Review of Draft Biological Opinion conducted by National Marine Fisheries Service, Southwest Region: Operation of Klamath Project

Review submitted to the Center for Independent Experts

24 May 2008

Dr. Scott Hinch

Professor of Fish Conservation

Centre for Applied Conservation Research

Forest Sciences Centre

2424 Main Mall

University of British Columbia

Vancouver, British Columbia

Canada

V6T 1Z4

Introduction

The purpose of this independent review is to evaluate and comment on the use of the best available scientific and commercial information in the draft biological opinion (BO) of the National Marine Fisheries Service (NMFS) concerning effects of the Bureau of Reclamation's Klamath Project Operations on the listed threatened Southern Oregon/Northern California Coast (SONCC) coho salmon and its designated critical habitat for the period of 2008 through to 2018.

This review will focus on the technical aspects of the NMFS draft BO but NOT determine if its conclusions regarding the project's potential to adversely modify or destroy critical habitat or jeopardize the continued existence or recovery of listed coho salmon are correct. Specifically, this review provides comments on the science and related assumptions involved with the development and goals of the BO, and it provides suggestions for improvement. The review will start with general comments in terms of whether specific questions resulting from the 'Science Center review' and from the 'NRC's 2002 and 2004 reports', have been have been adequately addressed, and will follow with additional specific comments that elaborate on the issues raised with the Science Center review and NRC reports, and highlight other concerns, in an order that generally follows the numbering and titling sequence provided in the BO.

General Comments

<u>Questions from Science Center review</u>

1. Does the draft BO incorporate an ecological framework that emphasizes the geographic structure of habitats, populations, and diverse salmon life histories that contribute to salmon resilience and productivity (i.e., VSP concept, see McElhaney et al. (2000) and Lindley et al. (2006))?

The BO incorporates an ecological framework that emphasizes the VSP concept in several regards. McElhaney et al. (2000) emphasize four key population parameters: abundance, productivity, spatial structure and diversity. The major limitation in the framework of the BO involves the criteria of 'diversity'. The BO recognized that 'diversity' included the range of variability within a suite of life history traits including: anadromy, morphology, fecundity, run timing, spawn timing, juvenile behavior, age at smolting, age at maturity, egg size, development rate, ocean distribution patterns, male and female spawning behavior, and physiology and genetic characteristics. Several of these traits had little to no information provided on them, including some that could clearly be affected by changes in freshwater and ocean climates, and in-river flows, in particular those that related to life history ‘timing’ and ‘energy allocation’ (e.g. adult morphology/energetics, fecundity, run timing, spawn timing, age at smolting, age at maturity). Further details are provided below. I suggest that a broader consideration and discussion of ‘diversity’ is needed which should include a discussion of how ‘diversity’ traits may be affected by ‘human-caused factors’. If there is uncertainty on the magnitude of a potential effect to viability, this is where scenario analysis using ‘possible future conditions’ (e.g. Lindley et al. 2006) would be useful.

2. Does the draft BO consider a range of climatological conditions and water demand scenarios in the analysis?

I am assuming that the issue of ‘climatological conditions’ is intended to refer to climate effects on <u>freshwater</u> environments. This is how the BO interpreted it though it creates a somewhat artificial categorization because climatological changes also affect ocean fish, and this has direct impacts to freshwater production and diversity. In terms of effects of climate conditions specifically on freshwater environments, there is some very limited discussion of this in the BO focused largely on the findings of Bartholow (2005) but a range of climatological conditions is not considered. Specifically, Bartholow’s empirical model found a 0.5 °C increase per decade in the Klamath River. The data for this relationship ended in 2001. Since then until the present, there has been at least a 0.25 °C further increase in temperature and by the time the Action of the Project is complete in 2018, there will have been a further 0.5 °C warming. This 0.75 °C increase in <u>average temperature</u> is quite significant and will affect several ‘diversity’ measures, e.g. juvenile coho growth, movements, egg development rates, to name just a few. Equally important, Figure 2 in Bartholow (2005) shows that 3 of the most recent 4 years (up to 2001) were ‘record’ high temperatures. As of 2007, most rivers in the Pacific Northwest have had their warmest summer daily average temperatures in 8 of the 10 most recent years – this is probably the case for the Klamath River as well. I am certain that temperatures on some days in recent years could have exceeded critical thermal limits and number of days exceeding critical temperatures could increase considerably within the next decade. This will affect survivorship of adults and juvenile and could affect timing of migrations, yet, there are no scenario analyses in the BO which explores either the general warming trend or the exceedance of critical thermal limits. Increased summer flows expected by the Project may counter-act some of this general warming but scenario analyses to explore such interactive mechanisms were not explored.

The effects of a potentially warmer river are not fully evaluated in terms of fish disease, specifically as it pertains to *Parvicapsula minibicornis* or fungi. Also, whereever the term 'disease prevalence' is mentioned in the text or data figures, it is not clear if the BO means certain 'levels of ill-health' or simply that these fish carry a parasite – this has large implications for interpretation of disease and thermal effects. More details are provided below.

The BO considered a range of water demand scenarios which all seemed reasonable though the BO raised the concern that the Reclamation's water accounting system (i.e. the WRMIS model) may lack accuracy, and it further noted that in the event of that operational assumptions are not accurate, future IGD flows will not be accurately represented by Table 6 and the effects on coho salmon may be <u>less or greater</u> that those described in this BO. This raises the question of whether some type of sensitivity analysis is needed to explore what assumptions are most likely to be violated in that model and how sensitive model output is to assumption violation of these potential kinds.

Estimated water consumption over the years is provided in Figure 12. These data and the analysis raised several questions and concerns:

i) There is clearly a cyclical nature in the data that are not well described by the linear regression approach used (suggestive of climate or related interannual factors), so linear regression is probably not the best method to describe this pattern.

ii) Was serial autocorrelation considered in these data, and if not, then is the strength of the regression equation actually weaker than was suggested? The regression is statistically relatively weak to start with and such statistical uncertainty was not addressed.

iii) The regression line shows an average increase in consumption over 40 years of about 30 acre feet – is that a large amount relative to potential impacts on the population? Moreover, can this rate of consumption change not be used in a scenario assessment to predict the level of risk over the next 10 years such an expected consumption change would present to the population?

3. Does the draft biological opinion consider a range of ocean conditions in the analysis?

The BO does not consider a range of ocean conditions. The ‘ocean conditions’ section reviewed the concept of ‘salmon production and decadal scale shifts’, yet it did not review how ocean climate cycles or anthropogenic influences have been changing, andcould change, and what this may mean to coho. The literature suggests that ‘decadal’ oscillations may be occurring more frequently, as are ENSO events. Anthropogenically induced warming of the North Pacific Ocean is also occurring with general warming of at least 0.5 °C or more in recent decades, with an expected trend of continued warming. When decadal regimes get overlaid on top of ENSO conditions, and anthropogenically induced warming, it is not surprising that we now have such large interannual variability in ocean salmon production. The BO lacks an appreciation for this, and it does not consider future ocean climate scenarios. This is where analyses using scenarios of future conditions would be extremely helpful to quantify or at least contextualize the level of uncertainty that accompanies any prediction of production of coho salmon in the ocean, and hence expected coho salmon returns into freshwater and their contribution to production under the proposed actions of the Klamath Project. Such uncertainty scenario analyses would also be important to conduct to assess ‘viability’ of a population, a key element of a BO. More details are provided below.

4. Does the draft biological opinion consider the effects of hatchery fish on listed fish?

The BO does adequately consider the effects of hatchery fish though some editorial concerns were raised for this issue.

<u>Questions from NRC's 2002 and 2004 reports</u>

5. Did NMFS' draft biological opinion present convincing scientific evidence about the spatial and temporal extent of young-of-year and juvenile coho salmon use and occurrence in the main stem Klamath River?

In general, the BO provided sufficient evidence on this matter though some clarification is needed in terms of how 'straying' from the main stem was determined and its definition.

6. Has the draft biological opinion adequately evaluated the potential effects of main stem flows on the survivorship of coho smolts?

7. Are the draft biological opinion's scientific findings on the influence of main stem flows on the spatial and temporal extent of coho juvenile survivorship in the summer months scientifically supportable?

The BO adequately addressed aspects of both question 6 and 7 but share in the same limitation. Specifically, the direct effects of flows on the physical habitat (e.g. water velocity and depth) for smolt and juvenile survivorship were adequately considered. However, there was little focused consideration given to how water temperature, which is affected by flows and will be altered as climates continue to warm, will directly effect survival (e.g. lethally high temperatures) or indirectly affect survival (e.g. sub-optimal temperatures) via changes in energetics, physiology, growth, movement rates, etc. One of the problems is that there is no overarching conceptual framework that clearly structured potential temperature effects. The only 'framework' is provided in Table 11 which categorizes temperatures as "Suitable", "Low to Moderate Stress", or "High Stress" – my detailed comments below indicate why these categories are likely not scientifically defensible. The BO needs a framework that addresses the thermal categories which are 'optimal', 'sub-optimal' and 'critical', better defines the issue of 'duration of thermal exposure' as it pertains to these categories, and removes references to 'stress' unless physiological stress variables were measured. The BO needs to consider the effects of changing flow (and hence temperature) on thermal exposure duration to these thermal categories and then consider how specific traits of population viability may change for smolts and juveniles (e.g. smolt migration timing, egg development, growth, bioenergetics/feeding, movement, physical habitat use, etc.).

The Klamath River will continue to warm over the next decade with daily summer temperatures increasing and frequency of critical temperatures increasing. On the other hand, higher anticipated flows caused by the Project may reduce river temperatures at some times of the year. Will climate change effects to structure and diversity overwhelm, mask, or interact with effects caused by the Project? Scenario analyses and 'future condition assessments' (as suggested by Lindley et al. 2006) are needed to evaluate the potential range of outcomes and risk of change to population viability in the context of potential Project effects.

Specific Comments

<u>III) Analytical Approach</u>

E) Key assumptions of the NMFS Assessment (Pg. 33-35)

The BO noted several assumptions and model rules involved with WRIMS and commented that "the Reclamation's water accounting system may lack accuracy", though Reclamation concludes the "modeled predicted flows at IGD….are the best representation of flows we will experience in the future". The BO claims to have "considered the <u>uncertainties</u> associated with the future water availability …." and "considers the resulting outputs to reflect the flows reasonably certain to occur at IGD over the 10 year action". It was not clear to me what these aforementioned 'uncertainties' were. More importantly, the BO concludes this section by stating that "in the event of that operational assumptions are not accurate….we anticipate future IGD flows will not be accurately represented by Table 6 and the effects …on coho salmon may be <u>less or greater</u> that those described in this BO". This is quite a divergent conclusion. Can this uncertainty be further examined through scenario assessments? Though it may not be strictly quantifiable, levels of uncertainty could be explored and could be further enhanced by sensitivity analysis – e.g. what assumptions are most likely to be violated and how sensitive is model output to assumption violation of these potential kinds?

IV Status of the Species / Critical Habitat

3. Factors Responsible for Coho Salmon Decline

d. Climate Change and e. Ocean Conditions (pg 38-39)

The sections on ‘Climate Change’ and ‘Ocean Conditions’ were thorough in some aspects but weak in others. Specifically, ‘Climate Change’ only focuses on freshwater habitat in the BO yet recent climate change to ocean habitat has already had huge effects on coho (as was alluded to in the Ocean Conditions section) and will have even broader consequences in the near and distant future (never reviewed in this BO). The Climate Change section discusses “availability of water resources under future climate scenarios” yet the Ocean Conditions section never discusses how habitat or fish production may change under future ocean scenarios. The only future scenario that is mentioned in the Ocean Condition section is that the “strong upwelling in the spring of 2007 may have resulted in better ocean conditions for the 2007 coho salmon cohort”. I would argue that other scenarios should be explored, including, but not limited to, a general decline in ocean productivity over the next decade.

The Ocean Conditions section reviewed the concept of ‘salmon production and decadal scale shifts’, yet it did not review how ocean climate cycles or anthropogenic influences have been changing and could change, and what this may mean to coho. The Pacific Decadal Oscillation Index (PDOI), with which I am most familiar, has shown decadal oscillations from the early 1900s to the 1990s, but since then, oscillations have been shorter than decades indicating higher levels of variability in recent years in ocean productivity (I suspect the WOPI probably has shown similar trends). Even El Niño events are now occurring on what seems to be a more frequent pattern (these used to be 5-7 year cyclesbut now are occurring more frequently). Anthropogenically induced warming of the North Pacific Ocean is also occurring with general warming of at least 0.5 °C or more in recent decades, with an expected trend of continued warming. When decadal regimes get overlaid on top of El Niño conditions, and anthropogenically induced warming, it is not surprising that we now have such large interannual variability in ocean salmon production. The BO lacks an appreciation for this, and it does not consider future ocean climate scenarios either in this section or in later ones. This is where analyses using scenarios of future conditions would be extremely helpful to quantify or at least contextualize the level of uncertainty that accompanies any prediction of production of coho salmon in the ocean and hence expected coho salmon returns into freshwater and their contribution to production under the proposed actions of the Klamath Project. Such uncertainty scenario analyses would also be important to conduct to assess 'viability' of a population, a key element of a BO.

4. Population Viability

i. Spatial Structure (pgs. 45-46)

The BO states that because spatial structure has been reduced and that some habitats and streams are no longer available, that straying of coho into non-natal streams has likely increased. I have a problem with calling this phenomenon 'straying'. These non-available habitats have been that way for a long time and the fact that coho now use them means that these areas are part of their current life-history, so why is this considered straying? It was suggested that this type of increased straying would reduce population viability because coho are likely accessing unsuitable habitats or inbreeding with genetically unrelated individuals. Whether population viability is being affected would be more a factor of the scale of 'straying', which was not commented on, and related factors like density dependence. For instance, if the movements of coho into these non-natal streams or habitats are being done each year by large segments of the population, then I can envision high levels of competition amongst individuals (for this sub-optimal habitat) which could lead to negative effects on viability.

J. Diversity (pgs. 46-47)

Following on McElhany et al. (2000), the BO defines 'diversity' as the range of variability within a suite of life history traits including: anadromy, morphology, fecundity, run timing, spawn timing, juvenile behavior, age at smolting, age at maturity, egg size, development rate, ocean distribution patterns, male and female spawning behavior, and physiology and genetic characteristics. The more diverse these traits, the more diverse a population is and the higher the fitness of individuals, and the greater the likelihood that the population will be or remain viable. The BO uses a set of diversity guidelines which include among others: i) Human-caused factors such as habitat changes, harvest pressures, artificial propagation, and exotic species introduction should not substantially alter variation in species traits, and, ii) Population status evaluations, which should take uncertainty about requisite levels of diversity into account. In terms of discussing how coho salmon population viability is or may be affected by diversity traits, the BO only briefly refers the reader to the issue of limitations to spawning and rearing habitat as being most responsible for affecting “species’ basic demographic and evolutionary processes” and that “activities that affect evolutionary processes have the potential to alter the diversity of the species”. Later in the BO in the life history description of SONCC coho salmon (Pg. 54-58), there is a good review of several of these diversity traits (e.g. migration and spawning timing, egg incubation and fry emergence, juvenile rearing behaviour, smolt timing, and age at smolting). I was very surprised that none of these traits or variability within these traits for which data are available were discussed in the diversity section. The BO specifically discusses life history variability in terms of early life history behaviour and strategies on pg 58. Though data on some of the other traits (e.g. egg size, fecundity) may not be readily available for SONCC coho, certainly data are available for the species, as well as information on variability in these traits. The literature is full of examples of how several of these diversity traits, and their variability, are or can be affected by human-caused factors (e.g. hatcheries can affect juvenile behaviour and migration timing, climate warming can affect egg development rates and age at smolting (King et al. 2003; Ch. 18 in Northcote and Hartman 2004)). I suggest that a broader consideration and discussion of diversity is needed in this section of the BO, and this should include a discussion of how diversity traits may be affected by ‘human-caused factors’. If there is uncertainty in the magnitude of a potential effect to viability, this is where scenario analysis using ‘possible future conditions’ (e.g. Lindley et al. 2006) would be useful.

5. SONCC Coho Salmon Status Summary

d. Diversity (Pg. 47)

The BO summarizes the diversity issue as one being influenced largely by “hatcheries and out of basin introductions”. But the BO provides little information on how hatcheries could specifically alter variability in diversity traits (e.g. fecundity, egg size, juvenile behavior, morphology, etc.). Moreover, no consideration is given to climate change effects (which include both anthropogenic and natural causes) on diversity. Ocean conditions have a large influence on diversity in terms of body size at maturity and fecundity with warmer and less productive years producing smaller mature adults (Bigler et al. 1996, Cox and Hinch 1997) with less body energy (Crossin et al. 2004) thus decadal regimes, El Niño, and other ocean phenomenon will affect population viability. Moreover, as adults stop feeding just prior to initiation of upriver migrations, they must complete gonad formation, reach spawning grounds, and complete spawning, all on reserve energy. Smaller, less energy-dense adults can have trouble with this task (e.g. Rand et al. 2006) and, in years when river temperatures are also high, metabolic limitations and physiological stress can further limit migration success leading to high en route mortality (Lee et al. 2003, Keefer et al. 2008) and/or high pre-spawn mortality (e.g. reaching spawning areas but dying without spawning) (Gilhousen 1990). Thus, future trends in ocean conditions, in conjunction with freshwater climate, will define the limits to wild population productivity.

V. Environmental Baseline

C. Activities affecting SONCC coho salmon and their critical habitat in the Action Area

2. Klamath Project

c. Klamath Project Water Consumption (pg. 63-64)

Figure 12 shows the relationship between estimated Klamath Project water consumption and year. I note three things from this figure: i) there is a cyclical nature in the data that is not well described by the linear regression approach used (suggestive of climate or related interannual factors), so linear regression is not the best method to use; ii) the regression is statistically weak, even before accounting for serial autocorrelation (which was not addressed?) indicating that the statement "potential to decrease the viability of the interior population units" must better address the issue of statistical uncertainty, and; iii) the regression line shows an average increase in consumption over 40 years of about 30 acre feet – is that a large amount relative to potential impacts on the population, and can this rate of consumption change be used in a scenario assessment to predict over the next 10 years what risk such an expected consumption change would present to the population?

4. Timber Harvest (pg. 65-66)

The BO's review of how forestry practices affect streams and fish is rather poor. This is an area of extensive evaluation in the Pacific Northwest. Only two papers are referenced (e.g. Chamberlin et al 1991; Furniss et al 1991) both of which are from the same book (Meehan. –

"Influences of Forests and Rangeland Management on Salmonid Fishes and their Habitat" (1991)). These papers only address hydrology, sediments, and road issues. Clearly there are other potential impacts involving riparian and physical habitat in-stream issues, and cumulative and upstream temperature issues, to name a few. Other chapters in that book (e.g. Murphy and Meehan 1991; Hicks et al. 1991; Bjornn and Reiser 1991) would be useful to reference for these issues. Moreover, that book is getting dated and a more recent general reference I recommend is Northcote and Hartman's 'Fishes and Forestry' (2004), and I also recommend conduting a general literature search as published fish-forestry studies have been numerous in recent years with conclusions varying considerably among regions and species.

5. Climate Change (Pg. 67-68)

The BO states that "most life history traits in Pacific salmon have a genetic basis" …thus…"the extent and speed of changes in water temperatures and hydrologic regimes of the Klamath River and associated tributaries will determine whether or not coho salmon are capable of adapting to changing river conditions". It is these sorts of relationships that I was hoping to see elaborated on earlier in the BO in terms of how human-caused habitat changes affect measures of population diversity.

Climate change is only discussed in terms of freshwater habitat. As mentioned above, climate change is affecting oceans and will continue to do so but this is not discussed.

7. Hatcheries (Pg. 68-70)

I found this section of the BO confusing in its organization as the second half of the section mostly involved interactions between the 'hatchery issue' and 'climate change issues'. I recommend moving the section on 'Hatcheries' before the section on 'Climate Change', as well as moving the relevant paragraph from the Hatchery section to the Climate Change section that addresses interactions between hatchery fish and climate.

I also note that in the section that addresses an interaction between hatchery fish and climate, this is the **first** recognition in the BO that ocean climates are changing and that ocean carrying capacity can and will change with climate change and this will affect coho. This recognition needs to be made much earlier in the BO in a more appropriate section.

11. Water Quality (Pg. 72-73)

The last line from this section, the concluding comment, seems to be missing.

12. Fish Disease (Pg. 73-76)

There is a fairly good review of some of the major pathogens associated with disease involving SONCC coho salmon. Most of the emphasis is on how flow variation may affect disease, and I suspect this is because *Ceratomyxa shasta,* on which most of the research attention has been paid in this region, is believed to be affected considerably by flow. As a result, much of the emphasis later in the BO focuses predominantly on *C. shasta.* The focus of this review in terms of effects to fish was on the concept of parasite 'prevalence'. 'Prevalence' does not necessarily equate with disease states and associated health issues. For example, all adult Fraser River sockeye become hosts for *Parvicapsula minibicornis* when they depart the ocean and enter the estuary during spawning migrations [there is no evidence that *P. minibicornis* occurs in ocean rearing adults]. Prevalence is largely 100% as determined by RNA studies (Wagner et al. 2005). In terms of 'infectious state', histological examinations of their kidneys shows that none of these fish have indications of disease until they have experienced at least 350 °C degree-days in freshwater. Many adults have spawned (and died) before this threshold is met unless they enter freshwater too early or river temperatures are much warmer than usual (Wagner et al. 2005). This example raises several issues that the BO needs to consider:

i) Wherever the term 'prevalence' is mentioned in the text or data figures, does the BO mean certain 'levels of ill-health' or simply that these fish carry a parasite? This has potentially large implications to interpretation of disease influences on population viability. Has research been done to show that 'prevalence' of *C. shasta* relates to ill-health and or fitness consequences?

ii) Disease states associated with *P. minibicornis* are clearly linked to chronic temperature experience (see Wagner et al. 2005), is this also the case for *C. shasta*, or the other pathogens discussed in the BO?

iii) As freshwater environments continue to warm over the Action Period due to climate change, how will that affect these diseases? If the Project creates cooler temperatures at certain times of the year, how will that affect these diseases? For *P. minibicornis*, this could be specifically addressed through known degree-day disease issues and expected thermal regimes over the Action Period.

I note there was no mention of fungus and its role in fish disease and health. *Saprolegnia* is a common fungus which migrating adult salmon frequently display, particularly as temperatures increase and when fish are close to or on spawning grounds (Van West 2006). It can cause serious problems for vision and gill function and can have fitness consequences.

D. Habitat Conditions in the Action Area (pg. 76-77)

The introduction to this section provides a table (Table 11) which "provides properly functioning condition guidelines against which the current baseline water quality conditions may be compared. The table focuses on temperature, DO, and pH, three of the water quality variables critical to salmon survival and appreciably affected by Project operations." I agree that these variables are probably the most important water quality ones to consider, but I find this table conceptually confusing and inadequate for its intended purposes, and in certain instances, inaccurate. I itemize my concerns below.

i) It was never mentioned what the column categories mean (e.g. "Suitable", "Low to Moderate Stress", "High Stress"). 'Stress' has a physiological definition. It involves the release into the blood of glucose, ACTH, cortisol, and other compounds in order for the body to regain some level of homeostasis or to deal with disease or other environmental hardships. Exposure to some stressor for some duration is needed in order to elicit a stress response. There was no mention in the text or table what duration of temperature exposure (I assume temperature is the stressor) was needed to fit their categories of low versus high stress. Regardless, unless direct measures of physiological stress were measured for fish experiencing these temperatures, then it is incorrect to label these categories as low or high 'stress'. Other terms that have been used in the literature to reflect 'stress' are not any better defined and I wouldn't recommend them either (e.g. distress, impaired, etc.).

ii) A better conceptual framework is needed to categorize the influence of water quality effects on fish. In terms of temperature, the "suitable" category seems to reflect what most fish ecologists would call 'optimal' temperatures – that is, temperatures at which growth, feeding and swim performance occur at minimal physiological cost and maximum energy gain (optimality concepts are widely discussed in text books and have been examined for coho and other salmon species, across all life stages – e.g. Bjornn and Reiser 1991; Brett 1991; Diana 1995). There is generally a small temperature range over which thermal optimums occur, and temperatures cooler or warmer than this range are 'sub-optimal'. Thus, the category labeled "Low to Moderate Stress" would be considered 'sub-optimal' on the high temperature end. Also, missing is a temperature category that is 'sub-optimal' on the low temperature end. It is incorrect to state as the table does that temperatures less than 17 °C are 'suitable' for adult coho because cooler temperature below the optimum range will result in poor swim performance,and poor growth (e.g. Brett 1991, Lee et al. 2003). Chronic exposure to cool temperatures can have the same effect as chronic exposure to warm temperatures on several variables (such as growth, swim performance, feeding). Some variables will not follow the same optimality rules (e.g. disease development) thus temperature criteria for those variables needs to be categorized separately.

iii) In terms of relatively high temperatures, the "high stress" category seems to reflect a lower limit for a range of temperatures that fish ecologists would call 'critical' temperatures. These are temperatures that, depending on exposure level, can cause mortality. Higher temperatures will cause more immediate mortality than lower ones in this range. There are numerous references throughout the BO to the concept of 'critical' temperatures (e.g., pg 82 – "Ambient air temperatures tend to be higher…., can produce critically high water temperatures".; Pg. 79 – "below the upper threshold of 22 C"), yet the notion that certain high temperatures can have acute mortality effects on fish is not alluded to in the table. There has been considerable research into species- and life-stage specific critical temperatures, and in particular for coho salmon (e.g. reviewed in Beitinger et al. 2000). This concept was also in one of the papers cited as part of this table (Bjornn and Reiser, 1991), but the BO does not present data on this important metric in the table or elsewhere.

iv) To sum up the temperature issue, a better table, and hence conceptual framework, is needed that addresses the optimal, sub-optimal, and critical thermal categories, better defines the issue of 'duration of thermal exposure' as it pertains to these categories, and eliminates references to 'stress' unless stress variables were measured. These categories and concepts are much more scientifically defensible than what is presently in this table. The BO then should refer to these thermal concepts after this point (e.g. in pgs 78-89).

v) The general table information was put together from information in other documents. The temperature information came from "PacificCorp 401 certification application". On the surface, this does not appear to be an 'original source' of the information (e.g. what were their sources of data, and did they come from studies that were peer-reviewed?), andI am unable to judge this. There is an abundance of information in the peer-reviewed scientific literature that can be used for this table.

vi) Dissolved oxygen does not follow the same 'optimality' relationships as does temperature (extremely high levels do not affect fish as do extremely low levels), but the concept of 'critical' oxygen levels applies. I do not know any literature supporting the data presented in the table that oxygen concentration between 7 and 8 mg/L cause 'low to moderate stress' in adults/juveniles and that 'high stress' is caused at concentrations less than 7 mg/L. According to Bjornn and Reiser (1991, pg. 118), which is one of the table's references, growth rates of coho salmon do not start to decline unless oxygen is less than 5 mg/L, as do food conversion ratios (Bjornn and Reiser 1991, pg. 119). Thus, are fish experiencing 5-6 mg/L of oxygen categorized as under 'high stress' yet their growth rates are not affected? Unless physiological stress or some related index was actually measured, the 'stress' categorizations used in the BO should be abandoned. Certainly there can be 'sub-lethal' effects at certain low levels of oxygen concentration (e.g. slowed growth in juvenile coho; pg. 118, Bjornn and Reiser 1991) and 'lethal' levels where of oxygen is too low to support life. This would be termed as 'critical' oxygen concentration. Of course all of these metrics would have duration (time of exposure) requirements that would need to be defined in the BO.

vii) Aside from developing a better conceptual framework to summarize water quality guidelines for presentation in this BO, a more thorough means of summarizing temperature duration and magnitude data from the literature is needed. One example of a better approach can be found in Table 29 of Hardy et al. (2006), which is one of the key references used in the BO for flow modeling. I strongly recommend that the BO examine how Hardy et al. (2006) reviewed thermal issues, particularly as they dealt with upper and lower incipient lethal temperatures, as well as optimal temperatures (Pg 196-200; Hardy et al., 2006).

F. Critical Habitat (pg. 91)

This section is a verbatim repeat of the one that follows in the BO.

VI. Effects of the Action (pg, 93-132)

A. Evaluation of Instream Flow Needs… (Hardy et al. 2006; NRC 2007)

The paper by NRC (2007) was not in the reference list.

The BO relies a great deal on the in-stream flow recommendations made by Hardy et al. (2006). It also uses their flow recommendations as a "general predictor of the ecological fitness resulting from the implementation of the proposed flow regime". After reading Hardy et al., I agree that it is a very comprehensive model and appropriate document from which to draw inferences, but I felt that the BO did not sufficiently explain some additional strengths and limitations of the modeling efforts, particularly as it pertains to the 'master ecological factor' (*sensu* Fry) – temperature.

i) The BO states that "Hardy et al (2006) developed habitat suitability criteria for life history stages of anadromous salmonids in the main stem Klamath River based on the fundamental concepts of the ecological niche theory. Hardy et al. (2006) defines the ecological niche as 'the set of environmental conditions (e.g. temperature, depth, velocity) and resources (things that are consumed such as food) that are required by a species to exist and persist in a given location'. Species and life-stage specific habitat suitability criteria (HSC) used in in-stream flow determinations are an attempt to measure the important niche dimensions of a particular specie and life stage. These criteria are then used to measure niche changes relative to changes in flow". Based on this description, I had anticipated that predictions of how habitat change would occur based on changes in flow would include a specific consideration of thermal habitat but instead predictions (e.g. Tables 13, 15, 17, 18, etc.) are based just on physical habitat, specifically depth and velocity. Apparently, Hardy et al. (2006) did not specifically model how flow and temperature changes could affect coho (from what I could ascertain from their document), and if this is the case, then that should be made very clear in the BO.

ii) There were no sections in the text that specifically addressed how changes in flow conditions would affect thermal habitat, and subsequently, what such changes in thermal habitat would potentially mean to coho. Hardy et al. (2006) stated that "increased flows… reduce maximum daily and mean daily temperatures" thus there could well be a thermal response (potentially positive) to coho resulting from changes in flow. Hardy et al. apparently examined this for steelhead and Chinook and concluded that their flow models "integrated with temperature simulations demonstrate that the recommendations provide equal or improved growth rates (size and weight)…and that… the increased flows, which reduce maximum daily and mean daily temperatures, while increasing minimum daily temperatures provide a net bioenergetic benefit based on the equivalent 'fish mean daily temperature'. These conclusions are very powerful and the BO should be able to draw at least some inference from them in terms of how temperature may affect coho under different flow regimes.

iii) The fact is that temperature will be affected directly by changes in flow. Therefore, there should be specific sections in the BO focused on how temperature may affect fish 'diversity traits', e.g. growth, movement, survival, bioenergetics, smolt and adult migration timing, etc. (e.g. Hardy et al. 2006). Where temperature was addressed in the BO, it was integrated into several sections which largely focused on fish disease (e.g. pg. 110-112, 123-124). There are a few limited instances where the effects of flow-mediated changes to temperature are mentioned in regards to thermal preference and thermal refuges, but not in regard to specific 'diversity traits'.

iv) The issue of parasite "prevalence" is an important discussion point (pg. 112, Figure 20) and this concept is being interpreted as "disease rates". As I stated earlier in this review, the two are not necessarily the same. There is no information given about how 'prevalence' was determined nor what aspects of prevalence translate into actual disease states or states of ill-health in fish. Thus, caution must be made in interpretations of effects of parasite prevalence on population viability based just on prevalence measures.

v) I also note (pg. 112, Figure 20) that prevalence of *C. shasta* tends to track changes in flow rates (which is discussed in the BO) yet prevalence of *P. minibicornis* does not track changes in flow (and there is little discussion of this). Instead, all fish seem to get infected by the start of May and that remains constant through summer. Such findings support other research (e.g. Wagner et al. 2005) on Fraser sockeye that showed all adults pick up *P. minibicornis* after exposure in warm water during migrations but that kidney disease levels, based on histology, do not show any evidence of the parasite until at least 350 °C degree-days have been accumulated. Thus, *P. minibicornis* may be more affected by temperature exposure duration than by flows and I encourage the BO to consider 'degree-day' or other thermal-exposure metrics in this section of their document.

VIII. Cumulative Effects

I am not sure why this section was entitled 'Cumulative effects' when all it really discussed was freshwater warming due to climate change (and it only did this very briefly). Regardless, the paper that is cited and discussed in this section - Bartholow (2005) - was not fully utilized or appreciated by the BO. The contents of this section, and of the suggested revisions, need to also be incorporated earlier in the climate change sections of the BO (e.g. pg 67). Bartholow's empirical model found a 0.5 °C increase per decade in the Klamath River. The data for this relationship ended in 2001. Thus, since then (till the present), there has been at least a 0.25 °C further increase in temperature and by the time the Action of the Project is complete in 2018, there will have been a further 0.5 °C warming. This 0.75 °C increase in average temperature is quite significant and will affect several 'diversity' measures, e.g. juvenile coho growth, movements, and egg development rates, to name just a few. There are no scenario analyses in the

BO which explores these effects. Equally important, Figure 2 in Bartholow (2005) shows that 3 of the most recent 4 years (up to 2001) were ‘record’ high temperatures. As of 2007, most rivers in the Pacific Northwest have had their warmest summer daily average temperatures in 8 of the 10 most recent years – this is probably the case for the Klamath River as well. I am certain that temperatures on some days in recent years exceeded critical thermal limits, and the number of days exceeding critical temperatures could increase considerably within the next decade. This will affect survivorship of adults and juvenile and could affect timing of migrations, yet there are no scenario analyses in the BO which explores this. This is another reason why the BO needs to better describe and categorize thermal categories into optimal, sub-optimal, and critical levels (as was mentioned earlier in this review).

IX. Integration and Synthesis of the Proposed Action

Many of the conclusions about SONCC coho salmon seem to be drawn with the underlying tenet that ocean ‘survival may improve but that fluctuations in ocean productivity are expected’ (pg 140). With the North Pacific Ocean generally warming and becoming less productive in the immediate and distant future (e.g. Welch et al. 1998), it is hard to not expect overall poorer returns over the next decade compared to the past decade. How will continually poor ocean returns, and potentially smaller and less fecund adults (e.g. Bigler et al. 1996) affect freshwater productivity of coho and the interaction with the Actions of the Project? How will these potential changes affect population structure and diversity and, hence, viability? Similarly, the Klamath River will continue to warm over the next decade with daily summer temperatures increasing and the frequency of critical temperatures increasing. On the other hand, higher anticipated flows caused by the Project may reduce river temperatures at some times of the year. Will climate change effects to structure and diversity overwhelm, mask, or interact with effects caused by the Project? Will potential Project effects to population viability be detectable given likely climate change effects? For both aspects of climate change (marine and freshwater), scenario analyses and ‘future condition assessments’ (as suggested by Lindley et al. 2006) are needed to evaluate the potential range of outcomes and risk of change to population viability in light of any potential co-occurring changes to population viability caused by the Project.

References

Bartholow, J.M. 2005. Recent water temperature trends in the Lower Klamath River, California. North American Journal of Fisheries Management. 25:152-162.

Beitinger, T.L., W.A. Bennett, R.W. McCauley. 2000. Temperature tolerances of North American freshwater fishes exposed to dynamic changes in temperature. Environmental Biology of Fishes 58: 237-275.

Bigler, B.S., D.W. Welch, and J.H. Helle. 1996. A review of size trends among north Pacific salmon (*Oncorhynchus* spp.) Canadian Journal of Fisheries and Aquatic Sciences 53: 455-465

Bjornn, T.C. and D.W. Reiser. 1991. Habitat requirements of salmonids in streams. pp. 83-138, in Meehan [ed.] Influences of forest and rangeland management on salmonid fishes and their habitat. American Fisheries Society Publication 19, Bethesda, Maryland.

Brett, R. 1995. Energetics. *In* "Physiological ecology of Pacific Salmon" (C. Groot, L. Margolis and W.C. Clarke, Eds.), pp 1-68. UBC Press, Vancouver.

Chamberlin, T. W., R. D. Harr, and F. H. Everest. 1991. Timber harvesting, silviculture, and watershed practices. Pp. 181–205 in W. H. Meehan, ed. Influences of forest and rangeland management on salmonid fishes and their habitats. Amer. Fish. Soc., Bethesda, MD. Spec. Publ. 19.

Crossin G.T., S.G. Hinch, A.P. Farrell, D.A. Higgs, and M.C. Healey. 2004. Somatic energy of sockeye salmon at the onset of upriver migration: a comparison among ocean climate regimes. Fisheries Oceanography. 13:345–349.

Cox, S.P., and S.G. Hinch. 1997. Changes in size at maturity of Fraser River sockeye salmon (*Oncorhynchus nerka*) (1952-1993) and associations with temperature. Canadian Journal of Fisheries and Aquatic Sciences 54:1159-1165.

Diana, J. 1995. Biology and Ecology of Fish. Cooper Publishing. 441 pp.

Furniss, M. J., T. D. Roelofs, and C. S. Lee. 1991. Road construction and maintenance. Pages 297-323 *in* W.R. Meehan, editor. Influences of Forest and Rangeland Management on Salmonid Fishes and Their Habitats. American Fisheries Society Special Publication 19. 751 pp.

Hardy, T. B., R. C. Addley and E. Saraeva. 2006. Evaluation of Flow Needs in the Klamath River Phase II. Final Report. Institute for Natural Systems Engineering, Utah Water Research Laboratory, Utah State University, Logan, Utah 84322-4110. Prepared for U. S. Department of the Interior. July 31. 229 pp.

Hicks, B.J, J.D. Hall, P.A. Bisson, and J.R. Sedell 1991. Responses of salmonids to habitat changes, pp. 483-518, in Meehan [ed.] Influences of forest and rangeland management on salmonid fishes and their habitat. American Fisheries Society Publication 19, Bethesda, Maryland.

Gilhousen, P. 1990. Pre-spawning mortalities of sockeye salmon in the Fraser River system and possible causal factors. International Pacific Salmon Fisheries Commission. 26:1-58.

Keefer M.L., C.A. Peery, M.J. Heinrich. 2008. Temperature-mediated en route migration mortality and travel rates of endangered Snake River sockeye salmon. Ecology of Freshwater Fish. 17: 136–145.

King, H.R., N.W. Pankhurst, M. Watts, and P.M. Pankhurst. 2003. Effect of elevated summer temperatures on gonadal steroid production, vitellogenesis and egg quality in female Atlantic salmon. Journal of Fish Biology 63:153–167.

Lee, C.G., A.P. Farrell, A. Lotto, M.J. MacNutt, S.G. Hinch, and M.C. Healey. 2003. The effect of temperature on swimming performance and oxygen consumption in adult sockeye (*Oncorhynchus nerka*) and coho (*O. kisutch*) salmon stocks. Journal of Experimental Biology. 206: 3239-3251.

Lindley, S., C. Legault, P. Mundy, J. Murphy, and R. Waples. 2006. NMFS Science Centre Evaluation of the Peer Reviews of the Long-Term Central Valley Project and State Water Project Operations Section 7 Consultation. 16 pp.

McElhany, P., M. H. Ruckelshaus, M. J. Ford, T. C. Wainwright, and E. P. Bjorkstedt. 2000. Viable Salmonid Populations and the Recovery of Evolutionarily Significant Units. NOAA Tech. Memo. NMFS-NWFSC-42. U.S. Dept. of Commerce. National Oceanic and Atmospheric Administration. National Marine Fisheries Service. 156 pp.

Meehan 1991. Influences of forest and rangeland management on salmonid fishes and their habitat. American Fisheries Society Publication 19, Bethesda, Maryland. 751 pp.

Murphy and Meehan 1991. Stream ecosystems, pp. 17-46, in Meehan [ed.] Influences of forest and rangeland management on salmonid fishes and their habitat. American Fisheries Society Publication 19, Bethesda, Maryland.

Northcote, T. and G. Hartman. 2004. Fishes and Forestry: Worldwide watershed interactions and management. Blackwell, Oxford. 789 pp.

Rand, P.S., S.G. Hinch, J. Morrison, M.G.G. Foreman, M.J. MacNutt, J.S. Macdonald, M.C. Healey, A.P. Farrell, and D.A. Higgs. 2006. Effects of changes to river discharge, temperature, and future climates on energetics and mortality of adult migrating Fraser River sockeye salmon. Transactions of the American Fisheries Society. 135:655-667.

Van West, P. 2006. *Saprolegnia parasitica*, an oomycete pathogen with a fishy appetite: new challenges for an old problem. Mycologist 20:99-104.

Wagner, G.N., S.G. Hinch, L.J. Kuchel, A. Lotto, S.R.M. Jones, D.A. Patterson, J.S. Macdonald, G. Van Der Kraak, M. Shrimpton, K.K. English, S. Larsson, S.J. Cooke, M.C. Healey, and A.P. Farrell. 2005. Metabolic rates and swimming performance of adult Fraser River sockeye salmon (*Oncorhynchus nerka*) after a controlled infection with *Parvicapsula minibicornis*. Canadian Journal of Fisheries and Aquatic Sciences. 62: 2124–2133

Welch, D.W., Y. Ishida, and K. Nagasawa. 1998. Thermal limits and ocean migrations of sockeye salmon (*Oncorhynchus nerka)*: long-term consequences of global warming. Canadian Journal of Fisheries and Aquatic Sciences. 55:937-948.

Attachment A

Statement of Work for Dr. Scott Hinch

External Independent Peer Review by the Center for Independent Experts

Assessment of NMFS' Draft Biological Opinion on the Bureau of Reclamation's Klamath Project Operation

Project Background:

The purpose of this independent review is to evaluate and comment on the use of the best available scientific and commercial information in our draft biological opinion concerning effects of the Bureau of Reclamation's (Reclamation's) Klamath Project Operations (Project) on the listed threatened Southern Oregon/Northern California Coast (SONCC) coho salmon (*Oncorhynchus kisutch*) and its designated critical habitat for the period of 2008 through 2018. The review will focus on the technical aspects of the NMFS draft biological opinion; the review will not determine if NMFS' conclusions regarding the project's potential to adversely modify or destroy critical habitat or jeopardize the continued existence or recovery of listed SONCC coho salmon are correct.

Due to water limitations to meet all of the needs of humans, wildlife and fisheries resources, NMFS' 2001 and 2002 biological opinions on the effects of the Bureau of Reclamation's Klamath Project Operation (Project), including water deliveries to the Klamath Irrigation Project, have been subject to intense scrutiny and litigation. In an effort to ensure we correctly analyzed the effects of the Project, NMFS sought review from the National Academies Committee on Endangered and Threatened Fishes in the Klamath River Basin (NRC) on the strength of scientific support for the biological assessment and biological opinion. The NRC released its 2002 Interim Report on NMFS' 2001 biological opinion and their conclusions included:

- A lack of evidence indicating high mainstem flows influence coho year class strength.
- The relative increase in available habitat for coho salmon in the mainstem Klamath River resulting from higher flows required in NMFS' Reasonable and Prudent Alternative to the Proposed Action were minor.
- A lack of scientific evidence in the Klamath River of a positive relationship between mainstem Klamath River flows and coho smolt survivorship.
- Higher summer flows could be disadvantageous by further increasing water temperature and reducing available thermal refugial habitat in the mainstem Klamath River.

Following the release of NMFS' 2002 biological opinion on the Project for the period 2002-2012, the NRC released their Final Report on Endangered and Threatened Fishes in the Klamath River Basin (2004) in which the above conclusions were reiterated and additional information and recommendations for the continued survival of Klamath River coho salmon were provided.

Coincident to the NRC's review and recommendations, NMFS sought peer review on its Central Valley Project and State Water Project Operations, Criteria, and Plan (OCAP) biological opinion. NMFS asked the CalFed Bay–Delta Authority Science Program (CBDA) and the Center for Independent Experts (CIE) each to conduct independent peer reviews to evaluate whether the scientific information used in the biological opinion was the best available. The peer review reports raised multiple and complex issues that merited evaluation in the context of future improvements to NMFS' biological opinions on large-scale projects (*i.e.*, OCAP, Klamath Project Operations). In response to the OCAP reviews, NMFS' Science Center developed recommendations and guidance for the development of future NMFS biological opinions. NMFS' Science Center Review (Lindley *et al.* 2006) includes recommendations to improve the conceptual framework of section 7 analyses on large-scale projects. NMFS has in hand a general life cycle approach outlined by the Viable Salmonid Populations (VSP) report (McElhaney *et al*. 2000). VSP is accepted by NMFS as best available science. Lindley *et al.* (2006) concluded that within the framework provided by VSP, further improvements could be made by systematically examining all of the important linkages between project effects and VSP parameters, addressing climate variation and climate change, accounting for uncertainty, and making the connections between data, assumptions, analyses, and conclusions more transparent.

<u>New Information</u>:

NMFS' draft biological opinion will utilize the body of new scientific information on coho salmon in the Klamath River. This information includes (1) SONCC Technical Recovery Team documents defining the historical population structure of Klamath River basin coho salmon (Williams *et al*. 2006), and population viability (Williams *et al*. 2007); (2) Cramer Fish Sciences Klamath River Coho Life Cycle Model; (3) Evaluation of Instream Flow Needs in the Lower Klamath River Phase II Final Report (Hardy *et al*. 2006) ; (4) Reclamation's Undepleted Natural Flow Study Final Report (Reclamation 2005); (5) NRC's Review of Hardy et al. 2006, and Reclamation 2005; (6) new information on the effects of mainstrem flow and water quality on fish disease; and (7) other information provided in Reclamation's final biological assessment (2007). The breadth of new information includes disparate conclusions relevant to the potential effects of the Project on coho salmon and NMFS will need to reconcile these disparate conclusions in our draft biological opinion.

Overview of CIE Peer Review Process:

The Office of Science and Technology implements measures to strengthen the National Marine Fisheries Service's (NMFS) Science Quality Assurance Program (SQAP) to ensure the best available high quality science for fisheries management. For this reason, the NMFS Office of Science and Technology coordinates and manages a contract for obtaining external expertise through the Center for Independent Experts (CIE) to conduct independent peer reviews of stock assessments and various scientific research projects. The primary objective of the CIE peer review is to provide an impartial review, evaluation, and recommendations in accordance to the Statement of Work (SoW), including the Terms of Reference (ToR) herein, to ensure the best available science is utilized for the National Marine Fisheries Service management decisions.

The NMFS Office of Science and Technology serves as the liaison with the NMFS Project Contact to establish the SoW which includes the expertise requirements, ToR, statement of tasks for the CIE reviewers, and description of deliverable milestones with dates. The CIE, comprised of a Coordination Team and Steering Committee, reviews the SoW to ensure it meets the CIE standards and selects the most qualified CIE reviewers according to the expertise requirements in the SoW. The CIE selection process also requires that CIE reviewers can conduct an impartial and unbiased peer review without the influence from government managers, the fishing industry, or any other interest group resulting in conflict of interest concerns. Each CIE reviewer is required by the CIE selection process to complete a Lack of Conflict of Interest Statement ensuring no advocacy or funding concerns exist that may adversely affect the perception of impartiality of the CIE peer review. The CIE reviewers conduct the peer review, often participating as a member in a panel review or as a desk review, in accordance with the ToR producing a CIE independent peer review report as a deliverable. The Office of Science and Technology serves as the COTR for the CIE contract with the responsibilities to review and approve the deliverables for compliance with the SoW and ToR. When the deliverables are approved by the COTR, the Office of Science and Technology has the responsibility for the distribution of the CIE reports to the Project Contact.

Requirements for CIE Reviewers:

The CIE shall provide three independent scientists to conduct an independent peer review; this review will be conducted as a desk review and no travel is required. Expertise is required in water manipulation and management, instream flow and salmonid habitat modeling, application of the Endangered Species Act, salmonid population risk assessment methodologies, and conservation biology. Each reviewer’s duties shall not exceed a maximum of 7 days to conduct the literature review, peer review, and completion of the CIE peer review report in accordance to the Terms of Reference (ToR).

Statement of Tasks for CIE Reviewers:

The CIE reviewers shall conduct necessary preparations prior to the peer review, conduct the peer review, and complete the deliverables in accordance with the ToR and milestone dates as specified in the Schedule section.

<u>Prior to the Peer Review</u>: The CIE shall provide the CIE reviewers contact information (name, affiliation, address, email, and phone) to the Office of Science and Technology COTR no later than the date as specified in the SoW, and this information will be forwarded to the Project Contact.

<u>Pre-review Documents</u>: Approximately two weeks before the peer review, the Project Contact will send the CIE reviewers the necessary documents for the peer review, including supplementary documents for background information. The CIE reviewers shall read the pre-review documents in preparation for the peer review.

CIE reviewers shall review the following document which is the focus of the questions listed above:

1. NMFS’ Draft Biological Opinion on Bureau of Reclamation’s Klamath Project Operations 2008-2018.

2. To aid the reviewers, copies of relevant documents cited in this statement of work will be provided.

The above material will be provided by the NMFS Southwest Regional's (SWR) Project Contact.

This list of pre-review documents may be updated up to two weeks before the peer review. Any delays in submission of pre-review documents for the CIE peer review will result in delays with the CIE peer review process. Furthermore, the CIE reviewers are responsible for only the pre-review documents that are delivered to them in accordance to the SoW scheduled deadlines specified herein.

Desk Peer Review:

The primary role of the CIE reviewer is to conduct an impartial peer review in accordance to the ToR herein, to ensure the best available science is utilized for the National Marine Fisheries Service (NMFS) management decisions (refer to the ToR in Annex 1).

The itemized tasks for each reviewer consist of the following.

1. Read the draft biological opinion with a focus on the effects analysis.

2. Consider additional scientific information as necessary.

3. The CIE reviewers shall conduct an independent peer review and complete an independent peer-review report addressing each task in accordance to the Terms of Reference with a copy each sent to Dr. David Die at ddie@rsmas.miami.edu and Mr. Manoj Shivlani at shivlanim@bellsouth.net.

Each report is to be based on the individual reviewer's findings, and no consensus report shall be accepted.

Terms of Reference

CIE reviewers shall evaluate the draft Opinion to determine whether the following questions resulting from the Science Center review are adequately addressed:

1. Does the draft biological opinion incorporate an ecological framework that emphasizes the geographic structure of habitats, populations, and diverse salmon life histories that contribute to salmon resilience and productivity (*i.e.*, VSP concept, see McElhaney *et al*. 2000 and Lindley *et al*. 2006)?
2. Does the draft biological opinion consider a range of climatological conditions and water demand scenarios in the analysis?
3. Does the draft biological opinion consider a range of ocean conditions in the analysis?
4. Does the draft biological opinion consider the effects of hatchery fish on listed fish?

Additionally, CIE reviewers shall evaluate the draft biological opinion to determine whether the following questions resulting from the NRC's 2002 and 2004 reports are adequately addressed:

5. Did NMFS' draft biological opinion present convincing scientific evidence about the spatial and temporal extent of young-of-year and juvenile coho salmon use and occurrence in the mainstem Klamath River?
6. Has the draft biological opinion adequately evaluated the potential effects of mainstem flows on the survivorship of coho smolts?
7. Are the draft biological opinion's scientific findings on the influence of mainstem flows on the spatial and temporal extent of coho juvenile survivorship in the summer months scientifically supportable?

Schedule of Milestones and Deliverables:

5 March 2008	CIE shall provide the COTR with the CIE reviewer contact information, which will then be sent to the Project Contact
5 March 2008	The Project Contact shall send the CIE Reviewers the pre-review documents
19 March 2008	Each reviewer shall submit an independent peer review report to the CIE
2 April 2008	CIE shall submit draft CIE independent peer review reports to the COTRs
11 April 2008	CIE shall submit final CIE independent peer review reports to the COTRs
15 April 2008	The COTRs shall distribute the final CIE reports to the Project Contact

Submission and Acceptance of Deliverables (CIE Reports):

Upon review and acceptance of the CIE reports by the CIE Coordination and Steering Committees, CIE shall send via e-mail the CIE reports to the COTRs (William Michaels William.Michaels@noaa.gov and Stephen K. Brown Stephen.K.Brown@noaa.gov) at the NMFS Office of Science and Technology by the date in the Schedule of Milestones and Deliverables. The COTRs will review the CIE reports to ensure compliance with the SoW and ToR herein, and have the responsibility of approval and acceptance of the deliverables. Upon notification of acceptance, CIE shall send via e-mail the final CIE report in *.PDF format to the COTRs. The COTRs at the Office of Science and Technology have the responsibility for the distribution of the final CIE reports to the Project Contacts.

Key Personnel:

Contracting Officer's Technical Representative (COTR):

William Michaels
NMFS Office of Science and Technology
1315 East West Hwy, SSMC3, F/ST4, Silver Spring, MD 20910
William.Michaels@noaa.gov Phone: 301-713-2363 ext 136

Stephen K. Brown
NMFS Office of Science and Technology
1315 East West Hwy, SSMC3, F/ST4, Silver Spring, MD 20910
Stephen.K.Brown@noaa.gov Phone: 301-713-2363 ext 133

Contractor Contacts:

Manoj Shivlani, CIE Program Manager
10600 SW 131st Court, Miami, FL 33186
mshivlani@ntvifederal.com Phone: 305-383-4229

Roger Peretti, NTVI Regional Manager
Northern Taiga Ventures, Inc., 814 W. Diamond Ave., Ste. 250, Gaithersburg, MD 20878
rperetti@ntvifed.com Phone: 301-212-4187

Project Contact:

James Simondet
NMFS, Southwest Region, 1655 Heindon Road, Arcata, CA 95521
Jim.Simondet@noaa.gov Phone: 707-825-5171

Other Project Contacts:

Russell Strach
NMFS, Southwest Region, 650 Capitol Mall, Suite 8-300, Sacramento, CA 95814
Russ.Strach@noaa.gov Phone: 916-930-3621

Irma Lagomarsino
NMFS, Southwest Region, 1655 Heindon Road, Arcata, CA 95521
Irma.Lagomarsino@noaa.gov Phone: 707-825-5160

Request for Changes:

Requests for changes shall be submitted to the Contracting Officer at least 15 working days prior to making any permanent substitutions. The Contracting Officer will notify the Contractor within 10 working days after receipt of all required information of the decision on substitutions.

The contract will be modified to reflect any approved changes. The Terms of Reference (ToR) and list of pre-review documents herein may be updated without contract modification as long as the role and ability of the CIE reviewers to complete the SoW deliverable in accordance with the ToR are not adversely impacted.

ANNEX 1

REPORT GENERATION AND PROCEDURAL ITEMS

1. The report shall be prefaced with an executive summary of comments and/or recommendations.

2. The main body of the report shall consist of a background, description of review activities, summary of analyses and comments, and conclusions/recommendations.

3. The report shall also include as separate appendices the bibliography of materials reviewed and a copy of the statement of work.