Draft Individual Review Form

Proposal number: 2001-K205-2

Short Proposal Title: The influence of discharge, temperature and fine sediment on ….. anadromous egg survival.

1a) Are the objectives and hypotheses clearly stated?
Provide detailed comments in support of your conclusion [Note: in the electronic version, this will be an expandable field]

The main objectives are clearly stated, and at this level the proposal is very clear and direct. It is obvious that discharge, temperature and grain size will be examined as variables that affect salmon egg survival. I give the proposal very high marks for this overall statement of purpose.

The four hypotheses to be tested are also clearly stated. This is another strong point of the proposal, and leaves no doubt as to the direction of the *field* phase of the project. Laboratory hypotheses or contributions are not specifically addressed in this section, and aren't mentioned until several pages later. The stated hypotheses could apply to field or natural conditions without any laboratory experimentation, and would benefit from additional clarification of the proposed laboratory work.

Data needed to test the hypothesis are sometimes discussed, but are not linked directly to hypotheses 1-4 as outlined in the introduction. This issue will be addressed in section 1b2.

1b1) Does the conceptual model clearly explain the underlying basis for the proposed work?
Provide detailed comments in support of your conclusion [Note: in the electronic version, this will be an expandable field]

Background information and the conceptual model clearly explain the need for this type of study. Controlling factors that affect redd site selection and juvenile survivability are known, but the natural environment is highly variable and we are just beginning to analyze the importance of these factors in the microenvironment. Studies of the hyporheic zone are an emerging focal point, and very little is known about physical or geochemical conditions in the hyporheic zone and their influence on site selection and mortality of anadromous fish. This is a critical issue, and deserves more attention in projects such as this.

1b2) Is the approach well designed and appropriate for meeting the objectives of the project?
Provide detailed comments in support of your conclusion [Note: in the electronic version, this will be an expandable field]

Several problems may prevent the project from meeting all of the stated objectives. These issues will be addressed in terms of the four hypotheses outlined in the proposal.

Hypothesis 1: *"Hyporheic flow direction and magnitude differ with different discharge regimes and fine sediment load"*.

The good news is that I agree with the general concept: changes in stream discharge may affect flow in the hyporheic zone. Having said that, I don't see sufficient level of detail in this proposal to test the stated hypothesis. Data used to test the relationship between flow direction and discharge are not discussed. How will river stage be determined? How close is the nearest gaging point? What data will be used for streams that are not immediately downstream from a dam? Will gain and loss over shorter reaches of the stream be considered? Will the investigators do any stream

gaging themselves. The issue of hyporheic flow vs. discharge regimes *could* be addressed on a broad scale using seasonal plots of stream discharge vs. gain or loss in the hyporheic zone, but this has not been outlined in the proposal. As a side note- if this is the intended strategy, a single, simple graph for each study area might suffice to answer the question. In this case hypothesis 1 should not be a major consideration in the three year budget.

My main issue with hypothesis 1 is the inclusion of fine sediment load as a controlling variable in discussions of hyporheic zone flow. How would changing the fine sediment load affect flow direction? I can't see how this would be significant, and there is probably a wording issue with the hypothesis. It may be possible for fine sediment load to affect flow magnitude, but this concept is not developed. What scale are we talking about? Is the concern individual redds, or a larger reach of the river? If the issue is on the scale of individual redds, the only significant change would be an extremely high fine sediment input during the 60-90 day period that the eggs incubate and larval fish escape (see hypothesis 4). Is this likely to happen, and can it be documented? It is also possible that the intention is a broader comparison of bed load (grain size) between Deer Creek and the Sacramento River. In this case the issue is not individual redds, and the proposal should include a sampling plan that compares grain size distributions between the rivers. Much of this information may already exist. In either case a more detailed description of field methods and project design is necessary, since descriptions of data used to test this hypothesis are limited to a sentence or two. There is no clear indication of the number of samples to be collected, sampling method, sample size, method of analysis etc.

In spite of this negative comment I think the authors probably understand more than is written and may have a good idea. I'm suspicious that the geochemistry of the hyporheic environment (oxygen and trace metal content) would be affected by fine sediment input, and this is probably a larger issue than the stated problem of flow direction and magnitude. Are there any plans to examine the geochemistry of hyporheic zone pore water? A limited data set from a pilot study is included in the proposal, but this concept is not developed. Hypothesis 4 (below) does a better job of addressing this issue.

Hypothesis 2: *Hyporheic flow direction and magnitude influence salmonid egg survival"*.

This is a different issue. Field studies outlined in the proposal are probably satisfactory to determine flow direction during spawning. My problem here is the laboratory phase. Laboratory construction and model testing is a significant budget item, yet the only detail given is a diagram of flume dimensions. I would like to see much more detail about how salmon eggs will be placed and monitored in the flume. How will survivability be determined in a fast-flowing flume? The plans for the flume do not appear to have a mechanism for switching between influent and effluent conditions. Will influent and effluent conditions both be examined? Previous workers have documented spawning in both influent and effluent conditions. With this in mind, a larger issue for me is the effect of geochemical environment and grain size. Will these variables be addressed? Will the geochemical environment and grain size be altered experimentally? What about the issue of lower oxygen content during upwelling- can this be modeled? I think these issues of microenvironment are probably more important than the stated question of flow direction and magnitude, and unless some of these variables are included the project does not seem to justify the three years of laboratory experiments.

Hypothesis 3: *"Sacramento River hyporheic temperatures produce higher egg survival than Deer Creek temperatures in early-arriving fall chinook redds…"*

Numerous studies have documented survivability and the importance of temperature to anadromous fish, so at first glance a test of this hypothesis doesn't seem to provide new information. I agree completely that this is a limiting variable, but there are so many differences between Deer Creek and the Sacramento River it will be hard to prove that survivability is

controlled by differences in temperature. The project description does not give details of this phase of the study, other than a pilot study plot of river temperatures. If this is the only data set collected, hypothesis 3 can be addressed by a technician with an internet connection in about 3 hours. If experimental (laboratory) studies are involved, this phase of the project should be described in much more detail, and should be linked to other experimental variables like grain size, oxygen content or flow velocity to provide new information.

Hypothesis 4: *"Sacramento River fine sediment deposition is lower in anadromous salmonid redds and produces high egg survival than fine sediment loads in Deer Creek redds."*

Good idea. This is a testable hypotheses. Now I would like to see more detail about sampling plan, number of samples, sample size, disturbance to the redd, etc. Will sediment traps be used? Will these be bulk (grab) samples? What is considered "fine" sediment? Will the invertebrate fauna be considered. This has the potential to provide much useful information, although description of the field phase lacks detail. Now- will this also be part of the experimental (laboratory) phase? This hypothesis should be testable in the flume, but laboratory analysis of fine sediment input is not mentioned. Once again I'm left wondering when and why the flume will be used.

1c1) Has the applicant justified the selection of research, pilot or demonstration project, or a full-scale implementation project?
Provide detailed comments in support of your conclusion [Note: in the electronic version, this will be an expandable field]

Very little justification is given or needed. This is clearly a research project, as opposed to pilot, implementation, planning or education. My one concern here is the budget request for "outreach". This seems excessive for a research project, and details are not specified. I would prefer to see less than three person-months per year for "outreach", or more justification and explanation of the educational objectives of the project. How many meetings per year, what forum, are there connections with other civic or school groups, etc.

1c2) Is the project likely to generate information that can be used to inform future decision making?
Provide detailed comments in support of your conclusion [Note: in the electronic version, this will be an expandable field]

The field phase of this project should produce information that can be used by scientists and planners during restoration projects and management decisions on river systems. Flow direction and magnitude in the hyporheic zone is a very poorly understood process, and the general design of the field project is geared toward providing useful information. I'm not as confident that the laboratory phase will provide useful information, since the relationship between water temperature and salmonid egg mortality is already well established.

2a) Are the monitoring and information assessment plans adequate to assess the outcome of the project?
Provide detailed comments in support of your conclusion [Note: in the electronic version, this will be an expandable field]

Monitoring and assessment are not described in detail outside of the budget time estimates. It would be beneficial to include specific details about project design (see above) and implementation that include monitoring schedules and evaluation of data. Much of this is grouped into general catagories on the budget, but few details are described in the proposal. It is difficult to

assess potential outcomes with the limited descriptions available, especially for the laboratory phase 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?
Provide detailed comments in support of your conclusion [Note: in the electronic version, this will be an expandable field]

Details are vague here. Time is budgeted for data manipulation and analysis, but specific methods are not discussed in the proposal. One of the few details of data collection seems to be the type of paper used in the field. It would be beneficial to discuss sampling frequency, piezometer distribution, number of samples, number of sites etc. One specific statistical test is mentioned (ANOVA), but this is not sufficient for a project of this scope. Will other statistical comparisons be used? Is computer time or analysis involved? What graphical displays will be used? In a general sense the issues of data collection, management, analysis and dissemination are covered in the budget, but details are sparse. Time allocated for the task of data collection (observation?) may be excessive for this project, since time is also budgeted for field mapping of redd distribution.

3) Is the proposed work likely to be technically feasible?
Provide detailed comments in support of your conclusion [Note: in the electronic version, this will be an expandable field]

Field work is highly feasible and very valuable. Laboratory work (construction of the flume) is feasible as far as it is described in the proposal, although the outcomes are questionable.

4) Is the proposed project team qualified to efficiently and effectively implement the proposed project?
Provide detailed comments in support of your conclusion [Note: in the electronic version, this will be an expandable field]

This project team is highly qualified, and has all of the necessary credentials to complete the task. Although Walter Duffy is eminently qualified for this project, his role is not well defined. Why is his resume included if he is not mentioned on the budget narrative? Will he be one of the level 3 managers, or will he supervise a graduate student? This begins to look like padding.

Miscellaneous comments
[Note: in the electronic version, this will be an expandable field]

No additional comments.

Overall Evaluation Summary Rating

- ☐ Excellent
- ☐ Very Good
- ■ Good
- ☐ Fair
- ☐ Poor

Provide a brief explanation of your summary rating [Note: in the electronic version, this will be an expandable field]

This review is critical of some aspects of the proposal, but in a general sense the project addresses worthwhile issues and attempts to fill a gap in the scientific knowledge base. The field phase

of the project is especially valuable, and would advance our understanding of flow in the hyporheic zone. Detailed critique of the hypotheses, data collection methods, proposed laboratory study and size of the budget request lower the ranking of the proposal to “good”. Laboratory methods and data collection are not well defined and do not seem to justify the requested dollar amount. The “outreach” and “management components also seem excessive and poorly justified. I would support a project of this nature with a scaled-back scope, and limit the majority of the funded research to field data collection, with a minor component of data analysis and writing.
