

Draft Individual Review Form

Proposal number: 2001-B201-2

Short Proposal Title: Tuolumne River Restoration SRP 10

1a) Are the objectives and hypotheses clearly stated?

The CALFED proposal #2001-B201 "Tuolumne River Restoration - Special Run Pool 10" has a number of its strengths are that it is directly addressing two of the major problems for salmonid restoration in the Lower Tuolumne river, namely, availability of spawning and rearing habitat and direct predation by introduced Centrarchid species (large and smallmouth bass). Both of these problems will not surrender to easy solutions given the presence of major dams in the watershed. These are noteworthy and commendable objectives and fall directly within the CALFED program and scope. They are clear and well stated

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

Yes and No. The conceptual model presented in this proposal seems to rely heavily on a previous document, namely the Habitat Restoration Plan for the Lower Tuolumne River Corridor, for its justification. Since this document is not included in this proposal it is hard to evaluate the conceptual model as it is not included in its entirety. But as presented, the model appears sound.

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

This is unclear. There seems to be very little information/references to suggest that the large-scale hydro-geomorphic changes to be made will be either a) effective in the long run to increase salmonid habitat or b) that they will be stable over an extended time scale. In fact with respect to the latter, all the evidence provided in the proposal suggests that these types of manipulations have not been successful in the past. ("The 4-pumps program...was destroyed in the 1997 flood"). It would be informative to have references to projects where similar in-stream habitat restoration on this scale has been stable over longer time periods and during large flood events or to scale the project back (in terms of funding) and use it as a pilot project. Two million dollars is a significant amount of money for a project that may not have a high probability of success.

Having not read the Habitat Restoration Plan for the Lower Tuolumne River, it is also not clear to this ecologist that filling some in-channel and off-channel pools in this relatively small section of river will substantially reduce the Centrarchid populations in the entire river. These actions will likely reduce predator abundance in the local area but not necessarily elsewhere. It is possible that the restoration areas in this particular section of river are "major" contributors or "source" areas for the rivers bass population, but some indication that this is indeed the situation should be given. It is possible (given the details provided) to assume that bass are potentially moving into this area from above and below the indicated stream reach.

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

On the cover pages, the project is listed as a full scale implementation project, yet on page 5 in the third paragraph the proposal clearly states it is in actuality a demonstration project. This type of mistake is difficult to assess. Which type of proposal is it? and the justification and monitoring schemes should be different for the different types of proposals. Neither approach has been well justified, partially due to the dual nature of the proposal.

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

This is supposed to be an adaptive management plan and as such the key to success is to develop an understanding of the actual processes and mechanisms that are at work in the system. This information can not often be gleaned from such a major renovation of an area in the first two to four

years following the disturbance. This is the length of time scheduled for monitoring. Nature does not work that way. The monitoring should be very specific and focused on the desired outcome and should extend at least six-eight years past the project (in the best of all worlds this would continue for a decade).

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

I do not feel that there is adequate time and energy placed on appropriate monitoring. This goes for both the aquatic resources (the target of this proposal) as well as the riparian vegetation and the geomorphic features. As it is written, the proposal indicates that all monitoring will cease within 4 years of the finish of the project and the bulk of the monitoring will cease within two years of the project. With a project of this scope and magnitude, and one in which the desired results are not at all assured, it seems that a longer and more intensive post project monitoring should occur. This is especially true for two reasons, 1) this is supposed to be an adaptive management plan (see above 1c2) 2) California has a Mediterranean climate and as such inter-year variation in rainfall is often extremely significant. As one of the goals of the project it to re-create more natural hydro-geomorphic in-stream processes, it seems that to truly assess this goal, the project needs to encounter large variations in flow. It seems unlikely that a large variation in stream flow and rainfall will occur during the four years after this project. In fact, California's weather patterns seem to vary on something more resembling a 5-10 year cycle. I would recommend a longer period of post-project monitoring.

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

With regards to monitoring fisheries, there are scarce details provided regarding both the monitoring protocol and how the type of monitoring (i.e. techniques etc) are appropriate to determine if the overall project goals have been met. For example, if one of the major goals of the project is to reduce instream predation by introduced bass, there should be more focus on monitoring this. As it is written there is only two years of bass population monitoring, and these only occur during the construction phases of the project (i.e. the first two years). This should continue for longer (see 2a above). The details of this are lacking as well. All that is stated is that there will be snorkel and electro-fishing surveys. Who will do this work? How will it be standardized? How big of reaches will be surveyed? How many times will it be done? What time of the year will it be done? Will they count recruitment or will it just be adults? Will they examine any bass for gut contents to determine what they are eating? Will they monitor other predator species as well (i.e. Sacramento pikeminnow, bluegill sunfish, catfish species etc.)?

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

Everything they propose appears technically feasible, but as stated in 1b2 above, it is not clear (as it is written) that they have much chance of actually reaching the desired goals.

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

One of the other flaws in this proposal (as it is written) is that the focus is supposed to be on fall-run Chinook salmon restoration, yet the bulk of the details provided deal almost exclusively with the engineering and revegetation aspects of the project and monitoring. This is fine, as the majority of the people involved are fluvial geomorphologists, but it seems that more details of the biology of the fish-species-of-concern would perhaps inform the decision making process more. In other words, it would be good to have some salmon biologists or aquatic ecologists with significant research and field experience relating to salmonid restoration more directly involved with a project of this scope. It may well be that such people are involved, but as the proposal is written there is one bachelor's level biologist directly involved and peripherally (through a consulting firm) one

Ph.D. level anadromous fish biologist. I think it would be advisable to have at least one Ph.D. level anadromous fish expert on site during all phases of construction and monitoring.

Miscellaneous comments

<p>Overall Evaluation Summary Rating</p> <p><input type="checkbox"/> <input type="checkbox"/> <input type="checkbox"/> Good <input type="checkbox"/> <input type="checkbox"/></p>	<p>Provide a brief explanation of your summary rating</p> <p>The CALFED proposal #2001-B201 "Tuolumne River Restoration - Special Run Pool 10" has a number of strong points and is an ambitious and interesting proposal, yet it falls short on a few counts in terms of the CALFED proposal requirements. The major problems with this proposal is that it suffers from a lack of details in some critical places. It is very ambitious in its scope but the justification and appropriate reference material is not provided to allow this reviewer the opportunity to judge many of its statements.</p>
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