Panel Scientific and Technical Review Form

(Note: Review comments will be anonymous, but public.)

Proposal number:2001-J-202 **Short Proposal Title:** Propagation/Establishment Techniques for Special Status Plant Species

Summary: Bitterroot is proposing a three-year Phase 1 study to develop some knowledge and technology to assist habitat restoration for Delta at-risk species. The proposal especially focuses on seed germination requirements, propagation techniques, and establishment methods for 5 rare plants in the Delta, *Lathyrus jepsonii* var. *jepsonii*, *Lilaeopsis masonii*, *Limosella subulata*, *Aster lentus*, and *Hibiscus lasiocarpus*.

1a) Are the objectives and hypotheses clearly stated?

Summary of Reviewers comments:

Yes. Their objectives are: 1) To develop an understanding of the seed physiology and germination requirements for each target species, 2) to develop efficient methods for propagating each target species, 3) to develop efficient methods for establishing plants in the field, and 4) to conduct research regarding community structure and function of the required habitat for each target species.

Panel Summary:

Their objectives were pretty clear.

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

Summary of Reviewers comments:

Yes. They propose to gather information on the germination, propagation and establishment of 5 plants from Delta habitats in order to facilitate restoration efforts. They also claim the habitat should be studied as to community structure and function, but the proposed research barely addresses this topic.

They propose to conduct the research over 3 years; most work occurs in years 1-2; monitoring occurs in year 3.

Panel Summary:

Reviewers felt that the conceptual model left out several important issues 1)How will the results of the research on community structure and function be used for restoration of special status species populations? 2)What four different habitat types (as noted in Statement of the Problem section) will be investigated and why? 3)The basis for the choice of plot treatments is not provided.

Furthermore, issues of habitat loss, hydrological regulation in the Delta, and lack of species diversity in restoration projects are very generally discussed, with little depth.

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

Summary of Reviewers comments:

No, the approach is not well-designed nor appropriate for meeting the objectives.

The focus is on seed germination and two of the plants (*Limosella subulata* and *Lilaeopsis masonii*) reproduce mostly or entirely by rhizomes. Thus, the proposal spends a lot of space explaining how detailed information on seed germination and establishment will suit our needs, when actually two of the rarest plants don't even reproduce this way in general.

The study design is not appropriate either, since they propose plots 5m x 5m in size (six of them), which seems strange when most of the plants occur in nature in more linear-shaped areas, especially near the tidal edge for *Lilaeopsis* and *Limosella*.

Panel summary:

The ANOVA test is unsuitable for analyzing the mortality data.

The discussion of "ecosystem benefits" in the proposal are unjustified; for example the suggestion that anadromous fish would have increased survival following their work is a stretch.

There is no "adaptive management" strategy to the proposal, and the habitat calls for this aspect; for instance, erosion is known to be a serious concern in restoring *Lilaeopsis* and *Limosella*, and this potential danger to the experimental plots was not discussed.

Also, this proposal appears to ignore or be unaware of previous research on these 5 plants, so it does a poor job of building on past work. It is especially redundant with respect to DWR's (McCarten's) work to transplant *Lilaeopsis masonii* at Barker Slough and with Fiedler and Zebell's ecological work on *Lilaeopsis masonii.:*

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

Summary of Reviewers comments:

Phase 1 proposed here consists of research and a pilot project to transplant the Delta plants to experimental plots on DFG lands. If no previous work was known for these plants in this habitat, the work could be justified, but it is very disturbing that the proposal does not build on past work.

Panel Summary:

The proposal is not specific enough to determine whether or not the work is justified. The proposal lacks the ecological foundation acknowledging specific habitat features such as tidal elevation and tidal flux, and other known conditions.

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

Summary of Reviewers comments:

There could be some information generated but much will repeat past research. They propose evaluating the soils; this will contribute little to our knowledge of these plants in this reviewer's opinion. We already have some information from the DWR study regarding the transplantation of *Lilaeopsis masonii*. We already have quite a bit of ecological knowledge about LIMA from the Fiedler studies as well. The knowledge about propagating *Hibiscus*, *Lathyrus*, and *Aster* could be useful. However, this reviewer feels that the propagation of these three species may not be very difficult, and therefore a $150,000 study may not be justified.

In general, this reviewer feels that money would be better spent on research to study practical techniques to reduce the threat of erosion (primarily from boat wakes) to tidal habitats (habitats for *Limosella* and *Lilaeopsis*), since this is a major threat to continued existence of these plants.

Panel Summary:

No additional comments.

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

Summary of Reviewers comments:

Actually, the monitoring plans are not well outlined in the proposal, so it's hard to know. They propose monitoring pilot plantings for 2 years, which is of course inadequate to evaluate success by itself. However, there is hope on the applicant's part that future Phases would also be funded and these phases would have a monitoring component too.

Panel Summary:

All reviewers agreed that the monitoring is inadequately described and what is described is unrealistic. The plot sizes are probably too large. Counting individuals of *Lilaeopsis* and *Limosella* is not feasible. These plants reproduce by runners and one cannot identify single individuals easily.

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

Summary of Reviewers comments:

Somewhat "yes", with the exception of:

1) monitoring not well-described

2) their stated goal of "...would allow interested parties the opportunity to recover special status species to population levels sufficient to **delist** target species" (emphasis mine) is not feasible. First of all, only one taxon in the proposal is listed: *Lilaeopsis masonii*, CA Rare. Transplantation of this species has already been attempted by DWR contractors and although the monitoring indicated initial success, after 10 years, the plants mostly disappeared from erosion. Therefore, it is not reasonable to state that the knowledge gained from this or any other transplantation study would allow delisting of a listed plant.

Panel Summary:

Proposed data collection does not reflect the habitat and life history characteristics of the species. Data collection techniques for Task 2 are not sufficiently detailed and there is no discussion of how the information gathered in this task would be used in later phases.

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

Summary of Reviewers comments:

Some of the work is technically feasible. However, seed propagation of *Limosella* and *Lilaeopsis* is probably not feasible. Bitterroot says that, for taxa for whom seed is inappropriate, vegetative parts would be used. However, all of the other parts of the proposal focus on seed germination, and propagation from seed.

Also, the size of the experimental plots is too large (6 times 5x5m). This size and shape is inappropriate for LIMA and LISU since these plants occur in linear populations in the tidal zone. Furthermore, no control plots in the tidal zone exist of this size and shape.

Establishment plots include several herbivory exclosures; these are probably unnecessary and this design further demonstrates a lack of knowledge of these plants and their habitat.

Panel Summary:

Most reviewers felt only the seed germination studies on the *Lathyrus* and the *Hibiscus*, and maybe the *Aster* were feasible as lab studies. The field investigations have only marginal chance of success.

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

Summary of Reviewers comments:

The team comes from the northern Great Plains area and has no prior experience in the Delta. Their proposal shows a lack of understanding for this habitat and for these species.

Furthermore, it is not encouraging that they did not do even a minimum literature review before submitting the proposal.

Panel Summary:

No one felt the team established their qualifications for this study. The conditions in the Delta and the specific habitat parameters of the plants they propose to study are quite different from their described expertise.

5)Other comments

Three of the plants in their proposal, *Lathyrus*, *Aster*, and *Hibiscus* are likely to be quite easy to recover as are most of the other more common Delta species. I am not convinced that we need a detailed germination and propagation study to show this.

For the two remaining plants, *Lilaeopsis* and *Limosella*, what is needed is not studies on how to transplant them, but how to maintain them in habitat over time with mixed use of the Delta, including boat use. Erosion is a big problem for these tidal species; establishment is not.

Panel Summary:

This proposal does not propose to find out what we need to know for these species; we need critical habitat requirements, not the knowledge on germination and propagation for these particular species.

The comment in the proposal that, in California Dec-Jan constitutes the "pre-rainy" season is very unsettling.

Had the proposal identified a habitat-based restoration approach and identified the specific habitat needs of the special status plants, they would have had a better proposal. There was a clear lack of foundation regarding the ecology of the Delta and the special status plants. Many specific statements and proposed methods indicated this lack of knowledge.

The applicants should contact the state and federal agencies to obtain appropriate documents and literature on the species. The references cited in their proposal have little to do with the ecology of Delta plants.

Overall Evaluation Summary Rating	**PANEL SUMMARY COMMENTS [include the consensus conclusions by the panel including the strengths and weaknesses of the proposal]**
☐ Excellent ☐ Very Good ☐ Good ☐ Fair ☐ Poor **XXX**	

The rank of Poor was nearly unanimous with one Fair-Poor suggested. The only positive comment stated was that it would be nice to learn how to propagate rare plants. This proposal demonstrates a lack of understanding of the habitat and the species concerned.

Strengths: -the proposal is one of the few dealing with rare plants in the Delta ecosystem
-some of the information on germination and propagation on *Lathyrus* and *Hibiscus* could be useful

Weaknesses: -the authors did not build upon past work; they do not know the ecological system, nor do they understand the plants
-the experimental design is flawed
-the monitoring is deficient
-parts of the study are not feasible and other parts are not useful