CALFED Bay-Delta 2002 ERP Directed Actions
Selection Panel Review

Proposal Number: 193DA
Applicant Organization: University Of California, Davis
Proposal Title: Biological Assessment of Green Sturgeaon in the Sacramento-San Joaquin Watershed

Recommendation: Fund As Is
Amount: $998,222

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

Provide a brief explanation of your rating: The Selection Panel notes that previous concerns relative to the proposal have been addressed in this revision. This proposal will add to the body of knowledge and management capabilities of the sturgeon. We also note that DFG staff would likely not be available to conduct some of the subcontract activities in the proposal; however, the proposal as written states that other subcontractors would be used if DFG could not fulfill these duties. The Selection Panel recommends funding in full for this proposal.

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Research and Restoration External Review Form
CALFED Ecosystem Restoration Program 2002 Proposal Solicitation Package

Proposal Title: Biological Assessment of Green Sturgeon in the Sacramento-San Joaquin Watershed

Review:

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

The proposal represents a portfolio of research projects aimed at better understanding in green sturgeon, a species at risk,

1. *Distribution, diet, migration and habitat use patterns, spawning and reproductive behaviors.*
2. *Physiological response to possibly stressful water quality conditions*
3. *Reproductive biology, in support understanding reproductive parameters in the field and artificial propagation in the laboratory.*
4. *Development and application of DNA fingerprints for population structure analysis.*

These objectives have been addressed in previously supported research through CALFED, and substantial progress has been made regarding reproductive biology and genetic analyses. The first set of objectives has been well begun, but more time is required. Given the migratory and spawning behavior of green sturgeon, investigations on their natural ecology, the most important element of this project in my view, will take many years.

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 species merits the level of research and financial support requested and with moderate reservations, I endorse it. My chief criticism on the research is lack of integration. The "model" is a fairly mundane diagram of a life history; hypotheses are a list of largely untenable and unrelated expectations. One has to be an expert on sturgeons or really read between the lines in this proposal to understand why the various elements are justified on the basis of a species at risk. Why is the green sturgeon at risk?? What can be done about it? Will management of fisheries, habitat, or hatcheries play a role in their recovery? The research portfolio will provide information on all these issues, but I think the PI s miss a golden opportunity to direct science towards management.

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?

Despite lack of integration, there are many solid elements here – work on distribution, telemetry, spawning habitat delineation, population structure, and reproductive biology are absolutely critical. Better characterization of water quality tolerances and preferences will also be quite useful. Approaches seem sound to carryout objectives.

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 PI s have begun to demonstrate record of achievement related to artificial propagation and genetics work. Field work is well considered with new technologies for electronic tags and their remote reception. As indicated in miscellaneous comments below, I do not believe stressor biomarkers will have much utility

in field, nor is the proposal of applying results towards adaptive management well explained, or in my view 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?

See comments for 6. Products

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 planned conference and publication on green sturgeon is a nice idea, but inadequate I thought in terms of delivering management better tools for promoting recovery of the species. My criticism here stems in large part to past work on Columbia River white sturgeon, which for many years received high levels of funding akin to levels requested in this proposal. But that work also saw very nice integration under central questions related to the effect of hydroelectric power and fishing on sturgeon stocks. Indeed, MOCPOP, a population model for analyzing sturgeon populations was a very useful model arising from the work, one that served to direct research towards critical model inputs. As another example, the PI s suggest to use bioenergetics models to integrate physiological responses, yet beyond two very generic references, no specificity is given on how laboratory studies will be integrated into a bioenergetics framework. One is left with the view that the laboratory studies and development of stress markers are an end to themselves.

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 team of PI s are extremely well qualified to pull off their individual tasks. Apparently however, it will remain to others to integrate resulting research products into models as has been done for white sturgeon and other species (e.g., Beamesderfer et al.1995, Gross et al 2002, Ireland et al.2002).

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

PI and personnel involvement is well justified. I am concerned that sufficient technical help may not exist for field components of the study. I think cost/benefits would have been lessened with increased emphasis on field studies and integration of results for management purposes.

Miscellaneous comments:

P. 17. The low adhesivity of eggs and potentially high flow suggests that attachment sites for eggs may be substantially downriver from spawning areas. Egg attachment areas are poorly described for most sturgeons, and I would encourage PI to rethink sampling design and designate increased resources in investigation of the spatial relationship between spawning and egg attachment areas.

P. 18. Lots of general questions about genetics – would have been good to specify which issues specifically the DNA probes would be applied. One such question is related to population size. Will population size be estimated? I doubt it – probably too early. Still, if tagging could lead to estimates of in-river (transient) stock size, this could be quite useful.

P. 19. As indicated above, the conceptual model is poor rationale, but perhaps as portfolio of research, this project should proceed despite this shortcoming. Also, what is meant by "adaptive management" in

this particular instance? Management of what? How would management of the watershed be adaptive in a beneficial way for green sturgeon? Section on p. 21-22 on this is unhelpful. There seems little consideration of how this portfolio of research can be most immediately delivered to management.

P.23. Stranding issue is alluded to – what is this? Is this a potentially large issue with impacts to recovery of green sturgeon that this proposal can address?

P.24. Remote transistor dataloggers with thermal loggers is very nice idea for coverage of the system and the expense of 40 of these units is well justified. PI may also wish to look into combined acoustic radio tags (LOTECH – I think). What will be done with recaptured sturgeon collected by CDFG trammel nets during winter? Might be good to get condition growth information that could be paired up with summer information on the same.

P.25. Stress marker development is fine, but to what end? What are environmental/stressful conditions of interest? Biomarkers have fairly low success in field applications (in my opinion; also, the PI only provides one example from literature, which is in preparation) and I am skeptical that their development here will contribute much to issues related to green sturgeon recovery.

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	*Principal criticism is inadequate project integration and conceptual design, but individual components are strong. I am sympathetic to problems in advancing large proposals of this nature that involve multiple PI s and approaches. From this perspective, I would view this proposal as a portfolio of research and would rank all elements as worthy of study, barring perhaps the physiology section, which was poorly justified. I recognize that the review panel may not have "line item veto" authority, and therefore I would urge that the proposal be funded given the importance of continued research on this green sturgeon, and its role in the biodiversity and ecology of the Sacramento-San Joaquin watershed.*
W Good	
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Research and Restoration External Review Form
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Overall Evaluation Summary Rating	**Provide a brief explanation of your summary rating**
- Excellent	*Principal criticism is inadequate project integration and conceptual design, but individual components are strong. I am sympathetic to problems in advancing large proposals of this nature that involve multiple PI s and approaches. From this perspective, I would view this proposal as a portfolio of research and would rank all elements as worthy of study, barring perhaps the physiology section, which was poorly justified. I recognize that the review panel may not have "line item veto" authority, and therefore I would urge that the proposal be funded given the importance of continued research on this green sturgeon, and its role in the biodiversity and ecology of the Sacramento-San Joaquin watershed.*
W Good	
- Poor	

Research and Restoration External Review Form
CALFED Ecosystem Restoration Program 2002 Proposal Solicitation Package

Proposal Title: Biological Assessment of Green Sturgeon in the Sacramento-San Joaquin Watershed

I reviewed an earlier version of this proposal. Although I don't have the first one to compare with this version, this one appears leaner and somewhat more focused.

I still believe this is a well planned, thorough, and largely justified study. Green sturgeon are by far the least well understood of North America's anadromous sturgeons. Given their low abundances and restricted range, additional information is needed for management purposes. This research would provide a wealth of such information.

This cast of researchers are among the world's sturgeon experts, and they have admirably deconstructed the biology of the green sturgeon's sympatric congener, the white sturgeon. With such experience, I have little doubt that they will be able to perform the tasks outlined.

Concerning the topical areas, I very much like the genetic and movement studies outlined. The microsatellite analyses should fill the current information vacuum on stock structure. Mixed-stock analysis will provide a sense of the relative strengths of different populations. Unlike for the first version, details on how mixed-stock analysis will occur are provided. Genetic estimation of the number of breeding adults would be a fine contribution given how rarely effective population size is enumerated for any fish species.

Telemetry should be successful because of the high quantity and wide distribution of monitors, a component that is highly collaborative and cost shared. Although focused on the Sacramento-San Joaquin system, the investigators realize and address the fact that these are highly mobile and far ranging fish.

The proposed work capitalizes on the availability of some superb fish physiologists. But I am more supportive of the reproductive physiology work than I am on general stress research. The reproductive work will be useful in any culture efforts and in understanding reproduction in the wild. However, I'm not sure that the stress work has such a practical payoff. This includes swimming performance and temperature, D.O., and salinity stressors. Yes, it would be nice to know such things but if anything needed to be cut, this is what I would select.

Placement of the work within a hypotheses testing framework and superimposing a conceptual model are worthwhile means of keeping the project focused and integrated, with rigorous results.

Some of the writing within the proposal is substandard, such as the almost comical statement that "Egg, fry, and juvenile green sturgeon are sometimes difficult to tell apart except by expert sturgeon biologists." But this is the exception. Also, the Adaptive Management section has nothing to do with real adaptive management, but this is a widely misunderstood term.

The Phase 1 & 2 studies appear to have been successful with useful products and presentations emerging from them. The planned symposium on the biology of green sturgeon is an excellent idea that should yield a synthetic monograph that will be the bible for this species.

The budget is well within reason for such an ambitious program.

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 X	Continuation of a needed, comprehensive examination of fundamental questions concerning a poorly understood representative of the internationally beleaguered sturgeons.
- Good	
- Poor	

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Proposal Title: Biological Assessment of Green Sturgeon in the Sacramento-San Joaquin watershed

Review:

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

The goals, hypotheses and objectives were clearly stated. The principal goal is to advance the understanding of Green sturgeon biology through a series of studies designed as a followup to two previously funded projects directed at green sturgeon.

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 conceptual model covers the topics but it is weak. The proposed work is justified quite a bit throughout the proposal but the elements are not tied together as much as they could be and the hypotheses miss the mark as far as GS recruitment and survival. No hypothesis is put forward regarding the likely cause(s) for the decline of GS populations nor will any of those listed resolve the issue. It seems to me that recruitment and survival in the spawning reaches should be the focus if that is where they believe the problem lies for GS. The PI's certainly must have formed some idea on the bottlenecks for GS survival in a project attempting to move into phase 5. Nevertheless each element could stand alone as an independent study on GS physiology, migration, genetics, stress and reproduction. A greater knowledge for this species is needed in a general sense and this study is likely to produce some valuable results.

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?

Results of these studies are likely to add to the base of knowledge regarding GS. The team is highly qualified to conduct the research and they have added an element since their original submission (Werner) that should prove useful in tying some of the laboratory physiology to field observations, a weakness in the previous version.

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 proposed approach was adequately documented. I think these researchers are likely to achieve results that will be useful to the task at hand. Ultimately they need to focus on hypotheses specific to resolving the question of why GS have declined and what if anything can be done about it. Those kinds of results will be most useful to managers.

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?

Academic researchers don't seem to understand the performance measure concept. There are no true performance measures that I saw in the proposal.

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?

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?

Progress in the past projects on GS seems to be reasonable. Students have been graduated and some manuscripts have published and more should be on the way. The team as a whole is highly qualified and I believe they have the appropriate infrastructure to conduct this research. They are well connected within the sturgeon circles and there is strong support and cooperation from managers to see that this work continues.

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

The budget is high, but not surprising for the number of investigators. The justification could be better. This is an endangered species so cost/benefit may be irrelevant for this species and study.

Miscellaneous comments:

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 original proposal was criticized for not tying the elements together and weak justification of some of the methods. The proposal is improved but the linkages between the elements is still lacking. The tasks describe each PI's components but an overall goal is not presented for the research and the research tasks are not tied together through the hypotheses or the conceptual model
- Good XXX	
- Poor	

CALFED Bay-Delta Directed Action
Administrative Review
Budget Evaluation

Proposal number: 193 DA
Proposal title: Biological assessment of green sturgeon in the Sacramento-San Joaquin watershed.

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

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

3. Does the proposal clearly state the type of expenses encompassed in indirect rates or overhead costs? No

If no, please explain: It just says that the indirect costs are federally negotiated.

4. Are appropriate project management costs clearly identified? Yes

5. Do the total funds requested (Form I, Question 17A) equal the combined total annual costs in the budget summary? No

If no, please explain (for example, are costs to be reimburse by cost share funds included in budget summary). Typo. Question 17 has $98,222 stated instead of the real amount $998,222.

6. Does the budget justification adequately explain major expenses? Yes

7. Are there other budget issues that warrant consideration? No

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