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Draft Chapter 5 review issues statements

Steven Culberson <steven_culberson@fws.gov> Thu, Mar 7, 2013 at 4:31 PM
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Folks –

I've run out of time before our self-imposed deadline. Thank you for all your important input. I hope this document includes the "must haves" for our discussion and finalization. It is rough, but it should be useful in showing where I was able to get using what was given me to date.

I clearly still need to move items into the appropriate sections – something I started but did not complete. I did not presume to be able to write the introduction, nor did I think there necessarily needs to be one if we have something that introduces all of our review materials as one package.

I will not be available this weekend except by email. I will have time to work on this intermittently until Monday. I guess next week we'll be focusing on revisions. Please help where you can. Finalization is still expected by Wednesday, March 13 as far as I am aware.

Steve

P.S. I also attach Chapter 10 comments that I provided some time ago to comprise a complete set of what I believe is due from me in this regard.

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2 attachments

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Culberson BDCP Review Comments for Chapter 10.docx
13K

BDCP Chapter 5 (Effects Analysis) Draft Review

March 2013

Issue Statements regarding Perceived Deficiencies

[preamble]

Issue 1: Incomplete or inappropriate conceptual foundation for the Effects Analysis

The usefulness of the Effects Analysis is hindered by the lack of uncertainty estimates throughout. Although specific life-stage or parameter values are incorporated into the HSI model (for example) when evaluating project effects there has been no attempt to include a range of values for the HSI inputs when calculating model output. To be sure, we are satisfied with the structure and general approach of the HSI-related analysis, but deployment of the model is restricted in scope and represents only a proponent-based selection of input values. This amounts to an incomplete sensitivity analysis using the chosen HSI method. In order to more fully explore the possible range of outcomes using this approach, additional input value choices should be used when executing the HSI approach (including minimum, median, and maximum values – in addition to those values included until now). In other words, the HSI procedure should be repeated with less (and more) optimistic estimates of project performance.
Comment: Previously supplied comments in this regard included (March 2012): "The effects analysis deals with the critical concept of uncertainty inconsistently and does not effectively integrate, use, and report uncertainty in the Net Effects." The lack of sensitivity analysis (or similar measure of uncertainty) remains a critical missing element.

The rationale for choosing individual HSI input parameter values is not sufficiently documented precluding independent verification of modeled outcomes or evaluation of parameter estimation legitimacy. The Effects Analysis goes to some length to detail the relationship between sample-related GAM parameter estimates and an "HSI parameter estimate curve," but we cannot verify the actual parameter values used in calculating the HSI and Habitat Unit outputs. It is necessary to understand the relationship between the project description narrative and the resulting numerical representation of the project used in the calculations in order to independently evaluate the rationale for particular parameter input values. This evaluation is not possible in the current version of the BDCP.
Comment: This comment has previously been referred to as the "show your work" requirement, whereby an independent reviewer might plausibly recreate the analysis presented in the documents under review – an indispensable element of routine scientific review. This is at least an issue of incomplete documentation, and will likely lead to a discussion about why alternate values were not used to drive the various HSI and HU evaluations of future project effects (see comments regarding uncertainty, above).

Estimates of tidal marsh restoration acreages may be overstated simply because the tidal physical characteristics of the Estuary cannot support such a restoration objective. Upstream areas in the Estuary (east of major constrictions in tidal geometry such as Carquinez Strait, for example) may not receive sufficient tidal energy to provide tidal variation in stage or flow important to tidal restoration efforts critical to the success of the proposed project. Furthermore, it is unclear to the reviewers whether the existing project effects analysis incorporates information generated using multi-dimensional models (RMA11, in particular) when evaluating the likelihood of proposing the restoration acreages outlined in the proposal. We are concerned that actual acreages restored will be significantly less than proposed in the current document.
Comment: There has been a general request since late 2011 to clarify and/or justify the quantity of acres included in the proposed plan. A more recent discussion with the Delta Science Program's Independent Science Board in January 2013 has underscored the need to carefully explore the hydrodynamics and land use aspects of the proposed restoration program in light of its importance to the Habitat Conservation Plan. We remain unconvinced at the realism of estimates for proposed restoration acreages notwithstanding ongoing discussions regarding Goals and Objectives for various tidal and near-tidal habitats and their definitions. It is not clear how ownership, hydrodynamic, and habitat quality issues may impact the efficacy of the proposed tidal restoration, nor how these issues were explicitly incorporated into the latest Effects Analysis.

Issue Area 2: Inadequate conceptual models and analysis of estuarine fish habitat, and project issues resulting from same

The objectives for restoring habitat addressed in the Chapter 5's Restoration Appendix are simply described, but it is not clear whether the plan will or can achieve them. The draft Appendix E states that BDCP's habitat restoration has two objectives1[1]. The first is to "increase the amount of available habitat for covered fish species." This first objective is reasonable, but does not clearly articulate that new habitat needs to be good habitat. We know quite a bit about what determines habitat value to covered fish species. This knowledge is partly reflected in the habitat suitability indices that are currently under development, but is often discounted elsewhere in the Chapter 5 documents. The habitat for BDCP target fishes, and all estuarine fishes for that matter, is fundamentally created by the interaction of tidal and river channel flows with the broader estuary landscape. The Preliminary Project proposes to extract larger volumes of fresh water from the Delta than are currently exported against a backdrop of rising sea level and a re-design of the estuary landscape that will change tidal flows. Whether this can be accomplished while other parts of the plan simultaneously contribute to recovery of covered species is an unanswered question of central importance. Fully incorporating existing science on the interplay of freshwater flow and the Plan Area landscape and its constituent species would provide more accurate and defensible conceptual models for

[1] We note that these objectives are more akin to goals. They are not at present specific enough to function as objectives in the context of performance evaluation or adaptive management.

the Effects Analysis. We also suggest consulting the Department of Interior Adaptive Management Technical Guide and other adaptive management resources on the role of (potentially conflicting or alternative) conceptual models in the adaptive management process. The second objective is "to enhance the ecological function of the Delta." This formulation is not clear. The Delta provides multiple ecological services, and alterations to different parts of the Delta may potentially contribute to them in different ways. There have been several large-scale, unintentional or quasi- intentional "wetland restoration projects" in the Bay-Delta since 1920. These include Franks Tract in the 1930s, Mildred Island in the early 1980s, Liberty Island in the latter 1990s, and Napa River marsh in the past decade to name a few. There is also the seasonal fish habitat generated by large-scale floodplain restoration along the lower Cosumnes River that started in the mid-1990s. We believe these "unintended experiments" provide useful lessons in what we may expect from actions on similar spatial scales in similar circumstances in various restoration scenarios.
Comment: This issue remains as is from the March 2012 document.

The HSI-based analysis does not appropriately allow for lessor quality habitat restoration as part of the overall restoration effort – all habitats provided will be beneficial. The HSI-based approach rates outcomes on a scale from 0 to 1, although it never rates habitat as zero or allows it to be detrimental within the analysis by letting ratings values be "negative." This effectively means that no project or CM will ever be less than beneficial upon implementation. Is this realistic? Would 7-10 new Frank's Tracts or Mildred Islands be beneficial to native fish species like delta smelt? Given the likelihood of failed islands to contribute to the CM implementation, wouldn't the evaluation rubric be credible only if it includes the notion that some projects will fail to meet habitat and species preference objectives? We find the inability of the evaluation method to include this outcome to be inappropriate for what is being asked of the evaluation tool.
Comment: This is a critical piece of the analysis to get correct or estimate properly since it feeds the evaluation underlying a great deal of the Effects Analysis and is the mechanism of action for many of the Conservation Measures.

There is no reason to expect that invasive vegetation will not proliferate in the East and South Delta ROAs, and no reason to expect a meaningful increase in south Delta turbidity if vegetation could be successfully controlled. There should not be an a priori assumption that SAV can be controlled via ecologically sound methods in the east, central and south Delta. These are comparatively low turbidity, high vegetation areas already, under the existing hydrodynamic regime. There is nothing in the Preliminary Proposal that would dramatically change channel geometry, increase SJR flows, or increase sediment inputs that could be expected to change the turbidity of the entire southern half of the Delta.
Comment: This issue remains as is from the March 2012 document.

Increased residence times and reduced flushing of the Delta by Sacramento River water appear likely to result in interior-Delta channels that are further dominated by agricultural runoff, invasive aquatic vegetation, warmer

temperatures, and increased algal productivity with its associated dissolved oxygen swings. These environmental conditions favor non-native/invasive species (e.g. *Egeria densa*, largemouth bass, water hyacinth, *Microcystis*) and disfavor native fishes. The Delta is already more biologically similar to a lake than it once was, due to the historical accumulation of human modifications. We expect that by reducing Delta flows, the Preliminary Project would likely facilitate the spread of habitat conditions that are unfavorable to delta smelt, and less favorable to other target fish species survival and recovery.
Comment: This issue remains as is from the March 2012 document.

Issue Area 3: The Effects Analysis relies on selective use and interpretation of statistical and mathematical models

The effects analysis continues to insist on an analytical approach to entrainment that does not reflect the best available science. The delta smelt population is at historically low abundance, and population losses due to entrainment may have significant population effects depending on their magnitude and frequency. While it is true that some regression-based analyses have failed to reveal an export affect to the delta smelt population, other approaches that more effectively investigate the role of fish distribution to entrainment have revealed an important relationship between water operations and the risk of population-level entrainment effects to delta smelt. Kimmerer (2011) demonstrated that entrainment losses averaging 10% per year can be "…simultaneously nearly undetectable in regression analysis, and devastating to the population." We look forward to working with our partners to ensure that the best model-based analyses of proportional entrainment for both South- and North-Delta diversion facilities are brought to bear to resolve this issue.
Comment: This issue remains as is from the March 2012 document.

The effects analysis did not use the best available longfin smelt statistical models to support its net effects conclusion. The newest published statistical analyses of longfin smelt are quasi-life cycle models that account for prior abundance and spring flow influences (among other factors) on this species. These models were discussed and discounted as not being 'life cycle models'. Dismissing them because they are not 'life cycle models' is unhelpful: they are the best available scientific tools to evaluate project effects on longfin smelt. The older regression models that were used in the effects analysis are published, but can easily be shown not to perform as well as the newer models. The older models also average the flow influence on longfin smelt across half a calendar year, which likely affects conclusions about the reduction in springtime outflow seen in modeling outputs for the Preliminary Proposal.
Comment: This issue remains as is from the March 2012 document.

The effects analysis did not use the available splittail life cycle model at all to support its Net Effects conclusion. There is a published stage-based life cycle model for splittail where the effects of various environmental variables were examined for their effects on long-term trajectory of population abundance. This model helped frame the

preferred time-interval for floodplain activation necessary to ensure splittail persistence in the Central Valley. This available approach to an Effects Analysis for a listed species of native fish was not discussed in the present Effects Analysis. Additionally, food web related benefits were ascribed to the project with respect to splittail abundance without establishing that food limitation is more critical to the species than is adequate flood plain habitats for spawning and early rearing (Moyle et al., 2004).
Comment: This issue remains as is from the March 2012 document.

The effects analysis appears to rely on a curious set of assumptions that remain unjustified or at least equivocal. Among these (as examples) are that depth plays a key role in habitat suitability for the smelts (5E.0.2.1.1), and that most all habitat types are preferred by smelts at all times and by all life stages except eggs and larvae (Table E.4-2, Appendix 5.E). Additionally, information in Conservation Measure (CM) 4 does not match that in the Effects Analysis regarding habitat preference and habitat suitability index. Perhaps this is a "coordination issue" unavoidable with a document of this size and complexity, but it undermines the ability to replicate the analysis via independent review and erodes confidence in the credibility of the discussed evaluation of effects. It is difficult to achieve strong conceptual and technical foundation for the eventual conclusions given these inconsistencies and inaccuracies throughout the draft document.
Comment: This issue was identified for previous versions of the document and is re-identified as a remaining issue for the December 2012/March 2013 versions.

General and/or detailed analysis of restoration effects upon tidal geometries and alteration of the tidal mixing field are missing from the analysis. Conservation Measure 4 will be achieved largely via breaching levees and flooding subsided areas. What might this do to tidal energies within the system, and the geomorphologic processes associated with changing tidal velocities and asymmetries within the Delta? If velocities in general decline and tidal amplitudes diminish what are the general water quality consequences for the Delta writ large? The likelihood of warming and stagnating the central Delta by making withdrawals upstream of the Delta proper has not received adequate attention within the analysis of effects.
Comment: This is an issue that arose pending hydrodynamics analysis associated with other portions of the proposal since at least January 2010 and is identified as a major issue when considering the project as a whole under the "Effects Analysis summary" document.

Issue Area 4: The BDCP's net (summary) effects rest on an equivocal food web conceptual model

The Habitat Suitability Analysis does not include an accurate estimate of food/prey availability and fate for either of the smelts for existing or proposed future conditions. The phytoplankton productivity estimate used from Lopez et al. (2006) is an instantaneous productivity rate stated by the authors as not reflective of habitat and hydrodynamic conditions writ large in the Delta. A more accurate analysis of the productivity of Delta tidal environments would include analysis of the transport and fate

of such productivity as is likely to be encountered in the Delta environment. Specifically, Lopez et al. (2006) underscore the evidence that much of the productivity in the Delta is being shifted to exotic bivalves away from the pelagic environment, or advected away from the areas where the phytoplankton are originally found. The authors emphasize that analysis of instantaneous productivity estimates undervalues the environmental context otherwise at play in the Delta that will diminish the volume of primary productivity that actually finds its way to pelagic fishes of interest. This emphasis notwithstanding the Effects Analysis assumes an increase of 70% in productivity available to the pelagic food web as the result of the proposed restoration program – an estimate without justification insofar as our review was able to identify given the available documents.
Comment: Apparently the Lopez et al. (2006) article serves as the linchpin of how tidal aquatic habitat restoration will lead to food web improvements and increases in smelt abundance. Service biologist reviews of the Lopez et al. (2006) article reveals a rather more cautionary approach to extrapolating the reported instantaneous productivity rates to the larger Estuary. Indeed, the article itself includes the passage: "The unexplained patchy distribution of *Corbicula fluminea* in the Sacramento-San Joaquin Delta implies high uncertainty in the outcomes of creating new aquatic habitats (Lucas and others 2002)." We are not of the opinion that an overly optimistic estimate of increased production Delta- or Estuary-wide as the result of habitat restoration is warranted (nor are the authors of the Lopez et al. paper). We cite as additional information in this regard Nixon (1988); Cloern (2007); Lucas and Thompson (2012). In sum, these references would suggest a 70% increase in system-wide productivity is unlikely (high), and given the low productivity in the San Francisco Estuary, even an optimistically-high estimate of productivity boost owing to restoration is likely to have only marginal effects in any event.

The Conservation Measure 4-based approach to solving food web issues for smelts is internally circumspect – which is at odds with the determination in the Effects summary that smelt productivity will improve into the future. The document itself states (Appendix 5E):

> *Biological relevance relates to the dimensions of habitat discussed above regarding feeding, physiology, and habitat availability. The scientific literature discusses numerous factors that potentially define habitat for the covered fish species in the Delta. However, the list of modeled habitat factors is reduced by the other two criteria. To be used in the analysis, sufficient data had to be available to describe the condition at the scale of the geographic subregion, and it was necessary to be able to forecast conditions in the future with and without the BDCP either through modeling or conclusions. For example, planktonic food is an important factor in defining habitat for delta smelt (Bennett 2005) that likely relates to the presence of certain species of zooplankton (Criterion 1). However, there is not sufficient data to characterize zooplankton abundance or community structure at the scale of the subregion (Criterion 2), nor is there an ability to project zooplankton response to future conditions.*

Furthermore we a puzzled by the proposed “work around” to the foregoing problem:

> *To incorporate a measure relating to feeding, turbidity was used as an indicator of feeding potential in the subregions and in restored habitat (the potential of the restoration to add to the food supply in the Delta was treated as a separate analysis in Section 5E.4.3). Delta smelt abundance is strongly correlated with high turbidity, and it is believed to relate to the ability of fish to find and capture prey (Bennett 2005) (Criterion 1). There is sufficient information collected as part of the regional fish monitoring programs to characterize turbidity in the subregions (Criterion 2). At the present time there is no model available to project turbidity in the future, although there is reason to expect that turbidity in the Delta may decline in the future (Ruhl and Schoellhamer 2004). Recognizing the strong association with delta smelt presence, turbidity was used as a factor in the delta smelt model, but turbidity was assumed not to change over the BDCP period (Criterion 3).*

Comment: We remain curious over how turbidity is determined to be a surrogate for smelt food supply, and more so about how it is concluded that this supply will remain unchanging into the future given that turbidity values in the Estuary are expected to decline through time as has been the case over the last 40 years.

Issue Area 5: The analysis and interpretation of BDCP are hindered by incomplete analysis, indeterminate model baselines, and related issues

Decision tree-related alternate analyses reflecting project implementation and operations choices are not included as separate outputs for evaluation. Slightly-to-moderately different HSI model versions representing structural differences between decision tree choices will require separate model analyses and inter-comparisons to fully appreciate project outcomes under the different habitat and operational assumptions. This follows from the notion that each decision tree choice will require slightly modified conceptual models for relating project components under the different operational assumptions. The only way an identical model can be used for each decision tree option/choice is if there is no conceptual or real difference between the choices/options. In the absence of identical conceptual models and identical modes of effect across the management/operational differences between decision tree alternatives there is no utility in using the same model to evaluate each choice. The only way a single model will reflect identical outcomes for all four decision tree outcomes is if the choices are (conceptually, at least) identical. We understand this case (identity between ecological outcomes) to be highly unlikely if not impossible. At the very least we should see in the Effects Analysis a formal structural analysis detailing differences between each decision tree pathway to reflect our understanding of what differs between the paths.
Comment: This should not be seen as a request for a formal “alternatives analysis.” This is designed to point out that it will be necessary to construct a suite of variations in the existing HSI/HU framework suitable to detect and evaluate differences between the “branches of the decision tree.” A defensible examination of the alternative

configurations using this approach will go a long way toward building credibility or confidence in the evaluation tools used and in the underlying methods generally.

More than half of the proposed tidal aquatic habitat restoration acreage is not planned for implementation for at least 25 years (2040?). What are the implications of this schedule for listed species now and over the next 25 years?
Comment: We are not clear how deferred implementation will be permitted under the HCP rubric. How is this reflected in the administrative sections of the document?

Too often within the Effects Analysis summary a reasonable paragraph or two of a description of why a restoration action is hard to adequately assess results in a determination of positive outcome (effect) for a listed species or natural community. These undocumented and unjustified conclusions of beneficiary or remedial effect, made with little reference as to their surety, ultimately undermine confidence in the document and its technical underpinnings. A brief example is presented here:

> *While there is general support from the scientific literature for the value of shallow-water habitats to support phytoplankton production in the Delta, the effectiveness of conversion of that production to zooplankton food for pelagic fish can be reduced by the presence of introduced clam species. In some cases, these introduced clams consume much of the phytoplankton produced in an area. Lucas and Thompson (2012) and Lopez et al. (2006) as well as other studies point out that invasive bivalves such as Corbicula can consume large amounts of phytoplankton in freshwater and in some cases can keep up with production levels resulting in little or no net production leaving shallow areas (Lucas and Thompson 2012). In areas with higher salinity such as Suisun Bay, the bivalve Portamocorbula amurensis has a similar impact on phytoplankton (Cloern and Jassby 2012). (See Appendix 5.F, Biological Stressors on Covered Fish, for more detail regarding the potential for further bivalve invasion in the Delta, including in restored areas.) Consumption by clams and the effect of nutrients and hydrodynamics on phytoplankton transport result in a complicated relationship between habitat restoration, phytoplankton production and food for pelagic fish species (Lucas et al. 2002). The conclusion is that while the scientific rationale for restoration of normative tidal habitats in the Delta is sound, much needs to be learned regarding how that restoration is optimized to benefit covered fish species. For example, Lucas et al. (2002) found that the ability of clams to reduce phytoplankton is dependent on site-specific features. These features could be incorporated into the design of restoration to minimize the effect of clams and to maximize production of planktonic food in the Delta.*
>
> *Restoration of shallow tidal habitat called for in CM4 is the most ambitious action available at this time to enhance food production in the Delta while enhancing other ecological functions provided by normative tidal habitat in the Delta. In this appendix we evaluate the potential of restored habitat to enhance productivity of the Delta based on a simple depth relationship (Lopez et al. 2006) while cautioning that the*

realities highlighted by Lucas and Thompson (2012) may limit the value of restoration in regard to phytoplankton production. Due to the scale of restoration and the complexities of the Delta foodweb, this restoration should be approached in an experimental (i.e., adaptive) manner to ensure that lessons learned on early restoration projects are incorporated into subsequent projects. ***Using this approach, the effectiveness of restoration actions is expected to increase over time [emphasis added].***

Comment: We see no justifiable rationale for why the uncertainties outlined in the preceding paragraphs results in an optimistic assessment for project effects on delta smelt. Such instances are common throughout the summary and supporting appendices. At the very least (and as recommended elsewhere herein) sensitivity analyses of the related parameter estimates in the HSI and HU frameworks are required for proper understanding and contextualization of model outputs.

There are conflicting understandings regarding proposed acreages comprising proposed restorations. It's the understanding of the FWS that RMA's original hydrodynamic analysis, approximately 3 years ago, provided the estimates of potential tidal habitat for the BDCP's ROAs. Because of this, it's now troubling that RMA's current estimates of available tidal habitat are so different from those in the BDCP HCP (Chapter 5). Table 1 below compares estimates of tidal habitat from Table E.4-9 in the HCP document (numerically comparing aquatic habitat totals with and without BDCP in given time periods) with acres of modeled restored habitat provided by RMA on 3-5-2013. Uncorrected, all of RMA's latest habitat estimates are higher than those in the BDCP Chapter 5 analyses. However, applying RMA's correction percentages considering limitation of tidal energy through the Carquinez Straight changes the differences significantly. In all cases the actual "available" tidal habitat identified by RMA is lower than that identified by the BDCP--in many cases like the south Delta, significantly so (Table 2). There needs to a discussion and sensitivity analysis provided in the document to clearly identify why these differences exist and to update the tidal habitat expectations in the HCP (see Table 1 and Table 2, below).

TABLE 1 – BDCP/RMA Restored Habitat Estimates (acres)			
Cache Slough	BDCP	RMA (3-5-13)	Difference
Near-term	3,800	6,749	2,949
Early Long Term	8,620	12,897	4,277
Late Long Term	16,590	20,334	3,744
Suisun Marsh	BDCP	RMA (3-5-13)	Difference
Near-term	4,200	6,454	2,254
Early Long Term	5,900	8,133	2,233
Late Long Term	11,920	14,389	2,469
Western Delta	BDCP	RMA (3-5-13)	Difference
Near-term	1,600	2,300	700
Early Long Term	2,580	3,992	1,412
Late Long Term	2,790	4,236	1,446
South Delta	BDCP	RMA (3-5-13)	Difference
Late Long Term	20,950	22,483	1,533

TABLE 2 -- Restored Habitat Estimates Using RMA's Tidal Energetic Analysis (acres)					
Cache Slough	RMA (3-5-13) Habitat Estimates	Percent Actual Tidal Marsh	"Available" Tidal Marsh (corrected)	BDCP	Difference
Near-term	6,749	45%	3,037	3,800	763
Early Long Term	12,897	46%	5,933	8,620	2,687
Late Long Term	20,334	34%	6,914	16,590	9,676
Suisun Marsh	RMA (3-5-13) Habitat Estimates	% Actual Tidal Marsh	"Available" Tidal Marsh (Ac)	BDCP	Difference
Near-term	6,454	43%	2,775	4,200	1,425
Early Long Term	8,133	43%	3,497	5,900	2,403
Late Long Term	14,389	26%	3,741	11,920	8,179
Western Delta	RMA (3-5-13) Habitat Estimates	% Actual Tidal Marsh	"Available" Tidal Marsh (Ac)	BDCP	Difference
Near-term	2,300	51%	1,173	1,600	427
Early Long Term	3,992	69%	2,754	2,580	-174
Late Long Term	4,236	70%	2,965	2,790	175
South Delta	RMA (3-5-13) Habitat Estimates	% Actual Tidal Marsh	"Available" Tidal Marsh (Ac)	BDCP	Difference
Late Long Term	22,483	8%	1,798	20,950	19,152

Temperature models used for the analysis continue to be inadequate for the analysis to which they are applied. Has there been a sensitivity analysis performed to support the use of monthly average water temperatures, given the daily variability in meteorology and lethal thresholds for salmonids? Seems like comparing results of an investigation of Sacramento River conditions using daily results vs. monthly results would serve this purpose. This comment applies everywhere the USBR Temperature Model is used.
Comment: This issue remains as is from the March 2012 document.

We continue to be confused over what's in each model and scenario and what is not, and what comparisons are actually being made. Didn't BDCP Lead Agency discussions establish that the NEPA baseline shall include effects of climate change and sea level rise?
Comment: This issue remains as is from the March 2012 document.

The State Water Resources Control Board established water-year type INDICES for classification purposes, yet somehow this becomes biologically useful when discussing biological objectives. The purpose was to match requirements to Water Project ability to meet those requirements, i.e., water supply-based, not biologically-based management. Are we suggesting that the species of concern know or care what the year type is? It might be more appropriate that species' welfare be more tied to the temporal distribution of runoff within any given year. Hence, maybe "wet winter years," "dry winter years," "wet spring years," etc., would be a more meaningful way to aggregate the results/do the analyses. Section 5C.4.4.1 seems to suggest analyses outside the year type box were indeed done. Some mention of this here would allay fears the reader may have that water-year type was the primary basis/construct for the analyses. Similarly, biological processes often don't operate on an annual level, so project effects in one Below Normal (BN) year may have a much different effect than another BN year. We recommend classifying sub-periods of the 82 year simulation in a way that is meaningful for the particular species for which effects are being analyzed. In this way, if an average value is presented or used for November of Above Normal years, we could be sure if the Novembers being averaged are Novembers following Above Normal years or if the Novembers being averaged are from Above Normal years? The bottom line here is, given the highly regulated nature of the system, Novembers following Above Normal water years are likely more similar than Novembers actually in Above Normal years.

Certain methods included in the effects analysis are incomplete, thereby leaving confusion as to what was performed in the effects analysis. It is unclear which transmission alignment was used for evaluating the quantitative impacts (that were subsumed in the conveyance footprint). It is also unclear which transmission alignment is being proposed as part of the project since the effects analysis provides quantitative impacts for one transmission alignment (that was included in the conveyance footprint), yet reports qualitative impacts for both the N-S and E-W alignment.

The document lacks a discussion that details connections between the conservation strategy, conservation measures, avoidance and minimization measures, and adaptive management. Further, the avoidance and minimization measures lack species-level detail related to specific restoration programs. For example, it would be useful to integrate the Suisun Marsh Programmatic BiOp conservation measures into the Plan for the salt marsh harvest mouse, California clapper rail, California least tern, and delta smelt.

There are outstanding comments from past reviews and TTT meetings that still need to be addressed in the Plan, in addition to comments now being provided by the federal agencies (DFWs review in Nov. 2012 and DWRs review on the Jan 21 draft). For example, the Service has had a long-standing policy to not cover pesticide use as a covered activity. Language within the draft Plan is inconsistent with FWS guidance.

The analysis does not account for the connection of life stages across areas and time. To be successful, fish need habitat of suitable quantity and value for each life stage at appropriate transition periods. Life history trajectories plot the habitat pathways that determine species performance. However, an HSI considers each life stage and associated habitat in isolation.
Comment: This comment is from a list of caveats provided by the authors in the Appendix 5E. This is a critical shortcoming in the analysis and leads to the possibility (eventuality) that habitat restoration could benefit one life-stage and be detrimental to the other. It is not stated how this is resolved using the HSI approach.

The HSI-estimated egg-larvae life stage suitability curve using the GAM-based method is up to 50% more optimistic than the sample-based data would lead us to believe (see Figure E.4-4).
Comment: This is another example of where the bias in the estimation procedure is clear and yet stands as such for the entirety of the analysis. We recommend using a sensitivity procedure for identifying the impact of such bias in all cases.

The analytical design incorporates inappropriate or inconsistent characterizations of alternatives and baseline conditions. See section 5E.4.2.3.3 as an example.
Comment: This issue remains as is from the March 2012 document.

Analysis of contaminants in general and methylmercury in particular is deficient and is not useful for resolving contaminant-related issues important to the BDCP proposal. Uncertainty in outcome is not adequately addressed in this section. Conclusions provided in the summary are not supported by basic conceptual models for mercury methylation. The summary of the mercury analysis (5D.4.1.2.3, 5D.5.3) reasonably states that "methylmercury likely would be generated by inundation of restoration areas, with the highest concentrations in the Yolo Bypass, Consumnes and Mokelumne Rivers..." The summary also identifies that Conservation Measure 12 will "help to minimize increased mobilization of methylmercury at restoration areas." The current body of science can provide very few tools to facilitate the restoration of wetland habitat while at the same time emulating the historic habitat which provided the needed range of services for native species without also creating conditions which will methylate mercury. For example, turbidity needed for delta smelt will reduce photodegradation. Increased dissolved organic carbon, the direct result of increased primary productivity and the principle benefit of CM 4, is correlated with MeHg production. Risks to human health from drinking water and fish consumption will supersede covered species concerns. These issues, in combination with the very large acreage of proposed restoration, and the importance of restoration in BDCP will result in both a high risk for covered species as well as a high degree of uncertainty. The resulting uncertainty and risk are not reflected accurately in the analysis or the summary.

The Effects Analysis is inadequate because it is limited to only the beneficial effects of BDCP activities. It does not include negative effects of construction and other activities, almost all of which occur within delta smelt critical habitat. There is no discussion of how restoration activities in the Cache Slough complex may affect delta smelt spawning, which is a take avoidance issue. The effects, both positive and negative, of land conversion (e.g., of managed wetland into tidal perennial aquatic) are ignored.

The Effects Analysis does not say how benefits to one species were weighed against potential detriments to others. It does not discuss the uncertainties inherent in restoration outcomes. And most importantly, the Effects Analysis does not describe the effects of the changed hydrology that will result from the North Delta diversions.

The benefits of restoration in the South Delta are based upon optimizations, and are therefore likely to be overestimated. Not only is it assumed that ample acreages will be available, but that 100% of historic habitat functions and processes will be reestablished. For an excellent discussion of why this is unlikely, see Rose (2000). Not only is this rate of efficiency highly unlikely, but we have no reason to believe that areas restored by BDCP would not be subject to colonization by non-native invasive species of all orders present in the Delta. The one thing that we *know* about shallow water habitat is that it is readily invaded by non-natives; we also know that even under Dual Conveyance, the risk of entrainment for organisms in the South Delta will remain relatively high, even at relatively moderate rates of OMR, simply due to proximity to the diversion points. Even if productivity in these areas is greater than clam consumption, which is unproven, the continued high risk of entrainment makes the South Delta inherently unsuitable habitat for aquatic species. Completion of construction is NOT the same as restoration; restoration must be determined by the performance of a functional assessment.

Integration of Independent Science in BDCP Development

A review of Chapter 10 reveals a stated intent to include independent science in the development of the BDCP. What is written is largely a review of previous independent review reports of previous draft versions of the BDCP and listings of recommendations with assertions that appropriate changes were made to the subject document/process/criteria. It would improve the credibility of the document considerably if explicit examples of these changes were included in the narrative and others were cited by reference.

An alternative to listing the recommendations from each of the various reviews might be to list how the BDCP was modified in response to the recommendations and to show with examples how the document has been responsive to the input of all the various review panels and documents. As Chapter 10 now stands there is no discernible change to the original offering, and therefore the likelihood of future response to further reviews or Adaptive Management procedures must remain suspect.

The BDCP may have been subject to independent scientific review, but it is difficult to see how the subsequent drafts have been substantively altered to a point where it can be determined that this independent science has been integrated.