Role of the Alien Macrophyte Egeria densa in the Sacramento−San Joaquin Delta Ecosystem

Larry R Brown

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0079: Role of the Alien Macrophyte Egeria densa in the Sacramento−San Joaquin Delta Ecosystem

Final Panel Rating
inadequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

This is an important problem and a clearly written proposal. However, there are several problems with the proposed research. It is not clear how managers would use the information gained from this study. There are several technical issues (details follow). In addition I have concerns about project coordination. The widespread team proposes meeting only once or twice/yr; I do not think that is sufficient for coordinating a complex study. The sedimentation study (task 1) does not seem to be an essential part of the research. It is not clear why the comparison of sedimentation rates is with Egeria vs. without SAV. Why not Egeria vs a native SAV? In addition, there will be no boat wakes in the experimental channels. That condition does not seem relevant to the rest of the Delta. Glass slides are a poor substitute for Egeria leaves for a study of epiphytes (task 2). Why no measure of Egeria production? Tasks 3 b,c, and d do not seem central to the questions being addressed in this proposal. Inadequate attention is paid to cage artifacts in Task 4. I have concerns with the experimental design in Task 5. It assumes that the important predators are resident, i.e. not moving in or out of channels; I doubt that is the case. Further more only gut content analyses during spring and summer are proposed to analyse food web connections; clearly

this is a problem requiring the use of stable isotopes. The experimental design of only one vegetated and one unvegetated habitat does not seem adequately replicated to provide conclusive results. Releasing such a large number of fishes into a habitat that may already be at carrying capacity will probably result in very unnatural mortality patterns. Using a mark−recapture program with fish that are already in the habitat would likely yield more valid inference.

Additional Comments:

Other reviewers rated it very good and fair. The reviewer rating it most highly expressed concern about poor linkage between project findings and restoration and felt that the first task (sedimentation study) was not needed and that the budget was very high. That reviewer wrote "I don't think it likely that the project will generate novel methodologies or approaches. What is described is standard." Te second reviewer concluded: "While I am supportive of the question the PIs wish to address, I am not supportive of the way in which they have made their case. The approach is simplistic....The budget seems enormous, yet the data collection is very basic and traditional."

This is an important problem and a clearly written proposal. However, there are several problems with the proposed research. It is not clear how managers would use the information gained from this study. There are several technical issues (details follow). In addition I have concerns about project coordination. The widespread team proposes meeting only once or twice/yr; I do not think that is sufficient for coordinating a complex study. The sedimentation study (task 1) does not seem to be an essential part of the research. It is not clear why the comparison of sedimentation rates is with Egeria vs. without SAV. Why not Egeria vs a native SAV? In addition, there will be no boat wakes in the experimental channels. That condition does not seem relevant to the rest of the Delta. Glass slides are a poor substitute for Egeria leaves for a study of epiphytes (task 2). Why no measure of Egeria production? Tasks 3 b,c, and d do not seem

central to the questions being addressed in this proposal. Inadequate attention is paid to cage artifacts in Task 4. I have concerns with the experimental design in Task 5. It assumes that the important predators are resident, i.e. not moving in or out of channels; I doubt that is the case. Further more only gut content analyses during spring and summer are proposed to analyse food web connections; clearly this is a problem requiring the use of stable isotopes. The experimental design of only one vegetated and one unvegetated habitat does not seem adequately replicated to provide conclusive results. Releasing such a large number of fishes into a habitat that may already be at carrying capacity will probably result in very unnatural mortality patterns. Using a mark−recapture program with fish that are already in the habitat would likely yield more valid inference.

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

Role of the Alien Macrophyte Egeria densa in the Sacramento−San Joaquin Delta Ecosystem

A study of the role of Egeria in the ecosystem addresses an important issue. However, the conceptual framework of this study was inconsistent, and the researchers' logic was hard to follow.

It was not clear why there was a need to look at sedimentation with and without Egeria. A contrast with native emergent vegetation (tule marsh) would have been useful. Glass slides were considered a poor substitute for Egeria surface for sampling epiphytic vegetation; and it was not clear how these results would be extrapolated to the entire ecosystem.

The benthos study would use Corbicula in cages, but it was not clear what question it addressed. Cohort analysis to measure amphipod productivity may not be appropriate because Hyalella is often not univoltine.

There were problems with the fish predator study, because predators may not be residents. The fish release experiment was not expected to result in reliable data because native fish would be released into a confined area. Furthermore, the use of only two sites without replication in the fish release study was considered insufficient. The absence of stable isotope analyses makes the food web study of little value.

The methodology was naïve in many areas. The budget seemed excessive for the type of data being collected. The research team planned to meet only two times per year; the researchers need more coordination. Another shortcoming is the absence of a link of this study to management.

Rating: inadequate

Technical Review #1

proposal title: Role of the Alien Macrophyte Egeria densa in the Sacramento−San Joaquin Delta Ecosystem

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The stated goal of developing a better understanding of the ecological processes occurring within Egeria beds and interactions with surrounding habitats is a laudable one. If the desire is ecological understanding of this non−native ecological engineer, then the proposal is fine. The broader view of conservation/restoration is less clear.
Rating	very good

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?

Comments	Much of the background of this proposal is couched in terms of restoration, and explicit connections between the results of the ecological research and restoration are lacking. The information that will undoubtedly emerge from research described in this proposal is less helpful in decision−making regarding tidal wetlands restoration. A better approach would be to attempt wetlands restoration in the presence and absence of Egeria, and evaluate the outcomes. What the proposal does is describe the impact of Egeria. Better justification would revolve around post hoc

	determination of the effects of a non−native structural bioengineer, and the effects on native biota (e.g., Melaleuca in Florida).
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? 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?

Comments	The various approaches to the study of the ecological impacts of Egeria range from excellent to fair. Investigation of the connections between the two special status species and the presence/density of Egeria is on the money (Objectives 5, 6). I believe that elucidating the trophic pathways within the beds is a valuable objective (3), though I was puzzled that the focus is on another non−native (bivalve). Will this help restoration decisions? Objective 4 will examne macroinvert parameters in sparse and dense patches of Egeria. There is a voluminous literature that shows that patch size WILL play a critical role in determining predation losses. This needs to be considered. Egeria may support epiphytic algae and actually prove to beneficial in this one way (Objective 2). I felt that this idea wasn't carried far enough, though. Reliance one primary production by epiphytic algae is probable, given the results from analogous seagrass systems. What about production by Egeria channeled into consumers, native and otherwise? Did I miss something? Also, measurements described are of standing crop; how well do these describe primary production?? In part, this depends on sampling frequency, something I couldn't determine from the proposal.

	Objective 1 (sedimentation) is the weakest of all the components for several reasons. The information derived from this section does not, in my opinion, integrate well with the rest of the objectives. I question whether the time frame is adequate to determine the eventual impact of Egeria on sedimentation. If Egeria is found to promote sedimentation, does this represent an argument FOR its introduction, since it stabilizes and adds to the current sediment base? It's not clear to me from the information provided whether the ranges of stem density and circumference, and plant biomass are sufficient to produce a measureable effect (i.e., statistically visible) on sedimentation rate. Finally, measurements of the physical characteristics of the sediments could be (and should be) incorporated into the trophic study. These don't require special knowledge and techniques outside the capabilities of a benthic ecologist, and certainly don't merit the budget allocation (below). Some interesting information on the ecological impacts of a non−native aquatic plant will result from the project. I don't think it likely that the project will generate novel methodologies or approaches. What's described is standard.
Rating	good

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 and within the grasp of authors?

Comments	The project appears feasible. The authors have done the groundwork to identify and obtain access to a field site that is adapted for this work. Given their backgrounds, the project is well within the grasp of the investigators.
Rating	excellent

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Only objection: I would like to be assured that three snapshots of benthic infaunal invertebrates will provide accurate estimates of secondary production (Objective 3).
Rating	very good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The products derived from this project will expand our understanding of the ecological role and impacts of Egeria and, by extention, other structure−forming invaders. The results are difficult to apply in a conservation/restoration framework. Egeria is in the delta and will remain so. Removal is impossible. Is suppression possible? If so, a multi−attribute analysis of the treatment alternatives will likely incorporate information obtained in this project.
Rating	very good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? 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?

Comments	

	As stated above, the backgrounds provided by the investigators assures me that the team is well qualified. Each has experience and can be considered an expert in their respective area. The site identified for the study is a good one. The project will be successful.
Rating	excellent

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	Overall, I'd say the budget is on the high end of a reasonable range for a project of this scope and duration. On average, each investigator requests slightly over $100,000 per year. This includes a good deal of support salaries and their overheads. I think the budget could be trimmed by excision of most of Objective 1 lines.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	Remove or significantly reduce Objective 1 (work planned, budget) and I think this is a proposal that outlines some interesting research.
Rating	very good

Technical Review #2

proposal title: Role of the Alien Macrophyte Egeria densa in the Sacramento−San Joaquin Delta Ecosystem

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	I would say the goals and hypotheses that are proposed are clearly stated. since the key hypothesis of restoration of ecosystem processes being correlated with native fish recruitment i think this would be a timely question which could be of interest to the applied scientist community as a whole. it could also be of interest to the broader community of ecologists if this group can link ecosystem processes to classic community structure.
Rating	very good

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?

Comments	The authors state that restoration of tidal wetlands, marshes and shallowwater habitats is key to the restoration of native fishes to presumably some baseline. The basic assumption is that this restoration will restore ecosystem processes that supported native fishes in the past. Subsequent studies and analyses suggest that restoration of freshwater tidal wetlands in the SacramentoSan Joaquin Delta might not be as beneficial to native fishes as

	expected since much of the shallow subtidal habitat offshore of tidal wetlands has been invaded by an alien aquatic macrophyte, Egeria densa, that forms dense beds. These E. densa beds provide habitat more suitable for alien fishes, many of which are likely predators or competitors of native fishes. In addition to these effects on fish communities, E. densa beds may have altered trophic pathways and physical processes, particularly sedimentation at restoration sites. As such developing a more complete understanding of the ecological process occurring within E. densa beds and nearby habitats is needed. The exotic species are largemouth bass and two species of sunfish which have been stocked in a many aquatic systems through out the continental united states for recreational fishing purposes. the data to support this conclusion are not yet available as they are part of a manusript being developed at the time of proposal submission.
Rating	very good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? 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?

Comments	in terms of the conceptual model, it is not terribly informative and i am uncertain how some of the tasks, sedimentation for example, are related to egeria's facilitation of exotic fish recruitment to the restoration sites. moreover they have not addressed the possibility that even if a native species of SAV were to have colonized the restoration sites these "exotic" fish species would have colonized the area anyway. the vague description of the study design leaves unsure at times times if the low density treatment is actually a no plant treatment which would obviously confound testing of the stated hypotheses.

	if these issues are not clearly addressed, it will be difficult to pin any conclusions solely on colonization by an exotic species of SAV being the agent hindering native fish recovery. at times, this study appears to be planning to construct a simplistic food web in which trophic links are based on some crude estimates of production at lower trophic levels and poorly resolved trophic links based on the last meal collected fishes had. no isotopic work is included. and there remains the issue of how these tasks will be integrated such that they address the question at hand. i am also concerned about the simplicity of the sample design where treatments seem to be levels of exotic species density. as such these treatments will not be fixed.
Rating	fair

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 and within the grasp of authors?

Comments	this is quite difficult to evaluate. the methodological approaches to testing their hypotheses are vaguely describe at best. i do not find proposal page limitations to be an acceptable defense for this as nsf lter proposals are similarly limited in length and the level of methodological detail provided in them is far greater. given the budget size, i am not inclined to give them the benefit of the doubt based solely on the publication records of the more established pi's who are well published. the data analyses are also vaguely described and cause me to have some significant reservations as to the probabilities of the authors being able to actually address the questions they pose. the absence of some sort of ecological modeler also leaves me with serious

	doubts as to how these tasks will integrated into an end product.
Rating	fair

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	this is also vaguely described in the proposal, with no clear statements on study design, treatment identification, or levels of replication. in reading what is provided, there is temporal sampling that seems to be related to seasonal shifts in sav abundance, yet time is not a treatment in the analyses. temporal sampling seems limited given the budgets anyway. some recognize caging artifact issues while others do not. also, replication seems low, as each creek, represents the replicate not sites within each creek as one methodological statement implies. again high and low SAV density will not be fixed treatments. the absence of some sort of integrative analysis is also problematic and it appears integration will be conducted via some sort of correlation. i also noticed no mention of standard analysis packages, for example primer or development of an ecopath model. these are now widely used by ecologists to these kinds of assessments, nor nonparametric techniques given the low replication and the reliance on count data whose variances are notoriously problematic.
Rating	fair

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**it seems likely some aspects of this work could reach the peer review literature while others seem pretty basic. certainly the lead pi and a few of the others have a record of getting data out into the literature so some of this information will reach the scientific community. i am less sure about some of the others as they are relatively new to the profession and have not yet had an opportunity to develop a track record.**
Rating	**good**

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? 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?

Comments	**more or less i would say the lead pi and least two of the others have a record of good or adequate publication productivity. the study design is not overly ambitous so i think this work would be accomplished with little difficulty. the methodologies, to the extent they are described, are not innovative so off the shelf equipment will work.**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	to me this budget is very high given the simplicity of the work they are conducting and the low numbers of analytical work being done.
Rating	fair

Overall

Provide a brief explanation of your summary rating.

Comments	while i am supportive of the question the pi's wish to address i am not supportive of the way in which they have made thier case. the approach is simplistic in most ways i can see, and the pi's seem to represent a loose confederation of individuals trying to shoe horn what they do into a bigger conservation question. the absense of detail in the methods coupled with an inadequate description of the study design illustrate my point. at issue for me is doubts as to whether the pi's will be able to convincingly test the question they are posing. that is difficult to say based on the information provided. the budget seems enormous yet the data collection very basic and traditional.
Rating	fair