EXHIBIT A

The Court, having considered the parties input and objections, has the following instructions for and asks the following questions of the 706 Experts:

<u>Preliminary Instructions</u>: Your responses to these questions should be limited to information contained in the Biological Opinion ("BiOp"), administrative record, declarations, and pleadings in this case (collectively referenced as "the record"). You need not comb the entire administrative record to find answers to these questions. Rather, you may rely on the BiOp, the parties' pleadings, and the declarations to help identify relevant portions of the administrative record. However, if you do find other relevant documents in the administrative record, you are free to use them as you see fit. Your answers should be provided in the form of a written memorandum directed to the Court's attention. If a question cannot be answered based on the record, please indicate why the question cannot be answered. If you need further clarification, please communicate your requests to the Court.

1. Please explain the evidence in the record that supports the Fish and Wildlife Service's ("FWS") conclusion that entrainment has "sporadically significant" effect on population dynamics?

- **a. If not encompassed in the answer to the above question, please explain any other evidence in the record that supports FWS's general conclusion that entrainment has an effect on subsequent abundance?**

Quinn response:
Figure E-4 in the BiOp indicates that in many years on the order of 10% of the adult smelt population is entrained and these appear, to my eye at least, to be mostly in years with more negative flows. However, the BiOp stated (P. 210), "delta smelt entrainment can best be characterized as a sporadically significant influence on population dynamics". Later, on the same page, it stated, "… currently published analyses of long-term associations between delta smelt salvage and subsequent abundance do not support the hypothesis that entrainment is driving population dynamics year in and year out (Bennett 2005; Manly and Chotkowski 2006; Kimmerer 2008)." How are we to interpret these statements?

It is plausible, in principle, that some factor could have a large effect on population dynamics but not in every year. In an extreme example, if a species of fish matured entirely at age one, any factor that eliminated reproduction in a single year would cause the species to go extinct, even if this factor had never operated in the past. A massive, one-of-a-kind pollution or disease event that killed all larval fish in one year would extirpate the species, even though such factors had not affected the species' population dynamics in previous years. In the case of delta smelt, circumstances might bring the fish into the proximity of the pumps, leading to large losses, but only from time to time.

I inferred from the BiOp's statements (quoted above) that conventional stock-recruitment analyses would not (or did not) detect an effect of entrainment but that it nevertheless constitutes a threat to the persistence of the species. Bennett (2005) concluded that "… it is unlikely that losses of young fish to the export facilities consistently reflect a direct impact on recruitment success later in the year." The abstract of Kimmerer's (2008) paper stated, "The effect of these [entrainment] losses on population abundance was obscured by subsequent 50-fold variability in survival from summer to fall." Kimmerer (2008) concluded, "… despite substantial variability in export flow in years since 1982, no effect of export flow on subsequent midwater trawl abundance is evident. This is not to dismiss the rather large proportional losses of delta smelt that occur in some years; rather, it suggests that these losses have effects that are episodic and therefore their effects should be calculated rather than inferred from correlation analyses."

It would seem that evidence should have been presented in the BiOp to demonstrate such effects, based on some calculation. In which years were there large losses that can be directly attributed to the pumping operations, and what were the effects on subsequent recruitment? Because the smelt are largely annual fish, a catastrophe in a single year could put them at great risk of extinction and two bad years in a row could accomplish it. The risk inherent in the statistical and ecological uncertainty is borne heavily by the species but there still should be some evidence in the record to reveal these effects.

Hilborn (Document 393) criticized the BiOp for its failure "…to use available quantitative population dynamics models that track a population through its life histories." He noted that the BiOp did not "determine whether … entrainment produces population level effects on delta smelt..." I concur with Hilborn that "… there is ample available data regarding the delta smelt and the conditions relating to delta smelt survival and abundance" (his Point #6). Given the amount of data collected on smelt and other biotic and abiotic aspects of the system for decades, the analysis in the BiOp was very limited.

Punt response:
It is indisputable that the population size of delta smelt is currently at an historical low. This population is subject to a large number of potential stressors. The ideal way to assess the relative role of each of these stressors is to develop a population dynamics model which captures the temporal and spatial dynamics of delta smelt as well as those of the various stressors, and to use standard peer-reviewed statistical methods to assess the impact of those stressors given variability in population dynamic processes (survival in particular) as well as the uncertainty associated with the various data sources. Considerable amounts of data are available for delta smelt compared to most fish populations, and the biology is fairly well understood. Several authors (e.g. Bennett 2005; Docs 396, 455, 508, and 605) have conducted analyses to explore the relationships among different life stages given data collected during monitoring surveys, as well as possible impacts of export losses. To date, however, no authors have explored the changes in long-term population size which would result from changes in various stressors, in particular entrainment[1] and changes in flow due to the project operations using population dynamics models which include the whole lifecycle.

[1] The terms "entrainment" and "salvage" are used somewhat interchangeably in this document.

It is surprising that a population dynamics model was not developed for delta smelt for the BiOp. For example, Bennett (2005) developed a simple population dynamics model, accounting for seasonal effects and two annual cohorts, although this model was not fitted to the available data for delta smelt in a formal way. This model provided suggestions that it may be difficult to detect impacts due to losses (but did not show this formally) and that export mortality could be easily offset by very small changes in other biological parameters. The model developed by Bennett could have been extended to more fully account for the biology of delta smelt and fitted to data to assess the population-level effects of impact of the project.

The term "sporadically significant" is interpreted here to mean that over the long term, the expected size of the delta smelt population will be lower with entrainment than without entrainment. For example, it could be interpreted to mean that in nine out of ten years, the impact of entrainment is negligible at the population level, but that in one year in ten, the impact is sufficient so that over the long-term the expected population size is reduced to the extent that the probability of population recovery (or at least non-extinction) is impacted by a non-trivial amount.

The term "sporadically significant" appears in the BiOp related to the population-level effects of delta smelt entrainment (BiOp pg. 210; AR 225), noting in particular that Kimmerer (2008) estimated the annual entrainment of delta smelt (adults and progeny combined) ranged from 10-60% per year from 2002-2006 (BiOp pg. 210; AR 225). The term is used because the BiOp (pg. 210; AR 225) notes "currently published analyses of long-term associations between delta smelt salvage and subsequent abundance do not support the hypothesis that entrainment is driving population dynamics year in and year out". The latter statement is not in dispute and several analyses draw the conclusion that entrainment is not driving the population dynamics every year (e.g. Bennett 2005; Manly and Chotkowski 2006; Kimmerer 2008; Docs 396, 455, 508, 605)

The BioOP (pg. 203; AR 218) notes "the following analyses assume that the proposed CVP/SWP operations affect delta smelt throughout the year either directly through entrainment, or indirectly through influences on its food supply and habitat suitability". Kimmerer (2008) outlines some of the biological rationale for assuming effects: (a) large numbers of fish are entrained and (b) large quantities of water are exported. He notes that "manipulations of flow patterns in the Delta provide the only apparent tool to managing some fish populations such as delta smelt". The latter is, of course, not a reason to assume effects.

A priori, it is not unreasonable to assume that higher levels of entrainment would lead to lower population sizes. However, the size of any effect depends on the frequency with which entrainment occurs to a substantial extent, and the impact of entrainment when it occurs. The only analysis in the BiOp that shows a potential population effect is Figure E-22, which identifies a linear relationship between the TNS index and the FMWT index in which X2 is significant predictor. However, the relationship between TNS and subsequent FMWT is not shown to fully quantify the effects.

It is not clear, however, whether one should expect to be able to detect sporadic impacts in the data set for delta smelt even if there were such impacts. The (statistical) power to detect an effect even if present can be low, and hence standard statistical tests (such as those in Doc 605) may fail to identify a relationship between subsequent population size and entrainment if one exists. Doc 605 claims that the methods employed have high power to detect an effect if there were one but detailed results are not shown.

In conclusion, the record is clear that (a) it is reasonable to hypothesize that entrainment may have population-level effects on the delta smelt population, and (b) entrainment does not have population-level impacts every year. However, the record provides little evidence that entrainment is "sporadically significant", although the power of the methods used to detect such effects (e.g. in the BiOp and in Docs 396, 455, 508 and 605) has yet to be fully explored. It is possible, therefore, that the FWS is correct that entrainment has long-term population effects, but the data are not sufficient to show this at present.

2. Please explain the evidence in the record that supports FWS's contention(s) regarding the existence of "break points" (i.e., that at certain flows less negative or equal to -5000 cfs, entrainment of smelt increases noticeably)?

Quinn response:
The BiOp stated (P. 347), "Visual review of the relationship expressed in Figure B-13 indicates what appears to be a "break" in the dataset at approximately -5,000 OMR; however, the curvilinear fit to the data suggested that the break is not real and that the slope of the curve had already begun to increase by the time that OMR flows reached -5,000 cfs." The non-linear (i.e., curved) regression formula explained, in a statistical sense, 44% of the variation in the data, as shown in Figure B-13. The BiOp went on to describe a piecewise polynomial regression with a linear-linear fit that indicated a "change-point of -1162" and then a linear-linear-linear analysis indicating two change points, at -1500 and – 2930 cfs (P. 348-349). However, the amount of the variation in the data explained, in a statistical sense, by these relationships was similar not greater than that in the curved relationship. Given the many sources of uncertainty around the data going into these analyses, I am not convinced that a "break-point" is evident, though my basis for saying so is more intuitive than statistical. The addition of a new data point from another year, or adjustment of existing data because of a new calculation scheme, would alter the estimated break point. Shakespeare's line that one might "cavil on the ninth part of a hair" comes to my mind. I suspect that the underlying relationship is probably curved, and that year to year variation in fish distribution and abundance, and other factors, will make it unwise to overestimate the precision associated with the points and any line fit to them.

Punt response:
RPA Component 1 Action 1 limits exports so that the average daily flow is no more negative than -2,000 cfs over 14 days (with a 5-day running average no more negative -2,500 cfs) [Action 1; BiOp pg. 329; AR 344]. RPA Component Action 2 limits exports to various levels between 1,250 and -5,000 cfs over 14 days depending on various triggers [Action 2; BiOp pg. 352-356; AR 367-371].

A key consideration with respect to this question is determining the best method for predicting entrainment of delta smelt. The BiOp (Pg 347-351; AR 362-366) focuses on the numbers entrained as a function of combined Old and Middle River flows (OMR flows) [Figures B-13 and B-14] while other analyses (e.g. Docs 396, 455 and 484) focus on entrainment expressed as a fraction of the number of adults (as measured using the FMWT survey). The approach taken in the BiOp is only justifiable if (a) "as the population of delta smelt declined, the number of fish at risk of entrainment remained constant" (BiOp pg 349; AR 364), or (b) any entrainment, no matter how small relative to the total population size, has long-term consequences for the population size of delta smelt. Expressing entrainment as a function of population size would be more standard in population ecology because it reflects that fact that, all things being equal, a doubling of population size should lead to an expected doubling of the number of animals entrained. Note that this question does not address the issue of what level of entrainment would have population level effects, merely whether there is a relationship between entrainment and OMR flows, and what that relationship is.

a. **Does the Johnson study cited in the BiOp, or any other document in the record, such as AR 9454, provide support for this?**

Quinn response:
Document AR 9454 is a set of draft minutes from the BiOp technical team's meeting on October 10, 2008. The minutes do not formally present new data but nevertheless merit comment for two reasons. First, they describe a specific discussion (on P. 5) of data presented by Peter Smith. I infer that these are the data graphed as Figure B-13 in the BiOp because the figure caption lists "P. Smith" as the source. Assuming that this is correct, I do not see specific inflection points. I think the human eye is disposed to see patterns even in random data, and there is a general tendency to look for and find break points in plots such as B-13. I suspect that this reflects over-interpretation of the data, given the many sources of uncertainty (see also my comments above on the BiOp). Second, however, I find the draft minutes interesting because they clearly reflect the efforts of the group to balance water needs with the needs of the fish, given the many variables involved. Year to year variation in environmental conditions, uncertainty about fish distribution, abundance and population dynamics, and other factors must be massaged into a set of adaptive responses that can be written down for others to comment on or implement. These informal notes from the meeting are a revealing glimpse into the reality of adaptive management.

It is my understanding that the "Johnson study cited in the BiOp" mentioned above refers to analysis by Michael L. Johnson of data on smelt salvage and flows. I received a series of memos and draft meeting notes in February 2010 and I am operating under the belief that these are the materials to which the question refers. If so, my comments (above) apply here as well. The data seem to be the same and my feelings (that the break points are rather artificial, and also that the group was clearly wrestling with the need to meet the goals of fish protection and water expectations) are unchanged.

Punt response:

In responding to this question, I have interpreted the question as relating to the relationship between entrainment and OMR flow rather than that between entrainment per unit population size (i.e. entrainment rate) and OMR flow.

A "breakpoint" can be defined in various ways. In a general sense, it can be interpreted as a change in the slope of a relationship (most commonly a linear relationship). However, for the purposes of relating entrainment to OMR flows, a "breakpoint" should rather be interpreted as the point at which entrainment changes from a constant (low) level to increasing entrainment with increasing flows. Figure B-13 of the BiOp shows a gap at about -5,000 cfs. However, that cannot be interpreted to be a "breakpoint" because, although the mean entrainment below -4,000 cfs is lower on average than above -5,500 cfs, there is no statistical evidence for a breakpoint. Rather, if there is a breakpoint, that breakpoint occurs at around -1,200 cfs (BiOp 349-351; AR 364-366). It should be noted, however, that a variety of relationships will fit the data in Figure B-13 (BiOp 348; AR 363). Therefore "Johnson study" (Figure B.14) does provide support for a breakpoint in the relationship between entrainment and OMR flow at approximately -1,200 to 2,000 cfs.

b. What role does figure B-13 play in FWS's rationale for flow restrictions?

Quinn response:
It appears to me that this figure, and alternative analyses of what seem to be the same data (e.g., Figure B-14) played a large role. The apparent increase in salvage as flows become more negative is very visual.

Punt response:
The flow values referred to in Action 1 (-2,000 cfs) do appear to be linked to the results of the "Johnson study". Doc 470 notes that "the selection of -2,000 cfs for RPA component 1A to protect pre-spawning adults was validated by an analysis conducted by Dr Michael Johnson". In contrast, the section on the justification for the flow guidelines for Action 2 (BiOp pg. 355-356; AR 370-371) does not refer to any specific analysis as the basis for the particular choices of flows, although it is noted that entrainment can occur at flows of -5,000 cfs. Figure B-13 is referred to in the justification for the flow values for Action 1, but this figure appears not to have been used directly to determine the flow values for Action 1.

c. Was relevant population data available as of the date of issuance of the BiOp (December 15, 2008) for use in evaluating the impact of negative OMR flows on entrainment?

Quinn response:
The BiOp does not present a single table or set of tables with the actual data on which the figures and analyses were based, and this is apparently why Deriso felt compelled to visually estimate the point values from the graph itself. I find this regrettable; I would have expected summary tables of the annual values for the various fish surveys and also summary information on such abiotic features as X2 and flow. Indeed, the lack of common access to the basic data was puzzling to me. In contrast, daily counts of salmon ascending the Columbia River are available on-line, along with daily records of flow, temperature, and

turbidity. Nevertheless, it seems clear to me that the relevant data were available to the FWS.

Punt response:
It is always the case that more data improves the sophistication of an analysis and with more time a better analysis could be conducted. In the context of evaluating the impact of negative OMR flows on entrainment, the data sets analyzed (salvage numbers, OMR flows, and FWMT numbers) do constitute an adequate basis to develop relationships between salvage and OMR flows. Ideally, the analyses should have considered also the uncertainty associated with each of the data sets and the location of smelt within the Delta. However, given that the ultimate intent of the analyses was to develop a way to implement the RPAs, the data sets analyzed are adequate.

d. If so, was it unreasonable for FWS to rely in part on the information represented in figure B-13 (which compares OMR flows against raw salvage numbers)?

Quinn response:
I do not regard it as unreasonable for FWS to have relied in part on this figure and the data behind it. To rely entirely on it would, however, have neglected the complexities of the issue, including but not limited to some of the points raised by Hilborn, Deriso, Newman, and others. Both the number of fish salvaged and the proportion salvaged (or some proxy for proportion) are relevant, in my view, as are other kinds of information. By itself this figure and the associated data are much too limited.

Punt response:
The reliance on Figures B-13 (and B-14) depends critically on the assumption that the number of fish at risk of entrainment is independent of population size. However, the BiOp (BiOp pg 349; AR 364) identifies that the residuals for four of the five most recent years in Figure B-13 are negative, and suggests that this may be due to lower abundances in those years. The BiOp (BiOp pg 349; AR 364) also refers to normalizing the salvage data by the estimated population size, and this forms the basis for the lookup tables for the incidental takes (BiOP pg 388; AR 403). Moreover, salvage expressed relative to population size was considered when Actions 1 and 2 were being developed (e.g. AR 9457-9458) and the BiOp itself states “salvage itself is clearly at least partially a function of abundance. In other words, the more delta smelt there are out there, the higher the salvage numbers will be, given the same operational conditions and delta smelt distribution” (BiOp pg 383; AR 398). Thus, I conclude that it was unreasonable (given that appropriate data and analysis methods were available to account for population size) to have only relied on the information in Figures B-13 and B-14 rather than on an analysis in which salvage is expressed relative to population size.

e. Does Dr. Deriso’s independent analysis of data in the record take into consideration all relevant factors (e.g., geographic distribution)?

Quinn response:

Deriso (Document 401, filed 11/13/2009) raises a number of valid issues with respect to the BiOp, including the use of raw salvage numbers vs. scaling the numbers relative to an index of smelt abundance. The BiOp itself seems to also recognize this issue, as indicated by comments on P. 349, but the emphasis is still on numbers of fish, as in Figure B-13. It is unclear why, given the availability of the data, the BiOp did not present the alternative analysis because this seems like an obvious thing to do. Based on his re-analysis of the data using salvage index and OMR flow, Deriso (point #63) concluded that "… there is no scientific basis for FWS's imposition of OMR flow restrictions at flows less negative than -6100 cfs (and potentially -7000 cfs)." Newman (Document 484) concurred with the need for some scaling of salvage by abundance but his analysis indicated an increase in salvage at OMR flows of – 4000 cfs. Newman (#13) pointed out that "variation in the spatial distribution of the fish can confound attempts to quantify the nature of the relationship between OMR flows and salvage." In his rebuttal (Document #508), Deriso pointed out that the BiOp did not explicitly consider spatial distribution either (e.g., in Figure B-12). Thus the explicit answer to the above question would seem to be "no, Deriso's analysis did not take into consideration geographic distribution". The more complete question might have been, "and would this have made any difference?" It is possible that in some year or years, conditions that would otherwise have resulted in a high salvage count or index did not because some other factor(s) unrelated to pumping kept the fish out of the area. Including such years with anomalous spatial distributions in the analysis makes it more difficult to detect the underlying relationship between pumping and salvage. Some adjustment for spatial distribution might be included in the models of salvage but it is not clear to me whether the existing fish data could be used in this manner. The issue of spatial distribution and sampling accuracy, including graphical presentations, was explicitly addressed in Document 398, especially the Exhibits).

Stepping back from the specific issue of the existence of a "break point" and considering the issue of population level effects more broadly, Deriso correctly noted that analysis of populations should include consideration of possible density dependence, and this is taken up in other documents, including but not limited to Bennett (2005). Indeed, Bennett (2005) cites the book by Quinn and Deriso (1999)[2]. One factor that was not noted by Deriso is the effect of various sorts of error and uncertainty in the power to detect patterns. For example, he constructed a Ricker-type spawner recruit relationship using the FMWT data from one year and the next (Point #71) and then seems to have used this relationship as the basis for concluding that "there is no statistically significant relationship between salvage and the population growth rate." However, when I plot the FMWT data using pairs of consecutive years as Deriso describes, the relationship is extremely variable. What is the consequence of using a very poor relationship in a subsequent adjustment?

More fundamentally, what are the uncertainties in the population estimates themselves, and might there be shifting levels of accuracy as population levels change? Deriso stated (Point 15), "The population growth rate is an appropriate and reliable measure of the population increases and decreases from year to year." This is certainly true, if it is known without error but what are the assumptions about sampling? That is, as smelt become increasingly scarce, does their overall distribution become "thinner all around" or is it "patchy", and how might

[2] The Quinn that wrote this book with Deriso is Terrance Quinn, not myself

such changes influence the reliability of data from different surveys of abundance? In general, "noisy" data make it more difficult to detect underlying patterns, even if the patterns are genuine.

Finally, Bennett (2005) presented evidence (Figure 21 and text) of climate effects on abundance of delta smelt, as indicated by various forms of survey data. This factor does not seem to have been explicitly included in Deriso's analysis, though of course it is implicitly included in the variation in the data.

Punt response:
No analysis can ever take all sources of information into account. Dr Deriso's independent analysis of the entrainment and flow data (Docs 401 & 455) provides an investigation of the relationship between the entrainment rate (salvage / FMWT) and OMR flow. The analysis is predicated on a variety of assumptions, in particular that FMWT is a representative measure of population size. It ignores several possible factors, including changes over time in geographic distribution and the uncertainty associated with measuring population size. However, information on geographic distribution and the extent of measurement error does not appear to be in the record (in fact, the numerical data on which many of the figures in the BiOp are based were not listed in the BiOp), and these factors are not explicitly taken into account in the development of the incidental take levels. Thus, while all of the factors which should ideally have been taken into account were not, the analysis does make use of all the readily available information.

f. Does the record contain evidence supporting FWS's conclusion that the specific flow regimes imposed by the BiOp are necessary to minimize entrainment of delta smelt by project operations?

Quinn response:
Entrainment, in this context, refers to the removal of adult and larval fish from the water column at a pumping station, which is typically lethal for comparatively fragile species such as delta smelt. The process is, therefore, a "take" and the only questions are how many individuals are to be taken, and how large a fraction of the entire population this will constitute, and whether there are effects on the long term viability of the population as a whole. Adult delta smelt can be identified and counted, and are referred to as salvage. However, there seems to be agreement that larval smelt (notably those less than 20 mm long) are not readily detected in the salvage operations so their entrainment is inferred, not measured directly (e.g., BiOp P. 338).

The number of fish entrained depends in part on the total population, and the extent to which their distribution (in space and time) places them in sufficient proximity to the pumps that they are drawn into the system. BiOp Figure B-12 displays the cumulative proportional salvage over water years 1993 to 2006. This reveals the seasonal pattern of smelt entrainment, reflecting their spawning migration in winter. Thus the BiOp's protective actions with respect to flow regulation necessarily have a seasonal component.

In addition to the overall seasonal pattern (i.e., predictable in a general sense by a calendar), specific environmental factors appear to stimulate spawning migration by the fish. This is a common pattern in many fish species, and flow, salinity, tides, temperature and turbidity are often cues to migrate. Thus it is reasonable that protective measures include both an overall seasonal component and also respond to such events that might be cues to migrate or otherwise affect fish distribution. Thus the BiOp states that "Action 1, Part A covers the period (December 1 to December 20) when first flush salvage events were historically uncommon" (BiOp P. 345 and Figure B-12).

As discussed below (Quinn response to Question 3), salvage of smelt seems to be related to rapid increases in turbidity if they occur within the general time frame of migration. The use of any specific cutoff level is always a bit arbitrary when dealing with continuous variables such as flow and turbidity and temperature, but the use of 12 NTU seems to generally capture the high turbidity events that are associated with salvage. It should be possible, with the data in hand, to calculate the extent to which this is so (as opposed to some other value of turbidity). Perhaps this was done but I did not notice such analyses.

BiOp Figure B-13 depicts the relationship between number of smelt salvaged and the combined OMR flows, weighted for salvage. This appears to indicate an increase in numbers of fish salvaged that is gradual from about 3000 cfs to about -3000 cfs, after which salvage seems to increase more rapidly as flows become more negative. Piecewise polynomial regression suggested an inflection point at -1162 cfs (Figure B-14). From a biological standpoint it is not clear to me why the underlying nature of the relationship should have this "hockey stick" shape rather than a gradually increasing curve. Given this, the high degree of reliance on data points that are themselves subject to various forms of uncertainty make me uneasy when it comes to setting a specific threshold value.

As was pointed out (e.g., Deriso's Document 401, filed 11/13/2009, Point 54), this salvage does not consider the size of the population of fish. For example, if flows were the same every year but the total population of fish varied, we might expect to see more fish salvaged in years when they were more numerous, if the spatial distributions were similar. This need for adjustment was recognized in the BiOp (P. 349, third paragraph); it is not clear why such an adjustment was not made for the data examined in this part of the report. The Cumulative Salvage Index was defined on P. 385 and values from 1993 – 2008 are listed in Table C-1 on P. 386. Deriso's analysis of OMR flows, adjusted for salvage, and the salvage index data led him to conclude that "..there is no scientific basis for FWS's imposition of OMR flow restriction at flows less negative than -6100 cfs…" (Point 63 in Document 401).

Dr. Newman (Document 484, Point 11) stated that "I concur with Deriso's general notion of scaling salvage by some measure of population abundance" but he expressed concerns related to the model fitting procedures and also to the ways in which variation in spatial distribution of fish can greatly alter the number or proportion salvaged from year to year at a given flow regime.

The question posed above by the Court is somewhat paradoxical. It refers to "… the specific flow regimes imposed by the BiOp…" and the need "… to minimize entrainment of delta smelt…" By definition, minimization is a continuous process and specific flow regimes (e.g., -5000 cfs) are discrete values, as are the values of 12 NTU and 12° C. There seems to be no question that at highly negative flows the number and also the proportion of delta smelt entrained increases. The key questions seem to be 1) what combination of flow regime, time of year, and environmental conditions are associated with the marked increase in salvage, and 2) how consequential is this for the population. In answering the first part I must again emphasize that any point value has a measure of arbitrariness. If -5000, then why not -4900 cfs? Given the many sources of variation in the data, it strikes me as necessary to set limits even though there may not be strong statistical basis for a particular figure rather than a slightly different one. The second point, consequences for the population, is part of a deeper question related to extinction risk for the species. Use of raw salvage numbers was questioned (e.g., by Deriso) and an index was suggested as superior. This would seem to imply that it is the fraction of the population, not the count, that is important in assessing harm. I have two concerns about this line of reasoning. First, there seems to be evidence that when smelt are less abundant, higher percentages of them are lost to salvage. This is illustrated in Figure 1 of the "Independent Expert Panel Review of the Family Farm Alliance's Information Quality Act Correction Requests" (Document 473-3, filed 12/29/2009). Thus plotting flow against a salvage index might not fully capture the risk to the population. Second, even if the percent losses were independent of population size, the population becomes at greater risk of extinction as it gets smaller. Almost all the fish in this species mature and die at age 1, thus there is very little "carry-over" from year to the next. In a year with few adults spawning and adverse environmental conditions, the population might decline so low that it might not recover. In contrast, long-lived species will fluctuate much less in total population from one year to the next. The counter-argument might be that at low population sizes the species would be released from density-dependence and should be especially productive. However, given how scarce these fish are, it seems unlikely that intra-specific competition is limiting production.

On balance, though it is appropriate for the FWS to have some leeway in making decisions and setting limits in their efforts to protect and recover listed species, and despite my reservations about some of the objections raised in declarations from scientists supporting the plaintiffs, the validity of specific flow regimes is undermined by the incomplete analyses that were done on the available data.

Punt response:
The objective of Actions 1 and 2 is to reduce entrainment of pre-spawning adult delta smelt during December through March by controlling OMR flows during vulnerable periods (BiOp pg 280; AR 295). Component 1 will increase the suitability of spawning habitat for delta smelt by decreasing the amount of Delta habitat affected by the projects. The specific flow regimes for Action 1 do appear to have been selected based on Figure B-14 (BiOp pg 347; AR 362). The BiOp also mentions that "Recent analyses indicate that cumulative adult entrainment and salvage are lower when OMR flows are no more negative than -5,000 cfs in the December through March period" (BiOp pg 281; AR 296), but which these "recent analyses" are is not specified.

If population size were not important for determining entrainment, then figures such as B-13 and B-14 could have been used to justify the flow levels for Actions 1 and 2. However, reliance on these figures is not justified given the information which was available in the record at the time the BiOp was written, i.e. that entrainment is related to population size, and that methods that can account for the impact of population size on entrainment were available.

3. Please explain the evidence in the record that supports the use of turbidity as an indicator for the timing of upstream migration of delta smelt?

Quinn response:
The BiOp (P. 150) stated, "Delta smelt seem to prefer water with high turbidity, based on a negative correlation between the frequency of delta smelt occurrence in survey trawls during summer, fall and early winter and water clarity." The BiOp references papers by Feyrer et al. (2007), Nobriga et al. (2008) and Nobriga and Herbold (2008) on this point. Examination of Table 1 and Figure 4 in Feyrer et al. (2007) indicates that the probability of capture of delta smelt declines with water clarity, though it also declines at high salinities, and the statistical analysis reported in this paper supported the conclusion that both factors were important for this species. The relationship with temperature is less strong. The FMWT catch rate data used in this analysis are not the same as upstream migration data. However, the general life history and ecology of this species seems adapted to use the combination of rising temperature, turbidity and flow to cue their breeding migration in winter. The BiOp described this pattern on P. 331, and then produced a series of plots that combine data on OMR flow, turbidity (NTU) and salvage of adult smelt (Figures B-1 through B-6). The nature of these time series of data (that is, changes in flow, turbidity and the entrainment of fish over a period of time) are not amenable to simple regression analyses because the "pool" of fish available to be entrained or otherwise sampled varies over time. Thus stimulatory conditions at the beginning and the end of the migration period will not elicit fish because at the beginning the fish have not yet arrived in the vicinity and at the end they have all left. However, if one examines the graphs it seems evident that sharp increases in turbidity are indeed associated with sharp increases in salvage. For example, B-5 (water year 2004) shows that there were two distinct modes of salvage (i.e., periods of time when salvage was markedly above the baseline of or near zero). They both appeared to coincide with dramatic (high and rapid) increases in turbidity. The period between these two modes was not characterized by notable changes in flow but rather by a decrease in turbidity. In water year 1993 (B-3) there was a single sharp increase in salvage that immediately followed a sharp increase in turbidity but was some time after the increase in negative flow.

Punt response:
The record contains evidence (e.g. BiOp Figures S-7 and B-1 through B-6) that entrainment is related to turbidity (also BiOp pg 146; AR 161) and that occurrence of delta smelt is related to turbidity (Freyer et al. 2007). That turbidity is an indicator for the timing of upward migration of delta smelt is a reasonable assumption because entrainment, which must relate to delta smelt being in the South Delta, is evidence for migration having occurred.

4. Please explain any record evidence of historical delta smelt migration patterns that reflect migration of delta smelt into the

South and Central delta each year, independent of operation of the pumps?

Quinn response:
I may have missed it in the myriad files that we have received but I am not able to determine precise dates when the pumping operations commenced, and when they became fully operational (i.e., at their current capacity). For example, in NRDC v. Kempthorne (505 F.Supp. 2d 322) we read, "For over thirty years the state and federal agencies charged with management of the CVP and SWP have operated the projects in an increasingly coordinated manner…" The BiOp (P. 276) stated that "Diversions of water from the Delta have increased since 1967 when the SWP began operations in conjunction with the CVP." Tow net sampling apparently began in 1959 (Turner, J. L. and H. K. Chadwick. 1972. Distribution and abundance of young-of-the-year striped bass, *Morone saxatilis*, in relation to river flow in the Sacramento San Joaquin Estuary. Transactions of the American Fisheries Society 72: 442-452) and by this time there were already diversions taking place. Thus there was apparently no sampling of smelt during periods that were "independent of operation of the pumps", or I found no such data.

Punt response:
It is clear from the record that delta smelt undergo a spawning migration annually. The BiOp (pg 212, AR 227) notes that "although WY type may sometimes affect the spawning distribution (Sweetnam 1999), there is wide, apparently random variation in the use of the Central and South Delta by spawning delta smelt". However, attempts to comment on migration patterns independent of pumps would be purely speculative given the lack of data collected before the pumps impacted the Delta. Moreover, the impact of entrainment may have changed the migration patterns for delta smelt, making any inferences concerning past migration patterns even more uncertain.

5. Is FWS's comparison of historical baseline data to CALSIM runs scientifically reasonable, in light of FWS's conclusion that CALSIM runs intended to represent historical conditions did not accurately nor precisely match historical baseline data?

Quinn response:
As described in Aaron Miller's declaration (Document #400, points #10-12), "CalSim II uses historically based hydrology data from October 1922 to September 2003, while water supply demands (i.e., level of development) are held constant… CalSim II determines the flows required to meet the salinity-related Delta standards, including the required flow for meeting the X2 standards." The BiOp used "Dayflow", a different model for estimating the location of X2. As described by Miller, "Dayflow's X2 estimates are based on an equation developed by Kimmerer and Monismith (KM equation)." Miller stated that the estimates for the X2 location and areas of low salinity from the two models differ. These different outputs result from fundamental differences in how the models operate, and their purposes. Importantly, Dayflow incorporates changes in development and operational regulation over the years whereas CalSim II does not. This is a major difference, as indicated by the fact that the removal of water increased about 4-fold from 1967 to 2002.

Derek Hilts (Document #482) commented directly on the declaration by Miller and noted that the BiOp stated (P. 206), "The historical time series are intended to show where changes in water project operations have caused or contributed to changed Delta hydrology." This strikes me as a reasonable objective but one must be able to distinguish operations-related changes in flow from the background variation in the system caused by runoff and other sources. Hilts concluded, "The Dayflow model data captures the X2 that prevailed historically. The CalSim II simulations that Mr. Miller would have the FWS use do not."

Miller stated (Point #29) that the most valid comparisons are between model and model runs, such as CalSim II and CalSim II, and that comparisons between CalSim II and Dayflow are not "meaningful". As long as we bear in mind the fact that these are two very different models, I do not see why we cannot compare them. It is sometimes said that one cannot compare apples to oranges but I disagree. We can readily compare apples to oranges in terms of color, caloric density, citric acid concentration, sweetness, skin thickness, and other attributes. We just need to remember that they are different kinds of fruit. Likewise, the CalSim II and Dayflow models are different and any comparison of use of outputs from both models needs to explicitly recognize this.

Recently (4 March 2010) Dr. Punt and I received fileswith extensive raw data of X2 and OMR flows as generated by CalSim II and Dayflow, and differences between the two outputs by month over a number of years (Suppl_Resp_to_706_-_Ex_B). These data, summarized as bar graphs with red and blue columns representing mean monthly values for the two outputs. We were then sent files in the program Microsoft Excel upon which the graphs were based, for us to examine further.

The graphs indicate that the OMR flows were more negative when estimated from the CalSim II than Dayflow, regardless of the month. I did a quick and rather crude analysis of these data, using a paired t-test. Specifically, I took the reported values for each month and year and paired them for the two data sources. This analysis neglects the possible effects of year and month that a more sophisticated analysis could have considered. The t-value was 4.75, far exceeding the traditional level of significance accepted by most scientists. Indeed, the chances that these two group of data (monthly Dayflow and CalSim values in cfs) are drawn from the same population are vanishingly small. The two series of data are correlated (that is, high values of one tend to be associated with high values of the other) but the correlation coefficient (0.62) is not exceptionally strong (values for the coefficient range from 0 for no correlation whatsoever to +/-1.0 for a perfect correlation)[3].

The graph of X2 values also shows differences between the models but they vary seasonally. The CalSim II values are farther upriver from September to February and the Dayflow values are farther upriver from March to August. Both the fall-winter and spring-summer values from the two models differed very significantly from each other, as indicated by paired t-tests (fall-winter: $t = 17.6$, average values: CalSim = 81.6 and Dayflow = 72.4; spring-summer, $t = 15.9$, average values: CalSim = 68.2 and Dalflow = 74.0). These t-values indicate that the

[3] The two series of data are not fully independent so my analyses are not strictly appropriate but I present them as approximations, to indicate general patterns, rather than precise levels of statistical confidence.

sets of data from the two models are very different from each other, in each of the two seasonal periods.

It is therefore clear that the outputs from the two models cannot be used interchangeably for estimating either X2 (in km) or flow (in cfs). This does not reflect any criticism of either model. Their inner workings are apparently quite different, as are their fundamental purposes, as explained to us. However, any comparisons between them must explicitly account for the differences. One could not legitimately compare one period of years with data from one model to a subsequent or future set of years from the other.

Punt response:
In responding to this question, I do not claim to be an expert on the specifications and use of the Dayflow and CALSIM II models. Rather, I am commenting on the implications of there being differences (for many reasons, see, for example, Doc 400) between Dayflow and CALSIM II, and the potential effect of these on inferences concerning the project's impact on entrainment of delta smelt as well as potential delta smelt habitat, as quantified using X2.

The comparison between the "historical" values and the output from CALSIM II is used in three places in the BiOP:

- The relationship between salvage and OMR flow [salvage = 3757 – 0.4657*OMR Flow[4]] developed by Grimaldo et al. (accepted manuscript) is parameterized using historical OMR and salvage estimates. This relationship is then used to predict changes in salvage which would occur under various future scenarios that are modelled using the CALSIM II model (BiOp pg 213-214; AR 228-229; Table E-5).
- Predictions are made of X2 during September through December (and hence habitat) using the CALSIM II model and compared to the "historical" (Dayflow) estimates in Figure E-19.
- A relationship between the TNS index and X2 is estimated using the historical data and predictions of changes in the TNS index made using the CALSIM II model and the fitted relationship (reported in Figure E-23 of the BiOp).

In principle, there is nothing wrong with fitting a model using a set of OMR / X2 values from one model and making predictions using OMR / X2 values which are based on the output from a different model, as long as the two sets of values are calibrated (for example by multiplying the outputs from CALSIM II by 0.9 if the CALSIM II values for comparable years are 1/0.9 of the historical values). Not calibrating the two sets of model outputs will lead to some bias in the inferences, with the level of bias dependent on the net effect of all of the differences between the "historical" and CALSIM II values for the same years.

The ideal way to understand the implications of the project on salvage and X2 given that there are (potential) differences between the "historical values" and the output from the CALSIM II model would be to conduct predictions of OMR flows and X2 for four cases:
(1) the historical years using the "historical data";
(2) the historical years using CALSIM II set up for the historical years;

[4] It was not clear from the BiOp why this relationship was used rather than those in Figures B-13 and B-14.

(3) the future years using a CALSIM II model set up under conditions mimicking those in the RPA; and
(4) the future years using a CALSIM II model set up under conditions mimicking what might occur in the absence of the RPA.

A comparison of cases 1 and 2 allows identification and quantification of differences in the way the historical data are constructed and the CALSIM II model operates, while a comparison of cases 3 and 4 allows an evaluation of the extent to which changes in X2 and salvage can be attributed to the project rather than to other changes which may occur in the future. Results are only available for cases 1 and 4. Unfortunately, without cases 2 and 3, it is not possible to determine (a) whether the predicted differences are due to differences between the "historical data" and the CALSIM II outputs, and (b) whether the predicted differences are due to the project or other factors which could be changing in the future.

The Federal defendants provided Dr Quinn and me with comparisons between historical data and CALSIM outputs (Docs 594 and 613) for comparable years. Tables 1 and 2 summarize these comparisons for OMR Flows (December through March for comparability with the Table E-5) and X2 (September through December for comparability with Figure E-19). The specific request which led to the information on which Tables 1 and 2 are based was:

> *Several of the quantitative results in the BiOp depend on making forecasts using predictions of OMR flow and X2 from CALSIM (or CALLite) using relationships estimated using actual measurements of OMR flow and X2 (e.g. Table E-5, Figures E-5, E-23). Is it possible to directly compare actual and predicted OMR flows and X2 for some years (e.g. the recorded OMR flow for 2008 and the value predicted for 2008 using CALSIM set up for 2008)? If so, we would like to see a table of comparisons with as many years as possible.*

If the CALSIM outputs and "historical data" are indeed as comparable as is feasible[5], the results in Tables 1 and 2 suggest that the average difference between CALSIM II and historical OMR values is -2469, 273, -1561, and -1728 cfs (December through March) and the average difference between CALSIM II and historical X2 is 4, 7, 13, and 14km (September through December).

It is not straightforward to compare the values in Tables 1 and 2 with the information provided in the BiOp. However, a first order approximation is possible. Specifically, the differences between the median flows for run 7.0 and the historical average are 4223, 2031, 4056, 934 and -490 cfs respectively for wet, above normal, below normal, dry, and critical years respectively (Table E-5a), while the difference in X2 between the historical (dayflow) data and CALSIM II (study 7.0) is approximately 10km (Figure E-19). Thus, impact of the use of CALSIM II to forecast both X2 and OMR flows seems non-trivial. In particular, the differences in X2 appear to be as large as the differences seen in Figure E-19.

The comparisons above are necessarily approximations, but the calculations are reflective of what is standard practice when the method used to measure (or predict) a quantity (such as X2 or OMR flow) is changed (for example, in fisheries management when the vessel used to conduct a fishery survey is changed, it is common to conduct several surveys with both

[5] I am unable to evaluate the extent to which the results provided to Dr Quinn and me are comparable, but I have assumed here that they are as comparable as possible.

vessels to calibrate the old and new vessels). It is recognized in the record that the modelled X2 does not reflect the "historical" X2 (AR 10041; BiOP Figure E-28), and the BiOp does compare historical and CALSIM- predicted X2 values by month (Figure E-26). However, the BiOp does not make this comparison for comparable years. Failure to attempt examination of whether it is necessary to calibrate the historical data and the CALSIM output would not normally be considered appropriate scientific practice in the field.

6. Were FWS's decisions (in light of the justifications offered by FWS) regarding the years it chose to construct the incidental take statement scientifically reasonable?

Quinn response:
P. 287 of the BiOp states, with reference to salvage of adult delta smelt, "Water years 2006 to 2008 were years in which salvage, negative OMR flows, and delta smelt abundance were all lower relative to the historic values. The Service therefore believes these years within the historical dataset best approximate expected salvage under RPA Component 1." The incidental take is 7.25 x the FMWT index in the previous year (BiOp P. 386). As Deriso (Document #401) pointed out, the BiOp (P. 348, Figure B-13) excludes data from 2007 (and some other previous years) because it "had low (< 12 ntu) average water turbidity during Jan-Feb at Clifton Court Forebay." The Cumulative Salvage Index values in the three years in question were 8.3 in 2006, 0.88 in 2007, and 12.6 in 2008 (BiOp Table C-1). Thus inclusion of 2007 pulled the average down from 10.45 (using only 2006 and 2008) to 7.25. Incidentally, 1994 was another year for which data were excluded from Figure B-13 and that year had the lowest CSI value (0.33) reported over the period from 1993 – 2008. Fundamentally, an average calculated from three data points would generally not be considered to be very reliable, and especially so if the variation among the three values is high. Intuitively. we have very little confidence in such an average, and this intuition would be backed up by statistical calculations. However, removing one of three years leaves only two from which to calculate an average, and that also seems like a weak basis for such an important value. Perhaps the take estimate could be based on the longer data series and integrate a metric for water clarity in a multivariate relationship rather than as a simple function of the FMWT trawl index times a fixed value (i.e., 7.25 at present). I note that 1994 had the highest FMWT trawl index value of the 1993 – 2008 period but the adult salvage was less than one tenth the average (359 vs. 4335). This seems consistent with the idea that water clarity affects the relationship between trawl catches and salvage, and is encouraging from the standpoint of developing an adjustment factor.

The BiOp goes on to state (P. 289) that "The Service has largely followed the methodology for estimating incidental take of larval delta smelt similar to that for adults." However, the data from 2005 – 2008 were used, because "… the apparent abundance of delta smelt since 2005 as indexed by the 20-mm Survey and the TNS is the lowest on record." Deriso (Document #401) correctly pointed out that the juvenile salvage index in 2006 was anomalously low (0.4 vs. 23.4 in 2005, 65.1 in 2007 and 60.9 in 2008), so inclusion of that year's data point pulled down the average. He further pointed out that the only other years with similarly low juvenile salvage index values (0.2 in 1995 and 0.8 in 1998) had positive OMR flows at the most important months, as did 2006. My view of the juvenile salvage index and take values is along the same lines as my view of the adult data. Calculating an average from a small number of values (especially if they differ markedly from each other) and then assigning great significance to that average makes me uncomfortable. I am especially uncomfortable if there are other factors that apparently affect the values but data on these factors (e.g., turbidity and flow) are not used to adjust the take levels. I would think a multivariate relationship using more of the data would be more reliable than using a small subset of the data, especially if it leads to disputes over which year to include or exclude. This latter approach seems to allow the addition or removal of a single year to have too much influence. Whether this could be implemented as part of the decision process, however, is uncertain.

Finally, when making judgments about larval smelt salvage it seems critical to remember that the juvenile salvage numbers are very questionable, as was clearly stated in the BiOp (e.g., P. 163, 164). Fish smaller than 20 mm are not even estimated, and the sampling efficiency for those greater than 20 mm is not known. If there is variation among years in the timing of successful reproduction or environmental conditions that influence larval growth, the fraction of the population susceptible to being quantified may vary.

Punt response:

The ideal way to determine limits on removals from a population is to construct a model of the system, select a set of goals (e.g. a rate of recovery or the probability of not declining further), identify a set of candidate management actions, and use the model to determine which set of management actions best achieves the goals and hence the upper limit on removals. This ideal cannot be achieved for delta smelt because no population dynamics model currently exists for delta smelt, and the BiOp does not identify quantitative recovery goals. A problem particular to delta smelt is that total mortality is indexed by means of salvage, but raw salvage is an uncertain measure of mortality (BiOp 383; AR 398).

The incidental take statements reflect average levels of adult and juvenile (>20mm) salvage expressed relative to the FMWT Index (the previous year for adults and the current year for juveniles). The incidental take for adults is cumulative over the year, whereas the incidental take for juveniles is cumulative over the month. The BiOp (BiOp 383; AR 398) notes that "take of adult delta smelt via entrainment will be minimized when OMR flows are limited to -2,000 cfs during the first winter flush when adult smelt move within the zone of entrainment. OMR flows held between -1,250 and -5,000 cfs following the first flush until the onset of spawning will protect later delta smelt migrants and spawners".

I have interpreted that the aim of selecting years to define the incidental takes for adults and juveniles is to allow the incidental take to be reflective of what should happen if the RPA were implemented[6]. Moreover, the level of incidental take is considered by FWS "not likely to result in jeopardy to the species or destruction or adverse modification of critical habitat when the RPA is implemented" (BiOp 293, AR 308). When calculating the incidental takes of adult, the data on salvage / FMWT is restricted to years when FMWT is low (WYs 2006 - 2008). The reason for doing this is, however, not entirely clear because salvage is scaled by FMWT.

[6] The implicitly presupposes that implementing the RPA will achieve the recovery goals, but addressing that issue is beyond the scope of this question.

There are many ways to justify the years selected to obtain the formulae for computing the incidental takes for adults and juveniles. For example, averaging the ratios of adult salvage to FWMT the previous year in Table C-1 for the years indentified in Figure C-1 as having OMR Flow > - 5,000cf leads to 12.03 rather than 7.25. However, the value 12.03 is not statistically different from 7.25. Docs 401 and 508 argue that using the data for 2007 is inappropriate when computing the incidental take formula for adults because 2007 was excluded from Figure B-13, and Doc 401 argues that including the data for 2006 when computing the incidental take formula for juveniles is also inappropriate. However, every year is different from the others in some way. Without a clear link between take and the impact on the population, it is not possible to argue definitely against the set of years chosen by the FWS to compute the incidental take formulae. It is easy to find problems with any given set of years but, unless a better set of years can be proposed and justified, the current set are as scientifically defensible as any other set of years.

7. Please explain the evidence in the record demonstrating that project operations exacerbate the effect/impact of other "stressors" (e.g., toxics)?

Quinn response:
It is entirely plausible that different and apparently unrelated factors can interact in such a manner that they contribute to the decline in productivity or abundance of a given species of interest. Such interactions may make it difficult to determine with conventional statistical analyses which factor, if either, is affecting the population dynamics of the species.

The Sacramento – San Joaquin Delta system has seen many and diverse changes over the period of Euro-American development, including but not limited to changes in physical attributes of the shorelines, vegetation, predators, competitors, zooplankton, filter feeders, pesticides and other chemical contaminants, and changes in flow regime. The changes in flow (e.g., export of water) mean that a given point in the Delta may now experience different regimes of salinity than it did in the past, causing a mismatch between physical habitat and attributes of the water, with respect to smelt.

The BiOp stated (P. 203), "The following analysis assumes that the proposed CVP/SWP operations affect smelt throughout the year either directly through entrainment or indirectly through influences on its food supply and habitat suitability." To the extent that food supply has been altered or limited to the detriment of the smelt, one might expect a decrease in body size over time, or as a function of flow, X2 or some related physical factor. Indeed, there seems to be some evidence to this effect though I did not notice it in the BiOp. Rather, Bennett (2005) presented data credited to Kimmerer in Figure 29 that seem to show smaller average sizes of smelt recently than in earlier years. Given the discussion of shifting zooplankton composition in the BiOp, it is not clear why data on fish size were not presently prominently.

At various places the BiOp mentions the possible effects of contaminants but no conclusive data were reported on their effects, or clear mechanisms by which they might interact with flow-related factors. Bennett's (2005) review also mentioned the possible effects of toxic materials but characterized the effects as "highly uncertain" and noted the difficulties in disentangling effects of contaminants from the many other factors affecting smelt.

In conclusion, there does not appear to be "evidence in the record demonstrating that project operations exacerbate the effect/impact of other 'stressors' ", though such interactions are possible. Without a better sense of the population dynamics of the species and the seasonal and inter-annual shifts in spatial distribution, it is difficult to evaluate the factors contributing to the decline of delta smelt, and hindering their recovery.

Punt response:
It has been postulated that the delta smelt is impacted by a variety of stressors, in addition to the direct effects of entrainment by the project. The impacts of the project on the location of X2 (Figures E-19) may have indirect effects on the population dynamics of delta smelt through changes in habitat. A number of factors are listed in the BiOp (pg 182-188; AR 197-203) which, in addition to entrainment, could impact the population size of delta smelt over the long term.

- Colonization of the interior Delta by submerged aquatic vegetation (specifically *Egeria densa*).
- Increased predation due to increased water transparency.
- Increased predation due to the presence of delta smelt in the Clifton Court Forebay (CCF).
- Competition between delta smelt and introduced fish species that share habitat with delta smelt (such as inland silversides; Bennett (2005)).
- Changes in prey species and their abundance. A change in the dominant prey items of delta smelt has occurred. The dominant prey item in the diet between 1988 and 1996 (when data were available) was *Pseudodiaptomis forbesi*, an introduced copepod. The BiOp notes that preliminary information suggests that the abundance of this copepod has declined (BiOp pg. 185; AR 200; see also AR 10651). Miller (2007) [AR 19703] showed that there is statistical evidence that the co-occurrence of delta smelt and *Pseudodiaptomis forbesi* is positively related to FMWT.
- Effects of *Microcystis* blooms on the abundance of the prey of delta smelt.
- The effects of contaminants and disease.
- The impact of climate change. Climate change does not, *per se*, impact population size, but rather impacts other factors (such as habitat, temperature) which themselves impact population dynamics.

None of these factors have definitely been related to changes in the population size of delta smelt, although it is plausible that some of the factors are responsible in part for the decline of delta smelt. Only one of the factors is specifically argued to be likely related to the impact of the project. Specifically, the BiOp notes "during periods of high exports, much of the lower trophic level production is entrained rather than dispersed downstream to Suisan Bay" (BiOp pg. 185; AR 200). However, the BiOp does not show this quantitatively and does not show that such entrainment would have impacts on delta smelt at the population level. However, this is a plausible hypothesis that warrants further analysis. The BiOp also notes the "big mama" hypothesis, whereby large fish may contribute disproportionally to future recruitment to the population. Entrainment of such fish would likely have larger effects than entrainment of younger, less fecund, animals.

The BiOp also notes (pg. 237; AR 237) that the indirect effects of the project may "(a) contribute to higher water toxicity, (b) contribute to the potential suppression of phytoplankton production by ammonia entering the system from wastewater treatment plants, (c) increase the reproductive success of overbite clams allowing them to establish year-round populations further east (d) increase the frequency with which delta smelt encounter unscreened agricultural irrigation diversions in the Delta". Again, these are plausible hypotheses, but there are no direct data available to test them.

Table 1. Differences between modeled CALSIM II and measured (USGS) flows in cfs for December through January. Negative differences mean that CALSIM II produced greater reverse/upstream flow.

WY	DEC	JAN	FEB	MAR
1987		-1452	-3147	-3559
1988	-2323	-456	6117	5306
1989	-60	4258	5658	-1843
1990	477	3211	6094	5567
1991	570	-400	918	90
1992	254	4475	-3566	3824
1993	-5986	1008	-1292	-3366
1994	555	-875	-4347	512
1995	-2666	546	-4294	-5150
1996	-6706	-445	-3452	-7913
1997	-3566	5559	-18370	-6331
1998	-878	-4348	-8074	-10204
1999	-9040	-5282	-4473	-3667
2000	-4430	-535	2277	-4866
2001	-2646	-1925	-3098	-2456
2002	-509	42	1920	526
2003	-2554	-465	3007	2325

Table 2. Differences between modeled CALSIM II and actual (Dayflow) X2 in km. Negative differences mean that CALSIM II produced more westerly/downstream X2 locations

WY	SEP	OCT	NOV	DEC	WY	SEP	OCT	NOV	DEC
1956	9.8	12.8	18.0	37.4	**1980**	7.4	10.8	17.5	18.1
1957	7.1	10.2	13.7	15.9	**1981**	1.6	8.9	10.7	15.0
1958	8.2	18.4	14.8	17.0	**1982**	15.9	6.1	26.2	13.1
1959	7.7	4.7	9.1	14.2	**1983**	2.4	6.4	6.6	4.6
1960	3.1	6.9	8.1	11.2	**1984**	11.2	1.1	17.5	7.5
1961	5.2	7.8	18.4	18.9	**1985**	-3.6	11.6	21.7	4.0
1962	5.2	6.5	13.9	16.7	**1986**	8.6	-1.1	10.1	4.2
1963	8.0	21.9	-3.0	9.7	**1987**	-6.4	12.7	11.6	13.8
1964	8.4	14.9	23.9	7.5	**1988**	-3.6	2.5	6.4	8.7
1965	10.9	12.3	20.2	33.8	**1989**	-0.2	3.2	11.9	9.1
1966	4.1	15.5	24.0	9.0	**1990**	-4.9	0.7	7.4	5.2
1967	5.8	8.9	23.5	28.9	**1991**	2.0	4.3	9.4	9.1
1968	4.4	6.1	9.5	17.1	**1992**	1.5	3.8	7.6	13.1
1969	16.4	7.2	16.2	24.7	**1993**	0.9	7.4	7.1	9.0
1970	11.1	10.5	11.7	26.5	**1994**	3.5	2.7	9.7	14.0
1971	13.5	17.8	27.0	22.8	**1995**	4.6	0.8	7.6	9.0
1972	10.4	12.5	15.6	23.5	**1996**	1.1	-3.5	3.7	19.5
1973	8.2	15.7	25.0	15.2	**1997**	-3.1	-0.9	8.4	27.1
1974	12.5	17.2	32.8	2.4	**1998**	1.0	4.8	15.3	13.1
1975	9.3	11.8	15.6	19.7	**1999**	-0.6	-6.0	5.8	8.6
1976	3.2	11.8	8.9	10.8	**2000**	-1.3	-2.4	4.3	7.4
1977	-0.7	4.3	6.3	4.8	**2001**	0.8	4.5	3.9	5.8
1978	8.8	-0.6	3.3	11.6	**2002**	0.5	6.0	13.2	23.6
1979	0.6	11.2	15.7	12.8	**2003**	-7.1	4.3	7.3	19.2