

## Draft Individual Review Form

**Proposal number:** 2001-C200-1

**Short Proposal Title:** Merced River Salmon Enhancement...

**1a) Are the objectives and hypotheses clearly stated?**

My comments concerning this element are noted below:

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

Not really – comments below:

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

Reasonably well designed – comments below

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

Not particularly well, but not a critical aspect of the proposed project – see below:

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

This is addressed in the comments below:

The proposers seek to expand a currently-funded CalFed project to enhance chinook salmon production by creating 4 miles of improved channel habitat. This seems more of an implementation project than a real research project, and developing hypotheses that will be ‘tested’ seems a bit unnecessary; at any rate, the agencies doing the work are beyond the hypothesis-testing phase. In reality, this is a proposal to enhance a moderately poor habitat that was then further damaged by flooding a few years ago and made into a wide, shallow bed that poses difficulties for rearing/spawning of, as well as a barrier for fish. The interesting element of the project is that it has been accepted that the natural floodplain has been diminished by water removals from upstream reservoirs, and so they are trying to simulate an environment scaled downward to contain the ‘permanently’ diminished flow. In fact, this should be the research element of the project, how to modify a fluvial system adapted to a historic discharge regime to one that will provide natural functioning conforming to a reduced hydrograph (a smaller watershed?) yet the proposal does not really put the project in this context (which doesn’t necessarily mean that it is not a fundable project). It may be more of an engineering research project than an ecological one, yet the question is quite interesting.

At the same time, I have some reservations about the approach and the ecological context of the project. Basically, gravel and other substrates that moved around during a recent flood events will be reconfigured to yield a nicely meandering stream form, and additional gravel will be added mechanically on a regular basis to create the necessary habitat for spawning in a 4-mile stream reach. This seems to be a major expenditure to create and maintain a relatively artificial ecosystem. In this ‘stable’ context, there seems to be little consideration regarding establishment of riparian vegetation and how a more diverse riparian ecosystem might be established. The need for promoting salmonid habitat is agreed upon by most, but it is unfortunate that single-species management seems to take precedence over ecosystem restoration in this project. It seems that substantial manual effort will be required to both establish plant growth and to maintain it. It is unclear that all this work will be then be resistant to future high flow events such as occurred in 1997, meaning that there is a fair probability that repeated, massive

intervention will be necessary to maintain this fairly limited habitat. Will it really be stable and self-sustaining over time? I recognize that there are serious perceived constraints to tampering with discharge in this river, but it would be useful to at least consider the possibility of releasing water from upstream dams to simulate the effects of flood discharges and letting the channel be created by a more naturalistic process. It was stated that in the first year following a pilot channel project, natural sedimentation and vegetation establishment was occurring, so maybe more time should be allowed to promote such processes. Regarding channel stability, it should be better substantiated that a 'larger project site translates into a geomorphically more stable project'.

I agree with the value of filling in the abandoned gravel pits to reduce the threat of exotic predators, although it would have been useful to have data to support the need here rather than relying on information from other river systems (I don't dispute that the threat was probably real). I also like the fortuitous result of recent flood damage highlighting the need to set back the flood control berms, but I wonder whether flood control through constructing new berms is an appropriate expenditure for the CalFed ERP [this is for others to decide]. Is it really appropriate to move forward with the larger-scale project at this time, or should additional time be allowed to monitor the results of the pilot channel creation work undertaken previously? It would have been useful to see more detailed results from the Demonstration Project. Presumably landowner interest has increased because of an increased awareness of flood control needs, and commercial interests appear to impinge on management decisions, but is the time factor that critical? Maybe so, but this seems a legitimate question that was possibly addressed more fully in all the supporting documentation - too much documentation, in fact, to give full attention to. This is a large increase that sounds basically like a major cost overrun on an already expensive project, and perhaps it would be helpful to know that results are good before increasing the budget.

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

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

Actually, the intended monitoring program seems to be quite good, although it is unclear how much of it has been implemented already. Perhaps the results of the monitoring program so far would be useful to examine before allocating additional funds to the project. The engineering components of the original proposal also appear reasonable, but it would also be helpful to consider the initial results of monitoring and modeling hydrologic function and sediment routing before proceeding.

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

Already addressed above - it seems interesting and feasible in the short term, although long-term prospects and need for intervention are unclear.

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

The team seems strong from an administrative perspective, and certainly has the qualifications to conduct appropriate technical evaluations of geomorphic, fisheries and wildlife status and trends. On the other hand, the team could benefit from a stronger research and theoretical background related to the complex factors involved in carrying out the elaborate physical and ecological goals that are proposed.

**Miscellaneous comments**

Misc. comments are incorporated into the above narratives.

**Overall Evaluation Summary Rating**

- Excellent
- Very Good
- X  **Good**
- Fair
- Poor

**Provide a brief explanation of your summary rating**

**Good.** This proposed revision and expansion of the Merced River salmon habitat creation is not particularly strong, although there is certainly a high potential for positive results from several aspects of the project. The project suffers from an over-emphasis on short-term engineering fixes (requiring major maintenance) to a more complex need for sustainable ecosystem restoration, and an inadequate ecological justification for expanding the current project.

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