# Draft Individual Review Form

**Proposal number: 2001-C204-2** **Short Proposal Title: -** Sedimentation in the Delta...

**1a) Are the objectives and hypotheses clearly stated?**
**1b1) Does the conceptual model clearly explain the underlying basis for the proposed work?**
**1b2) Is the approach well designed and appropriate for meeting the objectives of the project?**
**1c1) Has the applicant justified the selection of research, pilot or demonstration project, or a full-scale implementation project?**
**1c2) Is the project likely to generate information that can be used to inform future decision making?**

The researchers propose to use bedform mapping, in lieu of direct bedload sampling, along with standard suspended sediment loads to quantify the amount of sediment transport that is occurring at several locations within the tidally-influenced region. Mapping potentially provides a very cost-effective way to determine whether local transport is sufficient to generate beneficial deposition in restored tidal habitats or whether erosion (or lack of adequate deposition) may affect restoration success. In addition, the research is intended to provide more general documentation of temporal variation of sediment dynamics in the region. The methods are not necessarily novel, but are appropriate and interesting for addressing the issues at stake. I was particularly interested in the generation of detailed data related to large temporal scale trends or events in the watershed, such as documenting the movement of the hydraulic-mining sediment 'pulse' through the watershed, and characterizing the influence of relatively rare large events (large floods). These are less predictable processes but play an important role in short-and long-term geomorphic dynamics. It is also encouraging that the work is to be explicitly tied to current and proposed restoration projects, so that the influence of sediment dynamics on ecosystem recovery, and maybe vice-versa (influence of resotred tidal vegetation on sediment dynamics) can be characterized.

This research is presented as a set of hypotheses to be tested (as per the CalFed RFP), although a picky point is that the alternative hypotheses were not included in such as manner as to conform to a standard hypotheises testing set-up, but this does not detract from the quality of the proposed study. One thing that did bother me is that it is often not clear how the intended research approach will discriminate between the competing (but 'not mutually exclusive') elements of the system. For example, as for differentiating the effects of large events, historical (mining) pulses, and land-use changes or reservoirs, it was simply stated that 'monitoring will be adjusted to discriminate posible sources', while I was unclear on how that is actually done. I don't doubt that the authors have their ways for doing so, but I was unable to evaluate whether these ambiguities can truly be fully addressed - sort of like taking the data and then in the computer, 'magic happens'! Also, while the proposers mentioned the sediment relationships between thalweg and channel margin zones, and between tidal reaches and the upper watershed, I further felt that the ability to quantify the explicit influences of these dynamics was not strongly substantiated. Too often the reviewer is left to assume that the authors know what they are talking about (or are referred to a website for checking out the data), and so we must read between the lines. Preliminary data or some sort of graphic representation to support the proposed study would have been useful so that it all makes more sense.

Nonetheless, this work is good on-going research, and is well-linked to other programs. It is clear that the researchers will be adjusting their approach and methods to account for new information, which satisfies the ‘adaptive management’ requirement.

**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?**

This is, in essence, a strong monitoring program, and the process for reporting is fully adequate. I have no reason to doubt that the proposers are fully competent at conducting appropriate analysis and evaluation of their results, but that process was not really very clearly stated in the proposal. Based on their previous (on-going) work and commitments, it seems reasonable to assume that their assessment will be professional and subect to suitable peer review.

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

Same as stated above - this is high quality research using approriate methods , even if I am not entirely clear on how the research analysis and evaluation will be conducted. The study methods are designed to take into account unforeseen events (weather events) and the process for integration with other projects and regional programs seems clear, even if not addressed in great detail. Integration is likely to be a real strong point in this proposed work.

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

All researchers are highly qualified to conduct this work, and the project represents a continuation of on-going work that already appears to be competently done.

**Miscellaneous comments**

As noted above, it would be useful to provide greater detail regarding the relationship between sediment transport in the main channel and the lateral movement of this sediment (which presumably is very different for suspended vs. bedload sediments) into the nearshore habitats intended for restoration.

Sediment is generally treated as a homogeneous variable, yet it can differ greatly in ‘quality’ for ecological processes. This may be especially important in terms of supporting establishing vegetation and associated wildlife in restoration sites. It is mentioned that nutrients and toxic substances are important elements, but is it possible to incorporate some measure of organic content and other qualitative measurements in this study? Is it relevant to the issues at hand? It seems so, or it wouldn’t have been mentioned.

When sediment in the Delta reflects combined sources of both eroded upstream material (transported at high point on the hydrograph) and movement of unconsolidated material earlier on the rising limb, as well as re-suspended material in-situ, how does one differentiate between material coming from multiple watersheds which are exporting run-off according to different schedules? It seems hard enough to differentiate just within a single watershed.

An 'uncertainty' that was mentioned was the investigation of 'flood mgt. as an ecosystem tool'. This is extremely interesting and important in restoration work, and I would like to know a lot more about how this will be incorporated in to the research design.

**Overall Evaluation Summary Rating**

X ☐ **Excellent**
X ☐ **Very Good**
☐ Good
☐ Fair
☐ Poor

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

**Very Good** to **Excellent** I like this proposed work, which offers high potential for integration with current and proposed restoration work in the region to foster successful ERP goals. It also seems to offer some very interesting possible answers to basic questions regarding complex sediment dynamics and how they related to season, annual and long-term processes in discharge and land-use. The overhead on the budget, however, seems quite high (90 to almost 100%)!