

**Human - Environment Linkages in the South Florida Coastal
Ecosystem:
Cumulative Effects of Natural and Anthropogenic Stressors**

Review of Progress to Date

**Standing Technical Advisory Committee Report to
the Center for Marine and Environmental Analyses and
the NOAA Coastal Ocean Program**

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INTRODUCTION

In September of 1995 the Center for Marine and Environmental Analysis at the University of Miami's Rosenstiel School of Marine and Atmospheric Science initiated a complex and ambitious research project funded by the NOAA Coastal Ocean Program (COP). Approximately twenty different principal investigators at seven different institutions have participated to date. The project describes itself as an integrated system of process-oriented ecosystem and seascape level models supported by laboratory and field research that together support an integrated decisions-support framework for the managers and policy-makers of the South Florida Ecosystem Restoration effort and enable comprehensive ecological risk assessment. The explicit objective and litmus test of success is stated to be "policy relevance." Following the guidelines and rationale of a Request for Proposals issued by the COP, the project was organized into specific tasks: Task1-characterizing anthropogenic and natural stressors in the region ; Task2-developing and validating new indicators of stress for key organisms or ecological processes; Task3-characterizing ecosystem level stress effects; and, Task4-developing and applying tools (models) for ecological risk assessment and ecosystem management of the South Florida Coastal Ecosystem. The purported geographic region extended from Biscayne Bay through the upper Keys reef tract south to Long Key which it was argued represented a closely coupled system encompassing a stress gradient from urban to agricultural to pristine. The stresses to be investigated included salinity, temperature, nutrients, turbidity/sedimentation, hurricanes/storms and fishing pressure but specifically not organic or inorganic anthropogenic pollutants.

The project has already received substantial funding from NOAA (ca. \$1.2M dollars) and is scheduled to receive another \$1.4M dollar increment in April 1997. In its first year and a half it has focused first upon Tasks 1 and 2 and then upon Task 3. Most of Task 4 has barely begun. Before finalization of a Year 3 Work plan and issuance of any FY1997 funding the project leadership (CMEA) and the funding agency (the NOAA/COP) felt an outside peer review was both appropriate and necessary. A Technical Advisory Committee (TAC) was formed (see Appendix 1) to conduct this review. This is intended to be a "standing committee" and its' members have agreed to return for subsequent annual reviews. All but one of the TAC members attended a formal program review held in Miami in mid-November 1996. Over a two day period the numerous principal investigators gave detailed presentations of their work to date and their plans for Year Three. In addition there followed an extensive question and answer session between the TAC and the Task Leaders. This report represents a synthesis of the individual impressions of the various committee members who attended the program review. As requested by the CMEA it is organized first into General Comments and General Recommendations and then individual Subtask Comments and Recommendations. Except where specifically noted comments and recommendations represent a consensus amongst the committee members present. As directed the TAC

focused its attention first upon overall project logic, management and cohesiveness and second upon individual project relevance or appropriateness. In addition the TAC was asked to make recommendation on the changes required to fulfill project objectives. An annual standing TAC review is not considered to be a substitute for or in any sense equivalent to a rigorous peer review of individual projects. The document that follows was submitted in draft form to CMEA project leadership and the final version reflects their request for additional clarification. It is being simultaneously submitted to COP and CMEA with the assumption that the latter will distribute it to all of the relevant principal investigators.

GENERAL COMMENTS

The committee was impressed by the assemblage of talented investigators and the results (and promise) of some of the individual SubTask elements. We are hopeful that our suggestions will increase the likelihood that the project will contribute to its ultimate objective: providing managers and policy-makers appropriate and timely scientific information in a suitable organized framework on which they can base restoration decisions affecting the South Florida coastal ecosystem. We also endorse the stated project philosophy that quantitative formalized modeling is central to developing a useful integrative framework. That said, the members of the committee expressed substantive reservations about the theoretical underpinning of the overall project, how it has been managed and organized, what activities have been conducted to date (and are preliminarily planned for next year) and what it can realistically hope to accomplish with the resources it is projected to receive during its project lifetime. While we had no question that the presentations included some strong individual investigator science, they were a truly eclectic mix (what one panel member called "a potpourri of mostly RSMAS ongoing research being force-fit into the NOAA/COP program"); a number failed (or severely "stressed") the stated relevancy criteria and it was clear to none of us how they would ever coalesce into an integrated quantitative model structure. We could not help but note major spatial and temporal disconnects or mismatches between individual principal investigator efforts (both field and modeling) as well as a recurring tendency to blur the distinction between conceptual and quantitative modeling. Some of these flaws derive from the Task and Sub Task structure derived from the initial Request for Proposals. That structure has hindered rather than facilitated meaningful project integration. More than one of our members got the impression (mistakenly) that the investigators had not really met together as a group prior to this formal review and markedly stretched plausibility to explain their work within the overall projects explicit task structure. These flaw may be rectified by a fundamental reorganization. However, other difficulties derive from the underlying logic and rationale of the project and the applicability of its approach to the geographic domain being investigated. These are far more difficult to resolve.

At a basic conceptual level the natural gradients upon which the project was predicated: urban to agricultural to pristine, did not appear to be exploited by many of

the individual studies. Indeed the committee questioned the utility of the gradient concept within this geographic domain since it ill-fits the spatial distribution of the enumerated stressors. As one panel member remarked, "the stressors themselves were not always well defined nor the ability to discriminate the effects of one from another nor the role of synergistic effects". Moreover while the notion of a seascape extending from the mangrove transition zone to the reef tract is undeniably viable for the South Florida coastal ecosystem as a whole, it is patently unsuited to the explicit geographic domain of this project. The dominant coastal circulation pattern assures that if any embayment (and adjacent mangrove transition zone) closely coupled to the reef tract it is Florida Bay not Biscayne Bay! Biscayne Bay can influence at most only a small fraction of the reef tract for only a few months of the year. Without question this inherent problem resulted in the observed spatial and temporal discontinuities between individual project elements, Tasks and Sub Tasks.

We cannot overemphasize our concern that the underlying conceptual framework of the program overall offered little or no guidance to the participating principal investigators in designing or implementing their sub-task responsibilities. The principals would we felt be far better served by articulating clear and testable central hypotheses (rather than loose conceptual models). In the opinion of one panelist "...I believe that the way this program was initially organized, adhering too closely perhaps to the RFP, seems to have steered the work away from studies organized around sharply defined hypotheses and, in appearance at least, towards a more nebulous goal of dealing with all possible stressors and all possible effects. This latter goal is, of course, impossible." Another observed, "the problem starts right at the beginning where the title implies much more than the project can deliver".

The conceptual and structural problems discussed above made this a very difficult project to review. While individual elements were, so far as we could judge, of scientific merit their connection to other elements and their potential contribution to an integrated result was all too frequently obscure. We have tried to do what we were asked to do and no more: to assess the individual projects in regard to the contribution to the overall project and to provide some overall project direction to the management team. Without question the level of technical detail provided us was often insufficient to permit, nor in truth did we ourselves possess sufficient disciplinary breadth, to assure rigorous peer review of each and every so activity.

A substantial minority of the committee expressed concern that detailed research activities (Sub Tasks elements) were implemented on an *ad hoc* basis with minimal, or with insufficient, peer review. All felt the way the budget and personnel information was presented to us was entirely inadequate to enable the TAC to determine the resources actually provided to individual projects. We couldn't tell which project elements were generously funded and which operated on a shoestring or even why these decisions were made. Moreover some of the results presented to the TAC seemed to come from projects other than this. Funding information is an important component of the peer review process and particularly essential to an oversight TAC asked to comment upon the

relative progress (and priority) of individual project elements. It was nearly impossible to determine where the work supported by COP left off and that supported by other sources began. Nor was it possible to determine exactly the level of funds being expended for project management and infrastructure within CMEA. As one of our members wrote, "... NOAA appears to be buying into a vision and associated projects already underway at RSMAS et al. In some cases NOAA will provide the bulk of the support while playing a minor role in others." The TAC encourages project leveraging and feel it is essential that this project be closely coupled to many of the ongoing activities taking place under the aegis of South Florida Ecosystem Restoration (SFER). Nonetheless continuing NOAA/COP support cannot be justified unless the TAC and the COP managers are afforded explicit information on exactly what NOAA/COP is getting for its money.

GENERAL RECOMMENDATIONS

Forcing project elements to define themselves within the current Task and Sub-Task structure not only did not help but seemed to have hindered project integration and principal investigator collaboration. It also complicated presentation and interpretation of resource allocation information (see below). Alternative flow charts presenting project information in various formats: risk assessment (the present approach); habitat/discipline (the one we suggest below as a more useful central organizing principle); phyletic groups; and, physical/chemical stressor (temperature, salinity, nutrients etc.) would have been helpful but may be moot if our recommendations concerning reorganization are implemented.

The issue of spatial domain must be clearly clarified and resolved. Individual project elements were not integrated in part because they weren't even investigating phenomena on similar spatial and temporal scales. The CMEA program needs to resolve the degree to which its boundaries are the ones stated in the Introduction above , some other region (e.g.- the FKNMS?) or is this really a project about the entire South Florida coastal ecosystem?

We believe that to be viable the project must be fundamentally reorganized so as to explicitly use the Integrative and Assessment component as an hierarchical focal point for underlying habitat sub-components (each with their own models of similar design) arranged along some spatial gradient (mangrove fringe - bay -reef tract). Theoretically these habitat components could then be organized into an appropriate ecosystem level seascape model. This assumes individual model efforts became far more explicit as to where conceptualization will leave off and quantitative modeling begin. The conceptual models presented to us seem far too open-ended and ill-defined to elicit any confidence in either their eventual utility or the adequacy of the data available or being generated to support them. Unlike testable hypotheses the purely conceptual models we were shown can in no real sense be "rejected" so they do little to help principal investigators in prioritizing their research. The physical modeling (not a separate logical sub-component but more of a linking function in this reorganization scheme) was at least explicit and

quantitative but how and if it will be connected to the ecosystem models needs to be made explicit. Hopefully a fundamental reorganization/reconceptualization will focus the project and begin to move it in the desired direction; without such a re-invention there appears to be absolutely no possibility of success.

Budget and personnel allocations for each individual project have to be clearly specified in the manner of traditional competitive funding proposals. This must include precise discussion of contributions/essential collaborations outside of the project itself. In that case, as with NSF proposals, to be eligible for funding a project element must provide letters of commitment as to those ancillary funds (or outside collaborations). Given the complex networking within the SFER research community it would have been very helpful to reviewers, and likely the researchers themselves, if pie charts could be provided for each habitat (reef tract, mangrove fringe, sea grass beds etc.) indicating the total and percentage of funding from various sources both for the current year and for year 3. Moreover not all the information required by the models needs to be generated within this program. It is critical that this CMEA project be more complementary too and interactive with other SFER research activities. Last, the funding for CMEA project management and infrastructure must be explicitly delineated and distinguished from research funding. Is this a large or small fraction of the total award. What are these funds being used for? We were unable to answer any of these questions from the documentation provided.

We strongly encourage CMEA to take advantage of some peer review process (beyond annual advice from the TAC as to overall project progress and direction) to guide project evolution as well as equitable and scientifically rigorous redirection of project resources. Some of the committee feel this should be done only in regard to particularly specialized project areas (e.g.-ecological modeling) others feel it is essential in every project element area. We are aware that the original project was subjected to peer review but our evaluation of the program suggests the documentation provided was too general and the reviewers may have been unfamiliar with the South Florida coastal ocean ecosystem.

SUBTASK COMMENTS AND RECOMMENDATIONS

The following are offered not as technical peer review but primarily from the perspective of overall project integration and coherence. Nearly all individual efforts had some scientific merit in and of themselves. We have tried but it was frequently difficult for us (perhaps for the principals themselves) to determine the precise boundaries of the work they were asked to discuss-i.e., the difference between an individual principal investigator's contributions to two (or more) "Tasks". We also recognized that the various projects were in very different states of development ranging from comparatively mature efforts to as yet ill-defined planning exercises.

Task 1 - Natural and Anthropogenic Stress Characterization

SubTask 1a - Hydrodynamic Modeling and Validation for Salinity and Temperature Stresses

There appears to have been little consideration of the spatio-temporal resolution of circulation modeling in relation to the suite of stressors of interest, e.g.- deriving the appropriate geometry in relation to the scales of variation required to produce ecological effects on interest to risk assessment. It might be useful to use recurrence interval calculations (probability of occurrence of a given event or concentration in any given year based on the longest possible record) for these and subsequent Task1 parameters. Surprisingly even within the physical oceanographic projects themselves there has been some spatial and discontinuity -i.e., between the present (and future) model domain and the velocity measurements being made to the southwest. While the latter work is regionally useful information it would appear to be of secondary significance in this context. On the other hand, the circulation in Hawk channel would appear to be particularly important to this project and it has to date received little or no attention. In general there has been a dearth of critical field data along the boundaries of the present model and collection of such data is encouraged. The model needs to be validated over a range of conditions. Some of the committee members expressed concern about to the adequacy of the vertically averaged model even within Biscayne Bay as descriptor of material flux. Plans to extend the model domain out from Biscayne Bay to encompass Hawk Channel and the Reef tract heighten this concern. The difficulties associated with expansion to the larger, deeper and more stratified domain cannot be over-estimated. Moreover given the hypothesized importance of nutrient upwelling along the outer reef tract one wonders the degree to which the reef tract-Gulf Stream linkage can be meaningfully simplified. We remain skeptical about the usefulness of planned deployment of the OSCAR system given its technical limitations in the near field and restricted geographic domain and feel there are far more pressing field sampling needs. We note that little physical measurement accompanies other Task sampling although it would appear to be essential to rigorous data interpretation. Last, we see little or no justification for present and proposed DOM tracer studies and remain unconvinced that even if measurable they will yield useful information. Initial application of a wider field 2-D model would appear to be a reasonable first step however unlikely to be ultimately sufficient.

We have recently been made aware that the ACoE/WESLAB is extending their efforts into Biscayne Bay are currently moving their bottom-mounted ADCP units from Florida Bay to Biscayne Bay. Modeling and physical oceanographic field work conducted within the context of this program should take maximal advantage of whatever resources are available.

The above remarks are offered under the assumption that the physical model of Biscayne Bay and the Bay's interconnection to the upper Keys reef tract remain a central

focus of the overall project. Our general concerns with that hypothesis have already been expressed.

SubTask 1b- Characterizing turbidity, nutrients and sedimentation stressor gradients

Even more substantial spatial disconnects are apparent between the circulation model domain, the explicit overall project domain and where various parameters are being measured by different investigators. Details are sorely lacking about nutrient flux and transformation work. There appears to be little or no cognizance of the substantial literature concerning possible sediment trap artifacts and their possible amelioration. This would appear to be a real concern in this topographically complex environment. We are skeptical that individual traps will be representative in any sense of their position within the purported stress gradients. However the issue is important and some data is better than none at all (the present situation). Moreover, trap data can be checked against turbidity meter data.

Water column data sets (and AVHRR studies) appear to not only be well outside the domain of nearly all the other enumerated project elements (excepting the reef fish studies) but to have little or nothing to do with the seascape interconnection purportedly being studied. If anything they speak to the Florida Bay $\diamond$ FKNMS interconnection which is dominated by transport at Long Key, again just outside the specified domain. Moreover, it is entirely unclear what these water column and remote sensing efforts add to monitoring being conducted by the same investigators as reported to the Annual Florida Bay Science Conference. As we observed earlier, where this work leaves off and that supported by other projects was never clear to us. This is not say no water column monitoring is required! In fact if anything more is required but what data was shown to us appeared to be de-coupled from overall project concerns and objectives. For example mention was made of cyanobacteria as especially sensitive to trace metals but no where else in the program are trace metals being studied? Indeed they are not even one of the enumerated stressors. While AVHRR reflectance data are interesting (assuming a good reflectance to total particulate load/transmittance relationship can be developed) it would appear that sea grass communities in Biscayne Bay are rarely if ever light limited and re-suspension at the reef tract is purportedly a very local phenomenon undetectable at the level of resolution available with satellite data. The wind-wave data analysis from Fowey Rock historical data would seem of minimal import and available models to estimate the wave field would appear to be far more relevant although hardly of high overall priority. Fundamentally contributing to the question of re-suspension was felt to be well beyond the scope of this project and the present effort unsupported by requisite physical (and geological studies). To the degree the topic is relevant it will begin to be assessed by the HBOI meters and we strongly encourage their deployment. use. Indeed we are supportive of the basic sampling regime in which they will be used although exceedingly concerned with its density given the apparent spatial complexity and inhomogeneity of the reef environment. Apparently budget constraints preclude more adequate coverage as well as the physical sampling essential to extrapolating the point measurements to the larger field.

SubTask 1c- Characterization of Stress on Fish and Invertebrates

In contrast to other studies this research appeared relatively mature perhaps because much of what was described to us was an ongoing program that was initiated some years ago. Indeed it was not entirely clear what exactly the additional NOAA/COP contribution was (or would be) adding to the considerably more substantial NMFS-FKNMS contributions to this work. Moreover the stressor being investigated (fishing stress) would appear to have little or nothing to do with the other stressors being discussed. A major inherent problem was the lack of pre-fishing data but that is hardly the fault of these investigators. The domain of interest extended far beyond that of any of the other parameters being measured and the work is not only not spatially and temporally integrated but also not conceptually integrated with any of the other CMEA projects. As to the proposed Y3 work, protected FKNMS areas that have been designated are so small as to call in question the feasibility of the proposed comparisons. A pre- versus post-reserve designation effort in the Dry Tortugas would appear to be of vital significance to the FKNMS but of unclear relationship to this program. There appears to be an additional conceptual difficulty herein in that one could conclude from this work that the system at large had likely been over-fished but no distribution (much less a gradient) of stress was apparent. Arguably fishing pressure has not been intense in comparison to reef systems throughout the Caribbean. As earlier noted in regard to historical data, comparison with relevant regional data would be appropriate. In this context what might prove especially interesting is the difference in impact of commercial and recreational fishing activity in different regions and note with approval the apparent agreement between fisheries independent and dependent data on multiple stocks.

On the other hand at least here the link between stress and ecological effect was clear. For fishing pressure stress a quantitative ecological risk assessment seems to be a very viable task. We note however with concern the apparent omission (despite the title) of invertebrate assessment activities given that fish (and corals) together account for a small percentage (ca. 10%) of overall coral reef community bio-diversity.

Task 2 - Developing and Validating Indicators of Stress

The committee had even more serious difficulties with many of these projects. In general there appeared to be little consideration given to focusing upon keystone species or ecologically critical components. Moreover although biomarkers may be useful at an individual level there is little published evidence that they provide a good way of predicting population level effects. Indeed the link to individual level processes is very much an open question. A paper in press in Ecological Modeling reporting on a study trying to link biomarkers with higher level effects, notes the following: "Molecular approaches such as biomarkers (McCarthy and Shugart, 1990) offer a promising approach for quantifying exposure in nature. Biomarker approaches have been used in a comparative manner to determine if certain locations are relatively more stressed than others (Adams et al, 1992). However, biomarkers have not yet been linked to higher

level effects such as reproductive success, feeding, growth, and mortality because of their high variability that results from fluctuating environmental conditions (Adams et al. 1995). We urge future work in linking biomarker levels to higher-order, individual-level processes such as growth, feeding success and metabolism..." (Jaworska et al, in press). In short this whole topic is very much high risk - high return science. The potential contribution of these projects may be great but they clearly do not fit well with the orientation towards stress effects at population and ecosystem levels. In short, most fail (or severely stress) the relevancy criteria.

SubTask 2a- Physiological indicators: reef corals and other cnidarians

A very ambitious study and along with 2b the strongest of the Task 2 project elements. Yet it is not without inherent problems. Given the complexity of the system it appears it will be very difficult to parse the effects of the competing stressors. With regard to temperature perhaps something can be learned from coring for comparison with historical temperature records. On the other hand the described spatial inhomogeneity is sobering. How can it be dealt with? What is to be made of the reported observation that some coral assemblages look fine while neighboring ones are in peril? There appears to be promise in HSP but others are doing this kind of work as well. Absent some breakthrough in the next year it is unlikely to yield anything of use within the lifetime of this overall project. The fundamental (and exceedingly daunting) problem is likely to be relating historical, current or anticipated water temperature changes to some estimate of population level effects (e.g.-mortality) through some kind of "exposure-response" function developed on the basis of the HSP work. The coral recruit work is interesting but not conceptually coupled to much else and absent a detailed experimental design impossible to evaluate. Consideration of other invertebrates (omission noted in Task 1c above) is appropriate since the less obvious invertebrates might be far more sensitive indicators of stress.

SubTask 2b- Macroalgae composition and macroalgal/coral interactions

The efficacy of elemental composition as a community level indicator of stress will be determined largely by the spatial and temporal variability for "non-stress" conditions. In other systems these seem to be sufficiently variable to reduce their utility as indicators (low signal to noise ratio) but perhaps this system is different. Analysis of 1996 samples should provide a preliminary answer and indicate whether further efforts in this direction are warranted.

Rearing *Diadema* for mesocosm experiments seems unnecessary and any discussion of field experiments with transplanted *Diadema* premature. The mesocosm experiments might provide more decisive information regarding the potential development of the bio-indicators but they will be severely limited by the small number of possible replicates (low statistical power) and exceedingly ambitious set of treatment variables. Considerable thought will have to go into establishing initial conditions since they can markedly skew results obtained. It is entirely possible that the dynamic balance that sustains grazers, corals and macroalgae at the scale of a reef requires a spatial scale that cannot be addressed using mesocosms. However data on the relative abundance of

mesocosm components on “healthy” reefs and some clever bio-energetics calculation might assist in establishing ecologically relevant conditions. This is a very difficult, ambitious and expensive experiment. The devil will be in the details.

Apparently seagrass communities were not addressed herein because covered in separate Army Corps of Engineers work but to reach seascape scale the sea grass community is critical component. Apparently mesocosm work has begun with sea grass but establishing stands similar to those seen in the Bay (not the plugs depicted) takes a long time (perhaps years) since *Thalassia* is a slow growing species. Perhaps more importantly it appears that the NOAA/COP project will henceforth (Y3 and beyond) be assuming the sea grass work as well. The Y3 (and long-range) work plan has to be clear about what exactly will be done in the mesocosms and when. It seems clear that they are insufficient to address both habitats adequately; indeed they may be marginal to conduct the requisite experiments at all. To not fail the policy relevancy criteria and contribute to risk assessment within the lifetime of the NOAA/COP program these experiments would have to be done in Years 3 and 4, no later. Clearly some hard decisions will have to be made as to their most effective use. The devil will be in the details and the experiments to be performed in the mesocosms need to be carefully evaluated by competent reviewers provided with the wealth of relevant details.

SubTask 2c- Stress challenge experiments on nearshore marine fishes

The fundamental problem herein was the fact that the experiments were not necessarily realistic. Surprisingly these investigators conducted no preliminary study was made of the actual salinity variations experienced by the resident populations. One member questioned whether these might simply move out of the way but assuming stationarity the field of salinity variation must be characterized around the canal mouths and in the adjacent mangroves in order to perform meaningful experiments. Results might well indicate that the topic is of limited potential importance to variability on a Bay wide scale; it is simply too localized. In short future experiments should be explicitly justified (or omitted) based upon appropriate biological and physical field data.

SubTask 2d - Assay of fish brain neurotransmitters

We are not supportive of these studies. Not only are they unrelated to one of the stressors enumerated as the focus of this study but in addition the time/space scaling appears inappropriate. Perhaps a reading of Haldane's Time in Biology would be instructive.

SubTask 2e - Development of biomarkers of environmentally induced stress and application to fish populations in the South Florida coastal ecosystem

As with 2d the real challenge here lies in establishing some relationship between an appropriate indicator of exposure (biomarker) and an ecological effect of sufficient importance to be identified as an endpoint in ecological risk assessment. Some of these are not really not appropriate biomarkers. We are only slightly more enthusiastic about these studies than Task 2d.

SubTask 2f - Sea turtle Papilloma disease as an indicator of anthropogenic caused environmental stress in the tropical coastal marine habitat

This might prove valuable if the disease can be traced to a fairly specific set of stressors that can be mitigated or managed. More likely it appears the disease is indicative of chemical pollution or more general environmental degradation (perhaps the integration of a number of stresses). It is not clear how the presence or absence of the disease will provide additional information. In this context extending the biomarker concept to cover papilloma disease would seem a stretch. We note that NMFS intends to augment its funding for endangered and threatened species in South Florida and suggest that is a more logical avenue of funding.

Task 3 - Characterization of Ecosystem Level Stress Effects

SubTask 3a- Phytoplankton community characterization

As noted previously the domain of this work is considerably greater (and the only interesting conclusions outside) the stated spatial domain of the NOAA/COP project. It is entirely unclear about the utility of phytoplankton (or benthic algae) alone as sufficient indications of nutrient transport and distribution. One would think at the least interpreting the data would require some concurrent nutrient concentration sampling. Moreover the relationship between described work and studies by the same principals for EPA is unclear as well as its relationship to the ongoing FKNMS monitoring by FIU (same parameters) and ongoing Biscayne Bay monitoring by FIU and DERM. Any such monitoring must be carefully designed to complement and augment ongoing efforts and where appropriate to the scientific inquiry all available data need to be brought to bear.

SubTask 3b- Coral community characteristics

This effort was so diffuse and ambitious (with so little necessary detail) as to be difficult to evaluate the feasibility of attaining the various objectives. The issue is further complicated by an unclear relationship to the considerable amount of work under way within the FKNMS under other auspices as well as the concerns about reef inhomogeneity expressed above. It would appear this is one area that is truly suffering under the weight of an untestable exceedingly complex conceptual model with so many feedbacks and interactions that any possible outcome could be generated. Here a quantitative and far more parsimonious model (s) should be considered: simpler model(s) that ignore much of what is going on in a reef to focus upon particular questions. For example some theoreticians are attempting to look at corals from the point of view of modeling zooxanthellae strategy as a function of water nutrient concentrations and energy availability. To naive outsiders it also appears that little systematic quantitative effort has gone into making comparisons between the changes seen in Florida reefs and those seen in other Caribbean and Bahamian reefs and their correlations (or lack thereof) with natural and anthropogenically related environmental changes.

The issue here appears to be distinguishing between natural and anthropogenic stressors. Reefs in Florida are near their northern limit so natural stressors must be

important while anthropogenic ones are in some cases clearly documented in other cases only anecdotal. For example definitive evidence is still lacking that anthropogenic nutrients even reach the offshore reefs (and if so where). In any case there is no question that understanding the relationship between nutrient stress (bottom up) and changing community composition/grazing (top down) is a very significant problem and one to which this investigators (in conjunction with the SubTask 3c cast) might make some useful contribution. As one panel member noted, "On a positive note, the reef work is one of the few program elements that approaches the appropriate spatial scale."

SubTask 3c-Assessment of herbivory pressure on coral reefs

As noted earlier, the work described by Bohnsack bears an unclear relationship to ongoing and planned NMFS activities and we are informed that he and his project have in fact received no support from NOAA/COP. If that is the case, listing him as a co-principal investigator is highly misleading. On the other hand the committee was favorably disposed to the work of Hays and his co-workers. It appears innovative and highly relevant. There were however some concerns. One the bites recorded on film might or might not correspond directly to the ingestion or consumption of algae and corals. Moreover in the context of risk it would seem necessary to determine how grazing controls on coral reefs are altered by the stressors of concern; it is not sufficient to simply characterize normal grazing impacts on reefs. Evaluating potential alterations in reef herbivory would also have to be related back to changes in the distribution and abundance of these reef fishes caused by anthropogenic stress. Here the relevant stress appears to be over-fishing with its effect on the basic trophodynamic structure of the fish community. If there is no gradient in space (as it appears in the NMFS data given) or relatively short time for this stress the utility of this entire effort is diminished in regard to the stated overall project objectives however otherwise worthwhile it may be. We also note that, if anything, the Hays data indicate more rather than less grazing on algae which seems counter-intuitive given the reports of increased algal growth (unless of course this is due largely to nutrient increases). The way this issue was discussed by the SubTask 3b and 3c investigators was not indicative of effective collaboration and overall project integration.

SubTask 3d- Documentation of the type and prevalence of abnormalities in fish in the South Florida Coastal Ecosystem

As with some of the Task 2 activities (and we found it exceedingly difficult to categorize this SubTask) the key challenge lies in relating observed incidences of scale disorientation, fin ray abnormalities etc. to ecological processes that might have consequences on the production dynamics of the afflicted fishes. Moreover the abnormalities have to be related back to the stressors of interest. The only example given was a pollutant contaminant not in the list of stressors and in an area north of the entire project domain. The data we were shown suggests this effort is problematic since black spot syndrome was reported prior to and in areas remote from human influence. Frankly we have little if any confidence this work will yield anything of utility to the overall project.

SubTask 3e-Stress controls on normal and dwarf mangroves

Although the actual work discussed re. dwarf mangroves would appear to be of limited relevance (and to have been largely completed with prior funding), at least this project provided us a list of stressors which one would think would have been a preliminary step for each habitat. That said, if this effort focused upon the mangrove habitat in relation to the resident juvenile fish community it would be far more relevant and seemingly potentially coupled to the fish work in Biscayne Bay. One would think that over the range of conditions in Florida given sea level, temperature and salinity variation virtually every possible experiment has been performed many in near pristine environments. As a result more than one panelist questioned whether adequate use been made of these natural experiments in developing an integrated conceptual framework for evaluating ecological risks in relation to the stressors of concern before proposing a series of complex and difficult to interpret experiments.

SubTask 3f-Composition, structure and dynamics of fish communities

Problems here were previewed in the SubTask 1c discussion. This effort certainly speaks to NMFS management concerns albeit clearly so within the context of the CMEA project structure and logic of which it is virtually independent. Moreover how well the effort succeeds is not only limited by the data base (see SubTask 1c discussion) but also the uncertainties in model formulation and parameterization. We simply weren't given enough information to begin to evaluate these last concerns. This SubTask also raises substantial budgetary/logistic concerns not only because of what NMFS will be funding with its FY97 augmentation but also because the major management of reserve areas in the sanctuary won't be in place for nearly a year from now and any effects would emerge near the end of the overall project lifetime. The most significant reserve, the Dry Tortugas, may or may not even come into being during the project lifetime. On the other hand if the NOAA/COP role is to fund data analysis eventually useful to management of the FKNMS (regardless of its connection to the rest of the CMEA project) this work has the potential to yield important results in the not too distant future.

Task 4: Development and Application of Models for Ecological Risk Assessment and Ecosystem Management

According to the information provided us, Task 4 was the least developed although critical to the overall project and arguably its only unique aspect. The presentations given to the panel suggested, however, that some of these SubTasks consumed substantial principal investigator time and CMEA resources.

SubTask 4a- Developing predictive ecosystem and SEASCAPE models.

These modeling activities are ambitious to say the least. We noted yet again a substantial spatial disconnection this time between the scope of the Biscayne Bay model actually described and the regions emphasized in many of the presentations as well as the inherent inapplicability of this model to the subset of South Florida denoted as the project

domain. Moreover while the GIS capabilities (and attaining them) appeared likely to be very successful we were unconvinced as to the overall quantitative prospects of the modeling efforts. It might prove beneficial to select one well defined issue from the overall set and work it through the entire modeling process. This might afford the chance to demonstrate the feasibility of the SEASCAPE model for a specific issue while a larger scale system model is being developed. Surprisingly a number of the parameters one would think necessary for a WQ model of the type discussed for Biscayne Bay (yielding seagrass cover, phytoplankton chlorophyll etc.) were not even being measured! We noted above the dearth of essential field data. Considerable skepticism was elicited by the claims made of the ROXANNE acoustic data presented and we further note this activity is yet another ill-defined leveraging or collaboration. If these data are central to obtaining project objectives (that is information on bottom type at that scale and resolution) the technical adequacy of the data needs to be better documented as well as the details of the project inter-relationships and commitments.

Most important it was not clear to the model experts on our committee how we were ever going to get from conceptual models to simulation models to predictive models. Some habitat sub-models may be ready to move to simulation others perhaps to prediction. To responsibly assess the feasibility of this effort at the very least a careful timeline and flowchart of progression should be prepared. How else are we to have any confidence the effort will ever reach the predictive mode and absent that it has surely failed the policy relevancy criteria, perhaps more so than the simple data collection efforts which at least represent new knowledge about the system. We are concerned that alot more has been promised here than could possibly be delivered regardless of the level of investment. For a model to eventually be useful in any way to managers its sensitivity to various parameters and estimation of statistical errors must be explicit. We are unclear what if anything NOAA/COP much less the management community will learn from the modeling activities described to us.

SubTask 4b-Predictive statistical modeling methodology for reef fishes

This proposed decision support system for reef fish and macroinvertebrate communities (??who is providing these data??) might serve as the example of a SEASCAPE model subset that can be developed more fully in the near term to demonstrate the feasibility for the overall modeling-management tool for integrated risk assessment and risk management. Unless we missed something the purported trophodynamic modeling has not only not begun but none of the relevant data to validate or parameterize it is even being collected! We don't mean that the feeding habits of the fish are not being considered rather that the food web in which they are a part is not being assessed (nor in any rigorous sense simulated). Given the information provided us it is impossible to say what level of resources should be provided this effort.

SubTask 4c- Developing economic valuation models to evaluate alternative mitigation strategies

It seemed to us the likely application of this activity would be the focus upon the fisheries aspects (link with Task 4b) as the most promising avenue to yield a useful result

during the overall project lifetime since much of the requisite data base is relatively mature and independent of the NOAA/COP project. The relevance and utility of focus groups discussions and surveys was also lost on some of the committee as indeed was any understanding of what had been done to date with the substantial funding already awarded? Has anything at all been done? Additional funding should be contingent on proof of real progress in this area. At present it is entirely lacking.

SubTask 4d- Integration and Assessment

We are informed this effort has not been initiated as yet. While unlikely to be expensive this effort will eventually constitute the focal point of the overall project since here is where CMEA must deliver on its rhetoric, this is where the project can be distinguished from yet another ecosystem characterization study. In the reorganization we propose integration and assessment would provide a central organizing principle. A cautionary note: there will have to be some decision made as concerning the "state of the system" that defines a time(0) or a reference point from which to assess and evaluate risks posed by subsequent human activities. It's not necessary that all "endpoints" are assessed from the same time but some reference does need to be established for each endpoint and doing so is a non-trivial undertaking. Moreover, devoting substantial resources to this effort at the expense of other project areas would have to be justified by rigorous peer review of the detailed work plan developed to accomplish this SubTask. At present this is unavailable.

CONCLUDING REMARKS

Hopefully the report of this Technical Advisory Committee will assist the NOAA/COP and the CMEA in reorganizing, in a sense re-inventing, this ambitious project. Based on what we have learned, a fundamental reinvention is sorely needed. We stress that our procedural recommendations are as important as our substantive ones. These include more complete documentation, explicit budgetary information and much better use of the peer review process. We hope that by chartering a standing advisory committee to annually assess progress, NOAA/COP and the CMEA are expressing a genuine commitment to make whatever changes are necessary to formulate a scientifically defensible policy-relevant project, that present and future funding is made contingent upon such change. We will be examining with great interest the Year Three work plan and assume that NOAA/COP will take this TAC report under advisement in evaluating the plans eventually submitted to them. Our criticism is harsh but offered with a sincere conviction that the general objective of this project is laudable and that its talented investigators can make substantial contributions to our understanding of the South Florida coastal ocean ecosystem and to communicating this understanding to the management/policy community. We look forward to continuing to participate in the process of assisting them to do so.

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¹ Unable to attend Program Review and contribute to this report



MEMORANDUM

Date: 27 March 1997
To: Distribution
From: Mark Harwell *Mad*
Subject: Technical Advisory Committee Report

Enclosed is a copy of the final report from the NOAA-COP Technical Advisory Committee (TAC) from its review of the progress-to-date and preliminary Year-3 workplan from the project on Human-Environment Linkages in the South Florida Coastal Ecosystem.

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