



National Marine Fisheries Service Comments on Cramer Fish Sciences Tech Memo # 1
Estimation of Returns of Naturally Produced Coho to the Klamath River

General Comments:

General comment #1:

Reclamation and their consultants have not informed the reviewers with a proposal for responding to comments. While we are appreciative of the opportunity to review and provide meaningful comments, it is important for NMFS to understand how these comments will be used to affect and hopefully improve the resulting life cycle model. We are concerned that the accelerated timeline for reviewing and commenting on portions of the model does not allow time for the modelers to consider comments and respond prior to moving ahead with next steps. This may lead to commenters responding in a redundant fashion, and an inefficient use of our time. We request that Reclamation and their consultants clarify the review process with consideration on how comments will be used to improve the model. We also recommend that future comment periods for technical memos be increased to at least a 30-day comment period.

General comment #2:

Progress toward recovery of Southern Oregon Northern California coasts Klamath Basin coho salmon will be measured at the level of populations and diversity strata and we recommend presenting the results in this context. In all of our comments regarding the conceptual framework of the life cycle model, including supporting documentation prepared for Cramer Fish Sciences by Lestelle (2006), we have recommended using the framework of historical population structure presented in Williams et al. 2006. The Williams et al. report identifies 9 putative historically independent populations within the Klamath River Basin: Lower Klamath, Middle Klamath, Upper Klamath, Salmon, Scott, Shasta, South Fork Trinity, Lower Trinity, Upper Trinity. In contrast, Tech Memo #1 identifies 8 “coho producing reaches” that only partially correspond to the historical populations identified by Williams et al 2006. Specifically, the Salmon, Scott, and Shasta are consistent between the two reports. The Trinity is lumped in the Tech Memo, instead of being treated as three distinct units (one largely behind a dam). It may be impossible to derive individual estimates for each of these populations because of area-specific data deficiencies, however the memo would benefit if the estimates for the Trinity Basin were displayed as three putative historical populations.

Additionally, the mainstem of the Klamath is treated separately from the tributaries. There is no problem with identifying mainstem and tributary “production areas” within these populations; in fact, it is important to highlight the fact that it is assumed that the lower and middle mainstem reaches are not used by coho spawners.

General comment #3:

The results of this memo are difficult to assess without having a clear understanding of how the information will be input into a life cycle model. The magnitude of acceptable error in these estimates may be entirely dependent on how the numbers are used. In this regard, it is imperative that any subsequent model outputs thoroughly test how uncertainty around estimates may affect model outputs.

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General comment #4:

The document identified ten “distinct coho producing reaches,” then used eight separate methods for determining population size in these reaches. The only method used twice was the “assumed zero” method that was used for the lower and middle mainstem Klamath River reaches. Some of the methods were unfamiliar to the reviewers and there was a general lack of references to similar population estimation studies, making it difficult to evaluate the merits of each method. Specific comments for each method are in the paragraph specific methods.

General comment #5:

The authors appear unfamiliar with the Harvest Sampling Approach for determining number of naturally produced coho salmon returning to the Klamath and there were no references to studies using a similar methodology, therefore it is difficult to evaluate the merits of this method. It would be interesting to see how well this method estimates returns where run size is known, and we recommend running simulations in river systems where number of fish migrating over dams is known.

Specific Comments:

Page 1, last paragraph – Spatial structure is not addressed.

Page 2- The introduction could be better-organized and more informative. The first paragraph should be used to fully explain why estimates of natural returns are important to the life-cycle modeling. Specifically, how will these results be incorporated into the life-cycle model? Without this knowledge, we are unsure of the potential effect of assumptions that are made in deriving these estimates.

Page 3, paragraphs 1 and 2- In our previous status reviews for SONCC coho, NMFS expressed there are limitations with using Willow Creek weir (WCW) data for population estimates because the data set is truncated (since 1996, there has been only one year when the weir was not removed by Nov 18).

Page 3, first paragraph- The “Harvest Sampling Approach” does not predict distribution of coho salmon in the Klamath Basin.

Page 5, first paragraph- In the year the WCW was operated beyond November 18 (week 49), five fish were enumerated. It seems tenuous to assume that the migration is “100% complete” by Nov 18. Should also note that in 1992, 1993, and 1995, when the weir was operated until the first or second week of December, appreciable numbers of fish (up to 26% of fish captured in 1993 and 18% in 1995) moved through between Nov 18 and December 2. Also, we recommend the authors consider the influence of hatchery fish on the migration pattern of Trinity River coho.

Page 6, third paragraph- Some sensitivity analysis would be useful here. For example, how much would the differences in timing shown in Figure 3, panel 4, influence an overall population estimate? Without knowing how sensitive the estimates are to potential biases, it is difficult to assess the assumption.

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Page 8, first paragraph- The formula for escapement estimates provided on page 14 assumes a natural:TRH ratio for coho salmon trapped in the lower river that greatly favors hatchery fish over wild, yet the 2004 Yurok trapping data reveals a natural:TRH ratio of 1.6. The authors should address the conflicting data since it could imply that hatchery and natural coho salmon run timing may not be similar.

Page 9, second paragraph- Please clarify if Portuguese Creek is included in the upper Klamath or middle Klamath production reach. Do the boundaries correspond to Williams et al.? If so, we recommend a discussion of how the “distinct coho producing reaches” compare to the populations identified in Williams et al. (2006).

Page 9, last paragraph – More information than just the estimated average number of unmarked IGH coho salmon returns is needed to support the “diminimus” argument (see comments for Figure 5).

Page 9, last paragraph- Why round numbers when numbers are exact counts? This rounding error ranges from 0.25% to 8%. Why are numbers rounded to 100?

Page 10, first paragraph- More information about the extent of the surveys would be helpful. What percentage of the upper Klamath Reach was surveyed in each year?

Page 10, Figure 5 – This figure should include number of hatchery returns to IGH and proportion of “mis-marked” fish for each year. This would help support the “diminimus” argument.

Page 10- The modelers may be walking down the path toward a self-fulfilling prophecy. For example, no fish in mainstem now, no fish in mainstem Klamath River ever. Is that an expected assumption, or will the model consider the possibility that restoration of mainstem habitats could lead to greater use by spawners?

The authors assume that as many as 100 spawning coho mainstem Klamath River is “negligible.” Given the status of SONCC coho salmon, this description is incorrect and inappropriate.

Page 11, paragraphs 1 and 2- The anomalous year when abundance monitoring was conducted until late December (presumably because of lower flows) may not be an appropriate estimator of long-term run timing.

Page 12, last paragraph – Why were numbers rounded to the nearest 100?

Page 13, second paragraph – The number of redds (939) is different from any previously mentioned redd count and there is no year given for this count.

Pages 13-14, Scott River section – The Scott River is the only “distinct coho producing reach” discussed that shows such a wide variation in year class strength and may be the only “reach” in which two of the three broods are in immediate danger of extirpation.

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This illustrates the need for a modeling framework that assesses limiting factors at the population level.

Page 14, second paragraph – The authors should define “over-seeded,” provide more evidence of “over-seeding,” and state the years in which they think the Scott River was “over-seeded” before presenting it as a “fact.” There may be several explanations for the somewhat high fish to redd ratio. Redds can be superimposed even at low population densities. The highest density described is approximately 20 redds per mile of identified spawning habitat and does not constitute characterization as an “extremely high density.”

Page 14, last paragraph – Why are the numbers rounded?

Page 15, second paragraph – The cited work of Sumner (1952), Shapovolov and Taft (1954) and Salo and Bayliff (1958) all investigated coho salmon run timing as it occurred within small, coastal watersheds where run timing can be highly influenced by “freshets”. Tributaries of the upper Klamath River do not share the same characteristics as the watersheds within the referenced studies, and the authors should not assume that coho salmon migration patterns and stimuli are similar between the two distinct systems.

Page 16 – It appears that the entire analysis of Bogus Creek escapement is predicated on one years data (i.e., 2004), seriously eroding any confidence in the resultant estimate. Furthermore, the Bogus Creek escapement estimate is later extrapolated to other basins, further weakening the overall estimate of natural coho salmon escapement within the entire Klamath Basin. Finally, the timing of coho salmon returning to Bogus Creek has likely been highly influenced by past harvest practices at IGD. Extrapolating the Bogus Creek escapement numbers to tributaries located far downstream and are unlikely affected to the same degree by IGD practices or straying rates may lead to inaccurate estimates.

Page 17, paragraph 3- The IP model was developed by Burnett et al. (2003) and applied to the Klamath basin by NOAA (Agrawal *et al.* 2005). Also, the citation of Hicks and Hall (2003) as a basis for excluding reaches above 4% gradient is inappropriate. Hicks and Hall did not analyze any streams with gradients greater than 4%. Rather, they make a general statement about coho being scarce in the Drift Creek basin in reaches with gradients > 4%, citing the master’s thesis of Schwartz (1991). Schwartz (1991), in fact, surveyed 7 reaches with gradients greater than 4%. While most steeper reaches were not occupied by coho salmon, one reach with a gradient of 4.6% was occupied at a fairly high density (more than 25% of the maximum observed in any reach). Also note that Burnett’s Ph.D. work in the Elk River drainage found coho in reaches with gradients > 4% in certain years, explaining why her IP curves include 5% (Note also that Burnett is not spelled correctly in the report).

The use of IP to expand estimates is tenuous. IP is a model of the *potential of a reach to exhibit habitat characteristics favorable to coho salmon under undisturbed conditions*. It is not a predictor of current productive capacity. The attributes (low gradient, wide valley, moderate discharge) that lead to high IP scores also make these high-IP reaches

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most responsive to anthropogenic disturbances (e.g., low-gradient, unconstrained reaches respond very differently to increased sediment loads and loss of wood and other structure than steeper, unconstrained reaches).

Page 17- This method appears to assume that each stream mile with an IP of greater than 0.2 is occupied at the same density that Bogus Creek was in 2004. This approach assumes there is a known relationship between coho in Bogus Creek and nearby tributaries. Because of the proximity of Bogus Creek to Iron Gate Fish Hatchery, hatchery coho likely comprise a higher proportion of the annual abundance of Bogus Creek adult coho spawners than other tributaries in this production reach. Therefore, Bogus Creek adult coho abundance is likely inflated relative to other upper Klamath tributaries and does not serve as an accurate estimator of other tributaries. We suggest obtaining fish distribution data from California Department of Fish and Game and using those to better assess validity of assumptions regarding distribution of coho salmon in Bogus Creek and other tributaries.

Page 18, first paragraph – The paragraph states that Shasta river and IGH returns did not express such differences in brood strength, but did not state what was being used for comparison. We assume the comparison was to Scott River returns. If so, it would be helpful to know why the authors think coho salmon populations in Bogus Creek and other upper miscellaneous tributaries fluctuate more like those in the Shasta River and IGH than to those in the Scott River.

Page 19, paragraph 1- Some effort should be made parse out abundance estimates for the three putative independent populations within the Trinity subbasin. If nothing is known about how Trinity River fish are distributed, then this should be clearly stated.

Page 19, paragraph 4- How does the variability in freshwater and marine survival influence this estimate (*i.e.*, how do you derive an estimate with confidence limits, when you have such highly variable survival estimates?)

Page 19, second paragraph – Why were estimates rounded?

Page 19, Table 8 – Table 8 is confusing. If the intent is to show estimated numbers of natural and hatchery adult returns to WCW, we suggest a two column table with estimates of natural and hatchery adult returns from Appendix 17 in (Sinnen *et al.* 2006). Another option would be a four column table showing estimated returns of natural and hatchery adult and grilse from Appendix 17.

Pages 19-20, Klamath River Tributaries between Trinity River and Estuary – If the numbers in Table 9 are correct, then 14 adult returns in 2002 produced 33,812 juveniles, or 2,415 juveniles per spawner, or approximately 4,800 juveniles per female. This level of productivity is unrealistic (100% survival; egg to juvenile). Conversely, 1,483 adult spawners in 2004 produced only 7,188 juveniles, or approximately five juveniles per spawner. Assuming a 1:1 sex ratio and 4,000 eggs per female, egg to juvenile survival

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would be 0.25% in 2004. These disparate results signify the need to re-evaluate the use of these data before proceeding further with them as part of the life cycle model.

Page 21, first paragraph – The memo did not explain the need for knowing a population size for the entire Klamath River drainage. There should be some description of how the population estimates will be used in the model.

Page 21, second paragraph – Both methods are largely dependent on estimates of naturally produced adult returns to the WCW so it is not surprising that the trends are similar. The degree to which WCW returns are driving the estimates should be discussed.

Page 21, last paragraph- The Brown *et al.* estimate of 1,860 fish was based on a “20-fish” rule, where it was assumed that any tributary still thought to support coho salmon had a hypothesized population of 20 adults (the 1,860 number is the product of 93 tributaries x 20 adults per tributary). Brown *et al.* acknowledge these estimates as essentially best “guesses” of abundance. To compare the Brown *et al.* population estimates to those derived in the tech memo, using a different methodology, then concluding a 7-8 fold increase in abundance, is inappropriate.

Page 22, paragraph 2- The text in this paragraph appears misleading. Please reword to reflect that IP is, as noted earlier, an index of the potential of stream reach to exhibit habitat characteristics favorable to coho salmon. To state that the “greatest availability of high quality coho habitat is located in the Scott and Shasta watersheds” is not supported. Historically, this may have been true, but IP does not address current conditions. Similarly, stating that “the low estimated production of coho in the Salmon subbasin is as predicted by the IP method” is also not an appropriate use of IP, as current production is likely substantially lower than historical production. Williams et al. 2006 conclude that the Salmon River historically likely had sufficient capacity to support a viable population.

Page 22, Table 11 – There are ten reaches listed here but eight listed on page 9. It would be helpful to add a column listing the methods used to determine population for each distinct coho producing reach.

Literature cited

Sinnen, W. M. Currier, M. Knechtle, and S. Borok. 2006. Annual report Trinity River basin salmon and steelhead monitoring project 2004-2005 season. State of California, The Resources Agency, Department of Fish and Game. Northern California – North Coast Region, 601 Locust Street, Redding, CA 96001. 148 pp.

Williams, T. H., E. P. Bjorkstedt, W. D. Duffy, D. Hilemeier, G. Kautsky, T. E. Lisle, M. McCain, M. Rode, R. G. Scerlong, R. S. Schick, M. N. Goslin, A. Agrawal. 2006. Historical population structure of coho salmon in the Southern Oregon/Northern

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California coast evolutionarily significant unit. NOAA Technical Memorandum NMFS.
NOAA-TM-NMFS-SWFSC-390. 71 pp.

NMFS Comments on Technical Memorandum #2
Simulating Fall Redistribution and Overwinter Survival of Klamath River Coho

Enclosure

General comments

1. The document appears to assume that overwinter survival limits juvenile coho salmon production throughout the Klamath River Basin. There are nine populations of coho salmon in the Klamath River drainage (Williams *et al.* 2006) and instream habitat for those populations is highly variable. Treating all coho salmon in the Klamath as one population does not address the importance of population spatial structure to the Evolutionarily Significant Unit (ESU).
2. The memo does not clearly describe the reasoning that fall redistribution is a primary component of the life cycle model. The technical memo describes movement out of spawning/rearing tributaries during a time when the fewest juvenile coho salmon actually move out of spawning/rearing tributaries (*i.e.* October through early December). If the purpose of the document is to describe the proportion of the year class using mainstem habitat during winter, it would seem prudent to also consider young-of-the year/parr that move downstream from spring through early fall, and yearlings that begin moving downstream in winter. Chapman (1962) found that coho salmon smolt migration begins in winter with a substantial proportion of smolts moving downstream by the end of February. Screw trap outmigrant trapping in the Klamath River generally begins operating in March, but the few records from December – February suggests Klamath River one-year coho begin migrating downstream in winter. However, it may be difficult to distinguish between rearing/overwintering and downstream migration for the first part of the smolt migration (Rogers *et al.* 1987).
3. Although the memorandum references a number of studies describing juvenile coho salmon use of off-channel habitat, the analysis appears to ignore the importance of off-channel habitat, and the importance of connectivity to off-channel habitat, for overwintering juvenile coho salmon.

Paragraph specific comments

1. Page 2, second paragraph. Habitat quantity “bottlenecks” and “hockey-stick” stock-recruitment functions may not be appropriate for populations at very low densities. We suggest reviewing the capacity vs. productivity discussion in Mobrand *et al.* (1997).
2. Figure 1. The figure ignores fry redistribution in the spring through early fall. Numerically, “fry redistribution” often represents most of the year class.
3. Page 2, paragraph 2. Fall redistribution is not unique to coho salmon. Some populations of steelhead and stream type Chinook salmon also exhibit fall redistribution.

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4. Figure 2. Why were data from only one year at one trap presented? At least six other screw trap records extend into late November or early December.
5. Page 3, Figure 4. Please include sample sizes with individual bars (sample sizes vary between 44 and 44,000).
6. Page 4, last paragraph. All of the cited studies regarding juvenile coho fall migration patterns took place in small, coastal Oregon streams. Small coastal drainages differ greatly from Upper Klamath mainstem and tributary reaches in respect to hydrology, habitat availability, fish migrational patterns, etc.
7. Page 5, last paragraph. In the equation estimating the proportion of fish relocating from summer to fall, it appears that adding F_E to the denominator would in essence “double count” the number of fish emigrating in the fall. The number of fish emigrating in the fall is already accounted for within the number of smolts emigrating in the spring (*i.e.*, these are the same fish).
8. Page 6, Table 1. Why was the median computed and not the average?
9. Page 7, Tables 2 and 3. These tables appear to assume: 1) all winter mortality occurs on January 1, 2) every fish migrating prior to January 1 is redistributing, and 3) all fish migrating after December 31 are smolts. All winter mortality does not occur on January 1, and Rogers *et al.* (1987) suggests that coho salmon moving downstream in February and March might not be migrating directly to the ocean. If one were to assume that all winter mortality occurred at the end of winter (approximately at the end of March) and that fish migrating in winter are actually redistributing, then approximately 20% of the summer population redistributed downstream. If one assumes that fish redistributed prior to October (probably most of the summer population) migrated downstream to the mainstem, then the percentage would be much higher. A set 20% survival rate is likely to lead to erroneous results.
10. Page 7, first paragraph. The first sentence is only correct if one assumes that: 1) parr coho salmon do not emigrate from tributary streams prior to October, 2) all winter mortality occurs on January 1, and 3) all fish moving downstream after December 31 are smolts. Chapman (1962) showed that most coho salmon emigrated from Deer, Flynn, and Needle Branch Creeks prior to October, and Rogers *et al.* (1987) found indications of incomplete smoltification in coho migrating prior to April. We do not have a copy of Ebersole *et al.* (In Press), but it is interesting that he documented fall emigration rates as high as 38.3%.
11. Page 7, second paragraph. Studies referenced in this paragraph apparently found fall-winter juvenile coho salmon emigration rates to be 19%, 52%, and 80%. It is unclear why the authors chose to rely on studies suggesting lower rates of emigration.
12. Page 8, first paragraph. According to studies referenced in the previous paragraph, juvenile coho emigration from the tributaries should be 19% for good quality habitat

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Simulating Fall Redistribution and Overwinter Survival of Klamath River Coho (assuming alcoves represent good quality habitat) and 80% for poor quality habitat (assuming mainstem pools represent poor quality habitat).

13. Page 8, last paragraph. It reads as if only tributary streams will be considered suitable overwinter habitat. Why were off-channel habitats not considered? Information presented in Hardy *et al.* (2006) should be useful for describing connectivity to off-channel habitats at different discharges.
14. Page 9, first paragraph. This paragraph appears to illustrate the importance of a variety of off-channel habitat for overwintering juvenile coho salmon.
15. Page 9, second paragraph. Given the low numbers of coho salmon currently inhabiting the Klamath River drainage, how much is density dependence likely to be driving coho salmon movement?
16. Page 9, 3rd paragraph. Stating “the number of coho exiting the mainstem into off-channel habitats is directly related to the number of fish within the mainstem” would appear to support the notion of a density dependent situation, and not density independent as the author states.
17. Pages 9 and 10, Section 3. The studies referenced in this section describe juvenile coho salmon moving into a variety of off channel habitats, but the document seems to only consider movement into tributary streams. Why is movement into off-channel habitat not considered?
18. Page 10, first paragraph. Coho movement was simulated within the life-cycle model at a rate between 3-16%, even though this range is likely higher as explained on page 9. Also, the Ebersole *et al.* study that produced the coho movement rates in question took place in the West Fork Smith River, a small coastal tributary in Southern Oregon with instream conditions unlike the upper Klamath watershed.
19. Page 10, 3rd paragraph. Why would perennial flow be a factor when estimating winter coho distribution within tributary habitat?
20. Page 10, second paragraph. Intrinsic potential (IP) does not consider habitat perturbations. Streams considered suitable for coho salmon use based on IP alone may have degraded habitat that is not currently suitable.
21. Page 11, second paragraph. From this information, we are led to assume the following: the authors reviewed two studies, one showing that 3-16% of juvenile coho salmon moved into tributary streams (Ebersole *et al.*, in press) and one showing that 9.3% moved into off-channel ponds (Peterson 1982). From those studies, the authors determine that 88-99% of juvenile coho salmon in the mainstem Klamath River moved into tributary streams and no fish moved into off-channel habitat.

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22. Page 11, 3rd paragraph. The emigration per tributary rate of 9.5% (represent the midpoint within the range of 3-16%) could be higher, as discussed within the “page 10, first paragraph” comment above.
23. Page 11, fourth paragraph. Connor and Burge (1998) and Conner *et al.* (2003) also found flow to be a factor influencing migration survival. However, the assumption that risk associated with movement between spring-summer and fall-winter habitat is similar to that for migration to the ocean needs more support, especially given the physiological changes associated with smoltification.
24. Page 11, 5th paragraph. Why is it reasonable to assume that mechanisms influencing migration survival in the fall are similar to those observed during migrations in the spring?
25. Pages 11 and 12, Overwinter Survival. Strong density dependent relations may not be appropriate for describing anadromous salmonid populations that are imperiled due to habitat degradation (Mobrand *et al.* 1997).
26. Page 13, first paragraph. Concluding that mainstem survival rates should be 40% of tributary rates does not recognize the inherent differences between the WF Smith River watershed dynamics and those of the upper Klamath River.
27. Page 13, 3rd paragraph. Not clear why the author expects that the reach specific productivity (∇) “in streams the size similar reported by Lestelle (In Review) would not typically drop below 10% (22% of 45)”.
28. Page 13, Habitat Quality Scalar (H_Q). Why is survival in off-channel habitats not described? Do Klamath River coho salmon overwinter in off-channel habitats?
29. Page 14, Reach Specific Capacity (K). Did the model used to estimate capacity consider off-channel habitat?
30. Page 14-16, Temperature Dependent Survival Scalar (T). Although the memorandum states that many factors affect size of coho salmon, it relies on a single temperature/size relation from a study of coho salmon in Washington State. Given the abundance of studies on juvenile salmonid growth, it is difficult to understand why more information was not used to determine juvenile coho salmon size at the end of the growing season. We suggest a thorough review of the literature, possibly starting with (Harvey *et al.* 2006) and (Roni *et al.* 2006) to determine factors that should be incorporated into a juvenile coho salmon growth model.

Literature cited

Chapman, D. W. 1962. Aggressive behavior in juvenile coho salmon as a cause of emigration. *Journal of the Fisheries Research Board of Canada* 19:1047-1080.

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California coast evolutionarily significant unit. NOAA Technical Memorandum
NMFS. NOAA-TM-NMFS-SWFSC-390. 71 pp.

February 16, 2007

National Marine Fisheries Service
Comments on Cramer Fish Sciences Tech Memo 3
Marine Survival of Klamath River Coho Salmon – Review Draft

Comments

General Comment 1

The fact the survival of Oregon hatchery coho salmon and Klamath hatchery coho salmon are strongly correlated is very interesting. This is evidence that a common factor is affecting both. However, it is not clear how the correlation was tested (this comment actually applies to all the analysis in this document). The document suggests that one of the correlations may have been affected by high values in a single year (1983). To avoid this issue, it may be more appropriate to use a rank correlation test (i.e. Spearman's rank test) throughout the analysis.

General Comment 2.

Nickelson's work shows substantial differences in ocean survival of hatchery and wild fish. Hatchery fish have much more variability in ocean survival than wild fish.

Specific comments.

Pages 3-4, Methods – Methods used to estimate survival rates are not clear. Please provide equations used.

Page 4, second paragraph – In-river harvest ranged from 0 to 387 adults during the time series and could have had substantial effects on survival rates. The index of marine survival described is affected by both downstream and upstream migration survival. How variation in migration survival might affect this index should be discussed.

Page 4, Figure 1 – Are estimates of average size of hatchery smolts available? If so, it would be interesting and possibly useful to regress survival against size.

Page 5, first paragraph in Methods – Methods used to estimate survival rates are not clear. Please provide equations used.

Page 5, second paragraph in Methods – In river harvest in the Trinity River ranged from 0 to 2,955 adults during the time series and could have substantially affected survival rates.

Page 5, last paragraph – The logic of this paragraph is somewhat difficult to follow, but it seems as if the data presented could support a number of conclusions in addition to the one expressed in the last sentence. We suggest more in-depth description of results that includes confidence intervals around estimates of fishery contribution and escapement rate. A more in-depth discussion of how fishery contribution, escapement rate, and hatchery returns relate to each other would also be helpful.

Page 6, Methods – Methods used to estimate survival rates are not described. Please provide descriptions of how survival estimates and variance around those estimates were calculated. The descriptions should include all equations used.

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Page 6, Results and Discussion, paragraphs 1 and 2 – The text describes weak correlations among the three coho salmon groups compared and seems to suggest that there are substantial differences. Possible reasons for these differences should be discussed.

Page 6, Results and Discussion, paragraph 3 – The first two sentences of this paragraph seem to suggest that straying rates of Iron Gate Hatchery fish increased substantially between 1977-90 and 1991-01. If this is actually what is being suggested, it should be discussed in more detail. The last sentence seems to dismiss the possibility that Trinity River Hatchery fish survive at higher rates than Iron Gate Hatchery fish. From a standpoint of resource protection, it would be prudent to consider the possibility that IGH returns do not actually underestimate survival any more than TRH returns. A comparison of population trends in the Trinity and Klamath Rivers might provide some clues regarding overall survival of Trinity River and Iron Gate coho salmon.

Page 7, Figure 2 and Page 5, Results and Discussion – Review of this figure makes the logic in Results and Discussion on page 5 seem even more unclear. If we understand correctly, the authors are saying that survival based on returns to the fishery from 1977-1984 were higher for IGA than TRH, therefore the higher survival for TRH from 1995-2001, indicated by hatchery return rates, must be bogus. We suggest the authors also consider other possibilities, such as: 1) Something changed between 1984 and 1995 that caused the observed change in survival of IGH fish relative to TRH fish, or 2) Hatchery returns from 1995-2001 provide a more accurate estimate of survival than fishery returns from 1977-1984. We think that further investigation into possibility #1 could be especially interesting given the changes in flow management that have taken place in the Trinity River since passage of the Trinity River Fish and Wildlife Restoration Act in 1984.

Page 7, Figure 3 – The three methods for estimating survival are not well described. Please provide complete descriptions of how each survival index was calculated, including all equations used.

Page 8, Figure 4 – Methods for estimating survival for CRH and OPI are not described. Please provide a description of how these indices were calculated.

Page 10, Figures 5 and 6 – Are the same methods used to estimate survival for the two time periods? If not, this should be stated. Methods for estimating survival should also be described in detail somewhere, either in the text or the figure captions.

Page 11, Methods – The methods should explain why these particular coastal upwelling and upwelling transition locations were used. Were these the same parameters used in the Oregon studies or were locations closer to the Klamath River chosen?

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Page 12, Results and Discussion, second paragraph – The first sentence in this paragraph is not adequately supported by data presented. The changes observed being due to differences in how survival was estimated is a distinct possibility and should not be dismissed so quickly.

Page 15, Conclusions – The authors should not be so quick to dismiss the possibility that TRH fish survive at much higher rates than IGH fish.

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Comments

Introduction, third paragraph – “Baseline” should be defined.

Introduction, third paragraph- The authors note that development of the relationships between environmental factors and survival is based on the “best available data”. In Tech Memo 4, the “best available data” is often highly tenuous. Particularly problematic are these following data:

(1) The outmigrant trapping data, which form the basis for describing the temporal pattern of migration in various streams. These data are made up of fish counts (uncorrected for trap efficiency) that, with few exceptions, total fewer than 50 fish in any one year.

(2) In-river smolt survival estimates used to derive the relationship between migration distance and survival. Due to a scarcity of Klamath-specific data correlating juvenile coho salmon migration distance and smolt survival, the authors have instead relied primarily on data from populations such as Snake River Chinook salmon. These survival estimates include mortality associated with the Lower Granite impoundment, which is approximately 70 km long.

These very tenuous relationships once again highlight the need to have realistic expectations about the reliability of outputs from complex models when there is high uncertainty in the values of individual parameters and, even more importantly, the aggregate of many model components. The advice of Ludwig (1999) should be heeded here:

“It is understandable that biologists, when confronted by the requirement to make recommendations based on limited data and resources, may use ‘the best information available’ to generate predictions, even though these may depend upon computer models that have quite limited validity. Such an approach is encouraged by legislation and regulations based on the expectation that scientists will be able to arrive at well-founded conclusions based on the data and resources that are available. Legislators and management agencies are encouraged in such beliefs by scientists who are anxious to promote better decisions in critical situations. But such an exaggeration of our capabilities carries a high risk of failure and subsequent disillusionment. It would be better to be more modest about our understanding and achievements, and to help decision makers understand the complex, realistic arguments that pertain to most conservation decisions (Doak and Mills 1994).”

Second page 1, Figure 1 –How will the life-cycle model address movement between reaches during the fry and early parr stages?

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Second page 3, second paragraph – This paragraph seems to assume that migration conditions in the Klamath River below Iron Gate Dam (IGD) were optimal in 2006. Monthly IGD flows during the spring of 2006 are ranked near the highest for recorded years, and survival may have been compromised due to high flow conditions. This may explain why the 2006 survival rate of 87% per 100 km seems low for actively migrating smolts in unimpounded reaches. As a comparison, several Snake River stocks have higher migration survival rates (Table 1).

Table 1. Survival to Lower Granite Dam of yearling anadromous salmonids PIT tagged in the Salmon River drainage. Migration route includes approximately 100 km of Snake River impounded above Lower Granite Dam. The “free-flowing” portions of the migration route for Snake River sockeye salmon, Sawtooth Valley steelhead, and Lemhi River Chinook salmon are severely impacted by water diversions which could reduce migration survival.

Stock	Mean Survival to Lower Granite Reservoir	Distance to Lower Granite Dam	Mean Survival per 100 km
Snake River sockeye salmon	0.45	747 km	0.899
Sawtooth Hatchery steelhead	0.61	747 km	0.936
Lemhi River Chinook salmon	0.63	595 km	0.925
Marsh Creek Chinook salmon	0.52	630 km	0.901

Page 4, paragraph 1- Inspection of Table 4 in Appendix A indicates that the numbers of fish collected annually in most of the outmigrant traps in the Klamath Basin are very low: of the 49 year and trap combinations, only 6 had counts of smolts that exceeded 100 fish. Counts at Big Bar on the mainstem totaled 25 or fewer fish over the seven years sampled. The assumption that the temporal distribution of migrants is represented by this extremely small number of fish seems highly tenuous.

Page 4, paragraph 2- The text needs to clarify how mean passage date and standard deviation were calculated. What is meant by peak date? Is it the date that the largest number of fish passed? Is it the date where the 50% percentile was reached (which would be more accurately described as the median date)? Was the peak date calculated based on counts or the “abundance index” derived by multiplying counts by flow? How is the peak date defined with respect to time (*i.e.*, is it a specific day or the midpoint of a “julian week”)? A brief summary of the methods should be provided in the main text (and the Appendix needs to be clearer about how this date was derived).

Page 4, paragraph 1 reference to Appendix- The method for converting smolt counts to an abundance index (by multiplying counts and mean flow for the period) is not supported by any analysis or citations of peer-reviewed science. The lone reference provided to support the approach is an unpublished report prepared by the authors;

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however, a review of this document suggests that the flow/efficiency relationship may be more complicated than the linear/proportional relationship proposed. Some literature support of this assumption is warranted.

Page 4, paragraph 2- The approach to developing normalized curves has the potential to introduce significant bias that may affect model outcomes. In particular, when estimating the average “peak date” of migration, weighting by the total number of fish captured in a given year (note: it should be clarified if this is the total smolt count, or the total estimate adjusted for flow) may introduce substantial bias.

There are two distinct issues to consider, though both are tied to populations exhibiting substantial interannual variation in migration timing. Weighting by total count means that a year of high abundance will exert disproportionate influence, even if those years do not represent the typical migration pattern. Likewise, a year of low abundance that exhibits an early or late migration will have little influence on the normalized curve, even though such patterns may occur on a fairly regular basis. This is particularly problematic with the existing Klamath data because there are few years of reliable counts. Nevertheless, Spence (1995) found that standard deviation of the peak migration data for coho populations in Oregon and northern California averaged more than 16 days, indicating that interannual variation is the norm, not the exception. Weighting each year equally (rather than by the number of fish counted in each year) improves the probability that the full suite of potential migration patterns is represented, though does not completely resolve the problem of poor data quality.

The second issue is how a generalized curve will affect model outcomes. Generalizing a curve to the “average peak date”, even if the curve “accounts” for a large standard deviation, means that the model will tend to favor outcomes in which flow is maintained above thresholds during a relatively narrow temporal window and that maintaining flows outside this window are of minor importance. A goal of flow management in the Klamath River should be to ensure that flows are sufficient to maintain the natural diversity in migration timing exhibited by smolts, not just the “central tendency.”

Page 6, paragraph 1 – These data do not seem to support the assumption that all fish migrate to the estuary in a 2-week period. Paragraph 2 on page 6 describes a substantial proportion of fish holding for more than two weeks during the migration, and Figure 4 shows downstream migration extending for 3-4 months.

Page 6, paragraph 2 – Substantial numbers of fish holding and apparently rearing during the downstream migration is interesting and should be investigated further. Adding a habitat availability component to this portion of the life-cycle model seems prudent.

Page 6, Figure 4- The text should discuss the poor quality of data used to develop these curves. In this regard, the figure caption is deceptive, implying that 8 years of trapping data form the basis of the figure. In reality, Table 4 in Appendix A shows that the curve

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for Big Bar is based on a total catch of just 64 smolts over a 6-year period (excluding the year that was discarded)—an average of just 10 fish per year. The data for the Kinsman Creek trap is only slightly better, with 119 fish captured over 4 years (avg. 30 fish per year, excluding years that were discarded). Unbiased Trinity River weir counts are based on just four years of data, with the average count of 184 fish driven largely by a single strong year (574 smolts in 2002). These very marginal data yield substantial uncertainty about whether the proposed curves truly represent the temporal migration patterns within the Klamath River.

Page 7, paragraph 2 – A better description of flows in 2006 relative to historic flows should be given to support the assertion that migration conditions were “optimal” in 2006.

Page 8, Table 3 – All of the Snake River stocks had to migrate through Lower Granite Reservoir, which probably decreased survival substantially. Lower Granite Reservoir is about 70 km long so fish from Dworshak hatchery, which is 116 km from Lower Granite Dam, migrated through 46 km of free flowing river and about 70 km of impoundment. Fish from Sawtooth hatchery migrated through 677 km of free flowing river and about 70 km of impoundment. Average and best survival rates per 100 km are inversely related to the proportion of the migration distance that is impounded (Figure 1). The intercept from these relationships might be reasonable approximations of survival rate through the unimpounded portion of the migration routes. However, even the unimpounded portions of the migration routes for most of these stocks are severely impacted by human activities (*e.g.*, water diversions, grazing, altered hydrographs), therefore even the intercept from the highest survival relationship may not represent migration survival under “optimal” conditions. Likewise, the Yakima River system is highly impacted by water diversions, agriculture, and other human perturbations. Consequently, it is not clear what relevance these estimates have in establishing a “baseline” survival rate for the Klamath system.

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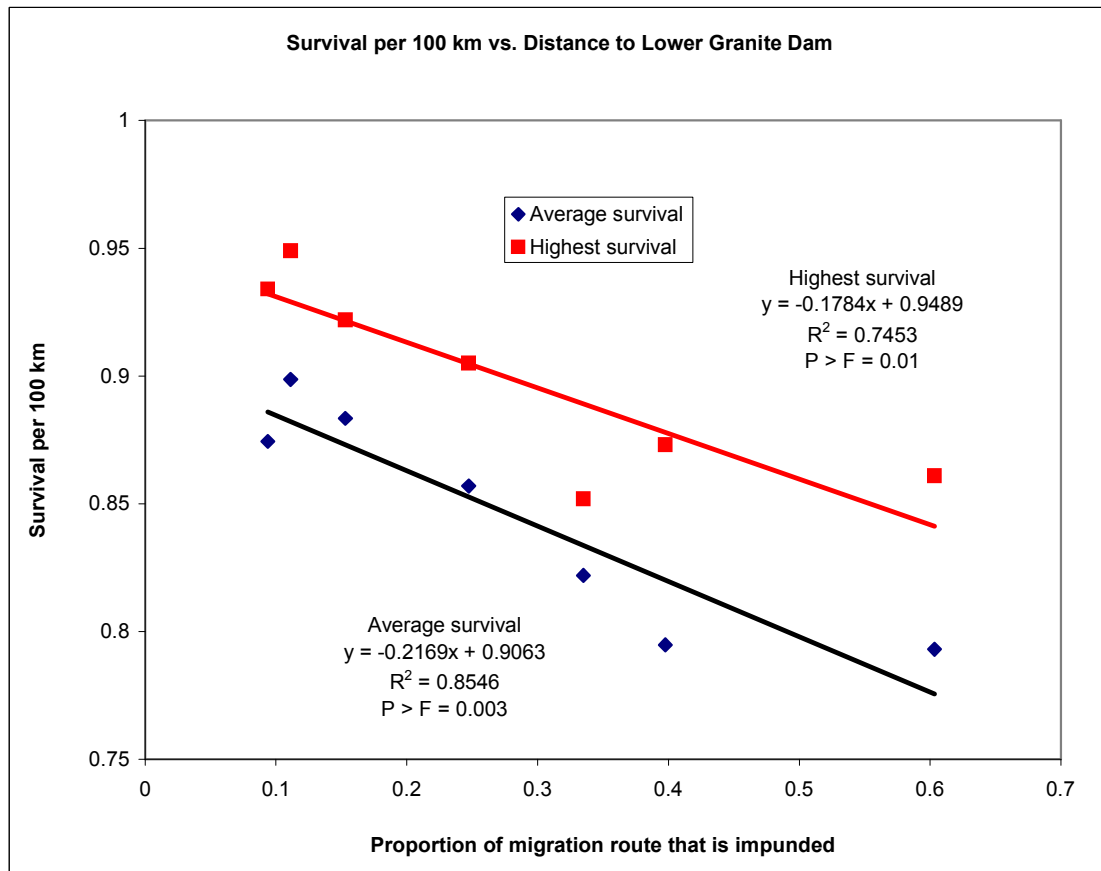


Figure 1. Survival per 100 km versus proportion of migration route that is impounded. Survival is inversely related to the proportion of the migration route that is impounded behind Lower Granite Dam.

Page 9, Figure 5 – The caption does not adequately describe the data presented in this figure and the usefulness of the figure is not apparent.

Page 13, paragraph 1 – A lower limit of 17 C seems somewhat high. The Oregon Water Quality Standards gives an upper limit of 15.6 C for adult migration and 14.6 C for juvenile rearing. Similarly, Hoar’s (1988) review of the salmonid literature suggests that physiological indicators of smoltification (gill ATPase levels and duration of elevated levels) are substantially different at 15 C than they are at 10 C, noting that a reversion to parr condition can occur at higher temperatures. Likewise, McCullough (1999) and Wedemeyer (1980) concluded that for most salmon species (including coho salmon), physiological processes involved in smoltification are inhibited at temperature between 13 and 16 C. It is not clear how such changes influence survival, but authors of these reviews recommended that water temperature remain below 12 C to protect the smoltification process. Some reduction in survival likely occurs between 12 C and the 17 C threshold.

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With respect to the upper limit, the Oregon Water Quality Standards (ODEQ 1995) propose an upper limit for growth in hatcheries (20.3 C) and an upper lethal limit of 25 C for juvenile coho salmon. It seems unlikely that many fish would survive a migration at water temperatures above that at which fish in hatcheries cease growing, particularly given that the period of smoltification is an extremely stressful portion of the life cycle of anadromous fishes. An upper limit of 26 C for migration survival therefore seems unreasonably high.

Page 15, paragraph 2 – The Snake River is not free-flowing and the reach described in this paragraph is only partially unimpounded. The Snake River above the confluence of the Salmon River is regulated by Hells Canyon Dam and the Upper Snake River projects and depleted by about nine million acre feet per year. All Salmon River fish have to swim through Lower Granite Reservoir before reaching the first PIT tag detection array in Lower Granite Dam.

Page 15, paragraph 4- The authors present no mechanistic explanation as to why they believe a flow threshold may exist, above which survival is no longer affected.

Page 16-17- There is no sound theoretical or empirical basis for the conclusion that there is no additional survival benefit for coho salmon for flows greater than 80% of the median flow over the last 10 years. Even if the relationships found for Yakima River coho and Snake River Chinook salmon are real (data presented below argue against such a threshold in the latter case), there is no basis for assuming that a threshold in the Klamath River would occur at the comparable percentage of the median flow. Nor is it appropriate to use a period of highly regulated flows (1996-2006) as the “baseline” from which the 80% calculation is made. If such an approach is to be taken (and we believe it to be extremely tenuous), then unimpacted flow estimates should be the benchmark against which flows are measured.

Page 17, paragraph 2 – Relationship of migration survival and migration flow was linear for two populations of wild Snake River spring/summer Chinook salmon, with no leveling off near median flows (Figure 2).

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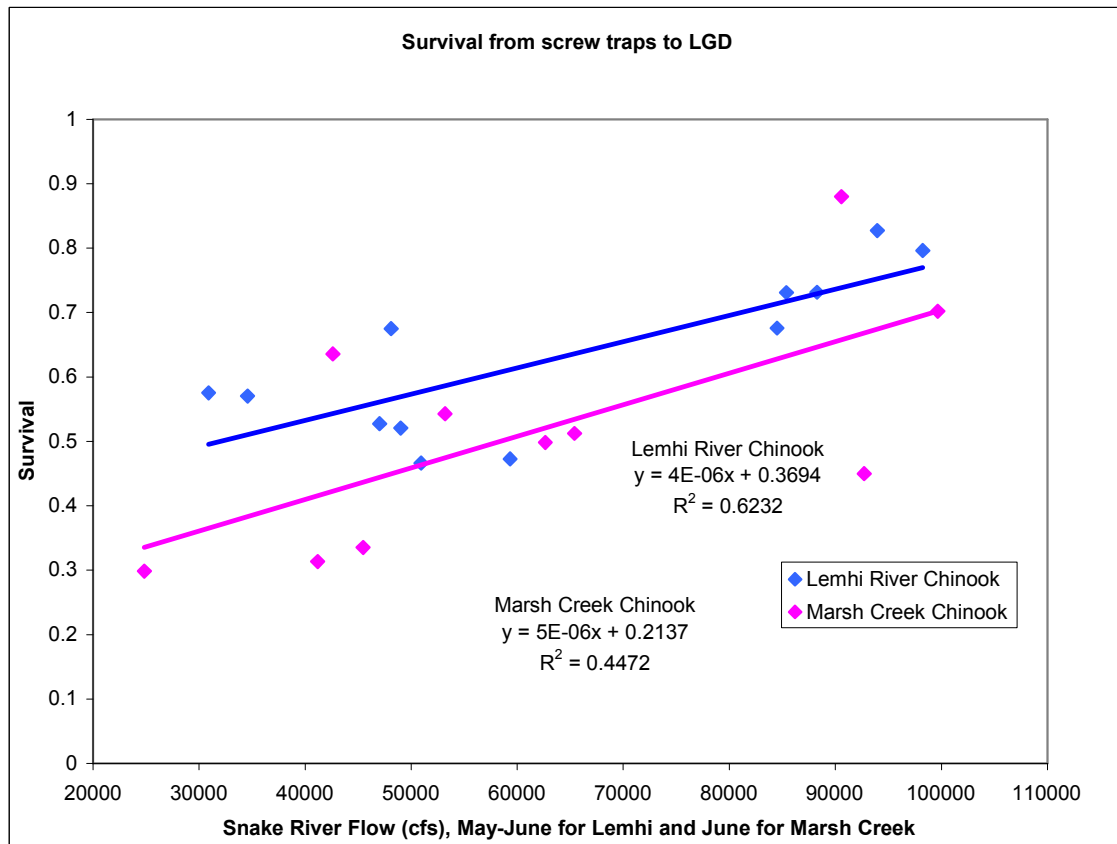


Figure 2. Survival of Lemhi River (upper Salmon River drainage) and Marsh Creek (Middle Fork Salmon River drainage) Chinook salmon smolts PIT tagged as yearlings migrating downstream. Relationship is linear for both stocks with highest survival occurring in migration year 1998, a year in which spring flows were 34% to 9% exceedence. There was no apparent “leveling off” at flows near 50% exceedence.

Page 21, first paragraph in Conclusions – There is insufficient data to support the “optimal range” described in this paper.

References:

Hoar, W. S. 1988. The physiology of smolting salmonids. Pages 275-343 in W. S. Hoar and D. J. Randall, editors. Fish Physiology, Volume XI. Academic Press, New York.

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