CIE Review of "Modeling habitat capacity and population productivity for spring-run chinook salmon and steelhead in the upper Yuba River watershed"

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Executive Summary

This document is a review of "*Modeling habitat capacity and population productivity for spring-run chinook salmon and steelhead in the upper Yuba River watershed*" (Stillwater Sciences 2012), a report that uses habitat and population modelling to estimate the potential of the upper Yuba River, California, basin to produce chinook salmon and steelhead trout in the event of a reintroduction of these species. The analysis uses a temperature model to identify reaches of thermally suitable reaches, a mesohabitat analysis to quantify the habitat characteristics, and biologically-based models or analysis to make predictions about the productive potential of four reaches of the Yuba River to produce salmon or trout.

The analysis contains much useful information that will assist planning for a potential reintroduction. The summer temperature predictions and habitat inventories will help establish whether viable populations can be established, and the preliminary estimates of smolt outputs give a sense of scale of production that can be expected.

I found that greater attention should be paid the details of the life history of each fish species and in particular the temperature analysis should be broadened to consider other life stages and times of years. Chinook salmon freshwater life histories, in particular, can respond to the thermal regime, and that will affect the relative importance of specific habitats at different times of the year.

I also note that "habitat" has more dimensions that those considered in this report and a full assessment should consider water quality (and productivity), the seasonal flow and temperature regime, and micro habitat conditions, and perhaps other feature. The report seems to assume that all of these factors are adequate for salmonid production but few details are provided.

In any modelling analysis difficult choices have to be made between simple models that rely on few parameters or proxies and make "first cut" predictions, and more complex models that contain detailed data, correct model specifications and parameters. Detailed models have the potential to provide precise predictions, but the accuracy of those predictions is entirely contingent on specifying the model correctly and having appropriate parameters.

Particularly for chinook salmon I suggest the use of simpler approaches to modeling productive capacity of the Yuba tributaries. I have concerns that the assumptions and parameters of the POP model for chinook salmon far outstrip our knowledge of the

species (and in fact the formulation of the model seems to contradict empirical information). The model can probably be adjusted and tuned but ultimately its predictions should be compared to other approaches. Simpler models may be sufficient given the many other uncertainties in the planning process.

Key Recommendations

1. Temperatures should be modelled for each month of the year along each reach and for each management scenario. Available habitat should be identified for each life stage based on seasonal habitat use.
2. Chinook salmon incubation rates, emergence timing and juvenile growth rates should be modelled from the temperature data. These can then be compared to observations from other systems to help clarify the life history that might occur in the Yuba River (in particular, the relative proportions and significance of each smolt type).
3. Alternative simpler models should be used to make predictions of chinook and possibly steelhead trout populations to compare to the HAB/POP results
4. The chinook salmon model should be modified and "tuned" to better emulate known properties of chinook salmon population dynamics.

The Review

This document is a review of "*Modeling habitat capacity and population productivity for spring-run chinook salmon and steelhead in the upper Yuba River watershed*" by Stillwater Sciences (February 2012). This project attempts to estimate the potential for habitats in the upper Yuba River to support anadromous salmonids in the event that passage around the Englebright dam provided. This is a complex problem that has many uncertainties that extend beyond the domain of freshwater habitat capacity. Nonetheless the current project makes a valuable contribution to the information base needed for decision making.

As provided in the Statement of Work, the goals of this review are:

1. *Review the RIPPLE model application for the upper Yuba River (Stillwater Sciences 2012) to determine whether the data sets, assumptions, and model parameters represent a reasonable modeling approach to assess the relative potential of upper Yuba River habitats under the three different modeled scenarios.*
2. *Does the RIPPLE model application for the upper Yuba River produce results that are relevant and appropriate to support the evaluation of anadromous fish reintroduction potential in the upper Yuba River watershed?*

The primary document that was reviewed was entitled "*Modeling habitat capacity and population productivity for spring-run chinook salmon and steelhead in the upper Yuba River watershed*" by Stillwater Sciences, dated February 2012 (Appendix 1).

The review was conducted in Vancouver BC in late August and September 2012. In this review I first provide a general overview of the review, and follow with detailed comments by section. I conclude with some recommendations.

Background and Overview

Attempts to predict the potential productivity of reintroduced salmonids to the upper Yuba are necessarily fraught with challenges and uncertainties, and it is likely that a multifaceted approach is needed given that each approach will have its strengths and limitations. The relatively detailed modelling of freshwater habitats generated by the RIPPLE model provides useful insights, but the results have to be tempered by the relatively large demands for parameters and model structures needed to generate them. Simpler approaches are less demanding of assumptions, and their results are less likely to be biased (due to model mis-specification). The trade-off is that predictions will be less precise due to the generality of the model.

The Agencies may wish to consider a hierarchical approach of increasing complex approaches. For chinook salmon, I first suggest using the model of Liermann et al. (2010) to develop some coarse estimates of potential population size and productivity. These authors developed empirical predictive models based on watershed size using data from a variety of North American chinook salmon stocks. The model will provide “order of magnitude” estimates for S_{msy} , the number of spawners that generate the maximum sustainable yield, on average. Their model won’t be able to accommodate the habitat restrictions imposed by water temperature, so to the extent overall production is limited by temperature, the model’s predictions may be overestimates. Unfortunately I don’t think there is an equivalent model for steelhead.

A useful next step would be to compile smolt-spawner data to develop expectations for smolt yield per length of rearing stream. These types of models (Bradford et al. 1997, 2000 for coho salmon) provide sufficiently accurate estimates for planning processes without the need for a large suite of vital rate and habitat measurements. Compilations have not been attempted for chinook salmon, but the data probably exist (see Bradford et al. 2008). Such data will be sparse for steelhead and will take some effort to compile. Smolt data are more direct than using summer parr densities corrected for overwinter survival, but that latter may have to be used. This approach does not use detailed stream habitat data to estimate stream-specific habitat quality. Instead, through the compilation of data from suite of streams ‘average’ conditions are represented in the analysis.

A second key parameter is productivity, expressed as the slope of the smolts/spawner relation at low spawner abundance. Such data may be more difficult to find, but with the recent resurgence in monitoring programs, estimates may be available. Petrosky et al. (2001; chinook) and Yuen and Sharma (2005; steelhead) are examples of dataseries suitable for this type of analysis. Such data are required for population modelling although the need is less critical for estimating habitat capacity and the dynamics of populations at abundance levels that can fully seed habitats.

The most detailed form of modelling is the stage-specific habitat and population modelling attempted in this report. Because chinook salmon life history is likely determined by a combination of genetic, environmental and density drivers, it is critical to identify how these factors will interact, both for the population dynamics model and to define a set of habitat requirements that will lead to viable populations. The current model makes a number of key assumptions about chinook life history and population biology that seem reasonable, but are not supported by empirical analysis (and appear at odds with a few data that I have examined). Because these issues are not easily resolvable, the value of the POP analysis for chinook salmon is unclear, relative to simpler approaches. My concerns are elaborated in the section-specific comments below.

Part of my motivation for promoting simpler approaches to estimating habitat capacity is freshwater habitat capacity is only part of the suite of analyses required to determining if reintroduction of salmonids above the dam will create (or contribute to) a viable population complex. As noted before, the dynamics of temperature suitability will affect productivity, as will the effects of the thermal regime on chinook life history. Survival of age-0 chinook smolts in downstream habitats may play an important implication for adult production.

Summary of Findings for each ToR

Here I synthesize my comments in response to the 2 questions posed in the Statement of Work.

Determine whether the RIPPLE model application for the upper Yuba River (including data sets, assumptions, and model parameters) represent a reasonable modeling approach to assess the relative potential of upper Yuba River habitats under the three different modeled scenarios.

Salmonid "habitat" has many dimensions, and the RIPPLE application primarily focuses on a subset of these: mesohabitat (pools, riffles, spawning areas), wetted area, and water temperatures during mid-summer. Other factors (year-round temperatures, microhabitats, water quality, lower trophic productivity, stream bed condition, biotic interactions) are not considered in the current analysis.

It is implied in the analysis that habitat factors that aren't explicitly considered are not limiting salmonid production. By using abundance data from other streams, it is assumed that the overall habitat conditions in the Yuba are within the range of those other streams. Presumably the authors are comfortable with this assumption based on their familiarity with the river, however, it should be articulated in the report.

The characterization of microhabitats can be useful in considering the suitability of habitats. The hydraulic geometry analysis in RIFFLE does provide a means of predicting channel width (which is needed for wetted area and habitat area calculations), but information on hydraulic habitat is not provided. For example, for larger rivers, or during periods of higher flows wetted area is not a good measure of available habitat, the centre of the channel may not be suitable for juvenile salmonids (particularly age-0 fish) because of high velocities. I assume that because the HAB analysis is conducted during the low flow period, and given the flows identified in Figure 1-1, the full width of the channel is suitable as rearing habitat. As indicated below, the dynamics of chinook salmon may be dictated by events that take place prior during the high flows of spring and early summer when microhabitat conditions could be important.

Conversely, age-1 trout generally prefer deeper and faster flows than age-0 fish, and microhabitat information may be useful to refine estimates of suitability, particularly if low flows may not produce the hydraulic conditions preferred by this life stage.

The stated goal of the HAB model is to "calculate stage-specific carrying capacities" (p. 37). Implied in this concept is that each stage abundance is "capped" at the carrying capacity of the habitat for that stage. Certainly for chinook salmon there is no evidence for hard caps; a better case may be made for steelhead, although the information base is poor (see sections below). This is a very strong assumption about the biology of these species that needs to be well supported by analysis or data from other populations.

I think it more useful to recast these sections as "habitat supply" or "habitat abundance" and use the calculated quantities as rough checks on any imbalances that might occur among life stages or habitat types. Using habitat abundance as a density dependent factor might be appropriate for a few life stages where there is empirical justification (late summer rearing for Chinook 0+ that will become yearling migrants and steelhead 1+).

I note in the detailed comments a number of key model assumptions and parameters need to be better justified or modified in light of empirical results for other populations.

In summary, the approach is reasonable, and the quantification of thermally suitable habitats is useful. There are many biological details in the detailed models, and in matching life histories to the habitat and physical conditions. It is important to be sure the analysis matches empirical evidence, and the implications of assumptions that have been made are clearly identified.

Question 2: *Does the RIPPLE model application for the upper Yuba River produce results that are relevant and appropriate to support the evaluation of anadromous fish reintroduction potential in the upper Yuba River watershed?*

Although the details of the summer temperature modelling are not provided in this report, the temperature-based habitat suitability analysis is a key component of the evaluation. The HAB component provides a useful inventory of the available habitats, and offers the potential to be converted to expected juvenile densities.

My impression is that the POP model is pushing at or beyond the limits of our knowledge of chinook salmon life history and interactions between populations and habitat. There are decidedly improvements that can be made to the implementation of POP in the current analysis, but whether further refinement of the model will provide significantly more useful information relative to the other sources of uncertainty outside of freshwater habitat is unknown.

I have provided suggestions on how the POP model might be revised, and some empirically-based approaches that could also be employed as alternatives. I have also suggested ways in which the POP results can be compared to demographic statistics for other populations as a way of truthing the models.

Ultimately, though, as long as the habitat estimates are approximately correct (which could be defined as similar to average conditions for the species), the analysis of freshwater habitat capacity will probably be sufficient in the context of the other sources of uncertainty in predicting the viability of reintroduced salmonid populations to the basin.

As an extension to the current work I would encourage using ouputs from POP to predict, or back-calculate, the likely escapement for each part of the Yuba Basin that has thermally suitable habitat, as population subdivision and fragmentation may be issues that could undermine the long-term viability of re-established populations. My concern is that the river segments identified as suitable in Tables 4-3 and 4-4 are not long enough to support viable populations. Escapements could be estimated by forward simulation, as was attempted by the POP model for chinook, or by back-calculation by determining how many spawners would be required to produce the expected number of juveniles (using information from other populations as biostandards). A full analysis of population viability will require some assumptions about metapopulation structure and the fidelity of spawners to natal areas. The resilience of a population (or segment of a population) will be proportional to its size, as very small aggregations of spawners will be vulnerable to genetic and demographic issues, and may not be able to withstand prolonged periods of adverse environmental conditions such as drought or poor ocean productivity.

Detailed comments on Stillwater Science 2012 to support Summary Findings

Section 2: Chinook salmon:

Modelling the freshwater dynamics of chinook is challenging as the species has a very flexible life history, both among and within populations. Further, the life history patterns of juveniles have been observed to change with anthropogenic manipulation of environmental conditions, adding to the complexity (Angilletta et al. 2008). Although the basic patterns are captured in Healey's (1991) review, the details vary considerably and a site-specific analysis is required.

While the general discussion on pages 6-8 is useful, it doesn't make full use of the existing data contained within the cited reports (and perhaps there are others that could also be brought to bear on this). Downstream trapping in Butte Creek shows that 80-95% of migrants captured were newly emerged fish, 5-18% were 1-3 months post emergence, and a very small fraction were "yearlings" (migrants > 80mm) that were captured in late fall to early spring (Ward et al. 2002). Although the capture efficiency for each size class undoubted differs somewhat, the vast numerical differences among groups are likely real. Slightly different ratios are suggested by Figure 25 in Lindley et al. (2004) but these are not quantified.

My observations have been the freshwater life history is the result of a complex interaction between habitat conditions, temperature and flow conditions, as is mentioned in the report. It appears that more could be done with existing data to better justify some of the assumptions and parameter estimates used in the model. Given the range of conditions for the three streams in Lindley et al. (2004), what can be learned about the relationship between temperature, flow, habitat, and migration timing or life history type? Although some of the rationale for model choices is indicated in the documentation, I think this could be strengthened with a more thorough review of existing information. Then the material on page 6 can be made more region-specific.

Section 2.1.2 of the report contains a number of assertions that are not supported by citation or data, and although the sections describing migration and spawning are not as critical to the model, assertions that spawning habitat can be a "key factor limiting production" needs to be supported. I anticipate that this will be difficult, and suggest that the assertion be better described as a hypothesis.

On page 8 it suggests that "fry in excess of carrying capacity are likely to disperse", however, it is unclear how general this statement is. For many populations, apparently including central California spring runs, there is an immediate migration of many newly emerged fry. For systems I am familiar with, this migration occurs during high spring flows in an environment where it unlikely that social interactions (feeding territories, aggregations, aggressive behaviours) will cause affect migration, due to high velocities, turbid flows, etc. Subsequent migrations of fish >40mm may be the result of behavioural interactions, but that will also depend on flow and habitat during the spring and summer months. Certainly later in the growing season when individual size (and territory size) increases and habitat availability decreases as flows recede, density limitations may come into play. However, the sentence on page 8 "summer rearing habitat limitations may also play a key role in regulating spring run populations" can only be true under the restrictive assumption that the summer rearing population contributes disproportionately (by orders of magnitude) to adult production. The recent analysis of Miller et al. (2010) for a fall-run population found that 68% of adult production resulted from migrants less than 75mm in length.

In Bradford et al. (2008) I describe some of these issues and test several hypotheses by assembling existing information on juvenile outmigration as a function of the number of eggs deposited each year for a handful of chinook salmon populations. The data are expressed as the proportion of the eggs deposited that migrate as juveniles (Figure 1). In the following paragraphs I review these and propose that a similar analysis be used to motivate and justify model structure choices for the POP model.

For the egg-emergent fry stage (left column, Figure 1), the proportion of the brood that migrates is independent of brood size, which does not support the hypothesis that spawner density impacts egg-fry survival. Density-dependent mortality in the egg-fry stage will result in a declining rate of fry production with increasing abundance. It is possible that spawner density was too low in these cases for density impacts to occur. The proportion of eggs that actually migrate as fry is likely a function of habitat conditions, flows and the location of the traps relative to the spawning areas. Although the database is limited the available evidence does not support the form used in the POP model.

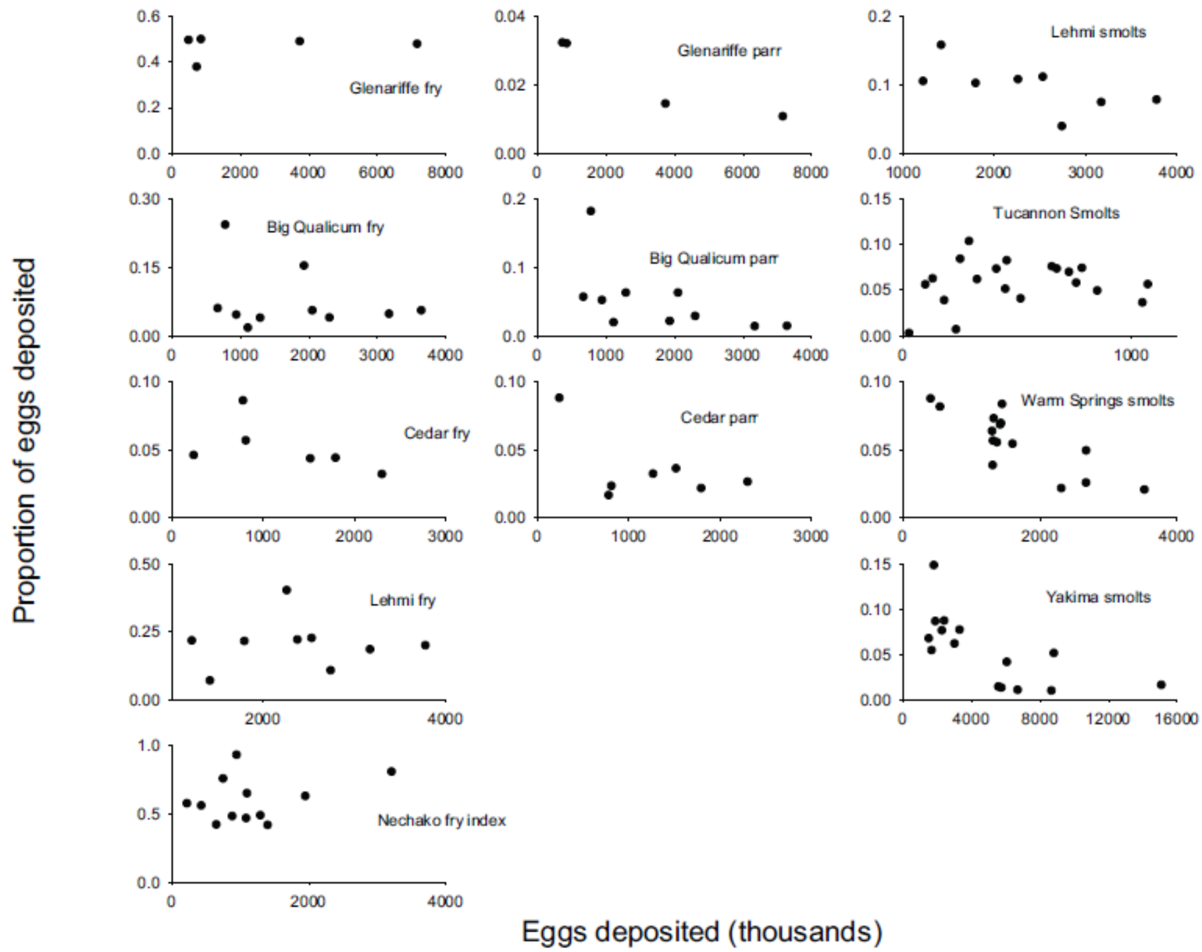

Figure 1. Population data from Bradford et al. (2008) for 8 chinook salmon populations. Data show the relation between the proportion of deposited eggs that migrate as fry, parr or yearling smolts, and the size of the brood. Decreasing trends indicate density-dependent mortality.

The limited data suggest that the production of age-0 parr migrants does decline with increasing density, suggesting density interactions in the early freshwater stage. The data do not support the hypothesis that age-0 migrants are those juveniles that are in excess of rearing habitat capacity (i.e., are forced migrants) as under that hypothesis, an increasing proportion of the brood would migrate as brood size increases. Most year-round trapping studies capture some juveniles every month of the year and it is very likely that these movements are due to ongoing competitive interactions.

Finally, age-1 smolt production rates tend to decline with increasing abundance, also suggesting density-dependent mortality. The data are not sufficient to determine if this is the result of additional density-dependence beyond that in the early freshwater stage that would affect the age-0 migrant production. However, these age-1 smolts migrate in the spring months and it is likely that some density-dependent interactions do occur during the summer, fall, and winter prior to emigration.

For the Snake River basin Petrosky et al. (2001) show the impact of density-dependence in each of the tributary streams is a smooth curve with a gradually declining rate of smolt production. At very low abundance the production rate is about 100 smolts/spawner, and declines to 50 smolts/spawner at higher abundance. These values are similar to those observed for coho salmon (Bradford et al. 2000). The data do not follow the “hockey stick” model that was used in the POP model, but the Snake River data combines a number of streams of different sizes and productivities and that may lead to a smoother curve as a result of aggregation. These are inland populations for which most migrants are true age-1 smolts that migrate in the spring months.

These comments are not intended to suggest that Yuba chinook salmon will follow the patterns indicated by other populations, but that there is empirical information available to inform decisions about life history processes in chinook salmon, and to provide an evidence-based approach to designing model structure. Certainly when the POP model components deviate significantly from the empirical information a strong rationale is required.

Section 2.2. Steelhead

I am not a steelhead expert but I am aware of more literature on their population biology than is acknowledged in the report. Information on microhabitat preferences for age-0 and age-1 trout are available and can be used to support the assertions about habitat use and habitat requirements. Other statements on their biology should be supported by reference to the literature. Not only does it make the report more credible, there is often insight (and data) within these other reports that can be directly relevant to the Yuba analysis.

4. Scenarios

The key factor in determining the amount of available habitat to salmonid populations in the Yuba basin is summer water temperature. Unfortunately, the description of the temperature modelling and the likely water management strategies is brief in the report. Some additional information on thermal regimes would be of great benefit to understanding the impact of water temperature on the sustainability of salmon stocks in the basin.

First, there is no mention of winter water temperatures. Chinook salmon spawner and eggs are sensitive to warm water during the initial phases of incubation, and that is identified in Table 2-1. However, development rate is also temperature sensitive, and the impacts of temperature and water management on the timing of emergence are well documented and can be modelled. Very small differences in the winter thermal regime (e.g., 1-2°C) can result in large changes in the eventual emergence dates. Watersheds with water storage projects tend to result in increased fall and winter water temperatures that accelerate development (Bradford et al. 2011). In more northern or high elevation climates this can cause fry to emerge into prohibitively cold temperatures; however, in California early emergence likely increases the length of the growing season for fry resulting in an increase size at date. This may have a strong influence on life history as early emergence and juvenile growth can increase the proportion of parr migrants relative to yearlings (Angilletta et al. 2008). Data provided in Lindlay et al. (2004) support this as the higher elevation populations have a later timing for fry migration, and there appears to be a corresponding increase in the number of yearling migrations. It would be straightforward to use an incubation model for chinook salmon to predict emergence timing in each reach of the Yuba River, and use the timing to either model juvenile growth and implications for life history strategies, or compare to existing data. If the expected juvenile life history types can be predicted with the thermal regime, the contribution of the yearling types and the impacts of temperature management on them can be determined. There is the potential to use water releases to manipulate emergence timing and parr growth if there are benefits to doing so.

Second, temperature modelling should be conducted on a monthly time step and the analysis should consider the availability of habitat by month. The current approach of considering only temperatures during the peak of summer ignores the interaction between the thermal regime and the life histories of each species. Available habitat for spawning, incubation, and early freshwater rearing may be quite different than the summer habitat, and an understanding of the seasonal dynamics of habitat can assist in better matching habitat availability, life histories and behaviour.

For chinook salmon the summer holding habitat may indeed be limited to headwater areas, but it is unclear how temperature-suitable holding habitat will expand as fall temperatures decline. In many chinook, rivers spawners migrate to lakes or holding areas in the upstream reaches, and move downstream as water temperatures cool in the fall. This could occur in the Yuba Basin and it means that the available spawning areas could be much larger than those encompassed by suitable mid-summer holding temperatures.

Data from Lindley et al. (2004) and Miller et al. (2010) suggest that in some Central Valley streams many (or most) juveniles will migrate from natal areas by June of their first year. Detailed mapping of water temperatures by month would also assist in evaluating the interaction between those areas that are used for spawning, those that have conditions suitable to produce parr migrants, and areas that can potentially be used for over-summer rearing (and age-1 smolt production). Temperature-based growth modelling may assist in determining the size that could be reached by parr during the first few months after emergence. Presumably if juveniles reach sufficient size (80mm is indicated in Miller et al. 2010), they could successfully emigrate before the onset of warm temperatures and contribute to the adult returns with a minimal requirement for summer rearing habitat.

The situation for steelhead is a little more complicated, as spawners are unlikely to run into temperature issues during holding and spawning, and therefore spawning could occur throughout each reach as habitat conditions and impasses allow. It is unclear what would be the fate of juveniles that find themselves in reaches that eventually too warm, but the approach that was taken considers that production will only occur from the thermally suitable reaches is likely conservative. At a minimum I would suggest that the existing calculations of suitable habitat be labelled “rearing habitat” as I don’t believe these are relevant for the distribution of spawning. The interaction between spawning distribution and summer rearing temperatures should be identified as a key uncertainty for production modelling.

Section 5, GEO

Not being a geomorphologist I have no major comments on this section.

Section 6 HAB

While the concept of stage-specific habitat limitation is useful, the notion of a “hard” cap at each life stage needs to be examined carefully. Although no equations are presented in this section, the implication is that the process by which density-dependent process occurs is through the use of a hard cap. This type of process has been modelled in the stock-recruit context by the so-called "hockey stick" model (Bradford et al. 2000), which explicitly models the hard cap, or the smooth Beverton-Holt function that has an asymptotic form. The hard-cap model has empirical support for juvenile coho salmon production (Bradford et al. 2000) because the strong territoriality displayed by juveniles can generate the space filling or hard cap model. The model assumes that once all of the available "spaces" is filled, other individuals perish or are forced to move elsewhere and suffer higher mortality as a consequence.

However, the evidence that this process is applicable to other life stages and species is less convincing. In many cases performance (growth, survival, movement) will decrease gradually change with abundance and the resulting function will be curvilinear, rather than asymptotic.

6.2.1 Chinook Model

Critical to the analysis of the potential for habitats to produce chinook salmon is the population model that links habitat quantity and population biology. Unfortunately this section is not well documented and should be populated with equations and clear definitions of parameters, variables, etc. Models forms and parameter choices should be supported by literature or identified as assumptions. Appendix D is helpful, but is not sufficient.

The following comments are in sequence following the description on page 48.

There is no mention of upstream migration in the Sacramento River in the model (presumably affecting the *escape* group). Migration mortality is of increasing importance in many salmon populations and land use and climate change is leading to gradually increasing temperatures. I assume that the early timing of the run minimizes risk of temperature exposure (and disease) and that there's no harvest or other losses. This should be documented.

The model assumes "*holders*" are distributed evenly in proportion to holding habitat and those in excess of the proposed maximum density are "truncated". I don't have experience with this prolonged holding behavior but my general observation is that adults tend to migrate as far upstream as possible and distribute downstream from there. More importantly, I don't know if there's any evidence that the "hockey stick" model would apply to holding salmon so that fish were excess of $1/m^2$ would be lost from the system. My sense is that densities would continue to climb until oxygen or disease issues became problematic. Perhaps there is information from hatchery operations where holding densities are often very high that could inform the discussion here, particularly if there are behavioral or territoriality issues during holding. Also, I found mention of the hockey stick model in the footnote on page 46, it should be provided in the text for each use, and values for the 2 parameters explicitly mentioned. Is *r* in the footnote the same as *holding_survival*? If so, the description on page 48 is not accurate.

Spawning. Page 48 mentioned the "effects of superimposition" but no details are provided. As noted earlier the use of the hockey-stick model should be justified as it is an exceptionally strong assumption. As noted in point #4 below, evidence for density-dependent egg-fry survival in chinook salmon is absent, largely because densities of spawners tend not to be high. For other salmon species, superimposition results in weak density dependence, although the potential for low oxygen levels and disease may occur in those species that spawn at very high densities. West and Mason (1987) provide detailed data for sockeye salmon, and show there is some evidence for density dependent survival; however, in general, fry production continues to increase with increasing spawner density. Some of the early studies on Alaskan pink salmon find evidence for reduced survival at very high densities, but for this species, the spawning densities are very high (and occur in waves), and oxygen and waste issues within the gravels becomes an issue. This is likely much less of an issue for chinook salmon as the bed material is much coarser and stream flows are usually much stronger, increasing irrigation to the eggs and alevins.

Egg-emergent fry survival data for chinook salmon are scarce, but the few values I am aware of suggest values in the range of 30-50% survival (see Bradford et al. 2008). I am skeptical that survival would ever be as high as 76% on a river-wide basis (Table G-1). Data for both coho salmon (Bradford et al. 2000) and chinook salmon (Bradford et al. 2008) find no evidence of density-dependent survival in the egg-migrant fry stage as the proportion of migrants is independent of brood size.

The model assumptions for survival and redistribution of fry in summer should match the observed patterns in nearby streams (i.e. Lindley et al. Fig 25). My interpretation of that figure is that there is a large migration of juveniles in March-May. This is a time when flows are high (Fig 1-1), the fish are small and are undoubted growing rapidly. Is this the period when strong density-dependent migration occurs, or is it more likely when the streams reach baseflows later in summer and fish establish rearing territories across the width of the stream? Are the stream capacity values provided in Table 6-3 from sampling in March-May or later in the year when flows are low and water clarity is high?

If fry surplus to stream capacity were forced to migrate, then the proportion of migrators should increase with increasing cohort size as the stream capacity is fixed across brood size. My review of a few of the relevant datasets suggests the exact opposite occurs, that is, the proportion of migrators decreases slightly with increasing brood size (Bradford et al. 2008). This would occur if there is some density dependent mortality at some point in the egg-migrant parr interval. The exact mechanism of these interactions is unknown but it is not the result of competition at summer low flows.

Thus another model that could be considered is that age-0 migration is largely size/growth/condition dependent and the fish that are migrating in the spring months have reached a sufficient condition to move downstream. This process could be modelled by assuming a constant fraction of *fry* migrate in Mar-May, perhaps dependent on growth conditions during the first few months after emergence, and coupled with weak density-dependence as indicated by Bradford et al. (2008; Figure 1 above).

Does model fish actually "redistribute downstream in search of available habitat"? Can the process be summarized by equations?

To summarize, I recommend that the structure of the POP model be carefully evaluated to determine if it is consistent with empirical information for similar populations. Full documentation of the model structures is also required.

Chinook POP Model results:

It is informative to compare results of the POP model to information on other chinook salmon populations to gain some confidence in the reasonableness of the model. I looked specifically at the North Yuba values in Table 6-7 for that comparison. Total egg-smolt survival rates can be compared to a compilation of data available in Bradford (1995) who found an average survival rate of 7% for a variety of stream and ocean-type populations. Those values should be compared to the smolt/egg ratio for *smolt1+smolt0,* the number smolts leaving the natal stream or *esmolt0+smolt0+smolt1*, the total number of all smolts produced. The egg-smolt survival rates for these two groups are 11.5 and 19% respectively, higher by a factor of 2 compared to empirical data (Bradford 1995). This suggests that one or more survival rates in the egg-smolt stage are too high, unless there is reason to believe that the Yuba population would be much more productive than the chinook population contained in the review.

A second check on the in-stream production of juvenile is the rate of yearling production/stream length. Although I have not seen a compilation of data for chinook salmon, abundant information for coho salmon suggests values of 1000-2000 smolts/km of stream are typical, regardless of stream size. For the North Yuba River, the model predicts a lineal density of 9700 smolts/km, far beyond values known for coho salmon and likely other species. Since the summer densities of juveniles was capped at the observations of typical densities in other streams, it is conceivable that the *summer0_winter1* survival is too high. I would prefer that parameters in the model be anchored on available empirical evidence (even if it is sparse). In Bradford et al. (2001) we estimated overwinter survival at 22% in a northern stream--there are a few citations in our paper for other sources of information, and I am sure a literature review might yield a few more.

On the other hand, “yearlings” in California are sometimes defined as late fall migrants, which can be very numerous in some systems. These would be expected to have higher survival rates than fish that overwinter and migrate in the spring. Nonetheless, the density-independent survival rate from emergence to age-1 migrant is 0.49 (0.7*0.7) which I think is far higher than is typical. Often the figure in the range of 10-20% is used for this period.

Smolt-adult survival rates have a large influence on salmon population dynamics and care and justification is needed for the values to be chosen for modelling exercises. Usually, natural and fishery mortality is partitioned so that the effects of alternative fishing strategies can be evaluated. In Table G-1 the values appear to be for the combined effects of fishing and natural mortality and are from data collected from hatcheries over 40 years ago. No justification is provided for using such data, but more recent values would be preferred as changes in survival in this stage (particularly climate-related declines) are very influential on the current status of salmon populations.

In an important review of chinook salmon population dynamics (Liermann et al. 2010), the rate of return (recruits/spawner not including fishing) at small population size is 6.8 for ocean-type stocks, and 4.3 for stream-type populations. The POP model generates R/S estimates (as *escape*/(*redds**2)) of 9.0, and if harvest were accounted for the value for an unfished stock would be even higher. McReynolds et al. (2006) indicate harvest rates are in the order of 50%, so that R/S (before fishing) for the North Yuba POP model is now closer to 18 ocean recruits/spawner. Given that many salmon stocks in the southern part of the range are not increasing rapidly, the R/S in the range of 1-3 seem more likely. Because the model produces much higher values it reinforces the view that one or more of the survival rates are too high.

The consequence of these high survival rates is the number of adult fish returning to the system is vastly higher than the number that actually spawn, and the model relies on the truncating functions in the adult phase to reduce the size of the spawning population. I doubt this ever occurs in nature, and under less restrictive (but more realistic) density dependent functions the spawning populations would be very large.

To reconcile these differences, I would suggest reviewing model parameters and model structure for the freshwater stages in an attempt to bring the freshwater production rates in line with observed values (and observed life history patterns). Then, the smolt-adult survival rates may need to be adjusted (tuned) to bring the R/S rates closer with those observed for chinook salmon stocks in the basin. If current estimates of exploitation rates are available, they should be employed in the model so that ocean and natural mortality can be separated, although the age-based vulnerability of these populations on the fishery does make the modelling more complicated than assuming a combined smolt-adult survival rate.

Section 7. Steelhead.

The assumption that age-1 summer habitat is limiting is reasonable, and it is also more likely that steelhead production follows a strongly density-dependent relation given their territorial behaviour. I am only aware of two datasets for smolts and spawners (Snow Ck. WA (unpublished), and Keogh R, BC, [Ward and Slaney 1993; Ward 2000]) and show a Beverton-Holt shape. Data from Yuen and Sharma (2005) could also be examined.

7.2.1.2

Similar to chinook salmon, the calculation of the amount of spawning habitat is a useful check to determine the amount of habitat available but I am less convinced that the this can be related to “capacity”. First, females are unlikely to not spawn if ideal habitat is not available: if pushed, they will spawn in pea gravel or on boulders. Second, spawning can occur over a protracted period and some spawning areas can be used multiple times. This will reduce egg survival but is unlikely to limit fry production (the hard cap). Data from Ward and Slaney (1993) do not support density-dependent fry production.

7.2.1.3

Empirical information on the densities of age-1 steelhead is used to estimate the potential production from thermally suitable waters. Recent studies on the detection probability of age-1 *O. mykiss* by both snorkel and electrofishing data indicate that some consideration of these probabilities is needed to scale observations to actual densities. It is unclear whether the averages in Table 7-1 have been corrected. See Bradford and Higgins (2001) and Korman et al. (2010) for reviews of the behaviour and detection probability of these fish. These studies were conducted in rivers that are someone cooler than those in California, but the potential for “single pass” or uncorrected observations to severely underestimate abundance needs to be considered. My experience has been that age-1 trout are quite secretive, and require nighttime sampling to reasonably estimate their abundance by visual means.

There are some density estimates from Southern Oregon in Satterthwaite (2002) that may be of interest.

The second concern I have about this approach is the lack of microhabitat information for the Yuba reaches and for comparisons to the empirical data. As noted in the report, Age-1 trout prefer areas of greater depth and higher velocity than age-0 fish, and based on the very low flows during summer in the Yuba there the potential for riffle and run habitats in the Yuba to be less productive than riffles and runs in the systems listed in Table 7-1. There is some comfort that some of the observations from the Yuba are in the Table. The addition of a column of discharge during the period of sampling in the table would be helpful to ensure the data are reasonably comparable. I infer that the full width of the Yuba channel is considered to be equally suitable in the analysis (this should be articulated)—for some systems fish are found (and sampling is conducted) solely along the margins of the stream making calculations based on wetted width quite erroneous.

7.2.1.4

The use of results from artificial substrates to estimate the stream-wide capacity for overwintering steelhead juveniles seems to hinge on the assumption that all of the Yuba habitat is equally suitable to the clean, coarse bed materials that are usually used in these trials. I doubt that is true, but don't think this stage will have much impact on stream capacity as the length of thermally suitable river expands significantly in the winter months making it unlikely that this season will be limiting.

7.2.2.2

I fail to see the evidence to support the statement that results show there "was ample spawning habitat to fully seed the thermally suitable age 1+ juvenile summer rearing habitat". This is more of an assumption, as far as I can tell as there are no survival rates used to convert eggs deposited to summer juveniles. The assumption is not unreasonable, given information that is available from other studies that indicate that relatively low spawner abundances can fully seed freshwater habitats.

P64. I am unclear how the summer thermal regime will restrict the distribution of steelhead spawning that presumably occurs long before the temperatures reach their mid-summer extremes. The optimal temperatures for steelhead spawning are much lower (<13 °C) as indicated in table 2.2, and I don't understand the relevance of 20 or 25°C temperatures to steelhead spawning and redd capacities. It would more important to model the appropriate threshold temperatures in the appropriate seasons to determine the spatial distribution of suitable spawning temperatures, and compare the overlap to the more restricted rearing temperatures that occur later in the year.

7.3.1

The 81% survival rate from summer to the following spring seems somewhat optimistic (I obtained estimates of about 40% for age-0 to age-1 survival in Bradford et al. 2011, and Ward and Slaney (1993) provide estimates in the 60% range) but apparently the empirical evidence provided seems to support these values. I converted a few of the values in Table 7-6 to smolts/km and obtained values of 500-1400 smolts/km based on the 20 degree threshold. These are only slightly less than those found for age-1 coho salmon smolt production but additional mortality from age-1 parr to age-2 smolting is expected. For the Keogh River smolt production is in the range of 300 smolts/km (Ward and Slaney 1993; Bradford et al. 1997), but the dominant smolt age is 3 in this system and somewhat lower values are expected with the additional year of freshwater rearing.

Finally, it possible to estimate the escapement required to produce these smolts by assuming a level of productivity for the system. The data from Yuen and Sharma for relatively pristine upper Columbia basin suggest production rates of 40-60 smolts/spawner, from which escapements in the order of 10-30 spawners/km can fully seed Yuba River habitats to produce the smolts predicted by the model. Productivity in the Yuba system may be lower, however, due to the anthopogenic changes that have occurred. See also Bley and Moring (1988) for a review of steelhead survival rates.

7.4

These points are well taken although I have a few comments on the bullets:

#1- The summer rearing values are based on empirical information and have a more substantive basis than other parts of the analysis.

#4 As noted, the interaction between the spawning distribution and summer rearing is not considered. It’s not clear to me how the summer densities are determined to be the key limiting factor based on the analysis presented.

#6 The presence of predators does not affect the carrying capacity but will alter vital rates of the population and thus productivity.

Tables:

Table F-6. It is unclear to me why the potential density of a pool in higher gradient areas is only 25% of a pool in a lower gradient reach. A pool is normally defined as a unit with near 0% water elevation change, and that characteristic wouldn’t change with the reach-scale gradient. If anything one might expect a high-gradient pool to support a higher density of juveniles if the higher velocities result in a greater delivery of drift to the pool.

This result conflicts with the steelhead data in Table H-3 where density increases in pools in higher gradient reaches.

Summary and recommendations

In summary, the results from the analysis provide a useful starting point for the analysis of the potential reintroduction of salmonids to the Yuba basin. Many of the comments and recommendations in this report are aimed at improving the predictions or clarifying analysis for the determination of potential capacity of the freshwater habitats. The following are the significant items that could be considered for the analysis.

1. Temperatures should be modelled for each month of the year along each reach and for each management scenario. Available habitat should be identified for each life stage based on seasonal habitat use.
2. Scenarios can be developed that compare the potential distribution of spawning (in thermally suitable water) and spring (for chinook) and summer (for both species) rearing habitat to evaluate what impacts mismatches in the availability of spawning and rearing habitat might have on production.
3. Chinook salmon incubation rates, emergence timing and juvenile growth rates should be modelled from the temperature data. These can then be compared to observations from other systems to help clarify the life history that might occur in the Yuba River (in particular, the relative proportions and significance of each smolt type).
4. Alternative simpler models should be used to make predictions of chinook and possibly steelhead trout populations to compare to the HAB/POP results. A greater effort to incorporate results from other systems and compare model results with data from elsewhere should be made.
5. The chinook salmon model should be modified and “tuned” to better emulate known properties of chinook salmon population dynamics.

While there are many ways the habitat analysis can be refined, the value in those revisions should be evaluated against the uncertainties and challenges with other part of the life cycle of these salmonids. One area of refinement that I believe warrants attention is to continue to develop the models to the point that rough estimates of expected adult escapements can be developed for both species. Some of the scenarios suggest that quite small fragments of habitat will be thermally suitable and the sustainability of very small salmonid populations needs to be considered, especially with respect to their resilience to prolonged periods of adverse environmental conditions, particularly in the downstream habitats and the ocean.

Finally I am compelled to comment on the uneasy mixture of Imperial and metric units within the report that required constant attention (and conversion). In the tables there are temperatures in °F and °C, measurements of stream and watershed characteristics in metres, kilometres^2, miles and cfs!. Consistent use of SI units is consistent with current practise and facilitates comparisons with the scientific literature.

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Appendix 1. Materials Reviewed.

Modeling Habitat Capacity and Population Productivity for Spring-run Chinook Salmon and Steelhead in the Upper Yuba River Watershed, Technical Report, Prepared for National Marine Fisheries Service, Santa Rosa, California 95404. Prepared by Stillwater Sciences, Berkeley, California 94705, February 2012.

RIPPLE: A Digital Terrain-Based Model for Linking Salmon Population Dynamics to Channel Networks, University of California, Berkeley Earth and Planetary Science, Berkeley, CA and Stillwater Sciences, Berkeley, CA.

Appendix 2: Statement of Work.

Attachment A: Statement of Work for Dr. Michael Bradford

External Independent Peer Review by the Center for Independent Experts

Review of Upper Yuba River Salmonid Habitat Assessment and Population Model

Scope of Work and CIE Process: The National Marine Fisheries Service's (NMFS) Office of Science and Technology coordinates and manages a contract providing external expertise through the Center for Independent Experts (CIE) to conduct independent peer reviews of NMFS scientific projects. The Statement of Work (SoW) described herein was established by the NMFS Project Contact and Contracting Officer's Technical Representative (COTR), and reviewed by CIE for compliance with their policy for providing independent expertise that can provide impartial and independent peer review without conflicts of interest. CIE reviewers are selected by the CIE Steering Committee and CIE Coordination Team to conduct the independent peer review of NMFS science in compliance the predetermined Terms of Reference (ToRs) of the peer review. Each CIE reviewer is contracted to deliver an independent peer review report to be approved by the CIE Steering Committee and the report is to be formatted with content requirements as specified in **Annex 1**. This SoW describes the work tasks and deliverables of the CIE reviewer for conducting an independent peer review of the following NMFS project. Further information on the CIE process can be obtained from www.ciereviews.org.

Project Description: NMFS is interested in assessing the potential for reintroducing anadromous fish upstream of the Narrows hydroelectric complex on the Yuba River as a recovery action for ESA-listed salmon species. In order to gain additional knowledge of the upper Yuba River habitats, NMFS contracted with Stillwater Sciences, Inc. to develop watershed-specific science products to help NMFS assess the potential for reintroduction of anadromous fish to particular areas of the upper Yuba watershed.

The Narrows Hydroelectric Development Complex was constructed approximately 50 years ago on the Yuba River at River Mile 23.4. The complex consists of Englebright Dam and two associated hydropower installations. The combined complex is a complete barrier to the upstream migration of anadromous fish into the South, Middle, and North Yuba Rivers.

The subject matter of this CIE review involves an environmental modeling application known as "RIPPLE," and a related technical report[1] produced for NMFS and other stakeholders by Stillwater Sciences, Inc. The model is built upon extensive research, field investigations, and a comprehensive synthesis of data relating many different physical and biological aspects of the upper Yuba River. The report adds to the base of existing knowledge about the upper Yuba watershed. NMFS is interested in using the information - combined with other relevant and available science-based information - to perform an assessment and relative comparison of potential anadromous fish habitats existing upstream of the Narrows Hydroelectric Development Complex.

The Terms of Reference (ToRs) of the peer review are attached in **Annex 2**.

Requirements for CIE Reviewers: Three CIE reviewers shall conduct an impartial and independent peer review in accordance with the SoW and ToRs herein. The CIE reviewers shall have the collective expertise, working knowledge and recent experience in the application of

- Knowledge of modeling of geomorphic processes of river systems, hydrology, and aquatic habitat.
- Theoretical mathematical ecology and conservation biology with knowledge in salmon population dynamics, salmonid community ecology, Pacific salmonid life cycle ecology, complex ecological interactions and population ecology of Pacific salmonids including life cycle ecology.

Each CIE reviewer's duties shall not exceed a maximum of 10 days to complete all work tasks of the peer review described herein.

Location of Peer Review: Each CIE reviewer shall conduct an independent peer review as a desk review, therefore no travel is required.

Statement of Tasks: Each CIE reviewers shall complete the following tasks in accordance with the SoW and Schedule of Milestones and Deliverables herein.

Prior to the Peer Review: Upon completion of the CIE reviewer selection by the CIE Steering Committee, the CIE shall provide the CIE reviewer information (full name, title, affiliation, country, address, email) to the COTR, who forwards this information to the NMFS Project Contact no later than the date specified in the Schedule of Milestones and Deliverables. The CIE is responsible for providing the SoW and ToRs to the CIE reviewers. The NMFS Project Contact is responsible for providing the CIE reviewers with the background documents, reports, and other pertinent information. Any changes to the SoW or ToRs must be made through the COTR prior to the commencement of the peer review.

[1] **Modeling Habitat Capacity and Population Productivity for Spring-run Chinook Salmon and Steelhead in the Upper Yuba River Watershed,** Technical Report, Prepared for National Marine Fisheries Service

Santa Rosa, California 95404. Prepared by Stillwater Sciences, Berkeley, California 94705, February 2012

Pre-review Background Documents: Two weeks before the peer review, the NMFS Project Contact will send (by electronic mail or make available at an FTP site) to the CIE reviewers the necessary background information and reports for the peer review. In the case where the documents need to be mailed, the NMFS Project Contact will consult with the CIE Lead Coordinator on where to send documents. CIE reviewers are responsible only for the pre-review documents that are delivered to the reviewer in accordance to the SoW scheduled deadlines specified herein. The CIE reviewers shall read all documents in preparation for the peer review.

Desk Review: Each CIE reviewer shall conduct the independent peer review in accordance with the SoW and ToRs, and shall not serve in any other role unless specified herein. **Modifications to the SoW and ToRs should not be made during the peer review, and any SoW or ToRs modifications prior to the peer review shall be approved by the COTR and CIE Lead Coordinator.** The CIE Lead Coordinator can contact the Project Contact to confirm any peer review arrangements.

Contract Deliverables - Independent CIE Peer Review Reports: Each CIE reviewer shall complete an independent peer review report in accordance with the SoW. Each CIE reviewer shall complete the independent peer review according to required format and content as described in Annex 1. Each CIE reviewer shall complete the independent peer review addressing each ToR as described in Annex 2.

Specific Tasks for CIE Reviewers: The following chronological list of tasks shall be completed by each CIE reviewer in a timely manner as specified in the **Schedule of Milestones and Deliverables**.

1) Conduct necessary pre-review preparations, including the review of background material and reports provided by the NMFS Project Contact in advance of the peer review.
2) Conduct an independent peer review in accordance with the ToRs (**Annex 2**).
3) No later than September 8, 2012, each CIE reviewer shall submit an independent peer review report addressed to the "Center for Independent Experts," and sent to Manoj Shivlani, CIE Lead Coordinator, via email to shivlanim@bellsouth.net, and CIE Regional Coordinator David Die via email to ddie@rsmas.miami.edu. Each CIE report shall be written using the format and content requirements specified in Annex 1, and address each ToR in **Annex 2**.

Schedule of Milestones and Deliverables: CIE shall complete the tasks and deliverables described in this SoW in accordance with the following schedule.

August 6, 2012	CIE sends reviewer contact information to the COTR, who then sends this to the NMFS Project Contact
August 23, 2012	NMFS Project Contact sends the CIE Reviewers the report and background documents
August 24 – Sept. 6, 2012	Each reviewer conducts an independent peer review as a desk review.
September 8, 2012	CIE reviewers submit draft CIE independent peer review reports to the CIE Lead Coordinator and CIE Regional Coordinator
September 22, 2012	CIE submits the CIE independent peer review reports to the COTR
September 29, 2012	The COTR distributes the final CIE reports to the NMFS Project Contact and regional Center Director

Modifications to the Statement of Work: Requests to modify this SoW must be approved by the Contracting Officer at least 15 working days prior to making any permanent substitutions. The Contracting Officer will notify the COTR within 10 working days after receipt of all required information of the decision on substitutions. The COTR can approve changes to the milestone dates, list of pre-review documents, and ToRs within the SoW as long as the role and ability of the CIE reviewers to complete the deliverable in accordance with the SoW is not adversely impacted. The SoW and ToRs shall not be changed once the peer review has begun.

Acceptance of Deliverables: Upon review and acceptance of the CIE independent peer review reports by the CIE Lead Coordinator, Regional Coordinator, and Steering Committee, these reports shall be sent to the COTR for final approval as contract deliverables based on compliance with the SoW and ToRs. As specified in the Schedule of Milestones and Deliverables, the CIE shall send via e-mail the contract deliverables (CIE independent peer review reports) to the COTR (William Michaels, via William.Michaels@noaa.gov).

Applicable Performance Standards: The contract is successfully completed when the COTR provides final approval of the contract deliverables. The acceptance of the contract deliverables shall be based on three performance standards:

(1) each CIE report shall completed with the format and content in accordance with **Annex 1**,

(2) each CIE report shall address each ToR as specified in **Annex 2**,

(3) the CIE reports shall be delivered in a timely manner as specified in the schedule of milestones and deliverables.

Distribution of Approved Deliverables: Upon acceptance by the COTR, the CIE Lead Coordinator shall send via e-mail the final CIE reports in *.PDF format to the COTR. The COTR will distribute the CIE reports to the NMFS Project Contact and Center Director.

Support Personnel:

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Annex 1: Format and Contents of CIE Independent Peer Review Report

1. The CIE independent report shall be prefaced with an Executive Summary providing a concise summary of the findings and recommendations, and specify whether the science reviewed is the best scientific information available.

2. The main body of the reviewer report shall consist of a Background, Description of the Individual Reviewer's Role in the Review Activities, Summary of Findings for each ToR in which the weaknesses and strengths are described, and Conclusions and Recommendations in accordance with the ToRs.

3. The reviewer report shall include the following appendices:

   Appendix 1: Bibliography of materials provided for review

   Appendix 2: A copy of the CIE Statement of Work

Annex 2: Terms of Reference for the Peer Review

Review of Upper Yuba River Salmonid Habitat Assessment and Population Models[2]

1. Review the RIPPLE model application for the upper Yuba River (Stillwater Sciences 2012) to determine whether the data sets, assumptions, and model parameters represent a reasonable modeling approach to assess the relative potential of upper Yuba River habitats under the three different modeled scenarios.

2. Does the RIPPLE model application for the upper Yuba River produce results that are relevant and appropriate to support the evaluation of anadromous fish reintroduction potential in the upper Yuba River watershed?

Materials provided for review:

1. Primary report:

- *Modeling Habitat Capacity and Population Productivity for Spring-run Chinook Salmon and Steelhead in the Upper Yuba River Watershed*, Technical Report, Prepared for National Marine Fisheries Service, Santa Rosa, California 95404. Prepared by Stillwater Sciences, Berkeley, California 94705, February 2012

2. Background and broad overview of the RIPPLE model structure and rationale:

- *RIPPLE: A Digital Terrain-Based Model for Linking Salmon Population Dynamics to Channel Networks*, University of California, Berkeley Earth and Planetary Science, Berkeley, CA and Stillwater Sciences, Berkeley, CA

[2] The reviewers should be aware that the original project budget was limited to the output and information produced in the report, as a Phase 1 investigation. Following the delivery of this report to NMFS in February 2012, Stillwater Sciences conducted an additional sensitivity analysis of model parameters, but that information is not yet completed and available for this review - which must go forward in order to meet deadlines. Furthermore, NMFS has secured additional funding to enable Stillwater to re-run the models (Phase 2) using updated field information that was gathered by NMFS and other Yuba River stakeholder groups after the model runs for this report were conducted.