Puget Sound Recovery Implementation Team (RITT)

From: RITT
Date: October 1, 2008 DRAFT

To: Rosemary Furfey

Re: Peer review of the Lake Ozette Sockeye Recovery Plan

Upon request from the NMFS NW Regional Office, the RITT solicited peer reviews of the **Proposed Recovery Plan for Lake Ozette Sockeye Salmon (*Oncorhynchus nerka*)** from many qualified scientists throughout the Pacific Northwest. The five completed peer reviews we received are given below in their entirety, but are not identified as to reviewer and comments are grouped by topic as indicated in the table of contents. The RITT has not commented on the validity of the reviews in this document, nor attempted to answer questions posed by the reviewers.

We followed the NWFS peer review guidelines:

> Review guidelines: The purpose of peer review of draft reports is to provide an expert, external assessment of the scientific validity of the methods, results and conclusions of the report. The review is intended to be limited to scientific issues only, and reviewers will be asked to avoid commenting on issues that relate primarily to agency policies. The specific guidelines that will be communicated to the reviewers are as follows:
>
> The purpose of the review is to assess the scientific validity of the substance of the report, including any assumptions, methods, results and conclusions. Specific aspects of the review will vary, but may include: quality of the data collected or used for the assessment, appropriateness of the analyses, validity of the results and conclusions, and appropriateness of the scope of the assessment (e.g., were all relevant data and information considered). Reviewers will also receive the following instructions:
>
> 1) Reviewers should confine their comments and evaluations to scientific issues and refrain as much as possible from commenting on issues that involve primarily agency policy.
> 2) In some cases, reviewers may be instructed to keep the report being reviewed and their review itself confidential and to provide their review to the NWFSC within a specific time frame.
> 3) The review, the reviewers' identities and the NWFSC response will become part of the public record and may be released with the final report. The reviewers' identities and their comments will not be linked, however.

The reviewers were: Milo Adkinson, Peter A. Bisson, Phil Mundy, Mark Scheuerell, and Neil Schubert.

Five Peer Reviews of "Proposed Recovery Plan for Lake Ozette Sockeye Salmon (*Oncorhynchus nerka*)" April 14, 2008 as appeared in Federal Register Notice (73FR21913)

Key words –bolded in text: adaptive management, invasive species, knowledge gaps, limiting factor, predator, predation, sediment, viability criteria, viable.

GENERAL COMMENTS

1. My comments are based only on the main body of the plan—I did not comment on any appendices or accompanying reports. This plan seems reasonable, but as correctly identified, any effective recovery plan will need to follow an **adaptive management** framework to modify future actions as necessary. I also really liked the educational component, as education is likely to play an important role in obtaining stakeholder support. Based on questions and comments in Appendix C, I would guess there is a lot of work to be done in this area. My overall impression is that fine **sediment** and **predator**s have been identified as the two biggest **limiting factor**s. It would therefore seem reasonable to start with those aspects.

2. The best summary of this plan is a quote from Chapter 8 of the document,

"In many cases, the plan simply acknowledges and recommends coordinating the preexisting, ongoing recovery efforts and pre-existing laws or regulations that are expected to benefit the species and its environment. Some of the ongoing actions that are integrated into the plan are required under other, separate resource management regulatory processes, such as implementation of forest practices habitat conservation plans, Clallam County road maintenance, operation of the sockeye hatcheries, and regulation of fisheries that may affect sockeye."

The recovery plan is strong in many aspects because many of these ongoing recovery efforts and regulatory frameworks are fairly good. In particular, regulation of two large threats, harvest practices on the extensive public lands through the FPHCP and the use of a hatchery in recovery efforts (described in the HGMP) seem thoughtful and thorough. Another potentially significant threat would be direct harvest of adults, but this seems not be an issue. The plan seems somewhat weaker in addressing another likely threat; the effect of introduced species, most likely due to direct **predation**.

This document does a very good job of identifying critical habitat and prioritizing threats and identifying the actions most likely to be effective in dealing with them. However, the reliance on voluntary implementation seems likely to result in the various entities implementing those actions most related to their existing activities. Some high priority actions might not be implemented.

There seem to be some significant **knowledge gap**s. Some of these were identified over a decade ago and have yet to be addressed (sockeye distribution, habitat quality, **predator** abundance and impact). In other areas, there has been significant progress (adult enumeration, hatchery monitoring). Although some of the remaining uncertainties would be difficult to resolve, others would be fairly straight forward. Sonic tracking of returning adults, visual spawner surveys in potential spawning areas, estimation of egg-fry survival, determination of factors affecting **sediment** loads, surveys of juvenile distribution, estimation of predator abundances and gut contents might have been attempted had there been an entity that prioritized research questions and had the ability to influence the allocation of a modest amount of funding.

The **adaptive management** approach is well-suited for recovering an ESU where so many critical uncertainties remain. Although the plan is to be developed later, the framework of the approach presented was quite good. In particular, I appreciated the description of the three types of "monitoring" activities that should be included. Adaptive management requires that a group periodically assess the monitoring data to determine what strategies are working and which are not, and to propose changes in the recovery plan. It was not clear how this would be done and who would do it.

In sum, I found the recovery plan to be thorough and scientifically very sound. The major weakness of this plan is that it is based on many actors with preexisting programs. There is no entity currently charged with implementing it. For this recovery plan to be effective, particularly to ensure that there will be the information necessary and the ability to adapt the plan in the future, it needs an entity or working group with the ability to affect the regulation of human activities and the allocation of resources to research and monitoring.

3. Status and Trend: I think the recovery team did as well as they could with the data at hand. I was somewhat surprised that more is not known about the distribution and abundance of Lake Ozette sockeye. Some of the information was anecdotal, but if that's all the information available, I think the statements in the documents were reasonable. Using run sizes from Lake Quinault as a surrogate for Lake Ozette sockeye in the **viability analysis** also seemed reasonable to me, although from the recovery documents it appears that the Lake Ozette fish have a somewhat different age and life history structure than those from Lake Quinault.

I was intrigued by the high juvenile growth rates and relatively high marine survival rates. The lake is described as oligotrophic-mesotrophic and the abundant population of large-bodied zooplankton suggests that factors other than food availability for age 0+ and 1+ sockeye are responsible for the decline. I didn't see anything in the evidence that would suggest that the trends in run size were somehow related to a loss of the lake's productivity.

From the Quinault sockeye data (Appendix Table B.1.) and the narrative description of the Ozette sockeye harvest and run estimates in the **Limiting Factor** Analysis, it appears that populations remained relatively healthy until about 1950, after which there was a gradual but fairly steady decline until the early 1970s, when both populations seemed to experience a rather sharp drop in spite of curtailed harvest. Based on these general (and crudely oversimplified) observations, I looked for factors that took place during these time windows that might contribute to the imperilment of the stocks.

However, I have no specific suggestions or recommendations for changing the Background and Current Status section.

4. I preface my comments, first, by saying it was a daunting task to review such a large and comprehensive document. It was even more daunting given that the proposed recovery plan itself is written using language accessible to a general audience while, in at least some of the topics, I needed to acess the more detailed technical documents that were in some cases almost as long as the recovery plan itself; I simply did not have the time to refer to all of them and in the detail that would be required. Second, my review was from the context of my experience with recovery planning for the Cultus sockeye population in the Fraser River system (I provide comparisons between the populations *ad nausium*). In general, I was struck by significant differences in process (especially in the number of individuals and agencies involved), as well in the quantity and quality of the assessment information that dictates the degree of certainty that can be attained regarding the extent of the decline and the appropriate targets for recovery.

The recovery plan's overall structure is a good one. The linking of recovery goals, strategies and actions to specific hypotheses about **limiting factor**s allows a clear understanding of the reasons for the plan's priorization of recovery activities – this is something that we would benefit by emulating in Canada.

A problem that I had with the plan was the degree of redundancy within and between chapters. I understand that it was prepared for an audience that is unlikely to read it in its entirety but, rather, use it as a reference for specific issues, so redundancy in the introductory parts of each section is required. To a reviewer who is reading the entire document, however, it becomes somewhat irritating. Editing to reduce the redundancy and the size of the document would be useful.

I expound in more detail on a few issues in the next section, and conclude with a few comments on smaller issues that struck me as a read through the plan.

5. I was able to spend this afternoon with the recovery plan and App. A, B, and D. Overall I was very impressed with its quality. Should it be adopted and implemented, I have no doubt it would achieve the purposes intended. In particular, the use of hypotheses to structure the goals, objectives and criteria for recovery is very effective. Sorry I don't have time to write more than that, but I think I told you I wouldn't be bale to write much if I found the time to read it.

My compliments to the TRT and others who were involved in this effort.

TOPIC SPECIFIC COMMENTS

Limiting Factor Analysis

3. The recovery documents do an excellent job of documenting the physical habitat changes that have occurred in the Ozette watershed. It was clear that this watershed has suffered the same fate as nearly all others in western Washington -- an increase in sediment and loss of large wood which has accompanied forest management and other developments. This part of the **limiting factor** analysis was well supported by data. I suspect the export of fine sediment from the tributaries and deposition of this material in beach spawning areas has been important, and the pattern of sockeye decline after World War II does coincide with an increase in logging, road building, and other activities in the watershed that would probably have contributed to increased sediment production.

Likewise, the removal of large wood from Ozette River has probably led to loss of cover for migrating adults, and possibly emigrating smolts. I was surprised that returning adults take so long to navigate the Ozette River, but if they really take 2-3 days, then the risk of pinniped **predation** in the absence of hiding structures must be serious. The rate of scarring of returning adults does give credence to predation by marine mammals as an important **limiting factor**, and restoring large wood to the lower river ought to help reduce the incidence of predation losses.

Because the majority of Lake Ozette sockeye are beach spawners, I also looked for factors that could impact their reproductive success and early post-emergent lake survival. The lowering of the lake level caused by removing large log jams at the lake outlet seemed to me to have been significant as it has apparently contributed to a reduction in useable spawning area, particularly as the shallow spawning beaches have experienced an increase in aquatic macrophytes. The timing of the log jam removals likely coincided with the period when the sockeye run was headed downward.

The importance of **predation** on juvenile sockeye in Lake Ozette was the factor that in my mind left the most questions unanswered. The Beauchamp modeling study suggested that predation on sockeye by cutthroat and northern pikeminnow could potentially be very large, yet there does not seem to have been much actual work on the consumption rate of sockeye by these two native species. Perhaps more importantly, the presence of largemouth bass and yellow perch, particularly in the shallow beach spawning areas, could expose post-emergent fry to predation losses by nonnative species. Scott Bonar's study[1] of largemouth bass predation on coho salmon juveniles in western Washington lakes showed that bass consume large numbers of juvenile coho, and I see no reason to believe that bass would not similarly prey on sockeye in shallow areas if they had the chance. More work on predation is needed, I think.

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[1] Bonar, S. A., B. D. Bolding, M. Divens, and W. Meyer. 2004. Effects of introduced fishes on wild juvenile coho salmon using three shallow western Washington lakes. Washington Department of Fish and Wildlife, Olympia, Washington. 24 p.

4. Not much to say here other than I like the structure of the analysis and, based on the information presented, agree with the factors that are identified as key.

Lake Spawning Populations

4. The Ozette is one of several relatively small, predominately lake-spawning sockeye population that have been identified as "at risk" over the last decade or so. In Canada, this includes the Cultus and Sakinaw populations, that have been designated *Endangered* by the Committee on the Status of Endangered Wildlife in Canada, as well as several other populations in the south and central coast areas that have yet to be reviewed. Although the causes for the decline of each population differ, perhaps these types of populations are less robust to density and random demographic effects, genetic processes and anthropogenic impacts, and greater precaution is required to ensure their persistence.

Our analysis of Cultus sockeye identified depensation (*i.e.,* reduced survival when sockeye fry abundance is low because the more stable **predator** populations continue to consume about the same number of fry) as a threat to the recovery of the population when sockeye abundance declines below about 7,000 spawners (see http://www-sci.pac.dfo-mpo.gc.ca/mehsd/projects/cultus_sockeye_e.htm, in the conservation strategy document). This is supported by behavioural observations. Ricker (1941) noted that, unlike other Fraser sockeye populations, Cultus fry school and move into offshore, deeper water immediately after emerging from the gravel. He interpreted this behaviour as an adaptation to the abundant **predator**s in Cultus Lake (primarily northern pikeminnow and salmonids).

There are similarities between Ozette and Cultus lakes and their populations. They are both small but productive lakes (Cultus is about ¼ the size of Ozette). The sockeye populations may historically have been similar in size (maximum abundances of 50-100,000) and spawning period (November to January), and both have collapsed in recent years to much lower levels from which recovery appears difficult. The fry show similar behaviours (Ozette fry from lake spawners move rapidly offshore, while those from river spawners migrate only at night and move rapidly into the lake) that may reflect inherent adaptations to **predator** populations.

In Cultus, there are early indications that we have been successful in eliminating depensation through **predator** control (pikeminnow removal) and dramatically increasing the sockeye fry population size through a 12-year hatchery supplementation and captive breeding project that annually adds about 1.0 million fry to the lake. Fry to smolt survivals have increased from 5-10% to about 20%.

Given the **predator** avoidance response of Ozette sockeye fry and the failure of the population to recovery to any substantial extent despite the complete cessation of fishing, depensation may be an important limitation to the recovery of this population. Section 7.4 of the proposed recovery plan proposes a number of sound actions to address the issue, including the removal of aquatic **invasive species** and the management of pikeminnow. I suggest giving this approach a high priority, especially the investigation of pikeminnow abundance in the lake. You might also consider the short term increase in hatchery releases, which I believe currently total only about

100,000 (subject of course to an evaluation of genetic impacts), as a means of quickly moving the population out of the "predator pit." Genetic sampling of our captive breeding project has shown that the approach can successfully produce substantial numbers of fry without a loss of allelic richness or gene diversity.

Lake Limnology

4. I am unclear on the extent of the limnological data that is available for Ozette Lake. The supporting paper on **viability criteria** (Rawson *et al.* 2008) seems to indicate that, while there is some limnological data, there are no direct estimates of primary productivity, invertebrate community structure or the lake's capacity to produce sockeye smolts and the number of spawners required to produce those smolts. Such estimates would be strong support for the recovery plan's abundance goals. The studies needed to produce those estimates can be conducted relatively quickly and inexpensively. Our lakes program routinely conducts such limnological, hydroacoustic and trawl surveys in BC lakes. Their procedures, described in Hume et al. (1996) and Shortreed et al. (2000), require about six monthly limnological surveys and a fall hydroacoustic/trawl survey, and typically cost less than $20 K per lake.

Habitat Restoration

3. Many of the watershed restoration measures are sensible and will surely lead to improvements in sockeye reproductive success, and to a somewhat lesser extent vulnerability to **predation**. I did not see anything specific to trophic support in the restoration actions, but the lake apparently has an abundance of prey organisms and improving the food web probably does take second priority to physical habitat rehabilitation. I would not be surprised if species such as coho and steelhead received more benefit from Lake Ozette tributary restoration actions than the sockeye.

As I mentioned above, re-introducing large wood to the Ozette River, particularly near the lake outlet, achieves two different and potentially helpful objectives – it raises the lake level (which could help make more spawning gravel available) and it provides potential cover for migrating fish (which reduces **predation**). The Recovery Plan makes a good case for large wood reintroduction, but my comment is that it will likely be necessary to bring in a lot of wood to significantly raise the lake level and to provide the kind of cover (perhaps through engineered logjams) to reduce the incidence of predation by seals and sea lions as adults swim gauntlet of the estuary and lower river. This is a situation that will probably require major intervention in order to be effective.

The Beauchamp model suggested that **predation** on juvenile sockeye in the open waters of the lake by cutthroat trout and northern pikeminnow could be an important source of mortality, and it is doubtful if any of the habitat actions mentioned in the Recovery Plan will address that issue. Possibly, placing a bounty on pikeminnow as has been practiced in the Columbia River might help but I would strongly recommend a very limited test fishery before this measure is widely implemented. The potential for unwanted bycatch and mortality of other species associated with a targeted pikeminnow fishery should be carefully evaluated.

Finally, since Lake Ozette sockeye historically were primarily lake spawners and not tributary spawners, it would seem to be prudent to focus more habitat improvement efforts on restoring or creating additional lake spawning habitat. Could aquatic macrophyte removal be a **viable** restoration action? Could artificial spawning structures (e.g., similar to those used in Baker Lake – although I don not know how effective these have been over time) assist in establishing new beach spawning populations in areas of the lake where sockeye formerly spawned?

Recovery Goals

4. I like both the vision statement (Section 3.2.1), the definition of a **viable** population in probabilistic terms and that also explicitly limits the role that hatchery production can play in recovery, and the three-component biological **viability criteria**. In Canada, we have struggled in our process of developing numeric population abundance benchmarks for the recovery on endangered populations. The recently adopted Wild Salmon Policy identifies target and limit reference points as benchmarks that delineate three zones in status, green, amber, and red, that represent increasingly depleted populations that require greater conservation measures. In the Cultus process, we considered the use of target reference points such as lake productive capacity (83,000), the maximum historic abundance of spawners (82,000), or S_{max} (56,000), limit reference points of S_{msy} (32,000), average abundance prior to the decline (19,000) or the level of abundance associated with the depensatory effect on freshwater survival (7,000). We quickly realized, however, that once the population has increased above the minimum genetically effective population size of 1,000 spawners, has positive population growth, and an acceptable risk of extinction in a defined time period, the choice of a long term recovery target such as those listed above becomes a process of optimizing societal values rather than a biological one. Despite having an intensely assessed population (adult spawner estimates since 1925, smolt production estimates since 1926, and total catch estimates since 1950), we have not yet been able to define a long term recovery goal (other than to exceed the genetic minimum and to maintain population growth) that is acceptable to all sectors of society.

Regarding Ozette sockeye, the point I am attempting to make is that you have identified an abundance target (35-121,000) that is considerably higher than current spawner population levels. My experience with Cultus has been that it is very difficult to defend large targets even when supported by one of the best salmon assessment data sets in the world. I suspect it may be more difficult to defend the Ozette abundance goal given the significant limitations to its derivation (the reliance on population parameters from other populations, the inclusion of very large estimates of tributary spawning habitats for a population that historically has been a beach spawning one, the very limited historic abundance data that suggests the maximum historic abundance (“greater than 50,000”) has never been larger than the lower part of that range).

Modelling

4. It would be useful to “game” the utility of alternate recovery scenarios under different productivity assumptions as well as assumptions regarding the relative recovery benfits that are

likely from alternate actions such as habitat improvement, enhancement and **predator** control. This modelling would also permit planners to establish explicit time frames and mile-posts against which recovery can be assessed, something that I didn't see in these documents (my apologies if its there and I missed it).

PAGE SPECIFIC COMMENTS

Two reviewers gave page specific comments. They are ordered here by page number.

P 8. It's not clear how actual measures of streamflow variability limit the 3 life stages described in the box. The first would seem affected by high flows whereas the second and third would seem most influenced by low flows. What specifically about "variability" is it that affects these fish?

P 15. I don't agree with the comments about the role of LWD in lakes limiting sediment. Recent research has shown the importance of LWD in actually trapping sediment and organic matter imported from the riparian zone (see Francis et al. [2007] *Ecosystems* 10: 1057-1068).

P 16. Most of the beach spawning populations of sockeye that I'm familiar with (Bristol Bay, AK) actually have very little LWD associated with them and tend to be large, open areas of gravel and cobble. Further, I don't see any treatment of whether adequate groundwater inputs to spawning beaches still exist, an important **limiting factor** in other areas (e.g., Alaska).

P 17. With respect to in-lake **predator**s (fish), has there been a change in the age of smolts? That is, has there been any shift from two- to one-check smolts as one might expect if the selective pressures (i.e. **predation**) in the lake were persistently high?

P 2-6. My understanding is that both river/creek spawned and lake/beach spawned juveniles reside in the littoral zone of lakes for several weeks before migrating to open limnetic waters (see Quinn 2005). The middle paragraph on this page suggests that lake/beach juveniles migrate rapidly offshore.

P 2-11. Is the data on smolt age recent, or is there any historical data? As mentioned above, I'm curious as to whether there has been a more recent shift to 1+ individuals, or whether they were always thought to emigrate as 1+ smolts.

P 2-12. Again, is there any information on groundwater inputs into the lake with respect to the location of spawning (historical or current) beaches?

P 2-13. I just read the answer to my previous question. I think this is a shortcoming of the **limiting factor**s analysis.

Page 2-15: The reduction in lake levels by as much as 3.3 feet below historic levels could have had a dramatic and irreversible impact on available lake beach spawning habitat. I didn't notice any follow-up on this point (I may have missed it in my rush through the documents) – has there been any consideration of constructing a lake outlet weir to restore the lake to its natural level?

P 2-31. The 1st sentence of the 2nd paragraph in sec 2.8.2.1 is misleading in that Fig 2.10 does not actually display any information related to "substantial uncertainty regarding run sizes during this period." I suggest rewording it.

P 2-35&36. In Table 2.3, I don't understand why there are no "low end estimates" or "high end estimates" for most years. Why, when it says on P 2-35 that "consistent run-size estimate methodology was applied to datasets from 1996 through 2003?"

P 2-36. The paragraph under Table 2.3 indicates that "trends were evaluated in four brood year groups," but the following comparison is based on means (not "trends") of two groups consisting of four brood years each. Please revise. Also, the explanation for the increase in mean abundance is "increased adult returns from Umbrella Creek Hatchery releases, and increased natural production in Umbrella Creek." What is the specific mechanism for the increase in natural production? Was it an increase in marine survival following the ocean-climate shift in 1998/99? Was it a change in freshwater? More information would be helpful here.

P 2-37. I don't understand why, in Figure 2.12, is roughly 20% of the estimated run size is based on expansion in 2002 when 100% of the run was monitored? This seems particularly odd when compared to other years like 1999 & 2000 when a lower percentage of the estimated run was based on expansion, but the weir was in place for a lower percent of the time.

P 3-5. It is unfortunate that the recovery plan could (did) not make use of a lake sediment core for establishing a time series of historical spawner abundance based on ^{15}N (e.g., Finney et al. [2000] Science 290:795-799; Schindler et al. [2005] *Ecology* 86:3225-3231), within which to place the abundance guidelines. I would strongly recommend that a core be taken and analyzed as part of the "**adaptive management**" strategy.

P 4-7. Under H#5 (marine survival) the statement is made that "Survival in the marine environment is driven by large-scale climatic processes, which are mostly not controllable." I would argue that the word "mostly" doesn't apply here (i.e. nobody has control over it).

P 4-8. What are the **predator**s of adult sockeye on the spawning beaches? I would guess **predation** at this stage is quite low. See next point.

P 4-10. What is different about the **predator** guild between beaches & streams that would seemingly cause high mortality of adults (and eggs) on beaches, but not in streams? This seems to be the key difference between the designations of "key" vs. "contributing" for the beach and tributary spawners, respectively.

Page 4-14: We assessed the disposition of adult sockeye holding in Cultus Lake through an acoustic tagging project. The adults move into the cooler, deeper areas of the lake where they stay without much movement prior to spawning. We did not note mortality during this period.

Page 4-14: Fry to smolt survival for Cultus sockeye was much lower, around 5-10%, in years when the sockeye population was small and pikeminnow control was just beginning. It appears to have increased to about 20% after large releases of hatchery fry and active **predator** control.

P 4-14. I'm not sure where to mention this, but juvenile sockeye fry are easy to enumerate during their lake residence with hydroacoustics. This would be quite useful when combined with smolt trapping.

Page 4-15: Like Ozette, Cultus sockeye migrate through 20+ degree water in Sweltzer Creek before entering the lake. There doesn't seem to be any acute mortality because transit time through the higher creek water temperatures is short. This may also be the case for Ozette sockeye if the lake's hypolimnion is similarly cool.

P 4-15. My understanding is that mercury concentrations in lake sediments have increased considerably over the past 70 years. Although the role of Hg in controlling salmon growth rates, development, etc. is not well understood, it's possible that other heavy metals and contaminants that negatively affect salmon have also increased over time.

P 4-24. Why does Figure 4.5 not include the entire life cycle as in Figure 4.3?

P 4-26. Has water clarity in the lake itself increased over time? If so, that might partially explain the increase in macrophyte abundance.

P 4-27. As stated above, I disagree with the rationale provided in the 1st full paragraph with respect to the role of LWD along lake shorelines (see Francis et al. [2007] *Ecosystems* 10: 1057-1068).

P 4-33. Why does Figure 4.7 not include the entire life cycle as in Figure 4.3?

P 6-15. Why bother with recovery strategy #7 when, in practice, it has essentially no chance of influencing ocean-climate conditions? Quite frankly, it sounds a bit absurd.

Page 6-18: The comment that water temperatures have increased and that climate change is the likely cause is supported by a recent assessment of Cultus Lake by Shortreed (2007). When comparing recent assessments with those conducted in the 1940's by Ricker, he noted that the lake has warmed substantially and that its productivity has likely increased as well. With those two exceptions, though, there were no other changes in the lake limnology over a 75 year period, indicating a high degree of stability in lake environments and suggesting that limnological change is unlikely to play a role in the collapse of sockeye populations.

Page 7-5: I agree that public education and outreach are critical to the recovery of salmon populations.
In the Cultus process, we developed an education package that has been well received that you might want to take a look at:
http://www.cultuslake.bc.ca/documents/Caring_for_Cultus_Lake.pdf

Page 7-8: I like the approach of identifying short and long term goals governing the re-establishment of sockeye fisheries – it manages expectations and establishes the precautionary approach from the outset. One of the main reasons that Cultus was not listed under the *Species* *at Risk Act* was a misperception (even among DFO staff) that to do so would require the complete closure of all fisheries for extended periods of time.

Page 7-10: The consideration of trading off cutthroat protection to promote sockeye recovery is a good one. Ricker's work in Cultus showed that salmonids such as cutthroat are several times more effective **predator**s on sockeye fry than are pikeminnows.

Page 7-41: I'm unclear regarding the reason for the decision not to use hatchery production to augment the beach spawning populations. If this is the "normal" reproductive pattern for this population, shouldn't its abundance also be increased in balance with the tributary spawners, especially if the intention of enhancement is to provide a rapid initial increase in abundance, then shut enhancement down completely to preserve the population as a **viable** natural one?

Page 7-45: I suspect that the restoration of beach spawning habitats is the single most critical element in the recovery of this population. The rehabilitation of such habitats is also something with which we have the least experience – a much more complex task than river habitat restoration given the interactions between substrate type, upwelling water, prevailing winds and the lake's water circulation patterns.

P 7-45. I applaud the decision to only use tributary spawners as broodstock for hatchery supplementation. Earlier hatchery practices that took beach spawners for broodstock and then subsequently released resulting fry into tributaries instead of the lake itself seems questionable. I also agree with the proposal for future seeding of eyed eggs in beach habitats not currently supporting spawning aggregations.

P 7-49. How reasonable/feasible is it to assume that spawning gravel could be "cleaned" before the summer spawning season?

P 8-1. I don't understand why NMFS has agreed to separate the process of writing a recovery plan from writing **adaptive management**, monitoring, and implementation plans. It seems that these would be best addressed in parallel.

P 8-2. The 2nd full paragraph highlights one of my biggest frustrations with salmon "science" in the region. Too often I am asked to assist with quantitative analyses of varying degrees, yet the "data" I request and am given is actually a highly derived product without sufficient information as to how the numbers were derived. So, I take exception to the statement that "to be useful in decision making, the raw data, or metrics, must be reduced to a more directly applicable form or indicator." In many (if not most) instances, the raw data would be much more useful than the "reduced" product. The problem persists throughout the region and will likely require a sea change in ideology before it changes. I once repeated an analysis of Oregon coastal coho 3 times in one year because ODFW changed the "data" that many times, which is simply unacceptable (just ask taxpayers how they feel about that kind of stuff). All recovery/monitoring plans should require that all raw data be maintained in a database complete with necessary metadata, but I fear this is rarely the case.

P 8-5. As mentioned previously, I think the research and monitoring plan should include a complete analysis of a **sediment** core from the lake with the aim of establishing historical run reconstructions and an idea of the natural variability in spawner abundance absent any human influences. This technique has proven very useful in sockeye systems throughout the PNW, BC, and Alaska, and would provide a unique opportunity to use paleolimnology in deriving recovery goals—something that is impossible for other stream-dwelling salmonids.

Page 9-4: Figure 9.1 presents a mind-numbing level of complexity – good luck on implementation!

LITERATURE CITED

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