Proposal Reviews

#140: Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay

San Francisco State University, Romberg Tiburon Center

Initial Selection Panel Review

Research and Restoration Technical Panel Review

Bay Regional Review

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Environmental Compliance

Budget

Initial Selection Panel Review:

Proposal Number: 140

Applicant Organization: San Francisco State University, Romberg Tiburon Center

Proposal Title: Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay

Please provide an overall evaluation rating.

Explanation of Recommendation Categories: Fund

- **As Is** (a proposal recommended for funding as proposed)
- **In Part** (a proposal for which partial funding is recommended for selected project phases or components)
- **With Conditions** (a proposal for which funds are recommended if the applicant contractually agrees to meet the specified conditions)

Consider as Directed Action in Annual Workplan (a proposal addressing a high priority action that requires some revision followed by additional review prior to being recommended for funding)
Not Recommended (a proposal not currently recommended for funding-after revision may be considered in the future)

Note on "Amount":

For proposals recommended as Fund As Is, Fund In Part or Fund With Conditions, the dollar amount is the amount recommended by the Selection Panel.

For proposals recommended as Consider as Directed Action in Annual Workplan, the dollar amount is the amount requested by the applicant(s).

Fund	
As Is	-
In Part	-
With Conditions	-
Consider as Directed Action	-
Not Recommended	X

Amount: **$0**

Conditions, if any, of approval (if there are no conditions, please put "None"):

none

Provide a brief explanation of your rating:

The investigators are highly qualified to conduct this research and the reviewers generally considered an effort such as proposed worthwhile. The investigators justify the need for taking a refine look at primary production in the Suisun marsh, but a number of concerns were raised by external reviewers that should be addressed before the project is implemented. Location of the monitoring stations, frequency of exchange between instruments, frequency of accompanying field measurements and the complicated flow characteristics of the marsh are problems for the research as proposed. This effort also needs to be well integrated with ongoing monitoring and research on phytoplankton production and dynamics. The recommendation by the panel is not to fund at this time.

Research and Restoration Technical Panel Review:

CALFED Bay-Delta 2002 ERP PSP
Research and Restoration Technical Panel Review Form

Proposal Number: 140

Applicant Organization: San Francisco State University, Romberg Tiburon Center

Proposal Title: Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay

Review:

Please provide an overall evaluation summary rating:

<u>Superior:</u> **outstanding in all respects;**
<u>Above Average:</u> **Quality proposal, medium or high regional value, and no significant administrative concerns;**
<u>Adequate:</u> **No serious deficiencies, no significant regional impediments, and no significant administrative concerns;**
<u>Not Recommended:</u> **Serious deficiencies, significant regional impediments or significant administrative concerns.**

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
-Superior	**Phytoplankton dynamics are certainly central to the basis and evaluation of the CALFED Bay Delta Ecosystem Restoration Program. In addition, detection of subtle changes in ecosystem function in response to restoration action is a critical component of adaptive management. The authors place their current work into the context of a rich-history of estuarine-wide research and potentially provide the CALFED restoration program with noteworthy advances in the understanding of the Bay's phytoplankton dynamics and CALFED's detection of change in ecosystem function. However, the current proposal lacks essential detail to assure success. The panel recommends that CALFED fund a component of this proposal to enable the authors to gain the additional information that is required for success. CALFED should place high priority in the general objective and approach outlined in this proposal; thus, the panel recommends that CALFED fund a one-year pilot/exploratory study to allow the authors to address the serious concerns raised above.**
XAbove average	
-Adequate	
-Not recommended	

1. <u>**Goals and Justification.**</u> Does the proposal present a clear statement of goals, objectives and hypotheses? Does the proposal present a clear justification and conceptual model for the project?

   The overarching goal of this proposal is to evaluate phytoplankton biomass and NPP in the northern SF-Bay (and related environmental factors) on time scales that allow highly accurate detection of daily, weekly, monthly, and annual change in phytoplankton dynamics and potential connections to CALFED restoration activities. A series of six objectives are presented in support of this noble broad goal: 1) obtain continuous fluorescence

measurements in Suisun Bay using a moored fluorometer, 2) compare "continuous" fluorescence with direct measurements at various time scales, 3) provide a baseline for phytoplankton dynamics prior to the start of Delta restoration activities, 4) measure NPP directly to compare with a pre-existing Delta model, 5) evaluate the role of diatoms within the phytoplankton pool, and 6) examine phytoplankton dynamics in terms of important physical and chemical environmental variables. The overarching goal and associated objectives are very timely and important to the CALFED Bay Delta Restoration Program.

The work is certainly justified relative to existing knowledge. The northern SF-Bay and much of the Delta have low rates of NPP and low concentrations of phytoplankton biomass and the planktonic food web is thought to be tightly coupled to this low phytoplankton production. In addition, phytoplankton biomass has declined over the past two decades (partially in response to the invasion of Potamocorbula). Although multiple factors are likely responsible for the well documented declines in zooplankton and many native fishes, declines in phytoplankton biomass almost certainly have contributed to these declines. Regardless of the strength of the trophic connections, phytoplankton production and loss terms are critical ecosystem processes that warrant acute attention from the CALFED restoration program. Real-time monitoring of ecosystem processes provide Delta managers with powerful data sets that can be used to detect and infer ecosystem change in function in response to restoration actions.

CAPABILITIES: Excellent track records. Highly qualified researchers with the appropriate infrastructure.

2. **Likelihood of Success (Approach, Feasibility, Capabilities and Performance Measures).** Is the project likely to succeed based on the approach, feasibility and project team capabilities? Are the proposed performance measures adequate for measuring the project's success?

   In general, the approach is well designed and results are very likely to add to our base of knowledge. The proposed research builds upon the research of others throughout the Bay region and the authors' own research. Novel and useful information can almost certainly be generated. However, reviewers had major concerns about the successful implementation of a continuous monitoring program and associated discrete Measurements (and experiments).

   The following concerns were raised:

   1) INSTRUMENT DEPLOYMENT Suisun Bay hydrodynamics are EXTREMELY complicated and selection of a central mooring location (or several) requires a great deal of thought regarding the hydrology of the system.

   2) INSTRUMENT MAINTAINANCE There is great interest in using equipment that allows continuous measurements of biological, chemical, and physical parameters. There have been great advances in instrumentation to measure fluorescence at remote stations, BUT biofouling, signal drift, and signal noise present major problems in the analysis of the data generated by these instruments. In our opinion, monthly instrument swaps are not adequate.

   3) DATA ANALYSIS Long-time series data is notoriously difficult to analyze. Detection of change in response to restoration action will require sophisticated data analysis tools. More detail is required here.

   4) INTEGRATION Comparison with the primary production model used in Jassby et al. 2002 or earlier Cole and Cloern publications requires model specific chemical and physical parameters to be measured and a more thoughtful experimental design.

5) EXPERIMENTAL DESIGN Diatoms generally make up a large component of the northern SF- Bay's phytoplankton biomass and there is little previous evidence for sustained N, P, or Si limitation. Light limitation is well documented throughout the entire region, and Suisun Bay has EXTREMELY complicated hydrodynamics that result in amazing spatial and temporal variability in suspended sediments. Further, advective processes associated with adjacent wetlands, mudflats, and riverine inputs may account for much of the short-term variability within the Suisun Bay region.

3. **Outcomes and Products.** Will the project advance the state of scientific knowledge in general and/or make an important contribution to the state of knowledge of the Bay-Delta Watershed? For restoration proposals, is the project likely to contribute to ecosystem restoration or species recoveries in a significant way? Will the project produce products useful to decision-makers and scientists?

   In general, the approach is feasible, but requires significant refinement. Success is likely if the five points above are fully considered.

   YES-YES-YES Assuming that the above comments are fully and carefully considered.

4. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

   The costs seem reasonable, especially compared to other CALFED projects, and the potential contribution to the CALFED restoration program could be substantial.

5. **Regional Review.** How did the regional panel(s) rank the proposal (High, Medium, Low)? Did the regional panel(s) identify significant benefits (regional priorities, linkages with other activities, local involvement) or impediments (local constraints, conflicts with other activities, lack of local involvement) to this proposal? What were they?

   MED ---- There were concerns about direct CALFED connections.

6. **Administrative Review.** Were there significant concerns about the proposal with regard to the prior performance, environmental compliance and budget administrative reviews? What were they?

   No

Miscellaneous comments:

Steve Monismith's group at Stanford and Jon Burau's group at the USGS in Sacramento have made great advancements in our understanding of the hydrodynamics of Suisun Bay. These groups may be best positioned to help with instrument placement. Lisa Lucas at the USGS in Menlo Park and Ted Sommer at USFW have incorporated similar fluorescence instrumentation into field projects in other areas of the Delta and may be able to provide valuable supportive information.

Bay Regional Review:

Proposal Number: 140

Applicant Organization: San Francisco State University, Romberg Tiburon Center

Proposal Title: Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay

Overall Ranking: **-**Low   **X**Medium   **-**High

Provide a brief summary explanation of the committee's ranking:

Though this is an interesting study, and could offer an increased capability to track chlorophyll levels in one area of the bay, the panel did not think it rated high for the Bay region. It was felt that the first priority should be to get a better handle on historic sampling and the second to evaluate both temporal and spatial variability to ascertain what pattern of sampling should be enhanced. Also it was unclear if the effects of restoring Suisun Bay areas would actually impact the site proposed in this study - hence the value of this particular site is in question.

1. Is the project feasible based on local constraints?

   XYes **-**No

   How?

   Yes, the equipment and methods are sound. The plan is to evaluate the chlorophyll concentrations in North SF Bay on time scales that provide reliable estimates of phytoplankton biomass to determine baseline before CALFED restoration begins. The proposal addresses the temporal variability problem, and builds on other studies which address spatial variability; however, both forms of variability should be addressed at once if we are to have a reliable baseline. but doesnt really get at the problem in estimating chlorophyll due to spatial variability except by comparing this station with Central Bay.

2. Does the project pursue the restoration priorities applicable to the region as outlined in the PSP?

   XYes **-**No

   How?

   Yes; however the effect on restoration is indirect. It fulfills the following priorities: BR-6. Protect at-risk species in the Bay using water management and regulatory approaches, by providing a better understanding of a) primary productivity within Suisun Bay, North Bay, and Central Bay and linkages among internal and external inputs and b) the linkages between North and South Bay that might affect restoration or productivity in either, and c) the poorly known aspects of the food webs in these regions. In addition the study will demonstrate effective techniques for monitoring the phytoplankton base of the food web. BR-8. The proposed research will provide chlorophyll data, to help examine how trends in chlorophyll, zooplankton and pollutants impact different fish species locally (in Suisun Marsh) and across the Bay region landscape (interconnections between Bay segments).

BR-4. information on productivity might assist in the evaluation of wetlands restoration efforts on a local and regional scale.

3. Is the project adequately linked with other restoration activities in the region, such as ongoing implementation projects and regional planning efforts?

   XYes **-**No

   How?

   The proposal does not explicitly identify the programs aimed at restoration but does indicate direct connections to ongoing monitoring programs at DWR (Rio Vista, Yolo bypass). By collecting data on a different time scale from existing monitoring programs, the proposed research will also enhance interpretations of existing aquatic system monitoring information from the Bay.

4. Does the project adequately involve local people and institutions?

   -Yes **X**No

   How?

   Yes, other institutions are mentioned (DWR), but the degree of their coordination is unclear. Also, there are likely other studies collecting more spotty information on chlorophyll, this study does not mentioned whether or how these studies would inform this study.

Other Comments:

This is an important study however, it should be better tied to the spatial data gathering on chlorophyll to ensure that overall calculations of chlorophyll a and production are based on consistent methodologies. Also, a more comprehensive analysis of monitoring needs should be completed before new stations are added.

External Scientific: #1

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Research and Restoration External Scientific Review Form

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Proposal Number: **140**

Applicant Organization: **San Francisco State University, Romberg Tiburon Center**

Proposal Title: **Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay**

Conflict of Interest Statements:
I have no financial interest in this proposal.
XCorrect
-Incorrect

In the blank below please explain any connection to proposal, to applicant, co-applicant or subcontractor or to submitting institution (write "none" if no connection):

None

Review:

Please provide an overall evaluation summary rating:

Excellent: outstanding in all respects;
Good: quality but some deficiencies;
Poor: serious deficiencies.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
-Excellent **X**Good **-**Poor	**Pros - This is a well thought out proposal on a subject which the authors have had much previous experience. It is pertinent to CALFED goals and should result in usable products. Cons Lack of coordination with agencies working in the area. Weak description of modeling efforts and data analysis.**

1. **Goals.** Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the concept timely and important?

   The mentioned goals are to measure CHLA concentrations on a time scale that is more relevant to response time of the ecosystem, to look for connections between North and Central Bays, and to predict upstream conditions from downstream data.

2. **Justification.** Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

The authors provide an impressive presentation of data collected in the region. They show data aliasing of the monthly CHLA time series. Aliasing is the generic term for statistical artifacts resulting from sampling a time series too infrequently (surprisingly they never use this term in the proposal). They show the progressive improvement of water quality with distance downstream as a function of seawater mixing. All of this is couched in providing information feedback from ecosystem restoration efforts.

To their credit, they mention that DWR already has moored arrays for CHLA in two other areas but that these placements are not characteristic of Suisun Bay as a whole. The argument that there is no comparison with extractive CHLA measurements is moot because this could be done at those sites if proposed. It sounds l;ike they do noy know if the DWR sites are active or not. I believe they should have spent some effort in developing a collaboration with the agency to relocate and help operate the site. It is my experience that agencies most always welcome collaboration if the scientists offer to to analyze, report, and publish the results.

3. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology or approaches? Will the information ultimately be useful to decision-makers?

   The scientific approach the PIs would use to achieve the goals is a little unclear to this reader. I see no reason to increase ship cruises from monthly to weekly when they have an operating moored array in place. There is a disconnect between the CHLA based productivity model and the nutrient information. Do the authors expect to predict CHLA from ammonium drawdown and then forecast productivity from that? It seems possible but the propagation of error from predictions based on predictions can really add up. There is little justification for the need to discriminate between sizes of phytoplankton production. Phytoplankton counting by flow cytometry sounds like a good idea but the justification for doing so, that there is a lot of detritus present in the samples, may actually preclude this method as a alternative. The subject of using a filter fluorometer such as the Wet Labs to provide in situ CHLA values is questionable. Most programs calibrate the fluorometer using pure cultures of algae. Extrapolating fluorescence of field samples to CHLA concentration is problematic because of optical interferences from seston and colored dissolved organic matter. It is possible to develop a corrective algorithm if one measures these interfering agents but it should be explicitly included in the QA plan. In addition, the authors should be persuaded to switch to using the Welschmeyer lamp and filter combination for extractive CHLA analysis as this method obviates the need for phaeophytin correction and eliminates acidification artifacts.

4. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

   The project is entirely feasible. They have the people, facilities, and equipment to conduct the proposed research.

5. **Project-Specific Performance Measures.** Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Is there enough detail as to how the performance measures will be quantified? For restoration projects, are monitoring plans explicit and detailed enough to determine if performance measures will be adequately assessed?

   Performance measures are outlined in the proposal, however, the modeling component needs to be better described.

6. **Products.** Are products of value likely from the project? Specifically for restoration projects, are products of value also likely from the monitoring component? Are interpretative outcomes likely from the project?

   Products are outlined and germane to the proposed research, albeit a bit generic. They should be very useful to other scientists and managers. I am glad to see that they plan to upload their data to a web page to make it more accessible.

7. **Capabilities.** What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

   The PIs are all highly qualified and have put together an impressive field and research team.

8. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

   The budget is reasonable. The only questioin I have is the purchase of 2 complete in situ data arrays. Most projects operate with one which undergoes maintenance on a regular basis. Their monthly cruises to switch out arrays might instead be used to download the data and service the unit rather than replacing it each time.

Miscellaneous comments:

None

External Scientific: #2

Research and Restoration External Scientific Review Form

Proposal Number: **140**

Applicant Organization: **San Francisco State University, Romberg Tiburon Center**

Proposal Title: **Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay**

Conflict of Interest Statements:
I have no financial interest in this proposal.
XCorrect
-Incorrect

In the blank below please explain any connection to proposal, to applicant, co-applicant or subcontractor or to submitting institution (write "none" if no connection):

none

Review:

Please provide an overall evaluation summary rating:

Excellent: outstanding in all respects;
Good: quality but some deficiencies;
Poor: serious deficiencies.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
XExcellent **-**Good **-**Poor	**The researchers have the experience to carry out the proposed work. The proposed work will be critical to evaluating system changes in San Francisco Bay.**

1. **Goals.** Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the concept timely and important?

   The major goal is to use intensive automated and shipboard sampling to detect peaks in productivity in N. San Francisco Bay that would be missed by less frequent sampling. This goal is stated clearly. There are 4 hypotheses. Hypotheses 1 and 2 relate clearly to the goal in that they state that more intensive sampling will detect high chlrorophyll events that are missed at less frequent time intervals. In order to determine if restoration activities are having an impact good quality data are needed to document present and future conditions. In highly dynamic systems automated sampling that can provide close time interval sampling is often key to collection of this data. Automated instrumentation is becoming increasingly important in documentation of system changes in a number of coastal systems and the addition of this sampling to N. San Francisco Bay is important and timely.

2. __Justification.__ Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

   Existing data show that chlorophyll concentrations can be highly dynamic depending on a number of conditions including tidal and freshwater flow conditions. Because of this dynamic nature automative close sampling interval data is needed to assess productivity of the system.

3. __Approach.__ Is the approach well designed and appropriate for meeting the objectives of the project? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology or approaches? Will the information ultimately be useful to decision-makers?

   Frequent sampling for chlorophyll fuorescence is well justified as is weekly sampling to determine 14C productivity. Additionally, however, automatic recording insturments to determine dissolved oxygen and turbidity changes should be used. Nevertheless, the data proposed to be collected will be very useful in detecting enviromental change in N. San Francisco Bay and this documentation should be higly useful to decision-makers.

4. __Feasibility.__ Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

   The approach seems feasible and as I understand a similar automated system is already being used successfully in central San Francisco Bay. Further, automated sampling is being widely used in coastal areas and has frequently shown great success. The proposed work is consistant with the objectives of docuenting phytoplankton biomass, productivity and changes in both these parameters.

5. __Project-Specific Performance Measures.__ Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Is there enough detail as to how the performance measures will be quantified? For restoration projects, are monitoring plans explicit and detailed enough to determine if performance measures will be adequately assessed?

   The objectives of the proposed work is to monitor changes in the system and not induce them. The monitoring proposed is extensive and should detect even relatively small changes in phytoplankton biomass should they occur.

6. __Products.__ Are products of value likely from the project? Specifically for restoration projects, are products of value also likely from the monitoring component? Are interpretative outcomes likely from the project?

   This project will have give valuable monitoring data. Further, if this data is linked to models of food web transfers of material and system water quality the value of the output data will be greatly improved.

7. __Capabilities.__ What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

   Wilkerson, Dugdale, and Hogue have extensive experience measuring phytoplankton biomass and productivity. They have previously worked in this system and have the know how and ship time to carry out the project.

8. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

The total 3 year budget is just short of 0.5 million dollars. This budget is needed to acquire monitoring equipment (ca. 40 thousand dollars) and provide the labor to service this equipment and carry out the shipboard measurements at every other week intervals.

Miscellaneous comments:

Too frequently managemers are faced with a lack of adequate data to evaluate system condition and the impacts of management on system condition. This work will provide valuable information on primary productivity that will be absolutely required to evaluate fisheries changes and water quality changes. It is too bad that the researchers are not extending there monitoring to intensive measures of dissolved oxygen and turbitiy. These measures could be added for little additional cost.

External Scientific: #3

Research and Restoration External Scientific Review Form

Proposal Number: **140**

Applicant Organization: **San Francisco State University, Romberg Tiburon Center**

Proposal Title: **Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay**

Conflict of Interest Statements:
I have no financial interest in this proposal.
XCorrect
-Incorrect

In the blank below please explain any connection to proposal, to applicant, co-applicant or subcontractor or to submitting institution (write "none" if no connection):

None

Review:

Please provide an overall evaluation summary rating:

Excellent: outstanding in all respects;
Good: quality but some deficiencies;
Poor: serious deficiencies.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
-Excellent	**The PI's provide clear evidence that primary production is under-estimated on the monthly or weekly time-scales that it is typically measured, and propose to collect data that will help to identify the necessary sampling frequency. This appears to be a useful project that meets some key CALFED strategic goals. I only really have some minor concerns on the analysis techniques to identify the necessary sampling frequency, and the relative importance of spatial and temporal variability. I recommend the proposal is funded.**
XGood	
-Poor	

1. **Goals.** Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the concept timely and important?

   The goals of this project are clearly stated. The PI's provide clear evidence that primary production is under-estimated on the monthly or weekly time-scales that it is typically measured, and propose to collect data that will help to identify the necessary sampling frequency. The proposed project appears to meet the CALFED strategic goals.

2. __Justification.__ Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

   The PIs provide clear justification for the need to determine the necessary temporal sampling frequency in measuring primary production. Full-scale implementation of the project appears to be justified.

3. __Approach.__ Is the approach well designed and appropriate for meeting the objectives of the project? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology or approaches? Will the information ultimately be useful to decision-makers?

   The PIs plan to examine the hypothesis that continuous measurements of chlorophyll will show higher mean values than data obtained just using monthly shipboard monitoring, and then examine the hypothesis that weekly shipboard sampling will detect the peaks that are missed with monthly sampling. This approach does not identify the appropriate temporal sampling frequency. Based on the initial data the PI's present, it almost seems to be a foregone conclusion that weekly data will provide a better estimate of the peaks and mean values than monthly data. What I would like to see is a series of sensitivity experiments, where the investigators sub-sample from the continuous record, and determine the decrease in accuracy with successively longer sampling intervals. For example, what are the peaks and mean values when samples are taken every 2 days, every 3 days, etc., and how to these statistics compare with the daily record?

   The approach appears to be fairly well designed to determine the temporal sampling frequency necessary for measuring primary production. Given the high spatial variability in chlorophyll, is it appropriate to examine the temporal sampling problem in isolation? Are the improvements realized by more frequent sampling masked by high spatial variability? This is probably beyond the scope of this proposal, but should probably be considered with reference to previous work.

4. __Feasibility.__ Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

   There is a high likelihood of achieving the objectives of the proposal as they are currently written.

5. __Project-Specific Performance Measures.__ Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Is there enough detail as to how the performance measures will be quantified? For restoration projects, are monitoring plans explicit and detailed enough to determine if performance measures will be adequately assessed?

   The performance measures are restricted to successful completion of each research task. There is no method of quantifying if the research tasks are performed well (e.g., publication of results in high-quality journals, adoption of a revised sampling strategy by the organizations responsible for monitoring of chlorophyll).

6. __Products.__ Are products of value likely from the project? Specifically for restoration projects, are products of value also likely from the monitoring component? Are interpretative outcomes likely from the project?

Improved monitoring of primary production will be useful for decision-makers involved in CALFED restoration efforts.

7. **Capabilities.** What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

   The PIs appear to capable of successfully completing the proposed project.

8. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

   The budget appears to be reasonable.

Miscellaneous comments:

None.

External Scientific: #4

Proposal Number: **140**

Applicant Organization: **San Francisco State University, Romberg Tiburon Center**

Proposal Title: **Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay**

Conflict of Interest Statements:
I have no financial interest in this proposal.
XCorrect
-Incorrect

In the blank below please explain any connection to proposal, to applicant, co-applicant or subcontractor or to submitting institution (write "none" if no connection):

None.

Review:

Please provide an overall evaluation summary rating:

Excellent: outstanding in all respects;
Good: quality but some deficiencies;
Poor: serious deficiencies.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
-Excellent **X**Good **-**Poor	**I believe the primary goal of this proposal (evaluation of appropriate chlorophyll sampling timescales) and the general method for accomplishing that (deployment of a moored SCUFA for taking high-frequency fluorescence measurements) are clear and sound. LOTS of surprising and valuable information on ecosystem function and how to correctly monitor it can come from high-frequency timeseries. There were some detailed aspects of the methodology for accomplishing that goal for which I offered suggested modifications (e.g. subsampling high-frequency timeseries instead of comparing with shipboard measurements, location of SCUFA). Further, I believe the comparison of Suisun Bay timeseries with Central Bay timeseries is a good idea for understanding Bay-level chlorophyll dynamics. I agree with the authors that establishing a reliable baseline knowledge of current Bay productivity, as well as appropriate methods for estimating it, will be crucial to evaluating restoration impacts on Bay productivity. There were other aspects of this proposal, however, which were not as clearly stated, explained, or motivated (e.g. the large cell/diatom and esp. "ammonium limitation" aspects). Therefore, although the major crux of the proposal seemed reasonable and justifiable, I could not provide an overall rating of excellent.**

1. **Goals.** Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the concept timely and important?

   Some goals/objectives/hypotheses were crystal clear and made perfect sense to this reviewer, while others were/did not. Therefore, I?ll paraphrase them (as I see them) and take them separately:

   1) EVALUATE APPROPRIATE TIMESCALES FOR MEASURING CHLOROPHYLL CONCENTRATIONS AND PRIMARY PRODUCTIVITY IN NORTH SAN FRANCISCO BAY (NSFB). This seems to be the primary goal of this proposal and is clearly stated, timely, and important. The applicants clearly explain that chlorophyll (proxy for phytoplankton biomass) may be highly variable over timescales much shorter than typical (monthly or at most weekly shipboard) sampling timescales in the Bay, and that these methods therefore may ?miss? chlorophyll peaks and underestimate maximum or annual mean phytoplankton biomass. Because estimates of primary productivity are based on measurements of phytoplankton biomass, primary productivity may also be underestimated by temporally sparse sampling.

   2) STUDY THE ROLE OF DIATOMS/LARGE CELLS IN NSFB PRODUCTIVITY AND FOODWEBS. This goal was a bit less clear to this reviewer. The applicants clearly show domination of major NSFB blooms in 2000-2001 by large cells (Fig. 2). It is also stated that these larger cells are ?typically diatoms.? Through the proposal, the terms ?diatoms? and ?large cells? seem to be used almost interchangeably, and it is not clear to me which category the applicants are really interested in?the diatoms or the larger cells. This distinction is important because diatoms can be ?big? (> 10um) or ?small? (<10um) but generally are considered high quality food regardless of their size because of their chemical composition. Furthermore, ?big? cells are not necessarily diatoms or high in nutritional quality. Whether the scientists are interested in large cells versus diatoms may bear on the appropriateness of the proposed approach (discussed below).

   3) CONNECTIONS BETWEEN CENTRAL AND SUISUN BAYS. The applicants state that they would like use high frequency chlorophyll measurements in Central and Suisun Bays to look for synchrony/interconnections over space and time. I think that is a good idea: comparison of timeseries (e.g. magnitudes, ranges, phasing, dominant timescales of variability) from different environments may help elucidate mechanisms driving phytoplankton dynamics in the Bay overall (e.g. local processes versus system-wide processes).

   4) EXPLORE CONNECTIONS BETWEEN PHYTOPLANKTON BLOOMS AND AMMONIUM. This aspect of the proposal was confusing to me. The applicants say they wish to test the hypothesis that productivity peaks are preceded by ammonium decreases (Exec. Summ. and p. 7). From the timeseries in Figs. 2 and 5 this seems possible, and the exploration of ammonium depletion and shift to nitrate uptake (p. 5) is valuable. However, I did not think that the motive for pursuing this was well-explained. More troubling/confusing to this reviewer were statements suggesting that ammonium could have a negative impact on phytoplankton growth (top and bottom p. 6). For example: ?It is likely that the diatom growth in North Bay may also be limited by ammonium as well as light. High ambient levels of ammonium inhibit growth by inhibiting nitrate uptake.? I am not an expert in nutrient chemistry or in phytoplankton uptake of nutrients, but this statement (esp. the second sentence) makes no sense to me. The authors themselves say that ?phytoplankton ? apparently assimilate ammonium for growth first until it runs out and then continue to grow

using nitrate.? (top p. 5). So how can ammonium, a nutrient which the phytoplankton use to grow, limit phytoplankton growth?? I have spoken with colleagues knowledgeable about nutrients and nutrient uptake by phytoplankton, and none of them know of a mechanism by which high ammonium may inhibit phytoplankton growth. In fact, from what I know, ammonium is much more efficient than nitrate for the phytoplankton to use. Therefore, I am puzzled. If the authors know of a biochemical process by which phytoplankton growth may be inhibited by ammonium (i.e. a process they would like to explore in this study), they did not let on what it is. I suggest the panel consult with a scientist well-versed in nutrient uptake to fully evaluate this part of the proposal. The different question of whether phytoplankton can become nutrient limited (e.g. due to drawdown of nutrients through the course of a bloom) is in itself interesting and valuable information; however, it is not clear that that?s what the authors are concerned with here.

2. **Justification.** Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

1) SAMPLING TIMESCALES: The timeseries of phytoplankton biomass shown in Figure 2 provides great justification for the work proposed to investigate appropriate sampling timescales for phytoplankton biomass. I believe this is a real issue because I have witnessed several instances in the Bay-Delta system of high frequency variability (timescales of hours) in water quality (including chlorophyll a) that would not be resolved by low frequency sampling. In such cases, low frequency sampling may not be at all representative of the mean state or concentration range at a particular location. Furthermore, even if low frequency sampling captured (by luck) the range of actual variability in concentrations, determination of the actual timescales and processes of variability would be difficult if not impossible. This is a very dynamic and complex system, and I believe resolving the small timescales is a great direction in which to move, both for quantifying and explaining variability.

The applicants suggest that once the appropriate sampling frequency is determined, then evaluation of changes in primary production in NSFB resulting from CALFED restoration efforts will be possible. I am interested in knowing which specific restoration efforts the applicants are referring to, but did not see such specifics in the proposal. Appropriate geographical location as well as frequency of sampling could depend on the specific restoration actions of concern (e.g. whether the restoration will take place in the Bay or Delta). Perhaps the applicants are interested in knowing the local (Suisun Bay, Central Bay) impacts of any and all restoration actions. Although these specifics are not provided, I still believe this part of the proposal generally makes sense and is justified.

2) ROLE OF DIATOMS/LARGE CELLS: The applicants state (p. 3) that the larger cells/diatoms are typically algae with high growth rates and are good food items for higher trophic levels. Again, it was unclear if the applicants were talking about large cells or diatoms. If we assume the applicants are actually concerned with diatoms, then the investigation of phytoplankton community composition (specifically the role of diatoms) appears to be aimed at studying the supply of high quality food for the benthic and pelagic food webs. This seems reasonable and important. Knowledge of ?who?s there? not only tells us about food quality (i.e. to what degree upper trophic levels may benefit) but may also tell us something about mechanisms governing the phytoplankton blooms (different mechanisms may apply to different species).

3) SPATIAL CONNECTIONS: Although I think comparisons of Suisun and Central Bay chlorophyll timeseries is a good idea for exploring connections between these parts of the Bay, the applicants did not provide any background on what those connections might consist of. The applicants state that this comparison will allow them to evaluate the extent to which upstream conditions can be estimated from downstream data (p. 3). I wonder what causes the applicants to expect this could be the case, and they do not speculate in the proposal as to mechanisms which could cause such an interconnection. I suppose one could be transport of phytoplankton biomass, while another could be local conditions driving local blooms which happen to be synchronized across space. It would have been helpful if the authors said a bit more about what they think governing/connecting mechanisms might be. Regardless of the lack of discussion on this, I think these comparisons are a good idea (especially if the data?s sitting there!).

4) AMMONIUM LIMITATION: (Refer to comments under ?Goals? above). The applicants? (to this reviewer) confusing statements regarding ammonium limitations on phytoplankton growth lead me (not a nutrient chemist) to believe that this part of the proposal is not well justified. Information on phytoplankton growth limitation by nutrient drawdown (a different issue, I think) is interesting and valuable; however, it is not clear that that is what the applicants intend.

3. <u>Approach.</u> Is the approach well designed and appropriate for meeting the objectives of the project? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology or approaches? Will the information ultimately be useful to decision-makers?

1) SAMPLING TIMESCALES: In my opinion, the use of a Turner Designs SCUFA for measuring high-frequency variability in chlorophyll fluorescence in Suisun Bay is a great idea. These instruments appear to be robust, and long-term, high-frequency timeseries can provide extremely rich information on timescales varying from minutes to years. Processes governing chlorophyll variability may include oscillatory tidal advection, spring/neap variations in tidal energy, freshwater inputs and gravitational circulation, diel variations in growth and consumption, tidal variations in light-driven growth, deposition and resuspension, and more. The relative importance of such processes can often be inferred from timeseries analysis. In general, this is a powerful and reasonable approach, and new information will definitely come of these timeseries. Taking things a step further and measuring primary production rates for comparison with the Cole and Cloern model, using chlorophyll and PAR data collected at different time scales (p. 4) also is a good idea, if we want to know how to evaluate long-term evolution of productivity in NSFB.

However, I do have some specific questions and suggestions regarding the details of this part of the proposal. First, I wonder how the applicants chose the particular location for the proposed moored SCUFA/CTD in Suisun Bay (?south of the channel at 122.02 W, 38.04 N?, near the USGS Station #6 in the channel). I assume they wanted to coordinate with established sampling sites; however, do the applicants know that the proposed (apparently shallower, non-channel) mooring site is in fact representative of the nearby channel site (where I presume the discrete, less frequent sampling will take place)?? It has been shown that in many estuarine systems with variable bathymetry (including Suisun Bay, pre-potamocorbula, Cloern et al. 1985; South Bay, Thompson 1999) that chlorophyll gradients from shoal to channel can be substantial, with highest concentrations often occurring in the shoals. I don?t know if this is still the case in post-potamocorbula Suisun Bay, but I think it is certainly possible. If chlorophyll does vary significantly along bathymetric gradients, then the proposed mooring site *may* not be a) comparable with the shipboard discrete channel measurements for the purpose of identifying appropriate sampling timescales; b) representative of locations which may be impacted by

restoration. As I?m sure the applicants are aware, Suisun Bay is an extremely complex environment due to heterogeneities in bathymetry, organism distributions, and hydrodynamics. I doubt, therefore, from a chlorophyll perspective that we could expect it to look like a well-mixed reactor?we should expect spatial gradients in things like chlorophyll, especially during bloom periods. So, why is this particular location chosen?? I suggest that, if this proposal is funded, the applicants consult with other scientists working in this environment (e.g. hydrodynamicists, benthic ecologists, etc.) to very carefully and strategically choose the SCUFA location and to also perform preliminary reconnaissance of the area to ensure that the SCUFA measures the water masses intended. On the other hand, if the shipboard measurements are necessary for calibration of the SCUFA/moored CTD and/or coordination with the productivity measurements (not clear to me), then I suggest the SCUFA/moored CTD be placed closer to the discrete sampling station and within the same bathymetric range, or that calibration/productivity samples be taken at the mooring location from a smaller boat. In either case, preliminary reconnaissance is suggested.

Second, if appropriate sampling timescales are really what the applicants are after, then comparison of monthly and weekly shipboard measurements with the high-frequency SCUFA timeseries (Objective 2, p. 3) is not necessary: simple subsampling of the high-frequency timeseries could do the trick. This would also eliminate any problems related to possible spatial mismatching of discrete sampling and the SCUFA. Estimates of productivity (using the Cole and Cloern model) could be done based on these subsampled SCUFA timeseries and then compared.

Third, if co-location of the shipboard measurements and SCUFA is not necessary, and if spatial heterogeneity and governing processes are of interest to the applicants, they might consider placing the SCUFA in a completely different region (e.g. a shoal region that might exchange with the channel). Having SCUFA timeseries in the shallows and discrete measurements in the channel could allow for some exploration of processes governing bloom development (e.g. bloom initiation in shoals and subsequent transport to channel), as opposed to mere quantification. Even better would be to moor 2 SCUFA?s?one in the shallows, one in the channel (if adequate funds were available)?to provide high-frequency timeseries in both subregions. Just food for thought.

Fourth, I have several ideas of what the applicants *might* mean by ?spectral distribution of the fluorometric data? (w.r.t. comparing with discrete samples, p. 8), but I don?t know for certain what is meant here.

2) ROLE OF DIATOMS/LARGE CELLS: The applicants emphasize the use of size-fractionated biomass data and productivity experiments using C14, N15, and Si32 in assessing the role of diatoms in North Bay productivity and foodwebs. This is not my area of expertise; however, as I understand it, these methods are not a direct approach for getting at diatom abundance. A more direct method would be microscopic analysis (counts) of preserved phytoplankton samples, which indicate quantitatively which species are there and in what proportion; additionally, I believe such counts can provide categorizations with respect to size. Mentioned but not emphasized (p. 9) is ?phytoplankton enumeration? of preserved samples using the Utermohl technique; I am not personally familiar with this technique and whether it provides the same information as the microscopic analysis?it may. Regardless, if the applicants are interested specifically in the role of diatoms, my understanding is that the best method may be microscopic counting of individual phytoplankton species. The panel may want to consult with a scientist familiar with the specifics of these methods.

3) SPATIAL CONNECTIONS: With respect to exploring spatial-temporal connections between NSFB and Central Bay phytoplankton blooms, I believe the general approach of using moored SCUFA timeseries is appropriate. One concern I have is (again) the location of the

Suisun Bay SCUFA. Do the applicants have knowledge that that apparently shallower, non-channel location will be appropriate for comparing with Central Bay timeseries?? If the SCUFA is placed, for example, in a location which does not experience much exchange with the rest of the embayment and where local growth/consumption processes dominate, is it representative of Suisun Bay overall and should we expect there to be any connection between that site and Central Bay? Will we learn as much as we could if we picked another site? Again, I suggest that the PI?s communicate as much as possible with other scientists knowledgeable of esp. hydrodynamics in this region and perform some preliminary reconnaissance to ensure that the chosen site will provide the expected information. Several USGS scientists recently completed a multidisciplinary study of Suisun Bay (hydrodynamics, benthic ecology, phytoplankton, contaminants, etc.) and may have information valuable to the planning of this proposed new effort.

4) AMMONIUM LIMITATION: Setting aside my questions and confusion w.r.t. the ammonium limitation issue (discussed above), if we assume the applicants want to take measurements to explore limitation, for example, due to drawdown of nutrients during a bloom, then the proposed construction of timeseries of nutrients and chlorophyll from shipboard measurements sounds fine.

4. <u>**Feasibility.**</u> Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

   Some aspects of this project are well documented and should yield valuable information. For example, I am confident the proposed high-frequency timeseries will provide new, important information on 1) how this part of the system works (e.g. what timescales and associated processes are important), and 2) what sampling frequency is appropriate. I do have suggestions on how to do this a bit differently (e.g. instead of comparing shipboard discrete samples with SCUFA data, simply subsample the SCUFA data, pay careful attention to and possibly move the location of the SCUFA, see above), but I believe the general approach is good. I also believe that, if proper research is conducted before hand regarding the moored instrument location, then the comparison of timeseries from Central and Suisun Bays will be fruitful and provide information on spatial-temporal connections. On the other hand, I found the applicants? descriptions of the diatom and ammonium aspects of this proposal less clear. That, plus the fact that these areas fall less within my own expertise, made it more difficult for me to assess the feasibility of those parts of the study.

5. <u>**Project-Specific Performance Measures.**</u> Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Is there enough detail as to how the performance measures will be quantified? For restoration projects, are monitoring plans explicit and detailed enough to determine if performance measures will be adequately assessed?

   Within the bounds of my concerns and uncertainties stated above, the performance measures seem adequate.

6. <u>**Products.**</u> Are products of value likely from the project? Specifically for restoration projects, are products of value also likely from the monitoring component? Are interpretative outcomes likely from the project?

   The products described would be valuable contributions. Knowledge of the important timescales of chlorophyll variability in this part of the system?and communication of that knowledge through presentations and publications?would provide a great service to the SF Bay-Delta community, including decision makers and designers of monitoring programs.

7. **Capabilities.** What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

   I do not have intimate knowledge of the track record, qualifications, or resources of the applicants; however, the information provided suggests they should be capable of doing the proposed work.

8. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

   From my perspective, the budget seems reasonable.

Miscellaneous comments:

On p. 5-6, the applicants discuss possible mechanisms driving temporal variability in chlorophyll in Central and Suisun Bays and raise the question of why chlorophyll peaks would occur during spring tides. I don?t know the answer to this question, and the answer of course will probably vary over space and time. However, here are some possibilities to consider: 1) if shoals in fact have the highest chlorophyll, and if measurements are taken in the channel, then measurements may show highest channel chlorophyll during spring tides due to enhanced shoal-channel exchange; 2) if large cells dominate, then they may sink out during neap tides when tidal energy is low but remain suspended longer during spring tides when tidal energy is high; 3) gravitational circulation, which may greatly reduce water and particle residence times, is most likely to occur during neap tides when tidal mixing is weak and less likely to mix out vertical salinity gradients?therefore, on spring tides, gravitational circulation could be reduced and residence times lengthened; 4) resuspension of benthic diatoms could increase during spring tides and enhance water column chlorophyll.

More food for thought: If by some crazy chance hydrodynamics colleagues (e.g. USGS, Stanford, Berkeley) would happen to be installing moored instruments for measuring velocities in the Suisun Bay region during the proposed study period (they?ve done so in the past), it may be extremely insightful to coordinate the SCUFA location with their instruments, if consistent with the goals of this study. Co-location of velocity and concentration measurements allows for rigorous calculation of fluxes of water quality constituents. If it were at all possible for fluxes to be a by-product of this project, additional interesting and important questions could be addressed, like: Is the net flux of phytoplankton biomass up-estuary or down-estuary? How much of the biomass in a region is grown locally versus imported? How do the answers to those questions change over time?

External Scientific: #5

Proposal Number: **140**

Applicant Organization: **San Francisco State University, Romberg Tiburon Center**

Proposal Title: **Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay**

Conflict of Interest Statements:
I have no financial interest in this proposal.
XCorrect
-Incorrect

In the blank below please explain any connection to proposal, to applicant, co-applicant or subcontractor or to submitting institution (write "none" if no connection):

none

Review:

Please provide an overall evaluation summary rating:

Excellent: outstanding in all respects;
Good: quality but some deficiencies;
Poor: serious deficiencies.

Overall Evaluation Summary Rating	Provide a brief explanation of your summary rating
-Excellent XGood -Poor	Phytoplankton dynamics are ceratinly central to the basis and evaluation of the CALFED Bay Delta Ecosystem Restoration Program. In addition, detection of subtle changes in ecosystem function in response to restoration action is a critical component of adaptive management. The authors place their current work into the context of a rich-history of estuarine-wide research and potentially provide the CALFED restoration program with noteworthy advances in the understanding of the Bay's phytoplankton dynamics and CALFED's detection of change in ecosystem function. However, the current proposal lacks essential detail to assure success. I recommend that CALFED funds a component of this proposal to enable the authors to gain the additional information that is required for success. In particular, a hydrological component is needed and greater detail in regards to generation of CALFED-useful continuous data. In my opinion, CALFED should place high priority in the general objective and appoach outlined in this proposal; thus, CALFED should help ensure the best possible execution of this important program.

1. **Goals.** Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the concept timely and important?

   The overarching goal of this proposal is to evaluate phytoplankton biomass and NPP in the northern SF-Bay (and related environmental factors) on time scales that allow highly accurate detection of daily, weekly, monthly, and annual change in phytoplankton dynamics and potential connections to CALFED restoration activities. A series of six objectives are presented in support of this noble broad goal: 1) obtain continuous fluorescence measurements in Suisun Bay using a moored fluorometer, 2) compare "continuous" fluorescence with direct measurements at various time scales, 3) provide a baseline for phytoplankton dynamics prior to the start of Delta restoration activities, 4) measure NPP directly to compare with a pre-existing Delta model, 5) evaluate the role of diatoms within the phytoplankton pool, and 6) examine phytoplankton dynamics in terms of important physical and chemical environmental variables. The overarching goal and associated objectives are very timely and important to the CALFED Bay Delta Restoration Program.

2. **Justification.** Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full-scale implementation project justified?

   The work is certainly justified relative to existing knowledge. The northern SF-Bay and much of the Delta have low rates of NPP and low concentrations of phytoplankton biomass and the planktonic food web is thought to be tightly coupled to this low phytoplankton production. In addition, phytoplankton biomass has declined over the past two decades (partially in response to the invasion of Potamocorbula). Although multiple factors are likely resposible for the well documented declines in zooplankton and many native fishes, declines in phytoplankton biomass almost certainly have contributed to these declines. Regardless of the strength of the trophic connections, phytoplankton production and loss terms are critical ecosystem processes that warrant acute attention from the CALFED restoration program. Real-time monitoring of ecosystem processes provide Delta managers with poerful data sets that can be used to detect and infer ecosystem change in function in response to restoration actions.

3. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology or approaches? Will the information ultimately be useful to decision-makers?

   In general, the approach is well designed and results are very likely to add to our base of knowledge. The proposed research builds upon the research of others throughout the Bay region and the authors' own research. Novel and useful information can almost certainly be generated. With that said, I have major concerns with the successful implementation of a continuous monitoring program and associated discreet measurements (and experiments).

   I see three areas of concern regarding the continuous measurement program:

   1) INSTRUMENT DEPLOYMENT Susuin Bay hydrodynamics are EXTREMELY complicated and selection of a central mooring location (or several) requires a great deal of thought regarding the hydrology of the system.

2) INSTRUMENT MAINTAINANCE There is great interest in using equipment that allows continuous measurements of biological, chemical, and physical parameters. There have been great advances in instrumentation to measure fluorescence at remote stations, BUT biofouling, signal drift, and signal noise present major problems in the analysis of the data generated by these instruments. In my opinion, monthly instrument swaps is not adequate methodological detail.

3) DATA ANALYSIS Long-time series data are notoriously difficult to analyze. Detection of change in respose to restoration action will require sophisticated data analysis tools. More detail is required here.

I have additional concerns regarding the discreet measurement program:

1) There is a disconnect between the continuous measurements and discreet measures.

2) Comparison with the primary production model used in Jassby et al. 2002 or earlier Cole and Cloern publications requires model specific chemical and physical parameters to be measured AND a careful experimental design.

3) Diatoms generally make up a large component of the northern SF- Bay's phytoplankton biomass and there is little previous evidence for sustained N, P, or Si limitation. While I have no problem with the measurement of these nutrients or experiments that examine potential for limitation, I tend to think nutrinet limitation is a red-herring. Light limitation is well documented throughout the entire region, and Suisun Bay has EXTREMELY complicated hydrodynamics that result in amazing spatial and temporal variability in suspended sediments. Further, advective processes associated with adjacent wetlands, mudflats, and riverine inputs may account for much of the short term variablity within the Suisun Bay region.

4. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

   The approoach is feasible, but requires refinement. The devil is in the details. Success is likely with a more thoughtful and detailed experimental design.

5. **Project-Specific Performance Measures.** Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Is there enough detail as to how the performance measures will be quantified? For restoration projects, are monitoring plans explicit and detailed enough to determine if performance measures will be adequately assessed?

   Q1: The authors have exceptional publication records and success in highly likely in regards to publication.

   Q2: see above-- I have concerns regarding the global goal

   Q3: see above-- more detail is needed

6. **Products.** Are products of value likely from the project? Specifically for restoration projects, are products of value also likely from the monitoring component? Are interpretative outcomes likely from the project?

   YES-YES-YES Assuming that the above comments are fully and carefully considered.

7. **Capabilities.** What is the track record of applicants in terms of past projects? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

    Excellent track records. Highly qualified researchers with the appropriate infrastructure.

8. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed?

    The costs seem reasonable, especially compared to other CALFED projects, and the potential contribution to the CALFED restoration program could be substantial.

Miscellaneous comments:

Steve Monismith's group at Stanford and Jon Burau's group at the USGS in Sacramento have made great advancements in our understanding of the hydrodynamics of Susuin Bay. These groups may be best positioned to help with instrument placement. Lisa Lucas at the USGS in Menlo Park and Ted Sommer at USFW have incorporated similar fluorescence instrumentation into field projects in other areas of the Delta and may be able to provide valuable supportive information.

Environmental Compliance:

Proposal Number: 140

Applicant Organization: San Francisco State University, Romberg Tiburon Center

Proposal Title: Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay

1. Are the legal or regulatory issues that affect the proposal identified adequately in the proposal?

    -Yes **X**No

    If no, please explain:

    Depends:

    Suisun Marsh access, and sampling activities requires notification of BCDC and State Lands Commission, with corresponding permit/land use lease, and CEQA documentation.

2. Does the project's timeline and budget reflect adequate planning to address legal and regulatory issues that affect the proposal?

    XYes **-**No

    If no, please explain:

    If the environmental compliance requirements have been accounted for under Project Management.

3. Do the legal and regulatory issues that affect the proposal significantly impair the project's feasibility?

    -Yes **X**No

    If yes, please explain:

Other Comments:

Budget:

Proposal Number: 140

Applicant Organization: San Francisco State University, Romberg Tiburon Center

Proposal Title: Understanding the time scales of primary production responses to environmental perturbations in North San Francisco Bay

1. Does the proposal include a detailed budget for each year of requested support?

   XYes **-**No

   If no, please explain:

2. Does the proposal include a detailed budget for each task identified?

   XYes **-**No

   If no, please explain:

3. Does the proposal clearly state the type of expenses encompassed in indirect rates or overhead costs?

   XYes **-**No

   If no, please explain:

4. Are appropriate project management costs clearly identified?

   XYes **-**No

   If no, please explain:

5. Do the total funds requested (Form I, Question 17A) equal the combined total annual costs in the budget summary?

   XYes **-**No

   If no, please explain (for example, are costs to be reimbursed by cost share funds included in the budget summary).

6. Does the budget justification adequately explain major expenses?

   XYes **-**No

   If no, please explain:

7. Are there other budget issues that warrant consideration?

   -Yes **X**No

   If yes, please explain:

Other Comments: