May 2, 2005

Dan Ray, Grants Officer
California Bay-Delta Authority
Ecosystem Restoration Program
650 Capitol Mall, 5th floor
Sacramento CA 95814

Subject: ERP Proposal- California State University, Sacramento; Effects of structural enhancement on salmonid spawning submitted by Tim Horner

Dear Mr. Ray,

Dr. Tim Horner, CSUS, recently submitted a proposal to the CALFED ERP. I am in strong support of it, not only because the information would be beneficial to Chinook salmon spawning habitat enhancement I am responsible for on the Mokelumne River but because it has excellent merit.

I was quite surprised to see that it was not recommended for funding and that it received a "moderate" review score. Specifically, I would like to point out several aspects of the final reviewer's comments that are contrary to the overall External Review.

1) Specifically, the final review stated that all three external technical reviewers noted significant problems with study design, including the lack of suitable controls.

Rebuttal: In fact, the second reviewer states that the proposal, "...does an excellent job presenting theory, developing a conceptual model...using hypotheses that will be directly testable...linkage is explicit and clear". "Approach is well designed and appears to directly build upon field methods developed in previous investigations".

2) The overall synopsis states that "A major downfall of the study was that the authors were not able to make a strong case for how the insights gained from the study could be used by decision makers..."

Rebuttal: According to the Draft CALFED Gravel Augmentation Panel Report (Section 7.1.2. Knowledge Gaps), "Although much is known about what comprises suitable spawning habitats from a structural and hydraulic perspective, there are several little understood factors that may under some circumstances significantly influence gravel use by salmonids. First, although cover type and availability have been postulated as key factors influencing gravel use, there have been no definitive studies conducted to test these hypotheses. Cover type and availability could be important factors in large scale gravel augmentation plans, where opportunities exist for linking cover elements (e.g. boulder clusters, large woody debris (or LWD), root wads, etc.) into gravel augmentation concepts".

Therefore, the proposal stems directly from a request by CALFED to study this subject. The placement of structures is expensive and time consuming, yet little is know about how it influences spawning adults. This suggests there is a strong case for such a study and this is clearly stated in the proposal. If woody debris and boulders are not beneficial to spawning adult fish, they should not be incorporated into spawning enhancement sites. This information appears to be very important to decision makers.

4) Finally, the final review stated that the project team's... "track record for publication (beyond gray literature) and outreach is very limited"

Rebuttal: Considering both authors have published in such journals as: Regulated Rivers, Canadian Journal of Fisheries Management and Aquatic Sciences, Transactions of the American Fisheries Society, and the Geological Society of America, I wonder what is an acceptable publication to this person?

Mr. Ray, it is important to note that the proposal seeks to hire two graduate students to perform the study. It seems somewhat strange that the final reviewer seeks a level of study detail of this magnitude before an initial assessment can be made. The students will have to submit a study proposal to their advisory committee which must then accept it. It would be improper to create the entire monitoring protocol without them. Furthermore, I do see this level of detail provided in other recommended proposals. Considering CALFED has spent over $25 million on salmon spawning habitat enhancement, including the placement of structure without fully understanding the consequences of it, I sincerely hope the proposal might be reconsidered within the present budget cycle. Please feel free to contact me if you have any questions.

Sincerely,

Joseph E. Merz, Ph.D.
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Effects of Structural Enhancement on Salmonid Spawning,

Timothy C. Horner

Technical Panel Review

Technical Review Panel's Overall Evaluation Rating:
Inadequate

Explanation Of Summary Rating

All three external technical reviewers noted significant problems with the study design, including the lack of suitable controls. We believe there may be research of significant value here if the investigators re-orient the central restoration effectiveness question, design a sampling program that places the study within a broader restoration context, and make better use of existing hydrodynamic models. Additionally, we think further examination of the literature on large wood would help answer some of the questions they ask here.

Goals And Justification

The proposal does identify the Mokelumne R. gravel and structure restoration actions, although it does not go into much detail, especially about the history of structure (boulder and wood) additions. The structural enhancements and gravel additions are targeted for improving spawning and rearing conditions for Chinook and steelhead. According to the proposal, gravel has been added to the river because previous analyses suggested that suitable spawning gravel was the second most limiting factor for Chinook (the first was harvest). This proposal is to examine the effect of the structures on the location and hydrodynamic properties of redds in the vicinity of structures, and determine if the presence of structures changes the spawning behavior of salmonids in a way that can impact their fitness.

The proposal states that little is known about the flow properties of salmon spawning sites in the vicinity of

structures. We believe more is known about the influence of flow obstructions on substrate properties and salmon spawning, especially for large wood, than the proposal claims. For example, one of the more important references on the ecological functions of wood – "The Ecology and Management of Wood in World Rivers" (Gregory et al., editors, 2003) – is not cited, yet this volume contains several excellent papers that synthesize the evidence for the importance of wood in storing coarse sediment, promoting intra–gravel flow conditions that create suitable spawning sites, and providing habitat complexity that favors pre–spawning survival. All of these are elements of this proposal, yet we feel many of the research questions have already been pretty well answered.

Perhaps a more appropriate question for monitoring is "Have structure additions benefitted the Mokelumne spawning gravel augmentation project?" Addressing this question in the Mokelumne River would produce information that would be much more directly relevant to habitat restoration projects in the CALFED area. However, addressing this question will mean re–designing the study and greatly expanding the number of sampling sites. Refocusing the question would allow the investigators to look at spawning in the larger context of the entire freshwater life history of salmonids. For example, do augmented gravels become more prone to siltation when structures are present, do structures ameliorate or exacerbate redd scour during high flows, do improvements in rearing conditions overshadow changes in egg survival in the vicinity of structures, and what type of structures will function most effectively to enhance in different channel settings?

Approach

The proposal identifies 8 tasks, although two tasks (project management and data analysis) do not refer to different study elements. Two of the external technical reviewers go into considerable detail in describing opportunities to improve the sampling approaches, and their points are generally well–taken. The investigators would be aided by utilizing existing hydrodynamic models that would help address some of their initial questions and identify areas for additional

research (we suggest they contact Larry Weber with IIHR Hydroscience and Engineering at the University of Iowa).

There is a need to: (1) find a suitable control reach of the river, (2) better justify the choice of sampling locations within the treated reach (we couldn't tell if it was random or not), (3) increase the sample size (and identify what a "sample" is), (4) conduct more detailed behavioral observations of pre−spawning adults and devise a means of identifying individual fish in order to determine whether adult salmon show fidelity to particular structures, (5) measure egg survival, and (6) expand the hyporheic studies to a variety of discharge levels.

Feasibility And Likelihood Of Success

The project, as outlined, seems feasible. However, because of the need to redesign the study, expand the number of sampling sites, as well as a number of other study design problems noted by the technical reviewers, it seems likely that this will require the efforts of more investigators than are identified here. The two PIs in the proposal appear to be supervising the work of a single graduate student, but a revised proposal will require a more concerted field and analytical effort.

Performance Measures

Lack of suitable un−enhanced control sites will hamper the interpretation of study results in terms of evaluating Mokelumne River restoration actions. If it is not possible to find reasonable control reaches in the Mokelumne, then sites should be located in a nearby river with relatively similar conditions. We realize that perfect controls are never possible, yet without some type of controls, it will be impossible to answer the question of whether the structures are affecting salmonid spawning success.

This proposal, as it is formulated, will contribute to existing knowledge of the flow properties in spawning gravel in relation to structural roughness elements, and reproductive

behavior of Chinook salmon in relation to wood and boulders on the spawning grounds, but it will not adequately address the really important restoration questions relative to gravel augmentation and structure additions in the Mokelumne.

Products

The project does not appear to be closely linked with other restoration projects of a similar nature in the CALFED area, although it is assumed that it will provide some useful information relative to spawning gravel augmentation elsewhere in the region. The main products will consist of publications, technical reports, and databases archived at CSUS and EBMUD.

Capabilities

The investigators have proven research capability, and they have a good track record of publishing results in the peer-reviewed literature.

Budget

The total budget figure seems reasonable for the work described. However, if the study goes forward and more sample sites are added the cost will go up.

Regional Review

The regional review committee ranked this proposal "medium". They were primarily concerned that the study was not adequately linked to other habitat improvement project in the region and appeared to be a largely academic investigation without sufficient reference to past restoration in the Mokelumne River.

Administrative Review

There was no prior-phase funding review. No environmental compliance problems were identified. The only budget issue concerned the need to provide additional details about

cost−sharing – not a big problem.

Additional Comments

The investigators are encouraged to re−submit a proposal that addresses restoration effectiveness and makes use of newly−developed models and existing research findings.

Delta Regional Review

Delta Regional Panel's Overall Ranking:
Medium

Summary:

This proposal has considerable merit as a research proposal; however it did not focus on the primary purpose of the PSP, to monitor the benefits of past ERP projects. Results of the proposal would be site-specific to the Mokelumne River and linkages to similar restoration projects in the Central Valley were not made. Existing models used to evaluate other Central Valley gravel and instream projects were not mentioned. It was also unclear as to the biological oversight of project tasks.

1. Applicability To ERP Goals And Regional Priorities.

The proposed study will monitor and evaluate the importance of instream structures (e.g., large woody debris and boulders) on the Mokelumne River that can be applied to 73 other CVPIA and CALFED projects on Central Valley streams. There is a lack of this type of information available in the Central Valley region. The work will focus on fall-run Chinook salmon, a Big R species, as a surrogate for three other Big R species (winter-run and spring-run Chinook salmon, and steelhead) listed in the CALFED MSCS. Results of the study could be applied to high priority ERP areas like Clear Creek, Butte Creek, and the San Joaquin River tributaries. Not included in the proposal was whether the study would assess and compare results to other ERP projects with multiple species. Those performance indicators identified could be transferred to evaluate the importance of instream structures in other projects, however a model or strategy is not proposed for how this is to be accomplished. Basically, it gives you the tools to do the job, but falls short of any comprehensive evaluation.

2. Links With Other Restoration Actions.

Instream structures are important features of the salmonid ecosystem that are often overlooked and rarely quantified in cummulative effects analysis for biological assessments or opinions. This study meets the ERP rehabilitation goal by providing the tools to evaluate placing instream structure in salmon spawning habitat, a highly debated topic in the Central Valley, due to conflicts with water conveyance and human safety concerns. The study also addresses the goals of the CVPIA(Section 3406, and 3402) by protecting and enhancing anadromous fish habitats.

This project only evaluates projects on the Mokelumne River, however there are numerous other similar projects that involve restoration activities that would benefit from this type of evaluation. The proposal does rely on other monitoring efforts by CDFG and EBMUD and continues to provide long−term data on the status of earlier restoration projects on the lower Mokelumne River.

This project will provide baseline data on the influence of woody debris and obstructions on spawning behavior that is transferable to other projects. Preliminary results will be reported at an IEP symposium or at the AFS national meetings and available at CSU Sacramento. Final reports will be published in two peer reviewed papers available to the public.

Although numerous studies have shown the benefits of LWD on coastal streams in the Pacific Northwest, few have tried to quantify the impacts. The information provided fills a gap in on−going monitoring in the Central Valley. This study will provide information that can be used to evaluate similar past and future projects in other streams. A drawback to the proposal is that it does not link into recently available salmon models (e.g. CCDAM on Clear Creek, SAM on the Sacramento River, or IBM's on San Joaquin tributaries) that could be used to assess the cumulative response of several related restoration actions. There is no mention of linkage to existing monitoring in the the LMR and other Central Valley streams. The proposal does not indicate how this information

would be used in a standard assessment methodology or how it would be incoporated into existing monitoring programs such as CAMP or IEP.

3. Local Circumstances.

All proposed work would be conducted under EBMUD permits. No constraints are anticipated that would limit the success of the project. Oversight and field work would be conducted by experienced investigators. Some questions arise as to whether changes in flow and pressure can be detected and discerned from larger geomorphic effects (i.e., mid−channel islands or tailouts found adjacent to instream structures). Also, there does not appear to be a unrestored control reach for comparison purposes. Access to project sites are available through EBMUD.

4. Local Involvement.

This project coordinates with a number of different agencies at the local level (i.e., CDFG, USFWS, EBMUD, and UCD) that have all partnered in various spawning habitat enhancment projects on the lower Mokelumne River. In addition to providing the data to various sites: CALFED, IEP, CDFG, and CSU Sacramento, a series of public outreach forums are proposed to present the study efforts to local interested groups in Stockton and Lodi. The project involves staff and students from CSU Sacramento in completing the field work and thesis to be published in an accredited journal. This project is an extension of a local partnership that has proven effective in completing the numerous spawning enhancement projects to date on the Mokelumne River.

5. Local Value.

The value of the project lies in quantifying the benefits of instream structures that have been overlooked in most assessments and cumulative analyses of Central Valley streams. However, the project is more research oriented (i.e., eight different tasks, four of which are on salmon behavior), rather than monitoring projects already implemented. If performance

indicators such as: higher spawner use, physical measurements, and behavorial differences can be identified, then these tools can be used to evaluate how ecosytems respond to similar type projects. Since these performance indictors are applicable to salmon spawning habitat in all Central Valley streams, the results will be useful at various scales: local, watershed and regional.

6. Other Comments:

Using adult salmon to derive physical and behavioral performance indicators seems contrary to the majority of work done on this subject, which is usually based on juvenile responses to structure, riparian condition, shade, flow, substrate, etc.. Most models use some variable ranking criteria for spawning habitat that include instream structure or bank condition to derive differences in juvenile production. To be comparable to other methods of assessment, performance indicators would have to converted from adult to juvenile numbers.

External Technical Review #1

Goals And Justification

The authors target an important knowledge gap for gravel augmentation and spawning habitat enhancement projects in an assessment of the effects of in-stream structures on salmonid spawning. Unfortunately the proposed research misses the mark by not addressing the most relevant questions and the authors did a very poor job of developing their research plan. No clear line of logic can be followed that links the problem to a conceptual framework that is populated by the research questions and allows for a direct interpretation of how the insights gained from the proposed study can inform future gravel augmentation plans. Overall the proposal is not well developed and is of poor quality.

The proposal does identify the restoration actions that will be monitored; however, the goals and objectives of the monitoring are not clearly stated or internally consistent. Hypotheses were stated but some cannot be adequately tested by the proposed methods. The authors also show a lack of understanding in the study design when testable hypotheses were confused with 'theory'.

The weakest aspect of the proposal is the lack of a conceptual model that links the larger problem statement to the study design. No clear line of logic was developed for how the specific performance measures were selected and it is evident that the selected measures are not the most important parameters related to egg/embryo survival and/or fitness. It is also not clear how the insights gained from the study will inform future management actions.

Approach

The authors state in the abstract that an understanding of 'how and why structure' may effect spawning habitat is a critical uncertainty in designing gravel augmentation projects that will increase salmonid spawning habitat. To address this

question a process-based investigation is preferred over the simple correlative assessment proposed by the author's limited case study. If the correlative approach is used, the performance measures should be directly linked to egg or embryo survival and fitness, which they are not.

A major downfall of the study was that the authors were not able to make a strong case for how the insights gained from their study could be used by decision makers. Without substantial revisions to their approach and a more thoughtful development of their study design, the overall significance of their contributions to the broader knowledge-base will be very limited.

Technical Feasibility

The proposed project is not overly ambitious so tasks can be completed within the recommended time and budget.

Performance Measures

No clear rational was stated for how the specific performance measures were selected. Although some of the behavioral responses to changes in flow dynamics associated with in-stream structures may be of interest, they are not factors that limit productivity and the authors could not develop a clear linkage between behavioral responses and survival or fitness. It is also unclear why individual flow paths and pressure gradients will be characterized instead of measuring gravel permeability directly. If the approach is really to get at the question of: does structure force downwelling in sites where it would otherwise not occur, then a well developed strategy for selecting equivalent sites for comparison is needed. Alternatively, a well documented linkage between field measurements of permeability and survival curves from laboratory studies exists, and this directly linkage between a physical parameter and percent survival of eggs/embryos is more useful for informing management decisions than identifying site-specific flow paths. Field methods for measuring permeability are readily available, equipment is inexpensive, and a large sample size can be obtained over a

short time period. This and many other performance measures appear to be poorly suited to address the broader research questions.

Of notable omission from the project proposal are parameters such as fine sediment accumulation around structures and how structures influence gravel retention. It is also not clear how existing information and concurrent monitoring information will be used.

The question of sample size is particularly problematic. The authors proposed monitoring 8 to 10 gravel enhancement sites; however, it appears that individual structures are the experimental unit of interest. No information was presented on how many structures per enhancement site can be anticipated so the adequacy of the sample size cannot be determined. Some aspects of the study also have a smaller sample size for selectively chosen redds and/or structures that will be intensively monitored. No objective means of selecting intensively monitored sites was presented and subjective selection is likely to be bias and the sample size is too small (10 to 15 sites). It can also be expected that not all structures will function similarly because of differences in the size and orientation of the flow obstruction. No strategy for identifying sites with similar potential was developed; therefore it is unlikely that the authors will be able to account for a major fraction of the observed variation among sites. This is especially problematic given the extremely small size of wood included in the study design. In addition, parameters such as gravel permeability is highly variable over vary small spatial scales and require very large sample sizes.

To confound the problematic study design, some measurements are not being made at appropriate spatial or temporal scales for answering the proposed questions. For example, two behavioral responses of interest to the authors are survival times of females and construction time for redd building. Both of these measures require the identification and tracking of individual fish for the duration of the spawning season; however, the sample design is limited to 10 minute intervals for an unknown total duration for an unknown number of

individuals, which do not appear to be uniquely identified. Other physical measures such as depth and velocity are flow dependant and the authors did not present a strategy for identifying a representative discharge for sampling. Unfortunately some of the methods were not described in adequate detail for the reviewer to assess the quality of the data that will be collected. For example, no information was provided on whether grain size would be measured or visually estimated, and if measured, how.

No preconceived plan was developed by the authors for how to analyze and interpret data in a way that will inform future management, locally or elsewhere. By lumping together all types of 'structure' into a common category it will not be possible to infer what size or orientation of flow obstruction is most effective for producing high quality spawning habitat. By assuming that all structures will function similarly the authors can expect extremely high variability in their results and this will be especially problematic given the small sample size. Also, the authors do not identify how existing monitoring of substrate characteristics, permeability, dissolved oxygen, and temperature will be used to inform their design or how their results can inform future monitoring strategies.

Specific Comments on Proposed Research Questions: The structure and organization of the proposal could be made much clearer for the reader. Instead of ordering tasks without a clear line of logic, it would be preferable to see how each hypothesis is being addressed by each task. Here is the reviewer's best guess and some specific questions.

Task 2: Relates to Hypothesis 1, correlating redd construction with and without structure. What form of analysis will be used to determine whether redd construction near structures is greater than would be expected from a random distribution? How will the binary response variable (redds associated with or without structure) be used in the analysis? Will a logistic regression model for redd site selection be developed?

Task 3: What hypothesis is being addressed by mapping

structures?

Task 4: Addresses Hypotheses 5 through 8, regarding behavioral responses of spawners to structure. How will the authors relate the selected behavioral responses to a quantitative measure of survival and fitness? How were these response variables selected? They don't appear to be the most relevant.

Task 5: Addresses Hypothesis 3, related to vertical and streamwise velocity distributions near structures. How will velocity profiles be analyzed and how will they relate to a quantitative measure of survival and fitness? Currently, the approach sounds like a descriptive mapping effort. How many sites will be monitored? What type of sites will be monitored (i.e., what criteria will be used for selected sample sites)? What is the spatial resolution of sampling? At what discharges will velocity profiles be measured? How will the results be used to infer the potential risk of redd scour or fine sediment deposition?

Task 6: Addresses Hypothesis 2, related to hyporheic exchange near structures. The design suggests that sample sites will be stratified by size and shape of structure, which is definitely recommended, but the strategy is not explicit and the sample size may prohibit stratification if only 10 to 15 structures are monitored. How will the descriptive data be analyzed? At what discharge will measurements be made? How is it linked to survival and fitness? Why not measure permeability directly?

Task 7: What hypothesis is being addressed by measuring DO, pH, temperature, etc.?

Task 8: Data analysis of physical parameters appears very descriptive and poorly developed. A more appropriate statistical design is warranted if the results are to be made useful for decision makers. Also, explanatory variables such as 'structure classification' and average gradient were not discussed in the field methods section.

Is Hypothesis 4, relating to differences in particle size categories, addressed under Task 2? If so, will broad size

classes be visually estimated or will pebble counts be conducted? If size classes are used than very large differences in bed texture will be required if the study is to detect a difference. Why is fine sediment sampling not included? How will 'adjacent sites' not associated with structure be selected?

Products

A major downfall of the study was that the authors were not able to make a strong case for how the insights gained from their study could be used by decision makers and many of the critical knowledge gaps for understanding the role of structure in gravel augmentation sites were not addressed. Although some insights could be gained from this small−scale case study, they are unlikely to produce results that can be generalized and applied to other gravel augmentation projects. It is also doubtful that many of the results could stand up to peer review, especially given the lack of experimental design, small sample size, and subjective or vaguely described methods.

Procedures for data handling, storage, and dissemination were identified and adequately meet the needs of the program.

Capabilities

The project team has demonstrated knowledge and familiarity with the project site. Unfortunately their track record for publication (beyond gray literature) and outreach is very limited. The mix of disciplines is desirable; however, the physical habitat and behavioral response measures were not well integrated in the study design.

Budget

Budget is reasonable and adequate.

Additional Comments

The authors could bring desirable expertise to the CALFED program and the topic is definitely of interest to science and management. It is unfortunately that their proposal was not thoroughly developed. I encourage the authors to develop a new proposal for the next round of submissions but the current proposal is sufficiently lacking (conceptually and procedurally).

External Technical Review #2

Goals And Justification

The proposal provides an extensive review of issue of gravel augmentation in the Mokelumne River and provides a comprehensive review of the relevant literature.

The proposal does an excellent job of presenting exist thoery and develops a plausible conceptual model of the effect of woody debris and boulders on salmon spawning. It further proposes to test sub−components of the model using hypotheses that will be directly testable with practical field observations. This linkage is explicit and clear.

While the justification for the proposed program was thorough in general terms , however, did not make a explicit linkage back to program − the practical fish management and economic justification that we need to know restoration practices are biological effective as well as cost effective. Doing this would further justify the work.

Approach

Task 1 Project Management

No comments. Looks fine

Task 2 Conduct Redd Surveys

The approach for this task is well designed and appears to directly build upon field methods developed in previous investigations. The surveys use proven methods with adequate season duration and intensity to map spatial and temporal prgression of chinook salmon redd construction. However, elaboration of the objective of the redd survey is warranted. If representative sites are being chosen for the investigation then a complete spatial temporal redd inventory is not necessarily needed. The role of the complete temporal and spatial mapping of redds should be clarified. Implictly, I

expected this be a way to randomize redd site selection for the investigation. Consideration should be given to whether the proposed investigation provides data useful for some other management purpose (fish management, time series of chinook salmon spawning location at restored/ unrestored sites) so that the full benefits can be accounted for.

Minor questions:

a) What is the accuracy and precision of the GPS and will this be sufficient enough to provide resolution needed for the investigation? I expect previous investigations have refined this but this is a key information requirement for repeated surveys like this. b) Are there contingencies for high flows? Again have previous investigations shown the pins and anchors to hold in during floods from fall rainstorms?

Task 3 Map Locations of structure (LWD and boulders)

The approach for this task is well designed and appears to directly build upon field methods developed in previous investigations. The surveys use proven methods to map woody debris and boulders at spawning sites.

The rationale for the number and specific site selection is unclear. The proposal indicates that 8–10 gravel enhancement sites will be chosen to evaluate the association of redds to structure. Two experimental design issues are: 1) non random selection of sites for investigation, and 2) no control (no gravel enhancement). Field programs are never perfect but there needs to be some rationale why these can not be achieved or further consideration to achieve them. The full power of the spatial and temporal redd map developed in Task 1 should be used for site selection. It may also be prudent to direct effort to unrestored locations to provide control and more insight into preferred design for gravel enhancement sites.

Minor Question:

It is unclear why the ‘9 square meter’ habitat area requirement for chinook salmon is applied. Did Merz 2004 use

this? I'd be very suspicious of this because that parameter represents the average of a very wide range of observations. Since all relevant features (redds, woody debris, boulders) will be GPS located why not let the data estimate the right value for that parameter for the Mokelumne itself?

Task 4 Conduct Behavioural Studies

The approach for this task follows field methods and analysis procedures developed in previous related investigations for other rivers. While a good general description of the work is provided several practical details of the survey methodology are omitted. These include: a) What is the feasibility of elevated visual observations? The feasibility of the proposed methods are site specific and difficult in large rivers. Depending on water clarity during spawning (not usually a problem below directly reservoir, but tributary input of suspended sediments during floods can preclude observation) and feasibility of using existing or erecting suitable observation platforms over spawning areas. b) What is the experimental design? It is unclear how redds will be selected for observation, randomization considerations, the intensity of the effort, the frequency, or total duration of observation. These should be clarified

Task 5 Characterize surface water flow near woody debris and boulders

This component of the work is straightforward in the field technique, however, layout of the sampling is less clear. There is no indication of the spatial layout or resolution of flow velocity and depth measurements, and how the area will overlap or not overlap with measurements of redd location and depth/velocities. Is the approach to data collection ad hoc in that local areas surrounding the structural element will be measured to produce a map of velocities or will a large map be produced which includes the whole representative area where redds are being measured in the other tasks? How are measurements at constructed redds, fish locations in behaviour studies, and structural elements to be explicitly integrated in both space and time.

Task 6 Characterize hyphoreic flow near woody debris and boulders

The approach for measuring hyphoreic flows has been successfully applied by the investigators previously. While these field techniques are less straightforward than for surface flows surface in Task 5 a similar series of question arises: how are the 10−15 representative individual structural elements going to be selected in relation to the presence of or absence of redds, are locations without wood debris going to be investigated to act as control sites, and how are hyphoreic flow measurements going to be linked to redd locations.

Is the purpose purely to describe flow around embedded objects? There is value in understanding how subsurface flow is affected by woody debris as this provides a piece of evidence but this has already been demonstrated (ie Morita and Horner 2004). It is more powerful to link these observations to redd locations (i.e. spawning site selection). Is this going to be attempted, if so, how will it be accomplished.

Does hyphoreic flow vary with discharge? How will the proposed work deal with variation in discharge? Will the continuous monitoring sites be applied to infer effects of discharge variation? How will the sampling be timed to capture potential variation? These questions could be clarified.

Task 7 Measure field parameters

The stated objective of task to provide information on subsurface water quality in relation to salmonid embryo development. While the intent of this study and methods of this study appear to be fairly clear and shown to be feasible, there is little description of the sampling layout or experimental design. Is the objective here to determine whether flow 'deflectance' resulting from embedded structural elements causes reduce intragravel water quality?

To draw inferences about the linkage between structural elements−> hyphoreic flow−> redd construction the intragravel

water quality measurements should ideally also be extended to redds and in a layout that has areas with and without redds or structural elements. The act of constructing redds will likely cause localized changes in hyphoreic flow and because of that we expect micro (sub meter) scale variation in intragravel water quality. Will measurements be made in the location of the redds? If so where will the measurements be taken – at the upstream tip of the redd or at the egg pocket?

Quarterly field sampling events are planned. However it is unclear if index locations will be utilized for those measurements to capture seasonal variation typical to the reproductive cycle (i.e. spawning site selection through to emergence). It is important to provide this characterization over full temporal period of the reproductive cycle. Another question is the seasonal variation in water quality parameters – is four surveys enough to properly describe that variation?

Task 8 Data analysis and statistical methods

As per comments in previous Tasks above more consideration of the potential for integration among redd data, surface flow, sub surface flow, and behaviour should be undertaken. While it is most convenient to treat the conceptual hypotheses of the program as independent, it is possible (if not likely) that these attributes work together to help drive spawning site selection and survival of embryos. This is a common thread though comments on each task above.

Technical Feasibility

The proposal is likely correct in the statement that the work is feasible and can be practically implemented on the Mokelumne. I base this on the understanding that the work is largely based on techniques develop by the investigators and have been successfully applied in the past.

Performance Measures

As a result of the investigative nature of this program it is very difficult to develop meaningful performance measures

reflecting the success of the work in helping to provide better information for gravel enhancement programs.

Products

The products from this research have a high probability of providing designers of gravel enhancement programs in the Mokelumne and other regulated California salmon rivers valuable information to help improve the effectiveness of gravel augmentation programs for providing effective chinook salmon spawning habitat.

Capabilities

The investigators for the proposed program have proven capability to conduct the research. In addition the proposal has demonstrated that the investigators are very competent and knowledgeable about the methodologies proposed and scientific investigation.

Budget

The budget for this work is very reasonable and adequate for the work proposed. The investigation team have developed a cost effective approach and as a result of association with academic institution have been able to use per diem rates that are less than what is general expected in the 'consultant' market.

Additional Comments

This was a very high quality proposal that is expected to have a very high probability of success. The proposal is well organized and was very easily to read. The investigators have demonstrated a clear understanding of the issue, the theoretical background and related research. I recommend its implementation.

My primary concern was to do with experimental design. The design of the study appears to be largely descriptive and apriori does not emphasis cross comparisons between surface

flow, sub surface flow, structural elements and redd construction that allow deeper understanding of how all of the factors work together to influence redd site selection.

External Technical Review #3

Goals And Justification

The applicants intend to monitor a number of gravel augmentation projects that were previously funded by CVPIA's Anadromous Fish Restoration Program as well as several gravel−augmentation projects that were funded by East Bay Municipal Utility District (EBMUD). The goals of these previous projects was to increase salmon spawning habitat in the Mokelumne River. The hypothesis underlying these restoration activities is that, by increasing salmon spawning habitat, restoration managers can increase production of salmond species (principally, Chinook salmon) in the watershed. Gravel augmentation is a tool for salmonid spawning habitat restoration that is widely used throughout the Central Valley's salmon−spawning streams.

This proposal is designed to measure the effects of structural enhancements (boulders and large woody debris −− LWD) on salmon spawning behavior and the egg−deposition environment. The applicants identify both "Theories" and "hypotheses" as the basis for their monitoring efforts. (I appreciate the authors' effort to clearly lay out their conceptual model but I found their presentation of "Theories" and "Hypotheses" a bit confusing). There are several testable hypotheses regarding the effects of habitat structure on salmon spawning behavior and success. However, there were problems with the presentation of some of these hypotheses. For example:

−in some cases the hypotheses could be more explicit. For example, under "Theory 1" , the authors state, "structure...influences hyporheic flow through gravel". This is testable as a two−tailed hypothesis but I imagine that the author's actually believe that certain types of structure affect inter−gravel flow in very specific ways that might be tested as one−tailed hypotheses. These "softly stated" hypotheses occur elsewhere in the proposal; the proposal would benefit from making the predictions more precise regarding the "direction" of an effect when their hypotheses/theories depend on the direction of change. See also, null hypothesis #4 on

page 9 of the proposal.

–in several cases, multiple non–exclusive hypotheses are stated. The problem here is that, when the researchers measure an effect it is not clear that they will be able to definitively reject hypotheses or accurately attribute effects to the operation of a certain mechanism. For example, the authors hypothesize both that spawning females will spend less time hiding from competitors and more time conserving energy by resting in the eddies created by structure. I imagine that it is difficult to seperate time spent "hiding" from time spent "conserving" energy. This relies on an inference about the salmon's motivations as both resting and hiding can happen simultaneously. Another problem is that these two predictions would seem to have opposite effects. This leads the possibility that the applicants will find a "beneficial" effect of struture regardless of whether salmon "using" structure take a long time or short time in redd construction.

The authors predict that salmon spawning closer to structure will require less time to construct their redds (again their hypothesis is stated as a two–tailed hypothesis ––i.e. "no difference" in redd construction time). But the authors state that they will compare differences in salmon time–budgets using total time spent on activities. If salmon spend different amounts of time building redds (and they will), then the total amount of time they spend in the activities associated with building redds should vary as well. The researchers should thus consult with a statistician to determine metrics for comparing the distribution of time/effort across different spawning activities.

Approach

The proposal includes 8 tasks. Task 1 is "project management" and task 8 is "data analysis"; these are obviously necessary and appropriate. Tasks 2, 3, and 4 are clearly relevant to monitoring the effect of the previous restoration efforts on salmon spawning success in the Mokelumne. Tasks 4–7 involve studying the effect of structure on parameters believed to be important to salmon spawning success. The question to be

answered here is: "how do boulders and LWD affect physical conditions in and above the spawning gravel (e.g., flow rate, flow direction, DO, temperature)?" These tasks seem to be more research−oriented. In other words, they do not involve measuring change in conditions over time (monitoring) but instead, they are intended to determine the specific changes that occur around "structure" that make those areas more suitable for salmon spawning. There appears to be an assumption that "structure" (all structure?) produces benefits for spawning salmon and developing embryos.

The applicants cite studies (by one of the authors) that indicate that some monitoring has already been done on these projects and their effect on salmon spawning. Thus, this proposal "builds upon previous monitoring".

The suite of parameters the applicants hope to measure will undoubtedly increase our knowledge about the function and proper design/implementation of gravel augmentation projects. Understanding the exact effects of spawning gravel augmentation on salmon spawning and hatching success may indicate methods for improving gravel augmentation projects and/or they may suggest other factors limiting salmon recovery in tributary streams.

It would be particularly interesting (and perhaps more relevant to the "monitoring" focus of this particular solicitation) if the researchers proposed to study how all of these physical parameters change over time −−near structure and away from structure, in low flow years and high flow years, etc. Such research would not only quantify the benefits (and maybe even some detriments?) of "structure" and "gravel augmentation without structure" but would also indicate how long these effects last. For example, I can imagine that after some time of sustained flows, the differences between gravel−near−structure and gravel−away−from−structure may change (maybe they become more similar, maybe they become increasingly different).

Technical Feasibility

The project is well-documented and feasible. The project spatial scale appears to be consistent with the objectives. The applicants have not addressed the level of variability they expect to find across "structural elements". I imagine that, due to differences in orientation, size, and duration in the stream, different downed trees may produce different effects laterally and downstream. If variability is high among structural elements, the researchers may need a very high sample size. If there is spatial auto-correlation (as seems likely), then river-reach becomes the sampling unit and the researchers will need to expand their sampling effort.

Performance Measures

The researchers have identified an interesting and valid set of performance measures to study the effects of structure. The authors clearly establish the case that more research on the redd-scale and reach-scale impacts of structure needs to be done. Provided that they address some concerns, I believe this research will produce interesting results of value to managers and researchers concerned with morphological restoration of streams for salmon production.

Specific comments: For tasks 4-7. The proposed measures should be made near structure and away from structure in order to establish a valid comparison. In order to say that structure "changes" flow dynamics, it will be necessary to compare it to the "non-structure" condition. Finding appropriate comparison sites may be difficult as channel morphology variables should be standardized.

Task 7: It seems that many of the field parameters the applicants plan to measure would be modified by the act of constructing a redd. For example, the movement and cleaning of spawning gravel by female salmon may affect DO within, or flow rate through, the pores in the salmon gravel. Thus, measuring these parameters when salmon have not modified the substrate is of uncertain value.

Task 8: Simple Linear Regression is not an appropriate analysis on raw (untransformed) proportions. (See additional statistical comments below).

Task 4: The applicants should recruit the help of a biostatistician for this task. These observations are not likely to conform to standard statistical assumptions. For example, the time spent constructing redds is likely to be highly variable and non−normally distributed, suggesting a non−parametric analytical approach might be appropriate. Also, the possibility for spatial auto−correlation should be explicitly addressed in advance. For example, each reach may have flow conditions that produce a certain gravel size/sediment distribution. All the redds in this reach have to be built under similar (correlated) conditions that are not found in other reaches. Also, if a certain reach of stream attracts lots of salmon, the levels of aggression on all redds in that reach may be higher than in other reaches. If the researchers treat the redds as independent sampling units, they will find a "higher level of aggression" because there are more spawners in the reach with high aggression.

Finally, if a spawning site is highly desireable (perhaps because of the presence of structure), shouldn't we expect to see lots of competition (i.e. aggression and nest defense) at those sites? This would mean that salmon spawning near good sites have to expend more energy (on nest defense) than salmon spawning at poor sites.

These problems are not insurmountable but the applicants' will increase their likelihhod of success by consulting, in advance, with a good statistician.

Products

As stated above, I think the data gethered by this team will be very valuable (provided they refine their hypotheses a bit and consult with a statistician as recommended).

The applicants should increase their efforts to dissemenate this information. The authors plan to make this information

available in the form of a Master's thesis in the CSUS library. Not only is this not a broadly available format, but the applicants cannot promise that the Master's student will complete their degree and publish their thesis.

The publication plans (two papers in peer−reviewed journals) are satisfactory but it would be nice if they also publsihed this in some less technical (more public) outlets. That way the information would rapidly make it to the managers, consultants, agencies, etc. who actually put the stone in the streams.

Capabilities

The applicants are highly qualified to do this work. They have worked in this system before and published their results in peer−reviewed journals.

Budget

The proposal seems relatively inexpensive given the amount of data the researchers plan to produce. If the researchers need to expand the number of river reaches they are studying, they may require more funds.

Additional Comments

This proposal will produce valuable information. The statistical concerns can probably be easily corrected by consulting early and often with a statistician. The types of data collected and the methods they use to collect the data are unlikely to change dramatically (except that the number of study sites may increase) as a result of these consultations.

Budget Review

1. Does the proposal include a detailed budget for each year of the requested support?
Yes.

2. Does the proposal include a detailed budget for each task identified?
Yes.

3. Are project management expenses appropriately budgeted?
No.

If no, please explain

Project management costs appear to be part of cost sharing. Therefore, the full project management costs were not identified.

4. Does the proposal clearly state the type of expenses encompassed in indirect rates or overhead costs? Are indirect rates, if used, appropriately applied?
Yes.

5. Does the budget justification adequately explain major expenses? Are the labor rates and other charges proposed reasonable in relation to current state rates?
Yes.

6. Are other agencies contributing or likely to contribute a share of the projects costs?
Yes.

If yes, when sufficient information is available, please sum the amount of matching funds likely to be provided:

Detailed information is recommended regarding cost−share and matching funds.

Cost Sharing−Recommend that grantee provide information regarding its financial capability and stability as well as its level of commitment for any proposed cost share funds. A detailed budget of the project's proposed cost share funds should be provided prior to grant funds being awarded. A financial evaluation is recommended for grant agreements that state/claim over 30 % or $250,000 (whichever is less) of

matching funds. The evaluation will avoid likelihood of the grantee requesting an amendment to increase project funding due to lack of or miscalculation of matching funds to complete the project.

7. Does the applicant take exception to the standard grant agreement's terms and conditions? If yes, are the approaches the applicant proposes to address these issues a reasonable starting point for negotiating a grant agreement?
No.

If no, please explain:

Not clear. The proposal states that Sac State will abide by previously negotiated standard terms agreed to by the State.

Contract Language Exceptions – Proposals submitted by grantees which identify exceptions to State of California's standard contract language provisions as provided in the 2004 PSP; and/or submit alternative contract language in lieu of the State's standard contract language should be carefully reviewed prior to awarding grant funds. Review will initially be conducted by the funding agency's contract office and referred to the legal department as needed.

8. Are there other budget issues that warrant consideration?
No.

Other comments:

none

Environmental Compliance Review

1. Is compliance with California Environmental Quality Act (CEQA) required for this project?
No.

2. Is compliance with National Environmental Policy Act (NEPA) required for this project?
No.

3. Does this project qualify for an Exemption or Exclusion under CEQA and NEPA, respectively?
Does not apply.

4. Did the applicant correctly identify if CEQA/NEPA compliance was required?
Yes.

5. Did the applicant correctly identify the correct CEQA/NEPA document required for the project?
Does not apply.

6. Has the CEQA/NEPA document been completed?
Does not apply.

7. If the document has not been completed, did the applicant allot enough time to complete the document before the project start date?
Does not apply.

8. If the document has not been completed, did the applicant allot enough funds to complete it?
Does not apply.

9. Did the applicant adequately identify other legal or regulatory compliance issues (Incidental Take permits, Scientific Collecting permits, etc,) that may affect the project?
Yes.

Comments:

No listed species will be affected.

10. Does the proposal include written permission from the owners of any private property on which project activities are proposed or, if specific locations for project activities are not yet

determined, is it likely that permission for access can be obtained?
Does not apply.

Comments:

No written permission attached. However, access will be via EBMUD property, and the project will be conducted in cooperation with EBMUD biologists.

11. Do any of these issues affect the project's feasibility due to significant deficiencies in planning and/or budgeting for legal and regulatory compliance or access to property?
No.