Panel Scientific and Technical Review Form
(Note: Review comments will be anonymous, but public.)

Proposal number: 2001-E207 **Short Proposal Title: Delta Tules: Assessment of Restoration Opportunities**

Note: Only one individual review of this proposal was received. The summary of reviewer comments is that of the one review received.

1a) Are the objectives and hypotheses clearly stated?

Summary of Reviewer comments:
The objectives and hypotheses are not stated clearly and the proposal was difficult to follow. Their order appears illogical.

Panel Summary:
The objectives and hypotheses are clearly stated. Both appear too numerous and broad in scope at this stage of understanding of tule habitat relationships with salmonids and macroinvertebrates. Before tule habitat inventory work at the landscape level is begun, it is critical that habitat relationships are understood.

1b1) Does the conceptual model clearly explain the underlying basis for the proposed work?

Summary of Reviewer comments:
No. If the conceptual model is a GIS model depicting restoration sites with high potential for tule marsh restoration, there is no actual description of specific GIS steps that will be taken to define the model. The parameters of the model are not discussed specifically as parameters. The GIS model is described in two sentences which I find inadequate to the task to clearly explain the model and the underlying basis for the proposed work.

Panel Summary:
The objectives and hypotheses cover multiple scales of ecological/biological organization from physical/habitat controls on tule growth and development to landscape/ecosystem-level processes and disturbances controlling existing and potentially restorable sites for tule communities including supported salmonids and macroinvertebrates. Making predictions with so many uncertainties from the individual tule plant, to population and community dynamics of habitat elements and salmonid use, to landscape processes, and finally gross remote sensing predictors does not lend confidence to the panel that

accurate predictions are possible. The conceptual model does not adequately indicate the predicted or expected ecological responses to project actions.

1b2) Is the approach well designed and appropriate for meeting the objectives of the project?

Summary of Reviewer comments:
No. With respect to tule habitat mapping, the authors appear to be limiting their proposed restoration effort even before they have gathered the data on current tule distribution based on what 'appears' to be present rather than any definitive work. Yet one of their goals is to map tule habitat and base their restoration efforts on those maps. The handling of remotely sensed coverage information is not adequately described. For example, cover classes are not defined; different agencies use different cover classes. Whose data will be used? What state, federal or private agency will be providing this data? If the authors will be determining their own cover classes, what software will be used to determine the cover classes? ArcView GIS is the software being used for mapping the layers, but ArcView GIS is not the correct software to determine land/habitat feature classification. If they are buying geospatial layers that have a land classification system already, they should spell out whose system is being used and why they chose that system. These aspects of the project have not been identified. No statement is given about what acceptable error will be used in acquiring data. How will the authors determine what is the best data for their project?

If historical photography and new aerial photography is planned for GIS analysis, much more detail on methodology needs to be defined in this proposal. I cannot see how their budget will be able to cover the costs of this trend analysis as well as all the other aspects of this proposal.

The tule restoration study hypotheses is timely but not clearly spelled out. An experimental design matrix would be helpful, i.e., number of plots, specific water depths as fixed effects , bank slope specifics (rather than 'gradual (what's 'gradual' mean?) vs. steep'.

There are more objectives embedded in the proposal than are stated on page 2 (like the habitat trends analysis over time) as goals of the study. This is confusing and needs to be better presented.

Panel Summary:
Overall, no. There is insufficient description of tule experimental design. The fisheries research approach is too simplistic and vague. The panel concurs with the technical reviewer comments. Again, the panel feels the proposal underestimates the complexity of the proposed work. The panel doubts that aerial photography can detect remnant tule stands of lower density, and expecting to relate plant density information to water depth, etc. is thought to be exceedingly challenging given the scale and uncertainty of salmonid migration routes in the Delta. The remote sensing based GIS modeling for change detection appears to be based on too optimistic assumptions on historical photography quality/coverage, substrate condition, and change prediction capability. There is no description of the

treatments and controls in this proposal. No evidence is presented that the proposed macroinvertebrate sampling method is proven and accepted; references should be provided; one minute sampling intervals for macroinvertebrates is likely too short. The term morphospecies in invertebrate section is not defined.

1c1) Has the applicant justified the selection of research, pilot or demonstration project, or a full-scale implementation project?

Summary of Reviewer comments:
No. The proposals clearly states on page 2 that 'Little if any scientific information exists regarding life history strategies, ecological requirements, or habitat value of the tule species ....' HART goes on to state that its team is currently involved with a number of pilot tule restoration projects in the Delta. 'Through these efforts, we have become aware of various factors associated with restoration success.' What are they? The findings from the ongoing studies should be given in this proposal as an appendix. It is difficult to review a design proposal or pilot project if research findings are not made available.

Panel Summary:
The ongoing research concerning tule restoration and habitat requirements as well as salmonid use needs to be established before landscape-scale prescribed restoration pilot/demo assessments can be justified. Without a sound basis for doing that work, the effort could be for naught. The applicant states this is a pilot/demonstration project, but it is mostly a research/monitoring project.

1c2) Is the project likely to generate information that can be used to inform future decision making?

Summary of Reviewer comments:
It is unlikely unless the limitations described above are dealt with in a thorough manner. Peer-reviewed journal presentation of the ongoing research findings could serve as a basis for further research along the lines described in the proposal.

Panel Summary:
The panel feels that in-progress findings should be presented to justify further investigation of related topics. However, because the proposal is poorly written one panelist was concerned about the ability to produce a project report that clearly explained the information generated.

2a) Are the monitoring and information assessment plans adequate to assess the outcome of the project?

Summary of Reviewer comments:
No. Only one paragraph of general description of the assessment plans is give. It is not a well-defined description of procedures for monitoring. How is it going to be monitored isn't answered.

Panel summary:
Generally, no. As presented above (see 1b2 comments), the tule experiments, and fisheries and macroinvertebrate monitoring and assessments are inadequately described and therefore lacking. This section of the proposal is confusing in that it implies the monitoring plan is still to be completed ("The completed restoration/monitoring plan will address..."), yet this is primarily a research/monitoring project.

2b) Are data collection, data management, data analysis, and reporting plans well-described, scientifically sound and adequate to meet the proposed objectives?

Summary of Reviewer comments:
No.

Panel Summary:
Data collection: Generally not well-described, adequate, or scientifically sound. See comments under 1b2.
Very little detail is given on data handling, not enough to judge scientific soundness or adequacy.

3) Is the proposed work likely to be technically feasible?

Summary of Reviewer Comments:
Yes, I think it is technically feasible, but not as written. Too many goals embedded in the project and not clearly stated or defined.

Panel Summary:
Additional research findings will need to be gathered before sound judgements can be made concerning the feasibility of the proposed work. As presented several aspects are not technical feasible. The overall approach appears very interesting, but much research-level work needs to be done beforehand. The proposal contains some serious flaws in both approach and methodology that raise questions as to whether or not this proposal will be successful.

4) Is the proposed project team qualified to efficiently and effectively implement the proposed project?

Summary of Reviewer comments:
Yes, I think the proposed team members are qualified.

***Panel Summary*:**
The team appears qualified. However, the poorly written aspects of the proposal caused one panelist to question applicant qualifications related to reporting and data summarization.

5)Other comments

None.

INDIVIDUAL REVIEWER OVERALL EVALUATION SUMMARY RATING AND COMMENTS:

POOR. Proposal is poorly written, has too many goals embedded within it, and not enough explanation of procedures.

Overall Evaluation
PANEL SUMMARY COMMENTS

Although the applicant proposes some good ideas to investigate, the approach is inadequate to effectively test these hypotheses. The GIS approach is not sound. There is insufficient description of the tule experiments. The fisheries sampling, the "experimental design" and methods, is vague and inadequately described. No evidence is presented that the macroinvertebrate sampling methods are proven or accepted, and thus may not be scientifically sound. There are inadequacies in the conceptual model. The proposal is poorly written.

OVERALL PANEL EVALUATION SUMMARY RATING: POOR