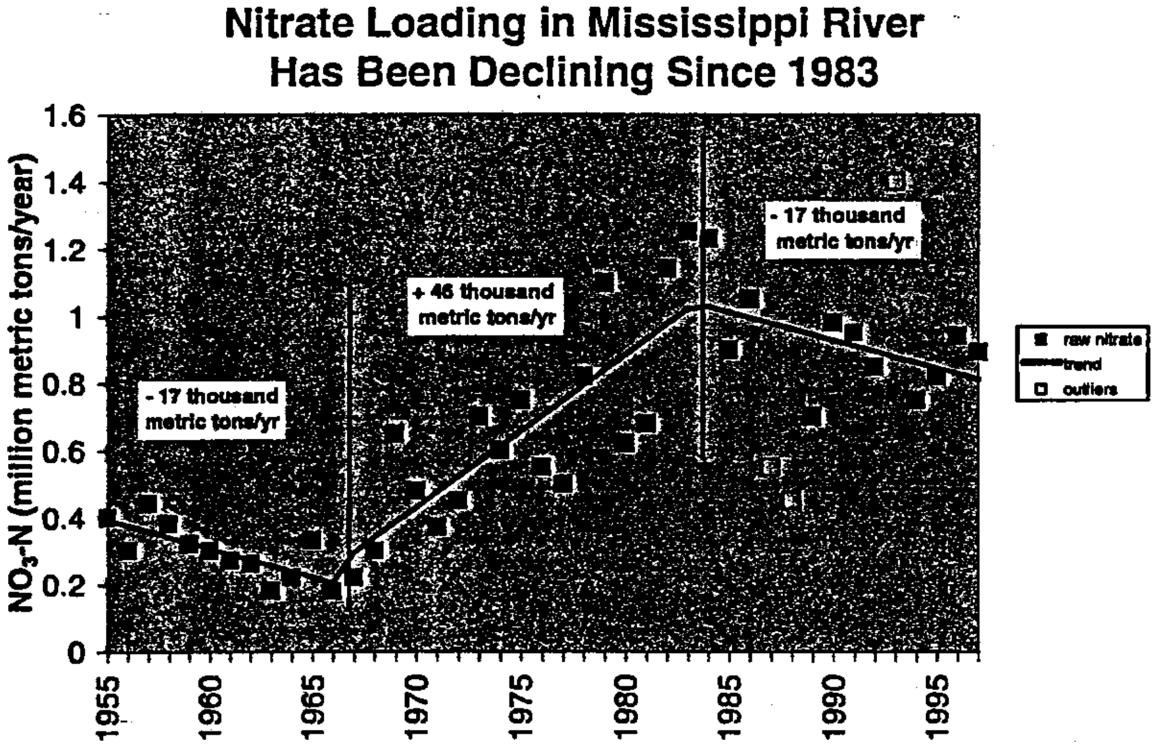


Figure 2.

Figure 3 is another graph indicating that the downward trend is real, as it shows nitrate nitrogen flux normalized for flow. Since 1982, both the highs and lows have been declining. Topic 1 states that, "There is no dobugt [doubt] that the concentration and flux of nitrogen (per unit volume discharge) has increased from the 1950s to 1960s, especially in the spring." However, Figure 3 clearly contradicts that statement by showing that, with the exception of 1969, Nitrate-N flux was lower in the 1960s than the 1950s, and in fact, it was not until 1975 where the Nitrate-N flux reached a level that was consistently higher than what it was in 1955.

Figure 3.

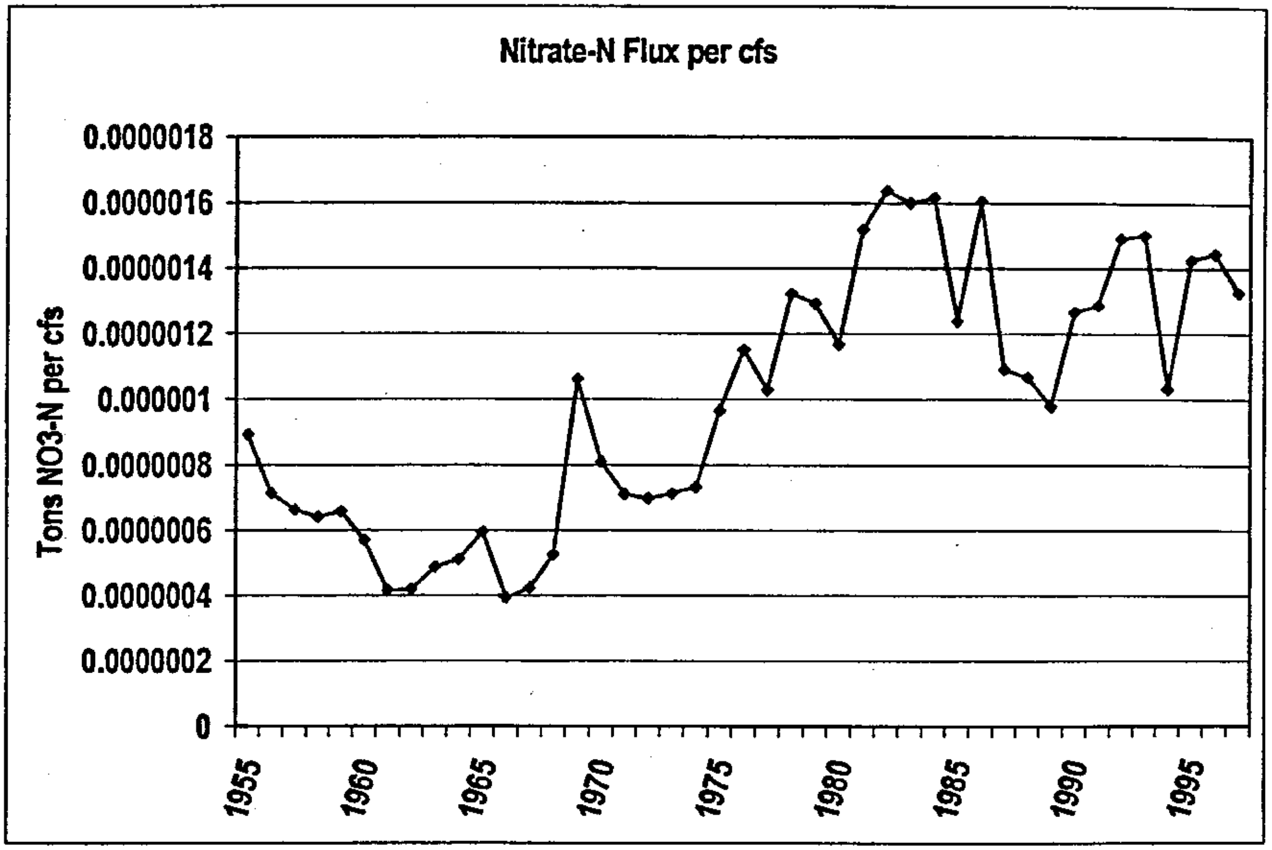
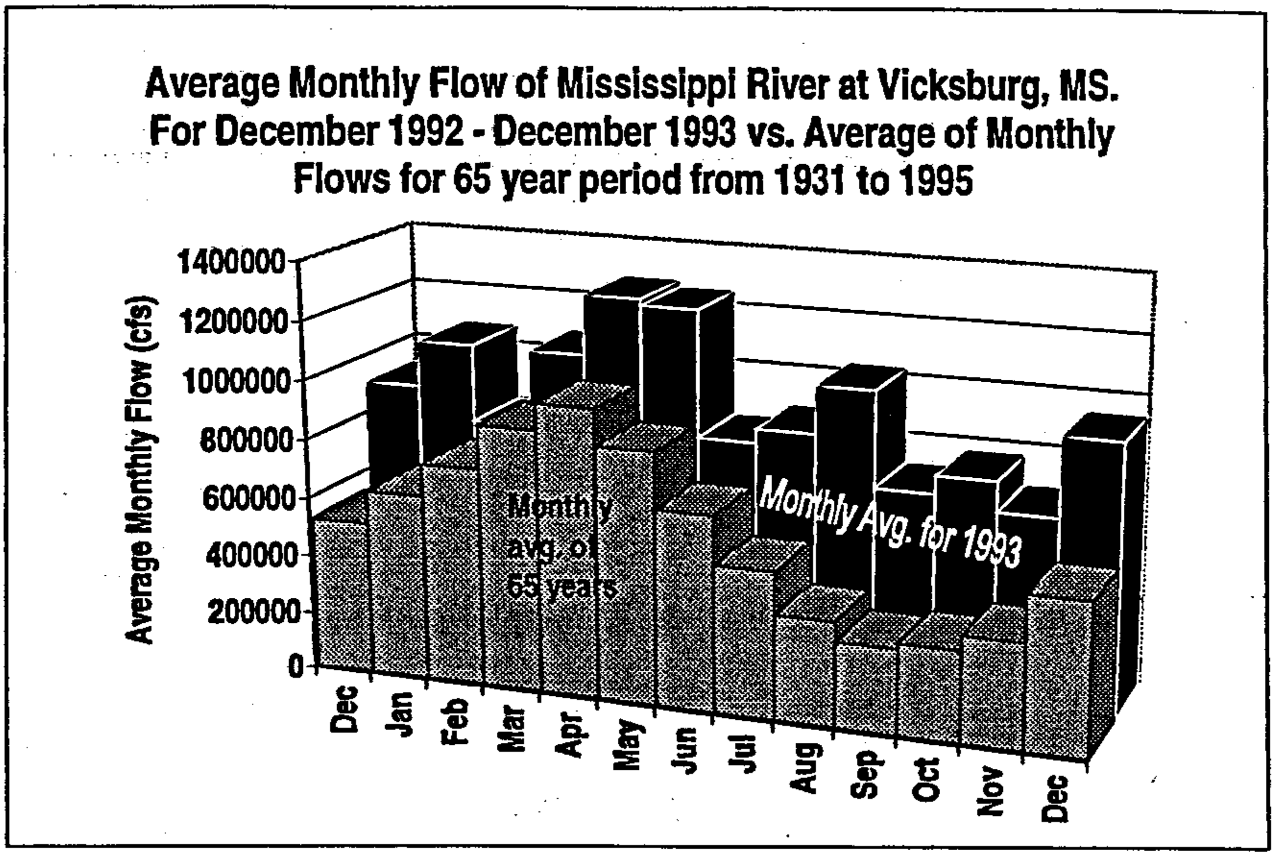


Figure 4.



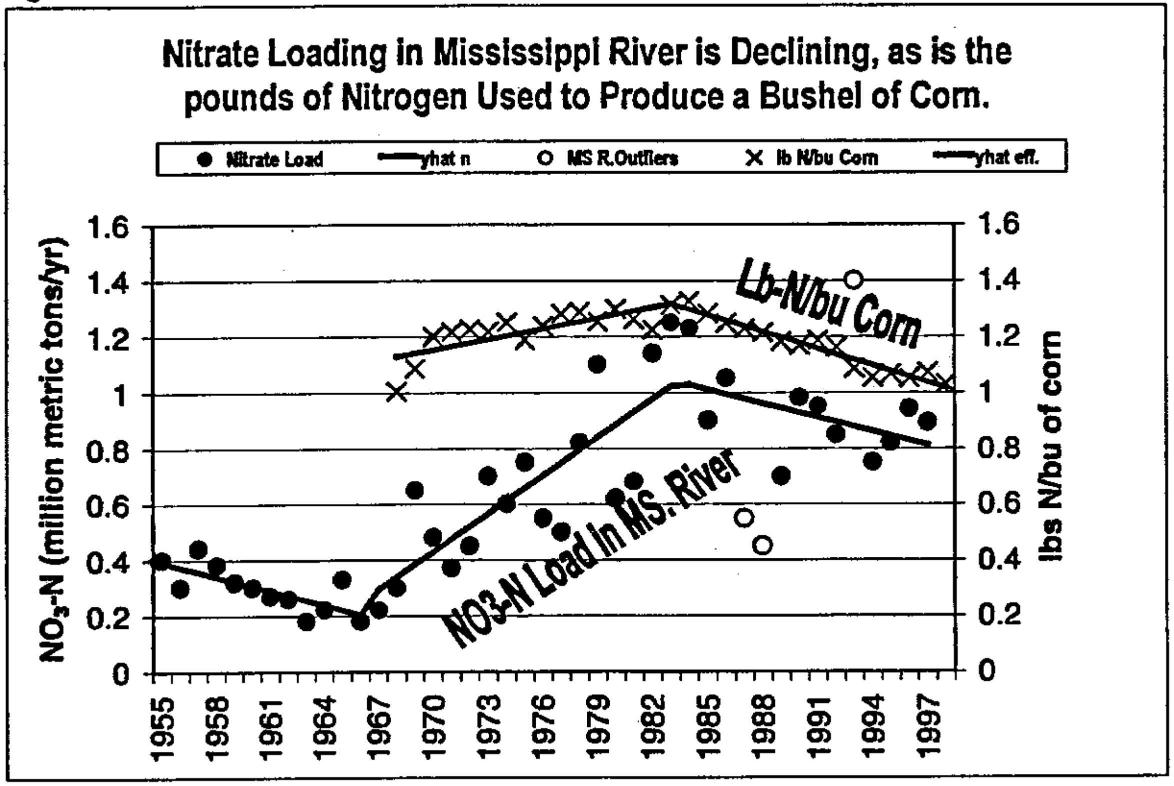
HUGE Flows All Year in 1993 Increase Volume and Area of Gulf Likely to Stratify.

An important point to note that is missing from all the executive summaries (and presumably from all the rest of the reports) **is that hypoxia cannot occur without stratification, regardless of the nutrient load.** Stratification occurs when lighter freshwater from the river floats over heavier salt water and there is not enough current or wind to mix the two. It is obvious from the graph in Figure 4 that above-average flow occurred for 13 consecutive months starting in December of 1992. The shallow Atchafalaya bay and adjacent coastal area received an average of over 300,000 cfs from the Mississippi River during the entire month of August of 1993, which is nearly 3 times more flow than it normally would receive from the Mississippi diversion in August. Assuming 30% average diversion of Mississippi River water for the entire year of 1993 would amount to an average of 280,000 cfs which is enough to cover 5.5 average-sized Midwest counties to a depth of 100 feet. While this fact is alluded to as, "episodic event such as the 1993 flood," in Topic 3, consideration of the fresh water volume itself is minimized in favor of extended discussion on nitrate. The magnitude of the volume of the fresh water itself and the stratification that it would cause at the warmest time of the year, whether it contained any nitrate at all, would probably have been significant enough to set up hypoxic conditions over much broader areas of the Gulf than normal. What role the fresh water stratification, nitrate and organic carbon carried by these flood waters had in prolonging the expanded hypoxic zone in the years after 1993 is not examined to any degree by the reports.

Are Inputs and Outputs Increasing or Leveling Off? The summary of Topic 3 states that the annual nitrogen flux has become highly variable since about 1980 due in part to increased (emphasis added) annual nitrogen inputs into the basin. However, on page 65, Topic 3 states that, "The leveling off (emphasis added) of fertilizer input, crop outputs and the N residuals may indicate that a new steady state condition was established about 1980." The latter statement contradicts the first statement with regard to nitrogen fertilizer inputs. Furthermore, to say that crop output has leveled off is erroneous since the national five-year running average yield for corn was 96 bushels per acre in 1980 and has climbed steadily to128 bushels per acre in 1998.

Market Signals and Voluntary Actions are Surprisingly Effective. On page 78 of Topic 3 the authors state that, "At present no programs or mechanisms are in place to determine if changes in nutrient flux in streams occur as a result of voluntary actions and new policies." While no formal wide-scale program is in place, there is some long-term data to show that voluntary actions do make a difference and have slowly and steadily been making a difference since 1983.

Figure 5.



Nitrate Loading in Mississippi River is Declining, as is the pounds of Nitrogen Used to Produce a Bushel of Corn.

As Figure 5 shows, there is a significant downward trend in the five-year running average for the amount of nitrogen applied to produce a bushel of corn. That trend almost exactly parallels the downward trend in the flux of nitrate-nitrogen moving into the Gulf. Voluntary actions and the marketplace were primarily responsible for these trends.

Atmospheric Deposition is Understated

Atmospheric deposition appears to be greatly underestimated in terms of its contribution to the total load of nitrate-nitrogen in the river. Topic 3 minimizes the atmospheric contribution stating, "These inputs are small relative to the other nitrogen inputs to most of the Mississippi-Atchafalaya River Basin." Inputs such as fertilizer which are buried in the soil, must take a much more convoluted route to get to a stream than does the nitrate in rainfall, much of which moves as surface runoff directly to the streams. The concentrations of nitrate in surface runoff almost always exceed the concentration of nitrate in the rainfall that created the runoff. Therefore, it is conservative to assume that the concentration in the rain can be multiplied by the volume of precipitation and the percentage of precipitation that ends up as gaged streamflow to determine the flux of nitrate-nitrogen due to wet deposition of nitrate from the atmosphere. Using the ratio of wet NO_3 to dry NO_3 from Table 5.1 of Topic 3 and multiplying it times the large basin totals for wet and dry atmospheric deposition from Table 5.2 and further multiplying by the percentage of precipitation that shows up as streamflow in each basin (calculated from 1997 NADP precip. maps) we show that 250,734 tons of NO_3-N or 28 percent of the Mississippi River's load of 887,000 metric tons of nitrate nitrogen could be coming directly from atmospheric deposition. And this does not count any of the dry deposition of NO_3 nor the NH_4. Taking the wet and dry deposition together from Table 5.2 increases the estimated contribution to 425,000 metric tons of nitrogen as N. This could be as much as 48% of the 887,000 metric tons average load of nitrate-nitrogen estimated for 1995-97 which is significantly higher than Topic 3's estimate of 23 percent. Of the atmospheric loading, the Lower Mississippi River Basin contributes the largest amount at 20 %. This could be expected with large amounts of precipitation and a high percentage of it showing up as stream flow.

% of 250,734 metric tons of Atmospheric Nitrate Wet deposition which shows up as NO_3-N Flux in MS. River. (by River Basin)	
18%	Upper Ohio
18%	Lower Ohio
3%	Upper Missouri
8%	Lower Missouri
9%	Upper Mississippi
13%	Middle Mississippi
5%	Arkansas
20%	Lower Mississippi
7%	Red & Ouachita

Point Source Contributions

On page 15 of Topic 3 it is stated that municipal and industrial point sources contribute about 11 percent to the total nitrogen flux. This number seems low because dividing their 287,000 metric tons contribution by the MAR's 1,567,000 metric tons discharge from Table 6.1 equals 18.3 percent. Since most of the output of these point sources is now showing up as nitrate, instead of ammonia, it could equal as much as 32 percent of the 887,000 metric tons of nitrate-nitrogen being discharged by the river.

Also, Topic 5 states that point sources of nitrate nitrogen appear to be of little consequence (<5% of the overall Mississippi River Basin load). However, Topic 3 states that point sources contribute 11% of the nitrogen flux. Which is it, 5% or 11%?

Minimizing Event-Driven Freshwater Contributions to Stratification vs. Maximizing Perception of Volume of Nitrate as the Culprit. In Topic 1, page xv, it is stated that flow rate in the "...Mississippi River at Vicksburg is remarkably stable near 14,000 m^3/s despite significant interannual variability and some decadal trends." Topic 1 tends to minimize the effect of increased flow by downplaying the effect of river flow by calling it remarkably stable, saying that the slight increase in discharge is accounted for by the September through December time frame and that this is a "much less important" time for the ocean than spring and summer. The authors also conclude that long-term effects on the offshore ecosystem are not due to the amount of freshwater discharge or alterations in delivery.

However, by looking at the graph in Figure 6, it is difficult to rationalize the authors' conclusions. First, the stated average flow translates into 494,340 cfs annual flow. Not even the dry decades of the 1930's or the 1950's fall below that average. The period 1991-1997 averaged 41% higher flow than Topic 1's statement of the average, and that has to have some effect, especially ever since 1977, when a full 30% of the Mississippi River flow was diverted into the Atchafalaya. Topic 1 even states that, "An effect likely to occur in the offshore region as a result of increased flow through the Atchafalaya delta is an increase in stratification west of Atchafalaya Bay and further westward into Texas." Furthermore, the yearly average for the flood of 1993 was nearly twice as large as the long-term average stated by Topic 1, and the spring and summer flows were HUGE compared to normal. Topic 1 doesn't say so, but an important point to note is that **hypoxia cannot occur without stratification, regardless of the nutrient load.** Also, no mention is made of whether conditions from December of 1992 through December of 1993 were more or less favorable for mixing the ocean water with the fresh water. If there was less mixing, there would be a greater tendency to stratify larger areas of the Gulf than normal.

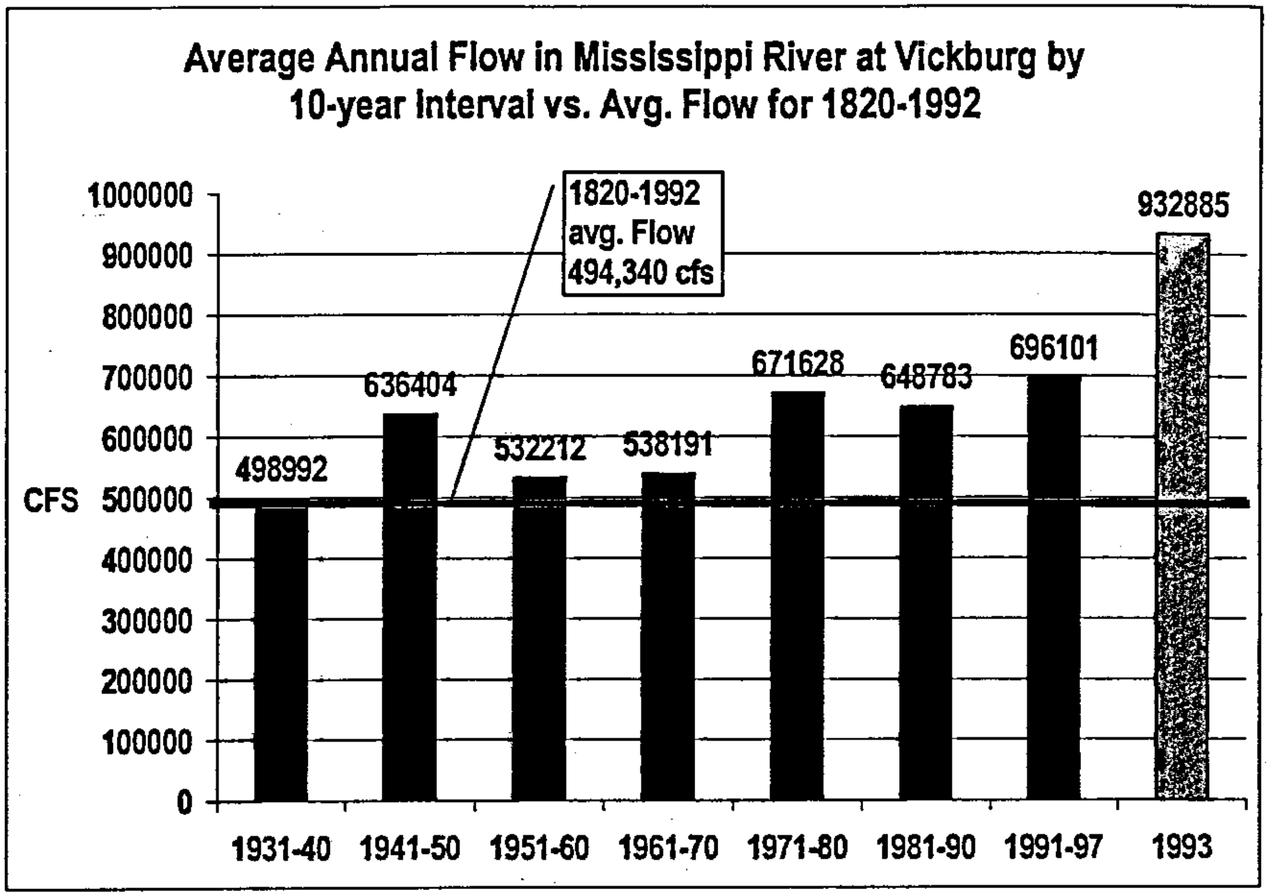


Figure 6.

On page xv of Topic 1 the average flow of the Mississippi River at Vicksburg is stated as 14,000 m^3/s which is equal to a total volume of 442 km^3 per year of freshwater per year flowing into the Gulf. However, in the same paragraph on the preceding page (xiv) the authors state that the Mississippi River delivers an average of 580 km^3 per year to the Gulf, which is a volume that is 31% greater than the volume that would be discharged based on the long-term flow rate of 14,000 m^3/s. The difference in total volume is 138 km^3 which is enough to cover three average-size counties in Iowa 100 feet deep. The authors of Topic 1 first use a high number (580 km^3) for volume that is almost exactly equivalent to the average flow rate of 648,783 cfs that occurred in the decade of 1981-1990 (see Figure 6.), but in the next breath use a low number 14,000 m^3/s (494,340cfs), to describe long-term rate of flow. By using the slight-of-hand of comparing a low flow rate with a much higher volume in the same paragraph, the authors essentially minimize the perception of flow rate as a significant long-term agent of change in the Gulf and maximize the perception that large volumes of nutrients in the short-term are the chief culprit.

Downplaying and Omissions of Key Information

Topic 3 uses data only through 1997 to describe the size of the hypoxic zone even though 1998 data was readily available in mid-1998. Topic 1 leads with a sentence on page xiv about how the extent of the hypoxic waters "increased" to 16,000 to 18,000 km2 in 1993-1997. The next sentence simply states that, "The estimated extent was 12,500 km^2 in mid-summer of 1998." The number for 1998 is mentioned briefly again on pages 1 and 5, but it is buried in the text and the word "decrease" is never used to talk about what happened in 1998. In the 1,000+ pages of the six reports these are the only mentions that we can find of what happened in 1998. None of the graphs in any of the six reports show the decrease in the extent of the hypoxic area in 1998. Clearly, the bias of these reports is to show a big increase and to obscure any mention of fact that might detract from the predetermined outcome. We have included as Figure 7 all the data that is available through July 31, 1999. While the size of the Hypoxic zone increased to record levels in 1999, it is important to keep in mind the admonitions of Topic 2 which state:

1) There is no discernable, measurable, or documentable "detrimental ecological and economic effect" to the Gulf environment or its fisheries from hypoxia. (Topic 2 p. 52.)

2) Even during the recent unprecedented extremes in hypoxic conditions (1993-97), Louisiana's coastal fisheries flourished, maintaining "energy flow to productive fisheries (crabs and shrimps) that depend on the bottom." (Topic 2, p.9);

3) Such ecosystem disturbances in the shallow continental shelf as can be documented are as likely to be "due to the other sources of stress that disturbs sediments such as trawling." (Topic 2, p. 50)."

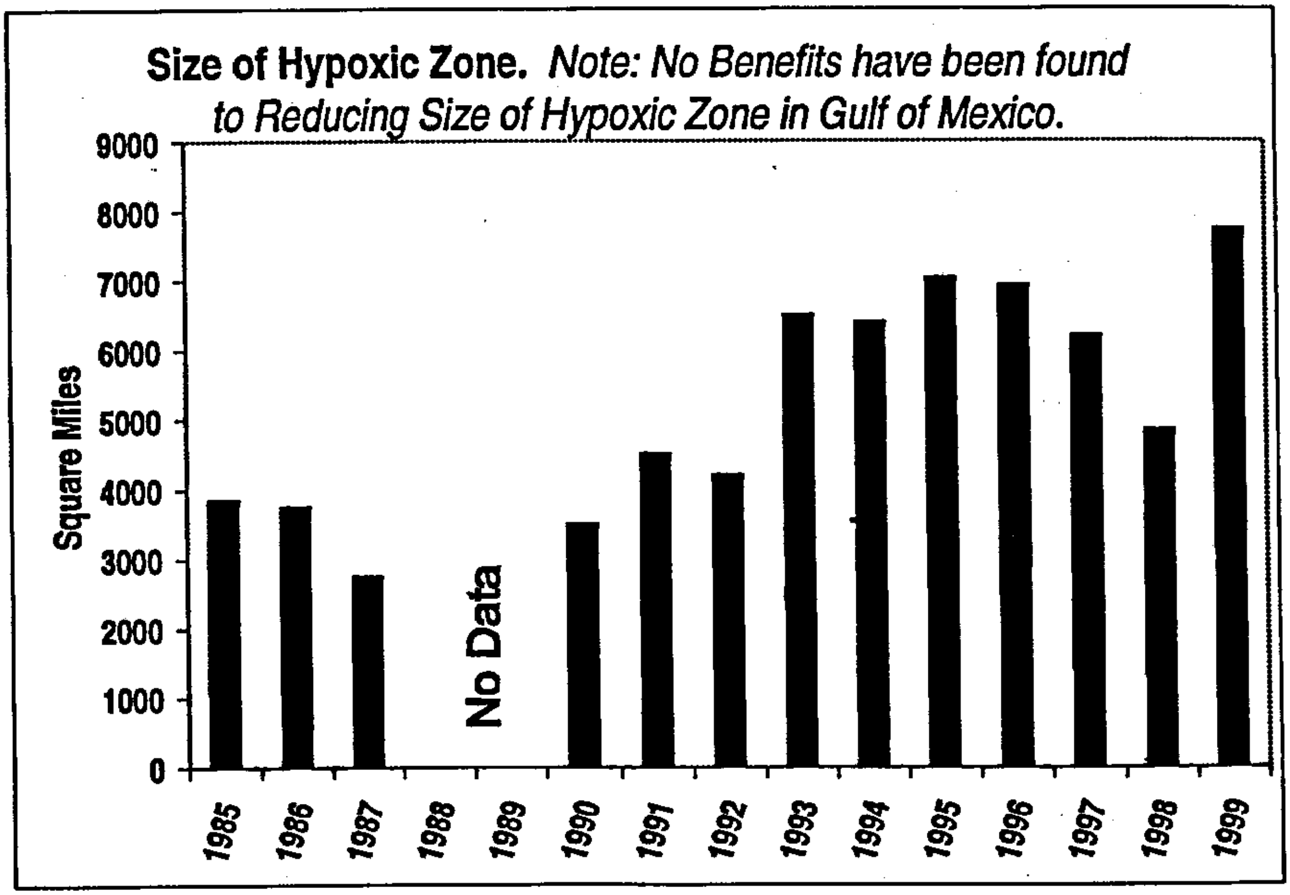


Figure 7.

Topic 4

Topic 4 Off by 6 Orders of Magnitude

In the original posting of Topic 4 in section 3.2.3.3, the last sentence of the second paragraph read as follows:

> "Concentrations of 1.5 mg/L for total N and 0.075 mg/L for total P were proposed as approximate boundaries separating mesotrophic and eutrophic conditions in these flowing waters (Dodds et al, 1998)."

Compare the sentence above, to the second sentence of the last paragraph of the same section 3.2.3.3 which reads as follows:

> "The mean TP concentration of water reported by Smith et al. (1987) for 381 riverine sites in the continental United States is 130 g/L, a value greatly in excess of the mesotrophic-eutrophic boundary of 75 g/L proposed by Dodds et al. (1998)."

One of these two sentences appears to be off by 6 orders of magnitude. Which is correct- 0.075 mg/L or 75 g/L? In fairness to the authors, it apparently was a glitch in transferring from one type of computer to another and it was fixed once the error was pointed out. However, the fact of the matter is that the error was posted on the web site in the first place, indicating another example of lack of quality control. With these kinds of factual errors distributed throughout the reports, it calls into question all the rest of the numbers in the reports.

Topic 5

With respect to Topic 5, we remain puzzled about how the authors developed their recommendations for a 20% reduction in fertilizer application in addition to other fertilizer best management practices. Another recommendation was " A major effort to restore or create 5 million acres (0.7% of the Mississippi River Basin) of wetlands and 19 million acres of riparian forests (2.7% of the Mississippi River Basin) or some combination of these two approaches to cause a combined 40% reduction of nitrogen loading in the Gulf---."

The question we have is what is the quantifiably/identifiable link to these practices and a reduction in nitrate concentration levels at the mouth of the Mississippi River Basin? As noted in our comments on Topic 5, until researchers can give some notion of what cause and effect of these various actions may be, it is difficult to understand why they would recommend their implementation.

According to the authors, the main focus of this report would be to identify and evaluate methods to reduce nutrient loads to surface water and ground water in the Gulf of Mexico. While the authors were true to their task, one nagging question remains. If nitrate levels in water are reduced in the Upper Basin of the Mississippi Watershed, what impact will that have on the hypoxic zone in the Gulf of Mexico. In other words, what is the link between the two, the cause and effect?

The Topic 5 report touches on many important issues with regard to the hypoxic question. For example, on page 69 they point out that "From the 1930s until the present, there has been a dramatic loss of wetlands in the Mississippi delta with estimates as high as 100 km^2 per year --." They go on to say that "An understanding of the causes of this land loss is important not only that effective management plans can be developed deal with the land loss but also to understand the relationships among land loss, water quality and offshore hypoxia."

"A number of factors have been linked to land loss including elimination of riverine input to most of the coastal zone due to construction of flood control levees along the Mississippi River, altered wetland hydrology mostly due to canal construction, salt water intrusion, wave erosion along exposed shorelines, high subsidence rates, and sea level rise --. Most have concluded that land loss is a complex interaction of these factors acting at different spatial and temporal scales ."

The authors go on subsequent pages to substantiate the potential reductions in nitrogen based on various measurements and experiences. They go on to conclude (page 79) that "The effectiveness of these large-scale diversions on significant uptake of nitrate-nitrogen has been controversial and is not universally accepted (Turner, 1998) so there needs to be additional study to determine the actual discharge rates of diversion, the area of wetlands needed, the potential and actual nitrogen reductions and the linkages between riverine input and offshore response. A 10-15% reduction of NO_3 loading during the spring flood might have had significant and official impacts on offshore production in hypoxia."

One statement, (page 87) should be retracted "High concentrations in drinking water cause a disease called "methemoglominemia" or "blue baby." It should be noted that recent research has refuted the link between nitrogen and blue baby syndrome. "— The evidence suggests that diarrhea and/or gastrointestinal infection/inflammation, not ingested nitrates, are the principle causative factor in infantile methemoglobinemia;--" "Given the estimated 1-2.4% of water sources contaminated with nitrate levels exceeding 10 ppm nitrate-N and a dearth of infantile methemoglobinemia cases connected to such water sources, there is little indication that relaxing the drinking water standard for nitrate (not nitrite) to 15 or 20 ppm nitrate-N would increase the health risk to infants (27)." (Avery, Environmental Health Perspectives, Volume 107, Number 7, July 1999).

The authors go on to discuss river diversions in Louisiana (page 91). "Removing 50,000 metric tons per year would require about 500,000 hectares and a diversion of about 13% of the river flow. Removal of about 100,000 metric tons would require about 1,000,000 hectares and diversion of about 26% of the river flow. Because nitrate removal can take place in marshes, swamps or shallow open water, these areas can be compared to the areas of these habitats in the coastal zone.

One of our concerns with these reports are statements such as the following. (page 92) "A "reasonable assumption" is that most of this nitrogen that leaves a farm field does reach the Gulf as Goolsby et al. (1999) point out that there appears to be little in stream loss of nitrogen once it reaches the streams and rivers. Reducing 1 million tons of nitrogen in a wetland "conceivably" causes a reduction of 1 million metric tons of nitrogen at the Gulf." The use of the words "reasonable assumption" and "conceivably" point out that this is far less than a totally scientific exercise. We congratulate the authors for being candid on the point and hope that readers understand that much of the information presented in the other five topic reports are of a similar vein, although couched in highly positive, scientific-type statements that may mislead readers.

On the next page (page 93) the authors talk about system delay and buffering. "There are two factors in the Mississippi River Basin that confound the idea that a reduction of nutrients well up in the watershed will have an impact in the Gulf of Mexico. First there is a delay between the time that fertilizer is applied and the time that it appears in streams and rivers. Second, there is a considerable delay between the discharge of a kilogram of nitrogen in the upper basin and its appearance in the Gulf of Mexico. On its way, it has perhaps spiraled through the nitrogen cycle several times and is also subject to in-stream retention. On the other hand, if all sources of nitrogen were eliminated in the upper basin of the basin, there would still be in-stream release of nitrogen chemicals from storages in the sediments of streams and rivers and from allochthonous sources along the streams, e.g., litterfall from riparian forests. It is almost impossible to estimate how important this buffering effect of in-stream processes would be."

On the next page (page 94), the authors make another important observation about nutrients. "The increase in nitrate-nitrogen observed in the Mississippi River near the Gulf over the past 50 years has not been a one-variable experiment. Sediment, phosphorus, and silicate loads have also changed as a result of pollution, dam building, and land use change in the Basin. Although significant literature implicates nitrogen as the limiting factor in coastal waters around the world, there are confounding factors involved in determining if the reduction of a known amount of nitrogen will reduce the area of hypoxia. There is the question as to whether other chemicals, particularly phosphorus and silicate, are now co-limiting factors in the Gulf. As the amount of nitrogen has continued to rise in the Mississippi River, the N/P and N/Si ratios have increased to the point that phosphorus and silicate could be seasonally limiting." (We would add to this organic carbon, an issue that has already been discussed under Topic 1.)

The authors go on to state in their conclusions and recommendations (page 100) "Nevertheless, we conclude that a significant >50% reduction of nitrogen loading to the Gulf of Mexico is possible through the implementation of a number of proven techniques working in concert. The suite of techniques includes: 1) modification of farm practices to make the use of nitrogen from fertilizer, soil, and manure more effective and efficient, 2) the creation and restoration of wetlands and riparian ecosystems between farmland and streams and rivers, but particularly in those areas where concentrations of subsurface nitrate-nitrogen is highest, 3) the reflooding of former wetlands that are not contributing excessive loadings of nitrate-nitrogen due to their drainage, 4) the implementation of nitrogen control on domestic wastewater treatment plants and on significant industrial sources, 5) flood control in the Upper Mississippi that involves retention of floodwaters rather than preventing flood waters from leaving the major river channel, and 6) the diversion of floodwaters to backwaters of the Mississippi River delta and coastal wetlands. Several of these recommendations need to be implemented in concert if a major reduction in nitrogen loading to the Gulf of Mexico is expected. If policies are devised to implement only one or two of these recommended approaches, then improvement in the Gulf of Mexico is not as likely."

Wetland Percentages Vastly Understate Real Impacts

Topic 5 proposes to restore or create 24 million acres of wetlands and riparian zones or 3.4 % of the Mississippi River Basin. However, the authors fail to point out that most of these restored acres would, by necessity, have to be located in six states MN, IL, IA, IN, OH and MO because that's where the nitrate flux is supposedly coming from. There were 83.5 million acres of corn and soybeans in these six states in 1997. Most of the suggested 24 million restored wetland and riparian zone acres would have to come out of existing corn and soybean acreages and that would be a whopping 29% of the productive land area, and that is a far different set of circumstances and impacts than using just 3.4 percent of the total land area in the basin as Topic 5 suggests. Two states, IA and IL would be forced to reduce corn and soybean acreages by 19 percent, assuming that Topic 3 has accurately identified that

35% of the nitrate flux is from these two states. That would amount to eliminating all the acreage that went into corn exports in 1997 and over 50% of the acreage that produced soybeans for exports.

On page 101 the authors drop the bomb shell in this report. "This would require approximately a 20% reduction in fertilizer/nitrogen application in addition to other best management practices such as optimum timing of fertilizer application, use of alternative crops, such as perennials, wider spacing of tile drains, and better management of livestock waste whether stored or applied to the land."

While there is much discussion about the use of nitrogen in this report, it fails to demonstrate the link between a 20% reduction in nitrogen fertilizer application and whether there would be a reduction in nitrate levels in the Gulf of Mexico. And, if so, how much and when. While we acknowledge that two of the seven authors are professional agronomists, there appears to be a real disconnect in regard to this recommendation and the data or research necessary to back up the recommendation.

This issue becomes even more important in view of the possibility that nitrogen may not be the only factor involved in creating the hypoxic zone in the Gulf of Mexico. If organic carbon is determined to be another important variable in the hypoxic equation, then any actions designed to mitigate the hypoxic zone must also include a plan for organic carbon.

Topic 6

While we appreciate the time and effort put forth by the contributing authors to this report, we have a number of questions and several concerns about the economic analysis.

First, and foremost, we would like to call attention to the underlying premise of an economic analysis, that is some type of cost-benefit analysis. Please note the following quote from Section 3.5.1 (page 24) of the report. "It is worth emphasizing, as did Topic 2, that failure to identify hypoxic effects in the commercial fisheries data does not mean that they do not exist. But, if hypoxic effects do exist, their magnitude must be small in relation to the other sources of variability of the data."

What follows is over 100 pages of text, charts and tables, and references for a cost-benefit analysis. If there is no identifiable cost, or if it is so small in relation to other variables that it is not identifiable, why go through the process of months of analysis?

The report goes on to say "nonetheless, given the lack of measurable economic benefits from reduced hypoxia, the Economic Analysis undertaken below was restricted to a cost-effectiveness analysis that sought to identify least-cost policies for attaining a representative reduction in non-point nitrogen runoff to the Gulf of Mexico. The fixed target level of nitrogen loss reduction 20%, was suggested by topic Group 5 as a reasonable level that could be attained given the current technology that would have the potential to decrease incidences of hypoxia in the Gulf."

If we understand correctly, what the authors are saying is irrespective of the fact that there is no identifiable cost associated with hypoxia, that they are going to go ahead and fix a target level of nitrogen loss reduction of 20% as a reasonable level that has some "unquantifiable potential" to decrease incidences of hypoxia in the Gulf. The logic of this bold position and the following analysis simply escapes us.

Section 4.2.1 (page 32) "However, transaction costs (developing site-specific plans honoring all producers' activity) would almost certainly outweigh the benefits. In addition, the results of a model such as USMP are not detailed enough for a real application of the results to the ground. Instead, policy must be designed around a few factors that are easy to observe and that are closely related to nitrogen loss. We determined that a combination of nitrogen fertilizer use reduction and land retirement were most responsible. To achieve the 20% N-loss reduction goal, fertilizer use appeared to be the most important factor." We point out these statements as an indication of the hypothetical and subjective nature of the entire Topic 6 report.

Section 4.1 (page 27) the USMP model was developed by the Economic Research Service, USDA to analyze the effects of government commodity programs, environmental policies on the U.S. Ag. sector on the environment." There is some concern among agricultural economists about the USMP model. For example, this model has never been available for public scrutiny and comment. Second, many agricultural economists are of the opinion that this model is relatively outdated and impracticable for the type of analysis necessary for this situation.

One of the obvious shortcomings in the model is that it shows that when fertilizer applications are reduced and crop production declines, higher crop prices ensue and offset much of the decline in income. However, in the internationally competitive environment of the real world, this could happen only for a very short time period. As soon as U.S. crop prices increase, international competitors ranging from Argentina to Thailand to China will increase their production and replace the United States in the corn export market. Consequently, the increase in prices is very transitionary, lasting only briefly until other countries increase their production. The ultimate loss of export markets is much greater as is the loss of farm income.

Section 5.2 (page 46) - "Along with the potential and direct agricultural benefits from hypoxia, reduced nutrient loadage from agriculture would also be expected to lead to reduced contamination of surface- and aquifer-based drinking water supplies, and thus benefits in the form of reduced costs to obtain potable drinking water." First, we are not aware of any direct agricultural benefits from hypoxia. Furthermore, the Avery report on infantile methemoglobinemia (blue baby syndrome) suggests that the drinking water standard for nitrate could be raised to 15 or 20 ppm nitrate-N which would eliminate the "nitrate problem" in drinking water in the vast majority of cases. (Avery, 1999) Thus, other than Des Moines, and some other cities that have already installed expensive nitrate removal equipment, most cities would not be faced with additional nitrate removal costs.

Table 4.2.1 – 2, (page 109) – Percentage Change in Crop Prices. Just as an example of how this model is not related to the real world, we point out the change in corn vs. sorghum chart shown in this table. According to the table, a 20% reduction in nitrogen would result in a 9.4% increase in corn prices and a 19.3% increase in sorghum prices. However, corn and sorghum are direct substitutes with sorghum priced at approximately at 95% of the value of corn. The idea that sorghum prices would increase at twice the rate of corn prices is simply not possible in today's world. While this is a relatively small matter, it does point out how models can quickly stray from real world realities.

The issues of excess or "insurance" nitrogen application.

For decades, there has been an ongoing discussion about what the optimal rate of nitrogen fertilizer application should be. Some agronomists suggest that farmers apply too much nitrogen. Yet, when you look at other recommendations from the various state land grant universities, there is some variance between states. Moreover, while there are some articles in the journals about this issue, they are relatively few.

The reality is that the optimal nitrogen fertilization application rate depends upon a number of variables, such as soil type, weather and the management skills of the operator. Farmers determine nitrogen and other fertilizer rates based on some predetermined yield goal and normal weather. The fertilizer is usually applied preplanted with some farmers side dressing additional amounts early on in the growing season. If the weather cooperates and the yield goal is achieved, all the fertilizer is utilized, at least in theory. But if the weather does not cooperate and yields slip, according to the agronomist, the farmer has applied too much fertilizer.

While there may be some farmers who apply more nitrogen than necessary for optimal yield, we would argue that this practice is much less frequent than suggested in the CENR report. There is evidence to support this position as reflected in nitrogen utilized per bushel of corn. As indicated in the Figure 5 (page 31), the amount of nitrogen fertilizer applied per bushel of corn has declined steadily since the mid-eighties.

To the extent that farmers were ever utilizing excess nitrogen, there are fewer today. There are many farmers that are probably very close, +/-10%, to the rates recommended by agronomists at land grant universities, probably the majority. A mandated across-the-board, 20% reduction in fertilizer nitrogen application rates could have a devastating impact of yields, much greater than the 2% to 6% reduction indicated in these reports. Moreover, mandated application rates may preclude the higher rates of application necessary for future yield improvements. Corn yields increase approximately one bushel per year over time. The advent of genetic manipulation may accelerate this trend. This is equivalent to reducing the salary of a wage earner by 20 percent and telling that wage earner that will be the cap on his or her future earning potential.

Agriculture Accused of 2.5 to 3 times more pollution than even EPA's National Water Quality Inventory Indicates.

Serious factual errors were made on page 5 of the revised Topic 6 dated April 15, 1999, and were posted to the web site.

The report read as follows:

> "A 1994 report to Congress (EPA, 1994) indicated that 23% of rivers surveyed, 43% of lakes and 47% of estuaries were impaired by nutrient enrichment."

These are serious factual errors overstating agricultural pollution by 2-1/2 to 3 times. This should have been caught in the peer review process. The numbers in this sentence should be 8%, 17% and 17% respectively, and even these numbers can be shown to be suspect on the high side[1]. The source for these numbers is the National Water Quality Inventory 1994 Report to Congress, Appendix pages A-15, B-14, and C-14. See attachment "Spring Cleaning Time for EPA's Black Hole of Data," Public Policy Digest, Volume 6, No. 2, May 1999, for details about how the National Water Quality Inventory lacks scientific process and lacks solid numbers from water quality monitoring data.

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[1] All the numbers contained in the EPA National Water Quality Inventory report are suspect on the high side as noted in, By the Numbers A Review of EPA's National Water Quality Inventory A Crisis in the Making. American Farm Bureau Federation, and Murky Waters Official Water Quality Reports Are All Wet, An Inside Look at EPA's Implementation of the Clean Water Act. May 1999. PEER White Paper.

Topic 6 continued with the next sentence:

> "Two years later, the 1996 National Water Quality Inventory, reported even higher levels of nutrient impairment, 40% of surveyed rivers, 51% of surveyed lakes, and 57% of estuaries."

Again, these are serious factual errors that overstate the issue by nearly 3 times. The actual numbers are 14%, 20% and 22%, respectively. (See Appendix of the National Water Quality Inventory 1996 Report to Congress, Tables A-4, B-4 and C-4.) These are the kinds of errors that the peer review process should have filtered out.

Other Issues:

There are two other issues of concern regarding the development of the six Topic reports. First is the relatively closed nature of the process. There was no formal process for outside input regarding the Topic reports as they were being developed. While Mr. Scavia did allow for some questions and or comments at the Mississippi Rive Nutrient Task Force/Gulf of Mexico Program meetings, this was not an avenue for meaningful input in such an extensive process. Moreover, there was one meeting of the coordinating board in which the public was barred. This occurred at the Memphis meeting last February. It is difficult to understand why a federal public body would not allow public observation.

The other issue is the choice of authors for the scientific reviews. While the scientists chosen to work on the Topic reports have good scientific credentials, some may not have been totally unbiased in their views. Two examples would be Dr. Nancy Rabalais and Dr. William Mitsch.

Dr. Rabalais has been involved with the Louisiana Universities Marine Consortium (LUMCON) for over 15 years. While she is a recognized authority on hypoxia in the Gulf of Mexico, her position with LUMCON is such that she also plays an advocate role. Likewise, all five of the principal authors for the Topic 1 report are on the Louisiana University system payroll. We would suggest that while this group may be knowledgeable in their respective fields, they may also have an inherent bias on this issue. Likewise, Robert Diaz, Virginia Institute of Marine Science, College of William and Mary, who was one of the principal co-authors of Topic 2, also was previously involved with LUMCON.

Dr. Mitsch, School of Natural Resources, The Ohio State University, was the principal author of the Topic 5 report. Mitsch has diligently pursued the topic of wetlands throughout the bulk of his career. His peers generally view him as Mr. Wetlands. Consequently, it came as no surprise that the group for which he was principal author recommended that millions of acres of farmland should be converted to wetlands. Sixteen of his articles were cited in the Topic 5 references. The next closest number of references for another author was six each from two authors, Rabalais and Randall.

We are also concerned to find that Don Boesch, the chair of the "independent" hypoxia editorial panel overseeing the peer review process, was one of the key scientists drafting recommendations to revise the Clean Water Act for the Center of Marine Conservation to submit to Congress. The Center describes itself as "...committed to protecting ocean environments and conserving the global abundance and diversity of marine life. Through science-based advocacy, (emphasis added) research and public education, CMC promotes informed citizen participation to reverse the degradation of our oceans."

Four of the other CENR report authors were involved in writing the advocacy recommendations for the CMC.

In a letter to Congress, dated April 15, 1999, the CMC's #1 recommendation was that "The Clean Water Act should be revised and strengthened to specifically **address nutrient reduction** (emphasis added) from both point and nonpoint sources."

We are not suggesting that these people took a deliberately biased attitude toward their respective assignments. However, it would be extremely difficult for them to suppress the subtle biases inherit with their careers and responsibilities. As a result, it does raise questions about the objectivity of some of the recommendations contained within in these Topic reports.

Summary and Recommendations

The CENR reports ignore many crucial issues which affect hypoxia including the improvements in nitrogen fertilizer use efficiency that have already been made by farmers and the impact of changes in the flow of the Mississippi River. U.S. Geological Survey data show that the nitrate load in the river has been declining. At the same time, the flow of the river has been changing. Rainfall has increased in the Mississippi River Basin in recent decades and engineering changes for flood control have added to the flow, especially into the shallow Atchafalaya Bay.

The CENR reports focused on nutrients from agriculture as the cause of hypoxia and predictably offered nutrient based solutions, including reduced nitrogen use and wetlands creation. The authors concentrated almost exclusively on the nitrogen issue. Other researchers have suggested that determining the source of organic carbon fueling hypoxia is critical for management decisions related to the hypoxic zone. In addition, the reports find no economic damage to the commercial fishery from hypoxia, and do not suggest there is any threat to human health. Unfortunately, the reports only scratch the surface of the complexity of the Mississippi River system, yet they suggest fixing one of the alleged causes by increased regulation of agriculture.

The better approach would be to acknowledge and work to understand the tremendous complexities of the issue. Farmers today are producing more crops with less fertilizer, meaning they are more efficient and are leaving less nitrogen in the environment. Voluntary, cooperative, incentive based programs to improve water quality will yield real gains for the Mississippi River, the Mississippi River Basin and the Gulf of Mexico without damaging the most productive farming in the world.

What is needed are additional resources better targeted to impaired watersheds and directed at on-the-ground activities and practices that will result in further water quality improvements. Agricultural research, improved water quality monitoring, technical assistance and conservation initiatives are keys to water quality and continued agricultural abundance.

It is critical that adequate federal resources be allocated to address water quality challenges. Over $100 billion has been spent over 26 years to deal with urban point sources of water pollution. With the increasing emphasis on nonpoint sources, resources should shift as well. The State Revolving Loan Fund should have more impact on rural areas and additional funding should be allocated to improving water quality monitoring, technical assistance and cost-share programs rather than new regulatory programs at the federal level. The Environmental Quality Incentives Program (EQIP) , and the buffer component of the Conservation Reserve Program (CRP) all are farmer supported, voluntary programs with important water quality benefits that must be fully funded to meet growing needs. Additional research and pilot projects are needed to determine how farmers can most effectively use wetlands as a tool to reduce nutrient loss to waters. Additional funding is also needed for state directed technical assistance and financial incentives for farmers and assistance in the development of nutrient management plans. Research funding that focuses on nitrogen utilization, efficiency and loss reduction technology in impaired watersheds is critical for long term solutions to nutrient loss.

The Natural Resources Conservation Service (NRCS) must be funded to support its primary mission of being the lead agency in the federal system that works with and assists farmers to improve water quality. Fully funded and supported programs that work with farmers to improve water quality through cost sharing and nutrient management that also supports the economics of the particular farm business is the most effective method of reducing nutrients to waters in the Mississippi River Basin.

Please advise if you need any clarification or expansion of the points we have raised.

Sincerely Yours,

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