Selection Panel Review Summary

Project No.: 014
Proposal Title: Wetland and Rice Management to Limit Methylmercury Production and Export
Principal Investigator: Lisamarie Windham-Meyers
Amount Requested: $197,416
Recommended Amount: $197,416

Summary: The proposed project will undertake measurements of labile carbon, as well as a suite of measurements of factors that are likely to affect mercury methylation activities, including the quality of organic carbon, total mercury, pH, etc. Porewater methylmercury will be measured to give site specific (within each type of pilot manipulation) information on the effects of the treatments. This project would build upon an existing grant that tests whether changes in rice harvesting methods, or control of water levels in wetlands, will lower the rates of microbial methyl mercury production (from inorganic mercury).

Assessment: The Selection Panel found this to be a well laid out project with clear, testable hypotheses and would be conducted by a highly qualified research team. It has the possibility of informing wetland restoration, and addresses PSP priority number 2. This proposal would build upon a currently funded project. Concern was expressed over whether the funds would be available at the right time to take advantage of “piggy backing” on this existing work effort and creating the opportunity to leverage funds. The Selection Panel cautions the project team to maintain an open mind on what factors are controlling methylation of mercury as they carry out their investigations. Additionally, it was felt that the project should examine light penetration and photodemethylation.

CALFED Ecosystem Restoration Program

External Scientific Review Form

Proposal Number: 014

Proposal Title: Wetland and Rice Management to Limit Methylmercury Production and Export

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 goal of this work is very clearly stated and is very well linked to the ERP goals. Methylmercury remains a very significant problem in the California Bay-Delta environment, and with the emplacement of a TMDL by the State of California for methylmercury fluxes to the Delta, there is an urgent need for science that can help land and water resource managers avoid exceeding regulated loads. The goal of this work dovetails nicely with the needs of BLM, and other resource management and stakeholders, and if successful should help to reduce methylmercury production from Cosumnes watershed wetland and rice fields. For years mercury researchers have been working to understand the mechanisms and triggers of methylation, with a significantly improved understanding now in hand, we can begin to apply this knowledge to practical purposes, such as that proposed here.

   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:

The approach used by the research team is very clearly laid out by the PI, and in my opinion will be successful in achieving the overall goal and objectives. All too often resource managers and researchers alike point solely at the mercury source when trying to achieve lower mercury and methylmercury exposure levels to local wildlife, fish and humans. However, achieving this goal by seeking to reduce or eliminate the mercury source is often not practical or realistic. This approach has been proposed and attempted in the Bay-Delta with limited success, and proposed alternative approaches like that of this PI have great promise. Mercury methylation has MANY triggers and causal factors, and generally limiting any one of them will significantly reduce methylmercury production in a significant way. This proposal is attacking this problem through the "labile carbon" limitation route, and if successful will be valuable scientific information not only for Bay-Delta managers, but resource managers more generally.

Rating: Excellent.

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 methods are quite straight forward, and have been used in the mercury research field generally, and in the Bay-Delta as well. That Dr. Windham-Myers is proposing to expand an already existing and much larger field research study makes this proposed work all the more feasible. If she were proposing to do everything stated in this proposal in isolation of collaborators I would have had doubts about the feasibility. As proposed, the number of field trips and numbers of samples are quite reasonable. I am not aware of any permits, infrastructure, etc… that this proposal would require that the existing EPA funded effort has not already provided or will provide.

Rating: Excellent

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 PI is making use of what I believe is a previously existing conceptual model that was developed through numerous research projects over many years (Alpers et al., 208): DRERIP and DRERIP-MCM. I don't believe there was any requirement to independently develop a new or completely original model for this RFP, so I do not believe this is a problem. I do have a few "picky" items I would like to raise about this "model", but I don't believe this PI is necessarily responsible to make the changes to a model that she is but one of many contributors to. First, while methylation is treated in complete fashion, demethylation is not. A simple, an likely, scenario of differences in UV light penetration in the experimental treatment plots could cause

observed differences in methylmercury concentration, an have nothing to do with the labile carbon triggers. Second, this model clearly indicates that "reactive Hg" (an operationally defined fraction of total inorganic mercury) is the precursor to methylmercury. Frankly, the literature supporting this supposition is very limited, and very few researchers outside the USGS Menlo group subscribe to it. As such, this model would be better if a more fair treatment of the bioavailable fraction of mercury for methylation were recognized: and that is, we really don't know what it is yet! Last, I would prefer to see a conceptual model that more clearly shows other limiting substrates may in fact control mercury methylation other than labile carbon. Factors like sulfate, ferric iron, nitrate, selenate/selenide, etc… all could play key roles and should be more carefully included in the "model".

Rating: Very 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:

Yes, the project clearly states that they will estimate the mass of reduced methylmercury export due to the various experimental treatments. Several hypotheses are stated in the proposal and the experimental approach will clearly allow for the testing of each. The PI has a good record for reporting and publishing results such that potential future studies will be able to extend these results

Rating: Excellent

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 would expect the outcome of this work will lead to at least two peer reviewed journal papers and a final project report – as well as the quarterly updates to the funding agency. In addition, Dr. Windham-Myers is a very good speaker and will no doubt bring her results to the annual mercury meetings for CALFED an other local and national meetings.

Rating: Excellent

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:

One of the real significant strengths of this proposed work is that the PI is proposing to add to an already existing, and much larger, project. This work fits nicely with that other project, but is

not duplicative, and should provide a more mechanistic view of methylation on the Cosumnes River watershed, and the Bay-Delta generally.

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:

Dr. Windham-Myers is a highly trained botanist, ecologist, and biogeochemist. In the past 5 years or so she has gained significant experience in the mercury research field and has authored or coauthored several reports on research conducted in the Bay-Delta environment. That she has botany training makes her well suited to attack mercury methylation as she does in this proposal: through examining labile carbon and primary productivity pathways. Her strengths are complimentary to the EPA funded research team, so I expect her presence will provide added benefit to that project as well.

Rating: Excellent

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:

Were this project to be conducted in isolation, the budget would be no where near what is needed to execute a project of this scale. However, because she is essentially extending an existing and larger project, the net result is a big bang for the buck project.

Rating: Excellent.

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: Superior

Please provide a brief explanation of your summary rating:

Overall, this is an excellent project proposal, the kind that the Delta Tributaries Council has needed to see for years an has been slow to come. It is time to start using the considerable knowledge gained by significant amount of research done in the Bay-Delta on mercury over the past 10-15 years. Focus to date from my external (many miles away) vantage point is that possible improvements to the situation regarding methylmercury exposure in the Delta have been myopically focused on just reducing mercury loading. Such a narrow minded approach limits the multitude of ways that reduced methylmercury production, export and exposure can be achieved. Proposals such as this one are gratifying to see, an I hope more work like this will be forthcoming.

CALFED Ecosystem Restoration Program

External Scientific Review Form

Proposal Number: 014

Proposal Title: Wetland and Rice Management to Limit Methylmercury Production and Export

Reviewer: #2

Conflict of Interest Statements:

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

- Correct XXX
- 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:

   This project will enhance a funded EPA 319 project that is manipulating wetlands and rice paddies to study the impact on methylmercury (MeHg) export in the Central Valley. Specifically, the proposal seeks to deliver process-based information focusing on labile organic carbon and mercury bioavailability on why the varying management techniques do or do not lead to a reduction in MeHg export The proposal is very specific in this regard and consistent throughout the narrative. The proposal specifically cites the key ERP goals that are addressed.

   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:

   The study design is clear and the methods are explicitly explained. The field manipulations that the authors discuss are already beginning to take place as part of the EPA 319 study and the proposed study will enhance that study with process-based measurements in 6 different treatments at three key times during the year. Each treatment has three replicates resulting in 18 treatment fields. The tasks and measurements in each treatment field are clearly delineated in the proposal and are likely to meet the objectives of the proposal. Field studies always have inherent risk involved and variables such as rainfall amount, timing of rainfall, and inter- and intra-annual temperature variability are some of the factors that can overwhelm observed changes relative to the manipulations. However, the authors have designed a feasible study that will likely add to significantly to the knowledge base on Hg/MeHg dynamics in wetland systems. The authors are well-respected in the Hg research community and have a record of producing scientific publications that have moved the field forward.

   Rating: Excellent

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 sampling scheme is entirely feasible, well-documented, the project personnel are appropriate and have completed projects of this scope in the past. The proposal clearly addresses environmental compliance and permitting and noted that the requirements are already in place given that the 319 project is just beginning. The scale of the project is ambitious, but consistent with the requirements for a project seeking to estimate changes in MeHg from a large,

heterogeneous system. The only concern may be with the evapoconcentration estimates. Are the water budgets of the fields constrained enough (e.g. losses to groundwater, direct runoff to the fields) to estimate evapoconcentration using Cl and is Cl conservative in the treatment fields? Using halides as tracers in other field studies have been tenuous; and while it is an easy and cheap metric to evaluate, I am doubtful it will yield the desired outcome. A well-constrained water budget seems a better approach, but is more complex. This is a relatively minor concern.

Rating: Excellent

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:

This aspect is a real strength of the proposal. The field manipulations are clearly supported with an underlying conceptual model that has already been developed, but will be informed by the field studies. The proposal seeks to validate the DRERIP-MCM model by examining key factors controlling mercury methylation in the system.

Rating: Excellent

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:

Yes, these are very clear in the proposal. This proposal was written specifically to enhance the EPA 319 funding in this regard. The 319 funding provides for the manipulations and this funding allows for more detailed mechanistic studies which will allow the results to be applied to other systems.

Rating: Excellent

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:

Yes, products of value will be produced by this study. The authors have produced valuable results in the past and the experimental design and sampling scheme of this project is sure to result in valuable mechanistic understanding for the Hg scientific community. The conceptual model has already been published and is readily available on-line.

Rating: Excellent

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 proposed work and specifically the field manipulations on this scale are invaluable to the understanding of mercury dynamics in the environment and represent a new contribution to the field of mercury research. Large-scale manipulations and field studies have validated (or invalidated) previous mercury cycling models and smaller scale laboratory experiments. It is large field studies like this one that have the potential to significantly enhance and constrain our understanding of mercury cycling in the environment.

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:

I have discussed these questions in previous answers and this team of researchers is clearly the best available to do the work in this region as they have a long history of doing high quality work in this region as well as strong publication records.

Rating: Excellent

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:

Leveraging the EPA funds is an excellent strategy and enhances the benefit of this proposal. The manipulations are expensive and the additional sampling proposed by the authors is a value add-on. The main cost in the budget is personnel to collect and process the samples, but considering the large number of treatment sites and the significant number of samples to be collected, I find the budget to be reasonable. I did not see a specific line for sample costs, and I assume these are included in the personnel costs and I note that the total operating costs listed are not consistent with the listed operating costs, but the subtotal adds up to the correct value.

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: Superior

Please provide a brief explanation of your summary rating:

This proposal was a real pleasure to review and is a very strong proposal considering the leveraging of 319 funds, the technical quality of the proposal, the qualifications of the authors, and the strong potential for significant advancement in scientific understanding in the field of environmental mercury research. In particular, the field is in need of ecosystem-based studies that contribute to mechanistic understanding of the important factors controlling mercury methylation. The proposed study is ideal in this regard.

CALFED Ecosystem Restoration Program

External Scientific Review Form

Proposal Title: 014

Proposal Title: Wetland and Rice Management to Limit Methylmercury Production and Export

Reviewer: #3

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 proposed work seeks to explore the physical processes that control Hg methylation in managed wetlands and rice fields in the Central Valley. The problem is very clearly described in Section 6 of the proposal document. A critically important aspect of this proposal is that the principle activities of the overall effort described are funded through a USEPA proposal. It is important to differentiate between the work that is funded by the EPA grant, and the outcomes that are proposed for the ERP funding. The EPA funded project specifically seeks to implement modifications to land-use management practices across wetland types to reduce MeHg production and export. The ERP proposed work focuses on specific mechanistic aspects of the management modifications (Hypotheses 1-5). These are explicitly linked to the DRERIP conceptual model developed specifically for CALFED-ERP and are critical to developing scientifically defensible **explanations** for observed changes as a result of the land use management modifications, in my opinion.

The proposal is differentiable from other work that I am aware of on wetland/rice fields that are supported through the ERP because of the scientific approach. The specific goals associated with the detailed investigation of labile carbon effects, sediment characteristics, and MeHg production are scientifically outside of the scope of other ongoing work, and have the potential to generate more transferrable knowledge than other mass-balance based investigations.

My only guidance for the investigators and project managers is to ensure that there is both clear conceptual and budgetary clarity in distinguishing between work executed on the EPA and ERP funded aspects of the project.

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:

The proposal clearly describes the approach outlined in Section 3. The control and experimental wetlands/rice field design is ambitious and scientifically well thought through. The EPA funded outcomes will reveal whether land management changes will result in a decrease in MeHg yield and exposure to organisms. The additional tasks proposed to be

supported by ERP funds (Tasks 1-8; pp 13-14) are explicitly described and coupled to the project hypotheses. My only recommendations are for a more thorough description of sediment variability (Task 3) including complete grain size distribution (or at least sand/silt/clay using gravimetric approach) that would be more informative than just %silt. Also, despite its wide use, Loss-on-ignition soil organic matter should be at least validated with a direct measure of soil carbon using a CNS analyzer or similar to ensure that there is no artifactual mass loss of hygroscopic water on clay or carbonate mineral degradation. Further, it would be beneficial to have some indication of the schedule of the measurement of physical parameters such as sediment temperature, redox etc. Are these one time only within the sampling windows, or can continuous measures of things like temperature be implemented? Continuous sediment and water temperature would be easily and inexpensively done (http://www.onsetcomp.com/products/data-loggers/utbi-001).

The proposed work will contribute to the knowledge base in two main ways: 1) the generation of mechanistic information on the processes that govern MeHg production in these systems, and specifically address the issue of labile carbon availability, which is a

Rating: **EXCELLENT**

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:

Since the EPA funded work that established the basic experimental framework is set to begin now, it is clear that the major constraints to the proposed work (land access, manipulation strategies, timeline coordination, etc) have been fully addressed. As such, this supplemental work is completely feasible within the scope of the proposal. The field sampling and analytical commitments are modest (relative to the scope of the overall work). Compliance and permitting is addressed, in that permitting is in place that fulfills the requirements for EPA CWA 319(h).

Rating: **EXCELLENT**

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:

This is the strongest aspect of a strong proposal. The explicit coupling to the DRERIP-MCM framework that Alpers was charged with formulating in an effort to synthesize and focus future research efforts is commendable. In fact, instead of formulating completely new conceptual models on a project-by-project basis, all process-based work funded through the ERP should be expected to specifically couple to DRERIP-MCM for several reasons. First, it ensures that the knowledge gaps that have been identified for the region as a whole are being systematically identified. Second, it allows the agency to ensure that current and future work is complementary and not redundant. The above comments rely on the presumption that the DRERIP-MCM

framework has merit – it is my opinion that it is a thorough and valid conceptual model for the area and processes of concern.

Rating: **EXCELLENT**

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:

Performance evaluation is addressed in Section 7 (p. 17) and will result in clear evidence of project performance over the course of the work. Data generated will follow SWAMP protocols and will thus integrate into existing databases and be available to future studies or restoration projects.

Rating: **EXCELLENT**

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:

Products of value are highly likely from the project. Specific guidance on practical land use and water management practices will be developed. Quantitative data on labile carbon controls on rice field Hg methylation, controls on variability in observed data, and evapoconcentration of MeHg are all important, and individually valuable components of the project.

Rating: **EXCELLENT**

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 proposed work builds on the work of a previous project led by the same principal investigator. The work logically builds on this previous work and capitalizes on knowledge and data generated from that work. It would not be considered a duplication of previous work. The proposed work should be considered along with the current ERP funded project of Stephenson, Foe, Heim et al., since both in principle have emerged from the previously funded project of Windham-Myers et al. This other focuses on the role of hydrodynamics of 'finishing' wetlands, photodemethylation and other processes and should be considered complimentary to this proposed work. I have no concern that they work is duplicated, however there should be ongoing dialogue between the two projects such that 'objective creep' does not result in replication of effort. The presence of Stephenson and Heim on this project should ensure a complementary effort.

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:

   There is no question as to the qualifications of the applicants to complete the work, given the strong outcomes of previously-funded CALFED-ERP projects, and related work. The infrastructure and labs are well proven, and the workers all have deep knowledge of the Bay-Delta system.

   Rating: **EXCELLENT**

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 perfectly reasonable for the work proposed. In fact, the investigators should be congratulated on capturing significant external funding to support ERP centred initiatives, and the ERP should be grateful to the principal investigators for their efforts in garnering external support. The work would have otherwise cost the program several million dollars to initiate.

   Rating: **EXCELLENT**

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: **SUPERIOR**

Please provide a brief explanation of your summary rating:

The work proposes to leverage a significant EPA funded program for additional funding through the ERP program that will be used to generate process-based information on controls on MeHg cycling in wetlands/rice fields. The focus on labile carbon availability builds on previous ERP-funded work, and provides a direct coupling to a conceptual model that has been developed for the region. The amount of funding requested is small, relative to the significant and large direct and indirect contributions to the ERP dataset and scientific mission that are expected from the proposed work.