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15 UNITED STATES DISTRICT COURT
16 EASTERN DISTRICT OF CALIFORNIA

17
18 THE DELTA SMELT CASES,
19 SAN LUIS & DELTA-MENDOTA WATER
AUTHORITY, *et al.* v. SALAZAR, *et al.*
20 (Case No. 1:09-cv-407)

21 STATE WATER CONTRACTORS v. SALAZAR,
et al. (Case No. 1:09-cv-422)

22 COALITION FOR A SUSTAINABLE DELTA,
23 *et al.* v. UNITED STATES FISH AND WILDLIFE
SERVICE, *et al.* (Case No. 1:09-cv-480)

24 METROPOLITAN WATER DISTRICT v.
25 UNITED STATES FISH AND WILDLIFE
SERVICE, *et al.* (Case No. 1:09-cv-631)

26 STEWART & JASPER ORCHARDS, *et al.* v.
27 UNITED STATES FISH AND WILDLIFE
SERVICE, *et al.* (Case No. 1:09-cv-892)

1:09-cv-407 OWW GSA
1:09-cv-422 OWW GSA
1:09-cv-631 OWW GSA
1:09-cv-892 OWW GSA
PARTIALLY CONSOLIDATED
WITH: 1:09-cv-480 OWW GSA

**REPLY DECLARATION OF DR.
RICHARD B. DERISO IN
SUPPORT OF MOTION FOR
INTERIM
RELIEF/PRELIMINARY
INJUNCTION**

Date: January 20, 2010
Time: 9:00 a.m.
Ctrm: 3
Judge: Hon. Oliver W. Wanger

1 I, Dr. Richard B. Deriso, declare:

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21 **I. INTRODUCTION**

22 1. I have reviewed the Federal Defendants’ Opposition to Plaintiffs’ Motion for

23 Interim Relief/Preliminary Injunction, Docket #469 (“Fed. Def. Opp.”), the Declaration of Cay

24 Collette Goude in support thereof, Docket #470 (“Goude Decl.”), and the Defendant-Intervenors’

25 Opposition to Plaintiffs’ Motion for Interim Relief/Preliminary Injunction, Docket #473 (“Def.

26 Int. Opp.”), filed on December 29, 2009. I am also aware of other declarations filed later by

27 Defendants in this case, including the Declaration of Ken B. Newman, Docket #484. My

28 understanding is that those subsequent declarations were not filed in opposition to the Motion for

Interim Relief/Preliminary Injunction. Therefore, in this reply declaration for the motion, my

comments herein are addressed solely to the Opposition briefs and the Goude Declaration.

1 **II. ENTRAINMENT DOES NOT AFFECT THE DELTA SMELT POPULATION**
2 **GROWTH RATE**

3 2. In my previous declarations, I used standard principles of quantitative fish
4 population dynamics to explain that salvage and Old and Middle River (“OMR”) flows do not
5 have a statistically significant effect on population growth rate. This conclusion applies to both
6 winter OMR flows and spring OMR flows.

7 3. Federal Defendants’ criticisms of my methodology are inaccurate. They are based
8 on mischaracterizations of both my work and several studies cited in the 2008 Biological Opinion
9 (“BiOp”). Defendants misuse and misquote these studies in an attempt to find support for the
10 flawed conclusion in the BiOp that certain salvage and OMR flows are likely to jeopardize the
11 continued existence of the delta smelt. Defendants do not point to any quantitative statistical
12 analysis that would support those conclusions. A review of the studies reveals that they do not
13 provide support for Defendants’ position. Further, they do not negate my prior work showing that
14 salvage and OMR flows do not have a statistically significant effect on population growth rate.

15 **A. The Studies Cited by Defendants Do Not Support the Conclusion That**
16 **Entrainment Affects the Population Growth Rate**

17 4. On page 18 of the Federal Defendants’ Opposition, several studies are cited for the
18 proposition that there are “statistically significant” effects to the smelt population from
19 entrainment. Fed. Def. Opp. at 18:2-28. These studies are:

- 20 • Manly and Chotkowski (2006)
21 • The Interagency Ecological Program’s 2007 Synthesis Report
22 • Kimmerer (2008)
23 • Rose (2000)

24 A review of those studies shows that they have been misrepresented by Federal Defendants.

25 **1. Manly and Chotkowski (2006)**

26 5. Defendants make the false assertion that Manly and Chotkowski (2006) “found a
27 statistically significant relationship between exports and smelt abundance as measured by Fall
28 Midwater Trawl (“FMWT”) catches.” Fed. Def. Opp. at 18:2-4; *see also* Def. Int. Opp. at 28:11-

1 14. Manly and Chotkowski drew no such conclusion. Indeed, Dr. Manly in his declaration stated
2 flatly that the above-quoted statement “is incorrect.” *See* Docket #489 at 2:19-3:28. Manly and
3 Chotkowski were not testing for the statistical significance of exports, or any other hydrology
4 variable, on abundance. Rather, their 2006 study is a methods paper. In it, the authors attempted
5 to improve the ability to detect when regime changes have occurred. They define “regime
6 change” as a change in the functional relationship between estimated abundance and the
7 underlying model. *See* Docket #489 at 3:9-10 (“the focus of the article was not about the reasons
8 for the recent decline in delta smelt numbers”). Defendants mischaracterize how exports are used
9 in the paper. Exports are not used as a single variable, tested for significance. Rather, exports are
10 incorporated into the models as one part of a multi-part variable used as a measure of gross
11 hydrology.

12 6. In the paper’s first application, the measure of hydrology is a quadratic polynomial
13 of a gross hydrology variable defined as average daily flow for the Sacramento and San Joaquin
14 Rivers minus flow from other Delta rivers from January to September each year. The paper finds
15 that expected values in the first regime change analysis for models with and without variables for
16 gross hydrology are very similar after 1980. *See* Administrative Record (“AR”) at 019681. In
17 the paper’s second application, the authors also add a quadratic polynomial of gross hydrology as
18 a variable. Unlike the first application, they do not discuss whether the expected values for
19 models with and without variables for gross hydrology look similar. However, a review of the
20 models in the study, and particularly Figure 4, shows that they are similar. *See* AR at 019683.

21 7. In his declaration, Dr. Manly himself explained the results of the 2006 study as
22 follows: “gross hydrology did not appear to have an effect on delta smelt subsequent abundance.
23 Instead, in this and other work I did preceding this 2006 article, predictions of delta smelt
24 abundances from the models used were almost the same whether hydrological variables,
25 including exports, were in the models or not.” Docket #489 at 3:25-28.

26 8. At bottom, the paper does not provide any statistical analysis to determine whether
27 exports themselves are statistically significant. Defendants’ reliance on this methods paper to
28

1 somehow support their conclusions about the significance of export effects on abundance is
2 misplaced.

3 **2. Interagency Ecological Program’s 2007 Synthesis Report on the**
4 **Pelagic Organism Decline**

5 9. Defendants next refer to the Interagency Ecological Program’s 2007 Synthesis
6 Report on the Pelagic Organism Decline (“IEP (2007)”), by asserting:

7 Moreover, the Interagency Ecological Program’s 2007 Synthesis
8 Report on the Pelagic Organism Decline Team stated that “. . .
9 entrainment of adults and larvae (top-down effects) are particularly
important to the delta smelt population”

10 Fed. Def. Opp. at 18:4-7. The use of ellipses by the Defendants is rather disturbing. Without the
11 use of ellipses, the report actually states:

12 *We hypothesize that* entrainment of adults and larvae (top-down
13 effects) are particularly important to the delta smelt population

14 AR at 0016957 (emphasis added). Defendants appear to have excerpted the quote to state a
15 hypothesis as a conclusion. A hypothesis is commonly defined as “a *proposed* explanation for an
16 observable phenomenon” that is to be tested—a hypothesis is not a scientifically established
17 result. *See, e.g.*, Wikipedia, <http://en.wikipedia.org>. To the extent the Federal Defendants’
18 Opposition uses the report as support for the flow restrictions in the BiOp, that use of nothing
19 more than a “hypothesis” is misplaced.¹

20
21
22 ¹ The misuse of a hypothesis for ecological purposes was specifically addressed in the
23 published journal piece, Elner, R.W. and R.L. Vadas, Sr., *Inference in Ecology: the Sea Urchin*
24 *Phenomenon in the Northwestern Atlantic*, *The American Naturalist*, Vol. 136., No. 1 (July
25 1990). The authors explained: “there appears a need for more rigor in conducting and
26 interpreting ecological research. We contend that, because of the lack of rigor and guidelines, the
27 tendency to rely on ‘weak’ inference and ‘common sense’ is pervasive, resulting in a propensity
28 to cling uncritically to ‘pet’ concepts rather than to test multiple hypotheses (Chamberlin 1897).
Such a soft approach has promoted circular reasoning, aided in the development of paradigms,
and retarded ecological discovery.” *Id.* at 108. That circular reasoning is evident, for example,
by FWS’s reliance on the Kimmerer (2008) paper—Kimmerer simply assumed a relationship
between OMR flows and salvage, but then FWS uses Kimmerer as a basis for justifying the
imposition of flow restrictions to protect the smelt.

1 **3. Kimmerer (2008)**

2 10. Defendants next refer to Kimmerer’s 2008 study. *See* Fed. Def. Opp. at 18:8-
3 20:15; *see also* Def. Int. Opp. at 28:8-11, 14-17. This study requires some explanation.

4 11. In Kimmerer’s paper, he created estimates of entrainment of delta smelt in order
5 “to place these losses in a population context.” AR at 018854. The estimates were generated and
6 then compared to population abundance (using survey results from the corresponding year) to
7 come up with a proportion of the population lost to entrainment.

8 12. In order to develop his entrainment estimates, Kimmerer developed equations that
9 include OMR flows as a variable—that is to say, his estimates build in a correlation between
10 OMR flows and entrainment. Kimmerer’s study did not set out to test whether such a correlation
11 exists (or, more importantly, whether OMR flows impact the population from one year to the
12 next). Thus, a critical element that should frame any discussion of Kimmerer (2008) is that he
13 *assumed* a relationship between OMR flows and abundance, which is stated plainly in the study
14 itself: “Despite the lack of evidence for population-level effects, a strong influence of the south
15 Delta export facilities on populations of estuarine and anadromous fish has been *assumed* for
16 several reasons.” *See* AR at 018855 (emphasis added). Defendants ignore this basic assumption
17 that underlies Kimmerer’s work, and make a series of mischaracterizations that are revealed
18 through a close review of the 2008 study.

19 13. Kimmerer repeatedly explains that the influence of export pumping on the
20 population is an assumption. For his analysis of adult smelt, he explains: “Principal assumptions
21 were: . . . Entrainment is proportional to the combined southward flow in Old and Middle River
22 flows.” *See* AR at 018865. The same applies for juveniles: “Principal assumptions for
23 calculating daily loss for each survey were: . . . The relevant flow toward the export facilities is
24 the southward flow in Old and Middle Rivers.” AR at 018868. Thus, for both adults and
25 larvae/juveniles, Kimmerer assumes a proportional relationship.

26 14. The analysis of the data contained in my declaration, however, tests this
27 assumption, and can be found at Docket #401 at 24-26, Docket #455 at 7. The data shows that
28 the assumption Kimmerer made is not statistically supported for flows below -6,100 cfs with

1 respect to adults, and is not statistically supported at all for larvae/juveniles. Docket #401 at 24-
2 25; Docket #455 at 7. Indeed, Kimmerer even seems to recognize this himself—one of the three
3 “reasons” Kimmerer gives for making this assumption has more to do with practical
4 considerations than scientific analysis. He states that “manipulations of flow patterns in the Delta
5 provide the only apparent tool for managing some fish populations such as delta smelt.” AR at
6 018855.

7 15. After making these key assumptions, Kimmerer then estimates the effects of
8 pumping on delta smelt “mechanistically” instead of through a correlative analysis—i.e., he has
9 assumed the existence of a mechanism, such as negative OMR flows causing entrainment, then he
10 calculates the loss by deriving an equation based on that mechanism.

11 16. Relying on Kimmerer’s estimates for determining where to set flows, as FWS has
12 in the BiOp, is fraught with problems because the estimates are derived using the assumption that
13 flows and entrainment are correlated. Kimmerer acknowledges this for larvae and juvenile
14 estimates when he states, “The variation in annual loss was related to flow conditions ($PI = -0.4 +$
15 $(1.7 \pm 0.6) Q_{sd}$, $r^2 = 0.79$, 9 df), but this relationship is *tautological*, since *Old and Middle River*
16 *flow was used explicitly in the calculations.*” AR at 018875-018876 (emphasis added). Here
17 again, Kimmerer bases his study on a foundational assumption—one that is ignored by
18 Defendants.

19 17. This also creates problems with any X2 analysis, as Kimmerer states, “The
20 relationship of proportional loss to Old and Middle River flow (by assumption) and inflow and
21 export flow (Figure 16) guarantees a relationship with X2.” AR at 018876.

22 18. The Defendants (and to some extent Kimmerer) then suggest that the inability to
23 show an effect on the population growth rate is due to variability in the population between
24 summer and fall, and that the effects are likely masked by this high variation. These arguments
25 about the effect of pumping being masked, however, are belied by the results Kimmerer reaches
26 regarding food supply. He observes that, “The summer-fall index of survival varied over a range
27 of 50-fold, and was significantly related to summer zooplankton biomass in the low-salinity zone
28 (Figure 17). This may indicate food-limited survival.” AR at 018877. If high variability was

1 masking the effects of the pumps, one would expect it to mask any other significant sources of
2 impacts to the population. However, because Kimmerer is able to detect a significant relationship
3 between adult abundance and zooplankton (smelt survival and zooplankton biomass are strongly
4 correlated), the argument about masked effects by other causes—such as pumping—is
5 circumspect. If high variability does not mask significant sources of impacts such as food
6 availability, one could certainly conclude that the variability is only masking insignificant
7 sources. Thus, if pumping were significant, it would be reflected by an analysis of the data just as
8 food availability is.

9 19. Kimmerer (2008) should also be read in light of its final conclusions—ones that
10 succumb to practical considerations: “Management of delta smelt should incorporate any
11 opportunities that arise to improve habitat or food supply and to reduce any negative impacts of
12 predation or toxic contamination. However, current evidence does not provide a clear path
13 toward improving the status of the delta smelt using these factors. Manipulating export flow
14 (and, to some extent, inflow) is the only means to influence the abundance of delta smelt that is
15 both feasible and supported by the current body of evidence, even though export effects are
16 relatively small.” AR at 018878. This “practical” conclusion is best informed by Kimmerer’s
17 repeated acknowledgments that “no effect of export flow on subsequent midwater trawl
18 abundance is evident,” and that “[i]f this variability is uncorrelated with entrainment losses, then
19 these losses will contribute little to the variability in fall abundance index,” and that there is a
20 “lack of evidence for population-level effects.” AR at 018855, 018878.

21 20. Defendants also seem to imply that Kimmerer has determined that the analysis of
22 correlative relationships is improper. *See* Fed. Def. Opp. at 18:8-15, 18-21. Defendants fail to
23 acknowledge, however, that Kimmerer conducted a correlative analysis in his own 2008 study.
24 *See* AR at 018877 (Figure 17). Not only does he conduct this analysis for survival and biomass,
25 but he finds that they are “significantly related.” *Id.* Thus, Kimmerer (2008) does not support
26 Defendants’ sweeping assertion that conducting a correlative analysis is not the best available
27 science; Kimmerer in fact performs one himself.

28

1 21. Defendants also ignore other key findings in Kimmerer’s study that undermine the
2 BiOp’s reliance on this work to justify flow restrictions. For estimates of adult delta smelt losses,
3 Kimmerer qualifies that the highest value of 50 percent “may have been biased upward.” AR at
4 018854. Overall, he concludes that the effect of losses on population abundance “was obscured”
5 by “subsequent 50-fold variability in survival from summer to fall”—meaning, ultimately, that
6 any effect is an undetected assumption. *See* AR at 018854.

7 22. In sum, Defendants improperly rely on Kimmerer (2008) to try and support their
8 position that entrainment effects are significant and that exports should be controlled to reduce
9 losses. Defendants misuse Kimmerer by failing to recognize the several assumptions and
10 tautologies that are built into the study and expressly acknowledged by Kimmerer. Kimmerer’s
11 assumptions cannot be used to counter the quantitative statistical analysis I conducted in my
12 previous declarations, because I used the actual data, rather than making assumptions about
13 significance.

14 **4. Rose (2000)**

15 23. Defendants next misuse Rose (2000), characterizing Rose’s work as using “several
16 individual-based models to show how multiple interacting stressors can result in fish population
17 declines that would not be readily discernible using linear regression-based approaches.” Fed.
18 Def. Opp. at 18:28-19:2. Rose, however, does not show that conventional fisheries population
19 dynamics models—such as the Ricker model—would fail to detect the impacts of stressors on
20 population declines. In fact, Rose argues that the time series regression that he applied in one of
21 his examples is appropriate. AR at 020016. That is the only example where such a regression
22 was done in Rose (2000), and the results are not a basis for rejecting time series analysis as a
23 useful tool for fisheries population analysis.²

24 24. Defendants then characterize the content of my previous declarations as a “narrow
25 statistical approach” in comparison to Rose. *See* Fed. Def. Opp. at 19:2. In my previous
26 declarations, I applied a nonlinear Ricker stock-recruitment model with multiple candidate

27 ² It should also be noted that Rose (2000) was not a study involving delta smelt.
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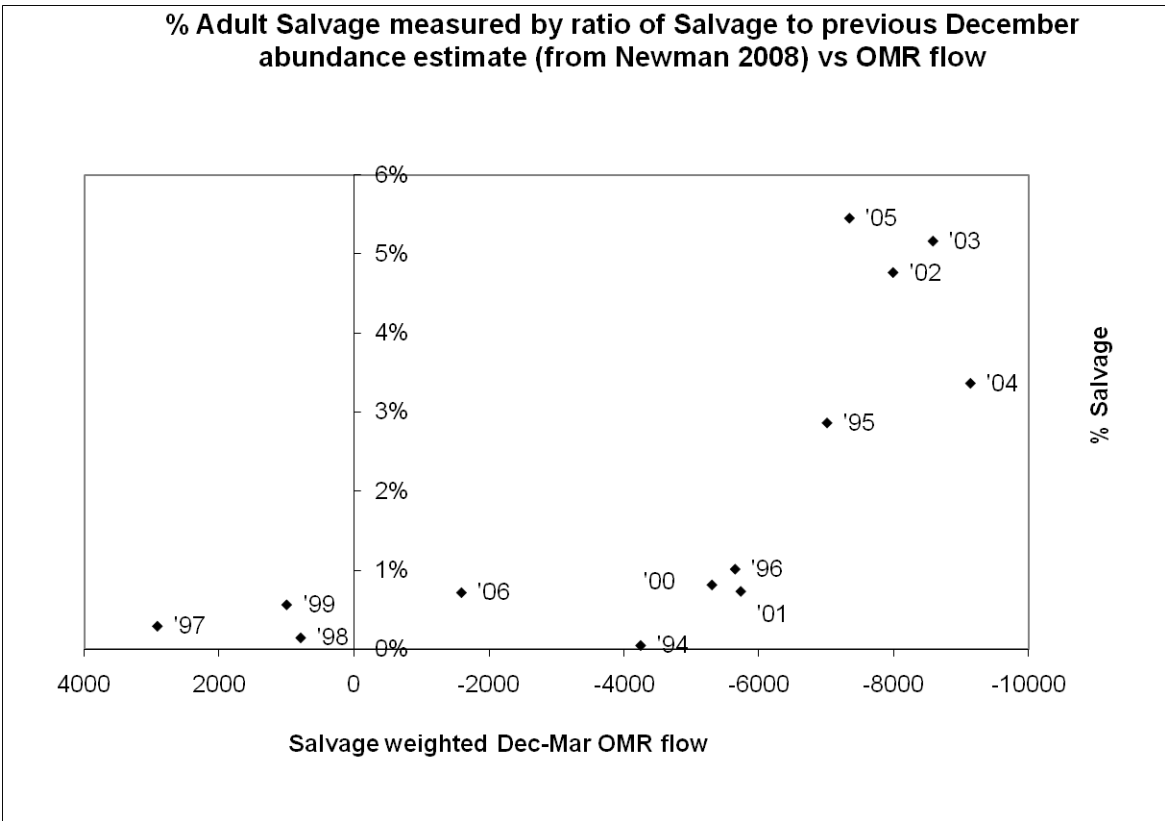
1 variables. It is a mischaracterization to call this a straightforward correlative analysis. This type
2 of analysis is commonly used, and should have been employed by FWS in developing the BiOp,
3 as I explained in my prior declarations. In fact, the approaches and methodologies I presented are
4 similar to what Rose used in his subsequent 2008 study. It should also be noted that I have been
5 working with others to conduct a full life stage model which is preliminarily confirming the
6 results I reached in my previous work—namely, that OMR flows do not have a statistically
7 significant effect on population growth rate.

8 **5. Family Farm Alliance v. Salazar**

9 25. There is a fifth piece referenced in the Opposition papers, even though the paper
10 was put together after the BiOp was issued. This paper is titled an “Independent Peer Review” of
11 the BiOp conducted by PBS&J as part of the U.S. Fish and Wildlife Service’s (“FWS”) response
12 to the Family Farm Alliance’s Information Quality Act Appeal (“FFA Peer Review”), which is
13 cited in the Fed. Def. Opp. at 19 n. 7 and the Def. Int. Opp. at 20 n. 13, 21:5-9, 30:15-22.

14 26. Table 1 in the FFA Peer Review contains data for winter salvage, population
15 estimates, and salvage as a percentage of total population for the years 1994-2006 (each year
16 includes data from the prior months leading up to March, such that salvage from December 1993
17 to March 1994 is listed as 1994). I plotted a curve comparing the latter variable—salvage as a
18 percentage of total population—against salvage weighted December-March average OMR flow
19 (taken from Figure B-13 in the BiOp at 348 (AR at 000363)).
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27. The following data, with the abundance estimates from the FFA Peer Review, was used for the above figure:

Year	Salvage weighted average Dec-Mar OMR	Salvage	December abundance estimate (in 1000s) (from Newman 2008)	% adult salvage
1994	-4242.011186	447	866	0.05%
1995	-7006.202454	2608	91	2.87%
1996	-5642.794285	5634	554	1.02%
1997	2909.257659	1828	618	0.30%
1998	784.0915287	1027	691	0.15%
1999	992.4281581	2074	366	0.57%
2000	-5298.918733	11493	1405	0.82%
2001	-5726.576409	8003	1087	0.74%
2002	-7984.825346	6865	144	4.77%
2003	-8576.809158	14305	277	5.16%
2004	-9132.953117	8148	242	3.37%
2005	-7338.401388	2018	37	5.45%
2006	-1599.518519	324	45	0.72%

1 28. As can be seen in the figure above, all of the higher salvage percentages occurred
2 with average OMR flows more negative than -7,000 cfs. Thus, even the data used by the FFA
3 Peer Review supports the same conclusions I reached previously, namely, that OMR flows do not
4 have a significant effect on adult smelt until flows become very negative.

5 29. I also wish to note that there appear to be many other problems in the FFA Peer
6 Review, but I have limited my discussion here to only what was raised in the Oppositions. *See*
7 *Fed. Def. Opp.* at 19 n. 7; *Def. Int. Opp.* at 20 n. 13, 21:5-9, 30:15-22.

8 **B. There Are No Significant “Episodic” Effects From Entrainment**

9 30. Beyond the general misuse of the studies and pieces described above, the
10 Oppositions also suggest that the lack of effects to the population growth rate somehow overlooks
11 “episodic” effects. *See Fed. Def. Opp.* at 18:18-22; *Def. Int. Opp.* at 28:19-27. For this
12 “episodic” theory, Defendants again cite to Kimmerer (2008). Defendants rely on this theory as
13 support for the reasonable and prudent alternatives (“RPAs”) using the erroneous assumption that
14 even though year over year trends show no impact to population, the sporadic occurrence of a
15 large salvage count in a given year could still somehow harm the population and therefore justify
16 the OMR flow restrictions. *See Fed. Def. Opp.* at 18:18-19:7.

17 31. The problem with Defendants’ assertion is that even an “episodic” effect should be
18 reflected in the population growth rate if it has somehow impacted the population. If the episodes
19 of large entrainment were significant, they would appear as such in a statistical model testing the
20 significance of entrainment. However, because the data shows that OMR flows do not have such
21 an effect, the “episodic” argument is not sustainable.

22 32. An explanation of Kimmerer’s analysis of larvae/juveniles is illustrative. The
23 annual percent loss of larvae/juveniles is presented in Figure 15 of Kimmerer’s paper, which I
24 digitized and then plotted against March-June average OMR flow. As would be expected given
25 his assumptions, a significant correlation exists between Kimmerer’s estimates of entrainment
26 percent loss and negative OMR flow ($R = -0.83$, $P\text{-value} = 0.005$). Kimmerer’s assumptions
27 drive the entire estimation procedure for the spring—for example, at flows of approximately
28 -4,800 cfs, more than 25 percent of the population is estimated by Kimmerer to be lost. Thus, an

1 “episodic” event translates merely into episodes of highly negative OMR flows in the spring. But
2 as I have shown in my application of the Ricker model, such events do not have a significant
3 detectable impact on the population growth rate.

4 **C. The Lack of a Spatial Distribution Variable Is Not a Valid Critique**

5 33. Defendants’ criticisms of the absence of a spatial distribution variable in my
6 previous declarations also have no merit. *See* Fed. Def. Opp. at 20:5-15. Like the argument
7 about “episodic” effects, one would still expect to see an impact to the population growth rate
8 regardless of whether spatial distribution was an important consideration. And as I have
9 explained, there is no such effect.

10 34. This spatial distribution argument is also a red herring in that Defendants ignore
11 the analysis in the BiOp itself at Figure B-13, which does not account for spatial distribution.
12 With regard to adults, the BiOp, in its formulation of RPA Component 1, develops flow
13 guidelines that are apparently based on the relationship of salvage to OMR flows. *See* BiOp at
14 348, 350 (Figures B-13 and B-14) (AR at 000363, 000365). Those graphs and any results based
15 on those graphs do not explicitly consider the spatial distribution of delta smelt. Rather, those
16 graphs offer the single “explanatory” variable of salvage weighted December-March average
17 OMR flow. In my previous work, I used the same data from Figures B-13 and B-14 in the BiOp
18 to show that the cumulative salvage index and OMR flows do not have a statistically significant
19 effect on the population growth rate.

20 35. With regard to larvae and juveniles, the BiOp’s discussion is based largely on
21 Kimmerer (2008). On this point, it is worth noting that Kimmerer estimates entrainment losses of
22 larvae/juveniles with a method that takes into account explicitly the spatial distribution of delta
23 smelt as measured by the spring 20-mm survey. His equations use nearfield density terms,
24 incorporating the number of fish caught specifically in the south Delta. AR at 018866-018868.

25 36. As I described previously, Kimmerer’s estimation method also relies heavily on
26 the assumption that the larvae/juvenile population percent daily loss is itself proportional to OMR
27 flow if the flow is negative. *See* AR at 018868 (Equation 19). That assumption has such a strong
28 influence on the overall results that Kimmerer writes, “this relationship is tautological, since Old

1 and Middle River flow was used explicitly in the calculations.” AR at 018875-018876. With
2 such a dominant assumption, additional information on spatial distribution of delta smelt is
3 simply not needed. All that was required was to evaluate spring salvage versus OMR flow, which
4 did not provide statistical support for the assumption that entrainment losses (as measured by an
5 index of salvage rates (salvage/20-mm survey index)) have a statistically significant relationship
6 to OMR flow. Moreover, spring OMR flow did not have detectable impacts on the population
7 growth rate. The spatial distribution argument, therefore, is again confounded by the lack of
8 population growth rate effects.

9 **D. The Data Used in My Prior Declarations Was Based on FWS’s Freedom of**
10 **Information Act Response and Analyses Set Forth in My Technical**
11 **Appendices**

12 37. Defendants’ criticism that my prior submissions were “unaccompanied by the raw
13 data purportedly relied upon” is inaccurate. *See* Fed. Def. Opp. at 19:11-13. I displayed tables of
14 the data I used and the analyses I conducted in the technical appendices to my declarations. *See*
15 Docket #401, Appx. 1-1 to 1-18; Docket #455, Appx. 1-1 to 1-2. Much of that was data provided
16 by FWS in its response to a Freedom of Information Act request from the Metropolitan Water
17 District of Southern California and in the BiOp itself. *See* Docket #455 at 1:26-2:1.

18 **E. My Prior Declarations Addressed Squarely Johnson’s “Break-point”**

19 38. Defendants make the false assertion that “Plaintiffs’ extra-record evidence fails to
20 address the statistical analyses that were actually provided in the BO” in reference to the BiOp’s
21 “regression analysis to determine the break-point in the OMR-salvage relationship.” *See* Def. Int.
22 Opp. at 30:11-13; *see also* Fed. Def. Opp. at 12:19-22, Goude Decl. at 12:15-19. To the contrary,
23 the work conducted in my prior declarations explains the analysis in the BiOp and specifically the
24 OMR-salvage relationship and “break-point.” I evaluated FWS’s analysis of that relationship as
25 depicted in Figures B-13 and B-14 of the BiOp. *See* Docket #401 at 16:9-21:10. I explained that
26 the figures were incorrect in that they relied on raw salvage as the quantity to be predicted by
27 OMR flow. As I further explained in my declaration, the appropriate quantity to be predicted by
28 OMR flow is the cumulative salvage index (i.e., an index of the proportion of the population
removed by salvage). Using the cumulative salvage index showed that the appropriate break

1 point is -6,100 cfs, rather than the -1,162 cfs in the erroneously constructed Figure B-14
2 (Johnson’s analysis). Suggesting that I somehow failed to address the statistical analyses, and
3 specifically the “break point” regression analysis, in the BiOp is simply wrong.

4 **III. THE ADULT AND JUVENILE INCIDENTAL TAKE STATEMENT IS NOT**
5 **STATISTICALLY VALID**

6 39. Finally, Defendants claim that the use of an unrepresentative data point in
7 calculating the Incidental Take Statement (“ITS”)—even they refer to it as “imperfect data” (Fed.
8 Def. Opp. at 30:23)—was nevertheless appropriate. The errors FWS committed in calculating the
9 ITS fall outside the range of scientific reasonableness, and they are not entitled to deference. As I
10 described in my prior declarations, to calculate the ITS for adults, FWS averaged the salvage rate
11 from three years—2006, 2007, 2008—which it chose because the salvage data from those years
12 “best approximate[d] expected salvage under the RPA Component 1.” BiOp at 386 (AR at
13 000401). Yet the model upon which RPA Component 1 is based (Figure B-13) excludes the year
14 2007 because salvage was unrepresentatively low that year due to low turbidity. BiOp at 348
15 (AR at 000363). For FWS to make use of the 2007 salvage data in calculating the ITS because it
16 “best approximate[d] expected salvage under the RPA Component 1,” after earlier deciding that
17 the exact same salvage data could not be used to calculate flow levels for RPA Component 1, was
18 per se unreasonable and cannot be entitled to deference. FWS failed to adhere to the basic
19 scientific principle that data should be used consistently throughout all parts of an analysis. *See*
20 Docket #401 at 6:17-20.

21 40. Similarly, FWS included an unrepresentative year in the juvenile ITS. To
22 calculate the juvenile ITS, FWS followed the same methodology that it used for adults in
23 choosing representative salvage years—2005-2008. The year 2006 had extremely low salvage
24 due to *positive* average OMR flows. FWS included 2006 despite its earlier assertion that
25 “positive OMR is usually associated with no, or very low, entrainment”—making years with
26 positive OMR flow unrepresentative for purposes of calculating the ITS. *See* BiOp at 163 (AR at
27 000178).

1 I declare under penalty of perjury under the laws of the State of California and the United
2 States that the foregoing is true and correct and that this declaration was executed on January 26,
3 2010 at Del Mar, California.

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7 DR. RICHARD B. DERISO

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CERTIFICATE OF SERVICE

I hereby certify that on January 26, 2010, I electronically filed the foregoing with the Court by using the Court's CM/ECF system.

Participants in the case who are registered CM/ECF users will be served by the Court's CM/ECF system.

I further certify that the court-appointed experts are not registered CM/ECF users. I have emailed the foregoing document to the following:

REPLY DECLARATION OF DR. RICHARD B. DERISO IN SUPPORT OF MOTION FOR INTERIM RELIEF/PRELIMINARY INJUNCTION

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I declare under penalty of perjury under the laws of the State of California the foregoing is true and correct and that this declaration was executed on January 26, 2010, at San Francisco, California.

/s/ Catherine L. Berté
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