



ARCADIS U.S., Inc.
2999 Oak Road
Suite 300
Walnut Creek
California 94597
Tel 925 274 1100
Fax 925 274 1103
www.arcadis-us.com

Mr. John Coleman
Executive Director
Bay Planning Coalition
10 Lombard Street, Suite 408
San Francisco, Ca 94111

Subject:

Peer Review of “Salmonid smolt outmigration and distribution in the San Francisco Estuary 2010. Interim Draft Report”

Dear John:

Per request of the Bay Planning Coalition, I coordinated a peer review of the interim draft report titled “Salmonid smolt outmigration and distribution in the San Francisco Estuary 2010” dated 2010 (but released in 2012) and authored by A. R. Hearn, E. D. Chapman, A. P. Klimley, P. E. LaCivita, and W. N. Brostoff.

As here applied, “peer review” connotes a process intended to be identical to that employed by the editors of most scientific journals. The ultimate goal of the review is to assist the authors in improving the report, thereby enhancing its contribution to the body of scientific knowledge. The results of the review are presented in this letter.

Three individuals were selected to serve as reviewers. Having agreed to disclose their identities, their names are provided below:

1. Dr. Katherine Zehfuss of ARCADIS. Dr. Zehfuss has extensive experience analyzing fish tagging data and performing modeling in the Delta.
2. Mr. Thomas Keegan of ECORP Consulting, Inc. Mr. Keegan, Director of ECORP’s Aquatic Resources Group, is a recognized fishery expert and was involved in an earlier phase of the LTMS fish tagging effort.
3. Dr. Phillip A. Lebednik of ARCADIS. Dr. Lebednik, former Chair of the LTMS Science Assessment and Data Gaps Work Group, was the principal author of the 2004 LFR document on dredging and sensitive fish species that is cited in the report.

Date:
November 14, 2012

Contact:
Phillip A. Lebednik

Phone:
925-296-7848

Email:
Phillip.lebednik@arcadis-us.com

Our ref:
B009101.0000

Imagine the result

The review was conducted in accordance with the attached instructions to reviewers (see Attachment 1). Prior to beginning their work, the reviewers read and agreed to the terms of the instructions.

The reviewers independently performed their reviews and their verbatim comments are presented below. The first section includes general comments on the report (i.e., "overall impression") and the second section includes comments that are directed to specific sections and/or topics. Comments in the second section are arranged according to the order of topics in the report. Additionally, one of the reviewers provided summary comments which appear after the specific comments. Each comment is followed by the reviewer's name for ease of attribution.

General Comments

All three reviewers concurred in the following general comments:

1. The overall impression is that the authors and their project team have conducted a solid study and are to be complimented on the work that was performed.
2. This report is very well written: remarkably few editorial/format errors were detected.
3. Most of the graphics are of high quality and very informative to the reader.
4. The data appear to have been carefully evaluated with respect to quality.
5. The data analyses appear to be thorough and extensive.
6. While many of the conclusions that are directly derived from the results appear to have merit, a number of broad conclusions appear to be weakly supported or entirely unsupported by information presented in the report.

Specific Comments

1. Executive Summary:

The Ex. Summary clearly follows the report. I am not going to spend much time here, because if the changes I suggest are made, then I am sure the Ex. Summary would be fine. (Keegan)

A “problem statement” is included in the Executive Summary, but not in the body of the report, specifically in the Introduction. In Section 1.1, the discussion of study objectives and uncertainties is therefore somewhat disconnected from the management issues described in the Executive Summary. (Lebednik)

2. Section 1.1:

Report states “It is also important to mention that this study does not address the issues of dredging windows”; however, suggests the importance of dredging window based on this study’s results in the discussion. (Zehfuss)

I’m not sure that the goal of the project is as narrow as specified. Other resource and regulatory agencies would also be interested in the results, along with the Maritime community for planning purposes. The paragraph after the 3 research questions (RQ) (objectives?) is somewhat misleading. RQ 1 asks about the general migratory pattern of late-fall run Chinook salmon smolts, and yet it is stated that migration timing of wild or hatchery fish cannot be inferred from the timed release of experimental fish (late-fall run Chinook salmon?). Further, it is stated that this study does not address the issue of dredging windows, because of the above statement, although in the Discussion it is stated that “the implementation of the environmental work (i.e., dredging) windows may be an effective tool to protect migrating salmonids”. This appears to be a contradiction. (Keegan)

The remainder of the Introduction is generally informative, and for the most part, clear and concise. (Keegan)

Paragraph 1. While the list of study objectives appears to be reasonable and appropriate when considered in isolation, statements made in the following paragraphs give pause to the reader. For example, the first objective is to identify “general migratory patterns of outmigrating late-fall run Chinook salmon smolts,” yet in the next paragraph it is stated that “the migration timing of wild or hatchery fish cannot be inferred.” Strictly speaking, these two statements are not necessarily contradictory; however, if they are to be considered compatible, the reader is forced to assume that the “general migratory patterns” are identical for all runs (and releases) of Chinook salmon. This assumption is neither expressed nor supported by any citations. (Lebednik)

Paragraph 2. The statement in the first sentence "dredging operations did not generally take place during the periods when salmonids were migrating" contradicts the last sentence which states "salmon ... migrate through the estuary at all times of the year," as the Bay is part of the estuary. [This statement also contradicts a statement in Section 1.3.1: see comment below.] (Lebednik)

Paragraph 2. The second sentence identifies an apparently substantial limitation of the study before the key aspect of the study associated with the limitation is revealed. This seems backward. Also, the reader is left mystified as to what the benefit of the study is, because of the statement "this study does not address" the key problem. (Lebednik)

Paragraph 3. citations are needed for these statements. (Lebednik)

3. Section 1.3.1:

The statement that navigational dredging has continued "non-stop" [in the Bay] since the 1980's is highly doubtful. (Lebednik)

4. Section 2.4:

The sample size was determined based on obtaining a success rate. What is the statistical power of that sample size, based upon variability in detections, non-detections, and false detections? (Zehfuss)

5. Section 2.5:

Why specifically were January 30th and February 5th chosen as release dates? Not clear in document. (Zehfuss)

6. Section 2.6:

It is not apparent that environmental data were used in any meaningful way within the report, other than a brief showing of river hydrographs for the two years. Data are not used well in an attempt to understand differences in salmonid survival between the two years. (Keegan)

7. Section 2.7.1:

Need to mention how missed detections were dealt with. In a situation where there are a lot of tagged fish at one receiver, the collisions could cause some fish to never be detected. (Zehfuss)

8. Section 2.7.4:

Transit times and rates: I am not sure why different protocols were used for determining overall and instantaneous transit times (first detection at receiver vs. last detection at receiver). One method or the other should be used. (Keegan)

9. Sections 2.7.4 to 2.7.7 et seq.

A number of statistical methods is employed across these sections with little evidence presented to justify their application. For the most part, the reader has to take these applications on faith. This comment also applies to some later sections. (Lebednik)

10. Section 2.7.5:

Which affects the fish more in assessing tidal sloshing: the tidal height or X2 location? Need to explain why tidal height information was used as the independent variable here. (Zehfuss)

11. Section 2.7.7:

Why was 5 minutes chosen as a “cut-off time interval” for each site? Does the literature support that number, especially when detection collisions may cause some non-detections of fish actually present at the site? (Zehfuss)

Why was the sampling design at the placement sites chosen? Fish, especially sturgeon species, can have an affinity for a distinct location based on certain environmental factors, even if that location has been impacted. Therefore an adjacent control site may not be appropriate for comparison. Rather, a temporal control such as sampling location before placement event, and after event. (Zehfuss)

The document states that the number of fish were calculated at EACH (?) potentially dredged site? Exposure time would imply that the test fish were being exposed to a toxin, which is not indicated without sediment chemistry testing. I find a problem with the general design, in that sediment plume testing was not conducted, or even discussed. We have no idea what concentrations of anything exist anywhere within the control or the "affected" site. Also, since there was no dredging involved (as would be expected in such a BACI experimental design) I don't understand the use of the two (control vs. affected) areas. I am not sure what is being controlled for.
(Keegan)

Firstly, although this section is titled "Residency and Exposure Times" I cannot find the word "residency" used in this section. Only exposure times are discussed in the text. Application of the term "residency" to organisms that are engaged in long-distance migration does not seem to be appropriate, in my opinion. Given the broad audience for this report, I am concerned about the use of the word "exposure," as this could have connotations related to bioaccumulation or some acute or chronic toxic effect. It is neither a stated objective of the study nor do the data presented in the report directly address those effects. Therefore, a more neutral term such as "transit time," "dwell time" or some such should be substituted for "exposure." Secondly, unless I misunderstand the text, I am concerned about the methods. One infers that there is an experimental design as there are two locations: "Alcatraz control" and "Alcatraz SF11" (the presumed treatment; labeled as "Disposal Site" on Figure 16). However, there is neither mention nor description of a study design. It is unknown therefore what is being controlled for or what the null hypothesis or alternatively the predicted outcome is. There is no justification presented for the location being suitable as a "control." [Furthermore, in non-laboratory studies such elements are best referred to as "reference areas" because their conditions are not under the control of the investigator and, while the conditions are presumed to be suited to the investigator's purposes, data justifying such presumptions typically are unavailable.] Figure 16 is somewhat confusing until one realizes that there are two arrays. Because the upstream-downstream arrays are identically labeled, one assumes that the detectors are paired but then one can find no discussion of this in the text. The relationship of the detector locations to the calculations is unclear. The graphic of this figure is of concern. While a grey box appears to depict the disposal site, there is no comparable area for the control site. It is unclear what a "hit" for any detector

means for the analysis. For example, if a fish comes within 100 ft upstream of detector A (top) is this a hit? What if the fish does not transit across the grey box? How is this possibility addressed in the analysis? All of these questions are somewhat moot absent a study design. (Lebednik)

12. Section 2.7.8:

The detections obtained from Green sturgeon appear to be sought out, rather than obtained methodically. It is mentioned later in the report, but perhaps it needs to be mentioned here as well. (Zehfuss)

13. Section 3:

Throughout results section, parametric and non-parametric tests are often used without explanation of a normality test. (Zehfuss)

14. Section 3.2:

The previous two sections did not adequately explain the differences between 2009 and 2010 sampling designs and releases, so Table 4 is difficult to interpret. (Zehfuss)

I am generally troubled by the apparent high mortality of salmon and steelhead upstream of the 'start of the study area', but wonder why more effort was not spent on attempting to describe differences in detail. What is causing this mortality, and is it affecting fish within the study area at similar or different rates? (Keegan)

Table 4. The table title refers to italicized data, but I find none in the table. One of the largest losses of tagged fish occurs between the release site and the Benicia Bridge, yet little discussion of this fact appears in the text. More information on this initial experience of the population under study may inform the interpretation of downstream results. Were there selective events associated with surgical procedures or other test-specific "error factors" or is it assumed that the losses were "random?" Is this initial loss factor consistent with other similar studies and can one presume that the test population did not experience initial events that could bias the outcome? (Lebednik)

15. Section 3.3:

Although this properly belongs in Discussion, It appears that the authors have no idea what is causing the changes in dispersal patterns. Yet, there is no discussion of potential reasons, such avoidance behavior by salmonids, predatory behavior of striped bass and other species, etc. (Keegan)

The speculations regarding dredging effects as related to CI seem overly simplistic and conceptually flawed. The nexus between an index of geographic migratory behavior on the one hand and an instantaneous and/or spatially restricted activity on the other, cannot be established without additional information. Such information is neither presented in the report nor cited. (Lebednik)

16. Section 3.4:

The comparison of STH versus LFC results is highly informative. Transit time is a function of fish swimming (direction/duration x speed) and more importantly water flow. There is no analysis in this section of flow patterns in the discussion of transit time data nor of the relative positions of the fish within the flow fields (however, “tidal sloshing” is discussed in Section 3.5.2). (Lebednik)

17. Section 3.5.1:

A conclusion is made that the “majority of migrating Chinook salmon would have been exposed” to dredging; however, 24 successful migrations out of 500 released is 4.8% exposed; discussion should emphasize mortality issues before they reach the Bay. Same comment concerning the percentages given for the steelhead. (Zehfuss)

Statements regarding percentages of LFC being detected “in areas which are dredged” appear to be dubious. The conclusion that the majority of migrating Chinook salmon would have been exposed to dredging operations in the absence of dredging restrictions associated with environmental work windows is unsupported by the analyses presented in the text. To validly address the subject of exposure of fish to dredging operations, the authors would need to present data on the spatio-temporal distribution of dredging and disposal operations, consider avoidance behavior, etc. (Lebednik)

18. Section 3.5.2:

Tidal elevation changes impose temporal variations in 3-D aspects of the habitat (e.g., emergence of shallow areas, decrease in depth, cross-sectional variations, etc.) which could have appreciable effects on the spatial patterns of fish migrating in the shallower portions of San Francisco Bay. One could posit that, if fish preferentially transit a reach at low tide, then the intertidal portion of the Bay in that location would be unavailable for fish passage. Such considerations would seem to be appropriate in this and some later sections. Given that the section is titled "tidal sloshing," the authors appear to presume that water surface elevation correlates directly with the current field. Use of tidal height as an index of tidal current is of dubious validity, considering forced-wave (i.e., tidal) flow dynamics and the distortions of these dynamics as the wave propagates across an estuarine system. In Section 2.6, the authors refer to river discharge and regional precipitation data. It would seem that the movement of fish discussed in this section should be considered in relation to the discharge information. (Lebednik)

19. Section 3.6.1:

In comparing Flats versus Control, 18 Chinook were located in both, which should be an additional class variable (i.e. both). (Zehfuss)

Again, it would be helpful to have a description of the study design.
(Lebednik)

20. Section 3.6.2:

Cross-section arrays: I am wondering if velocity is a factor here (determining transit patterns), rather than depth (or in addition to depth), especially given observations by Tiffan et al 2009 (Section 4.1.2). Velocity differences at depth and at channel vs. flat locations should be undertaken. (Keegan)

I wonder if the vertical detection statistics of the receivers are comparable to the horizontal detection statistics described in Section 2.3. Can one presume that all of the data shown in the figures have a 96% detection rate associated with them? (Lebednik)

21. Section 3.7:

Residency and Exposure times: The insets on Figure 43 show that the majority of both Chinook salmon and steelhead passed through the potentially-dredged sites in less than 15 minutes. Figures 44 and 45 show that the majority of both Chinook salmon and steelhead (Figure 45 mislabeled, should be steelhead, not Chinook salmon) pass through the SF 10 placement site within 45 minutes. These are significant findings because they show the rather rapid outmigration rate of both species. A review of LD 50s should be included, showing the time-mediated effects of the expected contaminants present in SF Bay, I believe that would show little chance for uptake of most contaminants in such a short time period (this should be present in the Discussion, but set up here). In addition, MacFarlane's (2010) observation (Section 4.1.2) that salmonid smolts gained little energy during their presence in SF Bay, further indicates the reduced potential for contaminant uptake, especially in such short time period. (Keegan)

I am wondering if the results of figures 50 and 51 (green sturgeon detected across arrays at Bay and Richmond bridges) are also related to velocity, rather than depth. (Keegan)

See comments on Section 2.7.7 above. (Lebednik)

22. Section 3.7.1:

Again as commented above, 141 fish out 500 released is 28 %. There needs to be more explanation of what causes the unsuccessful migration percent. (Zehfuss)

23. Sections 3.7.2 through 3.7.4:

Again as commented above, need another class variable for "both". In addition, juxtapositioned control sites do not seem adequate; event/temporal control would be more robust. I am not sure about the validity of the assumption of receiver's accuracy, thus leading to the scale-up. Vemco does state that there may be variability in receivers detections based on noise, etc. Also, the exposure time distribution looks Poisson (E-test perhaps?). (Zehfuss)

24. Section 4:

General comment: Some of the statements in this section need to be reconsidered in light of the comments above. (Lebednik)

25. Section 4.1:

The discussion begins with a discussion about the environmental work windows. The introduction of the report stressing that no conclusion about dredging can be made. The control sites used for placement testing sites were not robust enough to make the statement "many of fish would be exposed to operations". Also, many fish would be exposed to non-placement sites. The report would be enhanced with literature cited as to the striped bass predator issue, especially at release sites. (Zehfuss)

The first sentence states that the study 'confirms that the implementation of the environmental work windows may be an effective tool to protect migrating salmonids', which on first glance appears to be contradictory to the Study Objectives (1.1). The statement is based on the following statement that '75 % of salmonids may have been at risk if dredging or dredge placement had been permitted during outmigration'. This statement is based on the precept that all potential dredging and dredge placement projects would be operating simultaneously, which would never be the case. The document further states that many fish would be 'exposed' multiple times due to 'tidal sloshing'. I am left wondering, exposed to what? There is no discussion of any particular contaminants present or any specific indication of toxicity. The document also states the possibility that juvenile salmonids would be transiting the SF Bay estuary every day of the year. This is misleading, given the very large difference in seasonal outmigration patterns and timing of emigration of all races of salmon and steelhead. During some months, few juvenile salmonids emigrate through the system. Further, MacFarlane's (2010) observation that salmonid smolts gained little energy during their presence in SF Bay (cited above in Section 3.7) also applies here, in that significant contaminant uptake is not likely, particularly during such short potential exposure times. (Keegan)

The statement "...greater than 75% of salmonids may have been at risk if dredging or dredged material placement had been permitted during their outmigration..." is not supported by evidence presented in this report, as

indicated in an earlier comment. This assertion is further confounded by the statement that salmonids [may] transit the Bay “every day of the year.” (Lebednik)

26. Section 4.1.1:

There is a discussion of the differences in migration success between years, with the observation that these results may be counter intuitive. There are also some throw away comments suggesting possible reasons for migration differences. This section requires more thought and discussion. (Keegan)

27. Section 4.1.2:

The conclusion of this section that repeated tidal sloshing is harmful to the smolt's chances of successful outmigration needs more thought. The finding that fish preferentially utilized the channel sections of the estuaries, over that of the shallow water (i.e., flats) habitat is significant, and should be looked at in greater detail, and perhaps should be reflected in Discussion regarding the protection of salmonids by existing environmental windows. Perhaps the windows are currently over-protective. (Keegan)

28. Section 4.1.3:

Again as commented above, I am not sure about the validity of up scaling method. The conclusions of entrainment are hypothetical – this report did not show evidence as to entrainment occurring. It likely is an issue; however this report does not have the data to address it. (Zehfuss)

The statement that exposure time is generally less than 30 minutes, especially in marinas or dredged sites, again reflects the rapid outmigration of both species through the estuary and is significant. The document states that this time should be up-scaled in terms of the entire site. Perhaps the extent of dredging, especially the characteristics of the expected dredge plume, might be a more important factor. The document states that there was no evidence of fish residing at particular sites for extended periods. The document then qualifies this statement with a discussion of potential lethal and sub-lethal effects of dredging, citing older literature referring to body burdens of toxins. This discussion should include at a minimum the work being conducted on the Columbia River that documents avoidance behavior

of fish to dredge plumes, and to specific characteristics of dredge plumes (low DO, high sedimentation rates). (Keegan)

29. Section 4.2:

The discussion of the distribution of green sturgeon in SF Bay is interesting and well written. It is apparent that more data are necessary to make conclusive statements relative to their distribution and migration patterns. I am looking forward to the discussion of the next set of data to be presented in the next report. (Keegan)

30. Summary Comments

I do not believe that the statement confirming implementation of the environmental work windows as an effective tool to protect migrating salmonids is warranted. The fish distribution and abundance data need to be put into a population level assessment, where a more thorough discussion of the effects of suspended sediments and the likelihood of particular contaminants in SF Bay is necessary (i.e., population modeling) to be able to make statements regarding protection measures. There is no baseline population information or survival information against which these two years of data can be compared. In fact, there is no real description of why the two years of salmonid survival data are so different, other than they seem to be counter intuitive. (Keegan)

I think the objectives are sometimes contradicted by the results and discussion of the document, whereby the objectives call for an understanding of the general migratory patterns of Chinook salmon and steelhead, and to what extent their distribution coincide with the location of dredged material and placement sites. This almost seems to be a last minute add-on and may reflect the differences between the objectives of the UC Davis – NOAA Santa Cruz Laboratory riverine survival study and the LTMS study, which is more interested in dredging activities in the bay. More attention needs to be directed toward the dredge and dredge placement sites, and a comparison of these sites with the rest of the SF Bay environment needs to be considered before protective tools can be adequately assessed. (Keegan)

References Cited

MacFarlane, R. B. 2010. Energy Dynamics and growth of Chinook salmon (*Oncorhynchus tshawytscha*) from the Central Valley of California during the estuarine phase and first ocean year. Canadian Journal of Fisheries and Aquatic Science 67:1549-1565.

Tiffan, K.F., Koch, T.J., Haskell, C.A., Conner, W.P., & Steinhorst, R.K. 2009. Water Velocity, Turbulence, and Migration Rate of Subyearling Fall Chinook Salmon in the Free-Flowing and Impounded Snake River. Transactions of the American Fisheries Society 138:373-384. DOI: 10.1577/t08-051.1.

I and the other reviewers appreciate the opportunity to perform this peer review.
Thank you for inviting us to conduct this effort.

Sincerely,

ARCADIS U.S., Inc.



Phillip A. Lebednik, Ph.D.
Principal Scientist
Review Coordinator

Copies:
Tom Keegan
Katherine Zehfuss

Attachment 1:
Instructions to Reviewers

Attachment 1. Instructions to Reviewers.



ARCADIS U.S., Inc.
2999 Oak Road
Suite 300
Walnut Creek
California 94597
Tel 925 274 1100
Fax 925 274 1103

MEMO

To:
Technical Reviewers

Copies:

From:
Phillip A. Lebednik, Ph.D., Review
Coordinator

Date:
October 29, 2012

ARCADIS Project No.:
B0090101.0000

Subject:
Instructions to reviewers regarding "Salmonid smolt outmigration and distribution in
the San Francisco Estuary 2010. Interim Draft Report."

Ethical Guidance

In agreeing to serve as a peer reviewer of the subject report, the reviewers concur in meeting the following ethical requirements:

1. Conflict of Interest: You agree that you do not have any professional or financial affiliations that may constitute or be perceived as a conflict of interest.
2. Qualification: You agree that, by education and/or professional experience, you are qualified to serve as a "peer reviewer" of the subject report.
3. Objective evaluation: You agree that you are able to perform an impartial (objective) review solely based on scientific principles, practices and methods.
4. Confidentiality: As the subject report has been publicly released, you are free to distribute said report. Your review comments should be viewed as confidential [within the Bay Planning Coalition (BPC)] unless/until BPC authorizes their release.

If you do not concur with one or more of the above, you should immediately notify the Review Coordinator.

Review Guidance

As stated above, the purpose of your “peer review” is to perform an impartial (objective) review solely based on scientific principles, practices and methods. Suggestions for improvement are encouraged. For this review, there are no instructions regarding specific technical questions to be addressed. Your review should include as appropriate, but need not be limited to, the following general considerations:

1. Problem statement – Was the “problem statement” (i.e., ultimate goal or use of the information) clearly presented and was existing information comprehensively considered?
2. Objective(s) - Were the study objectives appropriate for the described problem and is there definition of the extent to which the objectives address the problem?
3. Study design – Was the study design appropriate and were suitable control/quality measures incorporated?
4. Methods – Were the methods appropriate with respect to the objectives and study design?
5. Data – Were calibrations required and were they appropriately performed? Were the appropriate data obtained, properly “managed,” and subjected to acceptable quality assurance and control procedures?
6. Results – Were the evaluations of the data clearly described and were they appropriate?
7. Conclusions – Were the conclusions warranted by the results, consistent with the stated objectives of the study and appropriately discussed with respect to the inherent limitations and uncertainties associated with the study and related knowledge?