ANNUAL REPORT OF THE
FLORIDA BAY SCIENCE OVERSIGHT PANEL:

Perspectives from the 1998 Florida Bay Science Conference

Submitted to the

Program Management Committee
Interagency Florida Bay Science Program

by

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June 17, 1998

Florida Bay Science Oversight Panel

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INTRODUCTION

The Florida Bay Science Oversight Panel (FBSOP) is an independent peer-review group, charged with providing regular, broad, technical, and management review of the Interagency Florida Bay Science Program. It reviews agency plans, Program Management Committee (PMC) strategies for program development, scientific quality of research, modeling and monitoring, and research results (Armentano et al., 1994; 1996). The Panel consists of senior scientists with significant experience in major estuarine restoration programs but without involvement in Florida Bay projects.

This is a period of turnover of membership of the FBSOP. Three original members, Drs. James Cloern, Ronald Perkins, and Susan Williams, rotated off the Panel after nearly three years of very effective service. They have been replaced by Drs. Charles Yentsch of the Bigelow Laboratory for Oceanography, John Milliman of the Virginia Institute of Marine Science, and Kenneth Heck of the Dauphin Island Sea Lab, respectively. In addition, the charter-Chair of the Panel, Dr. Donald Boesch, announced his intent to step down from the Panel after the 1998 Conference and review. Dr. John Hobbie of the Marine Biological Laboratory has been invited by the PMC to succeed Dr. Boesch as Chair and participated in the 1998 review in order to ensure continuity. Dr. Neal Armstrong, a regular member of the Panel, could not participate in the 1998 Conference and review and was replaced by Dr. William Boicourt as an alternate. Unfortunately, Drs. Deegan and Yentsch also were unable to attend because of late developing requirements.

The FBSOP submits a more-or-less Annual Report that assesses progress and directions in the Program based on its participation in the periodic Florida Bay Science Conference. Previous Annual Reports were produced in November 1995 (Boesch et al., 1995) and February 1997 (Boesch et al., 1997). The authors of this third report are those panelists and alternates who were in attendance at the Science Conference. However, Drs. Armstrong, Deegan, and Yentsch also reviewed a draft of the report and contributed comments.

The third Florida Bay Science Conference was held on May 12-14, 1998, in Miami, Florida. There were 33 oral presentations made at the Conference, many of which summarized results from several related projects (Anonymous, 1998). These were organized around the five central questions identified in the Strategic Plan for the Program (Armentano et al., 1996), with additional presentations on upstream assessments and paleoecology studies relevant to several of the central questions. A member of the PMC introduced each Central Question, and the groups of presentations were followed by questions from the FBSOP and audience and general discussion among the presenters. The oral presentations are cited here by reference to the last name of the first author. A large number of posters also were displayed during the Conference.

The FBSOP also arranged for *ad hoc* committees of expert reviewers in specialized subjects to participate in three workshops where critical science issues were addressed during 1997 and 1998. For continuity, members of the FBSOP chaired each of these committees. Reports from the committees were submitted to the PMC on the three workshops:

- the higher trophic level initiative, November 4-5, 1997 (Deegan et al., 1998);
- seagrass modeling (Williams et al., 1998); and
- paleoecology and ecosystem history (Boesch et al., 1998).

In addition, the Model Evaluation Group, a standing advisory committee operating under the FBSOP auspices met just prior to the Science Conference on May 11, 1998 and will shortly issue a report.

GENERAL OBSERVATIONS AND RECOMMENDATIONS

1. **Synthesis Reports.** The Interagency Florida Bay Science Program has reached a level of duration and accomplishment that Synthesis Reports would be very useful in forging scientific consensus, guiding future research and monitoring, and informing environmental and resource managers, policy makers and the general public. Such Synthesis Reports should: address each of the Central Questions (other overarching or highly specific reports also may be warranted); extensively use Program results; present the current understanding and uncertainties; and be cogent and easily readable. They should be completed in approximately a one-year time frame. They should be regarded as interim, rather than final, reports except in those instances in which major study elements are being concluded. Electronic communications media (websites, CD ROMs, etc.) should be used to supplement printed reports.
2. **Research Teams**. The FBSOP has previously stressed the importance of Research Teams of investigators in program integration and direction, ensuring rigor in interpretation of results, and consensus building. Of particular importance is identifying points of agreement and disagreement and future measurements or analyses needed to resolve disagreements or address emerging questions. While some Research Teams have been formed and are active (e.g. physical oceanography) or have been recently begun planning activities (seagrasses and higher trophic levels), other issues cry out for teams to work actively on interpretation and program direction (e.g. paleoecology, modeling and nutrient-algal bloom dynamics). Active communication within and among the Research Teams prior to workshops could make these workshops more goal-oriented and effective. Furthermore, the Research Teams should be taking the lead in development of Synthesis Reports and formulation of timetables, as discussed below.
3. **Timetable**. Although, the FBSOP has commended the Strategic Plan for the Interagency Florida Bay Science Program as well-focused and exemplary, the Plan lacks a timetable for implementation. Consequently, it is unclear how a set of “mature” program activities, such as those related to paleoecology and ecosystem history, are concluded and how open-ended other new activities, for example, the higher trophic level initiative should be. Of course, we realize that timetables depend on funding (push) and the timing of information needs (pull) as well as inherent limits to the pace of science. Nonetheless, we firmly believe that an overall program timetable, closely linked with the Central Questions, would be extremely helpful in making resource allocation decisions, inspiring focus, developing syntheses, and improving accountability to sponsoring agencies.
4. **Scientific Program Manager/Chief Scientist**. The FBSOP is pleased to learn that a full-time science program manager, as previously recommended by the FBSOP and specified in the Strategic Plan, is expected to be appointed shortly. Attention to synthesis reports, the organization and coordination of Research Teams, and development of performance schedules and timetables, as recommended above, should be central priorities for the scientific program manager.

5. **FBSOP Accountability**. The FBSOP appreciates the written responses it received from the Program Management Committee following the Panel's February 1997 report. However, as indicated in that report, it would be helpful if a "score card" of accomplishments and responses to various FBSOP recommendations could be received just prior to the next Science Conference and Panel review. In addition, there is a need to improve on-going communication about Program progress and issues to the FBSOP so that the panelists feel less "in the dark" leading up the annual review. Toward that end, perhaps the Scientific Program Manager could produce informative, routine briefing documents that would also serve to improve communication among principal investigators and with the management community.

6. **Geographic Expansion**. The PMC has recently been given broader geographic responsibility for interagency science activities in the marine and coastal environments of South Florida beyond Florida Bay, including Biscayne Bay, the Florida Keys and reef tract, and southwestern Florida. While this makes some sense from a management perspective and reflects the successes of the Florida Bay PMC, the FBSOP is concerned that geographic expansion should not dilute resources or PMC attention to the critical issues related to Florida Bay. It is our opinion that the Interagency Florida Bay Science Program has considerable financial resources that are adequate to address the Central Questions in a timely fashion. However, this would not be the case if those resources, without amendment, had to be applied to the science needs for the reef tract and southeastern and southwestern regions as well. The PMC should develop an organizational strategy that allows it to address its geographically expanded mandate while preserving necessary attention on Florida Bay.

PERSPECTIVES FROM THE 1998 FLORIDA BAY SCIENCE CONFERENCE

The inclusion of a number of new members on the FBSOP challenges the Panel to get "up to speed" on the very extensive and complex research being undertaken in the Interagency Florida Bay Science Program and by others operating outside of the auspices of the program, but it also affords some fresh perspectives. New Panel members were struck with the extremely interesting interweaving issues and challenges to environmental science and by the outstanding opportunities afforded because of the resources available to the program and the need to influence management decisions. For these reasons, the Interagency Florida Bay Science Program is very important on regional, national and global scales. While admitting some naiveté and yet incomplete understanding, the perspectives of new Panelists potentially reveal some truths which those close to the program, including veteran FBSOP members, may not see. In that regard, a strong impression is that despite the well-framed Central Questions, the Program lacks a tight matrix of organization under which the research can function and be interrelated and under which the various agency programs complement one another. The candid, overall impression is that the architecture of the Program is at least in part based on fitting in the science interests of individual agencies, their intramural scientists and extramural partners, rather than visualizing the final structure and identifying the components needed to build it.

Below the Panel provides evaluation and recommendations regarding investigations that address each of the five Central Questions of the Strategic Plan plus the Paleoecology Program. Some of the questions (e.g. Central Question #1) are treated in greater detail than others (e.g. Central Question # 5). This is a reflection of a number of factors, particularly the stage of development and integration of studies which address each question and the recent completion of relevant workshop reviews, rather than the relative level of importance assigned to the questions.

PALEOECOLOGY AND SEDIMENTOLOGY

Paleoecological and sedimentological studies were treated separately from the five Central Questions because they provide historical insights relevant to several of the questions. Progress in developing the paleoecology and ecosystem history of Florida Bay was recently reviewed at a January 1998 workshop and reported on by an *ad hoc* committee of the FBSOP (Boesch et al., 1998). Therefore, the Panel's comments here are abbreviated. Also, comments are made concerning the presentations given during the paleoecology and sedimentology sessions under discussions of several of the Central Questions, below.

Two particularly important issues emerged during these presentations and the ensuing discussions. First, the informal consensus which the *ad hoc* committee reported (Boesch et al., 1998) that the salinity of Florida Bay had increased during the last half of this century was challenged by Swart. Second, the important and provocative historical analysis of satellite imagery by Stumpf provided new perspectives on the time course and distribution of seagrass die off and algal blooms from 1985 to the present.

Swart and co-workers have used detail chronology and geochemical proxies in banded head corals to derive a rather careful analysis of environmental conditions in the lower Bay. The ability to relate these parameters and history to the inner Bay, however, rests on the extent to which they represent conditions throughout the Bay and can be related to proxy parameters in calcareous microfossils laid down in unconsolidated sediments as studied by Cronin and his USGS colleagues. This is an extremely difficult problem, however, because chronological controls in sediment cores are inherently less accurate than annual banding in a sessile coral, and the geochemical proxies used also seem less certain. Added to this probable mismatch, moreover, environmental histories in the inner and lower Bay may be quite different. Nevertheless, these types of disconnects must be addressed if there is to be any hope of constructing an environmental history of Florida Bay. Proxies must be constant or extreme care must be taken to relate dissimilar proxies.

Halley presented the results of an informal survey conducted among Florida Bay researchers following the January 1998 Paleoecology Workshop. The investigators were asked to provide estimates of salinity conditions in Florida Bay over the last century. This is a very important step toward answering Central Question 1 and defining ecosystem restoration goals, but, in itself, it does not provide sufficient synthesis. Rather, the Paleoecology Research Team should as a matter of priority build on this to produce a consensus reconstruction of salinity trends and variability in Florida Bay, particularly as they may be related to climatic variability and water management practices.

Stumpf's synthesis and interpretation of AVHHR imagery provides useful insight into the sediment dynamics of Florida Bay as well as into the dynamics of algal blooms and seagrass die-off and recovery (discussed below). Furthermore, Prager's coupling of observations and models of sediment resuspension shows the importance of biogenic binding of sediments. There are opportunities to interrelate Prager's model of critical wave height-wind velocity and Stumpf's satellite observations to provide a larger scale and historical perspective.

Finally, Orem presented results of studies of down-core geochemistry as it relates to nutrient and seagrass history. While these results are intriguing, we caution against over-interpretation of trends, particularly when the results are based on a limited number of cores that often show different patterns.

CENTRAL QUESTION #1

How and at what rates do storms, changing freshwater flows, sea level rise and local evaporation/precipitation influence circulation and salinity patterns within Florida Bay and the outflow from the Bay to adjacent waters?

This question, as with the other Central Questions, is well posed and should serve as a strong focus for the investigations. Although the physical modelers and observationalists seem the most together of any of the research teams, the driving question does not appear uniformly foremost in the minds of the all participants. Variation in the degree of focus is perhaps to be expected at this stage of the research, yet the time for specific recommendations to support management actions seems to be drawing nigh. Regardless of this timing, the duration of research support must be considered. Specific time lines and goals would prove helpful for both research and management.

Evidence for decadal shifts in salinity associated with runoff and evaporation was presented in the paleoecology studies discussed above. Swart's conclusion that there was no increase in salinity during the last half of this century is based on extending observations made in corals in the Atlantic Transition Zone to the Central and Eastern Bay based on autocorrelations among contemporary salinity records. However, the correlative extension of this result northward does not preclude significant salinity shifts in the regions closer to the coast most likely to be affected by water management practices. The correlations provided by Swart are simply not sufficiently strong. Furthermore, the inshore signals can be highly correlated with the offshore signal, but have markedly higher amplitude. The FIU/SERP water quality data set is attractive, both in its record length and its spatial coverage (which includes the central portion of Florida Bay, a region not well covered in other salinity surveys). But, this data set does not seem to be extensively used by many investigators in their attempts to address Question No. 1. Although interpretive maps and other products based on these data are available on a website and in reports (e.g. Jones et al., 1998) the full database does not appear to be accessible. Without such wide accessibility to these essential data, the ability to achieve a successful resolution of Question No. 1 is severely compromised.

The University of Miami (Lee) observations seem well suited for answering the majority of boundary condition issues, including the nearshore structure of buoyancy-driven flows and the larger-scale throughflow if the region. The current meter array covers the entering flow from the northwest boundary, which is an important region from the standpoint of nutrient inputs and pathways. Boundary conditions are also well sampled during the serial surveys along the Keys and through the array. The Lagrangian measurements are extremely valuable here, but unfortunately they are few. Some specific effort should be made in conjunction with the Eulerian measurements to gauge the representativeness of these temporally and spatially sparse measures. Moreover, as previously noted by the MEG, some effort should be undertaken to examine the vertical flow structure over the central portion of the Bay, even to the point where a high-resolution ADCP be placed close to the existing sediment surface in the deeper portions of an interior basin.

The preliminary nutrient budget suggests that a specific effort should be undertaken, perhaps with additional current meter arrays, to address the nutrient input (and export) across the entire western boundary of the model domain, particularly across the banks and channels separating the Gulf Transition Zone and Western Florida Bay. There is an obvious difficulty here in dealing with small differences between large numbers because the western boundary is deep compared to the shoals of Florida Bay. A set of modeling scenarios should be reserved for addressing nutrient pathways and budgets.

For the salinity balance in this region freshwater flow and evaporation are of paramount importance. The hydrological model appears as the only source of freshwater flow estimates, and they are possibly off by a factor of two or more in some regions. Apparently, the importance of evaporation measurements (despite their difficulty) has been recognized and a program has been initiated. This program should include some attempt to measure spatial structure of evaporation; wind patterns and local circulation are expected to create such structure in this region.

Nutrient input estimates, including those from the atmosphere, seem notably sparse, given their importance to the Florida Bay program. Although nutrient concentrations may be related to flow out of the Everglades, the relationship is insufficiently tight to support the assumption that we can provide accurate estimates from flow measurements alone.

The NEXRAD rainfall measurements appear to be sufficiently accurate that the rainfall map products will prove a valuable tool in analyzing the salinity trends. However, it is not clear how these maps are being analyzed or incorporated into the trend detection efforts. Furthermore, it is not clear how the effects of storms are being addressed within this program.

Ground water and its role in freshwater flux to the Bay constitute a major problem that clearly has significance to salinity and circulation and issues regarding nutrient inputs: to what extent are dissolved constituents discharged to the ocean via ground water as opposed to surface and riverine discharge, and where? Brand et al. suggested that a major groundwater source enters the Bay near Cape Sable and offered as evidence high concentrations of both dissolved P and radon. In the two posters by some of the same investigators, however, the picture is somewhat less clear. Brand and Top show one set of data, whereas Burnett and Chanton show no obvious link between P and radon concentrations; in fact, some of the Florida State University cruises found no P anomalies near Cape Sable. Thus, while the authors in the Brand et al. oral presentation state one thing, separately their posters seem to show results that apparently are contradictory. Taken another step, this seeming lack of communication is seen in the USGS study that shows pore water concentrations within a number of bore holes (on land and in bay). Why were P and radon (as well as other stable and unstable isotopes) not run on waters from these bore holes? Were the USGS scientists communicating with the University of Miami and FSU scientists? This deficiency in ongoing communication among investigators was apparent in the criticisms by USGS scientists of Brand’s restatement of the "river of sand" hypothesis and his lack of awareness of coring results, which showed that this subsurface deposit contains significant calcareous inclusions and is nonconductive.

The USGS wave measurement and modeling effort is very relevant and of high quality. The application of the HISWA model and its calibration to seagrass and bare bottom was innovative and successful. Given the importance of flow over mudbanks during high tide, a cooperative effort should be undertaken between USGS, RSMAS, and the WES modelers to address this question. Admittedly, flow over the banks will require some innovative adaptation of instrumentation such as ADVs and pressure sensors that can be placed in such a climate without altering the sediment structure. Obviously, flow through the channels should be monitored concurrently.

The physical regime inside of Florida Bay proper is not as clear as claimed. For instance, Lee shows impressive isopleths based on his cruises around the Bay—until one realizes that his isopleths for the lower Bay are based only on deep-water measurements to the west and east of the Bay; they were not based on measurements in the Bay itself! There is a need to integrate the underway hydrographic measurements made by the University of Miami shallow-draft vessel, the FIU water quality data, and the oceanographic observations made outside of the Bay proper.

CENTRAL QUESTION #2

What is the relative importance of the influx of external nutrients and of internal nutrient cycling in determining the nutrient budget of Florida Bay? What mechanisms control the sources and sinks of the Bay's nutrients?

The sources of nutrients to Florida Bay were the subject of a number of talks and posters during the May 1998 Science Conference. Although the nutrient source and effects issues remain contentious, more quantification is emerging and quantitative syntheses are being undertaken, offering hope that this Central Question can be answered in the near future.

Nutrient Budgets

While the collection of data on nutrients, chlorophyll, oxygen and salinity continues over Florida Bay and incoming waters, there is finally a first cut at an annual budget for the entire system (presented by Rudnik et al.). As is typical for budgets of this type, the dissolved organic forms of nitrogen and phosphorus dominate both in the marine and freshwater sources.

The panel recommends that attention be paid to the question of the use of the organically-bound nutrients by microbes including bloom algae. This is not an easy question, so definitive answers are not expected. Instead, the latest information could be brought into the information pool and perhaps some clever experiments carried out to produce a rough idea. After all, if the residence time of water in Florida Bay is three to four weeks, then there is enough time for the DON and DOP to become available to algae. Another and related question concerns the large quantities of nutrients that appear to enter the Florida Bay system from the northwest. To what degree do these nutrients traverse the Bay through the channels between the mudbanks?

The budget approach leads to the conclusion that P in the freshwater input is unimportant to Florida Bay, that the N input in freshwater is large enough to be important, and that the marine input is most important. This is true for inorganic and organic forms. However, budgets of this type can be misleading because ocean inputs usually consist of large quantities of water with low concentrations of nutrients. The question remains of the actual impact of this material on the biota.

Finally, the information presented shows that the impact of nutrients derived from sewage from houses and villages of Florida Keys is a very small fraction of the amount entering Florida Bay. This does not mean that sewage-nutrients are unimportant but that their impact is low and probably confined to the nearshore environment of the Keys.

Nutrient Geochemistry and Cycling

The studies of phosphorus-carbonate geochemistry continue to provide information on the fundamental chemistry. While these are valuable, concurrent studies should be begun using carbonate sediments from the natural system. In other words, both laboratory and field experiments should go forth.

Hitchcock et al.'s report on a study of western Florida Bay pointed out that concentrations of inorganic nutrients in water near the river mouths were only sufficient to sustain algal productivity for a few days. Obviously nutrient recycling dominates as the only source of nutrients for algae. Some rate measurements are needed of the recycling (isotopes?) and of controls.

Water Quality Model

The presentation by Dortch and Cerco outlined the numerical water quality model of Florida Bay. This model will be used "to assess nutrient mass balance and fate and to evaluate the impacts of a freshwater diversion management alternative." Because the model will include planktonic and benthic nutrient cycling components, it is intended to help scientists put bounds around the nutrient recycling implications of the primary productivity data. The modeling activities were the subject of a separate one-day review by the Model Evaluation Group, but we here offer a variety of perspectives from the multidisciplinary FBSOP based on presentations and discussions during the Science Conference

The RMA-10 hydrodynamic model should be completed and validated as soon as possible. Output from this hydrodynamic model will be used to drive the water quality model. The FBSOP and Model Evaluation Group has previously expressed some concerns about the feasibility of this linkage and we suggest that the functionality of this linkage be demonstrated as soon as possible. Moreover, it is evident that the biological and geochemical components of the water quality model have not been tested and accepted by Florida Bay researchers. The Panel recommends that the various components of the water quality model be explained to and discussed with geochemists and ecologists working in the Bay so that the latest understanding of the various processes involved can be appropriately incorporated.

The PMC and investigators have made some advances in the cooperative efforts needed to develop the water quality and seagrass models. As discussed above, the physical data collection team has come together in an exceptional way to advance a broad scientific understanding of the Bay that simultaneously supports the various modeling efforts. Aikman, Swain and other modelers are clearly committed to providing ocean and freshwater boundary conditions for the RMA-10 and water quality-seagrass models. As also previously mentioned, investigators of nutrient dynamic processes are advancing scientific understanding while beginning to develop budgets and define important boundary conditions needed for the water quality model. The seagrass team has been advancing insight into physiological and ecological processes of this important resource asisted by the October 1996 modeling workshop and the January 1998 seagrass modeling workshop. Nevertheless, the failure to involve the primary WQ-seagrass modelers in the January 1998 workshop was a missed opportunity to advance coordination of research and development.

Overall, a useful suite of hydrodynamic, salinity, water quality and seagrass models is under development. Budget shortfalls have left one gap that the USGS has filled with the FATHOM model. Dortch questioned whether a coarse, basin-scale salinity and nutrient mass balance is needed in the near-term and dropped this from the COE modeling plan. Yet, Johnson clearly shows the need for simpler model to guide development for Florida Bay and the Everglades. Jackson needs water quality exchange between basins for investigation of trophic-level cycling of carbon and nutrients. Rudnick's nutrient mass balances should be followed with a coarse-scale box simulation that could be based on the FATHOM compartments.

From a water quality modeling perspective, FATHOM is a new box model, the descretization of which has not been adequately coordinated with other model efforts. A new model requires extensive review before use in management decisions. FATHOM was peer-reviewed in 1995 and the scientific validity of the linear-reservoir theory called into question. The model ignores baroclinic, and for that matter, vital residual circulation. Therefore, the PMC is faced with a decision: is a reliable coarse-scale box model needed to address a variety of ecological questions and, if so, should FATHOM be validated and further peer-reviewed or should the COE WQ modeling team be commissioned to use the (marginally acceptable) RMA-10 and the (more valid) CE-QUAL-ICM to provide information useful to biologists and other project scientists over the scales of interest? The October 96 modeling workshop favored a coarser-scale CE-QUAL-ICM application to conduct a 1998 nutrient budget. The MEG should be asked to specifically address this issue in an upcoming meeting. So as not to put Nuttle et al. at a disadvantage they should also provide a fuller interim report to members of the MEG.

Finally, the progress in coordination should be stimulated by more specific definition of restoration goals. The Restudy seems to demand specific time lines to generate an impact assessment before the final alternative is selected. Research teams have generated specific conceptual models and specific hypotheses to be tested. The role of the WQ-seagrass model and the RMA-10 model in (1) testing these hypotheses, (2) providing critical flow and transport data to research groups, (3) testing other specific scientific hypotheses, and (4) simulating management scenarios, is only marginally defined. Both the PMC and the COE modelers (and USGS and FATHOM developers if this model is to be adopted for hypothesis testing and management simulation), should define specific modeling objectives, set definitive time lines, and formalize the process research support in a few critical cases like trophic level kinetics, sediment dynamics, and seagrass dynamics and effects.

The RMA-10 model seems to have reproduced tidal height and salinity reasonably. This necessary but not sufficient step in the calibration and verification process has been overdue. However, although tidal exchange is likely to constitute a major transport mechanism for salt, the residual circulation is of paramount importance to the ecosystem processes of concern. For this reason, we recommend more direct interaction with the physical oceanographic observational program to test the ability of the model to accurately simulate the residual circulation. These simulations should go beyond matching the seasonal mean flows. From the observations, a set of synoptic (5-10 day) scenarios should be developed that describe responses to the most common patterns of physical forcing.

Previously, the MEG and FBSOP recommended that the need (with respect to answering important management questions) for three-dimensional considerations in at least some components of the models be evaluated. Despite the shallowness of the majority of the Bay, two-layer, wind-driven circulation is expected, especially over the middle and western basins. Two-layer flow may also be especially important, as acknowledged, in the vicinity (20-30 km) of the buoyancy sources, especially the Shark River to the west. One of the most important products from the model is salt and nutrient transport. In a shallow region such as this where tidal oscillations and wind-driven flows dominate, substantial cumulative errors can result when the third dimension is ignored. Furthermore, many biological questions are likely to involve the vertical dimension. Biological transport is inherently Lagrangian; constructing particle trajectories from models requires high Eulerian accuracies. The need to include three-dimensional considerations in the models should be explicitly evaluated and these evaluations reported to the MEG and FBSOP.

The multiplicity of models should be regarded a strength of the program, and not a sign of unnecessary duplication. With modeling being so fundamental to the effort here, the series of complementary models is attractive. In that regard, could the NOS POM model be extended into Florida Bay with reduced coverage outside the region? It may serve this application, despite the sigma-coordinate pressure-gradient difficulties over steeper topography. Moreover, NOS is commended in its provision of offshore boundary conditions for the WES model.

The WES water-quality modeling group has demonstrated great skill in reproducing basic stocks and rate processes in the Chesapeake Bay. They have simulated primary production and seasonal oxygen depletion with remarkable accuracy. However, when this model has been extended to higher-order ecological processes such as submerged aquatic vegetation, the results have been less successful. This difficulty is by no means surprising, with the science of seagrass ecology not yet at the stage where it can quantitatively support such a model, particularly considering that very different ecological factors may govern the distribution and dynamics of seagrasses in Florida Bay. The water quality model should not be pushed overly hard in these directions, especially because these understandable failures cast unwarranted doubts about even the accurate simulations. If the Florida Bay water-quality model could accurately simulate even the nutrient and production stories, it would be a substantial asset to research and management.

Although it is understood that the modeling efforts are not ready for prime time, it would be more encouraging if there were evidence that the specific applications of these models were firmly in the minds of the modelers. We heard little discussion of specific scenarios that are to be run for either research or management. Five water-management alternatives were presented, but there was no mention from the modelers that these were going to be run, or when. Furthermore, we see these models as invaluable in the attempt to determine the causes underlying observed environmental changes, yet little has been said about model applications for this purpose.

As designed, the utility of the full WQ model for research and management of Florida Bay is limited to prescribed runs. Once calibration and verification have been completed, steps should be taken to reduce grid resolution and improve computational efficiency so that the model can be run conveniently on a fast workstation. Neither researchers nor managers should be assumed to have access to supercomputers for application of this model to Florida Bay. Given the importance of models for research and management of this complex ecosystem (especially to help in developing accurate water, salt, and nutrient fluxes), and given the state of the modeling art (and the speed of workstation computers), models with sufficient resolution, but not so detailed as to preclude running on a workstation, should ultimately be developed and be made available to the research and management communities.

CENTRAL QUESTION #3

What regulates the onset, persistence and fate of planktonic algal blooms in Florida Bay?

Continued research has focused on the distribution of phytoplankton biomass, composition and production rates in space and time, nutrient limitation, and zooplankton grazing rates. Based on station-based measurements by Steidinger, Tomas, and Boyer, satellite imagery interpreted by Stumpf, and areal observations of water color by Flamm, planktonic algal blooms within the Bay proper have been centered in the northern part of Central Florida Bay, particularly around Rankin Basin. However, depending on wind conditions these blooms may spread outside of this epicenter. Steidinger has found that these blooms are dominated by resident cyanobacteria (*Synechococcus*), small diatoms (*Cyclotella coctawatcheeana*), dinoflagellates and picoplanktonic flagellates. Occasional blooms in Western Florida Bay tend to be diatoms associated with intrusions of advection of water from the Gulf Transition Zone.

Steidinger characterizes high microalgal biomass as chlorophyll *a* concentrations of >5 $\mu g\ l^{-1}$ while nuisance blooms are > 20 $\mu g\ l^{-1}$. Biomass can reach can reach 40 $\mu g\ l^{-1}$. This can be contrasted with Stumpf's estimates from satellite imagery of chlorophyll *a* concentrations <1 $\mu g\ l^{-1}$ outside of Florida Bay, in Eastern Florida Bay, and in the Bay prior to November 1988. Notably, Tomas et al. (presented by Bendis) reported that chlorophyll *a* levels and primary production rates have declined in Central Florida Bay since the end of 1995.

The nutrient limitation bioassays conducted by Tomas et al. and Brand, the Central Florida Bay region is a zone of transition between strong P-limitation to phytoplankton growth to the east and primarily N-limitation to the west. Thus, the sources and dynamics of both major nutrients in the region are important to understand. Richardson has been experimenting with batch cultures of dominant phytoplankters to determine the importance of competition of nutrients among the taxa. Tomas et al. have also developed phytosynthesis-light curves and concluded that light in this shallow, but occasionally turbid system is always above the compensation intensity for phytoplankton.

Ortner summarized studies of zooplankton composition, production and grazing rates. While mesozooplankton may be trophically important as food for fish larvae, it is clear that mesozooplankton grazing is too low to influence or regulate phytoplankton production and algal blooms. Micozooplankton grazing may be more important, but it is not clear that the ongoing studies will provide an understanding of microzooplankton-phytoplankton dynamics at the appropriate time scale of those dynamics. Jackson presented results from his developing models of the flows of carbon and nutrients between different trophic groups and their interaction with the benthos. Early results show that these flows vary dramatically depending on assumptions about the role of bacteria and dissolved and particulate carbon pools—processes not being directly measured in the program. While the zooplankton and planktonic modeling research is of high quality, a strong case for the importance of zooplankton as important factors in the onset, persistence and fate of algal blooms has not yet been made. The role of the microbial loop and benthic-pelagic exchanges loom as important factors that are not being addressed. In addition, these studies currently lack a clear tie to the WQ model and, thus, to the management issues it is intended to address.

Despite this growing information on bloom phenomena and limiting factors, process studies leading to a full understanding of the formation and persistence of algal blooms in Florida Bay remain lacking. Given the variety of candidate explanations for the blooms and seagrass die-offs in the north central region of Florida Bay, a special focus on the causes and interrelationships of the die-offs, blooms and pelagic and benthic grazing in that area is merited. Distinguishing among allochtonous and autochtonous and benthic and pelagic sources of nutrients is difficult in this shallow water column, but it appears that understanding nutrient dynamics and their relationship to phytoplankton production are central to the question of whether there has been an ecosystem shift, and, if so, what the causes may be.

CENTRAL QUESTION # 4

What are the causes and mechanisms for the observed changes in the seagrass community of Florida Bay? What is the effect of changing salinity, light, and nutrient regimes on these communities?

The results presented at the 1998 Science Conference echoed to a significant degree many of the results presented at the Seagrass Model Workshop, held in January 1998 (Williams et al., 1998). However, there were also important new results presented at the Science Conference, that addressed issues raised at the Workshop, as well as some progress reported in implementing Workshop recommendations. These new developments are the primary focus of the following discussion.

First, it is important to note that changes in seagrass distribution continue to take place, with some areas gaining and some losing seagrasses (Durako et al.; Eichinger et al.; Zieman et al.). In general turtlegrass (*Thalassia*) seems to be declining in the western Bay and on some mud banks, while shoalgrass (*Halodule*) is increasing in abundance. Continuing efforts will document further changes in seagrass abundance and species composition, and recently enhanced monitoring of PAR and other abiotic variables (a Workshop recommendation) will provide data for statistical modeling of relationships between physico-chemical variables and seagrass biomass and species composition.

Of special significance to Central Question 4 were the presentations by Cronin et al., Hood et al., Swart et al. and the poster by Brewster-Wingard et al. These contributions, while not part of the agenda for Central Question 4, described attempts to resolve the prior distribution of seagrasses in Florida Bay. Stumpf et al. used AVHRR satellite imagery to reconstruct patterns of seagrass distribution from July 1985 to the present. Their conclusion that significant and previously undetected large seagrass losses occurred west of Everglades National Park before the well documented changes within the Park boundaries, suggests that seagrass losses might be much greater than originally documented. Stumpf et al. also note seagrass recovery during the 1991-1997 period. While additional groundtruthing in the western grass beds is advisable, continued analysis should provide valuable information on the recent past and current status of the seagrass resources in the Bay. The contributions of Cronin et al., Hood et al., and Brewster-Wingard et al. rely on the use of seagrass-associated microfossils and mollusks to reconstruct conditions hundreds to thousands of years ago. This work is based on small sample sizes, but holds promise for indicating past patterns in seagrass presence and absence. Selected results suggest that seagrasses have fluctuated substantially at various times in the past, but appear to have generally increased in abundance during the 1900s. Concurrent salinity changes may be involved with prior seagrass changes. Whatever the cause, these paleoecological studies are important for placing the recent changes in seagrass abundance into the historical context of cyclic patterns of increase and decrease.

There are very important studies that remain to be done of the single and combined effects of salinity and temperature change on seagrass survival and growth, which have been strongly implicated by Zieman in the initial loss of Florida Bay seagrasses. The mesocosm facilities and studies described by Chiapouras and Montague and Anastasiou and Montague hold promise for answering important questions about the environmental tolerances of the various seagrass species. However, operational difficulties have delayed the production of useful data. Given the importance of knowing the tolerances of the dominant seagrass species to changing patterns of salinity and temperature, it is imperative that high quality data from mesocosm studies become available soon. If the present efforts cannot bear fruit in the very near future other alternatives for obtaining such data should be sought.

Studies of the possible role of *Labyrinthula* infestation in the loss Florida Bay seagrasses continue with the presentation by Blakesley et al. Current emphasis is on field mapping of infestation and laboratory studies of the effects of salinity on pathogenicity. What is not clear at this time, however, is the extent to which *Labyrinthula* causes seagrass mortality under various field conditions, or whether it was a causative agent in the initial or subsequent seagrass die-offs. After the substantial amount of time that has elapsed since the hypothesized role of *Labyrinthula* in Florida Bay seagrass losses, it is now time that these critical questions be answered as conclusively as possible.

Recent information by Kenworthy et al. on substantial losses in the large *Syringodium* meadow to the west of the Park boundaries are cause for concern, especially since Stumpf et al. suggest that this is where major seagrass die-off began in 1985. The disappearance of seagrass and subsequent sediment erosion discussed by Kenworthy et al., presumably owing to reduced water clarity, suggests that if conditions continue unchecked there will be massive amounts of sediment (and nutrients?) flowing southward toward the reef tract. In addition, Sharp et al. document the destruction of substantial amounts of this *Syringodium* meadow by sea urchin overgrazing. The exact areal extent of the grazing losses is unknown, but estimated to be at least 10 square kilometers, and perhaps four times this amount. The fact that this “urchin outbreak” has persisted for the better part of a year, with urchin densities reaching several hundred per square meter, and the fact that *Syringodium* is unlikely to substantially recover from a grazing stress of this magnitude, suggests that this is significant event. Given the ongoing threats to this large and strategically placed *Syringodium* meadow, expanded study of its dynamics are needed promptly.

CRITICAL QUESTION #5

What is the relationship between environmental and habitat change and the recruitment, growth and survivorship of animals in Florida Bay?

A strategy to address this Central Question was discussed at a November 1997 workshop, reviewed in a FBSOP report (Deegan et al., 1998) and presented in a draft report of a Higher Trophic Level Workshop Group (Browder et al., 1998). This draft stragegy was presented at the Science Conference by Eklund together with results from ongoing studies on pink shrimp recruitment (Browder), mesocosm studies on the effects of nutrient enrichment on spotted seatrout larvae and their zooplankton prey (Clarke), the effects of habitat on fish larval growth (Hoss), modeling the effects of habitat dynamics on spiny lobster recruitment (Butler), changes in the distribution of molluscan communities (Lyons), and potential toxic effects of agricultural chemicals on higher trophic levels (Scott).

The FBSOP has the following summary comments on the direction and current scope of higher trophic level studies:

- The recommended research program strategy is a major step in the right direction and will help avoid duplication of efforts previously observed (Boesch et al., 1997), focus on priority issues, place results in the context of population and/or trophic models which provide understanding of cause and effect and improved predictive capabilities, and relate the dynamics of higher trophic levels to important environmental changes (seagrasses, algal blooms, salinity shifts, circulation, etc.).

- Having said this, it is also clear that the research plan is too broadly focused and open ended. It outlines many “life works,” includes too many species, and is not based on realistic schedules. The FBSOP was struck with the focus and elegance of the approach to modeling of spiny lobster dynamics by Butler, which allows research to be focused on issues critical from the lobster’s perspective, rather that trying to fill all the boxes in trophic or ecosystem models (Browder et al., 1998).

- Mesocosm studies may allow more realistic experimentation. They could provide a bridge between laboratory experiments such as those by Hoss and field observations. However mesocosms are fraught with potential difficulties in operation and interpretation. Clarke’s experiments did not deal effectively with such classic mesocosm issues as vertical mixing and yielded results which are hard to interpret with regard to the complexity of potential food chain interactions.

- Fundamental to the effort of separating anthropogenic changes from natural cycles is the effort to relate the recently observed processes and their variability to long-term records. Clearly, the paleoecological work addresses this directly, but for other studies, especially in the higher trophic levels, the interval of intense investigation seldom extends beyond a decade, and most only cover 3-5 years. For the pink shrimp, 60 years of record were mentioned. Would this record support a retrospective analysis, bolstered by the new knowledge amassed by the Florida Bay Program? For longer-term perspectives, Stumpf's satellite work is of great value. The inshore data appear to be corroborated by *in situ* measurements. The major shift indicated seaward of the Bay should be investigated, not only for further validation of the model, but also for determining the cause of the offshore shift. Such a dramatic large-scale shift would may be related to changes in the Bay ecosystem that may be of consequence to fisheries production and catch statistics.

- The ecotoxicology studies have thus far failed to establish a case that toxic agricultural chemicals are implicated in the broader ecosystem problems (seagrass die-offs, algal blooms, fisheries declines) and, at most, it seems that they may cause problems in the vicinity of drainage canals.

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