EXHIBIT WRINT-DFG 31

Rebuttal Testimony of

the Department of Fish and Game

July 27, 1992

REBUTTAL TO CROSS-EXAMINATION ON RELIABILITY OF STRIPED BASS MODEL IN WRINT-DFG-Exhibit 3

Several points were raised during cross-examination of Department of Fish and Game witnesses on the striped bass model which require additional clarification and response. The most fundamental point concerns the value of the model in predicting the consequences of future changes in operations of the water projects. The model can best be used to evaluate these changes if they involve flows and exports and do not make major alterations in the historic (1959-1989) monthly hydrology or physical configuration of the delta. If substantial changes in monthly hydrology or flow patterns occur, the model can still be used to project impacts on adult striped bass, but with less reliability. Evaluating model results in this situation is the provence of scientists familiar with the life history and population dynamics of striped bass in the delta and also familiar with delta hydrology. In this context, the results from the JSA Fisheries Model (WRINT-DELTAWET-15 [Revised]) might yield additional insight.

Some have questioned the statistical validity of the DFG model or have asked for more review by other statisticians or biometricians. In fact, this regression approach has already received substantial review and criticism from a number of individuals and agencies, including the Department of Water Resources, U.S. Bureau of Reclamation, Hanson Environmental for the State Water Contractors, Ecological Analysts for the Modesto and Turlock Irrigation Districts, Jones and Stokes for the Yuba County Water Agency and Delta Wetlands, and the three independent statisticians whose comments were included as part of WRINT-DFG-Exhibit 3. We responded to all written comments and incorporated many of the suggestions into the present draft of the model. No other model or means of evaluating the impact of delta hydrology has so carefully presented the basis for its structure and conclusions or received such a thorough review. Calls for additional study and review (and most other statistical objections) appear to be tactics to either delay adoption of the model as a tool in setting interim standards to protect striped bass or to cloud the issue and paralyze the process.

The precision of the model in predicting adult striped bass abundance was questioned during cross-examination and the nature of the confidence interval shown in Figure 6 on page 15 of DFG-Exhibit 3 was discussed. The unsubstantiated suggestion was made to us that the prediction interval might be "1000% wider". To literally "set the record straight", Figure 1 is a revision of the above-mentioned Figure 6 with the 95% prediction interval added, showing that the prediction interval averages about three times as wide as the confidence interval and encompasses all but one of the observed values.

A question was also raised about verification of the model. As all available data were used to calculate the regressions, no "verification" data set existed. However, we have addressed this verification problem, using the regression of adult abundance on weighted mean yoy index and weighted mean log(loss rate) 3-7 years earlier, in two ways:

1. The data set was randomly split into roughly equal-sized subsets. One subset was used to calculate a regression relationship between adults and weighted mean yoy and log(loss rate) and this regression equation was then used to estimate adults from weighted mean yoy and log(loss rate) in the other subset. This process was repeated 100 times, yielding approximately 50 adult abundance estimates for each year (each year was randomly included in the regression equation about one-half of the time and it was in the prediction subset the remainder of the time) . The mean $\pm$ 2 standard deviations of these estimated values is shown in Figure 2 and demonstrates that the predictions of adult abundance in the verification data subset were reasonably accurate and precise.

2. Jackknife estimation was used to predict adult abundance for each year from a regression relationship computed from all years except the year being predicted. This also demonstrated the ability of this equation in DFG's model to predict adult abundance reliably for years not included in calculating the regression relationship (Figure 3).

Based on the above results regarding precision of the estimates and validation of the regression equations, we conclude that concerns expressed by other parties in the Interim Water Rights Process are unjustified.

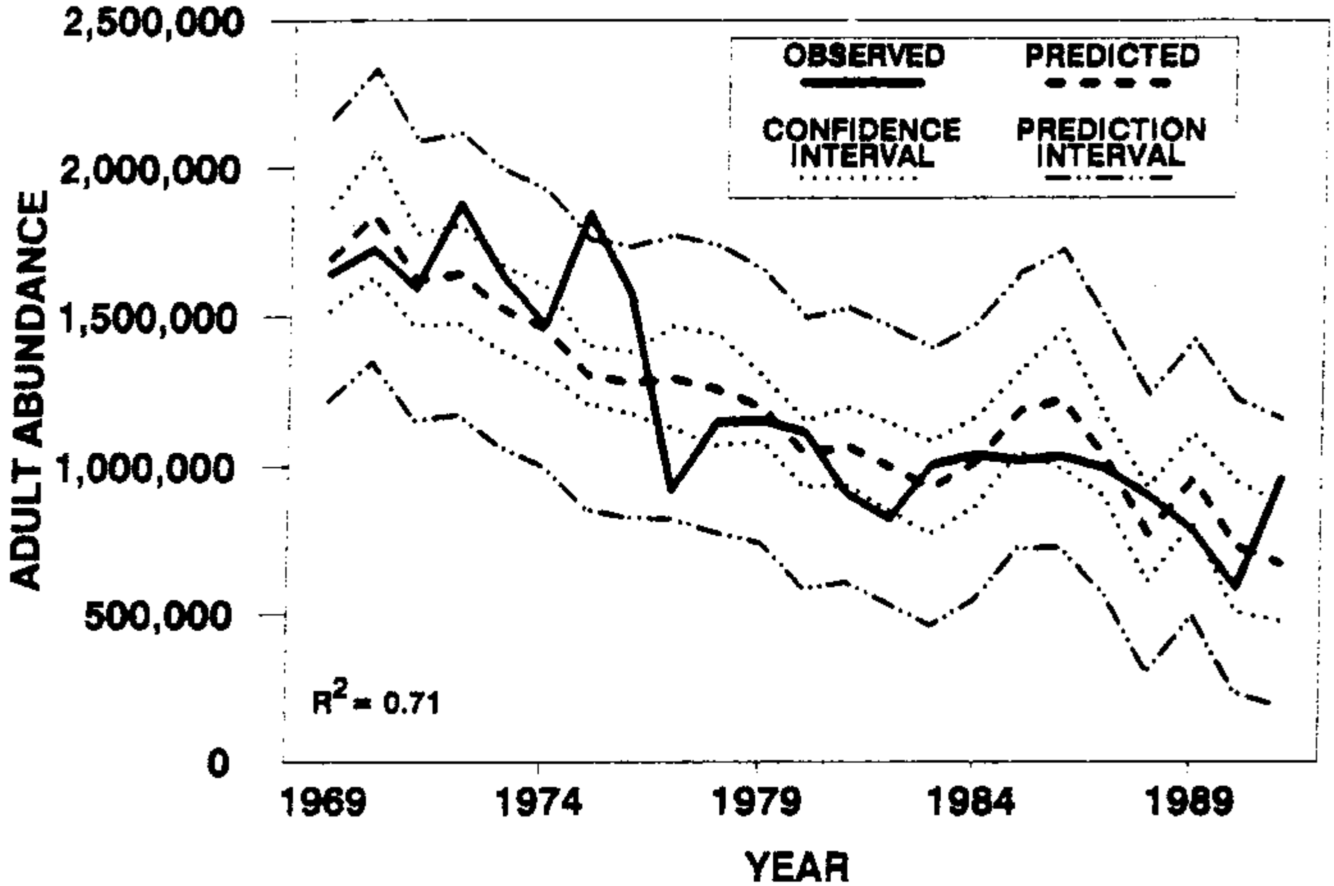

Figure 1. Observed and predicted adult striped bass abundance in the Sacramento-San Joaquin Estuary from 1969-1991. Predicted values are from the relationship between adult abundance and weighted mean young-of-the-year index and export loss rate 3-7 years earlier. The 95% confidence interval and 95% prediction interval are shown.

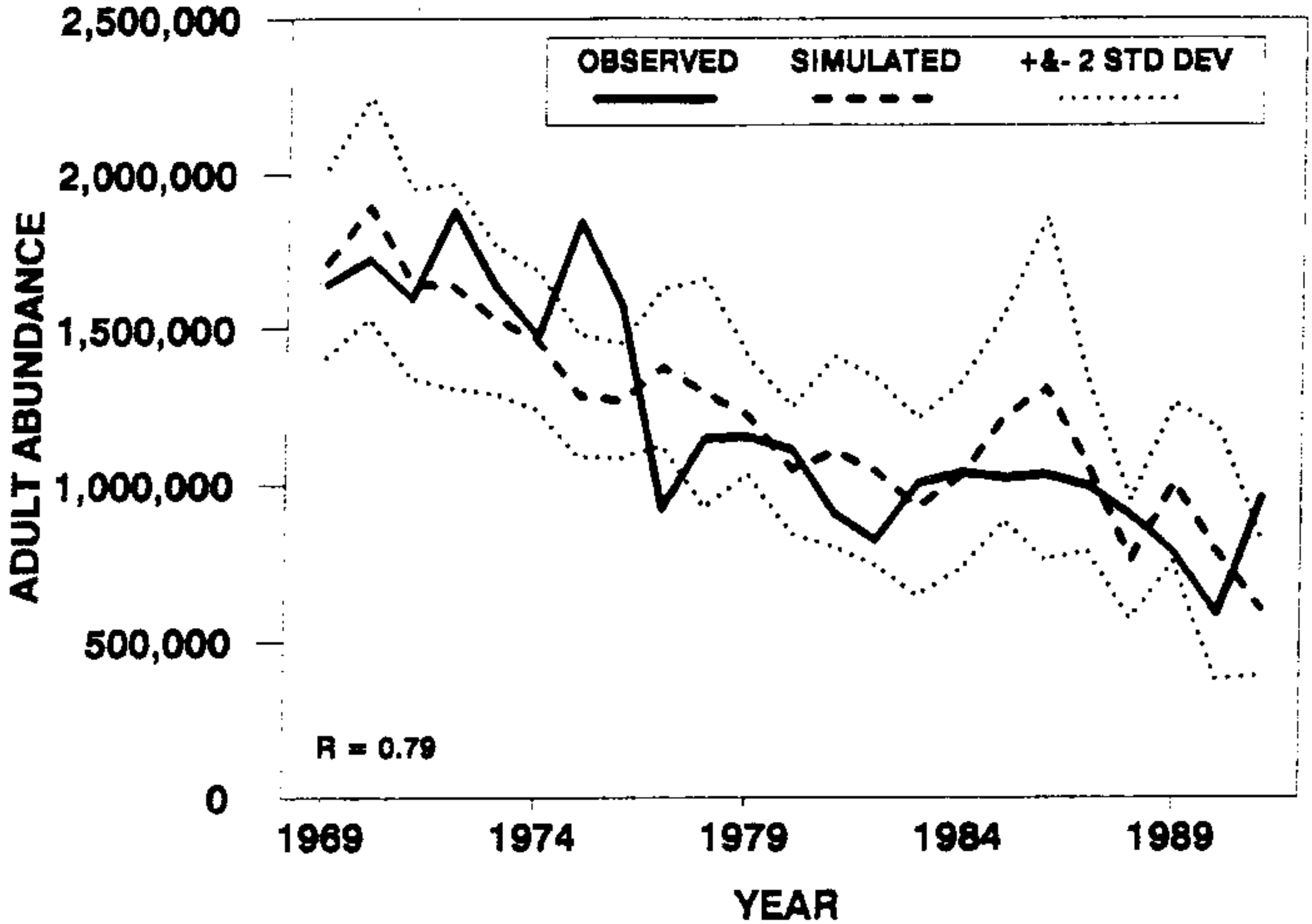

Figure 2. Observed and simulated adult striped bass abundance in the Sacramento-San Joaquin Estuary from 1969-1991. The data set for the entire period of record was randomly split and one subset was used to calculate the regression relationship of adult abundance on weighted mean yoy index and weighted mean log(loss rate) 3-7 years earlier. This regression equation was used to estimate adult abundance for the other subset. Results shown are the mean estimated adult abundance ± 2 standard deviations from 100 repetitions of this process.

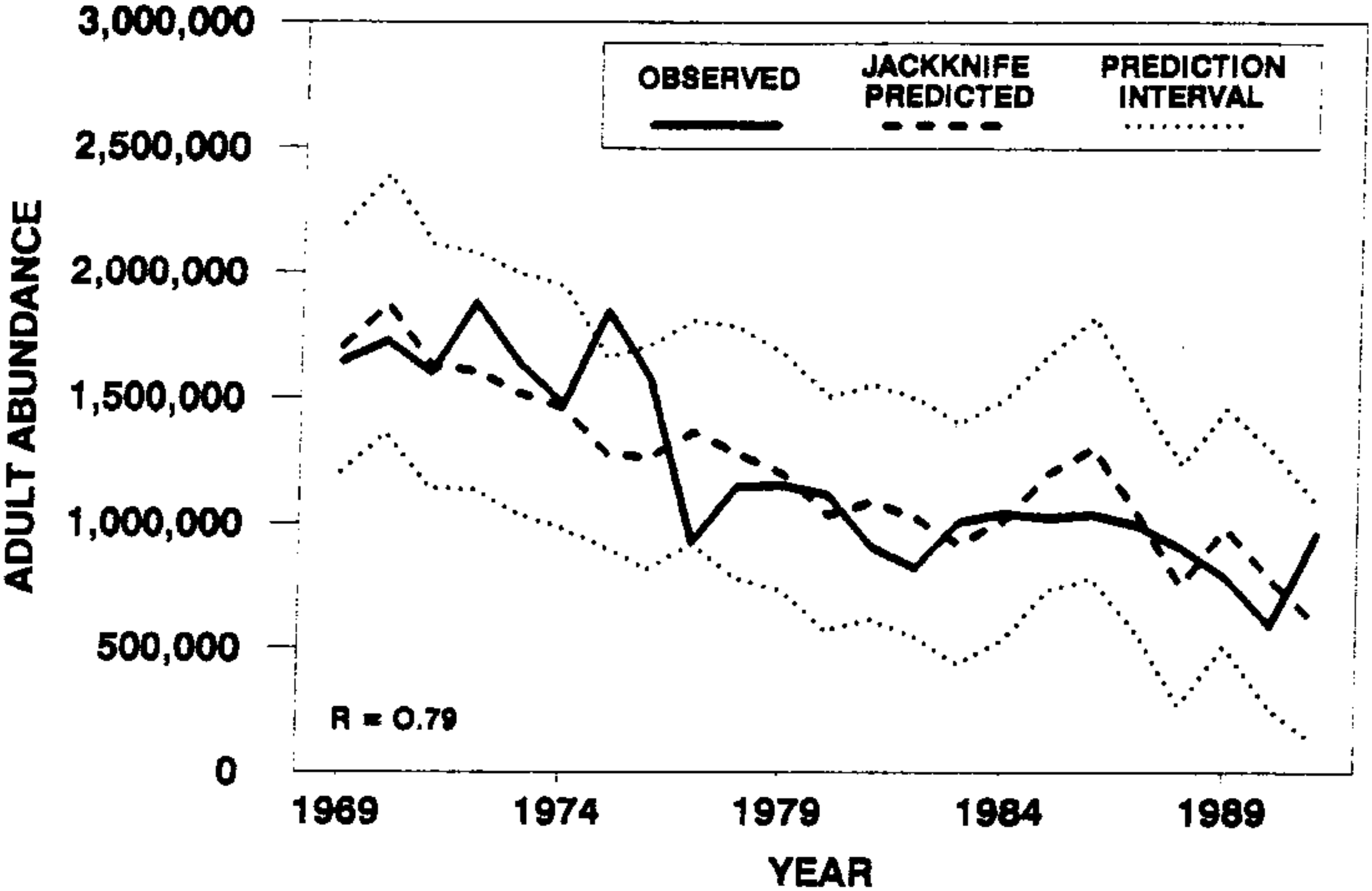

Figure 3. Observed and simulated adult striped bass abundance in the Sacramento-San Joaquin Estuary from 1969-1991. Adult abundance was predicted from a regression equation of adults on weighted mean yoy and weighted mean log(loss rate) 3-7 years earlier computed from all years except the year being predicted.

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Page 6, first paragraph states that the average impact to the total number of legal size striped bass would be the equivalent of about 86,000 adults per year. 1) This estimate does not consider that losses of bass smaller than 20 mm are underestimated because the nets used to sample these fish are not 100 percent efficient. Recent evaluations indicate that the probability of capture declines from 1.0 for 6 mm striped bass to 0.82 for 7 mm bass to 0.23 for 10 mm bass and less than 0.10 for bass between 11 and 20 mm in length. Thus, depending on size, losses of these smaller striped bass are underestimated by the reciprocals of these probabilities. 2) Losses in these terms of average impacts to the number of adults per year only apply to the recruitment of new fish. Losses stated in this manner do not consider that the population in any year is also reduced by the number of fish that would have survived from losses to recruitment in previous years. It also does not consider the compounding effect of these losses on egg production of the stock, the resulting reduction in numbers of young fish produced, and in turn, the subsequent reduction of recruitment in later years. WRINT-DFG Exhibits 2 and 3 demonstrate that this process (including losses to both the SWP and CVP) is largely responsible for the reduction in adult striped bass from about 1.7 million fish in the early 1970s to the current (1990) population of about 680,000 fish (about 590,000 naturally produced fish).

Page 12, last paragraph states that "Moyle et al (1992) postulate that diversions from the Delta provide the most likely explanation of declines in delta smelt abundance by shifting the entrapment zone to river channels which presumably results in habitat constriction and fish entrainment at the SWP and CVP pumping facilities and agricultural diversions. This theory is not supported by findings of the Department of Fish and Game (Stevens et al, 1990)". This statement ignores the fact that two Department of Fish and Game (DFG) biologists (D. E. Stevens and L. W. Miller) co-authored Moyle et al. (1992) and that if unmeasured or poorly measured factors have major effects on delta smelt abundance in some years, effects of diversions, reverse flows, and outflows may have been masked in the multiple regression analysis of Stevens et al. (1990). Thus, DFG's findings do not negate the postulation in Moyle et al. (1992). In fact, the DFG believes that this is a reasonable, albeit unproven, postulation.

The delta smelt summer townet and fall midwater trawl monitoring results for 1991 are particularly relevant to this postulation. WRINT-DFG Exhibit 9 (Figure 4) and WRINT-USFWS Exhibit 17A show that the 1991 summer townet index of delta smelt abundance continued at a low level while the fall midwater trawl index was improved over past years suggesting that young delta smelt survived unusually well. WRINT USFWS Exhibit 17A shows that in 1991, CVP/SWP water exports were at their lowest level since 1976; thus, these results are consistent with the postulation that entrainment at the SWP and CVP pumps has a strong influence on delta smelt abundance.

Page 13, delta smelt, paragraph 2 states: "Since 1989, the South Delta Striped Bass Egg and Larval Survey has caught less than 20 larvae each year, usually in April and May (Figure 5)." This statement is true, but to some extent it reflects a low sampling effort. In 1991, the South Delta Striped Bass Egg and Larva Survey did not sample from April 17th through May 27th, a nearly six week period during peak larva abundance (see Figure 5,*). Because delta smelt larvae were caught at higher densities in April and May both in 1989 and 1990, more delta smelt larvae would have been caught in 1991 if sampling had been done during this time period.

The text should also state that based on this sampling of larvae, in 1989 and 1990 estimated entrainment was around a half million fish (WRINT-DWR-30, Table 4).

Page 13, delta smelt, paragraph 3 states: "As indicated by the few larvae caught in the extensive sampling effort (every other day at seven sites from April through July), it appears few Delta smelt spawn in the southern Delta (DWR 1992)."

DWR did not initiate every other day sampling until 1992 when sampling effort was increased from either 2-3 weekdays/week to every other day sampling to conform with DFG's Delta-wide egg and larva sampling program. The total number of days that DWR sampled in front of Clifton Court Forebay (CCF) are as follows:

Year	Sampling Period	Sampling Days	Total Days Sample Period	%DaysSampled
1989	April 10-July 17	36	99	36.3%
1990	April 10-July 10	34	92	36.9%
1991	Feb 4-July 12 (no sampling April 17-May 27)	34	160	21.3%

The problem with this sampling program is that daily larval entrainment estimates must be must be extrapolated over longer periods of time, which reduces the accuracy of the entrainment estimates.

Page 14, delta smelt, Table 4. The 1991 estimate that 24,085 delta smelt larvae wereentrained between February 4 and July 12th is misleading. As we have already pointed out, sampling did not occur from April 17 through May 27, the approximate time of peak larva abundance in 1989 and 1990. The entrainment estimate is based on catches of delta smelt larvae on only one day in June at station 92 in front of CCF. WRINT-DWR 30, Table 4 further misrepresents average larval delta smelt densities in 1991 when the entrainment estimate for that one day in June is divided by the total SWP inflow from Feb 4 through July 12th (421,998 Ac-ft). There is no mention, or any attempt to compensate for the fact that no sampling was done from April 17th-May 27th when 40% of the total SWP inflow for the study period was taken into CCF (170,965 Ac-ft) and when larval densities of delta smelt would have been higher based on 1989 and 1990 densities.

Table 4 reports that total SWP inflow for Feb 4 through July 12 was 421,998 Ac-ft, although exports reported from daily export reports at the SWP for the same period were 880,839 Ac-ft. This disparity should be explained.

Page 15, American shad. Paragraph 3 states that there is not information available on predation rate and screen efficiencies from which to calculate losses from salvage estimates. While that is true, it should be recognized that the "salvage" of young American shad at the CVP/SWP fish screens typically is in hundreds of thousands and has ranged up to more than 8 million fish annually (DFG Exhibit 23 in the 1987 proceedings), that young American shad are an extremely fragile fish and that a substantial portion of the "salvaged" fish die in handling and trucking. Additionally, there would be losses before the "salvage" process. Considering that such losses have significantly reduced the striped bass population, it seems likely that the American shad population has also been reduced by entrainment losses.

Page 23 discusses the work of Tsai et al. (1991) which suggested that reduction in sewage treatment lowered fertility of striped bass nursery regions in the Potomac River Estuary and contributed to the recent decline of striped bass in Chesapeake Bay. Page 23 also suggests the same problem could occur for the Sacramento-San Joaquin Estuary. The SWRCB should be aware that an independent evaluation of this conclusion in the Emergency Striped Bass Research Study Report for 1989 (published by US Fish and Wildlife Service and National Marine Fisheries Service, April 1992) pointed out that "the results of Tsai et al (1991) however, fail to explain declines of striped bass in Chesapeake tributaries that were not subjected to comparable changes in nutrient loading (e.g., Nanticoke and Choptank rivers)." They state that "application of the sewage hypothesis to explain the decline of Chesapeake Bay striped bass would require evidence of comparable and simultaneous changes in sewage treatment practices." (page 4 of ESBRS, 1992)

This Emergency Striped Bass Research Study Report also concluded that "exploitation was excessive and undoubtedly contributed to the decline" (page 1), and that due to evidence and concerns that the spawning stock was too low, remedial steps have consisted primarily of reducing exploitation (overfishing) to allow the stock to rebuild.

Page 26, paragraph 5 (Errata) states that larvae may be receiving less than optimum rations. The only studies that have directly addressed this issue (stomach analysis by DFG and condition studies by Bennett and Hinton at UC Davis) have provided no evidence in support of this statement. To the contrary, the condition of all larvae collected from the Estuary and histologically examined at UC Davis has been as good or better than that of larvae fed in the laboratory. Furthermore, there is no evidence supporting the contention that starving larvae quickly die so they are not collected in the Estuary.

Page 31-33, Pollutants and Toxicity. As discussed in WRINT-DFG-Exhibit 2, Howard Bailey's hypothesis that rice pesticides are responsible for the decline in young striped bass abundance should not be given credence.

A problem with any statistical analysis relating variables that have time trends such as the downward trend in striped bass abundance and the upward trend in chemical usage is that there is danger of finding fortuitous correlations that do not represent cause and effect. Therefore, the statistical relationships should not be the end point of the analysis. Results must be evaluated within the context of intuitive reasoning and the biology and population dynamics of Sacramento-San Joaquin striped bass. Within this context, we note four important distinctions between Bailey's chemical models and DFG's model. All distinctions favor the DFG model.

1. All available data on Sacramento-San Joaquin striped bass population dynamics are consistent with the DFG model including the absence of a persistent reduction in

survival between the egg and 6 mm stage in the Sacramento River (WRINT-DFG, Figure 6) and between the egg and 38 mm stage for the entire Estuary (WRINT-DFG Exhibit 2, Figure 19) other than that accounted for by outflows and diversions. The chemical models make sense only if survival decreases. This requirement is inconsistent with the striped bass population data.

Furthermore, consistent with the absence of a persistent reduction in survival, the striped bass egg supply and young striped bass abundance have declined similarly since 1977 (eggs have declined due to decline in adults). Averages compared for 1969-1976 with 1977-1990 indicate young striped bass abundance has declined 67 percent while egg supply has declined 60 percent. If increased toxicity had substantially reduced survival of this reduced egg supply and its progeny (as postulated by Bailey), the average abundance of young striped bass would have had to decline even more precipitously than it has.

2. The DFG model is consistent with information on effects of outflows and water diversions over the entire period of record extending from 1959 to 1991. The amounts of variation in young bass abundance accounted for by the DFG model are similar during various subsets of years (Tables 1,2 and 3). The chemical models are based on only a portion of this period and account for more variable amounts of variability in young bass abundance depending on which subset of years and combinations of chemicals are included, supporting our contention that the results are fortuitous. The difference between the DFG model results in Tables 2 and 3 ($r^2 = 0.55$ and 0.65) and the 16 and 43 percentages ($r^2 = 0.16$ and 0.43) cited in WRINT-DWR-30 is that the 16 and 43 percentages are based on the 1959-76 DFG model which does not consider the importance of egg production.

3. The instream concentration indices used in the chemical models are not based on instream measurements. Thus, the estimated instream concentrations do not reflect the chemical degradation that occurs before the pesticides are discharged to the river. This point is important because major changes in rice field water management have been implemented to increase chemical degradation and reduce potential toxic effects of these chemicals. It is the toxicity of the discharge, not the amount of the chemicals applied, that potentially affects fish in the Sacramento River. For any given amount applied, the toxicity of the discharge is now less than it used to be, yet there has not been a rebound in young striped

bass abundance consistent with this reduction in discharge (WRINT-DFG Exhibit 2).

4. The mechanisms included in the DFG model are based on documented observations and measurements. Tests have shown that fish pass through the CVP and SWP screens, that fish die due to handling and trucking, that fewer fish survive when stocked near the entrance to Clifton Court forebay than when stocked near the screens, and that predators in the forebay consume small striped bass.

   The chemical models require supposition. First, DFG laboratory tests show no evidence that Colusa Basin drain water is toxic to striped bass, a result which conflicts with the UC Davis study. Second, even if the drain waters are toxic at times, that toxicity is diluted when the drains discharge to the river where the striped bass occur. Testing of river water by the DFG Aquatic Toxicology Laboratory has not provided evidence of toxicity to striped bass larvae (WRINT-DFG Exhibit 2, Figure 24). Furthermore, to account for the post-1976 decline in young striped bass abundance to about 30 percent of its previous average level, toxic exposure must be sufficient to kill more than the entire population of the roughly 55 percent of the population that spawns in the Sacramento River. Sampling by ourselves, the US Bureau of Reclamation, and State Water Contractors shows that numerous live striped bass eggs and larvae still occur in the Sacramento River. This postulated loss to toxicity also is inconsistent with the fact that the decline of young bass has been greatest in the Delta (WRINT-DFG Exhibit 2, Figure 13) where the CVP and SWP diversions are located and where eggs and larvae experience less exposure to rice field pesticides.

Page 33, second full paragraph states that from 1986 through 1991, the predicted and actual 38 mm index again became significantly correlated (Bailey 1992), apparently suggesting that when holding times for chemicals increased, the striped bass problem disappeared and the situation returned to that before the mid-1970s. Clearly, this correlation is misleading and that conclusion is not valid. Although the predicted and actual values (based on the 1959-1976 model) are highly correlated for these years, that correlation occurs only because the predicted and actual values for 1986 are relatively high while the predicted and actual values for 1987-1991 are lower. The correlation does not reveal that predicted values for 1987-1991 are considerably greater than the actual values (Table 4) as in all years since 1977, except 1986 (A higher actual value for

1986 is consistent with both the 1959-1976 and the WRINT-DFG Exhibit 3 models). When the present DFG model (WRINT-DFG Exhibit 3) is used to predict values for 1986-1991, the correlation between predicted and actual values also is high and there is considerably better agreement between predicted and actual values as reflected by a mean actual - predicted residual index of only -0.5 for the WRINT-DFG Exhibit 3 model vs a mean residual index of -23.4 for the 1959-1976 model (Table 4).

In essence, a high correlation between predicted and actual values for 1986-1991 is to be expected no matter which DFG model is used (1959-1976 vs WRINT-DFG Exhibit 3) simply because both would predict higher values in high outflow , low water export years than in low flow, high export years. Actual index values follow that tendency both before and after 1977, but at lower levels after 1977. This is demonstrated by a high correlation (r = 0.75) between 1959-1976 model predictions and actual values for all years from 1977 to 1991. Yet, there is general agreement among informed scientists that despite that correlation, the 1959-1976 model is lacking an important factor (WRINT-DFG Exhibit 3 demonstrates that this is egg production) because acual values consistently fall below its predictions.

Pages 33-34, Striped bass summer dieoff. While this annual dieoff is of concern, it should be recognized that it is not related to the decline in striped bass abundance. Much anecdotal information is available from people that have lived and worked around the Estuary that this dieoff has occurred for more than 50 years. At least two DFG staff members (Chadwick and Stevens) personally observed the dieoff in the 1950s and 1960s.

Page 34, Sacramento River striped bass liver studies. These studies by D. Hinton and W. Bennett of UC Davis are the same studies that yielded Table 16 in WRINT-DFG Exhibit 2. That table shows that the comparisons between areas are erratic from year to year (For example in 1988, the highest incidence of poor scores was from the San Joaquin River near Antioch), and in our opinion, do not warrant a conclusion that "liver sections of striped bass larvae from the Sacramento River show a much higher incidence of malformation than larvae from elsewhere." Furthermore, none of the "Sacramento River" samples were from the Sacramento River above the Delta (none were taken above the Rio Vista area), and the Collinsville group of stations included sites downstream from the junction of the Sacramento and San Joaquin rivers (W. Bennett).

Table 1. Associations between the Department of Fish and Game's 38 mm young striped bass abundance index and indices of concentration of 6 chemicals, 1970-1988.

Chemical	r^2	Years Used
Carbaryl .	0.23	17
Carbofuran	0.28	18
Methyl parathion	0.35	18
MCPA	0.34	18
Ordram (Molinate)	0.63	18
Bufencarb	0.42	8
DFG's striped bass model 1/	0.65	18

1/ DFG striped bass model r^2 is a DFG result. The chemical r^2's are from Bailey's study.

Table 2. Associations between the Department of Fish and Game's 38 mm young striped bass index and bufencarb, ordram and 3 chemicals individually in combination with bufencarb, 1973-1981.

Chemical	r^2
Bufencarb + Ordram	0.93
Bufencarb + MCPA	0.88
Bufencarb + Carbofuran	0.93
Ordram (Molinate)	0.52
Bufencarb	0.42
DFG's striped bass model 1/	0.55

1/ DFG striped bass model r^2 is a DFG result. The chemical r^2's are from Bailey's study.

Table 3. Associations between the Department of Fish and Game's 38 mm young striped bass index and several combinations of chemicals, 1973-1988.

Chemical	r^2
All six chemicals	0.86
Bufencarb + Ordram	0.76
Bufencarb + MCPA	0.32
Bufencarb + Carbofuran	0.42
Ordram (Molinate)	0.63
DFG's striped bass model[1/]	0.65

1/ DFG striped bass model r^2 is a DFG result. The chemical r^2's are from Bailey's study.

Table 4. Actual and predicted young striped bass abundance indices for 1986-1991.

Year	Actual 38 mm Index	Predicted 38 mm Index from 1959-76 Regression	Residual (Actual - Predicted)	Predicted 38 mm Index from WRINT-DFG Exhibit 3 Model	Residual (Actual - Predicted)
1986	64.9	69.0	-4.1	49.6	15.3
1987	12.6	26.0	-13.4	8.3	4.3
1988	4.6	30.8	-26.2	13.1	-8.5
1989	5.1	36.8	-31.7	7.0	-1.9
1990	4.3	38.8	-34.5	6.4	-2.1
1991	5.5	35.8	-30.3	15.8	-10.3
Mean Residual			-23.4		-0.5
Correlation coefficient between actual index and prediction			0.91	0.96	

DFG REBUTTAL TESTIMONY

Exhibit WRINT DWR-31, page 8 suggests that the present population of predatory-sized striped bass (fork length $\geq$ 180 mm) in Clifton Court Forebay is less than that observed in the mid-1980s. This suggestion is made in partial support of the argument that the Clifton Court Forebay predation (pre-screening) loss rate experienced by winter-run salmon entrained at the SWP in 1992 was less than the 75% used to determine chinook salmon losses for mitigation purposes under the 1986 "Four-Pumps Agreement" between DWR and DFG.

The Exhibit refers to "a very rough estimate" of 30,000 striped bass made in fall of 1991, which was based on an area expansion of seine catches made in the fall of 1991. As part of DWR's and DFG's SWP export facility predator control research program, two attempts were made to estimate predator-sized striped bass abundance in Clifton Court Forebay during the spring of 1992 using standard mark/recapture techniques. These two estimation efforts yielded estimates of approximately 162,000 (95% C.I. ~ 63,000-359,294) and 142,000 (95% C.I. ~ 72,000-275,000). These estimates suggest that the abundance of predator-sized striped bass is similar to or more than the 93,000 estimated to be Clifton Court Forebay in 1984, when the last spring-time estimates were made.

DEPARTMENT OF FISH AND GAME REBUTTAL TO WRINT-DELTAWET-10, 15 (Revised), 16, 17, 18, 26

The JSA Fisheries Model, presented and discussed in the referenced exhibits, may be a useful tool to evaluate the impact on young striped bass of specific changes in delta hydrology or physical configuration that cannot be addressed by the DFG Striped Bass Model. However, we have three significant overall comments on the JSA model:

1. The major conclusion of this model, as inferred from Figures 5, 6, and 9 in Exhibit 15 (Revised) is that water exports from the delta, and resulting entrainment of striped bass eggs and larvae, have an overriding influence on the production of young bass in the estuary. These figures show that up to 80% of each year's striped bass production is often entrained before the end of July and that this percentage is lower in wet years. This is consistent with the conclusion of DFG's model that striped bass abundance is strongly dependent on water exports, but DFG's model takes the analysis a step further by looking at the impact of entrainment after the end of July.

2. Given the similarity of our conclusions and the potential value of the JSA model, it is disappointing that Exhibit 15 (Revised) provides such poor documentation of the JSA Fisheries Model that its validity cannot be evaluated. Specifically, the origin and development of the regression and transport equations are not presented, no initial stock size is given, units are often confusing (e.g. Table 1, page 10, Exhibit 15 - the Delta Cross Channel operations can be either flow in cfs or percent of month open; page 15, Exhibit 15 - units are not given for flow and tidal volume in the adjustment of drag; page 15, Exhibit 15 - SPNSAC2 is summed with SPNSJ, SJSAC, and SJCON, but SPNSAC2 is a proportion and the latter 3 variables are numbers of fish.), and it is never made clear how proportions and actual numbers of fish are integrated to produce estimates of the yoy abundance index. Proper documentation is needed so the methodology can be understood and evaluated.

3. The JSA Fisheries Model deals only with the juvenile life stage of striped bass and not with adults, so it is of limited usefulness in predicting water project impacts on the fishery. The DFG Striped Bass Model deals with mortality after the yoy index is set and provides good estimates of resultant adult abundance. One reason why adult abundance is important in the

model is the evidence that adult abundance limits the production of young.

We have a number of specific comments on WRINT-DELTAWET-15 (Revised):

Page 2, paragraph 2 - The structure of the DFG Striped Bass Model is misrepresented by focussing on only the yoy portion of the model and ignoring that a major purpose is to predict adult abundance. In addition, the second sentence in that paragraph incorrectly implies that the DFG model predicts the yoy index from average April to July outflows and diversions, ignoring the important part that spawning stock plays in the DFG predictions of yoy (see Figures 7, 8, and 9 in WRINT-DFG-Exhibit 3).

Page 2, OVERVIEW, paragraph 3 - A figure is needed to better demonstrate the relationship between export losses and the proportion of export comprised of Sacramento River water.

Page 9, Assumptions - All assumptions used in this version (run) of the model are violated.

- Natural mortality varies between years and between locations and periods of the year.
- Growth is temperature dependent and may also vary with food supply. Spawning time affects the temperatures and food supplies that developing larvae will experience.
- While it is true that eggs and larvae move at a speed different than the water, there is no justification given for the drag factor used in this version of the model and it probably changes as bass become longer. We fail to understand how salinity constrains larval bass transport. The lack of explanation here is an example of the poor documentation and the confusing nature of this entire exhibit.
- Timing, location, and distribution of spawning are year specific. This assumption is generally unnecessary as these data are readily available.

Page 11, Options - We do not understand the reasoning behind nor function of the salinity (EC) limits. It is especially puzzling that the higher limit is at the upstream location.

Page 12, Natural Mortality - What data were used for this equation and how was the equation derived? How well does it fit the data? This is an example of poor documentation.

Page 13, line 5 - If SJR is less than the sum of CVP, SWP, and CCWD, why is SJR set equal to 0? Doesn't that result in overestimates of flow in the lower San Joaquin River (or underestimates of the magnitude of reverse flow)?

Page 13, Transport Rates - What is DTR? How was this equation derived and what is its justification?

Page 15, Drag - To reiterate an earlier comment, what is the justification for a drag coefficient of 0.8? Also, how was the adjustment factor determined? This is another case of poor documentation.

Page 16, Losses - Fish moving into San Francisco Bay (We assume this is really San Pablo Bay, but Figure 1 in the Exhibit does not depict the downstream limit of the Suisun Bay unit.) are not lost and fish moving out of the other units are not necessarily "losses".

Like many of the equations given without background, this one to calculate losses is confusing. In the explanation of the terms of the equation, MTR is described as the monthly transport rate for a given loss, but how can you know the loss before it is calculated?

Page 17, RESULTS, paragraph 1 - The 1967-1991 period is referred to as the historical period and this time frame was defended in cross-examination as the only period for which EC's at Benicia and Pittsburg were available. In fact, yoy indices are available for all but two years between 1959 and 1992 and this whole period of record should be used to verify the model, simulating EC's if they, in fact, are not available. Including the period before the SWP came on line in the late 1960s might be very informative.

Page 17, Calibration and Verification - This misrepresents the correlation between observed yoy and yoy predicted by the appropriate equations in the DFG model (found on pages 26 and 27 of WRINT-DFG-Exhibit 3); the correct correlation for the years 1969-1991 (when all necessary data, including spawning stock, are available) is 0.85. A comparison of the accuracy with which the DFG Striped Bass Model and the JSA Fisheries Model predict yoy abundance is shown in Figure 4 of this rebuttal. From this figure, the reason for the difference between the correlation of 0.85 for the DFG model and only 0.62 for the JSA model is readily apparent.

Since model development is not described, it is impossible to tell whether calibration has used the same data (yoy index) that is now being predicted. That doesn't invalidate the JSA model, but it is just another piece in a puzzle which needs to be fitted together.

Page 20, Figure 9 - We do not understand how the DFG yoy prediction equations were used to calculate losses to entrainment. This needs to be explained.

WRINT-DELTAWET-16 includes Jones and Stokes' comments and criticisms of an earlier version of the DFG Striped Bass Model. Rather than offer extensive rebuttal to these comments here, we refer to WRINT-DELTAWET-17, which contains DFG responses.

During cross-examination, Mr. Shaul referred to a figure based on DFG's model relationship between yoy abundance and outflow, which he submitted as WRINT-DELTAWET-26. The purpose and value of DELTAWET-26 is unclear, other than to demonstrate the obvious point that the relationship between the yoy index and mean April-July outflow is curvilinear and asymptotic; i.e. yoy abundance increases rapidly with April-July outflow up to about 20,000 cfs, after which flow seems to have little effect on young bass abundance (Figure 5 of this rebuttal). Either very high flows do not enhance bass survival or they transport young bass into downstream areas not adequately sampled by DFG's mid-summer tow net survey. Conversely, the relationship between the yoy index and mean April-July export is linear (Figure 6 in this rebuttal) and, even at high outflows, increases in exports have substantial negative effects on yoy abundance.

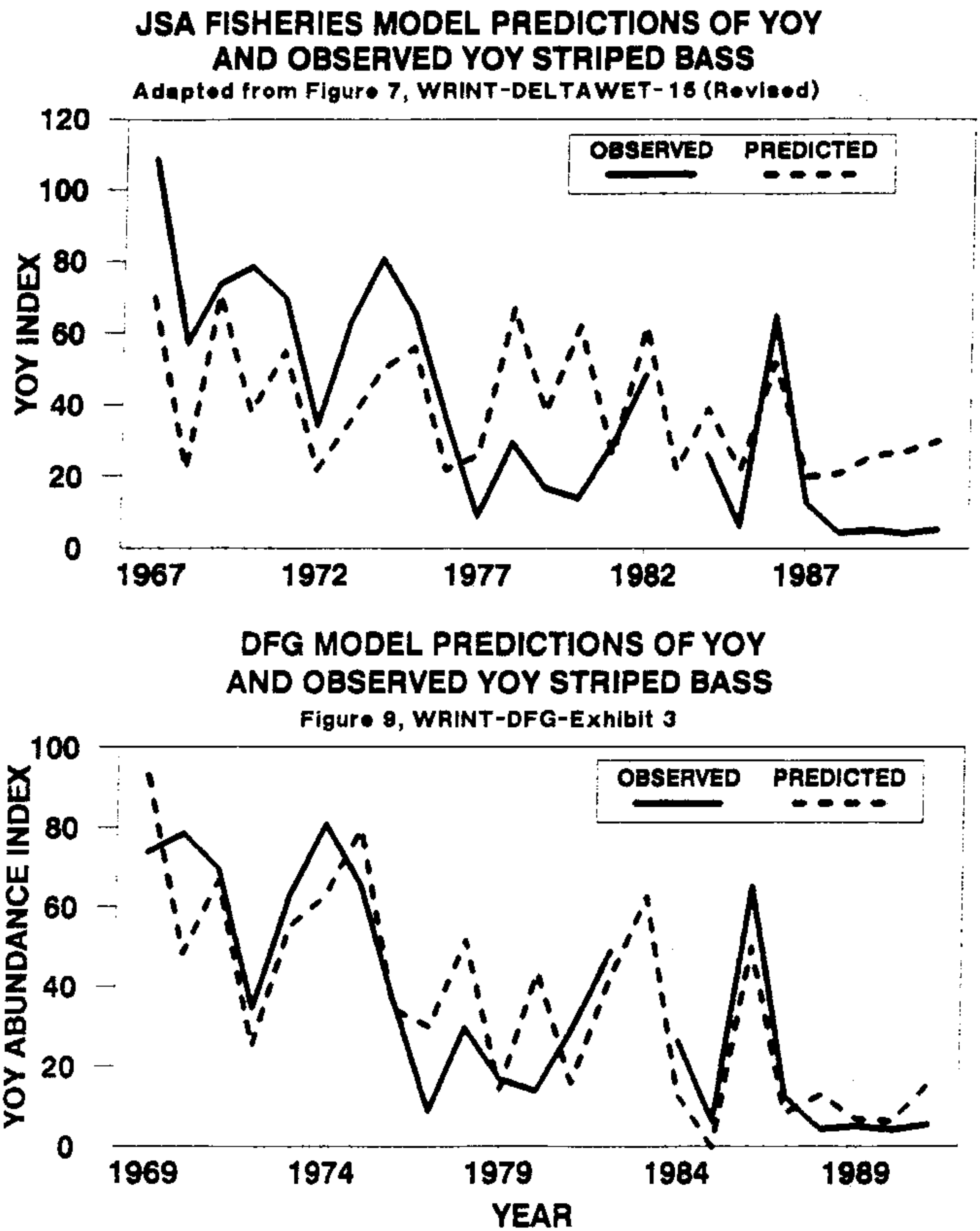

Figure 4. Comparison of the observed young-of-the-year striped bass index and predictions of the index by the JSA Fisheries Model and the DFG Striped Bass Model. The correlation coefficient for observed vs. JSA predicted is 0.62; for observed vs. DFG predicted it is 0.85.

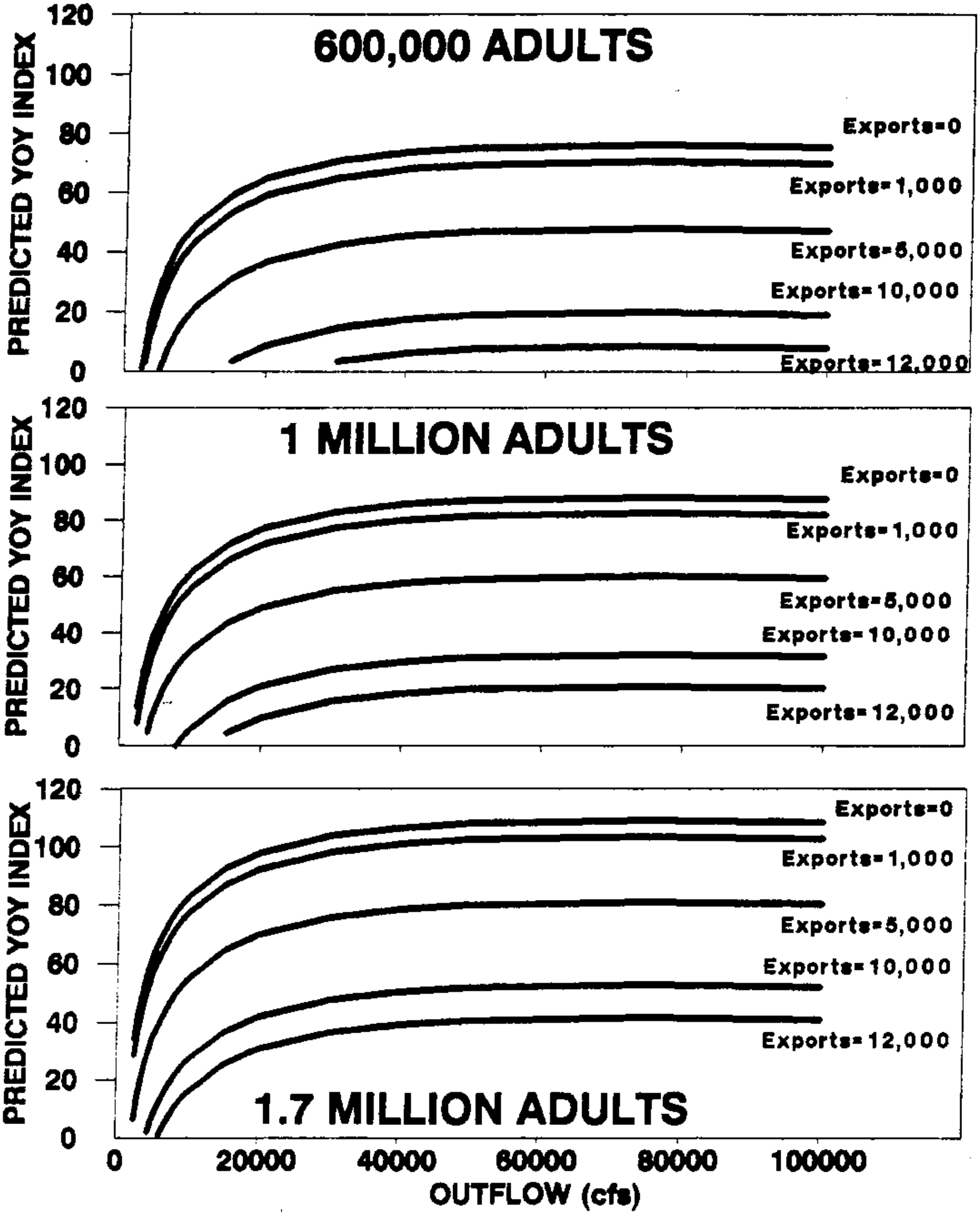

Figure 5. The young-of-the-year striped bass index predicted from mean April-July outflow by the DFG Striped Model at three levels of adult abundance (spawning stock size) and five values of mean April-July exports.

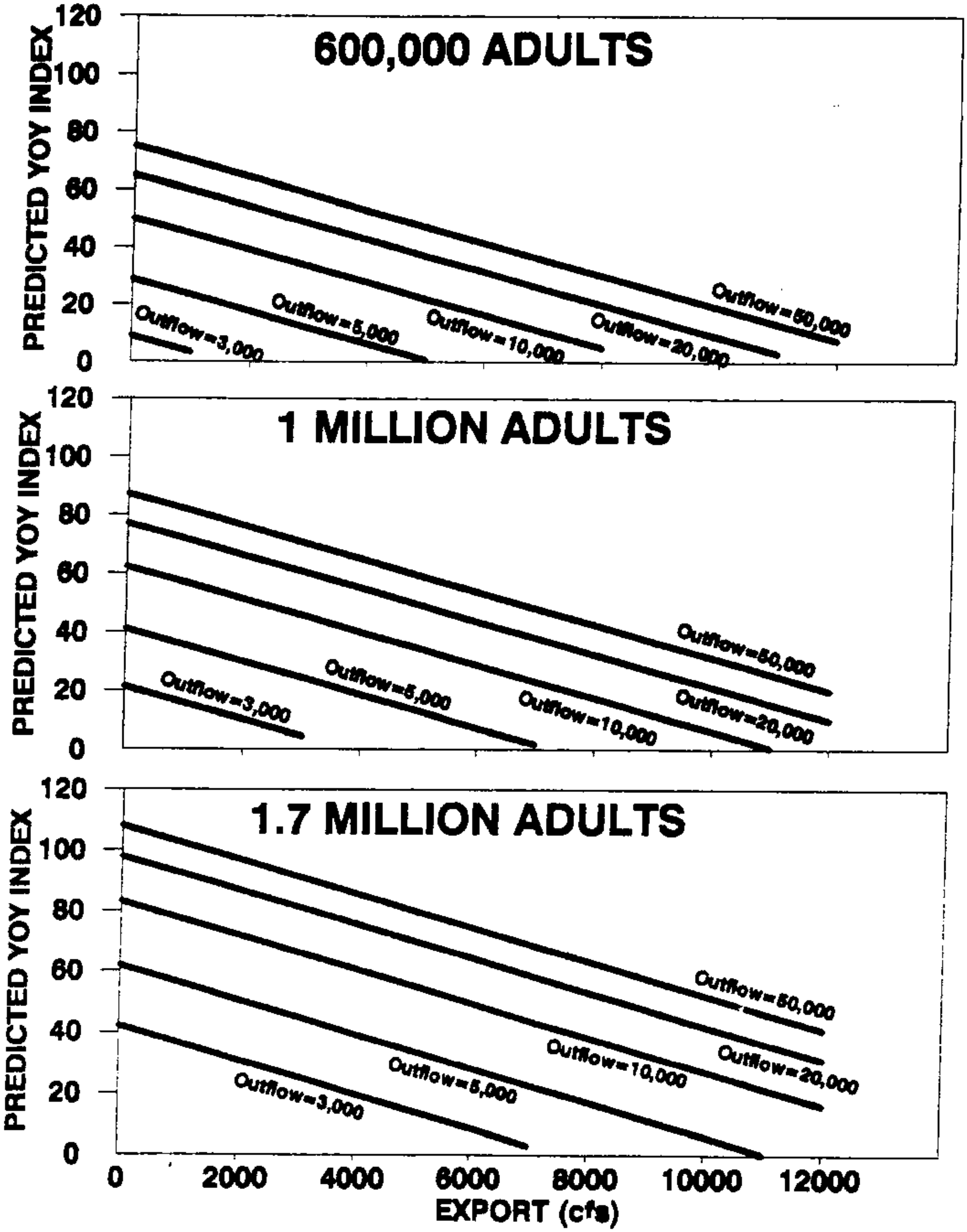

Figure 6. The young-of-the-year striped bass index predicted from mean April-July exports by the DFG Striped Model at three levels of adult abundance (spawning stock size) and five values of mean April-July outflow.

DEPARTMENT OF FISH AND GAME REBUTTAL OF WRINT-DELTAWET-24

Question 5 - It is true that the DFG model relies on past data to determine relationships among variables, but what other data are available (unless you have a crystal ball)? Literally, we are learning from the past so we do not repeat the same mistakes in the future.

Question 7 - Mr. Vogel has implied that "decisions based primarily on use of the DFG Striped Bass Model could result in ... changes in Delta flow patterns (that) could cause unanticipated impacts on striped bass". While this is a valid generalization, DFG's approach was structured to avoid this potential problem. To be specific, DFG's alternatives include flows within the range observed during the time when the model was being developed and with a similar seasonal distribution. Hence, this generalization is not applicable if standards are structured to follow DFG's alternatives.

Question 8 - It is true that many factors potentially affect the abundance of any organism, including striped bass. However, at any time the population is usually limited by only a few, or only one, of these factors. The DFG model indicates that water exports and outflow have been limiting striped bass abundance over the historical period, through their direct impact on young bass survival and their indirect impact on spawning stock size and egg production. The magnitude of the losses to entrainment associated with water export from the delta is also demonstrated by the JSA model (Figures 5 and 9 in WRINT-DELTAWET-15 [Revised]). While it is true that elimination of one variable as a limiting factor may cause another variable to become limiting, historical data suggest that striped bass abundance has been limited at lower levels of exports than are likely to be achieved by interim standards. Hence, it is improbable that, with modest reductions in exports and adequate outflow standards, the population-limiting effect of these variables will be eliminated.

In the second part of his response to this same question, Mr. Vogel demonstrates a lack of familiarity with past research involving striped bass in the Sacramento-San Joaquin Estuary. Extensive effort over the last 20 years has gone into evaluating the historical data involving a whole complex of variables potentially affecting the bass population. These evaluations have been conducted by agency scientists, independent consultants, and several task forces and have led to numerous technical reports and reports published in peer-reviewed scientific journals.

Question 10 - We question whether Mr. Vogel's careful evaluation of the JSA Fisheries Model and the Delta Wetlands Project included the impact on striped bass and other fish of reduced flow in the Sacramento River in May and June made possible by the release for export of water stored on delta islands. The reduced Sacramento flows during this period are implied by the response to Question 14 in which the "interesting operating scenario" of substituting Delta Wetlands water for export in place of water released from Shasta Lake is described. Given the effect of reduced flows in the Sacramento River on striped bass egg survival, does the Delta Wetlands Project still offer "modest overall" and "slight overall" benefits to striped bass?

REBUTTAL ON DELTA WETLANDS WITNESS

On July 21, Dave Vogel suggested that a partial fish screen (top portion of water column) could be used at the Delta Cross Channel and Georgiana Slough to benefit juvenile salmon by keeping them from being diverted into the central Delta at Walnut Grove. He stated that these devices could be implemented within one year at a cost under $1 million.

It is the consensus of the Fish Facilities Subcommittee of the Five Agency Delta Salmon Team that screening of the Delta cross channel or Georgiana Slough to protect juvenile salmon is infeasible. The screens under evaluation are full screens not partial. Key reasons for the lack of support for fish screens at those two diversion points were: 1) the flow reversals caused by tidal action, thus eliminating bypass flow and encouraging impingement, 2) need for a huge screen surface area to achieve high efficiency and 3) clogging and cleaning problems. Also, it is likely that any benefits associated with an inefficient partial screen would be off set by additional predation at the screen. Using a partial screen, allowing salmon to dive under the screen, would be even less efficient. Such a concept has not been tested or evaluated at any location to our knowledge.

The efficiency of the proposed surface screen would, in part, be determined by the vertical distribution of juvenile salmon as they pass the Delta Cross Channel and Gerogianna Slough. Although, juvenile salmon emigrating through the Delta tend to be concentrated near the surface during daylight hours, (Schaffter 1970, Gritz and Stevens 1971), they tend to be more vertically dispersed or concentrated near the bottom at night. (Wickwire and Stevens 1971, Schaffter 1980).

Schaffter, Raymond G. 1980. Fish occurrence, size, and distribution in the Sacramento River near Hood, California during 1973 and 1974. Calif. Dept. of Fish and Game Anadromous Fisheries Branch, Administrative Rept. No. 80-3.

Gritz, William J. and Donald E. Stevens. 1971. Distribution of young king salmon (<u>oncorhynchus tshawytscha</u>) in the Sacramento River near Pittsburg. California. Dept. of Fish and Game, Anadromous Fisheries Branch, Administrative Report No., 71-13.

Wickwire, Wrestle H. and Donald E. Stevens. 1971. Migration and distribution of young king salmon (<u>oncorhrynchus tshawytscha</u>) in the Sacramento River near Collinsville. Calif. Dept. of Fish and Game, Anadromous Fisheries Branch, Administrative Rept. No. 71-4.

Rebuttal Testimony on WRINT-SWC Exhibit Number 1

General Comment

While the recommended actions would, in general, contribute to increased survival of fishes, the biological benefits of the recommendations cannot be quantified (as pointed out on page 67). In our opinion, the limited potential of some proposed actions and uncertainties about the feasibility of implementation and effectiveness of the various measures make it unlikely that relying on the SWC program would stop the widespread decline in fishery resources in the estuary. Rather, we believe that effects of present and past water management overwhelm other factors so much (i.e. WRINT-DFG Exhibits 2, 3, 4, 5, and 6) that major changes in water management are essential for maintenance and recovery of most of the affected fish populations.

Specific Comments

Pages 19-22 - attempt to make the case that "significant" and "substantial" biological benefits, in terms of increased survival of entrained fish, could be attained by improving the screening, handling, trucking, and release procedures and facilities at the SWP's John E. Skinner Delta Fish Protective Facility (SFF). The Exhibit does not attempt to quantify the magnitude of potential survival benefits. The purpose of DFG's rebuttal on this subject is not to suggest that there is no room for improvement at the SSF, but to advise the SWRCB that we believe that, practically speaking, the potential for additional improvements in salvage efficiency at the SFF are quite limited, and should not be counted upon to contribute significantly to any interim plan to maintain or restore Delta fishery resources.

The Exhibit mentions a wide variety of potential improvement actions. Generally, these actions and/or potential benefits are vaguely defined, some have already been studied and implemented, others have been studied and rejected. We would like to emphasize that there has been a more or less continuous evaluation of the SFF and its operations since its completion. The recommendations of these evaluation efforts have, for the most part, been implemented. For example, Raquels's (1989) handling and trucking mortality study included recommendations regarding holding tank flows, adding salt to transport truck water, use of compressed O_2 in the trucks, all of which have been implemented. Other of Raquel's recommendations have not been implemented, generally because the benefits are small or too speculative. Skinner's (1973) original evaluation of the SWP screening facilities included numerous recommendations which have lead to operations criteria relating to approach velocities and bypass ratios, and addition of center-walls to the primary bays.

The exhibit mentions several potential "improvement" actions which are already being implemented in some manor or have been rejected. The frequency of fish transport is systematically controlled by parameters such as the number and size of fish salvaged and water temperature. The recent addition of DFG personnel to the salvage staff will allow more reliable adherence to trucking frequency criteria. The Exhibit also mentions the potential use of truck refrigeration to improve transport survival. DFG believes refrigeration may cause more harm than good because of the resulting need for in-truck holding periods for temperature acclimation.

Again, DWR and DFG have diligently improved the SFF as deficiencies have been identified, so DFG believes there is limited potential for further improving the efficiency of the existing SWP Delta fish protective facilities.

Page 28, paragraph 2. The SWC should recognize that the loss of 500,000 fish to poaching is pure speculation. There have not been any scientific studies to measure these losses.

Page 30, fishing restrictions. The striped bass population decline has been caused primarily by water management. Nevertheless, in response to the decline, new fishing restrictions were implemented in 1982 (increase in minimum size from 16 to 18 inches and decrease in bag limit from 3 to 2 fish). These new restrictions have not stemmed the decline, which is not surprising since recreational angling is not the cause of the decline. During the period since 1982, water exports continued to increase (WRINT-USFWS Exhibit 17A).

The declining striped bass population has resulted in a substantial decline in take by anglers which harvest only about 10 to 24 percent of the population in most years (WRINT-DFG Exhibit 2, Figure 1). Such harvest rates are considered safe for healthy striped bass populations and compare with rates which exceeded 40 percent on Atlantic Coast populations for many years and contributed to the decline of those populations (National Marine Fisheries Service and U.S. Fish and Wildlife Service 1984, 1992).

Thus, all parties should recognize that restrictions on exploitation of Sacramento-San Joaquin striped bass currently and historically have limited harvest to levels considerably below those in vogue elsewhere. Correction of the striped bass population decline and penalizing anglers and the fishing industry (who are not the cause of the decline) through additional fishing restrictions clearly is not justified in lieu of dealing with the cause of the decline.

Page 40, paragraph 1, 1991 striped bass population estimate. DFG testimony (WRINT-DFG Exhibit 2) is that "a more reasonable conclusion is that the 1991 population is at about the same level as the 1990 population" (680,000, not 1.2 million).

Page 52. The recommendation that an agreement be executed to fully mitigate for direct entrainment losses resulting from CVP pumping operations has been satisfied. Such an agreement was completed between the Department of Fish and Game and the Bureau or Reclamation on July 17, 1992.

Page 61. Bailey's toxicity tests are inconsistent with those by DFG (WRINT-DFG Exhibit 2). In any case, drain toxicity isn't the real issue, the issue is whether or not the river is toxic to striped bass. The toxicity tests that have been done with river water provide results to the contrary (WRINT-DFG Exhibit 2).

Pages 61-64, histological examination of livers. The liver necrosis apparently is consistent with toxic effects, but it is only speculation that the damage has been caused by the agricultural chemicals contained in the laboratory based toxicity studies.

References

National Marine Fisheries Service and U.S. Fish and Wildlife Service. 1984. Emergency Striped Bass Research Study Report for 1982-1983. 37 p.

________________________. 1992. Emergency Striped Bass Research Study Report for 1990. 47p.