

Proposal Reviews

#113: LIFE HISTORY OF EGERIA Densa IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY

Portland State University

Initial Selection Panel Review

Research and Restoration Technical Panel Review

Delta Regional Review

#1

#2

External Scientific Review #3

#4

#5

Environmental Compliance

Budget

Initial Selection Panel Review:

CALFED Bay-Delta 2002 ERP PSP Initial Selection Panel Review

Proposal Number: 113

Applicant Organization: Portland State University

Proposal Title: LIFE HISTORY OF EGERIA DENSA IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY

Please provide an overall evaluation rating.

Explanation of Recommendation Categories: Fund

- **As Is** (a proposal recommended for funding as proposed)
- **In Part** (a proposal for which partial funding is recommended for selected project phases or components)
- **With Conditions** (a proposal for which funds are recommended if the applicant contractually agrees to meet the specified conditions)

Consider as Directed Action in Annual Workplan (a proposal addressing a high priority action that requires some revision followed by additional review prior to being recommended for funding)

Not Recommended (a proposal not currently recommended for funding-after revision may be considered in the future)

Note on "Amount":

For proposals recommended as Fund As Is, Fund In Part or Fund With Conditions, the dollar amount is the amount recommended by the Selection Panel.

For proposals recommended as Consider as Directed Action in Annual Workplan, the dollar amount is the amount requested by the applicant(s).

| Fund | |
|------------------------------------|----------|
| As Is | X |
| In Part | - |
| With Conditions | - |
| Consider as Directed Action | - |
| Not Recommended | - |

Amount: **\$327,937**

Conditions, if any, of approval (if there are no conditions, please put "None"):

None

Provide a brief explanation of your rating:

This is a very sound research proposal that aims to elucidate the life history of Egeria such that control measures can be more readily designed, and/or we can better understand the implications of our management actions (both those targeted at Egeria and others) on the Egeria problem. The frustration of several technical reviewers seems to be that the proposal is not set in the context of existing/planned restoration measures for the Delta, and perhaps the proposers lack of explicit reference to such means they have not fully considered where their work might be useful. Had their conceptual model included components that showed the effects of management actions on the various potential controlling environmental factors, this may have clarified this issue. However, the Selection Panel feels this is an important step forward on Egeria in the CALFED adaptive management structure it certainly seems appropriate to present a research proposal because of the great uncertainty surrounding Egeria management. This study will likely provide some good information on Egeria that can aid ERP, and in a timely manner. No revision needed the need is to get started.

Research and Restoration Technical Panel Review:

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

Proposal Number: 113

Applicant Organization: Portland State University

Proposal Title: LIFE HISTORY OF EGERIA DENSA IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY

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 |
|-----------------------------------|---|
| XSuperior | The proposed research, investigating an invasive species causing great impact in the Delta, is well founded, with a clear conceptual model leading to well defined hypotheses. The research team is highly qualified and supported by Portland State facilities. The budget is modest in comparison to the information that should result. |
| -Above average | |
| -Adequate | |
| -Not recommended | |

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?

Egeria densa is a severe pest in the Delta and understanding its biology, especially as it pertains to vulnerability, should be a high CalFed priority. The goal of this project is to develop a mechanistic understanding of how Egeria invades, persists, and spreads in the Delta through well articulated hypotheses relating to environmental interactions with the plant's physiology, allocation patterns, and dispersal. A thorough review of previous findings is presented which supports the need for the proposed research. The conceptual model for this research project is well developed and guides the selection of hypotheses and methodologies.

There were 2 concerns; 1, that more research into photosynthesis than necessary was being proposed, and 2, that there is uncertainty how this information would aid in control efforts. However, 2 reviewers ranked this proposal as excellent, 2 as good.

2. **Likelihood of Success (Approach, Feasibility, Capabilities and Performance Measures).** 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 research approach breaks down various stages in the life history of the plant and examines the effect the environment has on each stage. This approach should result in a mechanistic understanding of the plant's life history that may be translated into a management approach. The panel thought that extending this to ecosystem levels may be a useful next step. The approach is well designed, appropriate for the project, and potentially useful to managers. The various analytical methods are fully documented (Table 1) and feasible. The scale is consistent with the objective. Project specific performance measures are illustrated in a flow chart (Fig. 5) and listed in the time line (Table 2). The presentation is clear, thorough, and informative. The research team of Dr. Sytsma and his grad student Ms. Pennington are well experienced in aquatic plants in general and Egeria specifically and should be able to complete this research. The necessary infrastructure will be provided by Portland State University.

3. **Outcomes and Products.** 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?

The product of this research will be a seasonal description of growth, resource accumulation, and fragment viability in Egeria. The latter will provide information on the most effective time in which to treat Egeria, but it will still need to be tested in situ. The descriptions of photosynthesis in Egeria are of potential interest to physiologists.

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

Yes, this is a modest budget.

5. **Regional Review.** 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?

The Delta Regional Review ranked the proposal Medium as a lab study and noted lack of linkage to local institutions/people. The panel felt interaction with current mapping efforts would be fruitful.

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

No

Miscellaneous comments:

None

Delta Regional Review:

Proposal Number: 113

Proposal Title: LIFE HISTORY OF EGERIA DENS A IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY

Overall Ranking: -Low Medium -High

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

Useful research that provides information that will improve our understanding of Egeria in the Delta

1. Is the project feasible based on local constraints?

Yes -No

How?

no permits required

similar research done in other places

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

Yes -No

How?

invasive species research (MR-1+ DR-5)

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

Yes -No

How?

lab studies to improve understanding of Egeria in the Delta

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

-Yes No

How?

no mention or linkage to other programs that are working on control of Egeria, only mention other lab research being done on Egeria

Other Comments:

could coordinate with Boating and Waterways, USDA

External Scientific: #1

Research and Restoration External Scientific Review Form

Proposal Number: **113**

Applicant Organization: **Portland State University**

Proposal Title: **LIFE HISTORY OF EGERIA Densa IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY**

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:

Excellent: outstanding in all respects;

Good: quality but some deficiencies;

Poor: serious deficiencies.

| Overall Evaluation Summary Rating | Provide a brief explanation of your summary rating |
|--|---|
| X Excellent | The proposed research, investigating an invasive species causing great impact in the Delta, is well founded, with a clear conceptual model leading to well defined hypotheses. The research team is highly qualified and supported by Portland State facilities. The budget is modest in comparison to the information that should result. This proposal should be funded. |
| -Good | |
| -Poor | |

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

Egeria densa is a severe pest in the Delta and understanding its biology, especially as it pertains to vulnerability, should be a high CalFed priority. The goal of this project is to develop a mechanistic understanding of how Egeria functions in the Delta through well articulated hypotheses relating to environmental interactions with the plant's physiology, allocation patterns, and dispersal. It is timely and important research.

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?

A thorough review of previous findings is presented which supports the need for the proposed research. The conceptual model for this research project is well developed and guides the selection of hypotheses and methodologies.

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?

The research approach breaks down various stages in the life history of the plant and examines the effect the environment has on each stage. This approach should result in a mechanistic understanding of the plant's life history which may be translated into a management approach. The approach is well designed, appropriate for the project, and potentially useful to managers.

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?

The various analytical methods are fully documented (Table 1) and feasible. The scale is consistent with the objective.

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?

Project specific performance measures are illustrated in a flow chart (Fig. 5) and listed in the time line (Table 2). The presentation is clear, thorough, and informative.

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 first step in controlling a pest is to know what makes it tick. This knowledge is the principal product to arise from this research. Additionally, reports, scholarly publications, and a PhD thesis are expected outcomes.

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?

The research team of Dr. Sytsma and his grad student Ms. Pennington are well experienced in aquatic plants in general and Egeria specifically and should be able to complete this research. The necessary infrastructure will be provided by Portland State University.

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

yes.

Miscellaneous comments:

External Scientific: #2

Research and Restoration External Scientific Review Form

Proposal Number: **113**

Applicant Organization: **Portland State University**

Proposal Title: **LIFE HISTORY OF EGERIA DENSA IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY**

Conflict of Interest Statements:

I have no financial interest in this proposal.

Correct

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:

Excellent: outstanding in all respects;

Good: quality but some deficiencies;

Poor: serious deficiencies.

| Overall Evaluation Summary Rating | Provide a brief explanation of your summary rating |
|---|---|
| <input checked="" type="checkbox"/> Excellent | The applicants propose to conduct some basic life history research on E.densa in the delta. Because the applicants based their hypotheses on existing literature and regional data, I think this proposal is well-written and carefully thought out. They have a good approach and the necessary experience for making this study a success. |
| <input type="checkbox"/> -Good | |
| <input type="checkbox"/> -Poor | |

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

The the three hypotheses outlined by the authors are clear and succinct. The authors clearly did their homework by gathering necessary background literature to support hypotheses. In addition, the hypotheses are regionally appropriate given the existing state of knowledge on E. densa in the estuary.

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?

The authors clearly justified the relevance of their proposed study to improving the state of the knowledge, not only on regional level, but also with broader ecological implications. E. densa is obviously a problem in the estuary given its widespread colonization of shallow habitats in the estuary. Beyond biological, physical and chemical influences, E. densa may substantially alter (as the authors state) restoration actions. Therefore, the need to understand the ecophysiology of the plant is critical towards understanding ecosystem effects of E.densa in the delta, thereby, reducing the uncertainty of CALFED restoration efforts.

The conceptual model was perhaps the weakest component, if at all, of this proposal. The authors laid out the basis for how environmental factors manifest variations in E.densa's physiology, however, there is no discussion how E.densa may conceptual affect the delta in other ways. For example, how does the variability of E.densa physiology affect seasonal senescence (time or scale) and DOC levels delta wide? Do plants compete with other primary producers for DIC? Or How do dense mats of Egeria alter flow dynamics and/or sediment deposition? These questions are beyond the scope of the study, but they do indicate broader implications, which the authors state in the introduction, but do spell out in the conceptual model section.

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?

The authors propose to conduct field and greenhouse studies. I think this completely appropriate for completing study objectives. In addition, the proposed studies are text-book experiments (e.g., productivity exps) that have existed in the literature for quite some time. Statistically, the number of samples proposed to taken are reasonable. I also think Franks Tract is an ideal location to conduct experiments due to the persistence and density of E.densa present during all times of the year.

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?

Yes, this study is fully documented and technically feasible. Given no hang-ups in the lab, this study has a good chance of success. Further, if successful, this study will substantially improve the state of knowledge in the delta.

The two-year time frame also seems reasonable given the task outline.

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?

Performance measures don't really apply to this proposal. Given no instrument flaws and reasonable plant collection (which shouldn't be a problem), the study should provide worthy data for the literature.

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 products will be very valuable from this study, from both regional level and under a broader ecological context

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?

The applicants are qualified for this project. In addition, because they are getting local assistance from Lars Anderson, they should have no problems implementing this study.

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

In general, the funding request seems completely appropriate for the proposed work. The applicants may be on the cusp of funding if hang-ups occur in the field or an experiment go awry and must be repeated.

Miscellaneous comments:

External Scientific: #3

Research and Restoration External Scientific Review Form

Proposal Number: 113

Applicant Organization: Portland State University

Proposal Title: LIFE HISTORY OF EGERIA Densa IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY

Conflict of Interest Statements:

I have no financial interest in this proposal.

Correct

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:

Excellent: outstanding in all respects;

Good: quality but some deficiencies;

Poor: serious deficiencies.

| Overall Evaluation Summary Rating | Provide a brief explanation of your summary rating |
|--|---|
| -Excellent | Some aspects of this research have the potential to develop useful information for the immediate control of Egeria in the Delta. Far more research is proposed than is necessary to test the hypothesis or to produce a useful model for control of Egeria. I recommend funding the portion of proposal dealing with determination of growth, allocation of reserves and fragment production and viability. |
| <input checked="" type="checkbox"/> Good | |
| -Poor | |

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

A large gap in useful knowledge is identified concerning the control of Egeria infestation in the Delta. This work proposes to acquire more data on the reproductive physiology of this invasive plant. More attention to new approaches to the control of Egeria in the Delta is certainly timely. The proposal does not have a single testable hypothesis but, rather, several hypotheses concerning aspects of growth and reproductive physiology for this plant. The first hypothesis is rather broad (page 5): There are certainly seasonal and environmental influences on photosynthesis in Egeria (temperature, light intensity, daylength, Ci concentration). But how can these be used to develop control strategies? Hypotheses 2 and 3 are more specific, testable, and have consequences for management. The authors propose

that maximum impact of control efforts will occur when carbohydrate reserves of Egeria are at a low point, and to this end they intend to determine seasonal carbohydrate and other reserves from field studies conducted over a two year period. Also, the production of viable fragments will be quantified and related to carbohydrate and nitrogenous reserves throughout the year.

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?

The present state of knowledge concerning the biology of Egeria is generally well summarized and the problem in the Delta is described in some detail. A conceptual model is presented (Figure 3) that ties the hypotheses together. The collection of some of the data is justified (Page 7) by the statement that they will be used to improve management strategies. But the proposal does not include work to examine the interaction of present management actions with measured physiological parameters. While determination of the P vs I relationship in Delta water, or amount of fixed carbon secreted to the water are interesting from the standpoint of physiology of the plant, there is no way of relating these properties to a control strategy, as there is for vegetative reserves and fragment viability.

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?

The experimental approach is overly broad, with experiments ranging from determination of the dependence of photosynthetic rate on light intensity to the analysis of allocation of carbon and nitrogen in plant parts to fragment viability. Some of the results will impact management strategy while others will not, even though they may contribute to the competitive ability of the plant (photosynthesis, fixed carbon secretion) or may be essential to understanding the life history of the plant. They go far beyond what is necessary to accomplish the experimental goals. Task 1 (p. 8) Field studies on productivity. The determinations of photosynthesis seem irrelevant to the problem of control. There is no information on how the results can be used to optimize management strategies. Task 2 and 3: Morphology, allocation and phenology. Determination of carbon and nitrogen allocation and of double node productivity and viability would be key to testing the model proposed. The timing of control to periods of low carbon and nitrogen reserves or low fragment viability has an immediate consequence for management. Task 4: Photosynthetic response to light and temperature. Again, as for task 1, this seems almost irrelevant to developing an immediate control strategy.

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?

The experiments proposed are feasible. The PI has experience in most of these areas. As noted above, the experimental plan is very broad and of a greater scale than necessary.

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 performance measures are a mixed bag. I was not enlightened at all by the use of the "adaptive management strategy"(p. 8). It seems like a descendant of Total Quality Management. On the Web, it is found mostly in Department of the Interior projects. It seems to mean if what you are doing isn't producing the results you want, try something else. But how do you know it's time to change the process? There must be some set of descriptive standards. The description of performance measures on p. 12 along with the work schedules for the 2 years (p. 14 and 15) give good quantifiable goals and a reasonable schedule for collection and analysis of data.

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 product of this research will be a seasonal description of growth, resource accumulation, and fragment viability in Egeria. The latter will provide information on the most effective time in which to treat Egeria, but it will still need to be tested in situ. The descriptions of photosynthesis in Egeria are of potential interest to physiologists. A comparison of these P vs I curves to those generated in Egeria from Japan or South Carolina will be difficult to interpret unless a genetic comparison of all these accessions has been conducted.

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?

The first PI has a good record of accomplishment in control of aquatic weeds and in effectively organizing resources to deal with aquatic weed problems. Resources to accomplish this research seem somewhat problematic. New greenhouse facilities are available at Portland State University for some of the growth studies. The CHN Analyzer is a very expensive piece of capital equipment.

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

This project is expensive due to the large amount of travel and the specialized equipment.

Miscellaneous comments:

External Scientific: #4

Research and Restoration External Scientific Review Form

Proposal Number: 113

Applicant Organization: Portland State University

Proposal Title: LIFE HISTORY OF EGERIA Densa IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY

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:

Excellent: outstanding in all respects;

Good: quality but some deficiencies;

Poor: serious deficiencies.

| Overall Evaluation Summary Rating | Provide a brief explanation of your summary rating |
|-----------------------------------|--|
| XExcellent | Excellently conceived and described study of great potential import to E. densa management in the Bay-Delta. |
| -Good | |
| -Poor | |

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

The proposal has a lucid and consistent goal and hypothesis structure. The concept is both timely and leads from preliminary studies by Obrebski et al. and UC-Davis.

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?

Concept of identifying weak nodes in non-indigenous species life history and biology is a viable approach to management and control. A detailed understanding of physiology in such a dominant, invasive submerged aquatic plant could be extremely useful in developing more effective management measures or examining new alternatives. A clear but somewhat simple conceptual model is presented.

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?

The approach is detailed and logical.

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?

As described, the project should be eminently feasible.

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?

Performance measures are appropriate and actually include assessment of data and analyses quality, with a flow chart.excellent!

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?

Reasonably valuable products are proposed; however, there is no direct indication of these results entering the peer-reviewed scientific literature. A Ph.D. graduate student (Pennington) is involved, so potential thesis publication may result?

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?

Both expertise and on-going relevant research (in Oregon), and excellent proposal preparation, suggest strong capability.

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

Cost (\$327,983) for two-year investigation is reasonable given combination of field, greenhouse and other tasks.

Miscellaneous comments:

*** tributaries of Franks Tract (a reflooded island)? * What evidence of E. densa being specific mitten crab habitat?**

External Scientific: #5

Research and Restoration External Scientific Review Form

Proposal Number: 113

Applicant Organization: **Portland State University**

Proposal Title: **LIFE HISTORY OF EGERIA Densa IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY**

Conflict of Interest Statements:

I have no financial interest in this proposal.

Correct

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:

Excellent: outstanding in all respects;

Good: quality but some deficiencies;

Poor: serious deficiencies.

| Overall Evaluation Summary Rating | Provide a brief explanation of your summary rating |
|-----------------------------------|--|
| -Excellent | The authors have written an EXCELLENT research proposal regards to aspects of the life-history of Egeria, but extension of the authors' proposed research to management improvement is limited. Egeria densa is a widespread invasive species in the Delta and warrants attention and the proposed research would certainly advance our understanding of the Delta's Egeria population. Aspects of this proposal merit funding, but full funding would require an ecosystem-approach that included better in situ documentation of coverage and net growth rates (including consumption). In addition, Egeria management change should consider Egeria's role as structural habitat for epiphytic food-resources and refugia for specific fish species. |
| XGood | |
| -Poor | |

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

The authors aim to improve the management of Egeria densa by studying its basic life history. They outline a thorough plan for describing Egeria's seasonal, morphological, and ecophysiological responses to various environmental variables. Egeria is a non-native, invasive plant that covers 3,900 acres in the Delta (and coverage is expanding 100 acres a

year). Current management strategy includes the use of herbicides that may have additional unwanted effects on the Delta's biota and drinking water supply, and in this regard the study of *Egeria* growth and dispersal is timely and important. Goals and objectives surrounding study of *Egeria*'s life history are clearly stated, although the hypotheses read more like objectives. Understanding the life history of a critical population (whether the goal is to re-establish or eradicate) is essential for devising sound management actions, but additional information is necessary to generate a complete management and/or restoration strategy.

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?

The authors have presented a thorough literature review and conceptual model and thus have integrated their proposed research with that of others. *Egeria densa* has been studied in other aquatic ecosystems, but Delta-specific life history is apparently absent.

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?

The authors propose to assess 1) seasonal and environmental (e.g. light and temp.) influences on photosynthesis and carbon assimilation/loss, 2) seasonal changes in morphology and nutrient/energy allocation, and 3) dispersal and establishment success. The approach is very strong in regards to understanding *Egeria*'s ecophysiology and morphology. The results will certainly add to the base of knowledge regarding *Egeria*, and may yield some novel information. The utility to decision makers is uncertain without additional information regarding the ecology of *Egeria*. In addition, I have some concerns about the value of greenhouse experiments for examining the growth and dispersal of a Delta-specific *Egeria* population.

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?

The approach and methods are carefully documented and technically feasible. Success is highly likely in regards to extending the range of knowledge concerning *Egeria*. Extension to management improvement is uncertain.

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 have done a solid job of outlining their plans, and success seems likely based on the thoughtful descriptions of experimental designs.

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?

Valuable products are highly likely to emerge from this research.

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?

Dr. Sytsma is an authority on the biology of aquatic plants and has prior experience with Delta fauna. The authors are fully qualified to conduct the thoughtful research plan they have proposed.

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

The budget seems reasonable considering the wide-spread coverage of Egeria in the Delta and aggressive eradication management.

BUT, I would like to see additional approaches combined with the thoughtful documentation of life-history traits.

Miscellaneous comments:

I really like many aspects of this proposal, so these comments are intended as "food for thought". The proposed research would be greatly strengthened if a more Delta-specific approach were considered. In terms of Delta-restoration, I question the strength of the greenhouse experiments. Could remote sensing or aerial photography be used to augment the mechanistic life-history approach? Coverage and change in coverage in connection with Delta-specific ecophysiological constraints would be very important information for Delta management. Why model idealized net growth for management purposes when realized net growth could be quantified? Differences in approaches may yield consumption terms, but that does not seem to be the goal of this proposal.

Environmental Compliance:

Proposal Number: 113

Applicant Organization: Portland State University

Proposal Title: LIFE HISTORY OF EGERIA Densa IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY

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

Yes -No

If no, please explain:

No permits or environmental documentation required.

***BUT on the Environmental Compliance Checklist, remove all of the "required" from the list and leave blank.**

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

Yes -No

If no, please explain:

N/A

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

-Yes No

If yes, please explain:

Other Comments:

Budget:

Proposal Number: 113

Applicant Organization: Portland State University

Proposal Title: LIFE HISTORY OF EGERIA Densa IN THE DELTA: FACTORS CONTROLLING PRODUCTION & FRAGMENT VIABILITY

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

Yes -No

If no, please explain:

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

Yes -No

If no, please explain:

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

Yes -No

If no, please explain:

4. Are appropriate project management costs clearly identified?

Yes -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 No

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

Requesting \$327,937 (17a); Grand Total for 2-Year Budget Summary is \$327,983.

6. Does the budget justification adequately explain major expenses?

Yes -No

If no, please explain:

7. Are there other budget issues that warrant consideration?

-Yes No

If yes, please explain:

Other Comments: