# Selection Panel Review Summary

**Project No.:** 009
**Proposal Title:** Impact of BDCP-Created Tidal Wetlands on Increased Fish Mercury Levels in the Delta
**Principal Investigator:** Kenneth Coale
**Amount Requested:** $600,000
**Recommended Amount:** $ 0

**Summary:** This project has an important goal of assessing the impact of created wetlands on methylmercury production and subsequent export of methylmercury to the Delta and its fish.

**Assessment:** The Selection Panel found that while the goals of the project were extremely worthwhile, the proposal had many deficiencies which included inadequate research design, lack of clear outcomes, and no performance measures. While the project would identify total mercury concentrations in biota along a transect starting at the outflow from each wetland, the ability to tie the concentrations to wetland fluxes is questionable. First, the geometry and location of the outflow is critical to interpretation of results, and if not specifically understood, the results will not be transferable to other wetlands. Second, the flow pattern might not be linear from the outlet, i.e., currents moving sideways may affect the methylmercury in water and sediments. Third, sediments underneath the cages may produce methylmercury and affect the results. Other concerns: 1) only total mercury will be measured in biota. This is likely valid for the fish (although it should be checked), but it is definitely not adequate for bivalves, 2) only 3-4 wetlands, out of the 10, will be done for one month of each year, so year to year variation will not be measured; additionally this system has seasonal variability which will not be addressed with the proposed design, and 3) samples will be composited from each cage, which will decrease the statistical reliability of the study. The proposal was poorly written with over 50% of the references missing from the Literature Cited section and the budget contained errors.

# CALFED Ecosystem Restoration Program

# External Scientific Review Form

**Proposal Number:** 009

**Proposal Title:** Impact of BDCP-Created Tidal Wetlands on Increased Fish Mercury Levels in the Delta

**Reviewer:** #1

## Conflict of Interest Statements:

I have no financial interest in this proposal (please mark correct response).

**Correct**

## General Review Questions:

Along with your written observations in response to the questions below, please rate each using the following criteria:

Excellent: Outstanding in all respects
Very Good: High quality in nearly all aspects
Good: Quality work, but with some deficiencies
Fair: Lacking in one or more critical aspects
Poor: Serious deficiencies

1. **Problem/Goals.** Is the problem that the project is designed to address adequately described? Are the goals, objectives, and hypotheses clearly stated and internally consistent? Does the proposal describe the ecosystem goals it is designed to address (link to ERP goals)?

   Comments:

The PI's do an adequate job of describing the problem of interest. The construction or rehabilitation of wetlands in the Bay-Delta region could have detrimental effects on contaminant bioaccumulation. The constant input of inorganic Hg from legacy gold mining provides a potential for bioaccumulation, but importantly, the key step for bioaccumulation is microbial methylation - - a process that has been shown to occur in wetlands. The PIs discuss a series of approaches that other investigators have recently used to assess the complexity of issues related to wetland effects on mercury methylation and bioaccumulation. They propose a simplistic approach that they purport to have several advantages over previous studies. Theirs is to simply monitor caged fish and clams in wetland transects.

At first glance, this would appear to be a straightforward way to assess bioaccumulation but I do take exception to using the other studies as comparisons to develop the objectives for this particular study. In a sense, it discounts the approaches of other studies and casts doubts on their interpretation. The comparison to large scale studies with differing watersheds have absolutely nothing to do with coastal wetlands in California yet they are used to build a contrary case for the PIs' approach to assess net import or export. The comparison to previous work on mass-balancing wetlands could have been used to build the groundwork for their approach rather than rejecting them because of the cost and attention details of hg bioaccumulation pathways.

This proposal would have been strengthened by telling the reviewer what this particular project would do to build knowledge off of other approaches. Instead, it almost rejects the previous studies in favor of their approach. I am certain that the authors of other process studies referenced in the literature would be similarly critical of the simplicity of this approach

Rating: **Good**

2. **Approach.** Does the proposal clearly describe its approach (including study design and methods, if appropriate)? Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal? Will the proposal contribute to our knowledge base?

   Comments:

I have serious doubts that this simple biomonitoring project will do much to increase our understanding of mercury bioaccumulation processes in the Bay-Delta wetlands. It most likely will end up asking more questions than it was originally planning to answer. If this particular project were a small aspect of a larger study that addressed wetland hydrology, chemistry and biological processes, I would be somewhat positive. While the PIs acknowledge that their study does not give an estimate of loading in grams per day, they would be able to "determine whether the wetland is a net importer or exporter of MeHg and the aerial extent of the impact". I have absolutely no idea how they can reach a conclusion like that. How can they possibly assess net import or export by simply analyzing fish and clam Hg content? How can you use a mussel, that filter-feeds on passing materials in a dynamic flowing system, as an indicator of a "hot spot"? When I first read the abstract of the proposal I thought that maybe the PIs were going to use stable isotopes of C and N to determine food web characteristics. I thought perhaps they would correlate these results with direct methylation measurements. Perhaps look at gut analyses to make sure that each of these biomonitors were consuming similar diets across different wetland types. There is nothing more here than the analyses of these biota for Hg (and I'm not even sure if they will look at MeHg/HgT ratios). Given the complexity of the pathways presented in Figure 1 (which even identifies pelagic and benthic food chains), I'm surprised by they propose to answer "big picture" questions on net import or export by using this approach alone.

   Rating: **Poor**

3. **Feasibility.** Is the proposed project's approach fully documented and technically feasible? Can the project be completed within reasonably foreseeable constraints (e.g., acquiring permits, construction, weather, etc…)? Does the proposal thoroughly address requirements such as environmental compliance and permitting? Is the scale of the project consistent with the objectives?

   Comments:

While technically feasible, the results from this study will be interesting, but will do little to advance our predictive capabilities. The sentence: "Since fish are going to be transplanted in transects from the mouths of wetlands to far away from the mouth the area that has significant bioaccumulation can be estimated" is extremely confusing. Do all wetlands have a single point of inflow and outflow? If an area of significant bioaccumulation is noted, what are we going to learn? What are the characteristics of the area? For instance, organic matter deposition is patchy in a wetland and there are zones with high methylation rates simply because the organic matter is labile and sulfate reducing bacteria are abundant. Clams may be elevated because they are likely close to sediment and periphyton on the surface uptake MeHg and are a food source for those clams. However, there are no grazers on periphyton and therefore, no route of uptake for silversides. Clams will be high, fish low. What does that have to say about net import or export or the aerial extent of the impact? It may simply be a substrate/vector issue.

I another case, zooplankton are transported by the tides. They graze during stagnation periods at the uppermost reaches of the wetland and then are transported to a site near the mouth where they are grazed by silversides. What would that indicate about the aerial extent of methylation if one were only to use fish analyses? Without any ancillary data, the PIs will have absolutely no confidence in their conclusions based on clam and fish analyses.

Rating: **Poor**

4. **Conceptual Model.** Does the proposal provide a conceptual model that describes the interconnections among the key ecosystem components relevant to the action(s) being proposed? Does the conceptual model clearly explain the hypotheses it is testing?

Comments:

The proposal presents a conceptual model as a diagram in Figure 1, but the description is totally lacking. I would have like to have seen an explanation of how they might interpret their data. An example of areal results from a fictitious wetland would have allowed the reader to at least get a better idea of their hierarchy of decision made during interpretation. I agree that fish and clams could be used as integrators, but I have serious doubt that they can be used to describe processes and more importantly for this project, flux characteristics (net import or export) of wetlands. There are simply no plans for collection of supporting measurements to use to draw valid scientific conclusions. The comparisons are made to "control" sites furthest from the wetland mouth. What if conditions at those sites furthest from the mouth are sites of optimal methylation? I'm not sure what conceptual model guides this research.

Rating: **Poor**

5. **Performance Evaluation Plan (Monitoring Plan and Performance Measures).** Does the proposal include a plan for project performance evaluation (monitoring to assess results and evaluate assumptions and hypotheses)? Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Will future studies or restoration projects be able to incorporate the information from this project?

Comments:

I can't say that I found actual performance measures in this study. The study involves placing multiple cages of fish and clams in wetlands. There is no description of milestones and alternate paths to achieve the outcomes. The reviewer has no idea about what characteristics of wetland will be compared by the study, merely that the length of waterways affected will be determined. That's a rather simple measure for success.

Rating: **Poor**

6. **Expected Products/Outcomes.** Are products of value likely from the project? Are products of value also likely from the individual components of the project? Will the results of this study be readily accessible?

Comments:

I cannot determine what products will emerge from this study. Is it that the results "could be used to inform the DRERIP Mercury Model"? That doesn't inspire much confidence in the use of the results. Typically, an outcome-based study is designed as an outcome first, then you work back to the best approach to get to those outcomes. This project seems to be designed the other way. It uses a technique and then hopes to reach an outcome. If the main outcome of this work is to determine the best approach for reducing "mobilization of mercury into the foodweb," I find it hard to believe that this biomonitoring project is best.

Rating: **Fair**

7. **Previous Related Work.** Does the proposed project continue past work or include any work that could be considered a duplication of work previously done or currently being done by others?

Comments:

In a sense, I wish that the authors did build off of their previous work, especially the Heim et al. study of the distribution of HgT and MeHg across the Bay-Delta system. That would have at least framed the study and prioritized sites. The current study does not appear to duplicate any other previous studies.

Rating: **Good**

8. **Qualifications.** 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? Do they have working knowledge of California streams and rivers?

Comments:

I have little doubt that the PIs have a working knowledge of the study area and that they have experience in studying Hg cycling in the Bay/Delta region. They most likely have the infrastructure to make this approach work. The PIs have no recent first-authored papers on Hg cycling in the Bay/Delta though they are active members of research teams.

Rating: **Good**

9. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed? If the budget is considered to be excessive or inadequate for the work proposed, please highlight areas of the budget that may be of concern.

   Comments:

This is an extremely expensive monitoring proposal. It is top heavy with staff salaries (equivalent of one full time staff member for two years) and there is little justification of subcontracts or material costs. Who does the collections? Are analyses in-house? Why aren't there any students involved?

Rating: **Fair**

**Additional comments:**

None

## Overall Evaluation Summary Rating

In the space below, please provide an overall rating of the proposal using one of the following categories:

- **Superior:** Outstanding in all respects with superior technical and scientific value and no significant concerns. Expected to add substantial new thinking/concepts to our knowledge/understanding of the topic proposed.
- **Above Average:** A very good proposal with at least high technical and scientific value and no significant concerns. Will add solid basic knowledge/understanding of the topic proposed.
- **Adequate:** A reasonable proposal without serious technical deficiencies and at least adequate value scientifically. Will add some useful knowledge to the topic proposed.
- **Inadequate:** A technically deficient proposal and/or one with low value, serious impediments or concerns. Will not likely change our basic knowledge/understanding of the topic proposed.

Rating: **Inadequate**

Please provide a brief explanation of your summary rating:

I was underwhelmed by the scientific rigor of this proposal and expected something quite different from this group. They have published on the general patterns of accumulation of MeHg and HgT in the Bay/Delta area and that should have been the basis for a new study. As such, this almost reads like a rejection of past approaches by multiple researchers in the Bay/Delta in favor of a biomonitoring approach. This is not innovative and in fact, Mussel

Watch has been using this approach for over 25 years. I have little confidence that the results of this study would be used to determine which sites or wetland types, are those best for wetland remediation or construction. As a component of a multi-investigator process-oriented study, this small phase would be an important component. As a standalone project, its applications are extremely limited.

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## CALFED Ecosystem Restoration Program
## External Scientific Review Form

**Proposal Number:** 009

**Proposal Title:** Impact of BDSP-Created Tidal Wetlands on Increased Fish Mercury Levels in the Delta

**Reviewer:** #2

## Conflict of Interest Statements:

I have no financial interest in this proposal (please mark correct response).

**- X Correct**
- Incorrect

## General Review Questions:

Along with your written observations in response to the questions below, please rate each using the following criteria:

Excellent: Outstanding in all respects
Very Good: High quality in nearly all aspects
Good: Quality work, but with some deficiencies
Fair: Lacking in one or more critical aspects
Poor: Serious deficiencies

1. **Problem/Goals.** Is the problem that the project is designed to address adequately described? Are the goals, objectives, and hypotheses clearly stated and internally consistent? Does the proposal describe the ecosystem goals it is designed to address (link to ERP goals)?

   Comments:

The proposal does indeed have a focus on an area of key interest to ERP goals (Delta and Suisun Marsh). The proposal's goals do not exactly match with the ERP goal of protecting and/or restoring functional habitat types (goal 4). Rather, the project goal's idea is a useful one in trying to assess the success of habitat restoration, in a spatial manner. The proposal's main objective is to "estimate the area affected by habitat restoration and creation of wetlands by determining increases of MeHg in tissue." The proposal also seeks to address goal 6 of the ERP (water and sediment quality) as an additional part of the prior objective described.

Overall, an objective to determine the influence of restored tidal wetlands on Hg bioaccumulation in fish of adjacent waterways is a very good idea. There is little precedent about exactly how much MeHg is exported by tidal wetlands (we know lots of MeHg is produced there, but there is pretty much no information on tidal exchange) and thus the "area" affected by MeHg export from tidal wetlands is relatively unknown. I'm not convinced this is the approach to use to answer this, but I will comment on that in the following section.

Rating: **Very Good**

2. **Approach.** Does the proposal clearly describe its approach (including study design and methods, if appropriate)? Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal? Will the proposal contribute to our knowledge base?

   Comments:

Given that this approach will allow the investigators to look at effects from a large number of tidal wetlands (10), this is a financially sound way to go. I disagree with the cost estimates of the investigators on page 8, where they believe the investigation of 20 diel cycles in a system over a year would cost $600K. I know people who have done this exact thing recently on the East coast and it cost a fraction of this amount, but arguably, with a relatively large effort.

The approach is relatively well described, however, I find the description of the need for a better approach to be more detailed than the approach itself. The approach assumes several things (presumably; the assumptions are not explicitly stated). One is that the investigators assume they are "catching" the flow of the wetland without a plan to ensure that. Second, they assume that any potential for MeHg bioaccumulation is due to export from the marsh and not due to production in sediments below, which is highly spatially variable, and again not planned to be accounted for in the proposal. Thirdly, there is no overall "spatial" plan here. Flow in these systems does not work in a linear fashion, so I have major doubts that the use of a transect will provide information that contributes significantly to the knowledge base.

Rating: **Fair**

3. **Feasibility.** Is the proposed project's approach fully documented and technically feasible? Can the project be completed within reasonably foreseeable constraints (e.g., acquiring permits, construction, weather, etc…)? Does the proposal thoroughly address requirements such as environmental compliance and permitting? Is the scale of the project consistent with the objectives?

   Comments:

The proposal's approach is relatively well documented and technically feasible, but it is risky to assume that the approach will provide unequivocal information. I do not foresee any constraints on carrying out the project as described. Environmental compliance and permitting section states that this work is to be conducted on state lands and water, so I presume this is fine. The scale of the project takes some allowances for increasing the number of studied wetlands, 10, which is an admirable goal. If only this study had some more discussion on how exactly the area affected by the wetlands would be calculated (correctly and in an unbiased way), then I would be much more impressed with the feasibility of the approach. This is not as straightforward an issue as is assumed so in the proposal. As it stands, I think it is risky and unlikely to yield meaningful results.

I question several major technical points of the approach:

1. How do you choose a "transect"? The flow path of water leaving will be key here. This would involve some sort of physical determination so as not to bias the results (transect in a major flowpath in one wetland versus a diffuse one in another).
2. How do you distinguish wetland flow from in situ production and bioaccumulation?
3. The study is limited to 2 "representative" flow periods (winter and summer). The authors, in their introductory material themselves note that multiple cycles (20 or more in a year) are necessary to yield good information about loads from wetlands. Although these one-month long deployments will encompass more than 20 diel cycles, they may not adequately take seasonal variations into consideration.

Rating: **Fair**

4. **<u>Conceptual Model.</u>** Does the proposal provide a conceptual model that describes the interconnections among the key ecosystem components relevant to the action(s) being proposed? Does the conceptual model clearly explain the hypotheses it is testing?

Comments:

The conceptual model is basic and nothing new, but adequate. The approach seems entirely feasible for some particular questions. It was not particularly explicit how it would be useful for this objective. I was disappointed to see that the principal references supporting the approach (Ackerman et al., 2010; Wood et al., 2010; Heim et al., 2011) were not listed in the references section.

Rating: **Good**

5. **<u>Performance Evaluation Plan (Monitoring Plan and Performance Measures).</u>** Does the proposal include a plan for project performance evaluation (monitoring to assess results and evaluate assumptions and hypotheses)? Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Will future studies or restoration projects be able to incorporate the information from this project?

Comments:

There is not a particular "performance evaluation plan" in the proposal. I presume the proposal will evaluate hypotheses as information becomes available. The only real deliverable here is an assessment of "area" affected in relation to a number of wetland parameters. As stated earlier, this is risky. If the results are ambiguous, as a result of inefficiencies in the design, no deliverable may be met at all. If it works, it would be very useful information to incorporate for future studies and restoration projects.

Rating: **Poor**

6. **Expected Products/Outcomes.** Are products of value likely from the project? Are products of value also likely from the individual components of the project? Will the results of this study be readily accessible?

Comments:

If the study works, the project results will be valuable. As stated before, I think this is a risky approach, unlikely to yield meaningful results. There is little to no breakdown of individual components in the study. It's an all-or-nothing scenario with one real deliverable that may or may not be able to be delivered, so value of products from individual components of the project cannot be evaluated. I anticipate the results to be readily accessible. The group has been relatively successful as of late in publishing results of studies.

Rating: **Fair**

7. **Previous Related Work.** Does the proposed project continue past work or include any work that could be considered a duplication of work previously done or currently being done by others?

Comments:

The project intends to build on past work (Ackerman et al., 2010; Foe et al., 2003), but apply it to a different approach here. It is not duplicative of past efforts. I am unaware of similar work being done with this approach, but I am aware of work currently submitted and in review in a scientific journal that looks at net tidal MeHg mass fluxes from a tidal salt marsh on the east coast. From what I know of the study, one of the main conclusions is that tidal marshes are net exporters of MeHg, but at overall loads which are small compared to other in-estuary sources.

Rating: **Good**

8. **Qualifications.** 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? Do they have working knowledge of California streams and rivers?

Comments:

This group seems entirely qualified to carry out the project. Their past track record seems good in relation to publishing results. They have the necessary infrastructure within the group to fully carry out the project. Given their past work, they must have a very good working knowledge of California streams and rivers.

Rating: **Very Good**

9. **Cost/Benefit Comments.** Is the budget reasonable and adequate for the work proposed? If the budget is considered to be excessive or inadequate for the work proposed, please highlight areas of the budget that may be of concern.

Comments:

The budget is entirely reasonable. Most costs are related to personnel and overhead. This is quite normal. The study will require a lot of labor, so personnel costs are to be expected. Given the large number of wetland systems that the group intends to look at, it is quite good value for money if the approach actually works. Again, the only issue here is that it's risky. If results come out ambiguous (and I think the approach leaves some definite room for that), then the one overarching deliverable here will be vague and the benefit will be very small.

Rating: **Good**

**Additional comments:**

None.

## Overall Evaluation Summary Rating

In the space below, please provide an overall rating of the proposal using one of the following categories:

- **Superior:** Outstanding in all respects with superior technical and scientific value and no significant concerns. Expected to add substantial new thinking/concepts to our knowledge/understanding of the topic proposed.
- **Above Average:** A very good proposal with at least high technical and scientific value and no significant concerns. Will add solid basic knowledge/understanding of the topic proposed.
- **Adequate:** A reasonable proposal without serious technical deficiencies and at least adequate value scientifically. Will add some useful knowledge to the topic proposed.
- **Inadequate:** A technically deficient proposal and/or one with low value, serious impediments or concerns. Will not likely change our basic knowledge/understanding of the topic proposed.

Rating: Just below **Adequate** (This is a reasonable proposal without serious technical deficiencies and at least adequate value scientifically. It will add some useful knowledge to the topic proposed.)

Please provide a brief explanation of your summary rating:

This seems like a good group and they intend to tackle a question that is not well understood (to what extent to tidal wetlands actually export MeHg to their adjacent estuarine systems?). This is indeed currently relatively unknown. For the most part, high MeHg production within tidal wetlands has been presumed to be equal to high export of MeHg from tidal wetlands, but this presumption has not been rigorously tested before. My principal issue with the proposal is that there are complicating factors to the approach that the investigators have not considered, largely revolving around the physics of the flow from tidal wetlands and consideration of *in situ* MeHg production. As written, the approach is unlikely to yield unambiguous results and thus it is risky. This is especially so since there is really only one overarching deliverable to be made. If possible, I would ask the investigators to re-consider the technicalities of the approach. Some tweaking would likely lead to either increased workload and expense or fewer wetlands being evaluated.

# CALFED Ecosystem Restoration Program

# External Scientific Review Form

**Proposal Number:** 009

**Proposal Title:** Impact of BDCP-Created Tidal Wetlands on Increased Fish Mercury Levels in the Delta

**Reviewer:** #3

## Conflict of Interest Statements:

I have no financial interest in this proposal (please mark correct response).

**X** Correct
- Incorrect

## General Review Questions:

Along with your written observations in response to the questions below, please rate each using the following criteria:

Excellent: Outstanding in all respects
Very Good: High quality in nearly all aspects
Good: Quality work, but with some deficiencies
Fair: Lacking in one or more critical aspects
Poor: Serious deficiencies

1. **Problem/Goals.** Is the problem that the project is designed to address adequately described? Are the goals, objectives, and hypotheses clearly stated and internally consistent? Does the proposal describe the ecosystem goals it is designed to address (link to ERP goals)?

   Comments:

**The problem—will constructed tidal wetlands increase Hg concentrations in fish?—is clearly stated in the proposal. The proposal's goals and objectives are straightforward means to answer that central question. The proposal specifically addresses the ecosystem goals of habitat restoration.**

   Rating: **Excellent**

2. **Approach.** Does the proposal clearly describe its approach (including study design and methods, if appropriate)? Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal? Will the proposal contribute to our knowledge base?

   Comments:

**Mercury has a complicated biogeochemistry in aquatic systems, but the ultimate concern is what changes occur to methylmercury concentrations in fish. Therefore, this proposal has an efficient approach to addressing the ultimate concern. Praiseworthy technical aspects of this proposal are, (1) transplanting fish and clams rather than a single species, (2) three replicate cages for each species in each wetland, (3) five downstream test sites, and (4) comparing 10 wetlands rather than only a few. My biggest concern with the experimental design is the compositing of 10 individuals in each cage. This leaves only two measured concentrations for each cage. This is an unfortunate loss of information about statistical uncertainty. I assume compositing is proposed to reduce costs to allow for more replicate cages at each site. The compositing will reduce the N from 60 to 6 at each site and, therefore, weaken the analysis of variation within and between wetlands. Ackerman and Eagles-Smith 2010 (ES&T 44, 1451-1457) used 30 mosquitofish per cage and analyzed for whole-body total mercury in each fish, which gave them tight error bars and allowed them to show significant differences between the center and outlet sites of rice fields. The proposal authors could consider doing a power analysis to determine the adequate sample size for statistical analysis.**

**A second concern I have with the robustness of the sample plan is its general statement that 10 wetlands will be tested but only 3-4 wetlands will be tested each year. Again, this appears to be a compromise to keep costs down; however, it weakens the ability to compare among wetlands if the climate conditions are substantially different among the three years.**

**The proposal emphasizes the affected area will be determined from the caged studies, but there is only a terse explanation of how these areas will be measured at the bottom of page 13. Given the transect extending 1600 m from a wetland outlet, it seems the measurement will be linear distance rather than area. This is supported by the statement made at the bottom of page 13: "**The liner [sic] length of the distance from the significantly different stations from the mouth will be estimated."

**And at the bottom of page 16: "**The length of Delta water ways in meters affected by methyl mercury contamination will be determined."

**The proposal states there are five test sites downstream of each wetland, but then states that there will be 10 sites in the "adjacent channel." This should be clarified. Are they proposing to have two stations at each distance downstream of the wetlands?**

**The project presents fish as the best measure of methylmercury production and mobilization; however, they will be using EPA method 7473, which is for total mercury. Fish tissue is typically analyzed for total mercury, knowing from several published studies that essentially all the mercury in fish muscle is methylmercury. At the low end of the food chain a significant fraction of the total mercury in whole organisms is often not methylmercury. Because Ackerman and Eagles-Smith 2010 (ES&T 44, 1451-1457) showed total mercury in western mosquitofish was 94% methylmercury, it is reasonable to assume the total mercury in inland silversides is essentially all methylmercury. I am not aware of a similar comparison reported for clams. The proposers should demonstrate from the scientific literature or their own analysis that total mercury in Corbicula is a good measure of methylmercury.**

**The results from this project will certainly contribute to the knowledge base. If the results are unambiguous the project will undoubtedly be a valuable contribution to the**

**knowledge base. There is, however, as with other approaches discussed in the proposal, there is realistic chance of the results being inconclusive.**

Rating: **Very good** for overall approach; g**ood** for approach to sample analysis.

3. **Feasibility.** Is the proposed project's approach fully documented and technically feasible? Can the project be completed within reasonably foreseeable constraints (e.g., acquiring permits, construction, weather, etc…)? Does the proposal thoroughly address requirements such as environmental compliance and permitting? Is the scale of the project consistent with the objectives?

Comments:

**The proposed project is consistent with the stated objectives for the BDCP. The proposal's approach is well documented and appears to be technically feasible. Caged fish and mussels have been used for contaminant studies for decades. It appears the study could be reasonably completed in the three-year period. It was not clear to me if permitting and compliance requirements were adequately addressed.**

Rating: **Very good**

4. **Conceptual Model.** Does the proposal provide a conceptual model that describes the interconnections among the key ecosystem components relevant to the action(s) being proposed? Does the conceptual model clearly explain the hypotheses it is testing?

Comments:

**The conceptual model presented in the proposal was very good, although it was frustrating that so many citations were not provided in the References. Under "Background and Conceptual Models" the following citations were not included in the References:**

**Ackerman et al 2010**
**Bergamaski et al. 2011**
**Compeau and Bartha 1984**
**Fleming et al 2006**
**Foe et al 2008**
**Heim et al 2011**
**Negrey et al. 2011**
**Sellers et al 2001**
**Stephanson 2008 a & b**
**Wiener et al 2006**
**Wood et al. 2010**

**The hypotheses for this project is that constructed tidal wetlands increase methylmercury in fish and clams.**

Rating: **The conceptual model was excellent; the presentation of the concept was good.**

5. **Performance Evaluation Plan (Monitoring Plan and Performance Measures).** Does the proposal include a plan for project performance evaluation (monitoring to assess results and

evaluate assumptions and hypotheses)? Does the project include appropriate performance measures to measure success relative to the project's goals and objectives? Will future studies or restoration projects be able to incorporate the information from this project?

Comments:

**A performance evaluation plan for a research study seems to be the proposed data analysis of the collected samples. Ambiguity of results could be interpreted as project performance. An unambiguous result would most likely be considered superior project performance. The proposal does include an adaptive management approach of evaluating the use of two caged species after the first year and monitoring plan if necessary. Undoubtedly, any future projects will benefit from the results of this project, whether or not the results are inconclusive.**

Rating: **Very good**

6. **Expected Products/Outcomes.** Are products of value likely from the project? Are products of value also likely from the individual components of the project? Will the results of this study be readily accessible?

Comments:

**The project is likely to provide valuable information about the availability of methylmercury to the base of the food web in the Delta wetlands. I expect the investigators will publish the results in a peer-reviewed journal, as well as provide a final report, which will be available to the public.**

Rating: **Very good.**

7. **Previous Related Work.** Does the proposed project continue past work or include any work that could be considered a duplication of work previously done or currently being done by others?

Comments:

**I am not aware that any of the proposed project would be duplication of previous work. The project appropriately builds on the studies that have been completed in the Delta.**

Rating: **Excellent**

8. **Qualifications.** 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? Do they have working knowledge of California streams and rivers?

Comments:

**The investigators and their facilities have excellent qualifications for the proposed project.**

Rating: **Excellent**

9. **<u>Cost/Benefit Comments.</u>** Is the budget reasonable and adequate for the work proposed? If the budget is considered to be excessive or inadequate for the work proposed, please highlight areas of the budget that may be of concern.

Comments:

**The budget is reasonable for the proposed work. I was not able to evaluate the specifics of the budget because it was a general budget divided into staff time, overhead, "subcontractor costs" and "materials." The total cost of the project seems appropriate for the proposed effort and deliverables.**

Rating: **Very good.**

**Additional comments:**

None.

## Overall Evaluation Summary Rating

In the space below, please provide an overall rating of the proposal using one of the following categories:

- **Superior:** Outstanding in all respects with superior technical and scientific value and no significant concerns. Expected to add substantial new thinking/concepts to our knowledge/understanding of the topic proposed.
- **Above Average:** A very good proposal with at least high technical and scientific value and no significant concerns. Will add solid basic knowledge/understanding of the topic proposed.
- **Adequate:** A reasonable proposal without serious technical deficiencies and at least adequate value scientifically. Will add some useful knowledge to the topic proposed.
- **Inadequate:** A technically deficient proposal and/or one with low value, serious impediments or concerns. Will not likely change our basic knowledge/understanding of the topic proposed.

Rating: **Above Average**

Please provide a brief explanation of your summary rating:

**The proposed project is an excellent approach to addressing the concern about increased methylmercury loading from constructed wetlands. The investigators have proposed an expeditious approach to addressing this concern and they have the staff, experience, and facilities to carry out the project in a timely manner. My only concern is that sample sizes are sufficient to account for random variability and to discern an effect. If possible, I would recommend a greater sample size by analyzing the fish and clams individually rather than compositing samples. Regardless, this is a proper continuation of efforts to better understand if tidal wetlands are sources of methylmercury and if constructed wetlands will contribute to significant increases in mercury within the Delta food web.**