Panel Scientific and Technical Review Form
(Note: Review comments will be anonymous, but public.)

Proposal number: 2001-C212 **Short Proposal Title: Large Scale Sediment Flow and Transport Model**

1a) Are the objectives and hypotheses clearly stated?

Summary of Reviewers comments:
"Well presented hypothesis." "While this is clearly good research that is proposed, there is also an element of risk involved in what is a fairly theoretical approach with a huge basin being modeled. My concerns primarily involve the scale of the project that seems to be proposed in a somewhat independent fashion."

Panel Summary:
Panel members felt that some of the hypotheses presented were well stated, but others not so well.

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

Summary of Reviewers comments:
"Yes. The concept is to measure the geomorphic processes of the Sacramento River in order to develop a predictive model for decision makers for future restoration projects."
"Yes. The types and extent of data upon which to base the model are fully explained."

Panel Summary:
Panelists found the conceptual model appropriate for a large-scale overview, but such a view may not be of great utility to CALFED.

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

Summary of Reviewers comments:
"...it is unfortunate that the research is not more closely tied to actual in-stream projects that would provide useful settings for model validation. By operating on the scale of almost the entire Sacramento drainage there is limited (but possibly critical) opportunity to really evaluate empirical sediment dynamics that are operating on local spatial scales." "Perhaps the proposer could consider working more closely with restoration managers along the River who could contribute in this manner." "Documentation of the approach selected, as well as alternate approaches that were rejected, is thoroughly discussed."

Panel Summary:
The panel found this is an extremely optimistic approach, proposing work of a very large scale, and would normally be attempted by a large, experienced team of researchers.

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

Summary of Reviewers comments:
"...it seems that model development and testing could be done in a more cost-effective manner on a smaller scale watershed, particularly if development is not successful. If such a project could be funded as a pilot or demonstration project, then research could proceed and, if successful, fuller-scale implementation could be supported at a later date." "This is a research proposal..."

Panel Summary:
This is research.

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

Summary of Reviewers comments:
"The proposal is to develop a predictive model, so to that extent it is useful for future decision making." "Many other workers are conducting related work in the Sacramento Basin, and while I'm sure that the potential for co-ordination is there, it is unfortunate that the research is not more closely tied to actual in-stream projects that would provide useful settings for model validation."

Panel Summary:
On Page 6 of the proposal, panelists noted the investigator indicated that, "In a later stage, if time and resources permit, we will develop the floodplain component model..." which represents a major uncertainty on whether the proposal will generate information relevant to future decision making.

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

Summary of Reviewers comments:
"The component of the research proposal that deals with collection of actual field data does not describe the statistical basis for sample design or frequency, although the collection methods are standard."

"This is in some ways the weak element of the proposed research, particularly integrating results of model development with resource management. I discount to some extent the overall importance

of immediate application of the project results, since this project is more research-focused than typical CALFED proposals, but it is still not clear how model validation will be accomplished, following the parameterization. The 'Expected Output' is reasonable, esp. given the long-term (decadal) relevance of the processes involved, but the linkage between most-likely outcomes and actual field conditions was not particularly strong." "Not clear if the ultimate outcome will be easily usable by someone who is not technically capable of understanding the parameters used in building the model. If it is user friendly then it could be a very valuable tool." "Yes. Sound and thorough data management techniques are described and will result in data being accessible to all users."

Panel Summary:
The panel observed the use of a proprietary model "FLUVIAL-12" is an integral element of this proposal, and this might preclude convenient public use of data and model outputs that could be produced.

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

Summary of Reviewers comments:
"This is where a closer collaboration with other workers would be helpful, so that model application and testing could go hand-in-hand with empirical results." "Yes."

Panel Summary:
This proposal is attractive in concept because it is a classic modeling situation, but success hinges on a tough data collection and even tougher validation to make it useful. The panel thought it unlikely the model would be testable in a local context, and thus its predictions would not be useful.

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

Summary of Reviewers comments:
"This modeling effort is obviously a long-term proposition, and it may be unreasonable to assume (both by the proposer and by the funding agency) that a 3-year project will yield the comprehensive and robust 'predictive model' that is anticipated. Annual variation in degree and timing of discharges, and all the other natural and management 'contingencies' that take place in this watershed will certainly mean that a longer period of time will be needed to build comprehensiveness. Again, I don't expect immediate results for basic research (with applied application), but maybe this reality should be more fully discussed." "Yes. The proposal uses well-documented and tested protocols. In the text of the proposal floodplain modeling is characterized as being done "if time and resources permit," therefore it is unclear as to whether covering this aspect of the work will be produced. The order of project work products (Table 1 of the proposal) appears to treat floodplain modeling as an afterthought to the main work."

Panel Summary:
The panel considers the work proposed to be technically feasible, but has reservations about the practical problems a sole investigator would encounter that would limit the utility of the model.

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

Summary of Reviewers comments:
"It appears the applicant is qualified to complete the work as proposed." "Singer has conducted some interesting work related to large river hydrology, although the lack of a large research and publication record is a bit of a concern. On the other hand his advisor, Thomas Dunne, is a leading authority on sediment transport and large river hydrology, and will presumably be intimately involved in this research even if not based in the region. Singer has 'communicated' with USGS researchers involved in Sacramento. R. sediment dynamics, but this does not constitute a 'project team'." "The principal investigator (Michael Singer) is the only person identified. His qualifications appear excellent for the work to be undertaken."

Panel Summary: The panelists were concerned that this proposal would not be feasible if implemented by a sole researcher.

5) Other comments
The panel felt it would be unwise to fund a project of this scale by a principal investigator with no official university affiliation. The portion of the proposal the panel members found most likely to provide new and needed information was the investigation of sediment deposition processes and rates on flood plains.

Overall Evaluation
PANEL SUMMARY COMMENTS

The panel was impressed with the ambitious scale of the proposal but considers this only fair to good as a prospect for funding.

Summary Rating

Excellent
Very Good
Good X
Fair X
Poor

Your Rating: FAIR to GOOD