Proposal Reviews

#16: POPULATION BIOLOGY AND GENETICS, HABITAT MODELING, AND PILOT RESTORATION FOR THREE CALFED AT-RISK PLANT SPECIES IN THE SACRAMENTO-SAN JOAQUIN DELTA AND SUISUN MARSH

California Department of Fish and Game

Research and Restoration Technical Panel Review	
Bay Regional Review	
Delta Regional Review	
External Scientific Review	#1 #2 #3 #4
Prior Performance/Next Phase Funding	
Environmental Compliance	
Budget	

Research and Restoration Technical Panel Review:

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

Proposal Number: 16

Applicant Organization: California Department of Fish and Game

Proposal Title: POPULATION BIOLOGY AND GENETICS, HABITAT MODELING, AND PILOT RESTORATION FOR THREE CALFED AT-RISK PLANT SPECIES IN THE SACRAMENTO-SAN JOAQUIN DELTA AND SUISUN MARSH

Review:

Please provide an overall evaluation summary rating:

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

Overall Evaluation Summary Rating	**Provide a brief explanation of your summary rating**
-Superior **-**Above average **-**Adequate **X**Not recommended	**Although this ambitious proposal aims high, it falls far short of actually describing how the large number of separate studies described will be logistically and conceptually integrated to address the primary goals of the project. There are also real problems with the justification, design, and interpretation of the separate projects. In particular, the population viability and habitat models are only vaguely described and the methods for obtaining model parameter values are very inadequate. The justification and sampling design for a very large and expensive analysis of microsatellite genetic markers is also inadequate. Finally, the primary field experiment focusing on local adaptation is designed incorrectly so that it cannot be used to develop management recommendations for translocation in these species.**

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

   There was general consensus that the broad goals of this project were timely and had significant merit. Unfortunately, however, most reviewers felt that the integration of the large number of separate research projects into the larger goals of the study was very poorly described. The applicants did not explain how they would integrate the data, nor did they provide a conceptual basis for understanding how one might integrate the information. Further, there was a generally weak presentation of current knowledge of these species and how the development of habitat and population viability models would be used to foster

conservation and restoration of these species.

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

   The likelihood of success of this project to reach its larger goals would appear to be low. As noted above, there is very little description of how the large number of separate studies will be coordinated and how the results from all these studies will be effectively synthesized. At another level, there were significant concerns about the studies themselves ranging from poorly described and poorly parameterized population viability models to largely unjustified molecular analyses to significant flaws in experimental design. It is not clear that the research team is qualified to complete this very large project and because of the problems mentioned above, it is problematic whether the performance measures proposed will be valid indicators of success.

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

   Given the problems with project integration, it is unlikely that the project will be able to address the primary goals that would make an important contribution. In particular, problems with the restoration experiments in terms of design and interpretation will significantly reduce their value for management decisions.

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

   In general, the budget was thought to be high for comparable type projects and was not well justified in terms of tasks performed by the consultants.

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

   Although the Bay Regional panel ranked the proposal High, it had some significant concerns that the proposal did not address known threats and management needs of the most endangered of the species covered, Suisun thistle. The rather generic approach of the proposal was interpreted to indicate a lack of strong coordination with local wetland managers and limited linkage to other state and regional applied research efforts on these species. The Bay Regional panel also felt that the large allocation to genetic analyses was not adequately justified relative to demographic causes of decline. The Delta Regional panel ranked the proposal Medium. The Delta panel also felt the proposal was poorly linked to ongoing efforts by agency and academic researchers on these species. In addition, the panel questioned the value of the translocation experiments in the Lilaeopsis habitat because the transient nature of this habtat type makes the long term benefit of translocation questionable.

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

There were no significant concerns regarding prior performance or budget issues. There was an issue regarding environmental compliance because of the proposed creation of tidally influenced channels as part of the restoration experiment. If the proposal is funded the applicants will need to obtain 1600 Agreement which will take 2 months to complete and either $154 or $772.75 in fees depending on cost of channel creation activities.

Miscellaneous comments:

None

Bay Regional Review:

Proposal Number: 16

Applicant Organization: California Department of Fish and Game

Proposal Title: POPULATION BIOLOGY AND GENETICS, HABITAT MODELING, AND PILOT RESTORATION FOR THREE CALFED AT-RISK PLANT SPECIES IN THE SACRAMENTO-SAN JOAQUIN DELTA AND SUISUN MARSH

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

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

x

1. Is the project feasible based on local constraints?

   XYes **-**No

   How?

   The methods and geographic scope of the survey are feasible. Relevant population and molecular genetic methods are available.

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

   XYes **-**No

   How?

   The species selection is not unreasonable based on CALFED priorities (MR-6: Ensure recover of at-risk species by developing conceptual understanding and models that cross multiple regions)

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

   -Yes **X**No

   How?

   The proposal does not focus on the most urgent specific priorities for addressing known specific threats and management needs of the most endangered of the species covered, Suisun thistle. Instead, it casts an unduly broad and basic scope of study over all species covered. This indicates lack of strong coordination with local wetland managers. The allocation of study resources on genetic studies is not adequately justified without preliminary assessment of the relative importance of demographic and genetic contribution to causes of decline, or constraints on recovery. The study scope appears to be generic and programmatic, rather than species-specific.

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

    -Yes **X**No

    How?

    There is limited linkage to other state and regional applied research on relevant native wetland thistle species in academic and natural resource institutions.

Other Comments:

Despite the inclusion of a species in urgent need of focused applied study, this proposal addresses too broad and diffuse a scope of research, with insufficient priority to applied research on the known principal threats and recovery needs. The experimental reintroduction components do have merit, but the feasibility of study sites needs more development.

Delta Regional Review:

Proposal Number: 16

Proposal Title: POPULATION BIOLOGY AND GENETICS, HABITAT MODELING, AND PILOT RESTORATION FOR THREE CALFED AT-RISK PLANT SPECIES IN THE SACRAMENTO-SAN JOAQUIN DELTA AND SUISUN MARSH

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

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

Work on these species is needed. The life history investigations proposed here would be useful, but need to include better outreach to others studying these species and their habitats.

1. Is the project feasible based on local constraints?

   XYes **-**No

   How?

   Yes on life history portion, Conditional no on translocation; Past efforts with translocation of Masons Lilaeopsis in the Delta have been difficult potentially due to the nature of the species rather than local constraints. Collecting life history information to develop habitat models should have no local constraints since many existing species population locations are already known and relatively accessible.

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

   XYes **-**No

   How?

   The transitory nature of Lilaeopsis habitat makes the long term benefit of translocation of populations questionable. This project would provide information that would help in defining conditions for restoring habitat that would benefit one or more at risk species.

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

   -Yes **X**No

   How?

   Weak linkage; limited work being done in this type of habitat. Would like to see stronger interaction between project proponents and other botanists who have worked on these species. Additional linkage should be made with experts on other species found in this habitat for a potentially broader perspective in approaching this work.

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

   XYes **-**No

   How?

   Translocation areas will be on DFG property. However, Rush Ranch Conservancy is not mentioned as a restoration area for Suisun Thistle. Because of their property ownership with appropriate habitat, a restoration project for Suisun Thistle should involve them.

Other Comments:

Previous work on these plants should be reviewed by technical panel.

External Scientific: #1

Research and Restoration External Scientific Review Form

Proposal Number: **16**

Applicant Organization: **California Department of Fish and Game**

Proposal Title: **POPULATION BIOLOGY AND GENETICS, HABITAT MODELING, AND PILOT RESTORATION FOR THREE CALFED AT-RISK PLANT SPECIES IN THE SACRAMENTO-SAN JOAQUIN DELTA AND SUISUN MARSH**

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

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

None

Review:

Please provide an overall evaluation summary rating:

<u>Excellent:</u> outstanding in all respects;
<u>Good:</u> quality but some deficiencies;
<u>Poor:</u> serious deficiencies.

Overall Evaluation Summary Rating	**Provide a brief explanation of your summary rating**
XExcellent	**A very sound scientific project which will lead to an excellent understanding of how to improve habitat and restore three endangered sp.**
-Good	
-Poor	

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

   Excellent, yes, yes.

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

Excellent, yes, yes, yes.

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

   Excellent, yes, yes, yes, yes.

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

   Excellent, yes, good, yes.

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

   Excellent, yes, yes.

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

   Excellent, yes, yes, yes.

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

   Excellent, good, yes, yes.

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

   Very good, Yes

<u>Miscellaneous comments:</u>

External Scientific: #2

Research and Restoration External Scientific Review Form

Proposal Number: **16**

Applicant Organization: **California Department of Fish and Game**

Proposal Title: **POPULATION BIOLOGY AND GENETICS, HABITAT MODELING, AND PILOT RESTORATION FOR THREE CALFED AT-RISK PLANT SPECIES IN THE SACRAMENTO-SAN JOAQUIN DELTA AND SUISUN MARSH**

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

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

none

Review:

Please provide an overall evaluation summary rating:

<u>**Excellent:**</u> **outstanding in all respects;**
<u>**Good:**</u> **quality but some deficiencies;**
<u>**Poor:**</u> **serious deficiencies.**

Overall Evaluation Summary Rating	**Provide a brief explanation of your summary rating**
-Excellent **-**Good **X**Poor	**This proposal is poorly written and extremely vague. It is not convincing that much of the proposed work is necessary to understand the the limited range of these species or to mitigate habitat loss.**

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

   The goals were vague, especially the ambitious attempt to model how these species works based on 3 years of data. The proposal lacks any detail of the type population and habitat model they are going to test. Yet this analysis is one of the main goals of the proposal (the fourth sentence of page 1) "The population and habitat models will be directly tested to determine if the species follow the natural growth patterns."

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

justified?

THe plants are endangered due to habitat loss. Clearly the main priority should be aquiring or restoring habitat. I there is only one population of Suisun thistle, as the proposal states may be true, then spending money to develop marker loci is akin to fiddling while Rome burns. Likewise models based on 2 or 3 years of monitoring have almost no chance of accurately predicting future viability. This proposal lacks a clear rationale for the proposed work. In addition little detail is given on how the data will be interpreted. The flowchart "models" shown in figures 2 and 3 lack parameters.

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

   Surveys for additional populations (in task 1), task 2 and task 3 would generate data useful for management decisions. Of most relevance to decision making are the pilot restoration projects. Initially pilot restoration attempts can be made in degraded habitats using locally collected germplasm. The genetic questions presented in table 1, could be adequately addressed using allozyme or markers already available.

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

   It is very unlikely that 3 years of demographic data will allow accurate predictions of population success. The same can be said for the marker analysis. Surveying variation randomly throughout the genome is unlikely to help predict restoration success. First it is clearly stated in the proposal that the main threat to these plants is habitat loss. Second, surveying neutral marker data allows inferences of historical population structure and relatedness. However variation in neutral marker loci does not allow straightforward predictions of variation in selected traits.

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

   There is no detail given on how perfomance measures of the project will be quantified.

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

   Surveys of populations, habitat requirements and pilot restoration products should produce readily interpretable results.

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

   I question the qualification of this team in population genetics. No questions or models are mentioned to warrant the large effort to develop and collect marker data. Certainly valuable inferences can be made estimates of allelic diversity and distribution, but this needs to be done in a population genetics framework. The proposal goes into detail regarding technical methods of

routine tasks of PCR and DNA extraction without setting up any possible interpreations of the data.

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

   I think the genetics work is unreasonably expensive. The point of the marker development seems to be to generate $1,000 per day for Larry Riggs. A genetic analysis using allozymes for example could adequately address the questions raised in the proposal. There is no reason presented as to why microsatellite markers need to be developed. The proposal states that no microsatelites exist in the Scrophularaceae, but they have been developed in many species including monkeyflower, and the genetic model system, snapdragon.

Miscellaneous comments:

External Scientific: #3

Research and Restoration External Scientific Review Form

Proposal Number: **16**

Applicant Organization: **California Department of Fish and Game**

Proposal Title: **POPULATION BIOLOGY AND GENETICS, HABITAT MODELING, AND PILOT RESTORATION FOR THREE CALFED AT-RISK PLANT SPECIES IN THE SACRAMENTO-SAN JOAQUIN DELTA AND SUISUN MARSH**

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

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

none

Review:

Please provide an overall evaluation summary rating:

<u>Excellent:</u> outstanding in all respects;
<u>Good:</u> quality but some deficiencies;
<u>Poor:</u> serious deficiencies.

Overall Evaluation Summary Rating	**Provide a brief explanation of your summary rating**
-Excellent **-**Good **X**Poor	**The general model for the proposal was much better than presentation of the overall methods. The proposal needs to more clearly present concepts and how the data would be integrated into models that can drive restoration experiments and improve long-term population viability. The methods for determining reproduction biology, clone assignment, population structure, and genetic diversity as well as the sampling methods need to be more clearly presented and justified. The methods and models for integrating demographics, biological and environmental factors, and genetic factors need to be explored and discussed. The methods for evaluating restoration experiments need to be more clearly presented and justified.**

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

   The overall goal is clear. The objective is to seek data that will be of value in formulating models about apropriate habitat for 3 rare species of plants and for projecting population growth and population viability. The data would be used to predict appropriate

environments and methods for restoration of plant populations. However, it is not clear how the models will be developed and used.

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

   It is clear that more data are needed for designing restoration and management programs for the 3 rare plant species. For example, there are no data reported on the population genetics or mating systems of these species. Although there has been some work on the demography and distribution of these species, the extent and quality of the data was not clear from the proposal. The work cited has not been published. A more detailed description about what is actually known would be useful to reviewers. The overall adaptive management model and conceptual model in Figures 2-3 help to illuminate how the different facets of the study would fit together. However, the authors did not explain how they would integrate the data, nor did they provide a conceptual basis for understanding how one might integrate the information. The proposal would benefit from an explanation of why knowledge of population genetics (Subtask B), reproductive biology, and life-history traits (Subtask C) are important, how information about demographics and reproductive biology might influence the degree of genetic variation in populations and its spatial arrangement, and why this matters. Subtask A describes how populations will be mapped, monitored, and documented but it does not justify or explain how these data will be incorporated into models of population viability together with genetic and life-history data, or what basic kind of models they plan to address.

   The authors propose restoration experiments that can guide future larger scale restoration. The experiments will be designed to test hypothesis derived from habitat and population growth models. This is an appropriated goal.

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

   The general model of the approach is good. The problem is in the details of the model components and details of data collection. Only portions of the proposal could be adequately evaluated as written. If the project design is improved and conceptual integration of tasks clearly formulated, this could be an important and successful project.

   In addition, there are problems with approach and methods described for Task 1population biology and genetics. Some of the problems are detailed below. In contrast, Attachment A showed that if data were collected appropriately, the team could very likely succeed in finding appropriate genetic markers for studies of hybridization and clonal membership. (Please note that mircosatellite primers have been used for species of Mimulus in the Scropulariaceaesee Kelly and Willis 1998 Molec Ecol; Awadala and Ritland 1997 Molec Biol.)

   Siusun thistle Hybridization: The justification and methods for the study of hybridization between Cirsium hyrophilum and common species of Cirsium are not well spelled out. The importance of any hybridization with other species is clear to me, but only because I am familiar with the concepts and the scientific literature. However, it is not clear what methods and models will be used to determine the probability or frequency of any hybridization among Cirsium species. My first complaint is that the evidence for suspecting that there may be hybridization is not given. The authors cite an unpublished report but do not say what the evidence is. Did the

unpublished study find putative hybrids? Can putative hybrids be identified from morphology? If so, what analytical models will be used to identify putative hybrids in the proposed study? It is not clear how plants will be chosen for pollen viability analysis and how the data will be utilized to judge if hybridization has taken place. It appears that individuals from the natural population will be sampled for pollen stainability. Are the authors assuming that plants with low pollen viability are F1 hybrids? Will the same plants sampled for molecular analysis be sampled for pollen stainability? Will the sampling be random or will morphological data be used to guide sampling? Will a combination of molecular data and pollen stainability data be used to identify putative hybrids in the natural population? Will there be any attempt to do controlled crosses between the suspected parents to test assumptions about the appearance of molecular markers, pollen stainability, and general morphology in parents relative to hybrid progeny? Will putative hybrids be tracked for survival and seed set? How will probabilty of hybridization be quantified?

Another point is that pollen stainability does not always correlate with pollen viability. The authors need to check that the acetocarmine method of staining accurately reflects pollen viability.

Siusun thistle life-history: The authors need to determine if plants are self-compatible and capable of self-pollination, or if plants are self-incompatible. The bagging method on page 5-6 can be used to determine if mechanical selfing occurs, but it is not useful as described for determining outcrossing ratesunless the flowers are completely cleistogamous or if there is a strong self-incompatibility mechanism or other mechanism that assures flowers are 100% outcrossing. Outcrossing rate traditionally refers to the proportion of seeds that are sired by pollen from plants other than self. Outcrossing rate cannot be determined from the methods given if the plants are self-compatible and capable of open pollination.

If the authors suspect that seed production is pollen limited, then an appropriate study should be designed to examine this question.

Masons lilaeopsis and Delta mudwort life-history: Are the authors planning on determining the relative contribution of clonal growth and seedling establishment in these species? The authors assume that plants are self-fertilizing but do not give justification for this assumption. In addition, it is not clear if they mean self-compatible or completely selfing. Understanding the mating system (including if self-incompatible), is important to understanding the sexual reproductive potential of clonal species. If, for example, the plants are self-incompatible, and highly clonal, seed production can be limited by receipt of compatible pollen. This is especially problematic in small fragmented populations composed of few genotypes (genets). In such situations, it can be beneficial to augment populations with genets from nearby populations to restore the ability for sexual seed production.

Population structure and genetic diversity: The authors do not indicate that they understand the complexity of doing population genetics on clonal organisms. The relevant published literature and models are not cited. A sampling regime needs to be figured out that allows genets to be used once (rather than repeated samples of same genet) in traditional calculations of genetic diversity. Alternative analyses of clonal diversity can also be useful. The extent of clone formation needs to be determined before an adequate sampling method can be determined. For an introduction and some solutions to the problems, see e.g. Parks and Werth 1993 AJB 80:537-544, Montalvo et al. 1997 AJB 84:1553-1564, Rogers et al. 1999 Evolution 53:74-90.

Task 2, the section on habitat structure and ecological processes is perhaps the most clearly written part of the proposal. It is clear that tidal and substrate factors can be important factors that can be incorporated into models of population growth and subsequently into the restoration

experiments under Task 3. In addition, the examination of an association between rare plant population occurrence and invasive plant species occurrence is important. It was not clear if the Task 3 restoration experiments would include a component of invasive species effects. For example, is it likely that thistle seeds will germinate and seedlings establish successfully in the thick of a perennial pepperweed patch compared to an area cleared of the weed? This could be evaluated easily.

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

   I can envision how the approach would be technically feasible and how data from Tasks 1 and 2 could drive the direction of Task 3, the restoration experiments. The feasibility of integration of data into the larger model is more questionable. Success depends on whether or not the research team can demonstrate an understanding about how to integrate the data from Tasks 1 (biology and genetics) and 2 (habitat structure and ecological processes). It will also depend on use of appropriate revised methods for data collection within Task 1, Subtasks A-C. For example, methods need to be improved and justified for population genetic sampling, determination of genetic diversity and population structure, determination of mating system, and evaluation of the relative importance of vegetative spread vs. sexual reproduction to population growth, structure and genetic diversity.

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

   The authors propose to base the pilot restoration experiments and methods on accumulated data and habitat models. It appears that they plan to translocate ramets of the Delta mudwort and Masons lilaeopsis into created habitat at several different locations and to compare performance of plants in the restoration plots with plant growth in un-manipulated control populations at natural sites. The methods for evaluating translocation success were not given. Instead, the authors cite unpublished reports (a practice that is unkind to proposal reviewers because we seldom have access to such reports and the reports are not reviewed). It appears that the controls will be naturally established plants in natural populations rather than plants planted into the native habitat. There are many confounding factors associated with this scenario which will make the results difficult to evaluate. It might be more rewarding to experimentally test specific hypotheses derived from habitat and demographic models. This could involve setting up more than one treatment (for example 2 or 3 different substrate types replicated each at several locations if substrate is identified as important factor under Task 2), and evaluating survival, growth and reproduction (in addition to cover) among treatments. Certainly both sexual reproduction and vegetative spread should be a part of the evaluation of plant performance. Once the plants are mature and well established, the comparison to the reference population would be useful.

   Seeds or seedling rosettes of Suison thistle will be planted into experimental plots that provide habitat factors hypothesized to be limiting to this species overlaid on organic vs. mineral soils. Again, existing populations will be the control. Again, it seems more appropriate to compare experimental treatments for testing specific hypotheses.

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

The project is organized so that valuable information can be garnered from all 3 proposed tasks.

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

   I was not able to evaluate the research team from the information provided. I found a few published papers from 1991 or earlier for two of the authors, but I did not find any more current publications. Perhaps they have published in journals that are not picked up by the major search engines.

   I recommend that future proposal guidelines give room for an attached CV (2 pages) for each researcher. The CV should cite published work and reports important to evaluating their expertise. If unpublished reports are listed, these should be made available to reviewers of proposals.

   Given the large dollar amounts requested in these proposals, it would be appropriate to expect that the work will be offered for publication in peer reviewed journals so that the public could benefit from the information.

   It appears that the participants have the needed infrastructure to carry out the project.

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

   The budget is very large. It is much larger than I am used to seeing for proposals by scientists from academic and federal institutions. The budget is more than adequate for the work proposed.

<u>Miscellaneous comments:</u>

The authors should provide more information about studies that are not readily available to the public. Citing work that is in unpublished reports without describing the methods, outcomes, and at least some detail of the work is not terribly useful to a reviewer.

The present proposal would benefit from a more thorough presentation of concepts and theory that support the chosen methodology. Citation of more published literature for models and concepts would be beneficial. A discussion of the conceptual framework that integrates the biology (including genetics) into models of population growth and viability and how this information can be used to improve restoration and management of rare species populations would be useful.

External Scientific: #4

Research and Restoration External Scientific Review Form

Proposal Number: **16**

Applicant Organization: **California Department of Fish and Game**

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

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

Dr. Riggs was a co-PI on an Integrated Hardwood Range Management Program grant with me (1995-2000).

Review:

Please provide an overall evaluation summary rating:

<u>**Excellent:**</u> **outstanding in all respects;**
<u>**Good:**</u> **quality but some deficiencies;**
<u>**Poor:**</u> **serious deficiencies.**

Overall Evaluation Summary Rating	**Provide a brief explanation of your summary rating**
-Excellent **-**Good **X**Poor	**In this proposal a large number of separate research projects are poorly integrated to address primary goals of study. Some of the separate projects are also poorly designed and/or poorly justified as to their ultimate contribution to overall goals.**

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

   3 - Good The general goals of this study are obviously important in that they argue for understanding the ecological determinants of the distribution of three rare plant species and the factors that may influence the viability of existing or restored populations. Further, the importance of local adaptation and evolutionary potential in restoration is now widely recognized and this proposal contains sections on this topic. However, the proposal fails in cogently linking the large number of separate experiments and hypotheses into a conceptually cohesive and internally consistent whole.

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

   3 - Fair

   There is a general problem with this proposal in that the actual application of studies to particular management issues is consistently vague. Although apparently some work has been done on these species, the results are either not published at all or they appear in documents that are difficult to access and are not peer reviewed. As such it is difficult to evaluate many of the justification statements provided. Although genetic variation is an important parameter for adaptation and evolutionary potential in rare species, the proposed microsatellite analyses of highly neutral markers will not address patterns of genetic variation in adaptively significant traits unless there is tight linkage (which is not known for these species). In addition, the applicants have not justified why less expensive markers such as allozymes or even ISSRs might not work. The justification for the entire hybridization experiment with the Suisun thistle is very sketchy and incomplete.

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

   5 - Poor There is very little indication as to how all the various independent research programs are to be successfully linked to address the larger goals of the project. There is a real danger here of various personnel pursuing their independent projects and not really cooperating in addressing the primary goals of the overall proposal. There are also real problems in the separate projects. The development of population viability models for species is a very difficult task, especially so with plant species that have both vegetative and sexual reproduction. The description of the demographic parameters that are to be measured for all three species are very minimal and are not sufficient to construct a useful model. There is no substantive discussion as to how spatial and temporal variability in transition parameters are to be incorporated into the models. Indeed, there is really no discussion as to the form of these models themselves and as a result one is left with the feeling that not much thought has been devoted to the strategy for developing these models. The genetic work is also poorly justified in that the sampling schemes and rationale are not developed. Generally, these molecular markers are not useful for measuring adaptive variation because they are neutral markers. This neutrality limitation is not addressed by the applicants and it really should have been. After all, the professed primary goal of the genetic work is to provide restoration guidelines for patterns of local adaptation and genetic variation in adaptive traits. The molecular markers can provide good information on gene flow but how patterns of gene flow would be quantified was not well developed (often not at all) in the proposal. Finally, the restoration experiment where transplanted material is compared to "control" local material is basically flawed in design. The comparison should be between non-local transplanted material and local transplanted material (i.e. material from the local population that has been treated in the same way as the non-local material). This would provide the proper controls for examining local adaptation

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

5 - Poor The weak or poorly described linkages between the separate experiments and the larger goals of the project significantly reduce the likelihood that the overall goals will be realized. In addition, the development of the population viability models is severely compromised by 1) the lack of attention to the necessary parameters required to adequately describe the demographics of the complex life history of the target species and 2) a lack of consideration of temporal and spatial variation in species demography. Because of design flaws (described above) the restoration experiment will not provide useful information on local adaptation.

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

   4 - Fair Two of the main performance measures would seem to be 1) the development of useful population viability models and 2) information of direct relevance to restoration efforts for these species. Because of the problems with approach and feasibility for both of these areas, there is a real question as to whether there is an accurate assessment of performance.

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

   4 - Fair Although there could be a large amount of potentially useful information obtained, it is not entirely clear from the flow charts how this information will finally be synthesized and organized for effective communication and outreach. Problems with restoration experiment might significantly compromise interpretation and capacity to develop recommendations.

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

   3 - Good

   Although it would seem that the personnel assembled for this project are quite capable, my confidence was shaken somewhat by 1) the rather vague and almost naïve descriptions of the proposed work to create population viability models, 2) the rather poor justification for a very expensive and extensive study using molecular markers (that are neutral to selection) and 3) a flawed transplant design in the focal experiment linking to restoration of these species

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

   4 - Fair

   Some of the allocations seem rather excessive. For example, the allocation of $1000/day to Biosphere Genetics seems pretty high and is not well justified.

Miscellaneous comments:

Prior Performance/Next Phase Funding:

New Proposal Number: 16

New Proposal Title: POPULATION BIOLOGY AND GENETICS, HABITAT MODELING, AND PILOT RESTORATION FOR THREE CALFED AT-RISK PLANT SPECIES IN THE SACRAMENTO-SAN JOAQUIN DELTA AND SUISUN MARSH

1. Prior CALFED project numbers, titles, and programs: (*list only projects for which you are the contract manager*)

   98-F08, Hill Slough Habitat Restoration Demonstration Project, Phase I; 98 F09, Rhode Island flood plain Management and restoration project; CALFED ERP

2. Prior CVPIA project numbers, titles, and programs: (*list only projects for which you are the contract manager*)

3. Have negotiations about contracts or contact amendments with this applicant proceeded smoothly, without persistent difficulties related to standard contract terms and conditions?

   XYes **-**No **-**N/A

   If no, please explain any difficulties:

4. Are the status, progress, and accomplishments of the applicant's current CALFED or CVPIA project(s) accurately stated?

   XYes **-**No **-**N/A

   If no, please explain any inaccuracies:

5. Is the applicant's progress towards these project(s)' milestones and outcomes to date satisfactory?

   XYes **-**No **-**N/A

   If no, please explain deficiencies:

6. Is the applicant's reporting, records keeping, and financial management of these projects satisfactory?

   XYes **-**No **-**N/A

   If no, please explain deficiencies:

7. Will the project(s) be ready for next phase funding in 2002, based on its current progress and expenditure rates?

   -Yes **-**No **X**N/A

If no, please explain:

Other Comments:

Environmental Compliance:

Proposal Number: 16

Applicant Organization: California Department of Fish and Game

Proposal Title: POPULATION BIOLOGY AND GENETICS, HABITAT MODELING, AND PILOT RESTORATION FOR THREE CALFED AT-RISK PLANT SPECIES IN THE SACRAMENTO-SAN JOAQUIN DELTA AND SUISUN MARSH

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

   -Yes **X**No

   If no, please explain:

   Need to apply for a 1600 Agreement for channel creation activities.

   Notification of BCDC required.

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

   XYes **-**No

   If no, please explain:

   No budget or timeline specified for obtaining permits because only seeking Scientific Collecting Permit. If a 1600 Agreement is needed, allow 2 months to complete and either $154 or $772.75 in fees depending on cost of channel creation activities.

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

   -Yes **X**No

   If yes, please explain:

   May need 1600 Agreement to create tidally influenced channels, and need to notify the BCDC.

Other Comments:

Budget:

Proposal Number: 16

Applicant Organization: California Department of Fish and Game

Proposal Title: POPULATION BIOLOGY AND GENETICS, HABITAT MODELING, AND PILOT RESTORATION FOR THREE CALFED AT-RISK PLANT SPECIES IN THE SACRAMENTO-SAN JOAQUIN DELTA AND SUISUN MARSH

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

   XYes **-**No

   If no, please explain:

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

   XYes **-**No

   If no, please explain:

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

   XYes **-**No

   If no, please explain:

4. Are appropriate project management costs clearly identified?

   XYes **-**No

   If no, please explain:

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

   -Yes **X**No

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

   difference of $156,028. Dept of fish and games comments states their salaries are covered in gov budget for gen. fund as well as prop 204 funding.

6. Does the budget justification adequately explain major expenses?

   XYes **-**No

If no, please explain:

7. Are there other budget issues that warrant consideration?

-Yes **X**No

If yes, please explain:

Other Comments:

the information provided is related to consultant services.