O'Laughlin & Paris LLP

Attorneys at Law

**VIA ELECTRONIC MAIL AND US MAIL**

January 17, 2007

Tam Doduc
Chair
State Water Resources Control Board
PO Box 100
Sacramento, 95812

**Re: Proposed San Joaquin River Flow Workshop**

Dear Ms. Doduc:

The San Joaquin River Group Authority ("SJRGA"), at the hearing on the adoption of the revised Water Quality Control Plan for the San Francisco/Sacramento-San Joaquin Delta Estuary ("2006 Bay-Delta Plan"), expressed its concerns in regard to holding a San Joaquin River flow workshop in 2007 based on preliminary "data". As we expressed at that December hearing and during numerous other hearings before the State Water Resources Control Board ("State Board") over the last five years, decisions are being considered and, in some cases made, based upon untested, unreliable, incomplete, and incorrect data. We cautioned the State Board about placing too much trust in the California Department of Fish and Game ("CDFG") model that has not been through a thorough public peer review. Our fears were well founded.

In the Periodic Review of the 1995 Bay-Delta Plan, Issue 9 was directed at the pulse flow and Issue 8 was directed at the February through June flow requirement. This hearing notice was issued in September 2004. In February 2005, CDFG filed its submittal to the State Board. (DFG-EXH-08). The CDFG asserted, solely on the basis of its new, untested, undocumented, incomplete "robust" model, that the current flow objectives in the San Joaquin River were not protective of Fall-run Chinook salmon. Based solely on this "robust" model, the CDFG further recommended a review of the current flow objectives and a review of the San Joaquin River Agreement and VAMP. No other party to the San Joaquin River Agreement, including USFWS, made such a recommendation.

At the February 2005, workshop, CDFG represented that it would complete the model and that the model would undergo a rigorous, open, collaborative peer review process, the same kind of process that the State Board and parties to the Bay-Delta hearings demanded of CALSIM II.

In April 2005, the SJRGA requested a copy of the CDFG model. The SJRGA had SP Cramer & Associates review and critique the model. This review was provided to the State Board, the CDFG, and to the public. (See attached) From April 2005 until

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September 2006, the "robust" fish model was nowhere to be seen. CDFG's explanation was that it was working on the model.

In October 2006, CDFG filed its comments on the Draft 2006 Bay-Delta Plan. In that document, and up to the time of the hearing on December 19, 2006, CDFG stated, "The model is undergoing a peer review." To the contrary, the model had already been reviewed. Unbeknownst to other parties, CDFG, rather than holding an open peer review, similar to that conducted for CALSIM II, held a blind, private review with its own selected reviewers, a far cry from an open, collaborative peer review process. Considering that CDFG is part of CALFED it is ironic to say that CDFG shunned the Bay-Delta Modeling Forum for a review. However, in doing so, CDFG was able to continue making unsubstantiated statements and recommendations before the State Board without revealing that the basis for these statements, i.e. the "robust" fish model, which to put it mildly, has been found to have fatal flaws.

The model appeared in October, when Mr. Dean Marston of CDFG made a cursory presentation on the model, its results, and their recommendations at the CALFED Science Conference and a month later, in November, at the EWA workshop. (See attached copy of presentation.) At the EWA workshop, the SJRGA learned, for the first time, that the model had been peer reviewed.

The SJRGA immediately requested the peer review results, but was told it could not have them. The SJRGA then made a public records request and received the reviews. (See attached peer reviews.) CDFG hand-picked reviewers were Josh Korman of the University of British Columbia, Dr. Kenneth A. Rose of Louisiana State University, Dr. Henrietta Jager of the Oak Ridge National Laboratory, Dr. Bruce Herbold of the USEPA, and Dr. Michael L. Deas of Watercourse Engineering. However, since the peer review was allegedly blind, CDFG will not disclose who drafted which review, with the exception of Henrietta Jager, of the Oak Ridge National Laboratory, whose name is on her review.

All five of the peer reviewers cited significant deficiencies and recommended substantial improvements. Dr. Jager, Reviewer #1, discussed few details of the model itself and could not suggest a calibration approach. Indeed, her only comment of significance is that the model has been slightly "dumbed down" so lay persons can use it. (Jager Review, p1.) The following excerpts from Reviewers Nos. 2, 3, 4, and 5 give a good overview of the fatal problems with CDFG's model:

- Is the model adequate? No. The population model has many flaws. The model in no way validates or confirms the importance of Vernalis flow. (Peer Review #2, p2.)
- By only including Vernalis flow and hatchery augmentation, there is no way of evaluating other alternatives like Delta exports or ocean harvest. This basic mistake suggests either that the author has either a very biased perspective, or has little experience with resource management modeling. (Peer Review #2, p5.)
- There is no statistical reliability to the model. (Peer Review #2, p.7.)

- The strong correlation between flow and adult returns does not necessarily imply causality. For example, the high abundance during the mid-1980's (Fig. 1) was a coast wide-phenomena seen from California to BC. It is widely acknowledged as a period of high-marine survival. Flow may have an important influence on Chinook production during some periods, but it is overstating the case to say that production is largely driven by flow. (Peer Review #2, p9.)
- [T]he strength of the conclusion that spring flows are the key determinant of salmon production is not substantiated by the data. (Peer Review #2, p11.)
- I strongly disagree with the conclusion that this model provides a tool to predict the amount of flow required to meet the doubling goal. The modeling effort violates many basic modeling approaches and biological principles and is deficient on all fronts (structure, parameter estimation, uncertainty analysis, policy evaluation). (Peer Review #2, p12.)
- Are products of value likely from the project? No. (Peer Review #3, p3.)
- Is the model adequate? No, not as a tool to manage SJ salmon freshwater life stages. It ignores significant factors, it gives unsupported reasons for leaving our [sic] other factors and its does not address factors affecting fry or adult abundances and it fails to make the case that SJ smolt production controls adult production. (Peer Review #3, p3.)
- The model as it currently exists largely can only recommend that we have flood year flows every year. Doubtless that would solve various salmon problems but it is not useful guidance to management or research. (Peer Review #3, p4.)
- The model suffers by trying to derive salmon management actions from flood situations. (Peer Review #3, p9.)
- The author seems to attach no value to work not done locally and that weakens the biological foundation and the resulting model. (Peer Review #3, p12.)
- This report describes a lot of relevant information but then discards most of it in favor of a regression model that captures almost none of the biology of this species and therefore provides no guidance to future research and with little relevance to comprehensive management. (Peer Review #3, pp13-14.)
- In short, I find that most of the assumptions and conclusions are either not supported by the data or cannot be supported by the analyses. As a consequence I find the model to be unsuited for the purposes to which it has been put. (Peer Review #3, p14.)
- In my opinion, the weaknesses of the model stem from its over-reliance on statistical correlations, and the appearance of ad-hoc decisions as to which statistical relationships are strong versus weak and thus included or excluded from the model. I do not agree with the principle assumptions of the model which removed ocean harvest, exports, and density-dependence from further consideration. (Peer Review #4, p2.)
- [M]any of correlations and arguments in the report approach become circular and convoluted because the same data seems to be used in multiple ways. (Peer Review #4, p3.)
- I am not convinced that the author has shown that it is flow and not exports. (Peer Review #4, p9.)

- The model does not provide evidence that spring flow is important. The model was built under that assumption. (Peer Review #4, p10.)
- [T]he Reviewer . . . does not see the model as a stand alone tool to provide long-term flow recommendations. (Peer Review #5, p2.)
- The reviewer is not convinced that Delta exports play no role as noted numerous times in the report. (Peer Review #5, p5.)
- In a complex system such as the San Joaquin River, Delta, San Francisco Bay, and Pacific Ocean, it may be difficult to identify the actual limiting factors – which may vary appreciably in space and time. That is, in any given year river flow, ocean conditions, tributary conditions (flow, habitat, and/or, temperature, predation), Delta export, and/or other factors may be individually a dominant factor or present a combination of stressors. (Peer Review #5, p6.)

The peer reviewers and SP Cramer found many substantial and serious flaws with the model. It is clear that this model cannot be fixed.[1]

Because of the egregious process used by CDFG in the purported peer review of its model which nevertheless produced results which condemn it, the SJRGA requests the hearing on the workshop for the San Joaquin River flows be cancelled. The sole basis for the workshop request of USFWS, NOAA, and CDFG was the now discredited CDFG model. Given the results of CDFG's non-blind review which does not meet peer review standards of open independent review, it is the position of the SJRGA that a workshop is unwarranted. If the State Board decides to hold such a workshop, the SJRGA respectfully requests that the State Board direct CDFG to have the "robust" fish model peer reviewed through the CALFED Bay-Delta Modeling Forum and direct CDFG to adequately respond to all of the major criticisms of the model identified in the existing and any new reviews before the workshop. If CDFG fails to comply, then the State Board should consider canceling the workshop.

The SJRGA has requested a meeting of the San Joaquin River Agreement Management & Technical committees for January 2007. The committees will address the State Board's requests to have the VAMP peer reviewed with respect to study design, adequate protection, and adequacy of information. The members of the SJRGA will be responding to the State Board's request after these meetings.

Very truly yours,
O'LAUGHLIN & PARIS LLP

By: TIM O'LAUGHLIN

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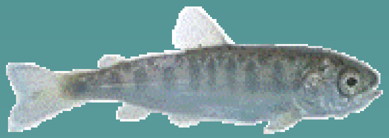
[1] As the SJRGA said at the Bay-Delta hearings, the "robust" model is really nothing more than a reformulation DFG exhibit from the Phase I Bay-Delta hearings in 1987, which was itself reformulated from a 1972 DFG exhibit in a matter involving New Melones (most likely D-1422). (DFG Exhibit #15, The Status of San Joaquin Drainage Chinook Salmon Stocks, Habitat Conditions and Natural Selection Factors (September 1987), p35 Figure 12.) The theory is no sounder now then it was then.

Cc: Board Members
Vicki Whitney
Gita Kapahi
Nick Wilcox
Tom Howard

Enclosures

# *Role of Spring Flow in Determining SJR Tributary Salmon Production*

**CALFED EWA**
**Conference**
**Nov. 28, 2006**



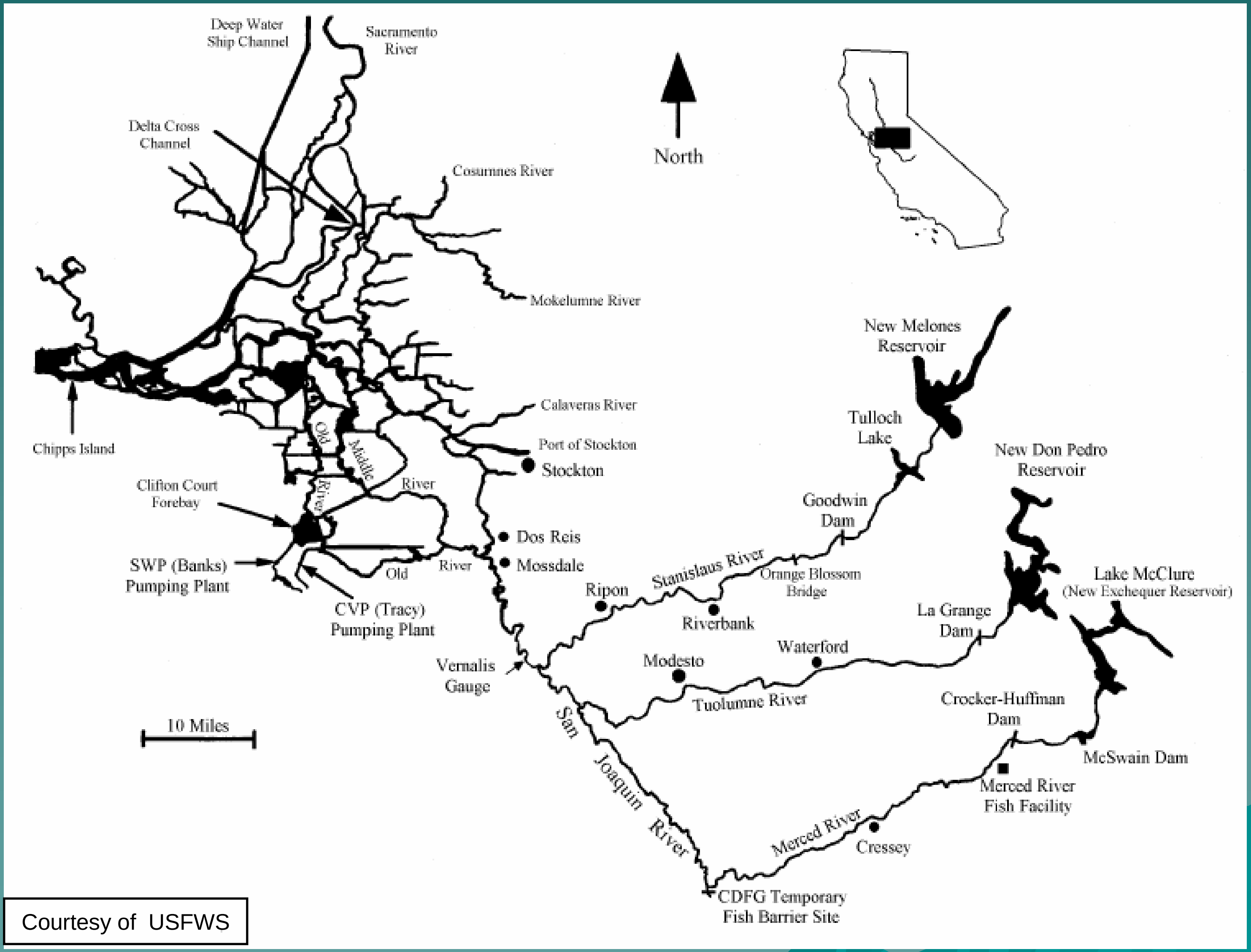
**Dean Marston**
**CDFG**

**Dr. Carl Mesick**
**USFWS**

# SJR Overview

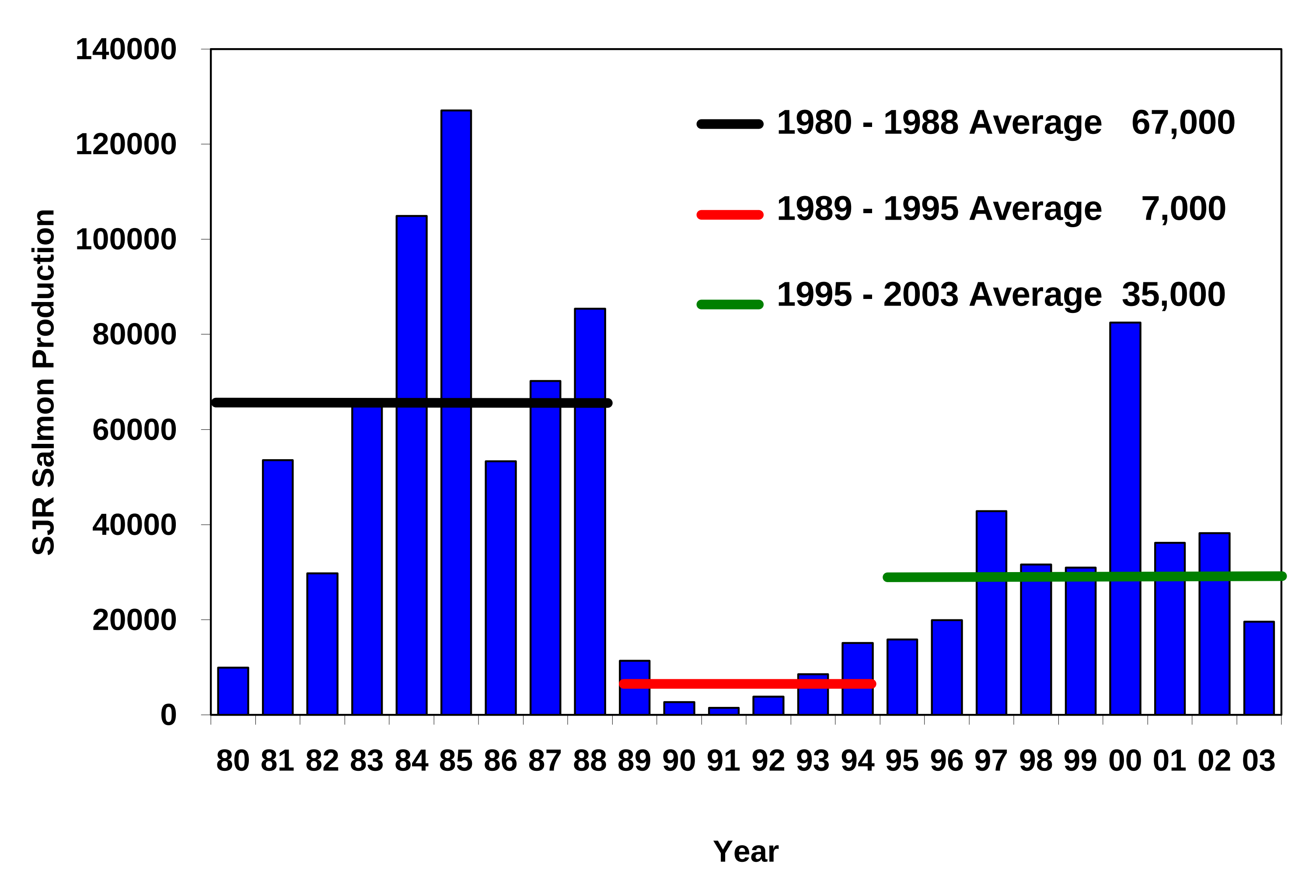
- Chinook Trend
- Pre & Post Drought Comparison
- Tributary Spring Flow
- Role of Non-Flow Factors
- Salmon Model
- Model Scenarios
- Recommendations & Conclusions
- EWA and Regulatory Actions

# Geographic & Model Orientation

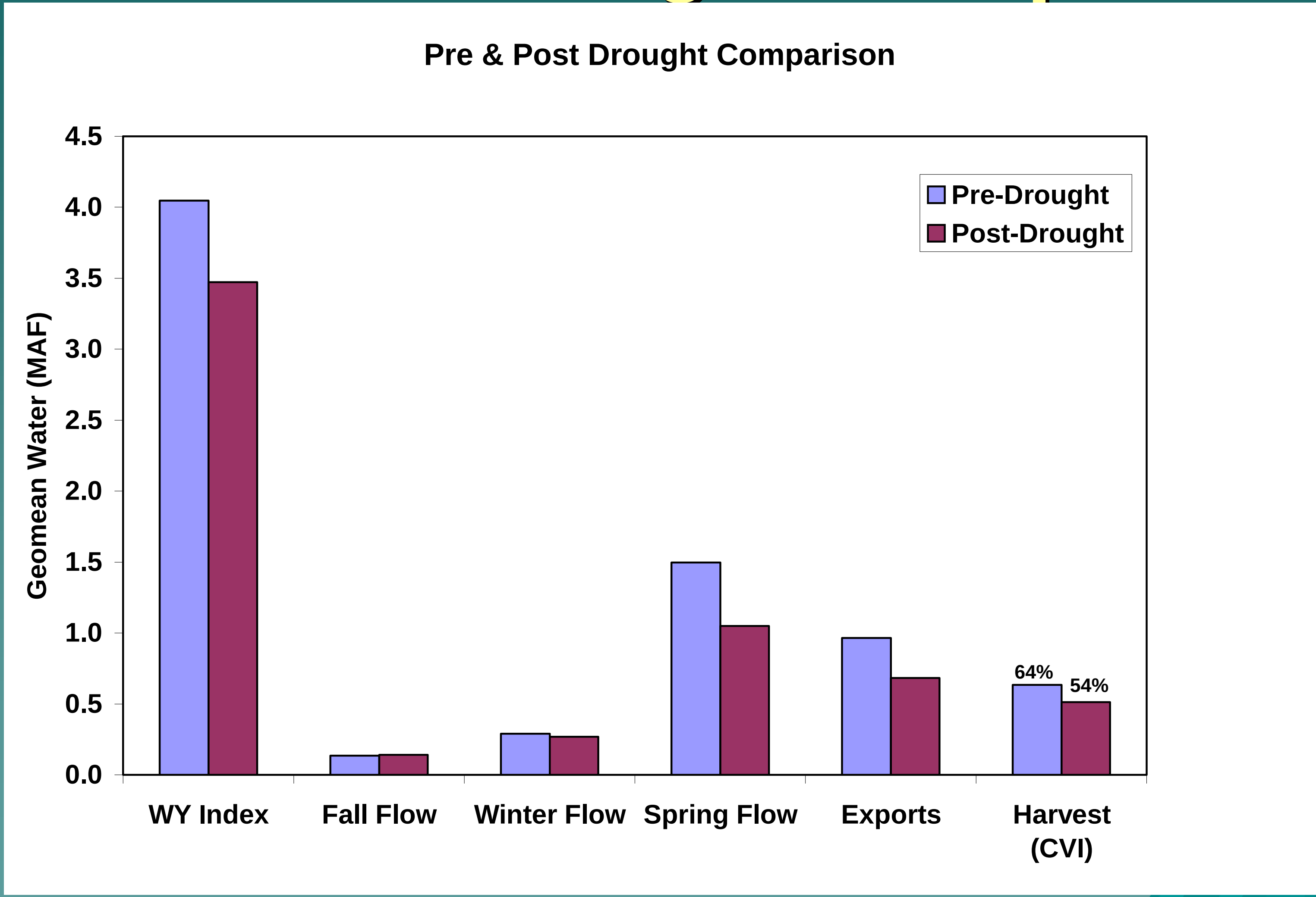



Courtesy of USFWS

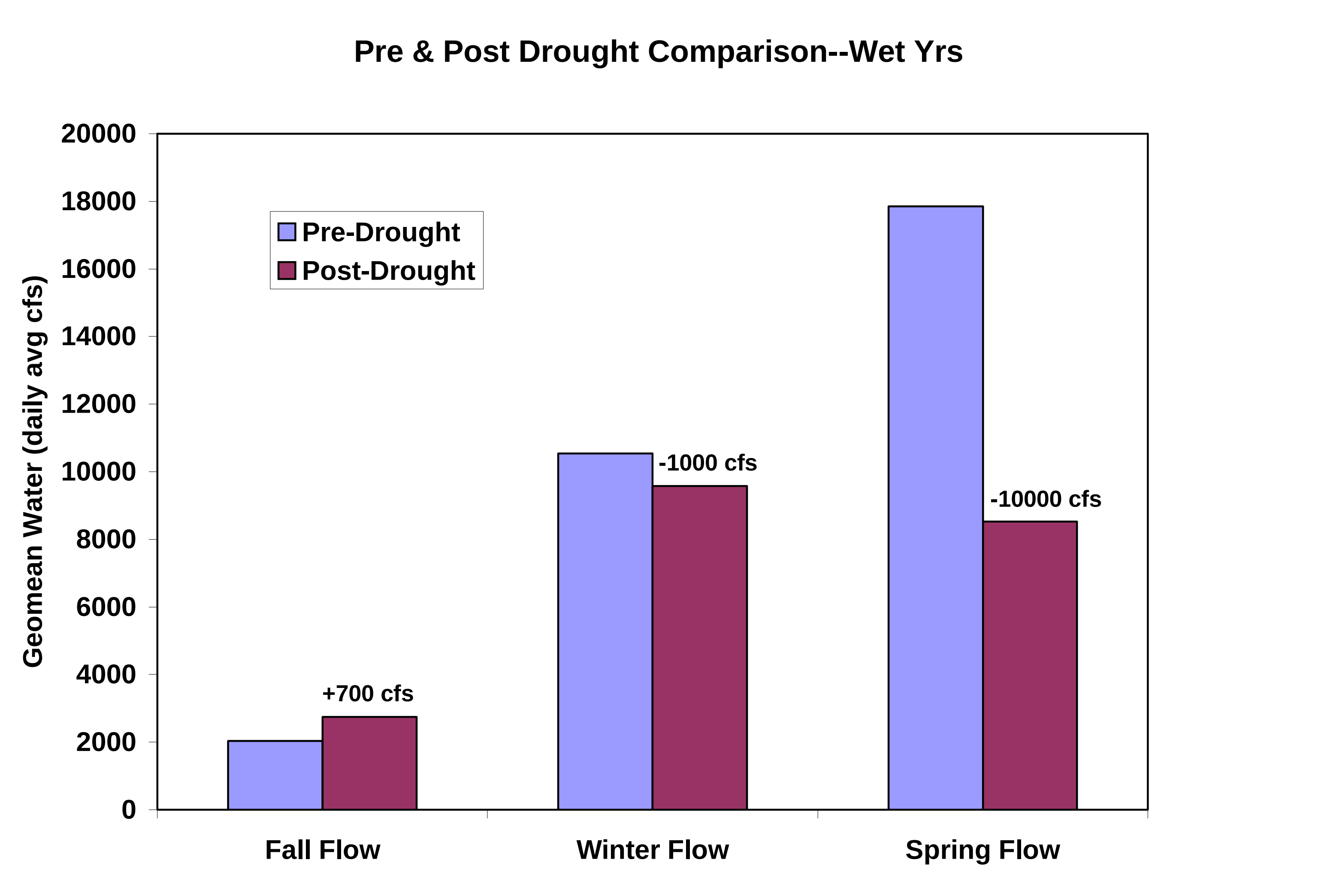
# SJR Salmon Trend



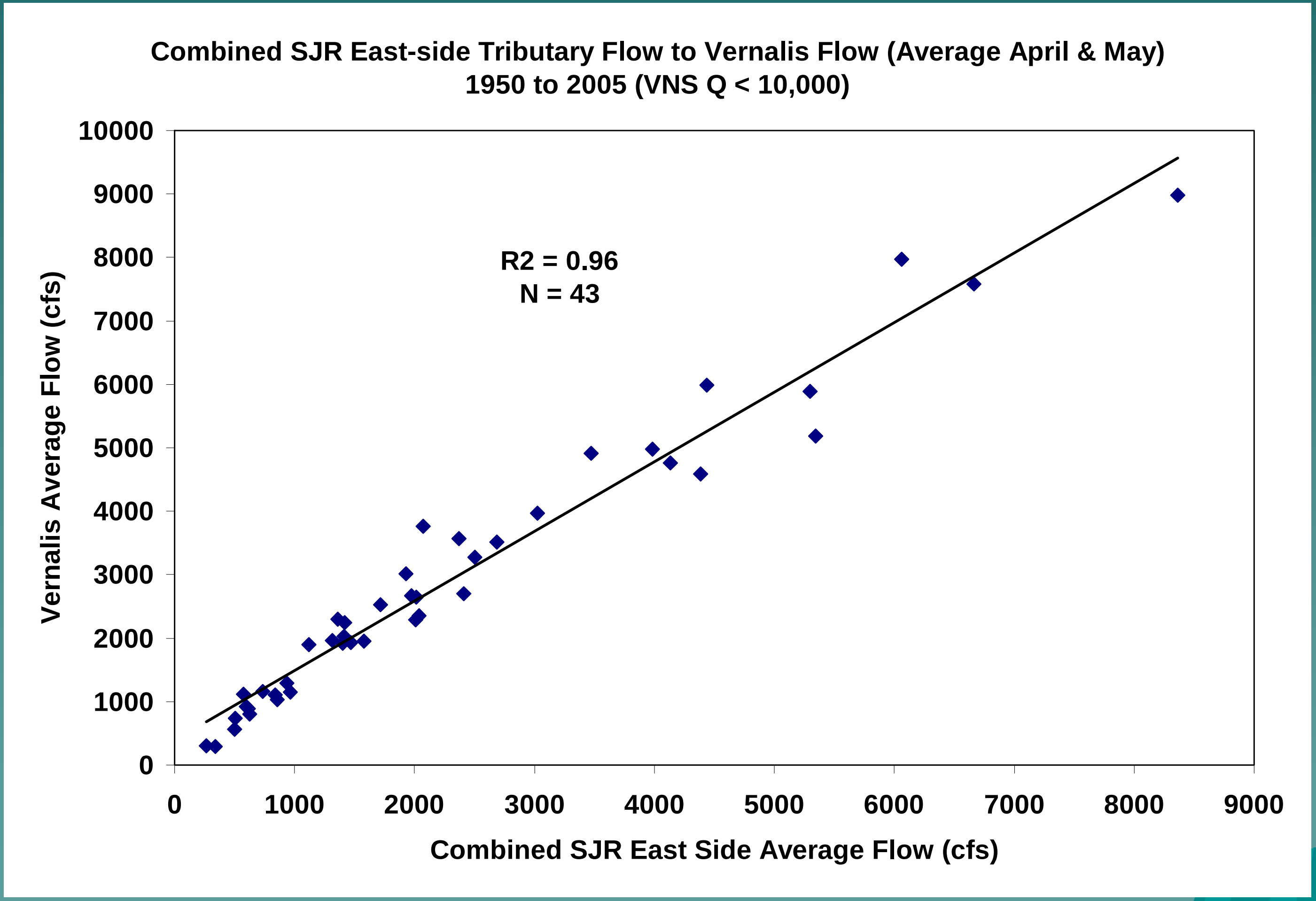
# Pre & Post Drought Comparison



# Pre & Post Drought Wet Yrs

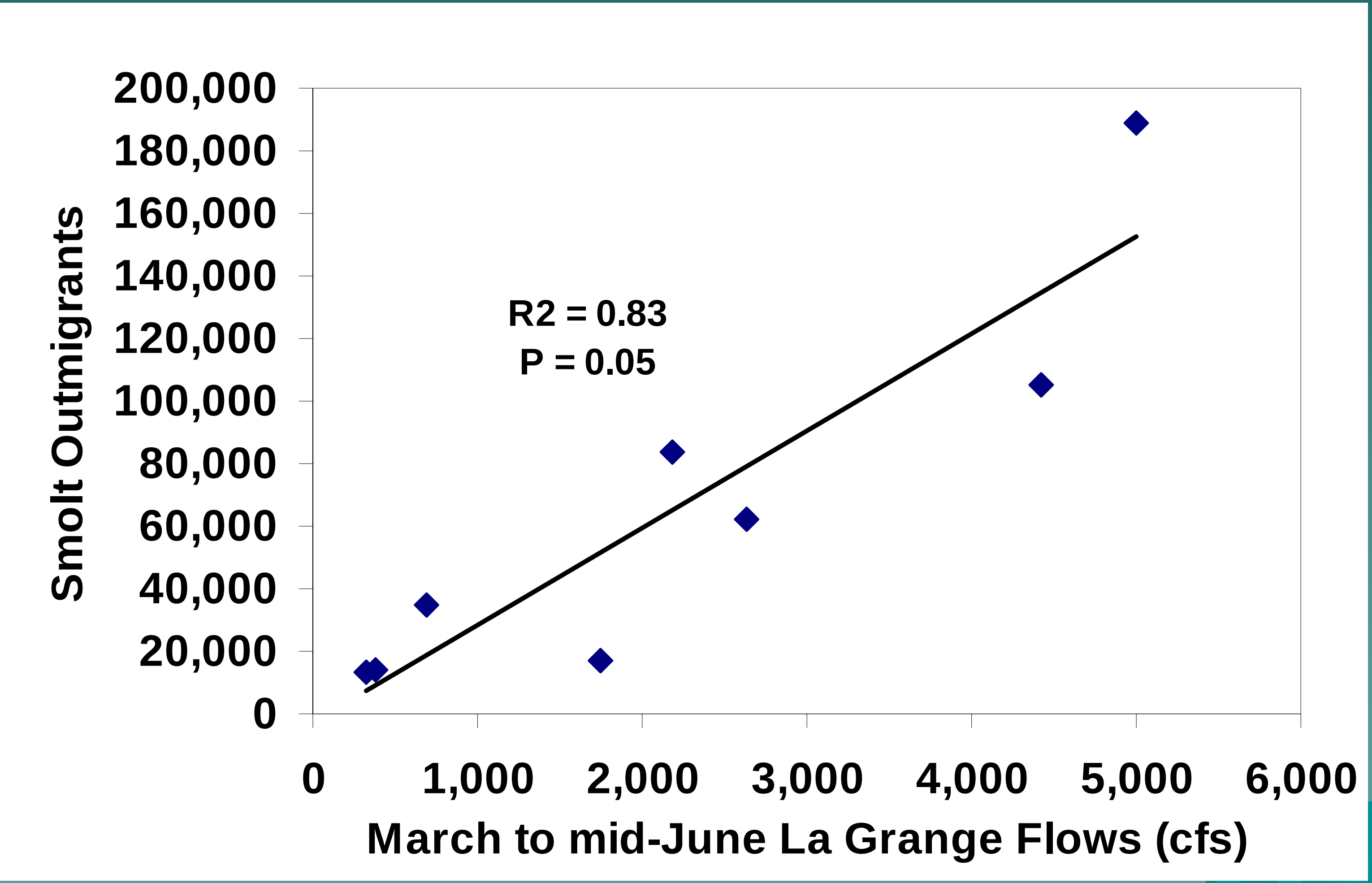


# VNS & SJR Trib Flow

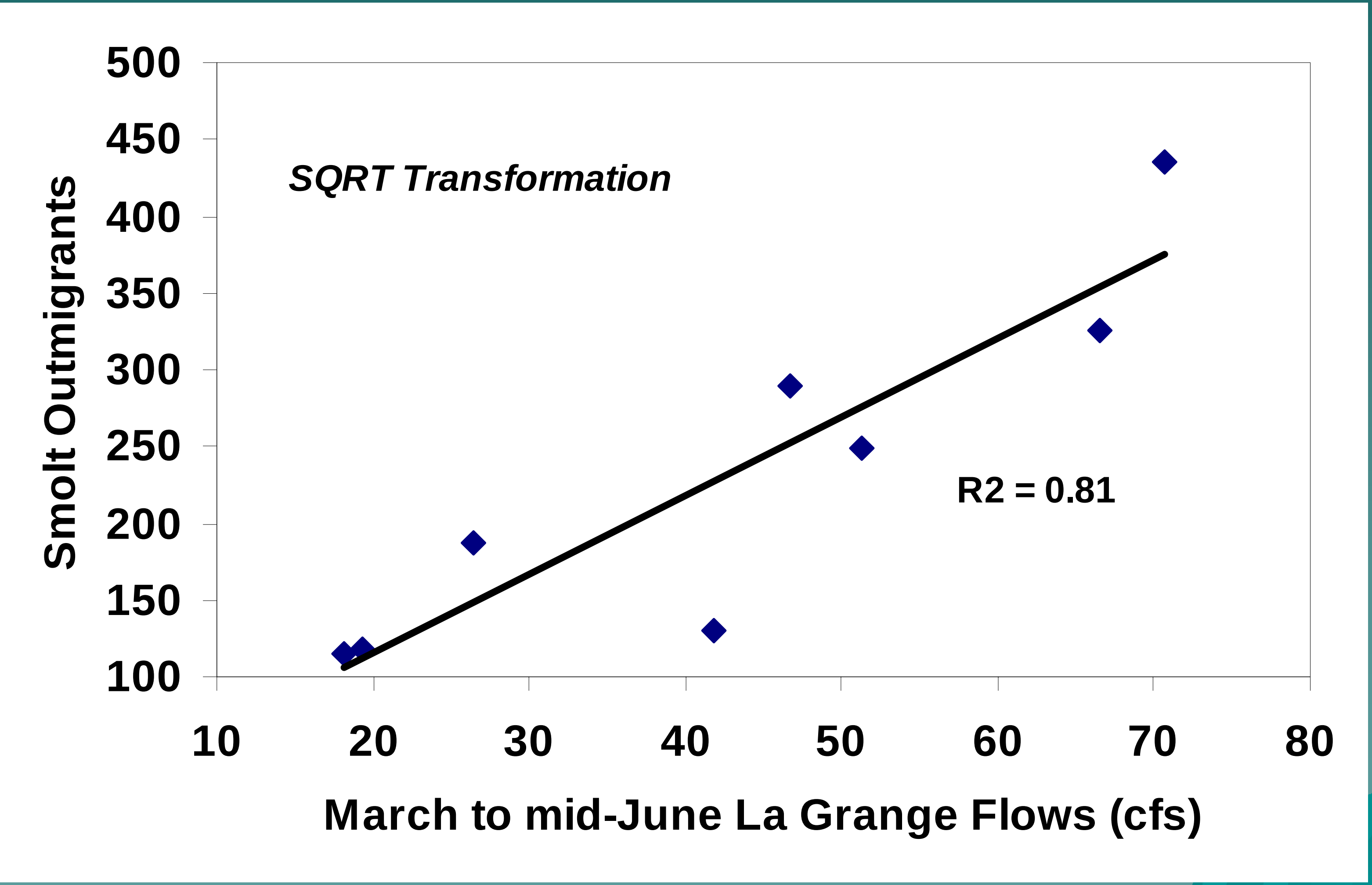


# Role of Trib Spring Flow

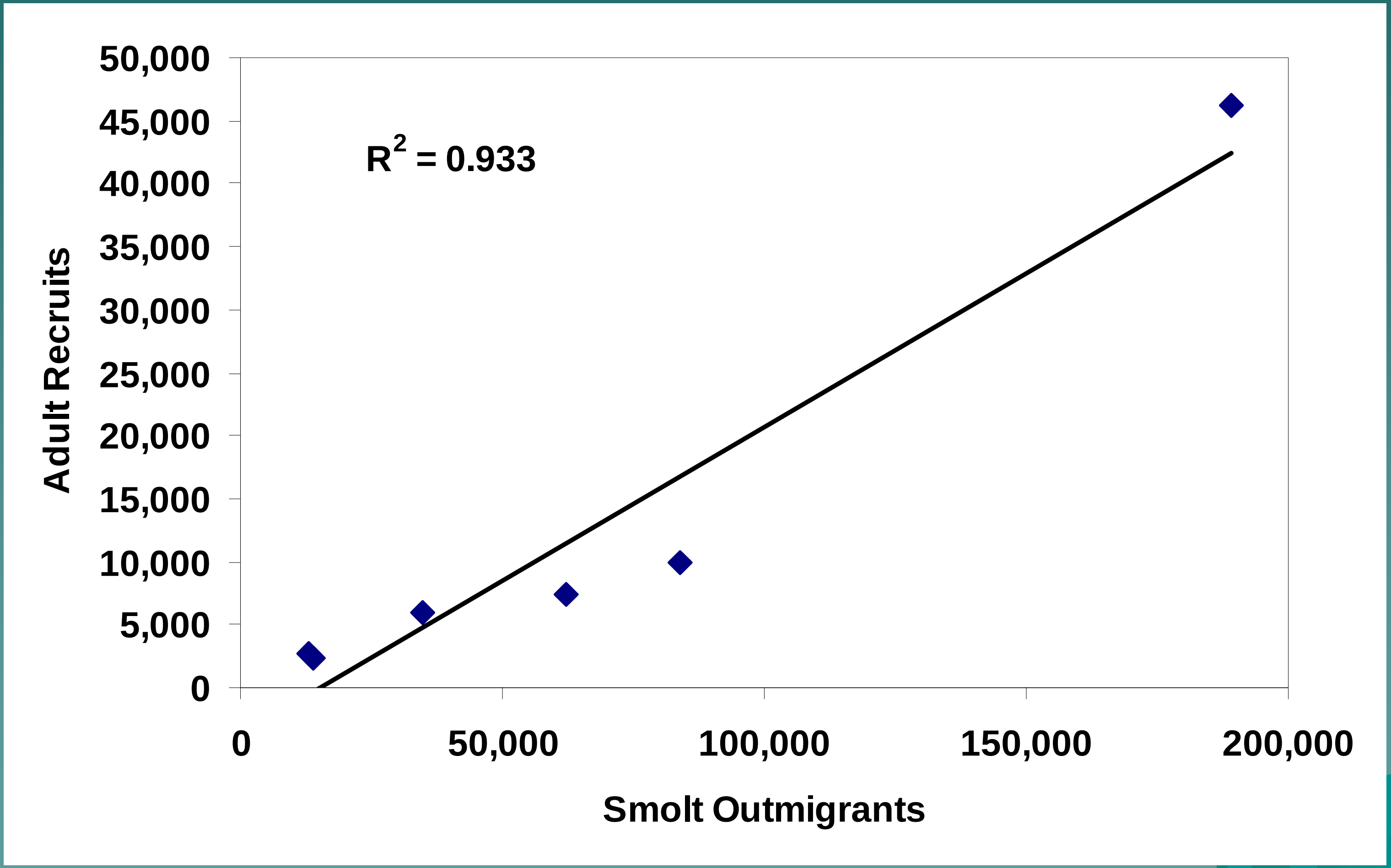
# Tuolumne River Smolts vs. Flow 1998 – 2005 (Preliminary Data)



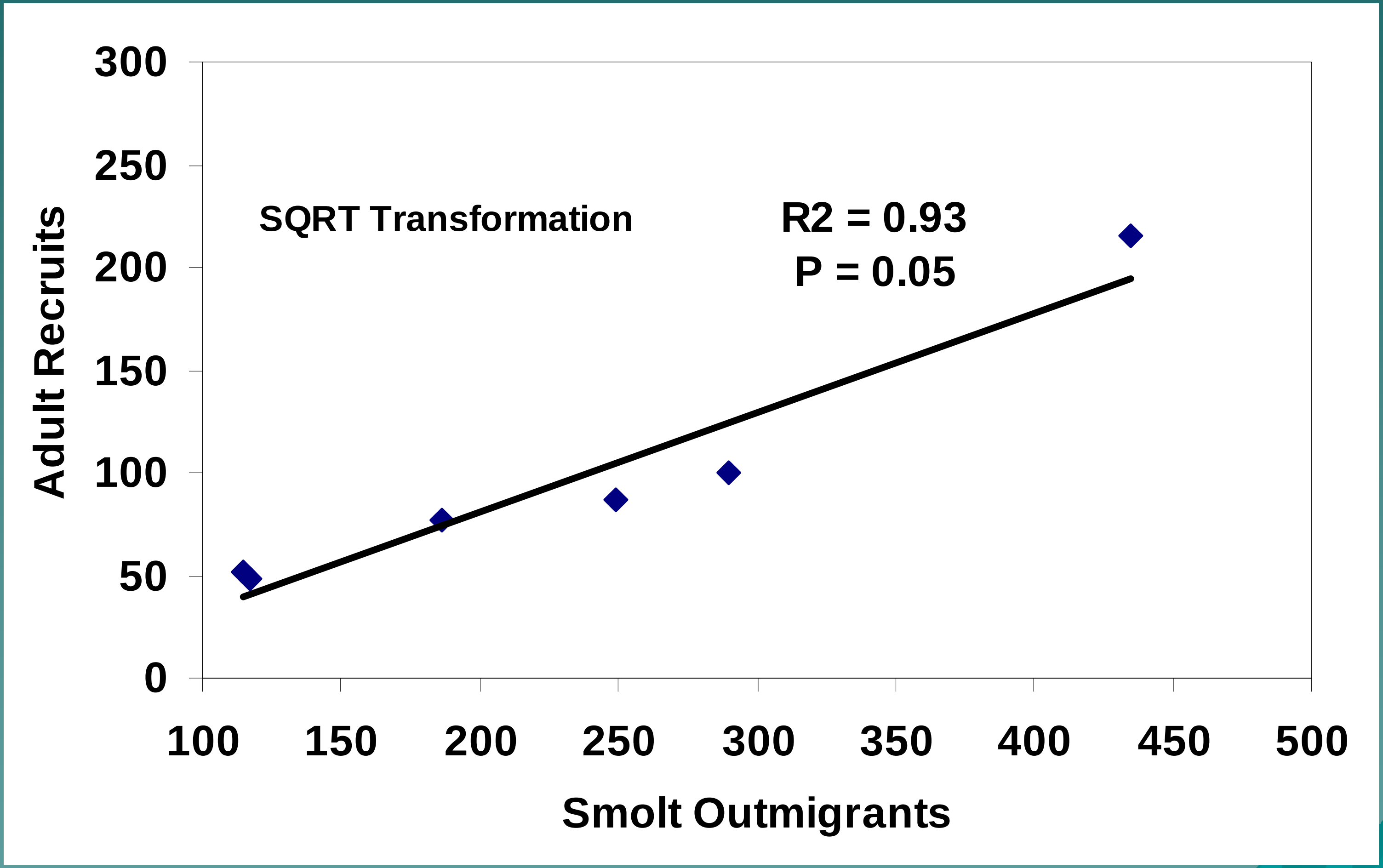
# Tuolumne River Smolts vs. Flow 1998 – 2005 (Preliminary Data)



# Tuolumne River Adults vs. Smolts 1998 – 2003 (Preliminary Data)

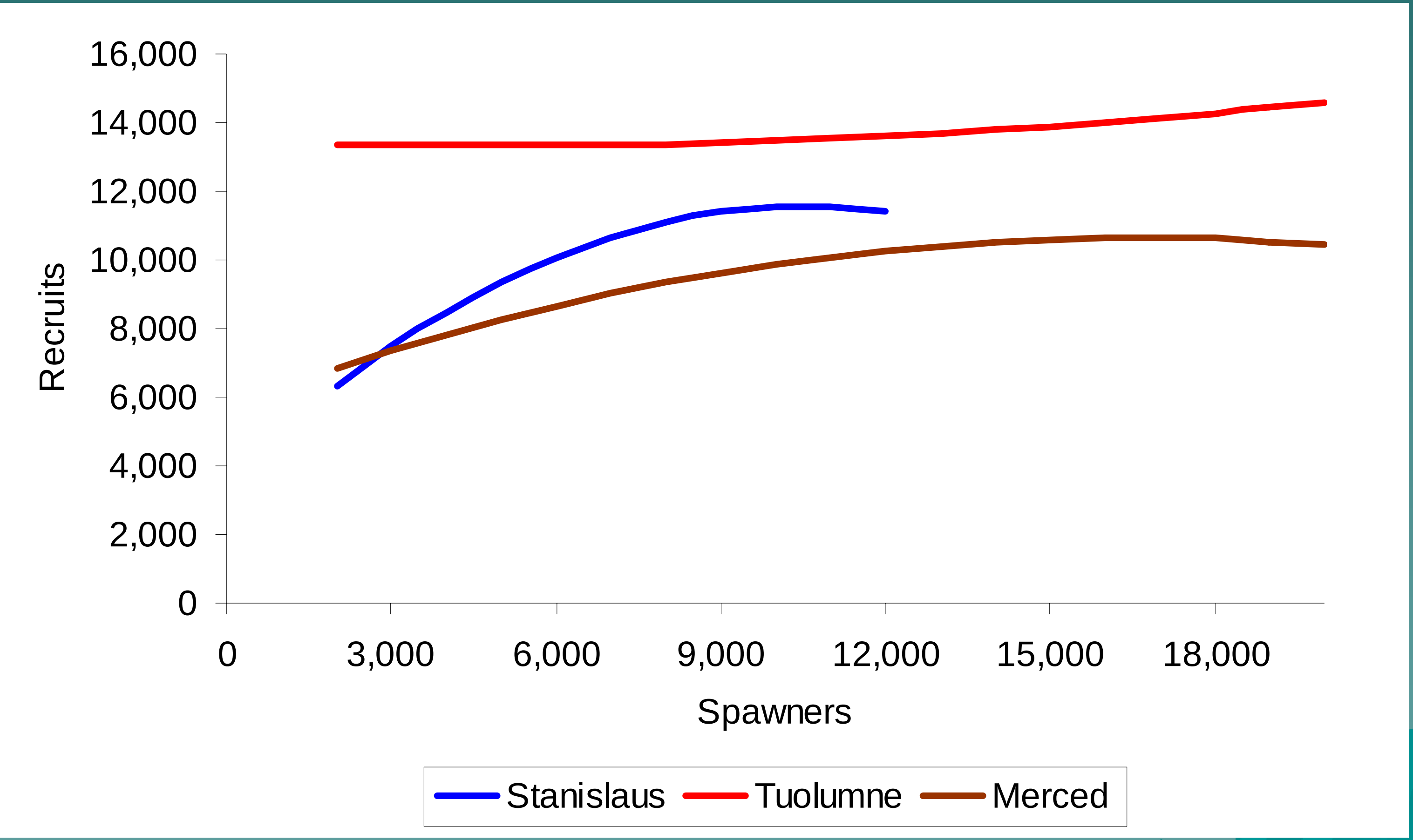


# Tuolumne River Adults vs. Smolts 1998 – 2003 (Preliminary Data)

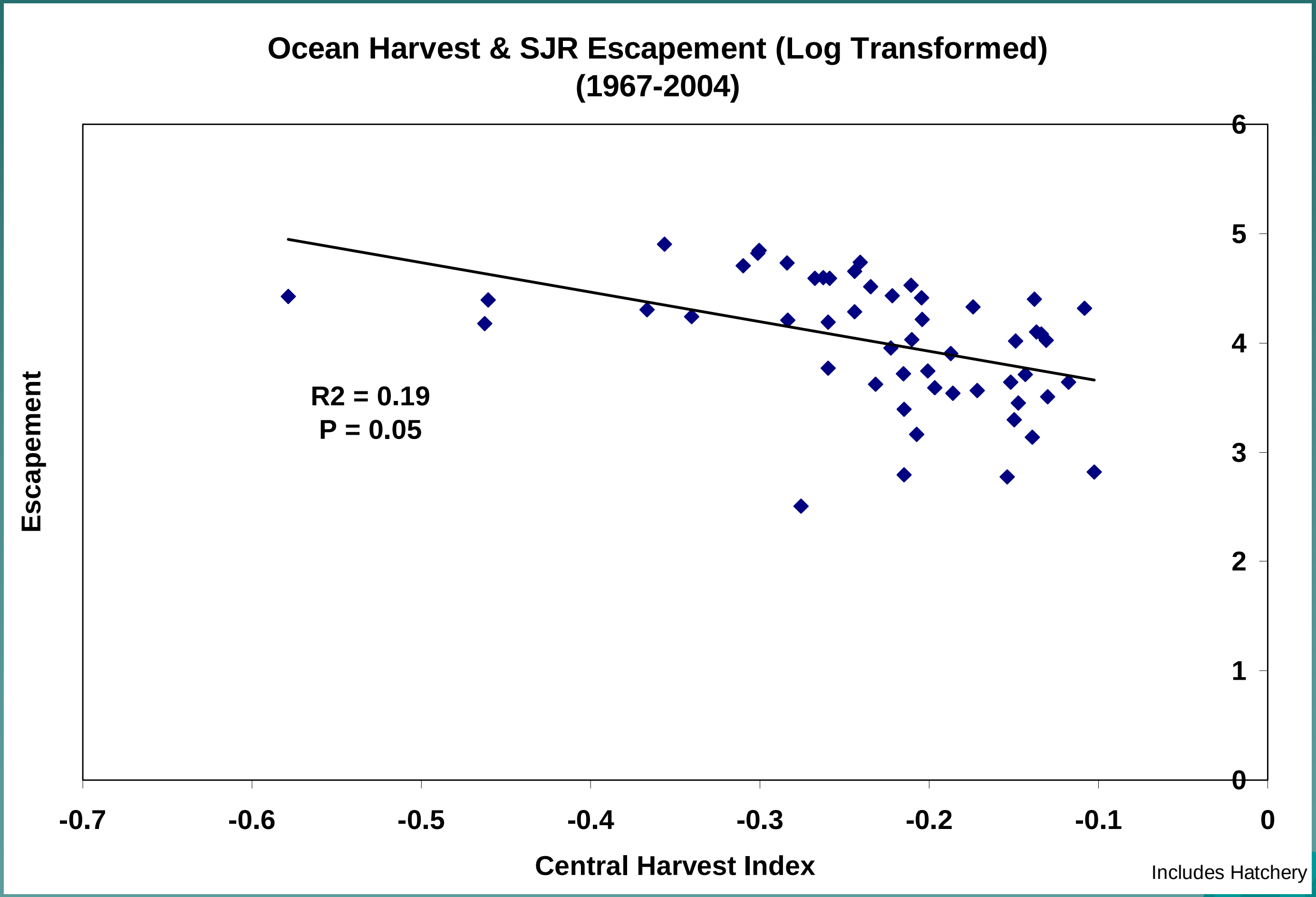


# Non-Flow Parameters

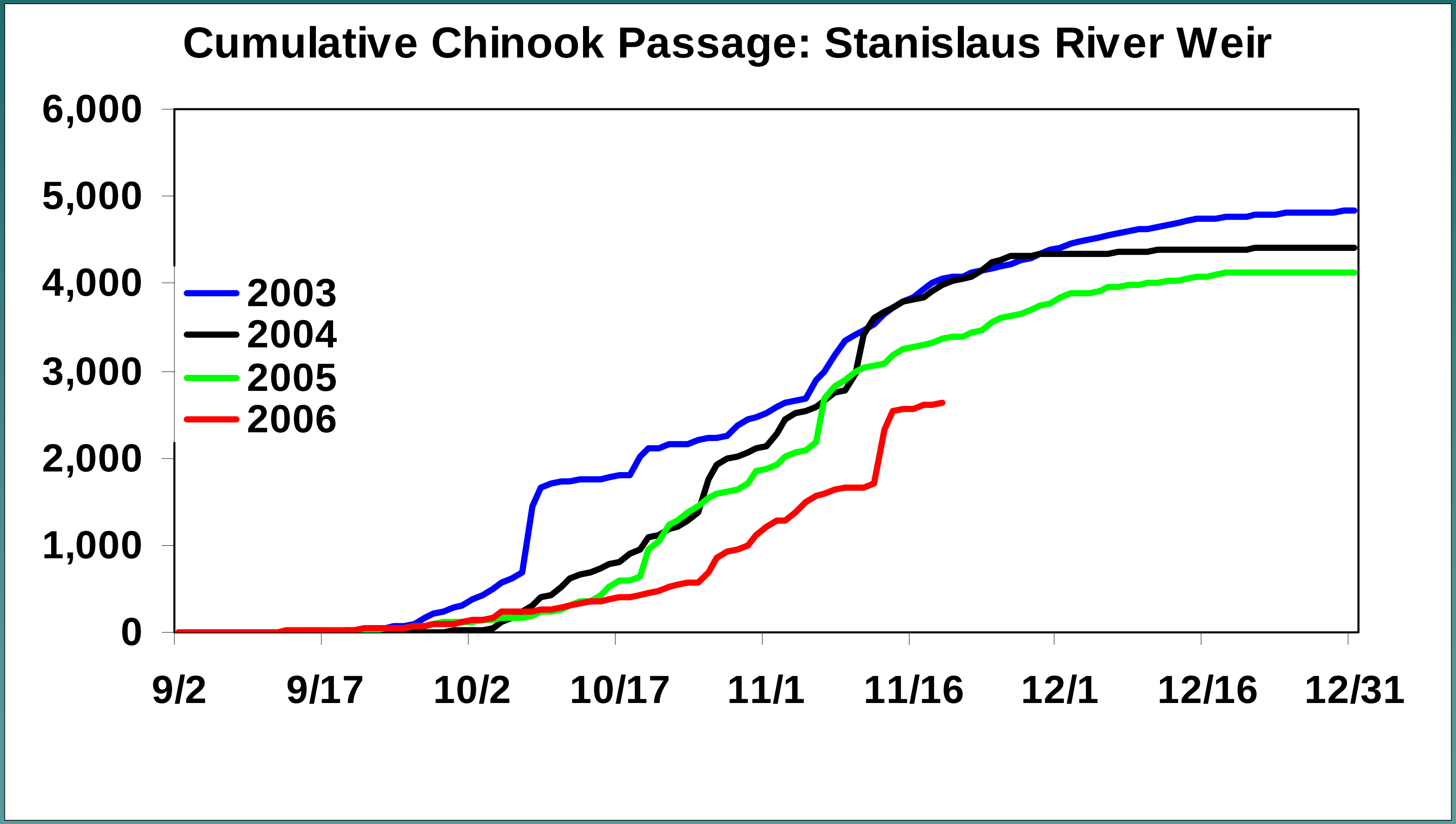
# Stock-Recruit Relationships 1983-2003



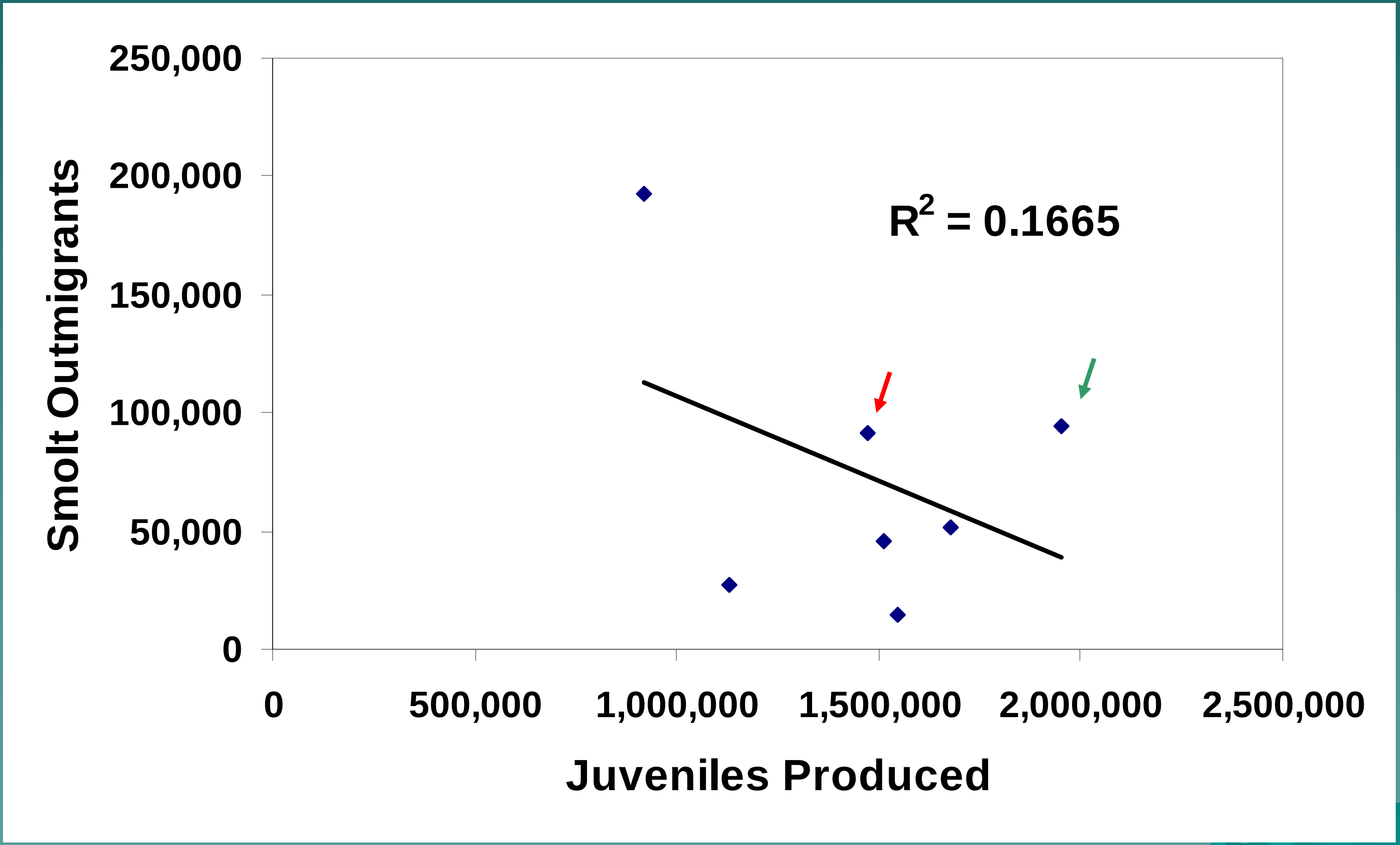
# SJR Salmon & Ocean Harvest



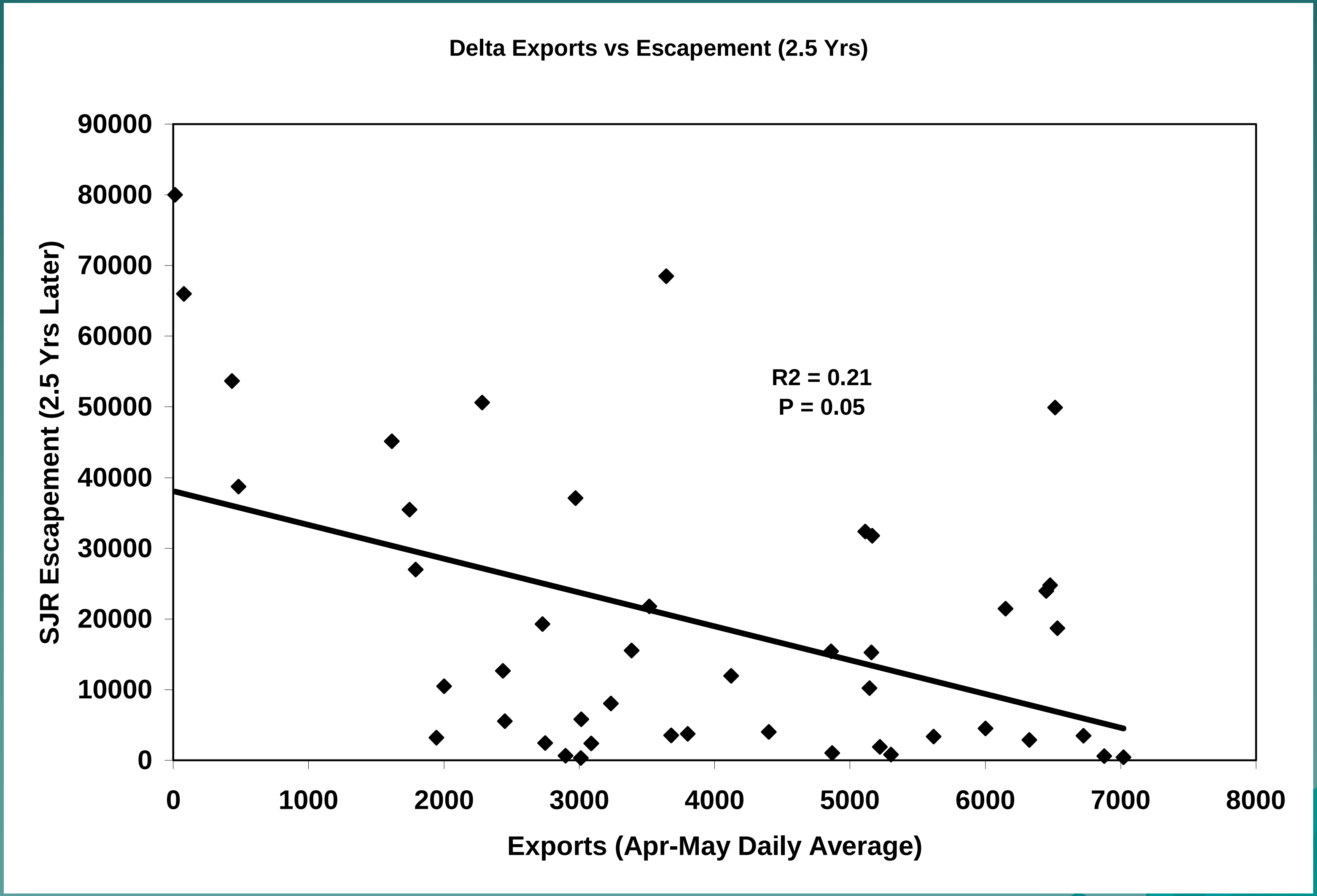
# SJR Salmon & Ocean Harvest



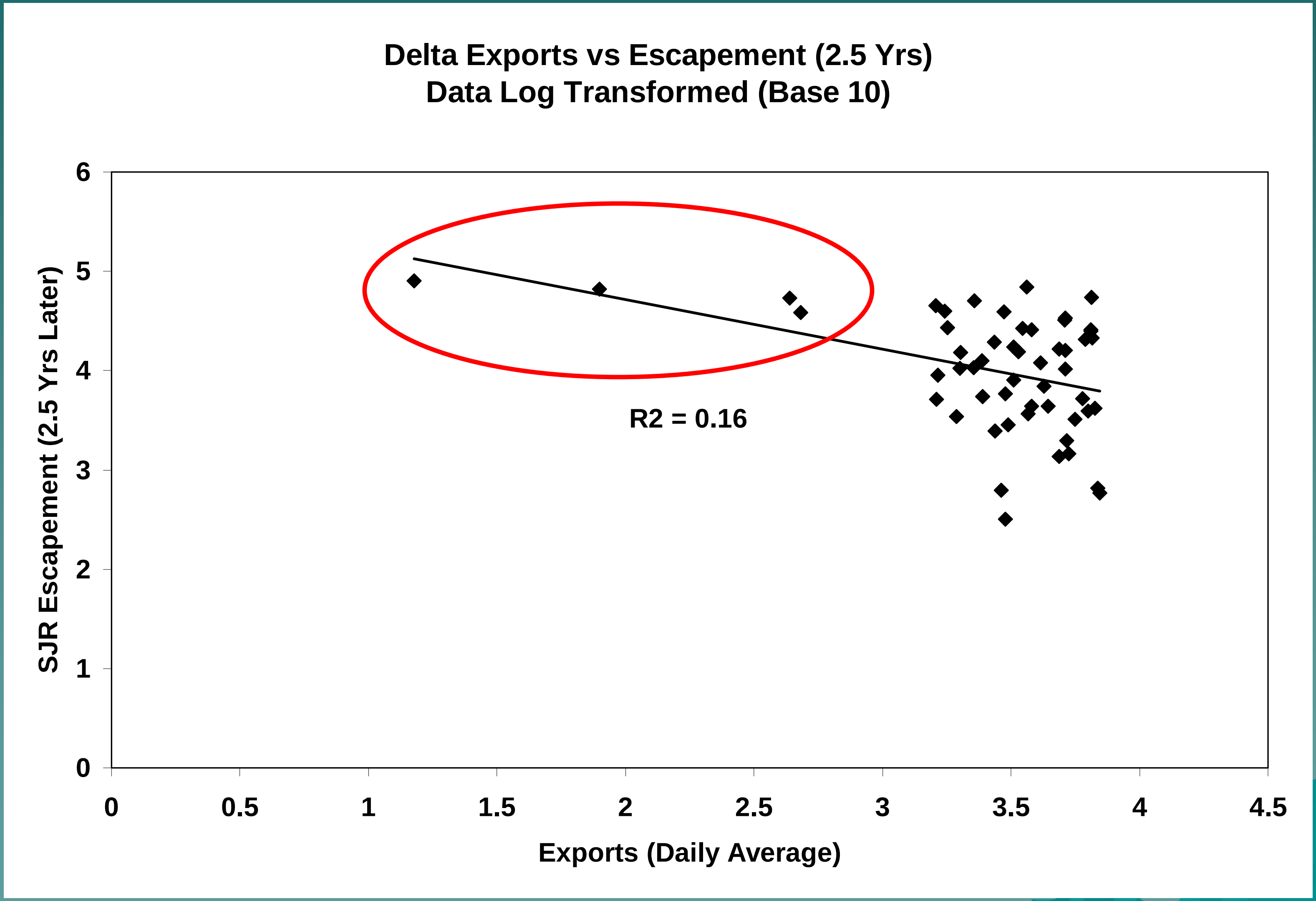
# Spawning Habitat Quantity



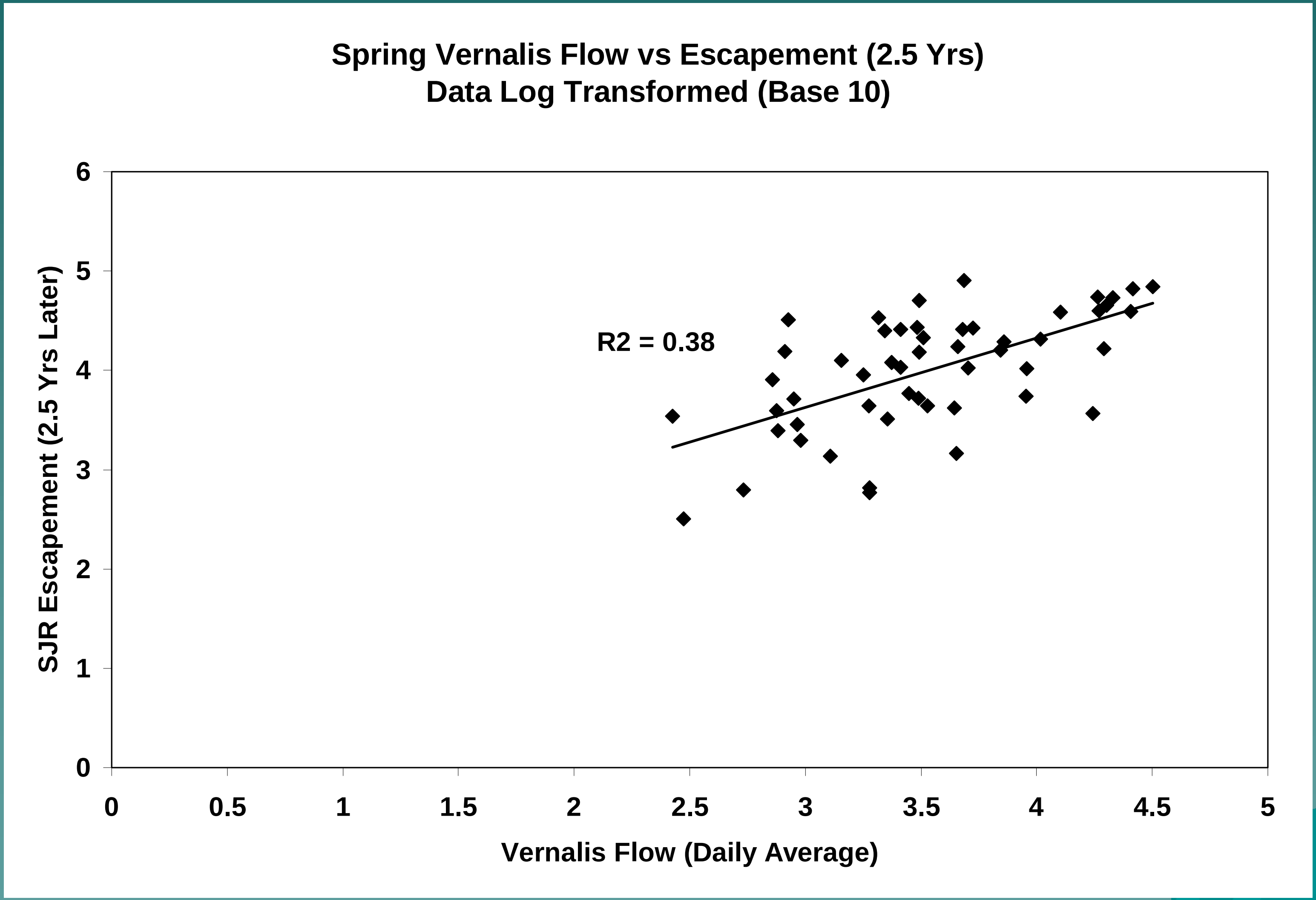
# Delta Exports & SJR Salmon



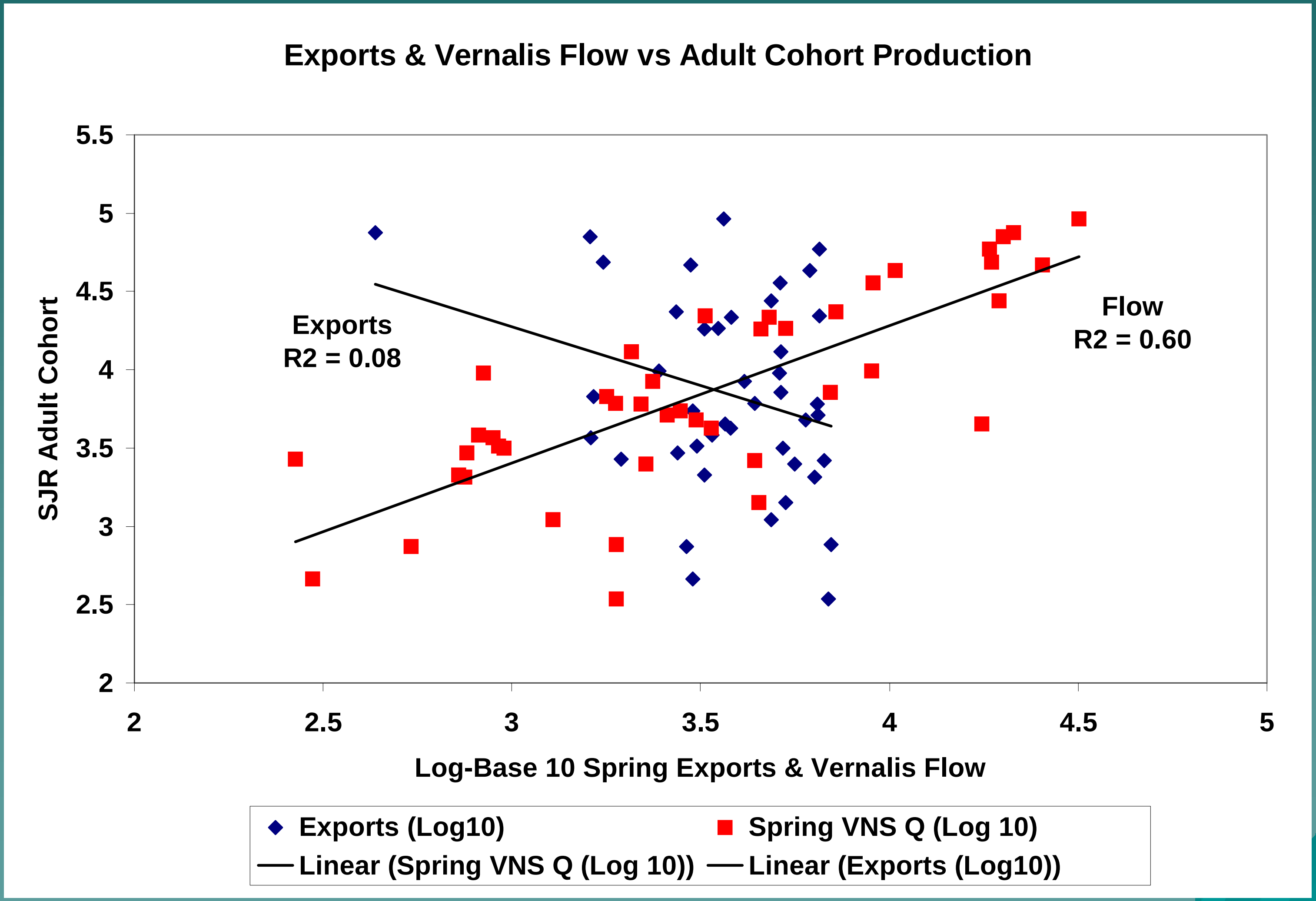
# SJR Salmon & Delta Exports



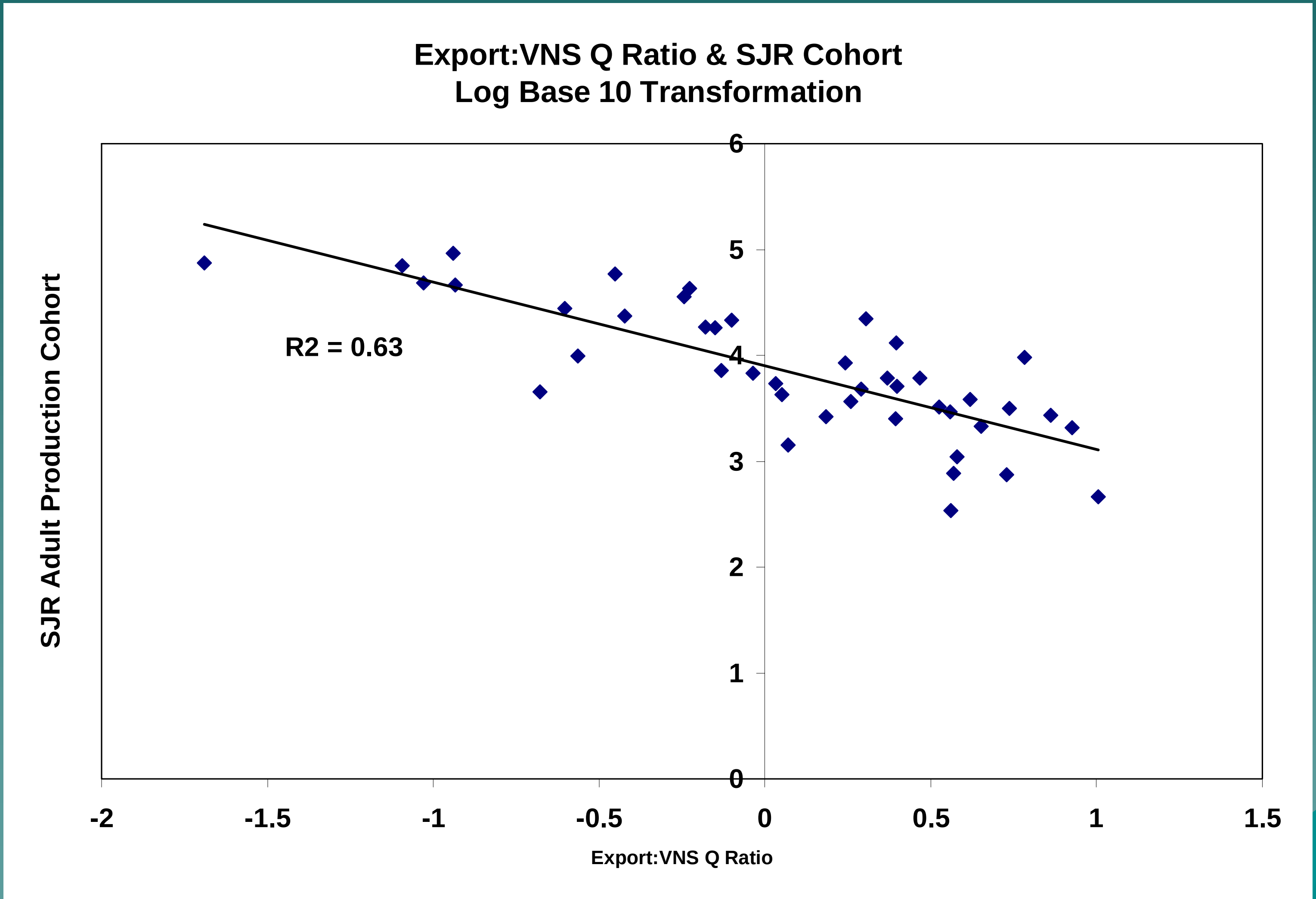
# VNS Flow & SJR Salmon



# Exports & Adult Cohort

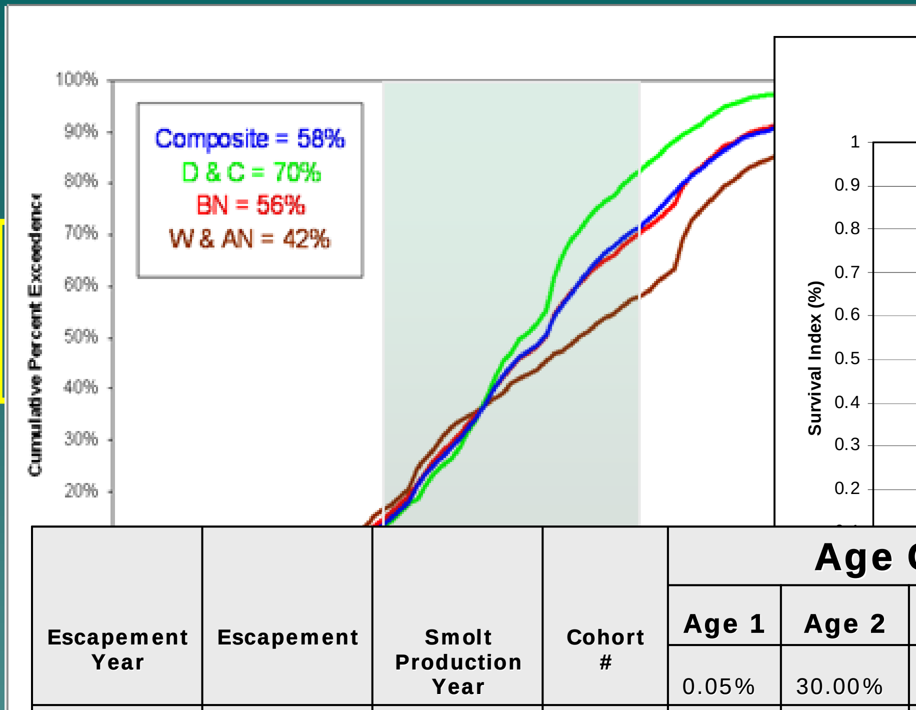


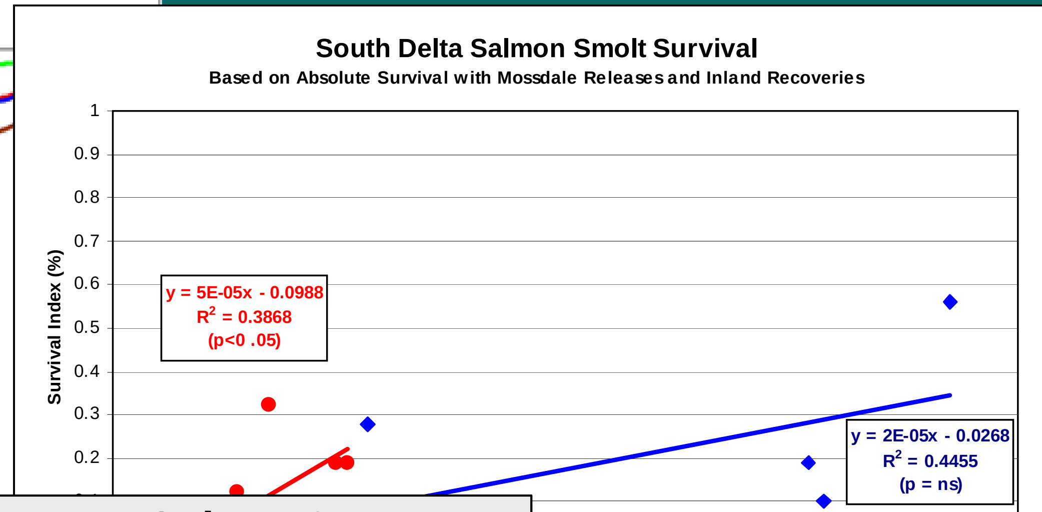
# E:I Ratio & SJR Cohort

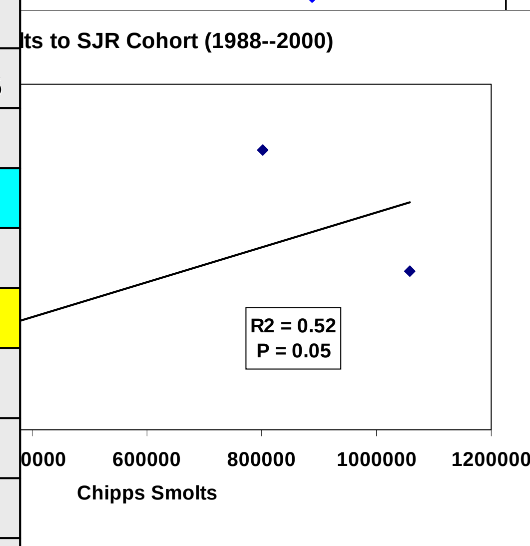


# SJR Model

- Role of Spring Flow & Production
- Parameters (what's in & out)
  - Flow
  - Harvest
  - Exports
  - Spawning habitat
- Structure




| Escapement Year | Escapement | Smolt Production Year | Cohort # | Age Cohort %'s | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | | Age 1 | Age 2 | Age 3 | Age 4 | Age 5 |
| | | | | 0.05% | 30.00% | 55.35% | 14.00% | 0.60% |
| 1967 | | 1968 | 276 | 0 | 83 | 153 | 39 | **2** |
| 1968 | | 1969 | 98603 | 49 | 29,581 | 54,577 | **13,804** | 592 |
| 1969 | | 1970 | 1403 | 1 | 421 | **776** | 196 | **8** |
| 1970 | | 1971 | 1119 | 1 | **336** | 620 | **157** | 7 |
| 1971 | | 1972 | 461 | **0** | 138 | **255** | 64 | 3 |
| 1972 | **14,919** | 1973 | 2638 | 1 | **791** | 1,460 | 369 | 16 |
| 1973 | 1,547 | 1974 | 3645 | **2** | 1,094 | 2,018 | 510 | 22 |
| 1974 | **1,213** | 1975 | 3304 | 2 | 991 | 1,829 | 463 | 20 |

# Model Scenarios

- How might EWA H20 be used?
  - *Drier SJR WY Types (C, D, BN)*
- Extend VAMP window
- Increase VAMP magnitude
- Do both

# Model Scenario Results

| Model Results Summary | | | | | |
|---|---|---|---|---|---|
| **Fish** | | **Water (TAF)** | | **Fish/TAF** | |
| **Magnitude** | **3200** | **Magnitude** | **3200** | **Magnitude** | **3200** |
| 4450 | 2 | 4450 | 74 | 4450 | 7 |
| 5700 | 3 | 5700 | 149 | 5700 | 7 |
| 7000 | 5 | 7000 | 226 | 7000 | 8 |
| **Duration** | **30** | **Duration** | **30** | **Duration** | **30** |
| 40 | 6 | 40 | 31 | 40 | 70 |
| 50 | 10 | 50 | 63 | 50 | 67 |
| 60 | 12 | 60 | 93 | 60 | 56 |
| **Both** | **3200 (30)** | **Both** | **3200 (30)** | **Both** | **3200 (30)** |
| 4450 (40) | 7 | 4450 (40) | 130 | 4450 (40) | 22 |
| 5700 (50) | 14 | 5700 (50) | 310 | 5700 (50) | 20 |
| 7000 (60) | 21 | 7000 (60) | 545 | 7000 (60) | 17 |
| *Notes: Fish #'s are multiplicative (2 = two times more production)* | | | | | |

# Flow Recommendations

- Extend Window
  - Does not complicate VAMP experiment
  - Late better than early
  - All tribs better than one trib
- Increase Magnitude
  - VAMP allows for change
  - Options:
    - One trib for all time period
    - Combo of tribs
- Benefits would occur with a late, but lower than VAMP level, release

# Conclusions

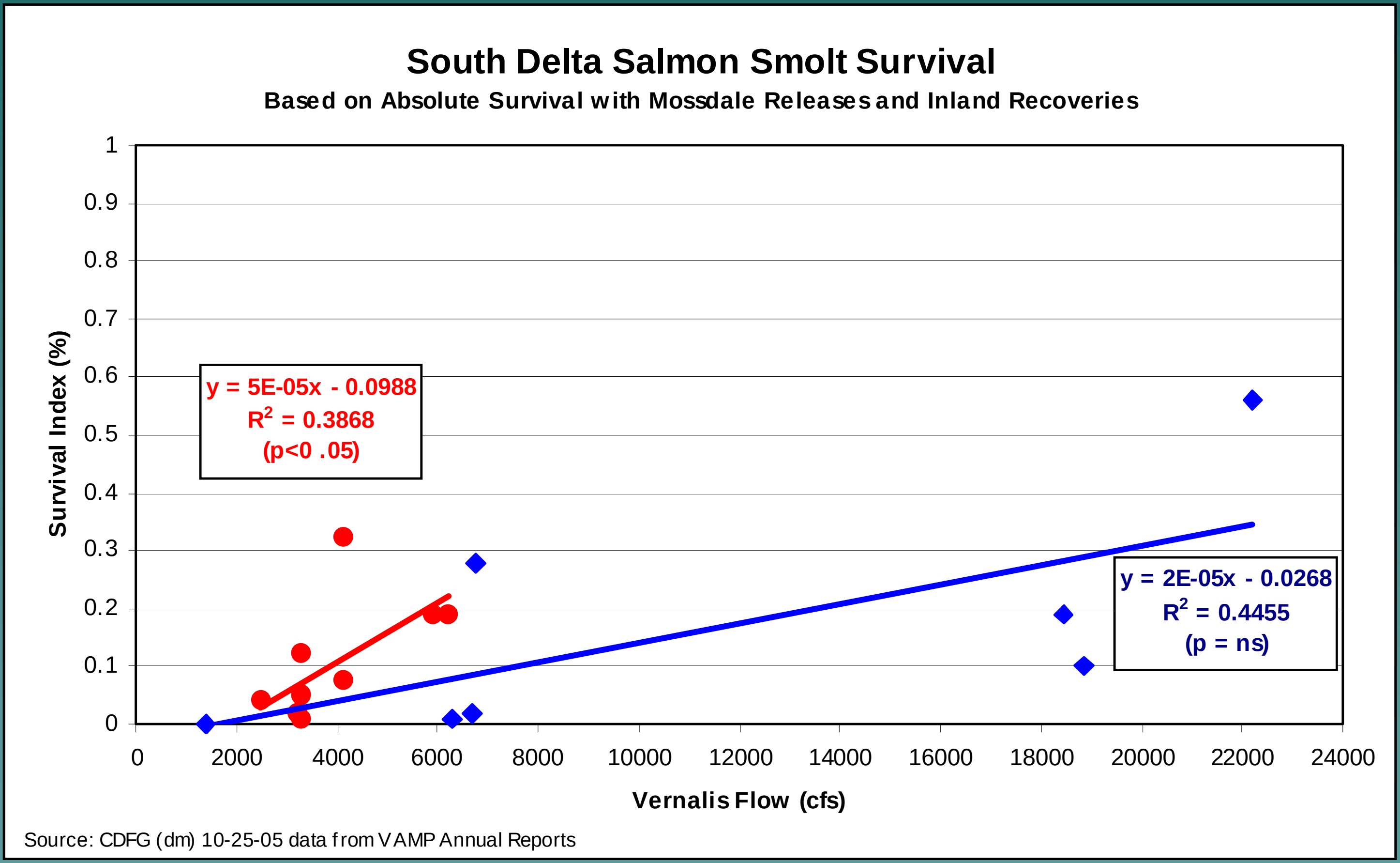
- Duration > Magnitude
- Late > Early
- Apr. 1 to May 31 window
- HORB in assumed

# EWA & Regulatory Actions

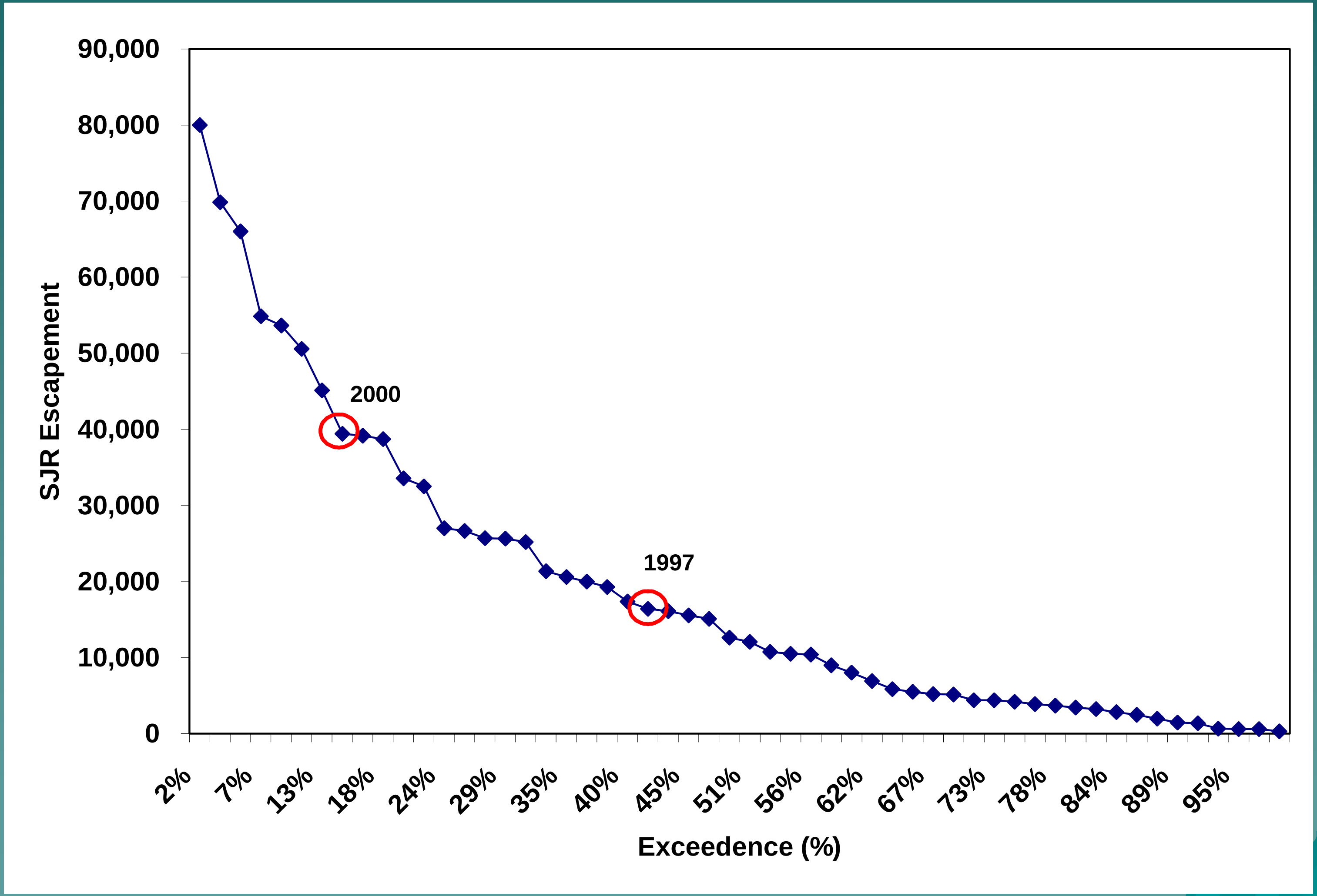
- SWRCB
  - Delta Plan
- New Melones ROP
- FERC
  - Tuolumne
- RWQCB
  - TMDL
  - Basin Plan

# Questions

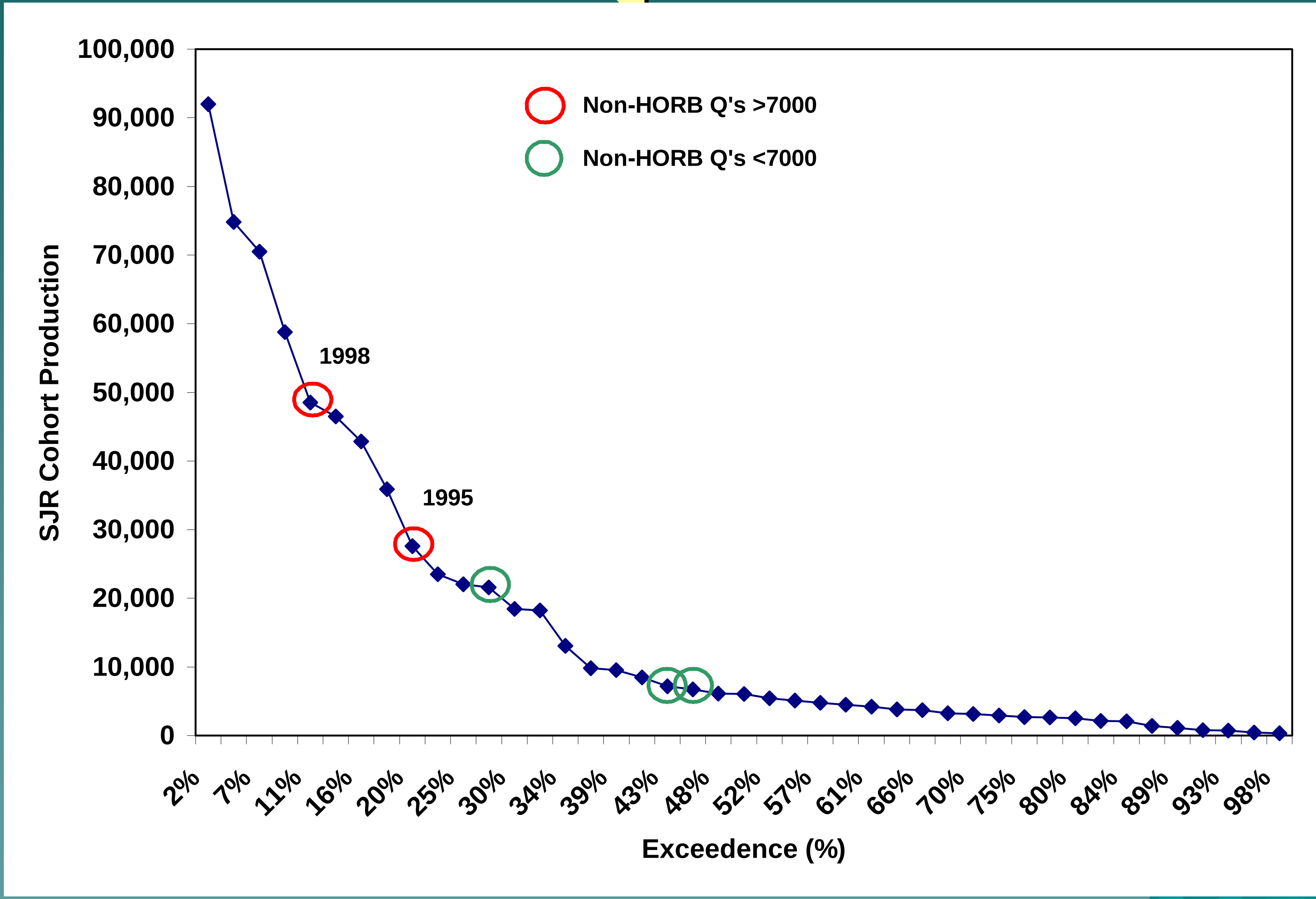
# Non-HORB Survival



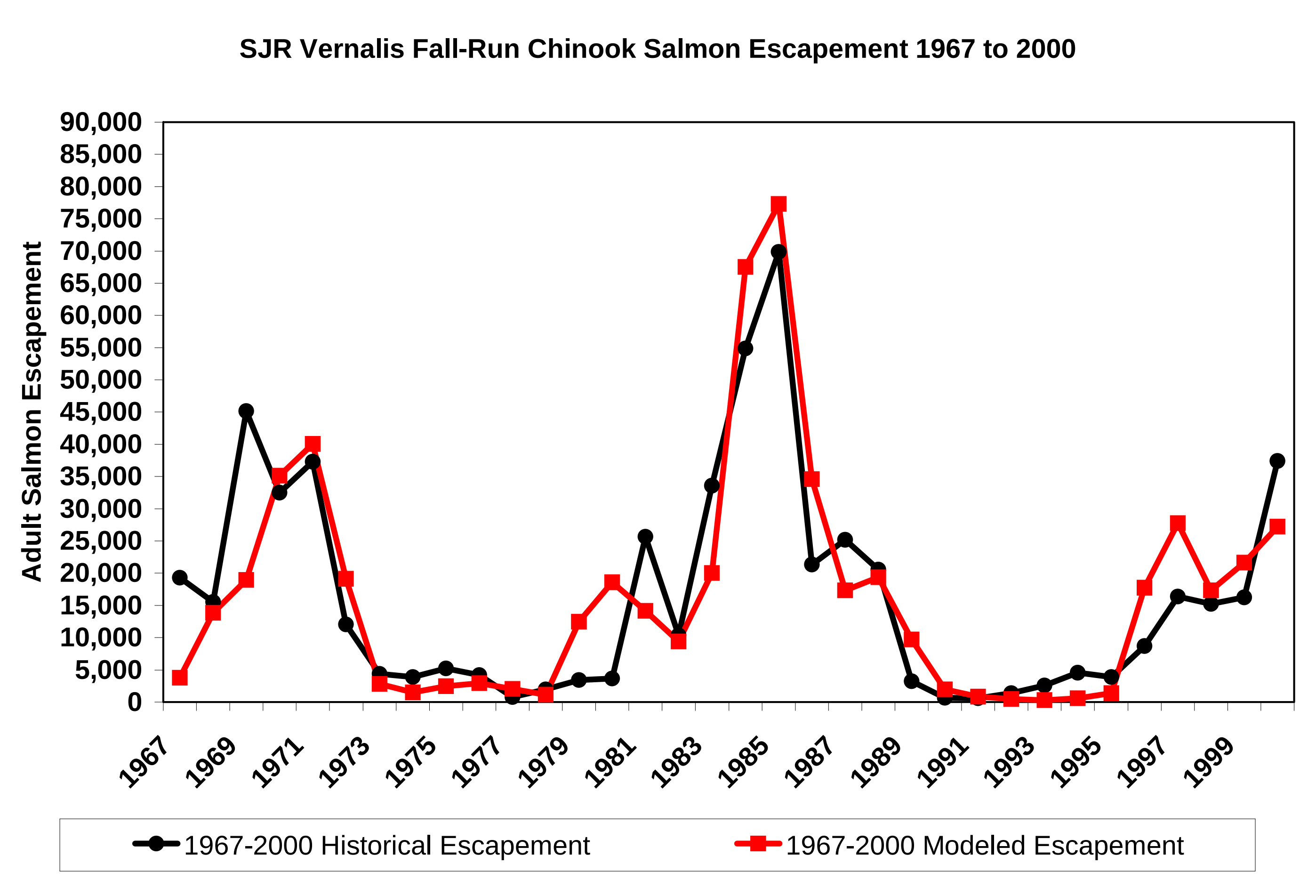
# Non-HORB Q & Escapement



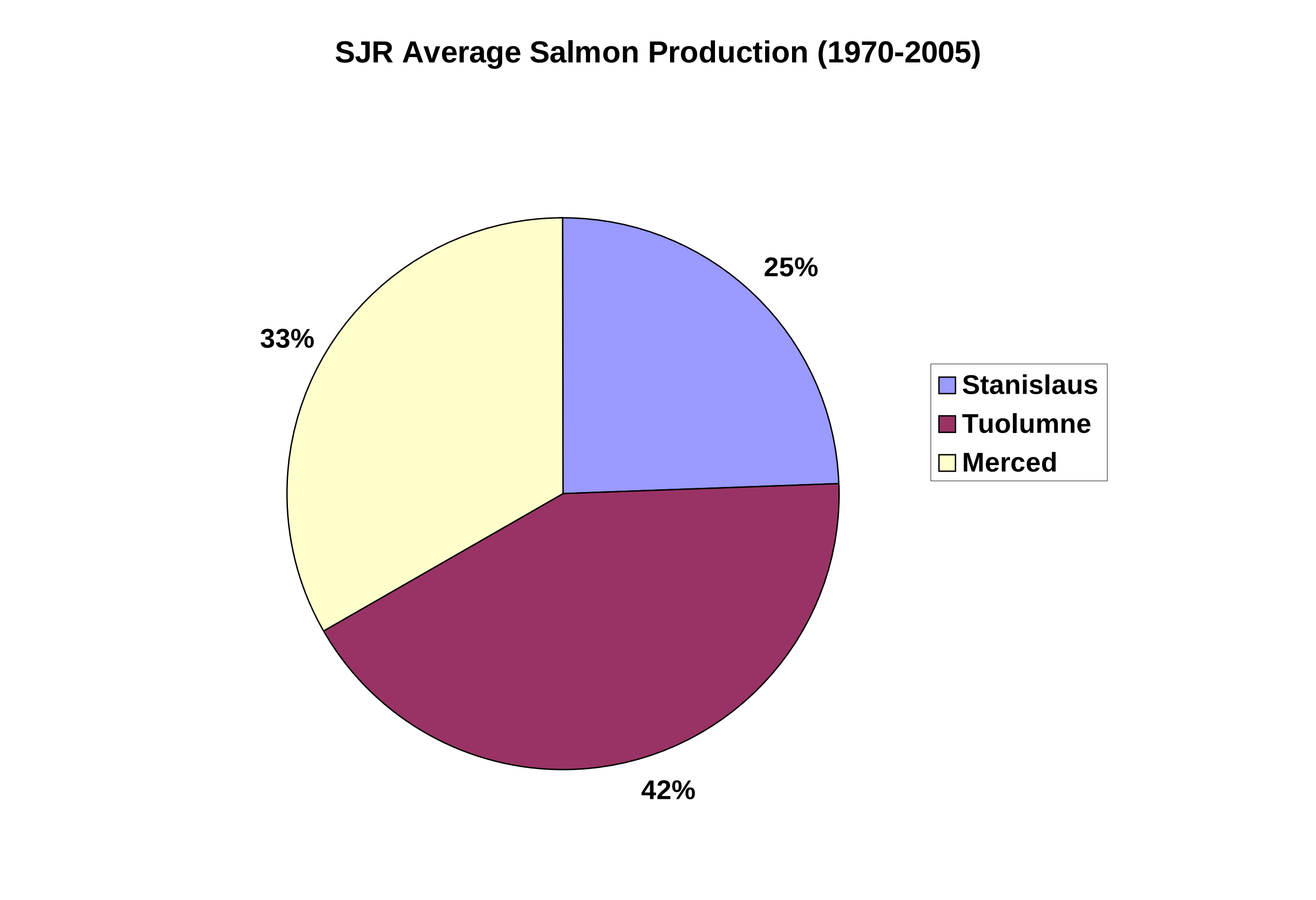
# Non-HORB Q & BY Cohort



# Calibration



# SJR Tributary Escapement



# Model Input ("knobs")

- Regression Variables
- Age Composition
- HORB In/Out
- HORB Years/Duration
- Flow Magnitude/Duration
- Use of Hatchery Production

# Model Output

- Water:
  - Additional water (AF) by year and water year type for VNS and Tribs
- Salmon:
  - Escapement for SJR and Tribs by year and water year type
  - Replacement ratio
  - Hatchery Augmentation

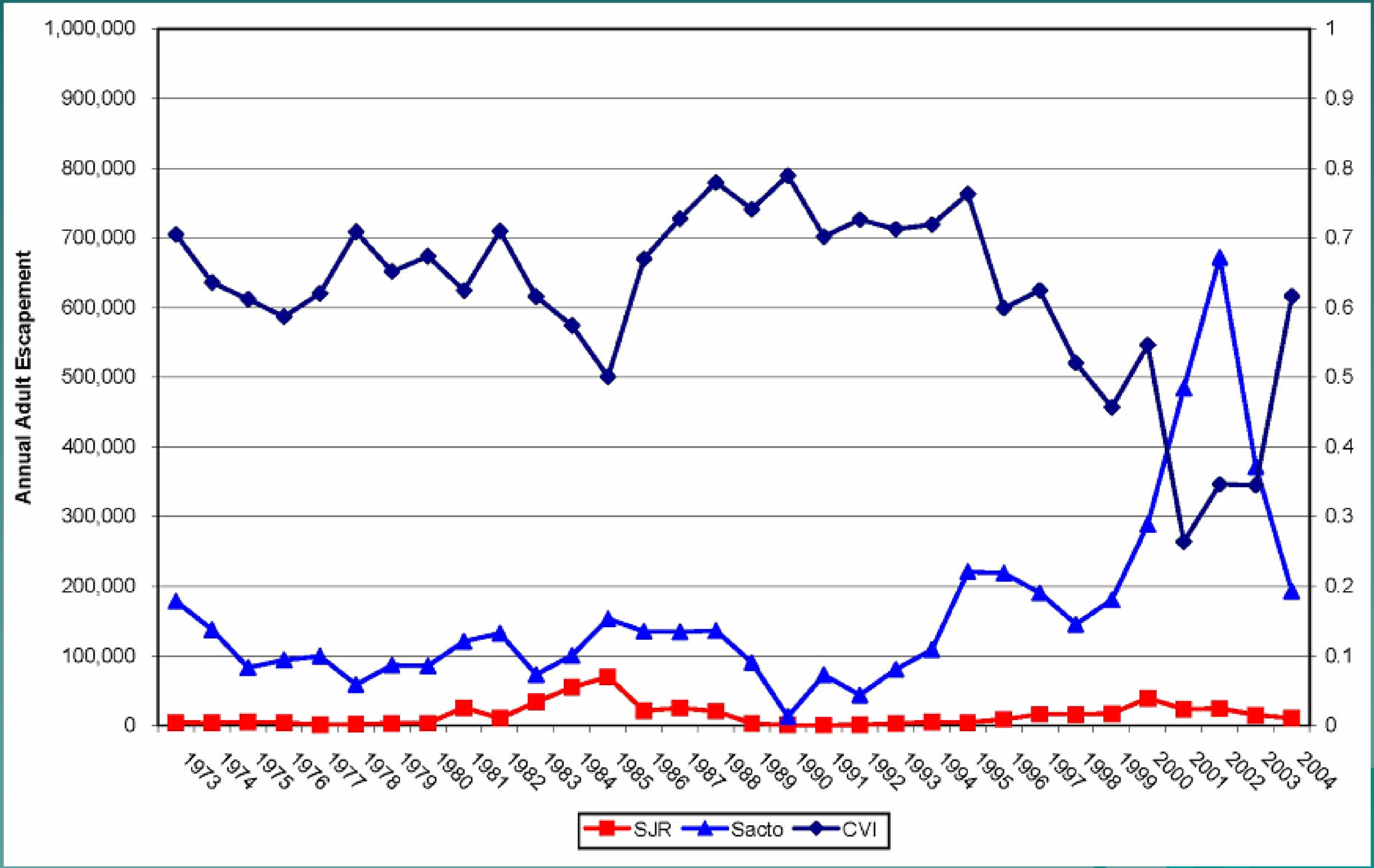
# Model Assumptions

- **Adult salmon population trend not substantially influenced by non-flow factors** *(Harvest, Pumps, Stock density)*
- **Smolt stage predominates adult cohort abundance**

# Result

| Spring Flow Levels | | |
|---|---|---|
| Water Year Type | Vernalis Minimum Flow | Window Days |
| W | 15000 | 70 |
| AN | 10000 | 60 |
| BN | 7000 | 50 |
| D | 5500 | 40 |
| C | 4000 | 30 |

# Ocean Harvest



**S.P. Cramer & Associates, Inc.**
**600 NW Fariss Rd**
**Gresham, OR 97030**
**(503) 491-9577, FAX (503) 465-1940**
**www.spcramer.com**

# MEMO

TO: Michelle Simpson

FROM: Brian Pyper and Jody Lando

DATE: May 5, 2005

SUBJECT: Review of Statistical Analysis presented in "Issue 8. River Flows: San Joaquin River at Airport Way Bridge … Comments of the California Department of Fish and Game"

The following review address addresses primary points of concern pertaining to the California Department of Fish and Game (CDFG) Issue 8 analysis, but is not intended to be exhaustive.

The analysis provided by the California Dep. of Fish and Game should be considered as "highly speculative" given the following:

- misrepresentation of smolt migration relative to flow in the VAMP period
- limited sample size
- unsubstantiated survival rate estimates
- the lack of confounding effects other than flow (i.e. temperature, fry migration)
- inference outside the range of the predictive data set
- reliance on strictly linear relationships without the consideration of density dependence
- unsupported inclusion of production as a function of flow in compound escapement estimates
- the use of Sacramento Basin data to estimate adult cohort abundance
- unconventional calculations of percent increase for various metrics
- the lack of supporting evidence for smolt survival as a function of flow reflected in the returning adult escapement cohort
- additional concerns regarding flow projections, data exclusion criteria and effectiveness assumptions

Statistical relationships and conclusions can be misleading if drawn without critical review and corroboration. As such, we have attempted to point out weaknesses or concerns that require more rigorous investigation or justification.

(1) Figure 1 of the CDFG comments documents the cumulative percent of salmon smolt catch passing Mossdale from 1988-2004. Although 50% of the salmon smolts migrate outside the VAMP window, the flows from 1988-2004 vary widely. Without consistency in the flow conditions, conclusions drawn from Figure 1 are highly uncertain.

Figure 1 of this report illustrates the proportion of April-May smolt migration occurring in the VAMP Period. Accounting for flow variation, it shows that in low water years, a greater proportion of smolt migration occur in the VAMP time period. Consequently a majority of benefits occur in the VAMP time period. This is best illustrated in the following table:

**Table 1: Percent Increase in Smolt Migration from Adjusted Flow Targets**

| | **Vernalis Flow Targets** | | | | |
|---|---|---|---|---|---|
| | **3200** | **4450** | **5700** | **7000** | **10000** |
| **vamp** | 63% | 66% | 66% | 66% | 68% |
| **preVAMP** | 13% | 12% | 11% | 11% | 11% |
| **postVAMP** | 24% | 23% | 23% | 22% | 21% |
| **total** | 100% | 100% | 100% | 100% | 100% |

At the target flow of 3200 cfs, 63% of the benefits are accrued in the VAMP time period, while 13 and 24% respectively are attributed to pre and postVAMP time periods. This is an important perspective that was not illustrated in the CDFG analysis.

(2) The key relationship used to calculate possible benefits of flow changes on adult returns is illustrated in their Figures 4 and 5, which depict the possible relationship between flow and smolt survival rate to Chipp's Island from Mossdale. This relationship is based on five years of data (2000-2004). Although it appears that two replicates per year were available, the effective sample size with respect to year-to-year variability in survival rates is essentially five (the number of years); replicates within a year could be considered as "pseudo-replicates" and must be treated cautiously. It is therefore difficult to assess the validity of the linear relationship used in this analysis.

(3) Furthermore, there is no indication of the quality of the survival-rate estimates or the

potential confounding effects (other than flow) that may also account for the observed variability in the estimates. For example the contribution of fry in wet years may artificially inflate perceived smolt survival with regard to regression of April-May flow and escapement 2.5 years later. In the Stanislaus Fisheries Summary the following statement was made – "The estimated poor survival of juveniles rearing in the Delta in dry and normal water years may be caused by a variety of factors such as predation; entrainment at numerous small, unscreened diversions; unsuitable water quality; and/or direct mortality at the state and federal pumping facilities in the Delta. Entrainment at the Delta pumping facilities does not appear to occur during very wet years since tagged fry were only collected at the pumping facilities during dry years (Brandes and McLain 2001)."

(4) In many years and/or periods, this highly uncertain flow-survival relationship was then applied outside of the flow range for which it was derived (approximately 2500-6000 cfs). With so few data, it is unclear if a linear relationship is even valid within the observed flow range; to extend inferences outside the range (e.g., below 2000 or above 7000 cfs) must be considered as "speculative" at best. In addition, the report forced the linear flow vs. survival regression (Figures 4 and 5) through zero to avoid survival estimates greater than 100%. Rather than applying a statistical fit without biological justification, a logistic regression assuming binomial data would be more defensible and should be investigated. Biological systems are notoriously complicated to represent statistically, typically requiring much more robust data sets, and rarely do they function within linear confines.

(5) The remainder of the analysis (all flow periods, flow intervals, years, benefits and compounding escapement values) was then predicated on the observed flow-survival relationship discussed above. All relationships were assumed to be linear (density independent) , which may not be reasonable. For example, the relationship between Chipps Outmigrants and Cohort Production, for which the linear regression line was forced through zero (Figure 7), potentially indicates a Beverton-Holt type relationship between smolt and adult production. Such a relationship would imply that fewer adults are produced per smolt as smolt abundance increases, which in turn implies a less-than-proportional benefit of flow increases on adult returns. Similarly, the "compounding escapement" analysis assumed a linear relationship between spawners and smolts, and hence, there is no assumption of density-dependence in any of these calculations. A density-dependent relationship between spawners and smolts would also imply lower benefits than calculated in the analysis.

(6) An additional concern pertaining to "compounding escapement" pertains to the assumed, but not justified relationship between flow and production. Until this point, the CDFG analysis related flow to survival. However at this stage, there is an unsubstantiated assertion that the number of smolts at Mossdale is a function of flow. So the compound escapement reflects a potential overestimate in density independent survival as a function of flow coupled with an assumption of production benefits. These assertions could dramatically and erroneously inflate compound escapement estimates.

(7) Assignment of returning adults to cohort was based on coded wire tag return data from the Sacramento Basin. It would be more appropriate to use one of the following data sets in decreasing order of preference 1) age data from San Joaquin Basin adult scales in recent years, 2) coded wire tag return data from the San Joaquin basin, and 3) length frequency distribution of adults returning to the San Joaquin Basin (Figure 3, Figure 6, Figure 7, Table 14).

(8) The CDFG analysis presented percent change for various metrics throughout the report (Tables 5, 8, 9, 10, 11, 12, 13 and 15). For example in Table 5, the increases in adult salmon escapement during VAMP ranged from 14-59 % which seems plausible. However the calculation was computed based on total increase, not according to the base production level. While this is unconventional, we can not state it is incorrect, rather potentially misleading. The calculation was made as follows:

$P_2/(P_1-1)$ where $P_2$ is the new production level and $P_1$ is the original production level

Had the calculations been computed according to standard statistical procedures $(P_2-P_1)/P_1$, the percent change would have ranged from 16-144%. Such a magnitude change appears excessive and would have likely prompted additional scrutiny. This calculation method was consistently applied throughout the report.

(9) Given the tentative nature of the relationship between flow and survival postulated in Figure 5 of the analysis, it is informative to examine the relationships between flow, Mossdale smolt estimates, and the returning adult escapement cohort. These data were provided in Table 14 of the analysis, and represent 13 years of data (rather than just five years) with high contrast in average Vernalis flows during the VAMP period (roughly 1,000 to 20,000 cfs across years). To the extent that the benefits analysis of Figure 5 is accurate, we would

expect a strong relationship between flow and adult returns per smolt (i.e., Mossdale smolt-to-adult survival rate). These data are plotted in Figure 2 of this analysis. The most striking aspect of the adult-per-smolt time series is the strong increasing time trend; this trend is only weakly related to Vernalis flow (Figure 2a and 2b of this analysis). Although smolt-to-adult survival tends to increases with higher flow, the relationship is highly uncertain (the correlation between the two is not significant) and much weaker than the (extrapolated) relationship used by the California Dep. of Fish and Game (their Figure 5). Note that forcing this relationship through "zero" would not be consistent with the data; an alternative would be to fit a nonlinear relationship. Similarly, there is only a weak apparent relationship between adult returns per spawner and flows (Figure 3 of this analysis). Note that in contrast to adult-per-smolt estimates, values of adults per spawner were relatively high during low-flow escapement years 1990-1993. The inconsistency of these relationships imply that several critical factors determining adult abundances are not accounted for and that, over the 13-year period examined, there is little evidence of a strong flow-survival relationship of the nature used in calculations by the California Dep. of Fish and Game.

Table 2 below illustrates the Percent Change in Predicted Subsequent Escapement given adjusted VAMP flow levels using the regression derived from Table 5 data in contrast to the regression derived from Table 14. Table 5 clearly generates a much larger response, particularly during the low flows of the late 1980's and early 1990's. Although we can not attribute this difference to a specific environmental variable, it does raise concerns regarding conclusions drawn from Table 5 regression equations.

**Table 2: Percent Change in Predicted Subsequent Escapement**

| Year | Vernalis Flow Actual VAMP (Table 3) | Predicted Subsequent Escapement (Table 3) | Vernalis Flow Adjusted VAMP (Table 4) | Predicted Subsequent Escapement (Table 4) | Percent Change: Table 5 regression | Percent Change: Table 14 regression |
|---|---|---|---|---|---|---|
| 1988 | 2093 | 2559 | 3200 | 3913 | 35% | 6% |
| 1989 | 2168 | 9496 | 3200 | 14016 | 32% | 6% |
| 1990 | 1280 | 362 | 3200 | 905 | 60% | 11% |
| 1991 | 1048 | 680 | 3200 | 2076 | 67% | 13% |
| 1992 | 1250 | 371 | 3200 | 950 | 61% | 11% |
| 1993 | 3915 | 1160 | 4450 | 1319 | 12% | 3% |
| 1994 | 2110 | 1087 | 3200 | 1649 | 34% | 6% |
| 1995 | 19636 | 2502 | 19636 | 2502 | 0% | 0% |
| 1996 | 6501 | 6564 | 7000 | 7068 | 7% | 2% |
| 1997 | 5314 | 1761 | 5700 | 1889 | 7% | 2% |
| 1998 | 19381 | 20896 | 19381 | 20896 | 0% | 0% |
| 1999 | 6892 | 1267 | 7000 | 1287 | 2% | 1% |
| 2000 | 5873 | 1439 | 5873 | 1439 | 0% | 0% |
| 2001 | 4049 | 2896 | 4049 | 2896 | 0% | 0% |
| 2002 | 3300 | 2792 | 3300 | 2792 | 0% | 0% |
| 2003 | 3223 | 2074 | 3223 | 2074 | 0% | 0% |
| 2004 | 3157 | 2032 | 3157 | 2032 | 0% | 0% |

* Percent Change was calculated as {Esc-Table 3/(Esc-Table 4 - 1)} to be consistent with CDFG methods

(10) We observed potential problems in Table 4 with the reporting of Vernalis Flow in 1993, 1996, 1997 and 1999. Table 4 was intended to project the response to VAMP flows adjusted to be a minimum of 3200 cfs. However for the years in question, the adjusted flows were all historically greater than 3200. Therefore it is unclear why the flows were simulated at the levels presented in Table 4. However, without clarification on this point, we simply relied on the reported values.

(11) Table 9 footnotes that the 1995 smolt migration data point was removed from the escapement analysis. The justification was that >90% of the smolts outmigrated after May 15 with an average from of >20,000 cfs which effectively "swamped improvements made by much smaller flow increments in other years". While this is true, 1989 had a similarly large effects on the results. It accounted for approximately 1/3 of benefits attributed to an expanded preVAMP time period, and >50% of the benefits attributed to an expanded postVAMP time period. In general, the exclusion of outliers based on statistical merit is not warranted unless there is corroborating biological merit. However if 1995 data is to be

excluded, the same criteria should be applied to 1989 data.

(12) It is premature to evaluate the effectiveness of the current standard as it was not implemented until 1999. Assuming that BY 1998 (1999 outmigrants) and since have been affected by implementation of the 1995 WQCP measures, we only have complete data for two cohorts assuming that fi sh may return at up to 5 years of age. With few fish returning at Age 5, it is reasonable to say that we actually have complete return data for three cohorts (BY 1999 and BY 2000). Regardless, three data points are not adequate to account for variability between water year types and to evaluate the potential influence of the current protective measures.

Given the multitude of concerns raised in this critique and the importance of the related management issues, we recommend that the statistical analysis be modified and expanded to address the points above. Conclusions that can be defensibly stated should be highlighted and those that are uncertain should be stated as such. In most cases, we need to collect additional data and consider more biologically reasonable statistical methods. Defensible identification of the linkages between environmental conditions and biological requires more robust data, analysis and assumptions than presented in the CDFG comments. Until that time, it appears premature to modify the current management procedures.

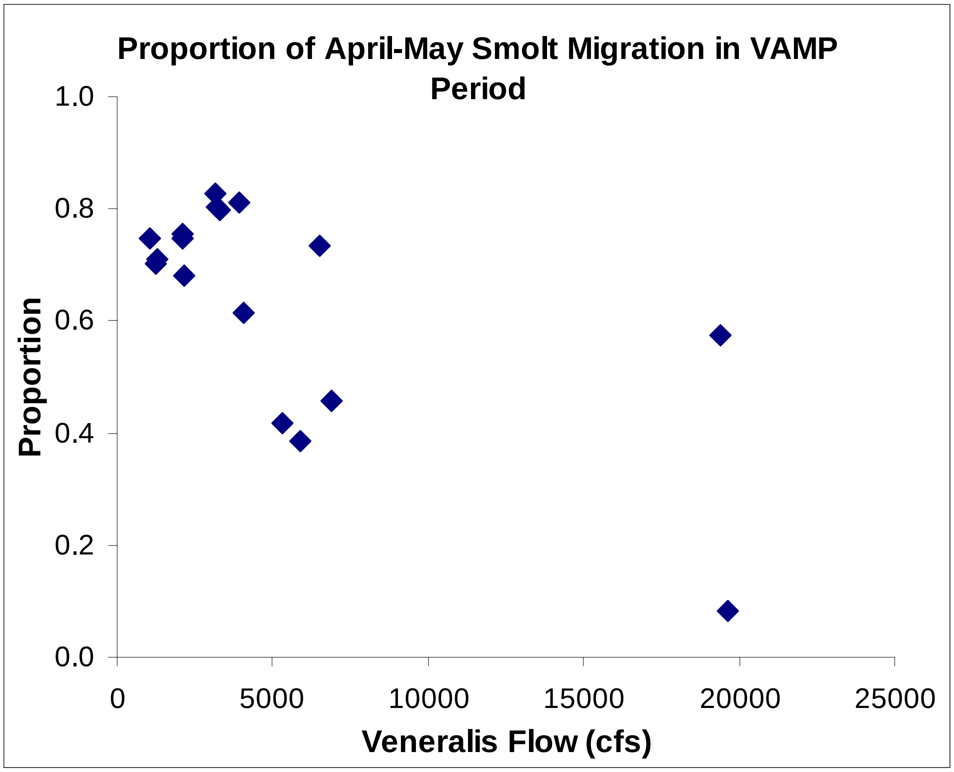



**Figure 1.** Proportion of April-May smolt migration occurring in the VAMP Period.

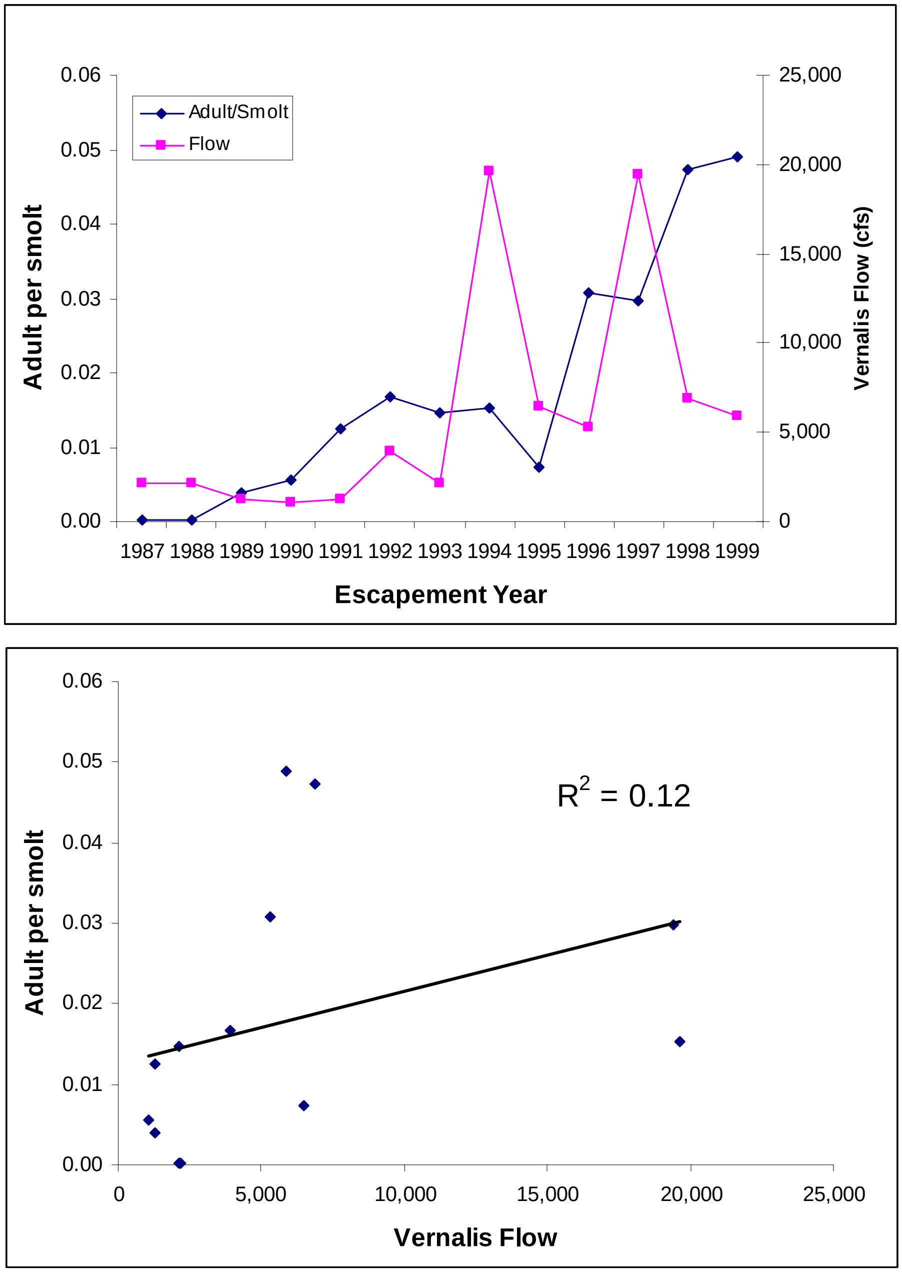
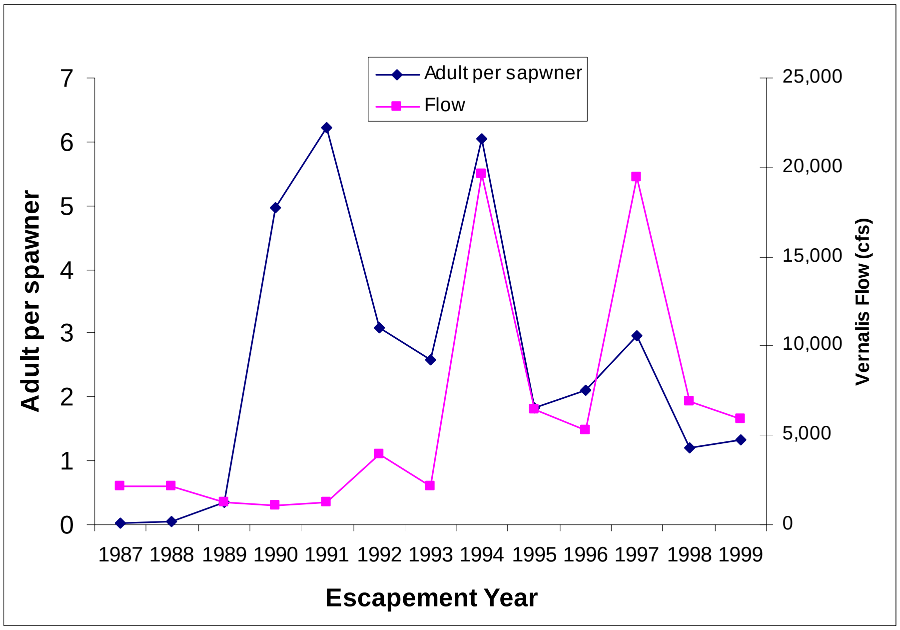



**Figure 2.** Top panel: Adult per smolt and Vernalis flow data as reported in Table 14. Bottom panel: Adult per smolt versus flow.



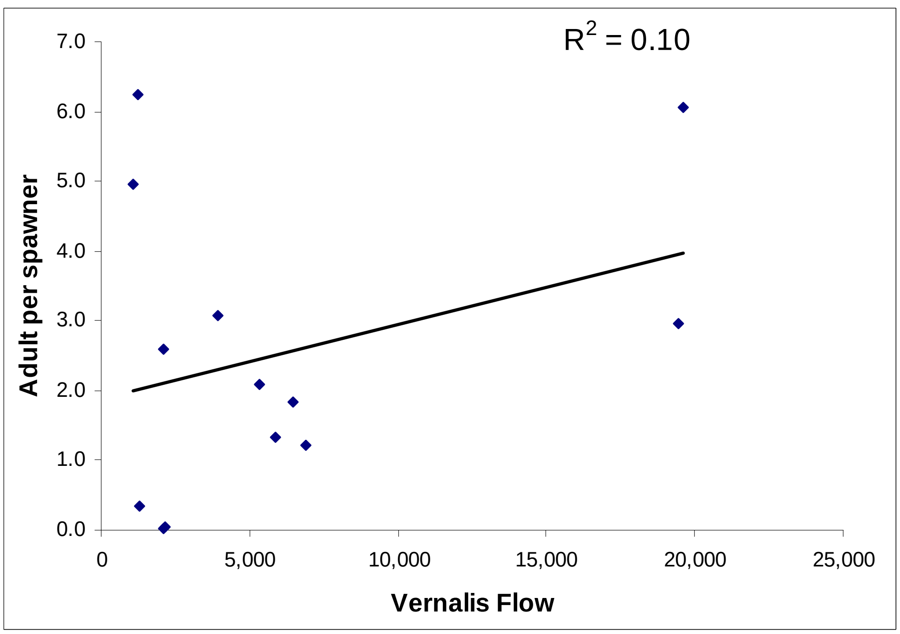




**Figure 3.** Top panel: Adult per spawer and Vernalis flow data as reported in Table 14. Bottom panel: Adult per spawner versus flow.

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9/13/2006

**Review of Final Draft 11-28-05 San Joaquin River Fall-run Chinook Salmon Population Model**

This report describes an empirical model based on regression relationships derived from historical data relating salmon survival to flow for each step of the population cycle. The model does an outstanding job of fitting the historical escapement record using an empirical approach. Although I have reservations about QA/QC with spreadsheet models, such a model is accessible to those without a PhD in statistics (unlike some other models of salmon dynamics), and might lead to greater use by stakeholders. I hope the comments below are helpful.

Yetta Jager
Environmental Sciences Division

Improving Predictive Reliability. The predictive reliability of the model can be assessed by holding out data to test against for validation purposes. Alternatively, the variation associated with predictions can be assessed by removing a handful of data and refitting the model's equations using a bootstrap approach to quantify how different the model's predictions are using parameters fitted to different subsets of years.

Extrapolation is a problem for empirical models. Any flow and temperature scenarios considered should be within the range of values that occurred during the time that the empirical relationships were fitted. Likewise, if something not included in the model (like density dependence) were to become more important in future, the model would not be able to extrapolate to the new situation.

Another way of avoiding problems with extrapolation is to use a model form that is bounded to give reasonable values. For example, I would recommend something other than a power model form for survival because if extrapolation were to occur, you could get unreasonable estimates (i.e., values greater than 1 or less than zero). Logistic models are often used, or exponential models, S = a exp(-bX), so that only values between zero and one are possible for any X. To be specific, you might consider fitting the following equation using linear regression methods:

$$logit(S) = ln\left(\frac{S}{1-S}\right) = \beta_0 + \beta_1 X_1 + \beta_2 X_2 \quad (1)$$

Where S is the survival you want to predict and $X_i$ are predictors like temperature (Figure 16) or Vernalis flow/exports (Figure 22, 35, 40). Figure 6 already seems to use a logistic function. To back-calculate survival, use:

$$S = \frac{1}{1+\exp\left(-\left[\beta_0 + \beta_1 X_1 + \beta_2 X_2\right]\right)} \quad (2)$$

Density dependence: I agree, in principle, that physical barriers and those sorts of engineering solutions may not be the best long-term fixes, and that focusing on DD could be used as a red herring, but I also think that this SJR model will be much more acceptable to those on all sides of the table if it allows for density dependence, which after all is a biological certainty beyond some density, particularly with the reduced amount of spawning habitat available after damming (see Achord et al. 2003). The results of our California Energy Commission-funded study of RST data in the Tuolumne River suggest that there is some density dependence because outmigrant estimates do not vary nearly as much as spawner abundances. However, there may be some disagreement between seining and RST data in this regard. A positive relationship between spawner density and fry density (Figure 28) does not contradict the density-dependence hypothesis, especially when the relationship clearly levels off beyond 12,000 females. Thus, again to avoid unreasonable predictions at high spawner densities, I don't see that it would be that much harder to fit a non-linear model to relate Mossdale smolts to spawner abundance and Vernalis flow. On page 18, a relationship to get smolt abundance at Mossdale from escapement and spring Vernalis flow is described as multi-linear. The Ricker equation below, which is what we used in the CEC analysis for daily data, could be used as an alternative. Equation 3 below shows a general or extended form of the Ricker that allows one to include other environmental predictors (e.g., Vernalis flow, Q) for calculating smolt outmigrants, $Y_t$ for each year *t.*

$$Y_t = Esc_t \cdot e^{b_0 + b_1 Esc_t + b_2 Q_t} \quad (3)$$

Linearizing equation (3) allows this to be fit using linear regression.

$$\log_e\left(\frac{Y_t}{Esc_t}\right) = b_0 + b_1 Esc_t + b_2 Q_t \quad (4)$$

Another advantage of using this relationship is that it will not predict any smolts when there are no spawners. A Beverton-Holt relationship with covariates could also be used, and there is a generalized stock-recruitment model that permits even more flexibility and fits both types (see Jager 2000).

Collinearity: I would probably not choose an alternative statistical approach to deal with this issue, but the interpretation of the model should be carefully worded to acknowledge and describe collinearity between flow and other variables. My experience with Tuolumne data and that of Speed (1993) suggest that escapement covaries with flow. Presenting correlations or collinearity diagnostics, or graphically showing that there are years with high spawner density and low flow and vice versa, would address this for spawner density. After reading the SJRGA review, it appears the outlier year 1989 might be such a year, and that keeping it in the dataset might address this issue in part, but it would be good to add more years that break the correlation.

In another example, Newman and Rice (1998, 2003) mention that exposure to salinity in coastal areas covaries with flow. In their analysis, salinity is the 2nd best predictor, after release temperature (which therefore has to be controlled for in any analysis). I don't know if additional data or experiments have been done since to measure survival under conditions with low flow-low salinity or high flow-high salinity.

Newman and Rice's (1998) analysis of ocean survival data concluded that the export effect was mildly negative, which suggests overall agreement with this study in that the effect is not statistically significant. In both cases, there is a possibility raised that this is due to covariation with flow – i.e., fish released into the delta when gates are open may benefit from increased water flow (yet another predictor correlated with flow).

Collinearity does not harm prediction – predictions using the model with both covariates are best, and reduced models might be equally good. However, the parameter estimates are unstable and interpreting the relative importance of the predictors is therefore problematic. For example, in describing the importance of flow, it would be important to explain that a number of different causal mechanisms might exist by which flow is beneficial. I suppose there are good reasons to believe that flow is causally related to both escapement and salinity.

Path analysis (structural modeling) might be a good exploratory tool for examining these variables together and quantifying the various routes through which flow is influencing survival. This would be an interesting research problem in its own right.

<u>Escapement reconstruction:</u> The population replacement rate is the ratio of the size of each 3-year old cohort with the size of the cohort that produced it 3-years earlier (it is unclear if this is restricted to females as it should be). The replacement ratio is used to calibrate the model on page "Output". I am not sure of the details of how this is used -- is a solve block is used to implement the calibration or is it done using trial and error, by adjusting what input parameter(s)?

From the standpoint of population viability analysis, this ratio is comparable to lambda, and it is encouraging that it is greater than one. However, the variance on this ratio is also very important from a conservation perspective because these runs have huge variation, cyclically dipping to very low levels that, without straying, could reach zero. The approach I used in calibrating the PVA model for the Tuolumne in my dissertation (Jager 2000) was to calibrate the variation in escapement.

Second, I used "functional calibration", which compares relationships between population predictions and environmental predictors. I calibrated the relationship between spawner abundance and annual flow by adjusting smolt survivals for each hydrologic year type. One parameter in my model, the ENSO $R^2$ was adjusted to obtain agreement for the relationship between escapement and the ENSO-SOI index, which was basically non-existent. Correlations between escapement and the following variables: flow lagged by 2, and 3 years, commercial fishing effort, sport effort, and combined effort lagged by one year, were all compared, but not used to make adjustments. The SJRGA review recommends using harvest data in the analysis – this may be a way to use it as a check, without revising the model to incorporate it. Of course, if harvest explains a lot of residual variation, then one would want to incorporate that data into the model.

I have not fully digested the argument made in the SJRGA review about calibrating juvenile outmigrants rather than escapement, but it could only be more informative to check fit at different points in the life cycle. My concern would be the relative quality of the types of data used for comparison. Validation reflects on the data as well as the model. Using high quality, independent data that has not already been used in the model should be a priority.

I don't have any objection to calibrating against the replacement ratio, but I agree that it would be nice to evaluate other predictions as well. It is difficult for me to suggest a calibration approach without knowing as well as the authors do what parameters are likely to influence the relationships of interest, but here are some things that could be checked:

1) The most obvious type of validation is to compare observed and predicted escapement by measuring the goodness-of-fit of a regression relationship between them, where the intercept =0 means no bias and root mean square error=0 means the model is an efficient estimator of

true escapement. This test would require the model to capture variation better than the replacement ratio alone (see discussion in previous paragraph).

2) One might be interested in the correlation among escapement predicted for the three populations and that in the data. If the tributary populations are highly correlated, this is bad because they may all wink out at the same time. It is not immediately obvious to me what parameters would influence this.
3) Functional relationships not explicitly in the model might also be compared. Here, comparing the correlation between escapement and flow and between escapement and density might reveal differences in response.

Autocorrelation: Autocorrelation is an issue when testing for significance of an estimated parameter in one of the model's equations, when using the confidence intervals on parameters. It also inflates the $R^2$ of each individual equation. In addition, autocorrelation in an exogenous variable (e.g., flow) can inflate the estimated density dependence in some models (Williams and Leibhold 1995) -- obviously not this one, but if the changes above are implemented, this might become a concern.

It is not that difficult to take autocorrelation into account and if you are re-doing the analyses, I would go ahead and do it. In my opinion, there is no point in testing for autocorrelation using a Durbin-Watson test because it is quite certain you will find it. The approach I would recommend is using generalized least squares and modeling the covariance of residuals – this can be done very easily in SAS's Proc Mixed with about 5 statements to solve everything simultaneously. The exponential covariance model in Equation 5 represents an exponential decay in autocorrelation over time, which is what I'd recommend using. According to this model, the expected correlation between pairs of residuals is smaller when they are separated by more years. Equation 5 would be fit to the residuals of the survival regression equation to estimate λ simultaneously with the other model parameters. If you don't have access to SAS and are using another software (e.g., R or Splus), you would first get residuals using the ordinary least squares solutions for the survival models, then fit Equation 5 to residuals, construct the appropriate variance-covariance matrix (the covariance between values for year i and j in the dataset is C(i-j)), and re-solve the original regression equation for survival using generalized least squares. Technically, the estimates are biased if you solve one and then get residuals and solve the other, but that's splitting statistical hairs. I believe Proc Mixed deals with that issue in a manner that would be technically acceptable to statisticians.

$$C(\Delta t) = e^{-\lambda \Delta t} \tag{5}$$

In some cases (probably not here), including lagged predictors (flows etc.) is an alternative option for dealing with autocorrelation. Keep in mind that collinearity will increase if lagged predictors are added. Also, the SJRGA review suggests that perhaps the predictors for the survival equations for different stages should be more carefully separated (i.e., not using flow during the same period to predict survival of two successive stages), which argues against using lagged predictors. However, this might be a reasonable approach for the escapement-reconstruction equation (see Stenseth approach below).

It might be worth exploring whether a statistical approach exists for solving the combined system simultaneously. Stenseth et al. (1999) showed how three equations for survival and recruitment, which included density dependence in early life stages, reduce to an ARMA(2,1) model. Because the Stenseth et al. model includes different lags, it is possible to simultaneously estimate the proportional influence of previous cohorts by fitting the time series parameters (e.g., for lags of 2,3,4,5 years). Advantages of the Stenseth-type model are that 1) it is stochastic and gives confidence bounds on its predictions, 2) parameter estimates for all of the different equations can be obtained simultaneously-no calibration, and 3) it would avoid double counting of effects. However, it is probably not possible to do this with exogenous covariates (e.g., see Zabel et al. (2006)).

**References**


Achord, S., P. S. Levin, and R. W. Zabel. 2003. Density-dependent mortality in Pacific salmon: the ghost of impacts past? Ecology letters 6:335-342.

Jager, H. I. 2000. Predicting the viability of fish populations in a modified riverine environment. Ph.D. Dissertation. University of Tennessee, Knoxville.

Newman, K. B., and J. Rice. 2002. Modeling the survival of Chinook salmon smolts outmigrating through the lower Sacramento River system. Journal of the American Statistical Association 97:983-993.

Stenseth, N. C., O. N. Bjornstad, W. Falck, J.-M. Fromentin, J. Gjosaeter, and J. S. Gray. 1999. Dynamics of coastal cod populations: intra and intercohort density dependence and stochastic processes. Proceedings: Biological Sciences 266:1645-1654.

Williams, D.W. and A.M. Leibhold. 1995. Detection of delayed density dependence: Effects of autocorrelation in an exogenous factor. Ecology 76(3): 1005-1008.

Zabel, R. W., M.D. Scheuerell, M. M. Mcclure, and J. G. Williams. 2006. The interplay between climate variability and density dependence in the population viability of Chinook salmon. Conservation Biology 20:190-200.

**San Joaquin River Fall-run Chinook Salmon Population Model**

**External Scientific Review Form**

**Reviewer:** #2

**Review:**

1. **Problem/Goals.** Is the problem that the project is designed to address adequately described? Are the goals, objectives and hypotheses clearly stated and internally consistent?

   Yes. Introduction is very clearly written. Objectives and history are well described

2. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal?

   No. The regression analysis and model structure does not provide an objective comparison of alternate hypotheses driving smolt production and escapement in the SJR.

3. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

   The regression analysis is reasonably documented but the analysis has some significant flaws (failure to recognize colinearity among Delta export and Vernalis flow regression parameters).

   The model documentation is very poor. The model structure and fitting procedure is very weak, thus the model cannot be used to evaluate alternate policy options. As such its likelihood of ‘success’ is very low.

4. **Project Performance Evaluation Plan.** Will a monitoring plan be developed to document changes in the restored habitat over time and the response of salmonids and/or riparian vegetation to the restoration in a scientifically rigorous manner?

   Not relevant to the report. It is clear, from a scientific/learning perspective, that monitoring of smolt abundance under 5700 and 7000 cfs test flows is required to provide informative contrasts in the data to determine the extent to which flow controls smolt production. How else could one determine whether more water = more fish?

5. **Expected Products/Outcomes.** Are products of value likely from the project?

   No. The modeling and analysis is very weak. Vernalis flow may very well be an important limiting factor on the SJR population. However, based on the analysis that is presented in this report, it remains an open question as to whether Delta exports, Vernalis flow, or both is the key flow-determinant, and the extent to which marine survival, ocean harvest, and freshwater habitat limiting the population. The strength of the author's conclusion that Vernalis flow is the key factor given the ambiguity in the data, and the decisions used in constructing the model, dropping outliers, etc., brings into question the objectivity of the analyst.

**<u>Additional Questions:</u>**

**General:**

The purpose of the model is to develop spring flow magnitude, duration, and frequency instream flow levels into the South Delta to adequately protect, and restore, fall-run Chinook salmon in the San Joaquin River basin. To accomplish this objective, please address the topics listed below for these questions:

Is the model adequate?

No. The population model has many flaws:

- It was very difficult to understand the model structure, parameterization, and uncertainty estimates. The non-standard presentation in the text suggests that the author has little experience with population modeling.

- Confusion in terminology makes it very difficult to understand the model structure. For example, the sentence "The first estimation parameter the model predicts is the total number of smolts…" (p. 18). The model <u>predicts</u> changes in <u>state variables</u> like smolt abundance. Smolt abundance is not a <u>parameter</u>. There is no <u>estimation</u> in the model because the parameters are fixed or adjusted by the user. The model simply makes predictions based on 'hardwired' parameter values. This confusion in terminology brings into question whether the author has sufficient background in population modeling.

- The model does not include any density dependence. This is simply ridiculous as it implies that the population can grow to infinite size or should have gone extinct years ago. I am somewhat surprised that the model doesn't show this behavior. I suspect there are a number of ad-hoc traps in the model code to address this problem, which could have implications to policy-relevant predictions. I looked at the spreadsheet model that was supplied. It was very difficult to follow given the plethora of VLOOKUP and IF statements that were part of the model equations.

- The model does not consider other explanatory relationships (e.g. variability in ocean survival, impact of Delta exports) that could be important determinants of smolt production and escapement that would in turn lead to fundamentally different conclusions about the CDFG flow objectives. The model in no way validates or confirms the importance of Vernalis flow since it doesn't allow us to evaluate whether other explanatory variables can be used to predict the historical escapement pattern. Even if it did, the ad-hoc way of tuning the model, rather than formally fitting the parameters to data using a maximum likelihood approach, doesn't allow for formal evaluation of alternate hypotheses.

- It is unclear how the model was 'tuned' to fit the historical escapement data (p. 22). The parameters that were adjusted were not identified. From looking at the spreadsheet, it appears that both key input data (Vernalis flow by day) and parameter values (e.g. regression slopes) are adjustable. Given so much latitude, it is not surprising that the model can reproduce the escapement trend fairly well. What the author fails to understand is that there are likely many other ways to fit the data just as well (different parameterizations) that would make very different policy predictions. These alternate parameterizations were not explored. Thus, uncertainty in policy-relevant predictions is not determined.

If not, how can model be improved?

The list is long but here are the key issues:

1. Model must allow users to select alternate flow time series (e.g. Delta exports) to drive predictions (not just Vernalis flow).
2. Model must include density dependence.
3. Model should be fit to the data using an objective method (maximum likelihood). This will allow rigorous and objective evaluation of alternate hypotheses and quantification of uncertainty in model predictions.

1. Foundation (justification)
2. Logic
3. Numeric representations
4. Application and reliability
5. Conclusions
6. Calibration and validation
7. Documentation
8. Testing (i.e. what monitoring could occur to validate or reject model predictions)

**Specific:**

**<u>Hydrology</u>**

1. Are the methods used in the Model (including Model Report) relating to flow sufficiently documented? If not, what improvements can be made to improve documentation?

No. There is very little documentation on how historic and projected flows at Vernalis are constructed. I am not even sure if the historic, or simulated data was used to calibrate the model.

2. What is the best metric (i.e. arithmetic mean, geometric mean, transformed data etc) that can be developed to adequately capture the variability in spring flow (i.e. magnitude and duration) on an intra-annual basis?

   I am not a hydrologist. Consult Maidment. D.R. 1993. Handbook of hydrology. McGraw-Hill Inc.

3. What improvements to hydrologic data utilization can be made to enhance model prediction performance reliability?

   As stated above, alternate flow time series should be used as input to the model to determine the extent to which they explain trends in escapement and smolt data.

4. Is there evidence of auto-correlation in flow calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

   For one thing it would be useful to see time series plot of Vernalis Flow, Delta inflows, and Delta Exports on one graph to answer this question. I am concerned that co-variation among these inputs makes it difficult/impossible to tease-out which is the key driver for smolt production. This is not so much an issue of auto-correlation (this is a correlation of values over time) as it is co-variation (correlation of two different variables over time, such as Vernalis flow and Delta exports).

5. Are there additional flow metrics, parameters, logic etc. that should be incorporated into model logic and function? If so, what are they and how can they be assimilated into the model (reference to logic and function)?

   Yes. See my comments above for including other flow variables in the model such as Delta exports.

**<u>Biology</u>**

1. Are the methods used in the Model (including Model Report) relating to fish abundance and/or production sufficiently documented? If not, what improvements can be made to improve documentation?

   No. See my comments above on poor documentation and confusion in terminology.

2. What improvements to fish data utilization can be made to enhance model prediction performance reliability?

Doesn't seem like a relevant question to this model as there is no formal habitat suitability component. I don't think I am clear on what this question is getting at.

4. Is there a way to improve how the model performs fish abundance prediction calculations and/or processing of fishery data?

   Model does not include any density dependence. See comments on model above and detailed comments below. As far as I could tell from the spreadsheet, it does not remove fish to force the predictions with the historical catch, or to allow one to evaluate the effects of alternate harvesting policies.

5. Is there evidence of auto-correlation in fish related calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

   Auto-correlation will influence stock-recruitment parameter estimates (see Chapter 7 of Hilborn and Walters 1992) through effects of time series bias. However this issue is somewhat moot given the basic mistakes used in analyzing the stock-recruitment data in the report. The stock-recruitment analysis that is presented is extremely rudimentary and violates fundamental principles of population dynamics. Confusion in terms, incorrect interpretations of data, and non-standard assumptions (e.g., linear relationship between escapement and recruitment) suggests the author has little experience with stock-recruitment analysis.

6. Does justification exist to include additional fish metrics, parameters, logic etc. in model logic and function (i.e. ocean harvest and/or Delta export entrainment)? If so, what are they and how can they be numerically assimilated into the model (reference to logic and function)?

   This seems to be a leading question, but I agree with it. There is no point in constructing a model that does not include the full range of management alternatives. By only including Vernalis flow and hatchery augmentation, there is no way of evaluating other alternatives like Delta exports or ocean harvest. This basic mistake suggests either that the author has either a very biased perspective, or has little experience with resource management modeling.

7. How can model representation of hatchery production, and underlying model logic, be improved upon?

   The details on the hatchery production model were lacking in the document. From what I gather hatchery fish are assumed to have the same reproductive fitness as wild fish when the return to spawn naturally. This is not supported by the literature (see detailed comment below). I also think that hatchery smolts are not subject to flow effects (the numbers reaching the delta are a simple function of the number of broodstock taken). This makes no sense, is inconsistent with the structure assumed for wild fish, and will likely provide an overly optimistic assessment of hatchery benefits.

8. Currently the model predicts a constant ocean survival rate (i.e. relationship between cohort abundance and Chipps Island abundance is constant). Is there a need to make this relationship variable? If so, how can this be numerically accomplished in model performance?

   Decadal-scale variation in marine survival rates for Pacific salmon have been well documented and are widely acknowledged to be a very important (if not the most important) determinant of escapement. A stochastic element could easily be added to this type of model, or alternatively, the model could be run at low, medium, and high levels of marine survival.

   The policy implications are enormous. Under a reasonable marine survival rate the population may still increase under lower flows. The doubling objective will be met, but it will take longer to attain compared to a scenario with a high marine survival rate. At low marine survival rate, the population may not be sustainable unless freshwater survival rate is improved. The model needs to be able to show these scenarios.

9. The model currently uses an adult replacement ratio of 1:1 as a numerically identified population health barometer. Is there a need to refine this ratio? What additional population parameter(s) could be incorporated into model logic and function?

   This term is misused in the text. By definition, the replacement ratio is 1:1. Tracking whether the overall survival rate for each cohort is above 1 seems like a sensible metric as it determines whether the population is sustainable or not. If the ratio is <1 for many years the population will of course decline to low levels. Tracking the abundance of the population in the model will therefore capture the effects of the overall survival rate. Another metric that should be captured is the % of hatchery returns in the escapement.

**Statistics**

1. Currently the model uses liner relationships between flow and fish production because this relationship provides the strongest correlation value. Is it necessary to include a model toggle switch, model logic, and mathematical functions, that allow users the option to test a variety of non-linear relationships between flow and fish survival and/or production upon model results?

   Yes. See my comments above.

2. What is the statistical reliability of model out-put given that model predictions propagate? How can model reliability be improved?

There is no statistical reliability to the model. The tuning procedure is ad-hoc and no objective means of assessing model fit is used. As a result, uncertainty in model predictions cannot be quantified. A maximum likelihood approach should be used to estimate model parameters and quantify uncertainty. These are standard procedures that are commonly used in fisheries stock assessment models.

3. Is colinearity present in model logic and/or computation, and what influence does it have upon model results? If present how can it be removed?

A regression analysis was used to select the most important determinant of SJR escapement. The analysis was flawed from the start by assuming that only a single variable (Vernalis flow, Delta exports, ocean harvest, <u>or</u> escapement) could influence the population, rather than a combination of factors. In addition, trends in ocean survival rate were not considered as one of the explanatory variables, a decision that is very inconsistent with the plethora of literature on effects of marine survival on salmon return rates.

The author failed to substantively recognize the extent of confounding between Delta flows and Vernalis flows on smolt production. The correlation in Delta export and Vernalis flow was not explicitly documented in the report, but the correlation was qualitatively described (p. 14) and can be evaluated (with difficulty) in Fig. 22. Given the regression results, there does not appear to be sufficient contrast to be able to determine which of these factors is the most important to the population. This sharply contrasts with the author's conclusion that Vernalis flow, and not Delta exports, is the key determinant.

4. In some cases, model predictions for salmon production occur outside the empirical data set range used to develop the regression. What limitations in model reliability result?

5. Presently smolt survival has a statistically significant regression correlation with Delta inflow level (i.e. less than 7,000). No statistically significant regression correlation for juvenile smolt survival and Delta export level exists. However when inflow to export ratio is regressed against flow survival, a moderate regression correlation occurs. Currently, exports are not included as a model prediction parameter. Should exports be included as a model prediction parameter (for smolt production)?

YES. See comments above

6. Are the methods used in the Model (including Model Report) relating to statistical evaluations and/or model logic justification sufficiently documented? If not, what improvements can be made to improve documentation?

No. The documentation is poor and does not follow the standard approach of sequentially writing down each equation with symbols used to denote parameters, which are then listed in a separate table along with the assumed or estimated values. The fact that the author has not done this suggests a lack of familiarity with modeling.

7. What improvements to statistical use and application can be made to enhance model prediction performance reliability?

   See comments above re. maximum likelihood estimation.

8. There is substantial disagreement amongst scientists regarding the issue of density dependent mortality and its influence upon SJR salmon abundance (e.g. fall spawner abundance and spawning habitat availability: aka redd superimposition). In the absence of flow the relationship between spawner abundance and stock recruit appears to show density dependence (i.e. Beaverton-Holt or other density dependent type relationship). However when flow is included with spawner abundance, in the form of a multiple-regression using spawner abundance and spring flow regressed against adult recruits, a significant correlation exists suggesting that density dependence does not explain the variation in SJR adult salmon escapement abundance. How can this issue be resolved with data to date, or in the future if data insufficiency exists currently?

   The analysis that is desired is relatively straightforward and described in modern undergraduate fisheries texts (e.g. Hilborn and Walters 1992, See Chapter 7 and p. 294 for a discussion on this exact topic). Density dependence must exist, however there may not be enough contrast in the data to separate out density vs. flow effects if both high densities and high flows occur in the same years. In the analysis that was presented, a linear relationship was used in the abundance/flow relationship. This is nonsensical as it assumes no density dependence at any stock size. This issue could readily be resolved (within the limits of information in the data) by a fisheries scientist with experience in analyzing stock-recruitment data.

9. How can the statistical relationships between flow and fish survival and/or fish production be improved?

   I have not reviewed the methods used to determine smolt abundance. Obviously these numbers must be reliable. A relationship between biases in abundance estimates and flow could lead to spurious conclusions about the effects of flow, so I would watch for this when computing smolt abundance data.

   The flow-survival relationship parameters should be directly estimated in the model (rather than computed independently via regression) along with other model parameters within a maximum likelihood estimation framework. Under this framework, confounding between the survival-flow relationship and other factors (e.g. escapement) will be apparent when analyzing model output. Most importantly, the increased uncertainty in model predictions that is driven by potential confounding will be accurately quantified. These approaches have been well documented and are in constant use in resource management and stock assessment fields (see Hilborn and Mangel, 1997, The ecological detective. Confronting models with data, Princeton University Press).

**Miscellaneous comments:**

Figures and tables were difficult to interpret without proper captions.

p. 10, 2nd paragraph. The strong correlation between flow and adult returns does not necessarily imply causality. For example, the high abundance during the mid-1980's (Fig. 1) was a coast wide-phenomena seen from California to BC. It is widely acknowledged as a period of high-marine survival. Flow may have an important influence on Chinook production during some periods, but it is overstating the case to say that production is largely driven by flow. It would be helpful to assign years to the data points in Fig. 3, and to add time series of spring flows at Vernalis, and marine survival (from an adjacent index stock) to Fig. 1.

Figure 1. Does the escapement time series include hatchery contributions?

p. 10, 3rd paragraph. The logic discounting the contribution of fry to future escapements is weak. Item 1 (unknown fry contribution) seems in direct conflict with the main conclusion that fry are not important; 3) is unknown; 4) is irrelevant; and 5) is not substantiated by Fig. 3, which shows the flow-salmon count relationship, not a smolt-adult relationship as the text implies.

Figure 3 (as many others) needs a figure caption. What is meant by the y-axis label "salmon count"? I presume it is the escapement but am unclear on this and whether it includes hatchery returns or not. If it is escapement, why is the maximum escapement in Figure 1 (ca. 70 k) less than the maximum point in Fig. 3 (> 90 k)?

Figure 4-6. Change temperature scale in Fig. 6 to Fahrenheit so the reader can more easily determine the mortality rate at water temperatures shown in Figures 4-5.

p. 13 - last paragraph. Why was ocean survival rate not included as a predictor of Chinook production? The term "Production" should be clarified. I assume it is equivalent to adult escapement.

p. 14 – Delta Exports. The delta-export, delta-inflow, and Vernalis flow – abundance arguments are hard to follow and I think the conclusions are not substantiated. Vernalis flow and export are likely strongly correlated over time (this should be shown) and therefore it is difficult to separate one effect from the other. Although not reported, I suspect that the details of the multiple regression analysis (covariance among Delta and Vernalis flow coefficients) will document this confounding. I am surprised that the survival-export/flow regression has such a low $r^2$ (Fig. 20). It looks like a fairly strong relationship with one outlier, which is not examined/discussed in the text. Why was only a linear relationship examined in this instance while in other cases non-linear relationships were used (e.g. Fig. 16-18)? It looks like a non-linear relationship would fit the data quite well. Hence the conclusion that exports are not an important driver for Chinook production seems tenuous and unsubstantiated.

p. 15, 1st paragraph. Further to my point above, the fact that the spring export data do not improve the flow-escapement relationship is not strong evidence that exports are not important. If one reversed the order of the computation, and first regressed exports and salmon abundance, and then asked how much more variance was explained by adding the Vernalis flow variable, I suspect the opposite conclusion would be reached. The order of variable addition should not alter the conclusion. The problem is that the relationships are confounded and there is not enough contrast in the data to sort out the strength of the two effects. Arguments in the 2nd paragraph demonstrate the lack of understanding of this issue by the author. The ratio of two variables (export to flow) will of course decline with an increase in the denominator (flow), but this does not imply that flow is the more important variable. The same issue applies to argument 2). Argument 3) justifies the conclusion based on the flow-production relationship only. Why is no mention made of the export-survival relationship (Fig. 21)? This seems like a very one-sided analysis.

Fig. 19-22. Survival rates should be logit-transformed prior to regressing them on explanatory variables. Note that the models can predict survival rates $< 0$ or $> 1$.

p. 15 – Ocean Harvest. The conclusion that ocean harvest does not influence escapement in the SJR is not substantiated by the data. Figure 1 shows a substantial increase in escapement between 1995 and 2000, a period when harvest rates dropped (Fig.  25). The regression between escapement and harvest index clearly shows that a single relationship is not adequate. There appear to be two negative relationships reflecting recent (lower) and historic (upper) patterns (labeling data points with years would help show this). Note in both cases the relationship is negative implying that either increased harvest reduces escapement (contrary to the author's conclusion), or harvest rates are reduced when escapements are high (as would occur under a fixed catch policy).

p. 16 – In-river Adult Salmon Density. This paragraph shows some serious misunderstandings about stock-recruitment relationships. The confusion is fist apparent when the authors conclude that fry density must decrease with spawner abundance if density dependent mortality is occurring. This will only be true at high abundance if over-compensation is occurring. Constant fry abundance (y-axis) with increasing spawner abundance (x-axis) is also indicative of density dependent mortality.

The fact that there is a linear relationship between escapement and fry density does not imply a lack of density dependence for the population. If this were the case the SJR population would either be infinite or would have gone extinct long ago. It is very possible that density dependence occurs after the fry life stage. The policy relevant density-dependent relationships that should be examined are between escapement and smolt production (under the authors untested assumption that fry don't contribute to adult recruitment), and escapement and returning adults.

It is not clear from the stock-recruitment analyses that were done whether hatchery contributions or harvest was accounted for. I am also confused as to why an overall stock-recruitment relationship (escapement vs. cohort abundance after harvest is accounted for) was not presented, with residuals compared to various flow indices. This is the standard way of evaluating density dependent vs. environmental effects as described in basic fisheries text (see Hilborn and Walters 1992). The analyses in Tables 1 and 2 are deeply flawed as they assume no density dependence.

p. 17 – Spring Flow. Again, the strength of the conclusion that spring flows are the key determinant of salmon production is not substantiated by the data. The smolt production-flow relationship (Fig. 29) rests on 2 data points (1995 and 1998). While the data do warrant evaluating production at higher flow levels, the conclusion is overstated. As discussed above, it is highly uncertain whether the key variable is Vernalis flow or export.

p. 18. Smolt production is predicted in part based on a linear relationship with escapement. The model will therefore predict that the carrying capacity of SJR is infinite. Although the model description is difficult to follow, there appears to be no density dependent relationship in the model. It is also not clear why the 1989 smolt 'outlier' was removed. I gather because it suggested high smolt abundance with low flow (this outlier should be shown in Fig. 32). The circularity in reasoning is concerning.

p. 18 – Smolt production. It is not clear from the text, whether the model-predicted escapements, or observed escapements, are used to drive smolt predictions. It should be the former, but I suspect the latter. If this is the case then the model structure is deeply flawed (see comment below).

p. 21 – Hatchery augmentation. Hard to follow this. As I understand it, the existing production relationship for the Merced (adults in – adults returned) will be used to drive the simulation. Given this, is hatchery production therefore independent of flow? Are there any competitive impacts on the wild stock? It is also not clear whether hatchery fish contribute to the total number of in-river spawners and therefore subsequent smolt production, or not. If they do, do they have the same fitness as wild-origin spawners? None of these important issues that have been clearly identified in the literature appear to be recognized in the model.

p. 22 - Replacement ratio. This terms is misused. The replacement ratio should be the number of spawners required to keep the population at a stable level. As defined in the text, the replacement ratio is simply the ratio of abundance of parents and returning progeny in any year. That ratio may not be sufficient for replacement. The misuse of this term brings into question the author's familiarity with population dynamics.

p. 22 – Model Constraints. These are not model constraints but desired outcomes. Again, confusion of terminology suggesting lack of familiarity of the subject matter. See Newman and Lindley (2005) for a relevant example of how to document the structure and parameterization of a model.

p. 22 – 'Validation'. The model was not validated in any way so the title of this paragraph is very misleading.

p. 22 – Uncertainty. It is very unclear how confidence intervals on predictions were computed but I gather that the 95% CL's for parameters were used in some way. This may be adequate when predicting an outcome from a single relationship, but is nonsensical when employed in a model composed of multiple relationships. Maximum likelihood or Bayesian estimation is the standard approach.

p. 25 – Model Results. The good fit of the model to the observed escapements is very surprising given the simplistic model structure. I can only conclude that the observed escapements are driving smolt predictions. If I have this right, the survival relationships could imply a non-sustainable situation (returns/escapement consistently < 1) yet the population would persist. If this is the case than the model serves no purpose.

p. 25. Fig. 53. The last figure referenced in the text was 41. There is no text or context for the missing figures.

p. 27 – 3rd paragraph. I strongly disagree with the conclusion that this model provides a tool to predict the amount of flow required to meet the doubling goal. The modeling effort violates many basic modeling approaches and biological principles and is deficient on all fronts (structure, parameter estimation, uncertainty analysis, policy evaluation). There are many better (and published) models (e.g. Newman and Lindley 2005, or simpler versions) that could be modified and applied to this problem.

p. 28. Conclusions regarding hatchery augmentation seem dubious. There is no reference to the large literature (e.g. Ford 2002, Nickelson 2003, HRSG 2004) on impacts of hatchery augmentation on wild stocks, especially weak stocks with low intrinsic rates of population growth such as SJR Chinook. The basics of this issue have been well thought out in the primary literature and do not agree with the optimistic or hopeful tone of the conclusion.

**References**

Ford, M.J. 2002. Selection in captivity during supportive breeding may reduce fitness in the wild. Cons. Biol. 16:815-825.

Hilborn, R. and C.J. Walters. 1992. Quantitative fisheries stock assessment. Chapman and Hall. 570 pp.

HRSG. 2004. Hatchery Scientific Review Group (HSRG)–Lars Mobrand (chair), John Barr, Lee Blankenship, Don Campton, Trevor Evelyn, Tom Flagg, Conrad Mahnken, Robert Piper, Paul Seidel, Lisa Seeb and Bill Smoker. Hatchery Reform: Principles and Recommendations of the HSRG. Long Live the Kings, 1305 Fourth Avenue, Suite 810, Seattle, WA 98101 (available from www.hatcheryreform.org).

Newman, K.B., and S.T. Lindley.  2005. Accounting for demographic and environmental stochasticicity, observation error, and parameter uncertainty in fish population dynamic models. Nor. Am. J. Fish. Manage. 26(3):685-701 Sacramento Chinook)

Nickelson, T. 2003. The influence of hatchery coho salmon (*Oncorhynchus kisutch*) on the productivity of wild Coho salmon populations in Oregon coastal basins. Can. J. Fish. Aquat. Sci. 60: 1050-1056.

**San Joaquin River Fall-run Chinook Salmon Population Model**

**External Scientific Review Form**

**Reviewer:** #3

**Review:**

This is such an important issue, especially with the pending effort to restore salmon on the mainstem San Joaquin River, that I have expanded much more on a number of points than I would normally. I believe that a useful salmon population modeling tool is ripe for development on the SJ but this is not that tool. The authors appear to be using Vernalis flow in much the same way that X2 was used as an ecosystem indicator but there are three significant differences: 1) we know a lot of the mechanisms affecting salmon dynamics which we still don't know for the X2 species, 2) the X2 effort was collaboration of diverse scientists working together in the context of clear connections to relevant ecological literature, and 3) the correlations between X2 and species abundance were not driven by a couple of extreme data points.

1. **<u>Problem/Goals.</u>** Is the problem that the project is designed to address adequately described? Are the goals, objectives and hypotheses clearly stated and internally consistent?

The author identifies four questions
1) the status of SJR salmon populations
2) what level of protection is being afforded salmon smolts outmigrating from the SJR into south delta?
3) what is the status of the VAMP?
4) What influence does spring flow have on SJR salmon production?

Question 1 is quickly answered; SJ salmon are not doing well.
Question 2 is the heart of the SWRCB request that led to this model. By referring to the adequacy of VAMP it is clear that the question is about survival of smolts through the delta.
Question 3 identifies that VAMP has not addressed conditions of 7000 cfs and that it has therefore not yet reduced the uncertainty surrounding the effects of flow and exports on survival of smolts through the south delta.
Question 4 is an extrapolation from Question 2 to cover all the effects of flow in on salmon in the watershed. This, unfortunately, became the goal of this model which now tries to use Vernalis flow as a surrogate for upstream habitat, temperature, transport and all other impacts of flow on salmon.

I am puzzled by the assumption that the SWRCB's request to provide comments on the adequacy of the 1995 Water Quality Control Plan for San Joaquin salmon referred only to the springtime flow requirements. The WQCP not only addresses springtime flows, but also October flow conditions to permit better returns of adults to the San Joaquin River. Are these measures adequate? How have they been implemented? Passing comments are made to the return of fall run adults and there is a reference to the reduced dissolved oxygen problem near Stockton, but no discussion of adult migration exists in this report. Does the Department feel that adult returns are not an issue or that the 1995 WQCP is fully adequate?

From this discussion of the WQCP inadequacies and concerns, this report clearly attempts to support the model's assumption that flow at Vernalis is the only issue controlling the abundance of San Joaquin salmon. The goal of this report then becomes to conclude that the size and duration of the flows in the WQCP are inadequate and that the recommendations given to the SWRCB by DFG are based on a reasonable interpretation of all available information. I do not comment on the structure of the model, which could be very useful if given the data relevant to delta survival. Instead most of my comments will address the model's choice of variables and input data.

The executive summary concludes with a discussion of the use of hatcheries which seems entirely separate from the SWRCB request for the evaluation of flows. Unlike the treatment of other factors affecting the abundance of salmon, no evidence is presented that hatchery production has affected escapement to the SJR. Amongst the suggested caveats are efforts to protect the genetic integrity of SJR salmon. However, the footnote on page 21 refers to a report that there is no genetic distinction between Sacramento and San Joaquin stocks. The authors of that report conclude that the previous 140 year of hatchery operations n the Central Valley are to blame for the loss of genetic integrity. As with most recent work on the effects of hatcheries, this suggests that greater use of hatcheries is a dangerous tool to use in managing salmon stocks.

2. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal? No.

The SWRCB asked only for what flows were needed to protect smolt passage through the delta . The model attempts to use flow at Vernalis as representing two separate things – not only the influences of flow (and the Head of Old River barrier) on smolt passage through the delta but also as a surrogate for all effects of flow on the tributaries. To assess delta survival the model could be used and fitted to survival data either from the CWT studies by FWS and/or with ocean recovery data. Unfortunately, because of this second use, the model has been fitted to overall abundance estimates which are driven by many factors that are related to Vernalis flow but separate from it. In fitting the abundance estimates to flow, the analysis is overwhelmed by two flood years when abundance was high and this results in model results that simply suggest that all years should be flood years.

For just the purpose of assessing flow needs at Vernalis the author correctly states that the data are not adequate to address the issue and flows in the intermediate range (7000 cfs) will be required. The clear recognition of this fact on p 10, where the author suggests that the SWRCB should ensure that such flows occur in the second half of VAMP, seems completely at odds with the description of the construction of the model that relies on the same data that the author describes as inadequate.

3. **<u>Feasibility.</u>** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

The structure of the model is clear. The decisions on which variables to include are much less clear. Many factors are inappropriately left out of the model for its use as an overall population model. Two factors are included without clear evidence that they are significant controllers of escapement: the role of number of days of flow and the relationship between outmigrant smolt abundance and subsequent abundance of adults. As stated above, the attempt to make a model to predict escapement far exceeds the grasp of the available data as well as the request cited from the SWRCB.

4. **<u>Project Performance Evaluation Plan.</u>** Will a monitoring plan be developed to document changes in the restored habitat over time and the response of salmonids and/or riparian vegetation to the restoration in a scientifically rigorous manner?

N/A

5. **<u>Expected Products/Outcomes.</u>** Are products of value likely from the project?

No. The model results provide a linear interpolation between data from flood years and low flow years. Without data from intermediate flow years the outputs of this model cannot be supported.

**<u>Additional Questions:</u>**

**General:**
The purpose of the model is to develop spring flow magnitude, duration, and frequency instream flow levels into the South Delta to adequately protect, and restore, fall-run Chinook salmon in the San Joaquin River basin. To accomplish this objective, please address the topics listed below for these questions:

Please note comments above about modeling survival through the delta vs. modeling SJR salmon recruitment. Below I address this model as a model of salmon escapement.

Is the model adequate?

No, not as a tool to manage SJ salmon freshwater life stages. It ignores significant factors, it gives unsupportable reasons for leaving our other factors and it does not address factors affecting fry or adult abundances and it fails to make the case that SJ smolt production controls adult production.

If not, how can model be improved?

There are two paths that this effort could pursue much more successfully:

1. Feed into this model data relevant to the survival of smolts through the delta. These data are already referred to in the report but they are a trivial part of the effort to model the entire population dynamics of SJ salmon by reference to flow at Vernalis. Sadly, as the author explains, the VAMP needs to apply its higher flow levels if we are to have any hope of relevant data about the impacts of flow and exports on success of smolt passage.
2. Develop a biological model, perhaps something like Steve Kramer's winter-run model, to incorporate all the several strands of information that have been developed for SJ salmon. This would not only tie together recent developments in temperature modeling, but allow inclusion of all factors that might affect salmon production and evaluate which ones are important, which ones we need to know more about, and how to integrate things like the in-stream effects of flow on each tributary with other management options like gravel restoration and Vernalis flow.

The model as it currently exists largely can only recommend that we have flood year flows every year. Doubtless that would solve various salmon problems but it is not useful guidance to management or research.

1. Foundation (justification)
2. Logic
3. Numeric representations
4. Application and reliability
5. Conclusions
6. Calibration and validation
7. Documentation
8. Testing (i.e. what monitoring could occur to validate or reject model predictions)

The Justification and logic (1&2) of the model fail to support the assumptions of the model and therefore I have written about each section:

Discussion of Fry importance. p. 10-11.
The author's justification for the focus on smolts rather than fry are inconsistent with my understanding of salmon biology (presumably they mean in regard to conditions in the delta)
1 – 'the fry contribution to escapement is unknown' – ignorance of something does not make it unimportant (the more I read that sentence the more peculiar it becomes).
2 – 'the fact that fry are abundant in the delta in years that are usually productive of adults is negated by the fact that smolts are also abundant in those years.' Clearly since smolts can only come from fry, than an abundance of smolts and fry suggest a high production of young in those years and it is impossible to assess the relative contributions of fry and smolt from the data in hand.
3 –'Low dissolved oxygen is problematic in the Stockton Deep Water Ship Channel', but fry are abundant in the delta only in wet years when SJR flows are high. The latest report from the people working on the Stockton Deepwater Ship Channel DO issue conclude that SJR flows greater than 2000 cfs negate any DO problems
4 – Smolts from all years return as adults – but their relative roles in regard to fry that reared in the delta is unknown.

5 – 'There is a strong correlation with smolt production and adult cohort production' which is simply a repeat of number 2 and still does not account for the fact that years of high abundance of fry in the delta also shows a strong correlation with cohort production.

In other systems it is common that salmon display an assortment of life history patterns (known as 'phenotypic plasticity' across years and in different habitats such that they may rely on fry growth in downstream areas in wet years but on fry and smolt growth in upstream areas in drier years. The author's attempt to homogenize salmon life histories undermines many of the analyses in this report.

The author goes on to recommend the "cessation of late winter/early spring freshets" to reduce the movement of fry downstream. This recommendation is not only counter to the idea of phenotypic plasticity but comes despite the author's later description of how higher flows likely increase spawning areas in the tributaries. I am unaware of any other salmon management efforts that suggest reducing spawning areas and reducing transport of young downstream.

Flow at Vernalis and its correlates. p 11 &12.
The author describes how flow at Vernalis is tightly correlated with flows from the three tributaries, a result that is singularly unsurprising. The author uses this correlation to justify their attention on Vernalis flows alone, as a surrogate for all other upstream flow effects. This is use of Vernalis flows leads to several unfortunate implications. Other methods have been discussed to enhance Vernalis flows that will have no impact on conditions in the tributaries – recirculation of releases from the Delta-Mendota pool and restoration of flows from the San Joaquin. In addition it leaves the possibility of serious discrepancies among the tributary flows; if the Merced is contributing higher flows to Vernalis, that will not improve conditions on the Tuolumne or Merced. A lot of excellent work has gone into temperature modeling of the tributary streams but more remains to be done. This use of Vernalis as a surrogate for upstream conditions suggests that such work is not necessary.

Even on the tributaries, the author's strong correlations of temperature with flow provides a weak basis for recommending flows. Historical conditions reflect the relationship of reservoir inflow and reservoir releases. If higher releases are mandated at lower reservoir inflows (i.e. when precipitation and the snowpack are smaller than in the historic relationship, the same level of decreased temperature per increase in flow is unlikely to be attained. Such uncertainties are well addressed by the temperature modeling that is going on, but this model does not reflect such interactions.

The flows at Vernalis are shown to correlate well both with the flows on the tributaries and with the San Joaquin River Index that incorporates information on reservoir storage as well as current precipitation patterns. Strong linear relationships are presented tying all of these flow parameters together. However, as they move into developing their model, the author chooses to translate these linear relationships into the standard 5 year-types and thereby obscure the fact that in many years the timing of flows changes considerably so that a below-normal year might have a wet winter but provide very warm, low-flow conditions in spring when smolt are at risk. Retaining the physical connections between reservoir inflows, reservoir releases, conditions in the tributary streams and their relationship to conditions at Vernalis would provide a model that reflected the known ties amongst various aspects of flow and their impacts on different life stages of salmon, This model does not do that.

Delta exports. p. 14
The author accurately reiterates the history that led to development of the VAMP – all years when salmon production was high occurred when the San Joaquin was in flood with Vernalis flows in excess of 10,000 cfs. All other years production was poor and flows were less than 2,000 cfs. VAMP is an experimental condition that tries to determine what controllable flows in the 2000-10000 range might accomplish and what impacts there are of export operations that have almost always exceeded Vernalis flows in all but flood times. As the author points out on page 10, VAMP has not had the experimental conditions that would allow the impacts of different export rates at the same flow rate to be evaluated, nor have we had the higher range of VAMP conditions in any of the first six years. Nevertheless, the author plows ahead to resolve this issue, for which they have already acknowledged the absence of appropriate data. To address this use he uses multiple regression techniques on 7 data points. This is not the statistical approach behind the VAMP design and is inconsistent with the common guidance that one needs at least 10 data points for <u>each</u> variable entered into a multiple regression model. Not surprisingly, this attempt yields results that are difficult to interpret and which run counter to any hypotheses about the impact of exports on smolt survival. In footnote 19 the author recognizes that no consistent results come out of any permutations of this analysis. Despite this evidence that the data cannot support the analyses performed, the author combines the results with conclusions that adults returns reflect the importance of flood conditions on recruitment to argue that exports do not affect smolt survival. It is unfortunate that such unsupportable analyses were the basis for official recommendations from the State Department of Fish and Game on an extremely contentious issue amongst stakeholders and management agencies.

Comparable analyses would suggest that the Head of Old River Barrier must be bad for salmon because it only can be installed in drier years when production has always been low. In fact, this may reflect the thinking of the author, since the recommendations would prevent installation or use of a barrier at the head of Old River in all but dry and critical years.

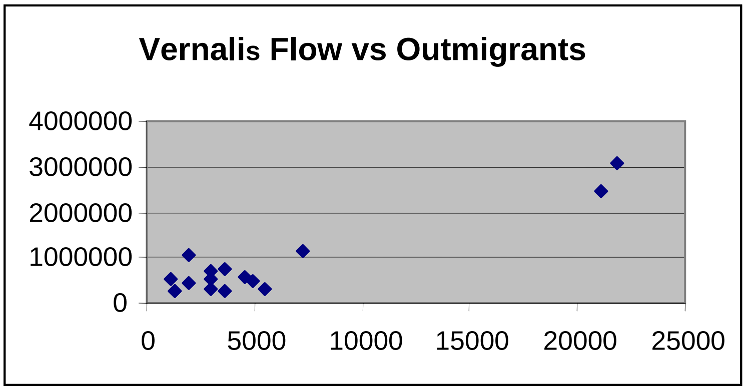
Ocean Harvest. p. 15
The author evaluate the importance of ocean harvest on SJR salmon abundance by examining the correlation/regression between SJR escapement and the Central Valley Harvest Index. Despite finding a significant relationship, the author concludes that ocean harvest is not important and do not include it in their model. Given that adult abundance is one of only two factors included in the model it would seem to be important to include a factor which has been demonstrated to greatly affect adult abundance in neighboring stocks. Given that the Central Valley Harvest Index is our best guess of the impact of fishing on ocean stocks it seems an easy factor to include in the model. It is unclear what criteria the author used to include parameters in their model.

As its name implies, the Central Valley Harvest Index reflects the ratio of ocean harvest of all Central Valley stocks to the escapement of those stocks. As such, both the numerator and the denominator in the harvest ratio are almost entirely driven by the year to year variability in Sacramento Valley escapement, which is on the order of 100,000 to 600,000 fish whereas the San Joaquin escapement varies from 227 fish to a maximum of 38,125. This difference in scale is so large that it is surprising that any significant correlation is found, as reported by the author. Two likely mechanisms probably explain this correlation between the harvest index and SJR escapement: 1. each year's SJR escapement is included in the harvest index so to some (small) extent the escapement is correlating with itself. 2. It seems more likely that the correlation arises from the simple straying of adults produced in the Sacramento River to spawn in the San Joaquin when Sacramento stocks are high. In any event, the correlation, or lack thereof, gives little insight into the importance of harvest on these stocks.

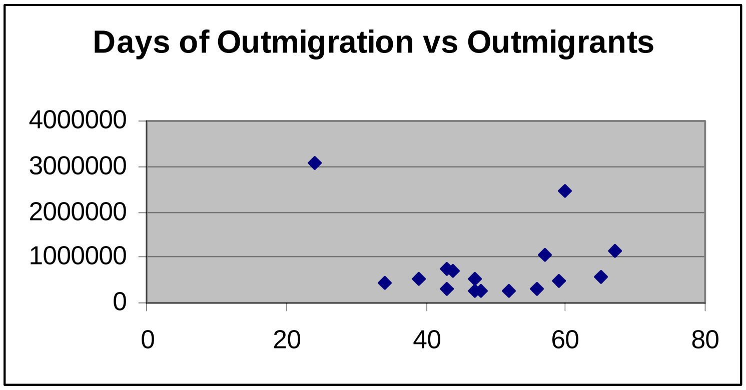
In-river adult salmon density p. 16
The author describes the three common patterns of recruitment – a straight line relationship where more spawners always increases the production of young, a Beverton-Holt curve where the number spawners reaches an asymptote in production and a Ricker model where increasing spawner density at some point results in actual decreases in production. The two latter density-dependent models have been shown appropriate for various salmon populations elsewhere; spawning habitat limitations can produce an asymptote whereas rearing habitat limitations are more likely to show a Ricker type of reaction. However, the author does not do a statistical test to determine which curve best fits the data but point to the fact that more spawners produce more fry as evidence that density dependence is not at work. This conclusion ignores that fact that all three models make that prediction. Visual inspection of the graph (figure 28) strongly suggests that a Beverton-Holt curve would be the best fit, which argues that density-dependence is at work in years of higher abundance; in fact the asymptote seems to be reached at female abundance on the Tuolumne River of about 4000 individuals. If this reflects spawning habitat limitation, than it points to a need to improve in-river habitat conditions.

Spring Flow p. 16-17
Despite their discussion on page 10 that the VAMP data are inadequate to evaluate the roles of flow vs export in delta survival, here the author concludes that "SJR salmon cohort abundance is strongly correlated with spring Vernalis flow magnitude and duration." The author has already described how flows in the tributaries can significantly change temperature and spawning habitat conditions, but by focusing on cohort abundance only in relation to Vernalis flow the author disregards any ties to biological mechanisms. Mechanisms such as San Joaquin River restoration from Friant or recirculation of releases from the Mendota Pool have been proposed to increase flow at Vernalis, but those operations will not change conditions where the fish that are the focus of this report actually spawn, hatch, and rear. Spring flow at Vernalis may be important in assisting in smolt passage through the delta, but for all other life stages it is a poor surrogate for habitats that can be much better described directly.

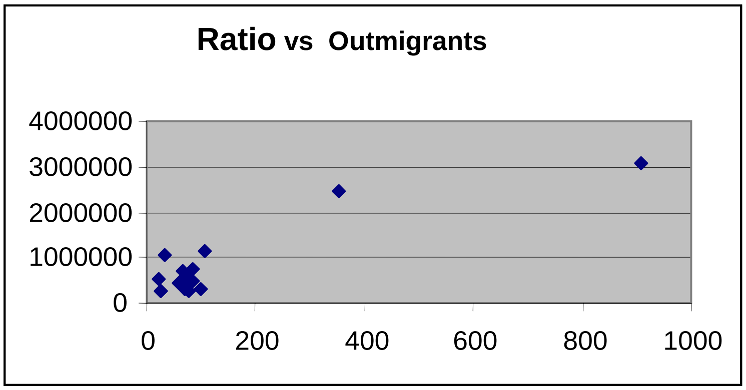
Linearly connecting the historical conditions of flood conditions and dry conditions is the basis of the model. The two types of historical conditions are so different in almost all physical parameters that there is no reason to believe that linearly intermediate conditions will produce linearly intermediate results. Graphical examination of the data presented in Table 3 and Figure 32 (although there appear to be some discrepancies between the Table and the graph). The figure clearly shows that the regression is driven by two flood years (1995 and 1998) and that all other years show no apparent relationship to Vernalis flow. The graph clarifies that the recommendations for flows of 5000 cfs, 7000 cfs, 10000 cfs, 15000, and 20000 cfs do not reflect conditions for which much data is available.




The inclusion of 'duration' as a variable in the model is based on an unclear treatment of the ratio of days and flow that is not described in the document. From the data presented wet years showed both the longest (67 days) and the shortest duration (24 days) and critical years were almost as variable (34-57 days). The author seems to assume that the duration of the outmigrant period is the factor controlling subsequent return of the cohort. It is much more likely that years of low smolt abundance may appear to have a short emigration window because the sampling program can only detect fish at higher abundances – in years of high abundance the fish appear in the nets more regularly than in years when smolt are less abundant. It is not clear how the author developed their recommendations for number of days in the window of protection, but inspection of the data presented in Table 3 shows no relationship.




The author refers to a ratio of flows/days in support of their argument for including the number of days of outmigration as a regression variable. Such a ratio suggests that increasing number of days should lower the value of flow for outmigrants, but since the scale of flows is so much larger than the scale of days (1086 to 21808 cfs vs. 24 to 67 days) the resultant ratio is simply a restatement of the relationship with flow.




**Specific:**

**Hydrology**

1. Are the methods used in the Model (including Model Report) relating to flow sufficiently documented? If not, what improvements can be made to improve documentation?

The assumption that past relationships of flow with temperature and of Vernalis flow with production should be carefully considered. Other methods of increasing flow at Vernalis will likely not have the same effects and taking more water out of reservoirs than is done under present operating rules could easily change flow/temperature relationships. Temperature models are being developed and including them into the model would be much safer than assuming past relationships will hold.

2. What is the best metric (i.e. arithmetic mean, geometric mean, transformed data etc) that can be developed to adequately capture the variability in spring flow (i.e. magnitude and duration) on an intra-annual basis?

Limiting flows to be considered to flows that can be controlled would reduce a lot of this concern. The model suffers by trying to derive salmon management actions from flood situations. It is unlikely that flood conditions would be created for the benefit of salmon or that artificial flood conditions would have the same effects as historic floods.

3. What improvements to hydrologic data utilization can be made to enhance model prediction performance reliability?

4. Is there evidence of auto-correlation in flow calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

5. Are there additional flow metrics, parameters, logic etc. that should be incorporated into model logic and function? If so, what are they and how can they be assimilated into the model (reference to logic and function)?

Either limiting the scope of this model to the direct effects of Vernalis flows or expanding this model to incorporate all the a factors affecting salmon escapement would be more useful than the attempt to disregard all other factors affecting salmon in favor of the one feature that reflects a complete state change of the system from flood control to water management.

**Biology**

1. Are the methods used in the Model (including Model Report) relating to fish abundance and/or production sufficiently documented? If not, what improvements can be made to improve documentation?

See above

2. What improvements to fish data utilization can be made to enhance model prediction performance reliability?

See above

4. Is there a way to improve how the model performs fish abundance prediction calculations and/or processing of fishery data?

See above

5. Is there evidence of auto-correlation in fish related calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

The absence of examination of stock-recruitment patterns in the model or the report is surprising. If SJR salmon are a self-perpetuating stock then we should see a stock-recruitment pattern and the model should not require inputs of the number of returning adults each year to keep it on track. If such auto-correlation is absent then attempting to apply a population model to the SJR salmon may be a vain undertaking. Recent genetic work certainly makes this a consideration that should be examined before any further population modeling work is done.

6. Does justification exist to include additional fish metrics, parameters, logic etc. in model logic and function (i.e. ocean harvest and/or Delta export entrainment)? If so, what are they and how can they be numerically assimilated into the model (reference to logic and function)?

Absolutely, see above.

7. How can model representation of hatchery production, and underlying model logic, be improved upon?

Test for impacts of hatchery on escapements. Include data from elsewhere about the survival of hatchery salmon in the field in comparison to wild stocks.

8. Currently the model predicts a constant ocean survival rate (i.e. relationship between cohort abundance and Chipps Island abundance is constant). Is there a need to make this relationship variable? If so, how can this be numerically accomplished in model performance?

The model should include CV hatchery index as an estimate of adult salmon loss (the author found a significant relationship in spite of considerable variance). Estimations of the impacts of ocean conditions could be included to estimate how often such conditions are drivers of adult abundance.

9. The model currently uses an adult replacement ratio of 1:1 as a numerically identified population health barometer. Is there a need to refine this ratio? What additional population parameter(s) could be incorporated into model logic and function?

**<u>Statistics</u>**

1. Currently the model uses liner relationships between flow and fish production because this relationship provides the strongest correlation value. Is it necessary to include a model toggle switch, model logic, and mathematical functions, that allow users the option to test a variety of non-linear relationships between flow and fish survival and/or production upon model results?

The two data points from high flow and the number of data points from low flow force the regression to effectively draw a line between two points. As the author implies, it is impossible from the data in hand to know if the line should be straight, or curved up or curved down without having data from intermediate Vernalis flows.

2. What is the statistical reliability of model out-put given that model predictions propagate? How can model reliability be improved?

For this sort of model I know of no way around the problem – stringing together variables must multiply the uncertainties. That can cast considerable doubt about the exact quantification of outputs but it does not mean that such models cannot usefully summarize our knowledge, identify the most important knowledge gaps and sources of variance. Tying together better strings of models – such as flow/habitat, flow temperature, spawning habitat/rearing habitat, velocity/transport, habitat/predation, export/entrainment, ocean harvest/escapement, ocean temperature/growth&survival models allows for minimizing uncertainty at each step and knowing what one doesn't know. I cannot see how this model can do that – this model could become a delta survival model within that larger sequence, and that would highlight the need for delta survival data as the author states in the introduction.

3. Is collinearity present in model logic and/or computation, and what influence does it have upon model results? If present how can it be removed?

The model relies on collinearity to justify the use of a downstream variable to stand in for upstream conditions. Focusing the model on variables known to be important to particular life stages allows a parsing of effects that this model cannot.

4. In some cases, model predictions for salmon production occur outside the empirical data set range used to develop the regression. What limitations in model reliability result?

See above

5. Presently smolt survival has a statistically significant regression correlation with Delta inflow level (i.e. less than 7,000). No statistically significant regression correlation for juvenile smolt survival and Delta export level exists. However when inflow to export ratio is regressed against flow survival, a moderate regression correlation occurs. Currently, exports are not included as a model prediction parameter. Should exports be included as a model prediction parameter (for smolt production)?

See comments above about scope of model. Compared to flood years exports have little impact, but at less-than-flood conditions the question is unresolved and data need to be gathered at those mid-range flows – exactly as the author says.

6. Are the methods used in the Model (including Model Report) relating to statistical evaluations and/or model logic justification sufficiently documented? If not, what improvements can be made to improve documentation?

For comments on the statistical evaluation see above. DFG has recently brought in a high-powered statistician and his help here would doubtless greatly strengthen the discussion.

Including any reference to the published literature of salmon modeling would be welcome. Models exist both off the shelf and from related runs that could usefully be adopted or compared with SJR salmon models. The author seems to attach no value to work not done locally and that weakens the biological foundation and the resulting model.

7. What improvements to statistical use and application can be made to enhance model prediction performance reliability?

See above

8. There is substantial disagreement amongst scientists regarding the issue of density dependent mortality and its influence upon SJR salmon abundance (e.g. fall spawner abundance and spawning habitat availability: aka redd superimposition). In the absence of flow the relationship between spawner abundance and stock recruit appears to show density dependence (i.e. Beverton-Holt or other density dependent type relationship). However when flow is included with spawner abundance, in the form of a multiple-regression using spawner abundance and spring flow regressed against adult recruits, a significant correlation exists suggesting that density dependence does not explain the variation in SJR adult salmon escapement abundance. How can this issue be resolved with data to date, or in the future if data insufficiency exists currently?

See above:
In-river adult salmon density p. 16
The author describes the three common patterns of recruitment – a straight line relationship where more spawners always increases the production of young, a Beverton-Holt curve where the number spawners reaches an asymptote in production and a Ricker model where increasing spawner density at some point results in actual decreases in production. The two latter density-dependent models have been shown appropriate for various salmon populations elsewhere; spawning habitat limitations can produce an asymptote whereas rearing habitat limitations are more likely to show a Ricker type of reaction. However, the author does not do a statistical test to determine which curve best fits the data but point to the fact that more spawners produce more fry as evidence that density dependence is not at work. This conclusion ignores that fact that all three models make that prediction. Visual inspection of the graph (figure 28) strongly suggests that a Beverton-Holt curve would be the best fit, which argues that density-dependence is at work in years of higher abundance; in fact the asymptote seems to be reached at female abundance on the Tuolumne River of about 4000 individuals. If this reflects spawning habitat limitation, than it points to a need to improve in-river habitat conditions.

9. How can the statistical relationships between flow and fish survival and/or fish production be improved?

See above

**Miscellaneous comments:**

Salmon exhibit a complex life history and present considerable challenges for management. However, their importance to many people has led to a deeper understanding of salmon biology than we have for most species. On the San Joaquin River we are blessed with an abundance of studies that could provide an excellent basis for a population model that incorporated the knowledge and data that have been developed. The work of Steve Kramer on winter-run salmon shows that even simple spreadsheet models can usefully gather together the various factors affecting the diverse life stages of this complex species. Such a biological based model is particularly valuable in identifying the important data gaps that need to be filled to more effectively manage the species. This report describes a lot of the relevant information but then discards most of it in favor of a regression model that captures almost none of the biology of this species and therefore provides no guidance to future research and with little relevance to comprehensive management.

In short, I find that most of the assumptions and conclusions are either not supported by the data or cannot be supported by the analyses. As a consequence I find the model to be unsuited for the purposes to which it has been put.

This report makes almost no reference to the mountain of work done on modeling salmon populations, in California and elsewhere. A quick Google search on "salmon 'population modeling'" turns up 57,400 webpages and an abundance of technical resources. Almost the only bibliography items in this report are data sources and grey literature reports by this one office of DFG. In science one sees further by standing on the shoulders of those who have come before; this report would be substantially more valuable if it took advantage of the wealth of knowledge available on salmon population modeling.

Similarly, the report suggests that lower flows might be adequate if hatchery production was augmented. The author acknowledges that this is a 'contentious issue' but give no reference to the abundance of studies in the last 20 years that document the behavioral, genetic, disease, and environmental problems amplified by hatcheries and which have led to widespread abandonment of the use of hatcheries as mitigation for environmental degradation. Although the author states that he is not advocating for or against hatcheries, his only comments about them are the attribution of questionable benefits and the possibility that they could reduce the water costs of SJR salmon management. A more balanced discussion would be useful.

**San Joaquin River Fall-run Chinook Salmon Population Model**

**External Scientific Review Form**

**Reviewer:** #4

**Review:**

1. **<u>Problem/Goals.</u>** Is the problem that the project is designed to address adequately described? Are the goals, objectives and hypotheses clearly stated and internally consistent?

The problem is well defined, and the goals are clearly stated.

2. **<u>Approach.</u>** Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal?

See "Is the model adequate?" section below.

3. **<u>Feasibility.</u>** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

Not applicable

4. **<u>Project Performance Evaluation Plan.</u>** Will a monitoring plan be developed to document changes in the restored habitat over time and the response of salmonids and/or riparian vegetation to the restoration in a scientifically rigorous manner?

Not applicable

5. **<u>Expected Products/Outcomes.</u>** Are products of value likely from the project?

Not applicable

**<u>Additional Questions:</u>**

**General:**

The purpose of the model is to develop spring flow magnitude, duration, and frequency instream flow levels into the South Delta to adequately protect, and restore, fall-run Chinook salmon in the San Joaquin River basin. To accomplish this objective, please address the topics listed below for these questions:

Is the model adequate?

Whether the model is adequate or not depends on the questions asked and how the results are interpreted. I think that the model is adequate to conclude that higher flows released over a broader time window later in the season would benefit the salmon. I think the model is inadequate if the answers need to be much more precise than that. Maybe one can deduce the rough magnitudes of the changes needed but not in much detail. I do not think the model, and the way it was applied, can produce accurate and precise enough predictions to make specific recommendations such as stated in the Executive Summary. I do not think the model contains enough biology to be able to quantitatively distinguish the long-term, multi-year effects of 5,000 versus 10,000 cfs or between the effects of a 30 day versus 45 day window. Further, when much larger differences are simulated, the predicted response in escapement seems, at times, to be unrealistic (i.e., >100% increase). This is likely due to reliance on mostly linear statistical relationships. What is critical is how the results are interpreted.

I could go into questions I have about specific relationships but you get the idea.

I will add up front in my review that I believe the model was carefully thought out and the data carefully analyzed. The description of the model is generally thorough and report is generally well written and understandable.

If not, how can model be improved?

In my opinion, the weaknesses of the model stem from its over-reliance on statistical correlations, and the appearance of ad-hoc decisions as to which statistical relationships are strong versus weak and thus included or excluded from the model. I do not agree with the principle assumptions of the model which removed ocean harvest, exports, and density-dependence from further consideration. I am also familiar with some of the more critical datasets used to derive the relationships, and have participated in debates about the interpretation and validity of the correlation relationships used to analyze the data, and which now appear in the model. Some of these relationships rely heavily on a few points which control the slope of the relationship. One may plug in x-values within the range of the data but the resulting y-value is still very sensitive to the few extreme points that control the slope.

1. Foundation (justification) – The reasons for developing the model is well stated. The devil is in the details of how should the model be developed. I am sympathetic with the author’s situation: the model must be defensible which pushes one to statistical relationships. Yet, sometimes the questions posed are not well suited to a regression-based population dynamics model. I think this is one such situation, especially since the desired answers need to be relatively precise. I think the model could be improved by the development of a parallel version that incorporates more biology and less reliance on whether statistical correlations of field data were significant or not. The two models would bound the answers.

2. Logic – The author lays out a very logic approach to developing the model. I understand his logic; I just disagree on philosophical grounds. But there is a saying that you put two modelers in a room with a problem and they come out with 3 models.

3. Numeric representations – I get a little nervous with models done in excel. The user interface is very nice but there can be problems with excel and numerical calculations. I would like to see some confirmation that excel, and the visual basic or however the model is represented in excel, is performing the computations correctly.

4. Application and reliability – The author does a nice job using a systematic approach to exploring how the magnitude and timing of flow would affect salmon escapement. I would likely use a similar simulation experimental approach, but with a different model. The author acknowledges that the computed confidence intervals are really not the appropriate variability around model predictions. This relates to the reliability issue. One can use a Monte Carlo approach or bootstrapping to derive more appropriate error bars on the final predictions. The treatment of water-type years is an excellent first step towards increasing reliability, but only addresses one of many possible sources of uncertainty and stochasticity.

5. Conclusions -- The actual conclusions in the report are reasonable; the specific recommendation in the Executive Summary is OK as long as it viewed and interpreted correctly. In my opinion, the highly quantitative model results should be viewed as qualitative or semi-quantitative predictions. The model results should be taken with other sources of evidence to determine whether it is worth changing VAMP. I personally think the results are probably pretty good, but if I am asked to place all of confidence on the model results then I back off somewhat.

6. Calibration and validation – The calibration and validation is weak in the present application of the model. It was not clear which regression coefficients (which are also model parameters) were adjusted by the author, and whether this was done in a systematic way or not. I wonder why the author did not simply use solver in excel and optimize the calibration. There is no model validation.

7. Documentation -- The documentation is pretty good. It could be improved by a table or appendix that actually lists the equations of the model and the order of computation. More information on the calibration method and which coefficients were changed and by how much in the final calibrated version from the data-derived estimates would help. It might be interesting to see if the adjusted coefficients result in relationships that still fit the data upon which they first estimated from.

8. Testing (i.e. what monitoring could occur to validate or reject model predictions) – I think the author tried to use the available information. Indeed, many of the correlations and arguments in the report approach become circular and convoluted because the same data seems to be used in multiple ways. The population model cannot really be tested at the level that would be ideal (i.e., the long-term population level). Perhaps one can use a more detailed model, less constrained by significant statistical relationships existing or not, to see if the same results would be predicted.

**Specific:**

**Hydrology**

1. Are the methods used in the Model (including Model Report) relating to flow sufficiently documented? If not, what improvements can be made to improve documentation?

I think adding a background section that explains exports, delta inflow, etc. in the context of the salmon life history would help. Nothing extensive, but an overview of the hydrology overlaid on the life cycle. Also, a graphical presentation the features of the different water-type years would be helpful.

2. What is the best metric (i.e. arithmetic mean, geometric mean, transformed data etc) that can be developed to adequately capture the variability in spring flow (i.e. magnitude and duration) on an intra-annual basis?

I am not sure.

3. What improvements to hydrologic data utilization can be made to enhance model prediction performance reliability?

Model simulations that use a variety of sequences of year-types would help model reliability, especially when the model is used to make forecasts. Also, what is the variability among years of the same water-year type? Do the patterns of water flows overlap among designations, or are they unique? I recall that specific years can look like one type in spring and another type in the summer. How is this dealt with? Perhaps using water-year types on a seasonal basis might help with reliability? At the other end of the scale, there is no mention of how climate change might affect the water flows. Will we expect to see more high-flow years occurring together, etc.?

4. Is there evidence of auto-correlation in flow calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

I can think of two levels of auto-correlation: day-to-day and year-to-year. I think the day-to-day is OK. The year-to-year is more problematic and more interesting. I suggested in (3) above that more investigation of how the sequences of year types affects predictions. This is a direct approach to adjusting the inter-annual correlation (either making it higher or lower).

5. Are there additional flow metrics, parameters, logic etc. that should be incorporated into model logic and function? If so, what are they and how can they be assimilated into the model (reference to logic and function)?

I am not sure about the question but I will use this opportunity to suggest that I would also look at how salmon habitat itself (without regression relationship to convert it to escapement) would change under the different scenarios. Before one even gets to salmon numbers, one would know how the physical habitat will change.

**<u>Biology</u>**

1. Are the methods used in the Model (including Model Report) relating to fish abundance and/or production sufficiently documented? If not, what improvements can be made to improve documentation?

The entire model is series of linked regression models. I think a table or appendix that shows the exact equations used would be helpful in terms of documentation. There are some details that would help, such as how the final escapement is determined from the multiple years of contribution. I can tell petty much what the model is by the report, but I am very familiar with population models.

2. What improvements to fish data utilization can be made to enhance model prediction performance reliability?

We know a lot more about salmon reproduction, growth, mortality, and movement than is implied by this model. I think the fish data can be further utilized to put more biology into the model. Perhaps a complementary model, less constrained by statistical significance, would help. Also, I think the data can be mined more for factors affecting interannual variation in salmon.

4. Is there a way to improve how the model performs fish abundance prediction calculations and/or processing of fishery data?

The model can be calibrated using formal minimization methods, and uncertainty analysis can be used to show probability distributions of predictions.

5. Is there evidence of auto-correlation in fish related calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

Their must and should be auto-correlation in the fish calculations. Escapement is comprised of multiple year-classes. Auto-correlation in the fish can also arise from auto-correlation in the hydrology. I would not want it to be removed. I might like to see how important the auto-correlation was so that I can judge how the historical time series (with its built-in auto-correlation) might have affected the predictions. One way to asses this is remove it completely (run with a random sequence of year types) and to manipulate the sequence of year types. This would be more as sensitivity analysis and would be helpful for interpreting the results based on the historical time series.

6. Does justification exist to include additional fish metrics, parameters, logic etc. in model logic and function (i.e. ocean harvest and/or Delta export entrainment)? If so, what are they and how can they be numerically assimilated into the model (reference to logic and function)?

I think export, ocean harvest, density-dependence, and stochasticity (real year-to-year and with-in year variability as it occurs in nature) should be included in the model.

7. How can model representation of hatchery production, and underlying model logic, be improved upon?

As noted by the author, how hatchery salmon will react to flow and temperature are unknown. Will hatchery fish follow the same relationships to flow as was (sometimes weakly) estimated for natural fish? The way hatcheries were included was consistent with the general modeling approach used with natural fish.

8. Currently the model predicts a constant ocean survival rate (i.e. relationship between cohort abundance and Chipps Island abundance is constant). Is there a need to make this relationship variable? If so, how can this be numerically accomplished in model performance?

Making ocean survival a constant is OK as a first step. All model predictions are then conditioned on this constant ocean mortality rate. I would make ocean mortality variable in the model.

9. The model currently uses an adult replacement ratio of 1:1 as a numerically identified population health barometer. Is there a need to refine this ratio? What additional population parameter(s) could be incorporated into model logic and function?

An adult replacement ratio is a reasonable health metric. Whether it should be 1:1 or something else depends on the management objective and whether 1:1 will get you recovery in the time frame needed. There are other metrics, but their use is limited by the structure of the model and what it can predict. I think looking at what the adult replacement ratio metric means in terms of survival in other life stages would be helpful. Can the conditions that resulted in 1:1 ratio be expected to produce the required reproduction or survival in other life stages through the life cycle?

**Statistics**

1. Currently the model uses liner relationships between flow and fish production because this relationship provides the strongest correlation value. Is it necessary to include a model toggle switch, model logic, and mathematical functions, that allow users the option to test a variety of non-linear relationships between flow and fish survival and/or production upon model results?

The short answer is yes – but even including options on linear relationships would not address my major concern of the general modeling approach that relies on a series of linked correlations. Linear relationships just make me much more nervous. We know from experience that correlation-based population model of fish do not work very well for long-term predictions. They will evidently fall apart because one or more of the correlations falls apart. This does not mean that useful information cannot be obtained from a correlation-based modeling effort. But it means that appropriate caution should be used in interpreting the results. The various correlations are based on different (often incompletely overlapping) years, with the associated differences in environmental and biological conditions.

2. What is the statistical reliability of model out-put given that model predictions propagate? How can model reliability be improved?

The reliability of the final model predictions is low in absolute terms (i.e., actual escapement). This is typical for population dynamics models, both correlation-based and process-based. More accurate confidence intervals would undoubtedly show that most of the different scenarios generate overlapping predictions of escapement. Also, the dismissal of variation in ocean harvest and the dismissal of other factors completely make the model predictions not accurate at the absolute level. The model is best used to generate relative changes, which the author highlights in the results tables (percent change from historical). There are techniques available for propagating uncertainty and stochasticity through this type of model. In fact, I think there is an add-in to excel (something called crystal ball) that would allow one to use Monte Carlo techniques to analyze the uncertainty in the model.

3. Is collinearity present in model logic and/or computation, and what influence does it have upon model results? If present how can it be removed?

I think there is the potential for a great deal of collinearity in the model because the same datasets seemed to be used in multiple ways. For example, lagged escapement is used to derive the arrivals at Mossdale to begin the model, and of course, escapement is the primary prediction variable. There are many examples of this. Another source of collinearity is the multiple uses of the same data. For example, the x-axis for the same data was expressed as export ratio, exports, and Vernalis flow plus exports. All of the resulting relationships were questionable (high scatter, outliers, cluster of influence points) relationships and the conclusion of no need to include exports in the model was a major assumption. The same approach was used to dismiss density-dependence from consideration. The best way to remove collinearity is to build the model differently.

4. In some cases, model predictions for salmon production occur outside the empirical data set range used to develop the regression. What limitations in model reliability result?

This focus on "outside-of-the-range" is valid but I am concerned that this implies that predictions within the range are therefore considered OK. Outside-of-the-range predictions are clearly a problem. Inside-the-range predictions are also a problem, as the model uses the slopes of relationships, which can be greatly influenced by a few points. The model can be used to generate qualitative or semi-quantitative predictions but not quantitative predictions. Also, the predictions presume all other factors remain constant, so the model is less reliable in making predictions for say the next 3 years. Over may years, one could hope that the other factor average out in their effects.

5. Presently smolt survival has a statistically significant regression correlation with Delta inflow level (i.e. less than 7,000). No statistically significant regression correlation for juvenile smolt survival and Delta export level exists. However when inflow to export ratio is regressed against flow survival, a moderate regression correlation occurs.

Currently, exports are not included as a model prediction parameter. Should exports be included as a model prediction parameter (for smolt production)?

I think exports should be included but if they are brought in consistent with the other factors in the model, then I suspect not much good will result. The author seems bent on only including statistical significant relationships. Yet, some of the relationships included are questionable and seem to me to be at the same level of confidence of other relationships dismissed as "not significant enough" to be included. Exports seem to be an example of this. To me, I would include exports and density-dependence and ocean harvest, given the way the model is currently being used.

6. Are the methods used in the Model (including Model Report) relating to statistical evaluations and/or model logic justification sufficiently documented? If not, what improvements can be made to improve documentation?

The logic of model development is well presented. There are too many instances where it seems the author deems one relationship "not significant enough" to include while including other relationships that look to me to be similarly weak.

7. What improvements to statistical use and application can be made to enhance model prediction performance reliability?

More sophisticated statistical methods can be applied to some of the critical datasets. There are well established alterative methods to simple linear regression, such as robust regression and regression trees. The issue of serial correlation due to the data being mostly time series is ignored, perhaps some regression-like methods that explicitly account for time series data could be investigated.

8. There is substantial disagreement amongst scientists regarding the issue of density dependent mortality and its influence upon SJR salmon abundance (e.g. fall spawner abundance and spawning habitat availability: aka redd superimposition). In the absence of flow the relationship between spawner abundance and stock recruit appears to show density dependence (i.e. Beaverton-Holt or other density dependent type relationship). However when flow is included with spawner abundance, in the form of a multiple-regression using spawner abundance and spring flow regressed against adult recruits, a significant correlation exists suggesting that density dependence does not explain the variation in SJR adult salmon escapement abundance. How can this issue be resolved with data to date, or in the future if data insufficiency exists currently?

I think the analyses dismissing density-dependence in the report is weak. I look at Figures 28 and 29 and I see the potential for density-dependence. The author states "Both the increased fry density with increased spawner density, and increased cohort abundance with increased spawner abundance, are contrary with the density-dependent hypothesis (page 16)." Either I mis-understand this statement or it is wrong. Both increasing with increasing spawners is consistent with density-dependence, it is the rate of their increase that tells you whether density-dependence is strong or weak. I think the presently available data are sufficient to resolve the density-dependence issue, or at least sufficient to specify a relationship and see if it affects the predictions and conclusions. Some of the scenarios involve higher salmon abundance which could trigger density-dependence.

9. How can the statistical relationships between flow and fish survival and/or fish production be improved?

One way to improve them is to use more sophisticated statistical methods than just linear regression. Even including regression diagnostics about outliers and influence points, patterns in residuals, and bootstrapping would help the rigor of the results. Perhaps some of these datasets are better suited for other methods than linear regression, such as robust regression, survival analysis (for the mortality data), regression trees, and time series methods...

**Miscellaneous comments:**

This modeling approach has the advantage of appearing to be based on statistical relationships and thus might appear to be more defensible. This often turns out to be a false advantage. People argue about outliers, whether the relationships included should be linear or not, and about the relationships not included. If you want to use the model to suggest that more flow (within reason and practicable amounts) and a longer, delayed time window would help the salmon, then I agree with the conclusions. If you want to specify the flow to within 1000 cfs and the time window within days or weeks, then I have problems with the current formulation of the model.

(1) page 10.—the argument for focus on smolt is confusing. For example, the author says more fry means higher escapement but also higher smolt abundance, as if this argues for smolts being more important than fry. I would think more fry would mean more smolts. If there is evidence of low DO having an effect, then it should be summarize and cited. So smolt and escapement are correlated, does that mean fry are unimportant? Some clarification is needed here.

(2) page 11.—I agree that the Delta may not be a good place anymore for salmon. But the fact they grew fast in the ocean does not mean the Delta is not helpful. There are also predation issues.

(3) page 12.—Figures 9 and 10 are interesting. I think they mean that VAMP is dealing with a small amount of water.

(4) page 13.—the model really does not allow for confidence intervals to be calculated.

(5) page 14.—the argument about the unimportance of exports is difficult to follow and seems convoluted. Evidence of this is footnote 19. I am not convinced that the author has shown that it is flow and not exports. The data by Pat Brandes has been the subject of much discussion and to use the way the author is using the data is risky. I am not convinced that the author has explained away Figure 18, especially if the model is required to generate absolute

predictions of escapement and be able to do this for difference of 1000's of cfs of flow. I think changes in operations confound the entire picture.

(6) page 15.—I disagree that ocean harvest can be treated as a constant. The relationship dismissed in Figure 26 is as strong as or stronger than some of the relationships included in the model (for example figure 36).

(7) page 16.—I found the discussion about density-dependence to be confusing and I think misguided. I do not follow the argument about how figure 28 is not showing density-dependence?

(8) page 17.—Figure 29 is presented as the evidence that Vernalis flow is important; yet, the relationship shown in the figure is very weak evidence. I would guess that the relationships depends on the two extreme points. The rest of the data is a cloud.

(9) page 19.—Figures 34 and 36 are critical to the model and pretty much dictates how important flow is. These linear relationships are suspect.

(10) page 22.—The model calibration is poorly documented. Which parameter were varied and how? How close are their final value to the values estimated from the data? Why not use an optimization (minimization) method.

(11) page 23.—There are methods for propagating uncertainty through these models.

(12) page 25.—The results are nicely presented.

(13) page 26.—Why not optimize the model to obtain a full exploration of the trade-offs between water usage and escapement enhancement?

(14) page 27.—The model does not provide evidence that spring flow is important. The model was built under that assumption. Be clear that the data were analyzed and the author concluded that flow was important. It is not a model result.

**San Joaquin River Fall-run Chinook Salmon Population Model**

**External Scientific Review Form**

**Reviewer:** #5

**Review:**

1. **Problem/Goals.** Is the problem that the project is designed to address adequately described? Are the goals, objectives and hypotheses clearly stated and internally consistent?

*The problem is well described (based on both the VAMP annual report and the DFG).*

2. **Approach.** Is the approach well designed and appropriate for meeting the objectives of the project as described in the proposal?

*The available documentation clearly states that the basis for the SJR Fall-run Chinook Salmon Population Prediction Model was in response to a request from the State Water Resources Control Board (SWRCB) for the California Department of Fish and Game (DFG) to provide a recommendation for Vernalis flow objectives. Specifically, in early 2005, the DFG evaluated the 1995 Water Quality Control Plan by asking four key questions: 1) What is the current status of the SJR fall-run Chinook salmon population?; 2) What level of protection is being afforded salmon smolts out-migrating from the SJR into the South Delta?; 3) What is the status of the Vernalis Adaptive Management Plan (VAMP) experiment?; and 4) What influence does spring flow have on fall-run Chinook salmon production in the SJR? The approach outlined in the documentation and supported by the spreadsheet model is consistent with this approach and well described in the documentation. However, there is a larger issue at hand: are the four questions identified by the DFG of sufficient scope to address the challenging problem in the San Joaquin Basin, Delta, and Ocean environs?*

3. **Feasibility.** Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives?

*The approach is generally well documented. The statistical details of the formulation of key relationships (e.g., regression equations) should be more thoroughly and formally presented. The modeling process has presented a useful framework for couching many of the factors thought to be important in fall-run Chinook production in the San Joaquin River. The document and model have largely been successful in this manner.*

4. **Project Performance Evaluation Plan.** Will a monitoring plan be developed to document changes in the restored habitat over time and the response of salmonids and/or riparian vegetation to the restoration in a scientifically rigorous manner?

*Sufficiency of existing monitoring is not addressed in the primary documentation. The VAMP annual report identifies the need for additional/ongoing monitoring/studies. The* *overall concept of using the model to set long-term flow recommendations is not paired with a monitoring program…but such a program would be a wise investment.*

5. **<u>Expected Products/Outcomes.</u>** Are products of value likely from the project?

*The reviewer is very pleased that DFG is taking a highly proactive approach to the VAMP process and pressing hard questions prior to the completion of the 12 year program. Development of a quantitative model to assess the response of fall-run Chinook salmon production is a valuable step in the overall recovery strategy and management of the San Joaquin River water resources and fisheries. The work completed by the Department allows additional questions to be asked and more refined hypothesis to be presented. However, the model in itself is only one aspect of a complex system. As such, the conclusion that flow recommendations can be based on the "simple spreadsheet flow-based" model is probably over-optimistic.*

*Modeling is a lengthy process. Not only does it take a fair amount of time to gather the data, develop the relationships, and place the whole affair into a quantitative numerical framework, but it also takes time to document the information and convey it to interested parties…and this is only the beginning. Review of model representations, processes, and data used to form fundamental relationships is not a static process – new information and interpretation continues to occur. The product developed by the Department is a valuable first step in a longer process of sharing information and ideas, modifying model relationships, conceiving of new ideas and abandoning previous held beliefs, and along the way making progress in resource management.*

**<u>Additional Questions:</u>**

**General:**
The purpose of the model is to develop spring flow magnitude, duration, and frequency instream flow levels into the South Delta to adequately protect, and restore, fall-run Chinook salmon in the San Joaquin River basin. To accomplish this objective, please address the topics listed below for these questions:

Is the model adequate?

*The model provides additional insight into the role of spring flow magnitude, duration, and frequency as these conditions relate to historical fall-run Chinook salmon in the San Joaquin River basin. Construction of the model has allowed assessment of several factors as they relate to potential impacts of increased flows on salmon. After examination of the available materials, the Reviewer identifies the role of this model as one tool that may provide insight into long term flow recommendations, but does not see the model as a stand alone tool to provide long-term flow recommendations.*

If not, how can model be improved?

*(See detailed comments under Hydrology, Biology, and Statistics, below.)*

1. Foundation (justification): *The justification is apparent. Fishery numbers are not increasing and DFG is asking the hard questions (as are others): why not? Pursing quantitative tools to assist in management actions and ongoing adaptive management frameworks is a laudable and necessary step.*
2. Logic: *The conceptual model and conversion of this to a quantitative tool in a spreadsheet environment is a method that has been employed by other agencies and entities (See CALFED, 2005, Appendix B). The methodology employed is transparent and the tool is readily used by stakeholders.*
3. Numeric representations: *Empirical models (e.g., regression equations) have strengths (based on real data) and weaknesses (sufficient data of sufficient variability are needed to form robust models). The challenge in the San Joaquin River is incorporating the appropriate factors to capture the spatial and temporal variability.*
4. Application and reliability: *The temporal and spatial variability in the San Joaquin River may not be fully represented through the application of linear regression equations based solely on spring flow conditions at Vernalis.*
5. Conclusions: *The tool is valuable in assessing several factors associated with flow conditions at Vernalis as this factor relates to fall-run Chinook production. However, there are other factors that may play a role. With modification, some of these other factors may be incorporated into the model, but more complex relationships may be required. As a result the current model may form an element of a suite of tools and relationships to formulate long-term flow recommendations, but as a stand alone model to formulate such recommendations it is probably insufficient.*
6. Calibration and validation: *The model is based on several sub-models (regression relationships). The nature of such empirical/statistical models makes calibration, in the true sense of deterministic model calibration, more difficult to explain. That is why complete presentation of sub-model development (data, comprehensive regression statistics, residuals, etc.) is a necessary step in documentation of the model. Recommend the comparison with historical data be called "validation" and drop the term "calibration." (See additional comments below.)*
7. Documentation: *Modest improvement in documentation could go a long way in supporting this model as a useful tool in identifying and testing hypothesis, as well as formulate the basis for future modifications and expanded capabilities.*
8. Testing (i.e. what monitoring could occur to validate or reject model predictions): *In general, the data are limited in space and time. One key outcome of this model is the modification, or better yet, the augmentation of existing monitoring programs to fill identified data gaps and test sub-hypothesis (e.g., that increased export increases smolt survival).*

**Specific:**

**<u>Hydrology</u>**

1. Are the methods used in the Model (including Model Report) relating to flow sufficiently documented? If not, what improvements can be made to improve documentation?

*Flow data are only peripherally discussed in the documentation. The user is left to find his/her way through the spreadsheet model. For example the reference to source flow data (United States Geological Survey) is found in the $15^{th}$ sheet of the model. Source of flow data should be referenced in the primary document.*

2. What is the best metric (i.e. arithmetic mean, geometric mean, transformed data etc) that can be developed to adequately capture the variability in spring flow (i.e. magnitude and duration) on an intra-annual basis?

*The period of record used in the model and sub-models is not clear. The regressions and/or other relationships are based on variable periods of time, e.g.,*
*- Mossdale smolt abundance: 1987-2003*
*- Delta Survival: 1994-2004 (including both HORB in and HORB out conditions)*
*- Smolt cohort production: 1988-2001*
*- Smolt outmigration pattern: 1988-2004*
*Rather than have this information included in an undocumented spreadsheet (SJR Model_Supporting Files.xls), it would be more useful to document these in the text of the report.*

*Once these periods are identified, then appropriate metrics can be determined. For example, perhaps a period of record arithmetic mean (or other metric) is developed…as well as a sub-period that is consistent/available among all data sets. Using the bullet points above, it may be useful to calculate the 1994-2001 mean for all data series so they can be assessed on a comparable time scale. Such an exercise may also point to data gaps or data limitations.*

*So as not to completely evade the question, basic summary statistics (mean, maximum, minimum, standard deviation) are not only useful, but also readily understood by stakeholders. If a particular metric provides additional information, e.g., particular excedance criteria, inter-quartile ranges, etc., they can be used so long as the definition and intention are clearly stated.*

3. What improvements to hydrologic data utilization can be made to enhance model prediction performance reliability?

*The 2004 flow data for flow at Vernalis should be updated with the latest USGS data (rather than using CDEC data which is provisional – this is a minor point). The Reviewer could not find a time series of Delta exports in any of the documentation. There were figures including export and export: flow ratio, but no tabulated export data paired with Vernalis flow.*

4. Is there evidence of auto-correlation in flow calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

*Review of an autocorrelation plot of residuals can provide useful insight. The Reviewer did not see a test for stationarity in any of the data (hydrological or biological).*

5. Are there additional flow metrics, parameters, logic etc. that should be incorporated into model logic and function? If so, what are they and how can they be assimilated into the model (reference to logic and function)?

*The reviewer is not convinced that Delta exports play no role as noted numerous times in the report (Pages 14,15, 17, 26). Actually, the report is not clear about this point, sometimes stating that export plays no role and at other times relying on the export:flow ratio to support a particular point. On page 15 there is a statement that "while the influence of Delta export upon SJR salmon production is not totally clear, overall it appears that Delta exports are not having a negative influence upon SJR salmon production they were once thought to have." This statement is contrary to the VAMP Annual Report:*

> *"These relationships suggest that adult escapement in the San Joaquin basin is affected by flow in the San Joaquin River at Vernalis and exports by the CVP and SWP during the spring months when juveniles migrate through the river and Delta to the ocean." (SJRGA, 2006, page 61)*

*Further, Figures 21 and 22(DFG, 2005) suggest that export, although inversely related to survival is much more sensitive than flow at Vernalis. Although not explored by the Reviewer, these findings suggest that there may be unanswered questions relating to export.*

**<u>Biology</u>**

1. Are the methods used in the Model (including Model Report) relating to fish abundance and/or production sufficiently documented? If not, what improvements can be made to improve documentation?

*The documentation provides a largely narrative description of the biological components of the model. The regression equation sub-models should be presented in equation and tabular form in the main documentation instead of requiring the user to open the spreadsheet to determine the main relationships. The supporting files spreadsheet, the model, and Tables 1 through 4 in the main report were not consistent in the form of the equations or the coefficients and constants values.*

2. What improvements to fish data utilization can be made to enhance model prediction performance reliability?

*The reviewer suggests that all available biological data and all data used in developing the sub-models (regression equations) be presented. All data sets include limitations, bringing such data limitations to light can help modelers and stakeholders interpret the results; identify data gaps; determine efficacy of the sub-models individually and as a group. Data limitations can range from limited data (e.g., relatively small sample size) to more complex issues such as identification of outliers (e.g., Mossdale smolt abundance estimate of 1989 is identified as an outlier but only briefly described).*

3. Is there a way to improve how the model performs fish abundance prediction calculations and/or processing of fishery data?

*A concern of the reviewer is that a wide range of regression based models are presented to support the general premise of the document – that spring flow is the primary driver of fall-run Chinook survival in the San Joaquin River – and to form the basis for the spreadsheet model. Some of these data are fit with power functions, others with exponential relationships, logarithmic functions, and some with linear relationships (e.g., see Figures 7-12, 18-22). The reason for selecting one form of the statistical equation over another is not sufficiently addressed. If there are specific non-linear and/or linear trends, the document would benefit from a more comprehensive discussion. One recommendation is to limit the range of models to one or two types and accept a lower correlation coefficient, but retain some consistency among the relationships. This is the case for the model (all based on linear relationships), but not in many of the other relationships supporting the model development.*

4. Is there evidence of auto-correlation in fish related calculations? If so, what is the affect? Does it need to be removed to improve model prediction (flow determination) reliability? If so, how can it be removed?

*Defer to other reviewers.*

5. Does justification exist to include additional fish metrics, parameters, logic etc. in model logic and function (i.e. ocean harvest and/or Delta export entrainment)? If so, what are they and how can they be numerically assimilated into the model (reference to logic and function)?

*In a complex system such as the San Joaquin River, Delta, San Francisco Bay, and Pacific Ocean, it may be difficult to identify the actual limiting factors – which may vary appreciably in space and time. That is, in any given year river flow, ocean conditions, tributary conditions (flow, habitat, and/or, temperature, predation) main stem San Joaquin River conditions (flow, habitat, and/or, temperature, predation), Delta export, and/or other factors may be individually a dominant factor or present a combination of stressors.*

*Statistical models based on limited variables do not explicitly include many factors by their very nature. Further, inter-annual variability may be under represented. These and other potential important processes are implicitly embedded in the constants and coefficients (e.g., and in y=ax + ). Further, the linear models developed are unbounded, i.e., as flows increase, salmon numbers increase without limit. Realistically, in-river adult salmon density or other factor (e.g., disease) would ultimately limit production. This suggests that the model would benefit by an upper bound, or perhaps a revision of the model to a piecewise linear or nonlinear form (e.g., Figure 27, (DFG, 2005)) that represents density dependent mortality.*

6. How can model representation of hatchery production, and underlying model logic, be improved upon?

*See "General Comments" below.*

7. Currently the model predicts a constant ocean survival rate (i.e. relationship between cohort abundance and Chipps Island abundance is constant). Is there a need to make this relationship variable? If so, how can this be numerically accomplished in model performance?

*See "General Comments" below.*

8. The model currently uses an adult replacement ratio of 1:1 as a numerically identified population health barometer. Is there a need to refine this ratio? What additional population parameter(s) could be incorporated into model logic and function?

*See "General Comments" below.*

<u>General Comment (Biology)</u>: *Including potentially important parameters, such as ocean survival or hatchery production, would provide a means to assess a broader array of potentially important factors. The questions posed herein are valid and present potentially useful additions to the model. However, the Reviewer is concerned that implementation of such models through a series of regression equations may not provide the sufficient flexibility to attain the ultimate goal of setting flow recommendations. Even through the collection of several additional years of data, the models would still be limited to "historical" conditions, thus limiting the predictive capability of the model to the range of sampled data. To encompass the broad range of potentially important parameters (ocean conditions; hatchery impacts; Delta conditions; flow and exports; San Joaquin River and tributary flow, temperature, habitat and other conditions, etc.) across multiple year types would may require a more rigorous modeling approach (e.g., life-cycle model).*

## Statistics

1. Currently the model uses liner relationships between flow and fish production because this relationship provides the strongest correlation value. Is it necessary to include a model toggle switch, model logic, and mathematical functions, that allow users the option to test a variety of non-linear relationships between flow and fish survival and/or production upon model results?

*Simply allowing the model to toggle among different relationships is a convenient feature for testing various relationships. However, caution should be employed in using the model for this purpose. The Reviewer suggests developing sub-models (regression equations) with care and identifying the relationships that exist among the data, determining if they make sense, testing them statistically, and documenting the assumptions and limitations prior to placing them in the spreadsheet model. For example, violations of linearity can be serious, and fitting a linear model to nonlinearly related data can result in considerable error, especially extrapolating beyond the range of the sample data – the overall spreadsheet model would not readily illustrate this weakness, but a comprehensive statistical assessment of the regression equation should provide such insight.*

2. What is the statistical reliability of model out-put given that model predictions propagate? How can model reliability be improved?

*(Defer to other reviewers.)*

3. Is collinearity present in model logic and/or computation, and what influence does it have upon model results? If present how can it be removed?

*(Defer to other reviewers.)*

4. In some cases, model predictions for salmon production occur outside the empirical data set range used to develop the regression. What limitations in model reliability result?

*(See response to question 1, above)*

5. Presently smolt survival has a statistically significant regression correlation with Delta inflow level (i.e. less than 7,000). No statistically significant regression correlation for juvenile smolt survival and Delta export level exists. However when inflow to export ratio is regressed against flow survival, a moderate regression correlation occurs. Currently, exports are not included as a model prediction parameter. Should exports be included as a model prediction parameter (for smolt production)?

*(See also hydrology comments above.) This question/comment appears to pre-suppose the relationship between Delta export and juvenile smolt survival. The model documentation actually identifies that "juvenile survival increases as exports increase" (DFG, 2005, pg 14, and Figures 19, 21, and 22). Further review of the VAMP annual report (SJRGA, 2006) suggest that there are still unanswered questions about the role of export on survival, and the fact that barrier operations at Head of Old River (HORB) apparently have an effect on survival and salvage indicate there is probably more to learn about this complex relationship. The recent wet year type will add a valuable data point, but one without the HORB in place. The limitation on placing the HORB at flows greater than 5000 cfs (or even the importance of having it in at flows greater than 5000 cfs), and the limited number of data points surrounding HORB placement or non-placement introduces uncertainty into the analysis. The DFG report has presented an interesting and useful hypothesis – that not only do exports not significantly affect San Joaquin River fall-run Chinook, but increased exports lead to increased survival – however, the supporting evidence does not appear sufficient to apply the model to set long-term flow recommendations at this time as per the documentation discussion (DFG, 2005).*

6. Are the methods used in the Model (including Model Report) relating to statistical evaluations and/or model logic justification sufficiently documented? If not, what improvements can be made to improve documentation?

*Including the regression information in the main documentation would provide a more complete document. Including plots of the residuals and standard error of the coefficients would provide the reader with additional information to interpret the results. For example,* *the magnitude of the standard error of the coefficients in many of the relationships presented in the "SJR Model_Supporting Files.xls" are on the same order as the coefficients and constants themselves. (There are many relationships in this Excel workbook and the Reviewer did not attempt to determine which were applicable and which were not, rather simply looked through the regression summaries.) Also, recommend minimizing the generally narrative descriptions of the validity of relationships as "very strong", "strong," "strong correlation," etc. Such statements are subject to individual interpretation. Presentation of full statistical modeling results in tabular form with comprehensive discussion of results will do away with the need for such statements.*

7. What improvements to statistical use and application can be made to enhance model prediction performance reliability?

*As per previous discussions, more description and interpretation of sub-model (regression equation) construction and performance.*

8. There is substantial disagreement amongst scientists regarding the issue of density dependent mortality and its influence upon SJR salmon abundance (e.g. fall spawner abundance and spawning habitat availability: aka redd superimposition). In the absence of flow the relationship between spawner abundance and stock recruit appears to show density dependence (i.e. Beaverton-Holt or other density dependent type relationship). However when flow is included with spawner abundance, in the form of a multiple-regression using spawner abundance and spring flow regressed against adult recruits, a significant correlation exists suggesting that density dependence does not explain the variation in SJR adult salmon escapement abundance. How can this issue be resolved with data to date, or in the future if data insufficiency exists currently?

*(See "Biology" question 6.)*

9. How can the statistical relationships between flow and fish survival and/or fish production be improved?

*(See previous comments in this section.)*

**Miscellaneous comments:**

- *(Figures 13-15) Recommend identifying why the various traces in each graph start at different upstream water temperatures and discuss any associated implications for the various flow rates.*
- *(Figure 16) The figure is not convincing. If the data are censored to include only data over say 56$^{o}$F, the relationship is practically flat. There appears to be little data available, thus this figure indicates a potential data gap.*
- *(Figure 25) A statistical analysis of the Sacramento and San Joaquin Rivers salmon escapements may provide more insight than a simple graphical comparison. The San Joaquin River generally follows the trend (albeit considerably lower) of the Sacramento River. If these fish commingle in the ocean, and Sacramento River salmon are correlated*

*to ocean harvest, then there is the potential for San Joaquin River fish to suffer a similar fate.*

- *Model Calibration/Validation: See BDMF (2000) for additional details. http://www.cwemf.org/Pubs/Protocols2000-01.pdf*
- *No explicit temperature component in the model. Thus, even short term adverse "events" that can hamper smolt production (or other life stage) are not explicitly included.*

**References**

CALFED. 2005. *Review of the Biological Opinion of the Long-Term Central Valley Project and State Water Project Operations Criteria and Plan*. Prepared by the Technical Review Panel for the California Bay-Delta Authority. December.

California Department of Fish and Game (DFG). 2005. *San Joaquin River Fall-run Chinook Salmon Population Model*. Prepared by D. Marston. San Joaquin Valley Southern Sierra Region. November.

Bay Delta Modeling Forum (BDMF). 2000. *Protocols for Water and Environmental Modeling*. BDMF 2000-01. January 21.

San Joaquin River Group Authority (SJRGA). 2006. *San Joaquin River Agreement, 2005 Annual Technical Report*. Prepared for the California Water Resource Control Board in compliance with D-1641. January

DFG Exhibit #15

# The Status of San Joaquin Drainage Chinook Salmon Stocks, Habitat Conditions and Natural Production Factors

California Department of Fish and Game
Region 4
Fresno, California

July, 1987

Prepared for the State Water Resources Control Board Bay/Delta Hearing Process PHASE I: Determination of Beneficial Uses and Determination of Reasonable Levels of Protection. September, 1987

State Water Resources Control Board
Bay-Delta Hearings Application No. 5626
PARTICIPANT: Save the San Francisco Bay Association
EXHIBIT: SSFBA005
INTRODUCED: 2/9/99
ACCEPTED IN EVIDENCE: YES NO
DATE 2/9/99

# I. EXECUTIVE SUMMARY

Chinook salmon, a very significant fishery, may be affected by this action of the State Water Resources Control Board.

The mortality of juvenile salmon is a major limiting factor in the production of fish in the San Joaquin Drainage. Spawning runs in the San Joaquin Drainage are 70% or less of historic levels due largely to inadequate streamflows in the San Joaquin River, its tributaries and the Delta when young migrate to sea.

High water temperatures in May during the seaward migration of young fish results in high chronic thermal stress when San Joaquin River flows at Vernalis are below 5,000 cfs. This, added to the additional stress factors in the river and south Delta, results in poor survival of juvenile salmon enroute to the ocean and consequently, low adult production.

Appropriate improvements in tributary streamflows alone can help reduce temperature stress, but will only be effective in improving the number of adult salmon if combined with measures which improve survival of juveniles in the south Delta.

In the absence of a total water management approach, very large amounts of water would be necessary to recover San Joaquin Drainage Chinook salmon runs to near historic levels. Measures which guarantee and protect acceptable streamflows and habitat conditions in the tributaries, San Joaquin River and Delta are needed.

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## III. INTRODUCTION

In previous actions, the State Water Resources Control Board (SWRCB) has confined the scope of this phase of the Bay Delta hearings to the Sacramento River, San Joaquin River Delta and has delineated its southern boundary at the USGS streamflow gauge, located at Vernalis. The SWRCB has requested supplemental information pertaining to upstream beneficial uses which may be positively or negatively affected by its decisions on salinity, diversions, pollutants, habitat requirements, and streamflows in the Delta channels, the estuary and San Francisco Bay.

Chinook salmon (or king salmon) are the primary fishery resource in the San Joaquin River drainage to be affected by this SWRCB action (Figure 1). Total production (adult harvest plus spawning run) has declined by over 70% of the 1940, 1944 and 1945 levels. Since 1968, 0.4% to 20% of the entire Central Valley fall spawning runs have occurred in this drainage (Appendix 1). It is believed that appropriate habitat conditions provided for the chinook salmon resource will also benefit other anadromous species in this drainage such as white sturgeon, and American shad, as well as resident fish populations.

The "Basin Plan" for the San Joaquin drainage as developed by the Central Valley Regional Water Quality Control Board (CVRWQCB) is in the process of revision. It should identify the beneficial uses of anadromous fish migration, spawning and emigration habitat needs throughout the San Joaquin River and all major tributaries.

This report summarizes available technical information on the life

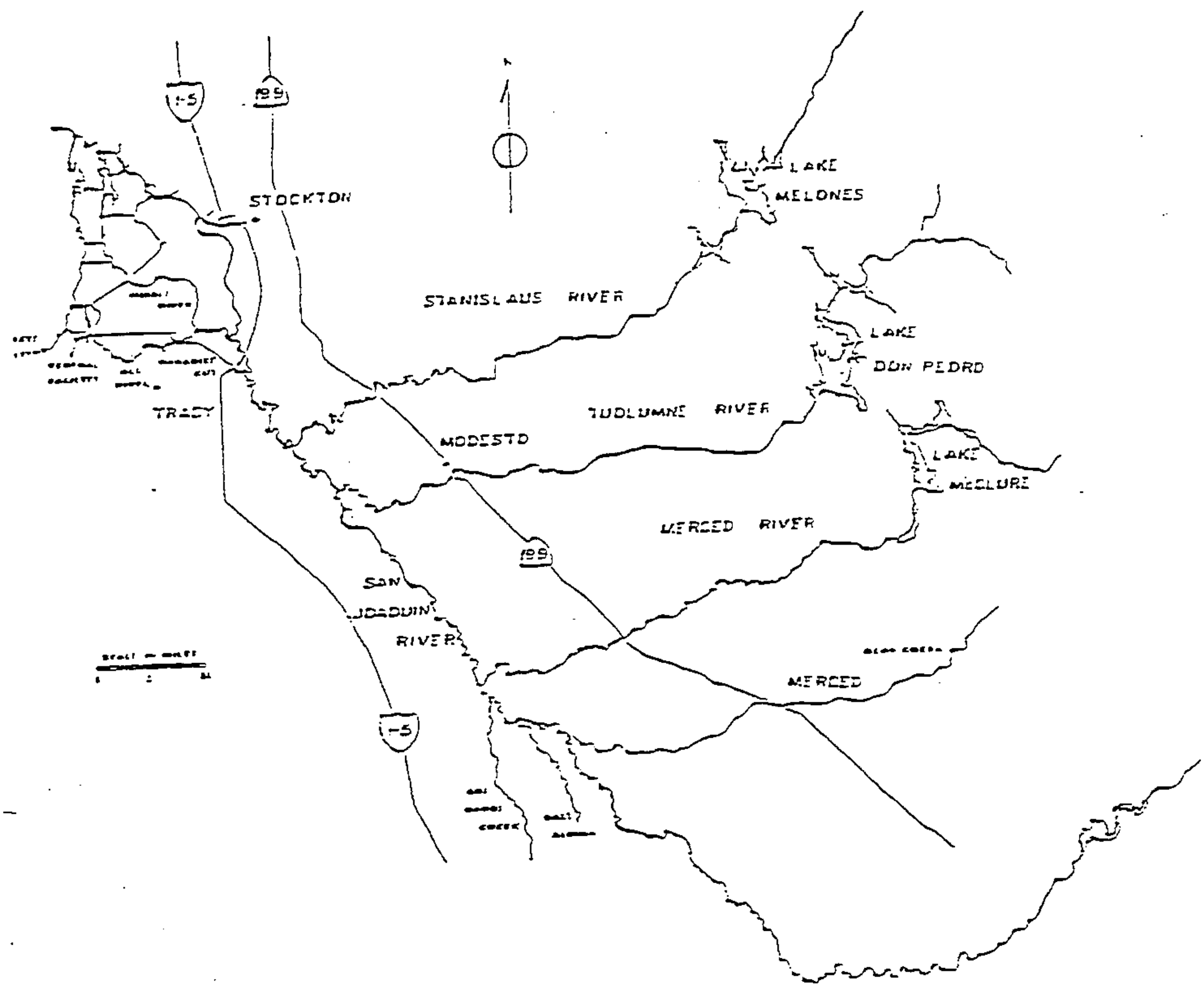


Figure 1. Map of San Joaquin River and Tributaries.

history and status of Chinook salmon and habitat conditions in the San Joaquin River drainage upstream of Stockton. More importantly, it provides available information on minimum protection levels needed for adult migration, spawning and rearing in the tributaries and emigration of young Chinook salmon of San Joaquin drainage origin into and through the Delta.

Under present conditions streamflow requirements for fall-run salmon below the major tributary reservoirs in this drainage are not adequate. All existing Licenses or Agreements fail to provide acceptable streamflow levels for young salmon emigrating to the ocean. High water temperatures on the mainstem San Joaquin are a problem during emigration. The amount of water export in the South Delta during April, May, and June of above average, average, dry and critically dry years is high relative to the San Joaquin River inflow. Consequently, juvenile salmon survival is reduced by export-related impacts.

Clearly, the needs of chinook salmon encompass water quantity and water quality conditions (e.g. streamflow, temperature, dissolved oxygen and discharge standards) in the San Joaquin River drainage and in the South and Central Delta and Estuary. Providing appropriate habitat conditions in only the Delta would result in some improvement in juvenile survival and increased adult numbers. Providing appropriate habitat conditions in the San Joaquin tributaries upstream of the Delta would also result in additional improvements in the salmon runs. Therefore, we believe the approach to recovering and maintaining this important beneficial use should incorporate provisions for the seasonal habitat needs of chinook salmon in the tributaries, the main

San Joaquin River and the Delta.

## IV. LIFE HISTORY DIGEST

Chinook salmon are the largest of the five Pacific salmon species. They are anadromous, meaning they return to fresh water to spawn after dwelling in the ocean.

The fall "run" is the principal spawning run remaining in the San Joaquin River tributaries. The distinct timing for each life stage is shown in Table 1. In addition, a small number of late spawning fish have been documented during January through March since 1984.

Table 1. Life Stage Periodicity Chart for Fall-Run Chinook Salmon in the San Joaquin River Drainage

| | JUL | AUG | SEP | OCT | NOV | DEC | JAN | FEB | MAR | APR | MAY | JUN |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| FALL RUN | | | | | | | | | | | | |
| Adult Migration | XX | XXX | XXX | XXX | XXX | | | | | | | |
| Spawning | | | X | XXX | XXX | XX | | | | | | |
| Incubation | | | X | XXX | XXX | XXX | XXX | XXX | X | | | |
| Rearing & Outmigration | | | | | X | XXX | XXX | XXX | XXX | XXX | XXX | X |

Adults return to their "home stream" to spawn using olfactory cues (smell) and some form of memory acquired during the later part of their juvenile freshwater residence. They instinctively select specific gravel size, substrate porosity, water depth and water velocity for redd (nest) sites. Behavioral and nest excavation activities precede egg deposition. All Pacific salmon adults die after spawning.

Egg incubation and hatching occurs in 50-60 days at water temperatures between 45-55°F (42.5-57.5°F is acceptable).

Thirty days after hatching, fry emerge from the gravel. They range from 30-40 mm FL (fork length) at emergence. Throughout this report we refer to 30-70 mm FL juveniles as "fry".

Fry are not well adapted to high velocity currents and spend much of their first month along the shallow stream margins in slower water. As emergence of fry increases (mid-January to mid-March) a density-dependent movement occurs. Their increasing number causes dispersal of the fry throughout the rearing habitat of the tributaries and lower San Joaquin River. This movement can be masked by premature dispersal associated with surges or spikes in daily flow or dissolved minerals within spawning areas. Dispersal of fry downstream and into the Delta appears greatest when flows (sustained or spikes) exceed 1,000-2,000 cfs in the nursery tributaries during December, January, February or March. As an example, over 400,000 fry were salvaged at the Tracy Fish Collecting Facility (Tracy Facility) and John E. Skinner Delta Fish Protective Facility (Skinner Facility) in mid-February of 1986 when a major storm resulted in tributary and mainstem discharge increases. The fate of prematurely dispersed fry into the Delta is unknown. We believe many are lost if water exports exceed inflow and flow reversals occur in the San Joaquin Delta.

Fry rear for a few months, both in tributaries and the mainstem San Joaquin River. They gradually increase utilization of deeper water, greater water velocities, and social hierarchies develop.

After a few months of growth, juveniles undergo the physiological transformation termed "smoltification". Once this process begins, "smolts" emigrate (move back) downstream to grow in salt water. The migration rate appears to be related to current velocity. During the smoltification and migration stage, imprinting of the "home stream" water occurs in natural smolts. This process is believed to occur quickly, in less than 10 days (Shirahata and Tanaka, 1969; Carlin, 1968; Jensen and Duncan, 1971; Mighell, 1975; Hasler and Scholz, 1983). Dislocation of young salmon from their "home stream" before or after smoltification can increase the straying rate for returning adults into non-origin waters. Additionally, water diverted from the "home streams" into other accessible channels which discharge into the San Joaquin River upstream or downstream of the normal entrance to the "home stream" also increases the adult straying and production loss due to inadequate habitat for spawning and juvenile survival.

Yearlings are juveniles which remain in tributaries approximately a year and migrate when a secondary "smoltification" occurs. Remaining in the stream beyond the first smoltification is a survival strategy that some chinook salmon have evolved which takes advantage of good rearing conditions when they are present in nursery areas. Yearlings enter the ocean at a larger size and generally have a greater survival rate than fry.

Yearlings have been common in the Stanislaus River in recent years. Fry, smolts and yearlings in both riverine and estuarine environments are exposed to diversions, predation, poor water quality (i.e. water temperature above 68 degrees Fahrenheit), food scarcity and disease. Once they reach the ocean, the greatest natural mortality occurs during the first year of life when salmon are still small enough to be eaten by predators.

Water conditions at the time of adult spawning migration are important, since adult salmon rely on the imprinting/learning obtained as smolts to detect and locate their "home stream". Based on this information, the key periods of concern in the Delta, the San Joaquin River and tributaries for Chinook salmon stocks are:

A) August through January: suitable water quality* and "home stream" flow from San Joaquin tributaries is needed in the principal south Delta channels connected to the estuary for fall-run and later adult migration/spawning and both wild and hatchery yearling emigration.

B) December through June: Suitable water quality* and improved streamflow conditions are needed in the upstream tributaries, lower San Joaquin River, and south Delta for fry rearing and acceptable survival of smolts during emigration.

*Water Quality implies (1) acceptable levels of chemical constituents discharge, (2) appropriate streamflow rates to afford an acceptable "receiving water" concentration, and (3) acceptable water temperatures for San Joaquin origin smolts to avoid high chronic or acute stress level for safe movement to the estuary and ocean.

## V. SAN JOAQUIN SALMON STOCKS, HABITATS AND WATER CONDITIONS

### Status

The Department of Fish and Game is required to protect salmon and their habitat and to monitor the status of salmon runs. The Fish and Game Commission Policy on Steelhead and Salmon, Department of Fish and Game Policy on stock management and the designated spawning areas as defined in Fish and Game Code Section 1505 are appended for clarification (Appendix 3).

Large runs in the early 1940's on the San Joaquin River near Fresno were predominantly comprised of spring-run fish. This run was completely eliminated after 1947 as a result of the Friant Dam closure and operation of the Central Valley Project.

Chinook salmon escapements have been documented by the Department of Fish and Game and the U.S. Bureau of Sport Fisheries (now U.S. Fish and Wildlife Service) using various techniques on one or more San Joaquin River tributaries since 1939 (Fry, 1961; Taylor, 1974; Reavis, 1983 and DFG unpublished data (Appendix 2). These estimates provide the best measure of resource status over time.

Large runs in the early 1940's on the San Joaquin River near Fresno were predominantly comprised of spring-run fish. This run was completely eliminated after 1947 as a result of the Friant Dam closure and operation of the Central Valley Project.

As indicated previously, escapement levels on the Tuolumne and Stanislaus Rivers have declined by more than 70% of the 1940, 1944 and 1945 levels (Appendix 2). Recent escapement levels remain cyclic but dramatically improve as a result of higher flows in the tributaries and the San Joaquin River at Vernalis during wet years (Figure 2). Dams on tributaries are not able to contain all the runoff during wet year periods. The recent increases from 1983 to 1985 associated with previous high spring runoff years are again dwindling to lower production levels. The 1987 run is expected to be back down to 18-25% of the 1940, 1944 and 1945 levels.

A base-level run of approximately 2,000 adults on the Merced River has been partially sustained by production and release of yearling chinook salmon from the Merced River Fish Facility (MRFF) since 1972. Salmon produced in this river also responded favorably during 1981 through 1986 suggesting that in addition to the yearling program there, achieving the full potential of this run is dependent on improved streamflow conditions. The annual production of chinook salmon at MRFF since 1970 is summarized in Appendix 4. Hatchery contribution to the San Joaquin River stocks is less than 5%.

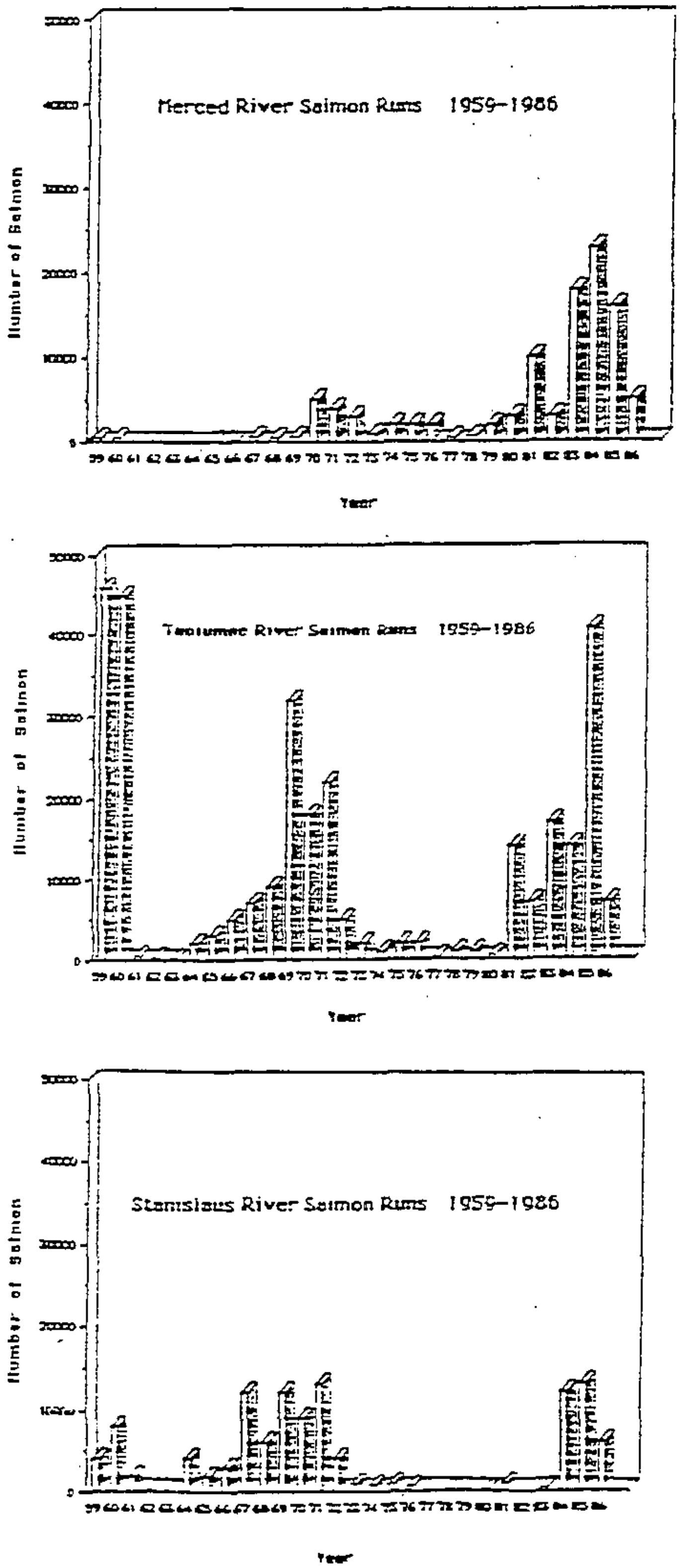


Figure 2. Recent escapement of fall-run chinook salmon in the Merced, Tuolumne and Stanislaus Rivers.

Chinook salmon runs in San Joaquin tributaries represent the southernmost latitude of freshwater existence for this species. Their ocean distribution is generally from California to Southern Canada but the majority of benefit to sport and commercial anglers is from Monterey north to the Marin County coastline. Inland harvest in the estuary and San Joaquin tributaries is less than 10% of the total adult harvest.

The existing required fishery streamflows in the Merced and Tuolumne rivers reduce in April and May prior to the end of the juvenile rearing periods (Appendix 5). Few of the fry or smolts that remain in these tributaries beyond this period survive. This is due principally to severe reductions in living space, high water temperatures and predation. In some years this represents a significant loss of annual production. Available water in the Stanislaus drainage has resulted in acceptable streamflow and temperature conditions in this tributary most of the last four years. Rearing and emigration flows on the main San Joaquin River are reduced and water temperatures increase just when smolts critically need suitable conveyance flows to enter the Delta. Agricultural return flows to the San Joaquin River above the Merced River confluence increase in April and May as water and ambient air temperatures rise significantly.

Spawning Gravels

The recent escapements of 23,000, 41,000 and 13,000 adult salmon on the Merced (1984), Tuolumne (1985) and Stanislaus Rivers (1985) provide the best measure of spawning habitat potentials in the San Joaquin tributaries. Redd (or nest) overlap problems which result in increased egg mortality were not documented during those years. The spawning adults were dispersed throughout the available spawning habitats.

Gravel renovation work on the San Joaquin River spawning tributaries in the early 1970's did not immediately result in improved escapement. Even today, spawning area capacity does not appear to be the most important factor limiting recovery of escapements to near historic levels. Increases in spawning habitat area may be needed in the future to offset gravel depletions or vegetation encroachment.

Basin Water Storage Trends

A joint study by USBR and South Delta Water Agency (SDWA) in 1980 identified a pre-and post- CVP reduction of April through September adjusted cumulative runoff at Vernalis of 1.02 million acre-feet (Figure 3). The study determined that the post-CVP change amounted to a 42% reduction in cumulative runoff.

The reduction in quality and quantity of the San Joaquin River streamflows in the Delta has affected San Joaquin chinook salmon production.

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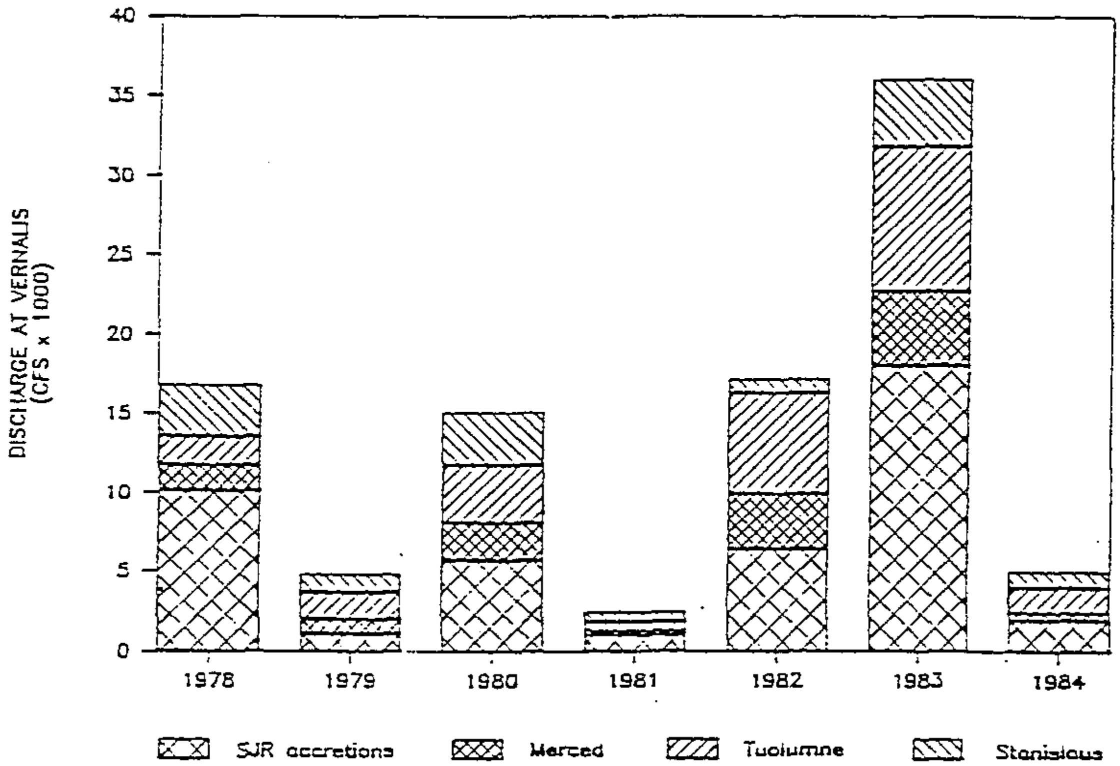
respectively (Figure 4). "Mean spring contributions" -78 — '84

Figure 4. Mean monthly streamflow in the San Joaquin River at Vernalis during the spring outmigration of fall-run chinook salmon. 1930-1944, 1945-1952 and 1953-1984.




## IV. NATURAL PRODUCTION FACTORS

### Timing of Juvenile Emigration

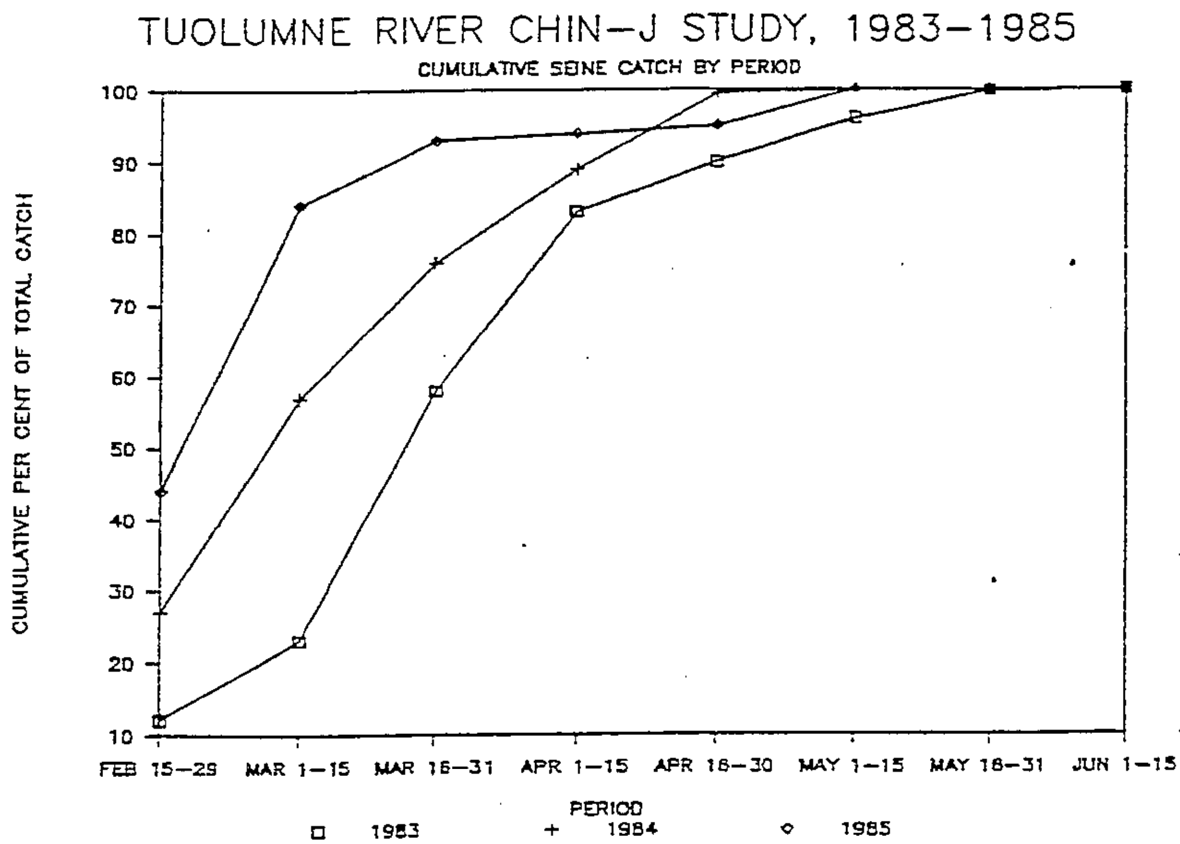
The majority of the annual salmon production leaves the San Joaquin tributaries as fry and smolts each spring. Results of our beach seine survey at four sites on the Tuolumne River in 1983, 1984 and 1985 provide one measure of fry and smolt migration timing (Figure

5). Very few fish were sampled beyond May 15 except during the high spring flows in 1983.

Tuolumne River flows in the rearing habitat during the fry rearing periods each year were dramatically different (Appendix 6). This data was plotted as hourly flow to show the changes in nursery habitat flows which occurred daily (i.e. 400 cfs to 4,500 cfs and back to 400 cfs daily). In reviewing published streamflow records, the effect of substantial flow changes are masked by records depicting mean daily or mean monthly streamflows.

The cumulative seine catch differences between our 1983 (high stable fry period flows), 1984 (high but less stable flows) and 1985 (dramatic fluctuating flows in the fry period) show that differences in tributary rearing periods did occur with different flow regimes. Those differences were most dramatic early in the season when fry dominated the catch. The 80% of cumulative catch level was reached in early March, late March and early April in 1985, 1984 and 1983, respectively.

Figure 5. Cumulative percent of total chinook salmon juvenile beach seine catch in the Tuolumne River, 1983, 1984 and 1985



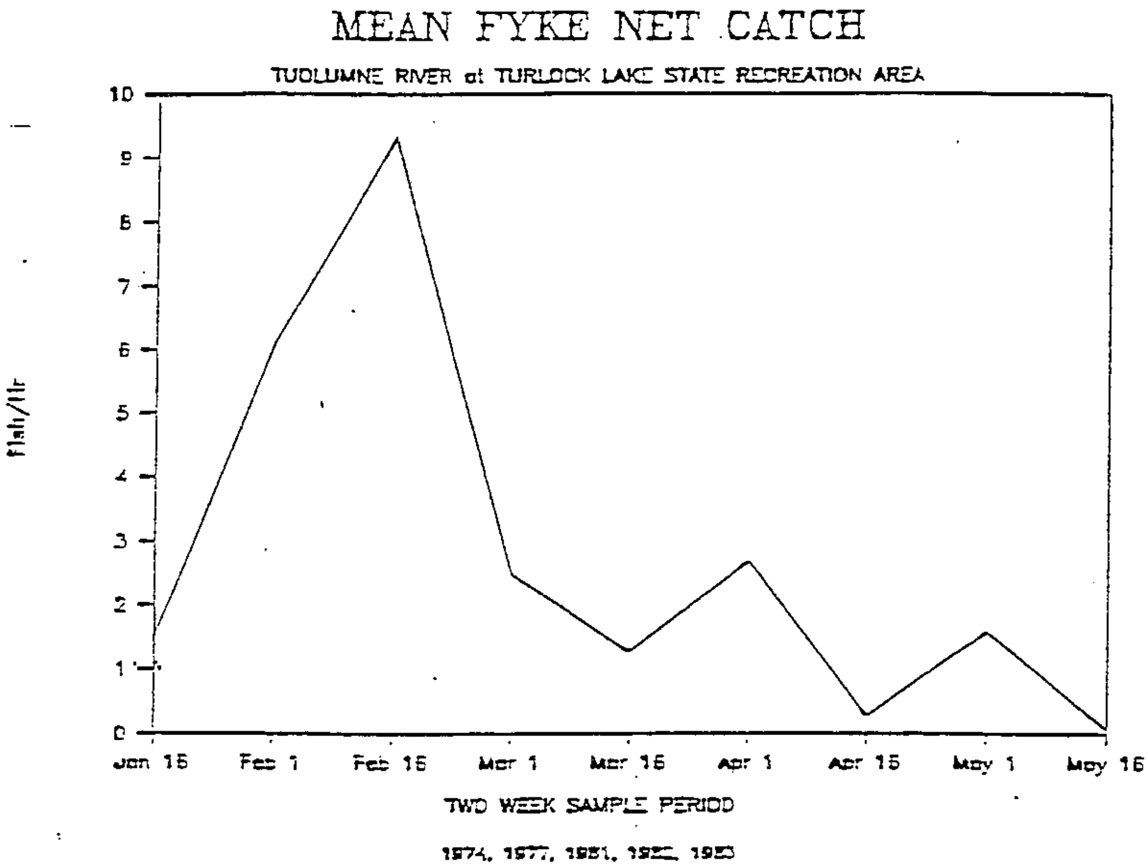

Juveniles were present into June in 1983 but very few remained by early April in 1985. External smolt characteristics and the size distribution data obtained during two years of our Tuolumne River seine survey (Appendix 7) showed many juveniles throughout the Tuolumne River were 70 mm in fork length and had begun their smolt transformation by April 1.

San Joaquin origin smolts migrating into the Delta via Old River arrive at the Tracy Facility first. It can be assumed that in excess of 90% of the chinook salmon salvaged are of San Joaquin River origin (Knutson 1980). Salvage refers to those fish which are successfully separated from the water to be exported and eventually transported by truck to the estuary. An estimate of the minimum fraction of Sacramento salmon collected at the facilities is provided in Appendix 8; the remainder are of San Joaquin drainage origin.

Since most adult salmon return to spawn in their third year of life, the escapements resulting from the juvenile production years of 1983, 1984 and 1985 are in the fall of 1985, 1986 and 1987, respectively. Spawning runs in 1985 and 1986 were 41,000 and 7,000 adults, respectively. The run in the fall of 1987 will coincide with the cumulative seine catch data showing the shortest rearing period.

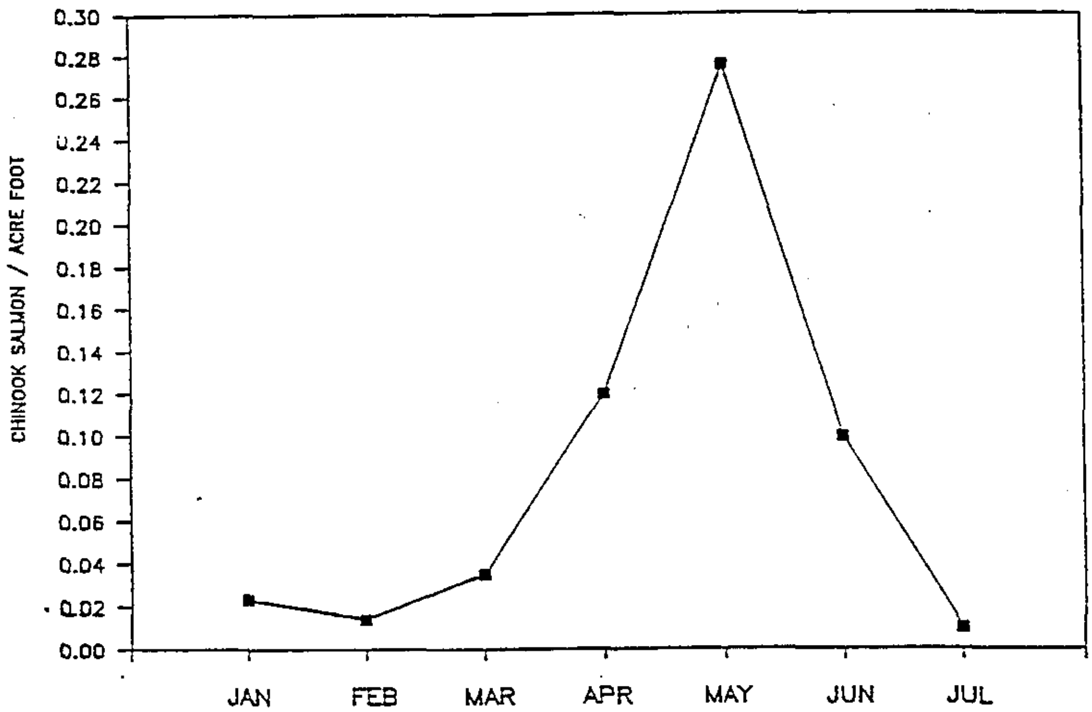
A second measure of fry and smolt migration timing was derived from five years of fyke netting (3' x 5' mouth) effort at Turlock Lake State Recreation Area in the Tuolumne River spawning reach (Figure 6). While this net is relatively ineffective for juvenile salmon exceeding 60-65 mm FL it does provide a reasonable description of the density-dependent movement period when fry are distributing to unoccupied rearing habitats. The declining catch into mid-March either infers that more juveniles are reaching the size where net avoidance is significant or fewer fish are actually moving past the net site. We believe both are probably true.

Figure 6. Catch of juvenile chinook salmon in fyke nets at Turlock Lake State Recreation Area (Tuolumne River) in 1974, 1977, 1981, 1982 and 1983




A third and more robust description of the timing of fry and smolt emigration from the San Joaquin tributaries into the Delta was derived from the Tracy Facility mean monthly salvage estimates for the period 1968-1980 (Figure 7).

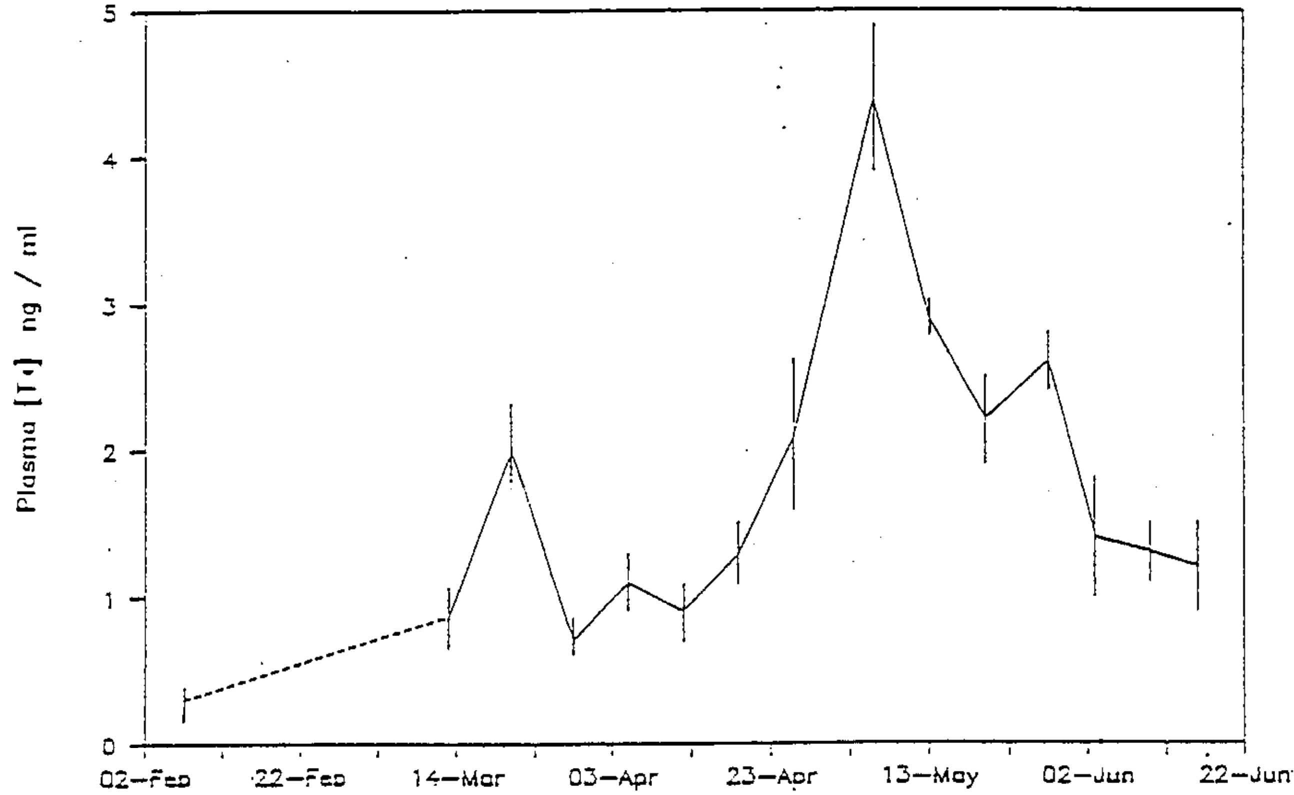
Figure 7. Chinook salmon salvage rates at the Tracy Facility (CVP) from 1968 to 1980.




To evaluate if fry movements occurring each spring were passive (density-dependent) or "active" (true emigrations) juvenile salmon blood plasma thyroid hormone thyroxine ($T_4$), levels were monitored weekly in the lower Stanislaus River (3/6/85 to 4/20/85) and in the spawning channel at Merced River Fish Facility (2/7/85 to 7/3/85). One sample was obtained on 5/7/85 from the Delta at the Tracy Facility to see how hormone levels there compared to those upstream in the tributaries. Increased thyroid gland activity during smoltification (Hoar, 1939, 1976; Baggerman, 1960; Nagahama et al., 1982) generally occurs in parallel to an increase in the blood plasma levels of thyroxine. Levels were measured by radio-immuno assay (Dickhoff et al., 1978). The acute rise of thyroxine levels in spring is normally associated with the peak of smoltification whereas the more gradual increases earlier are a physiological clue that "active" migration is occurring.

We did detect the expected elevation of blood plasma thyroxine levels (Figure 8). Water conditions in the spawning channel were stable throughout the monitoring period but the Stanislaus River flows fluctuated. In the absence of river-related stimuli the thyroxine hormone levels at MRFF spawning channel rose steadily in early April to a peak beginning in early May and stayed high through late May. The Stanislaus River fish thyroxine levels indicate several subtle hormonal responses occurred in late-March and late-April with a peak exceeding 4 ng/ml in mid-May. Chinook salmon sampled in the Delta at the Federal Fish Facility on May 7, 1985 had plasma thyroxine levels exceeding 13 ng/ml more characteristic of the peak of smoltification.

Figure 8. Merced Fish Facility (lower) Spawning Channel (upper) and Stanislaus River (lower) chinook salmon blood plasma thyroid hormone ($T_4$) levels through the 1985 smoltification period.




Based on these different measures of emigration timing:

A. San Joaquin origin chinook salmon smolts are dispersed throughout the tributaries and lower river by mid-February and generally emigrate to the Delta during the period March 15 through at least the first week of June.

B. Since fry are most abundant and are typically involved in passive, density-dependent movements during the period January through March, they are particularly susceptible to tributary streamflow changes and conveyance into the Delta.

Water Temperature and Vernalis Spring Flows

Water temperatures in the San Joaquin Drainage spawning tributaries and along the mainstem into the Delta is an important factor affecting growth and survival of Chinook salmon juveniles. Salmon maximum and minimum temperature tolerance, rapid-rise tolerance and the effect of acclimatization temperatures on salmon temperature tolerance has been examined (Brett, 1952; Orsi, 1971). Generally, acclimatization increases short-term temperature stress tolerance, but Chinook salmon mortality begins when temperatures reach 75 degrees F. (23.9$^{\circ}$C). Salmon swimming speeds, feeding, growth and vulnerability to diseases and predation are all affected by increased water temperature (Brett et al., 1958; Shelbourn et al., 1973; Coutant, 1973; Hughes et al., 1978). Wedemeyer (1973) showed that a rapid sublethal rise of 10 degrees C. caused several responses in salmon (eg. increased pituitary activity) and felt that such stress especially in downstream migrants should be avoided. However, substantial water temperature elevation is also one of the factors believed to beneficially stimulate salmon outmigration (Foerster, 1937; Grau et al., 1982; R.S. Nishioka, personal communication).

The sublethal effects of elevated water temperature on fish survival have been measured (Rich, 1987). Chronic stress is generally indicated in juvenile Chinook salmon by:

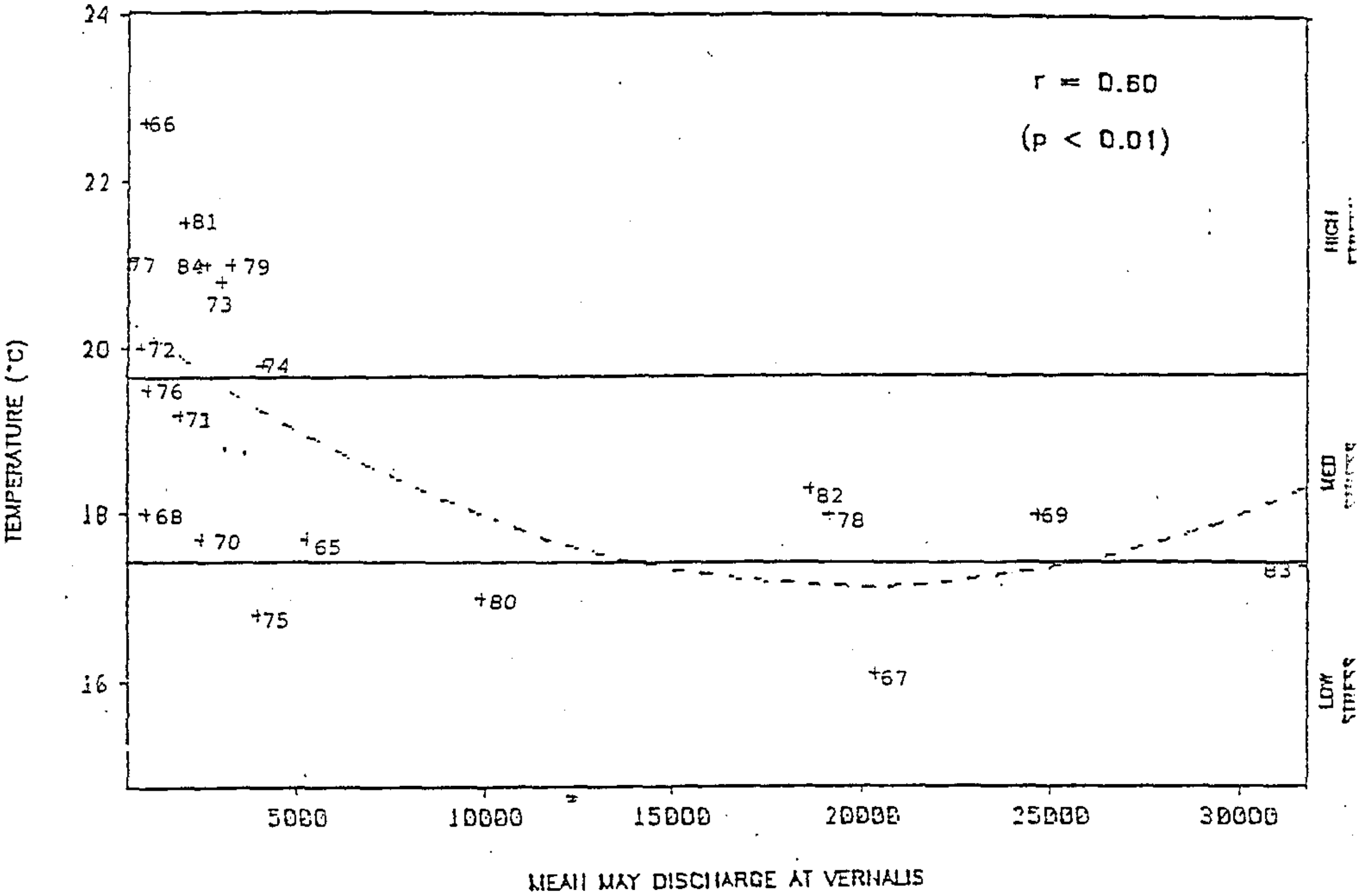
1) increased metabolic activity,
2) lowered resistance to disease,
3) reduced growth rates, and
4) clinical responses (e.g. increased blood hemoglobin).

Each symptom independently may not result in detectable reductions in the survival to adulthood but together and in combination with other stressful conditions, they can result in significant mortality. This is especially harmful if high chronic stress affects significant proportions of the annual juvenile production. Acute thermal stress results in high and direct mortalities or halting of downstream migration. Smolts constantly sense water temperature, therefore excessive temperatures may cause them to delay migration or return to cool water habitats upstream. High natural mortality results when fish remain in the Merced and Tuolumne River beyond May 1. These losses are generally a result of diminished streamflow, high temperatures and predation.

We analyzed the relationship between San Joaquin River flows at Vernalis and March, April and May water temperatures from 1965 to 1984 in conjunction with the chronic temperature stress levels (Rich, 1987) for San Joaquin smolts entering the Delta. The USGS temperature records at Vernalis were reported in different ways so the summary required different treatments. Mean monthly temperatures for the period 1964-1969 were used as published. The mid-point (median) was used when maximum-minimum temperature ranges were provided (1974-1984).

We found no correlation between streamflow and water temperature in March or April. A significant ($p < 0.01$; $r = 0.60$) curvilinear relationship was found in May under the streamflow and weather conditions existing 1965-1984 (Figure 9). Using this relationship and overlaying the chronic temperature stress criteria we found that at Vernalis flows of 5,000 cfs or less in May, chinook smolts entering the Delta are subjected to high chronic temperature stress (Figure 9). In looking at the actual temperature data for all May periods corresponding with Vernalis flows less than 5,000 cfs, in 8 of 13 years the water temperatures were in fact in the high stress range. The years 1971 and 1976 were also very close to the high chronic stress temperature of 67.6$^{\circ}$F (19.7$^{\circ}$C).

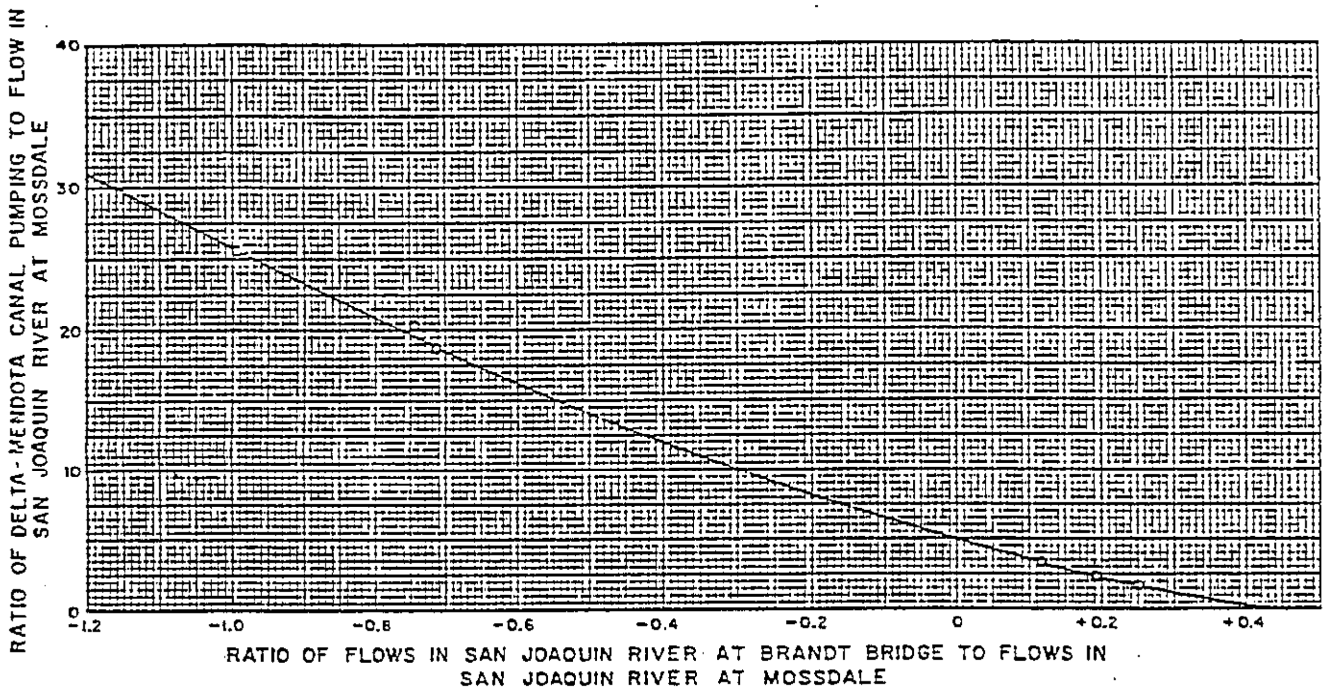
Figure 9. Relationship of San Joaquin River Streamflow (May) and Water Temperatures at Vernalis in Relation to Chronic Thermal Stress for Chinook Salmon Fry and Smolt Development



Recall that the peak migration of chinook salmon smolts from the San Joaquin tributaries is in early May. Therefore, at least half of the annual San Joaquin smolt emigration period would be impacted by high temperatures a minimum of 62% of the years when May streamflows are less than 5,000 cfs (i.e. 8 of 13 years).

Also consider that under current operations in May the Tracy Facility is pumping at 3,000 cfs and the Skinner Facility is filling the forebay on the flood tides and pumping at night. Based on this export rate and the relationships in Figure 10, if the Mossdale flow in May were 5,000 cfs then approximately 3,500 cfs (70%) would be diverted off the San Joaquin River and down Old River to the Tracy Facility. Approximately 1,500 cfs (30% of Mossdale flow) would enter the central Delta via the San Joaquin River past Stockton. This scenario results in temperature stressed salmon from the San Joaquin drainage being subjected to added stress associated with the State and Federal water export processes (see DFG Exhibit #17). Those salmon which remain in the San Joaquin River would also be subjected to high chronic thermal stress but fewer smolts emigrating down this channel are directly impacted by the Water Export process (see USFWS Exhibit #   ).

Figure 10. Ratio of flows at two locations on the San Joaquin River as Influenced by Delta pumping.




NOTE: Flows in northwesterly direction in San Joaquin River at Brant Bridge positive and in opposite direction negative.

STATE OF CALIFORNIA
THE RESOURCES AGENCY OF CALIFORNIA
DEPARTMENT OF WATER RESOURCES
DELTA BRANCH
DELTA WATER FACILITIES

RATIO OF FLOW AT TWO LOCATIONS ON SAN JOAQUIN RIVER AS INFLUENCED BY DELTA-MENDOTA CANAL PUMPING

APRIL 1962

Studies by Schreck et al. (1985) on the Columbia River have documented that stress factors are accumulative. Combining high chronic thermal stress with the rigors of salvage (trash racks, louvre or perforated plate screens, predation, tank handling, trucking and release into water of differing salinity and temperature) during the process of smolt transformation surely accounts for a large proportion of "natural mortality" in the Delta.

Finally, in reviewing the historic salmon runs (Appendix 2) it appears that escapements, two years after each May when high chronic stress occurred (including 1971 and 1976), were consistently lower than the previous or following years. This suggests that temperature conditions in the Delta directly affect the survival of smolts leaving the San Joaquin drainage.

Based on this information:

A) Up to half the production of San Joaquin chinook salmon smolts can be subjected to high chronic thermal stress in the south Delta in most (62%) years when Vernalis flows are 5,000 cfs or less.

B) Given that significant additional stressful factors exist immediately downstream of Vernalis in the Delta, juvenile salmon from the San Joaquin drainage need to enter the Delta in the best possible condition to optimize survival to the ocean.

Agricultural Drainage

Mud and Salt Sloughs (Merced County) are used to convey subsurface agricultural drainage to the San Joaquin River upstream of the Merced River confluence (Figure 1). The SWRCB appointed a technical committee on agricultural drainage in the San Joaquin Valley. The committee identified four primary water quality constituents of concern and recommended that water quality standards be adopted for them. Selenium was identified as the constituent that would most affect fish and wildlife beneficial uses (SWRCB Draft Order #W.Q.85-1, 1986).

Gilliom (1986) monitored concentrations of selenium, dissolved solids, boron and molybdenum at 11 sites on the San Joaquin River and tributaries twice per month from June to September, 1985. Dissolved selenium was lowest in the Stanislaus, Tuolumne and Merced Rivers and the San Joaquin River near Stevinson (all less than 1 microgram per liter except for one sample in the Tuolumne River). Mud Slough had the highest median concentrations of dissolved selenium (21 micrograms per liter).

Selenium exhibits direct toxic effects to fish from exposure to elevated levels in the water column and through bioaccumulation through the food web to harmful tissue levels.

Studies have shown that the survival of chinook salmon swim-up fry was significantly reduced by exposure to two types of dietary selenium of 26 parts per million (ppm) for 60 days and exposure to 6.5 ppm for 90 days. Growth was significantly reduced 10 to 28% in salmon fry fed 13 ppm selenium for 90 days in two different diets (Hamilton et al., 1987). A second chronic toxicity study by the same researchers used 70 mm chinook salmon in water siumlating San Luis Drain water (1.2% brackishwater minus trace elements) and fed similar diets as in the swim-up fry tests. Fingerling survival was not affected by dietary selenium but growth was significantly reduced 20% in fish fed 6.5 ppm selenium in the diet (San Luis Drain mosquito fish) for 120 days. In the other diet test (selenomethionine additions), growth was significantly reduced 16 to 41%. Following 120 days exposure to the experimental diets, a 10-day seawater challenge test was conducted (28% seawater). Survival of fingerlings was 37% in the 26 ppm selenium diet (San Luis Drain mosquito fish) and 24% in the 26 ppm selenomethionine diet compared to a control test survival of 87%.

The authors concluded that if irrigation return flows from the San Luis Drain were disposed of in a freshwater or brackishwater receiving water like the San Joaquin River system, chinook salmon population would be adversely affected by exposure to dietary sources of selenium.

In another study, a balanced diet containing selenium-contaminated mosquito fish from the San Luis Drain was fed to chinook salmon for the month preceeding smolting. The salmon tended to grow less than controls in fresh water. The ability of these salmon to osmogregulate was delayed, and their migratory behavior was reduced (Palmisano, 1987).

From this information we conclude:

<u>Chinook salmon fry and smolts are sensitive to both dissolved and dietary selenium.</u>

<u>Although levels exceeding 1.0 microgram per liter have not</u>

been detected in the San Joaquin River and tributaries where juvenile salmon reside and feed each spring, a potential problem exists and an appropriate water quality standard for selenium should be established.

Tidal Influence on Smolt Migration

During April 15 through May 15, 1987, downstream migration of CWT and wild chinook salmon in the lower San Joaquin River was measured with a Kodiak trawl net about one mile below Mossdale Landing in the South Delta, San Joaquin County. Ten 10-minute trawls were performed on a daily basis. The time of initiation and total salmon catch per trawl was recorded. A relatively constant water volume was sampled during each 10-minute tow.

The time of each trawl was ranked in relation to the tidal activity on a scale of 1 to 4, with 1 equal to the time of peak high tide (flood) and 4 equal to the time of peak low tide (ebb). These ranks were proportioned equally among the difference in time between the time of peak low tide and peak high tide, and then among the difference between the next successive tidal change. Each ranking included data from 50-75 tows. These ranks ignore the effects of varying tidal heights and account only for the variation due to relative difference in tidal changes by time.

The distribution of total chinook salmon catch per tow for each rank is shown in Figure 11 using non-parametric box plots (Tukey, 1977). These distributions for each rank met the assumptions and were analyzed using a one-way ANOVA (Sokal and Rohlf, 1969).

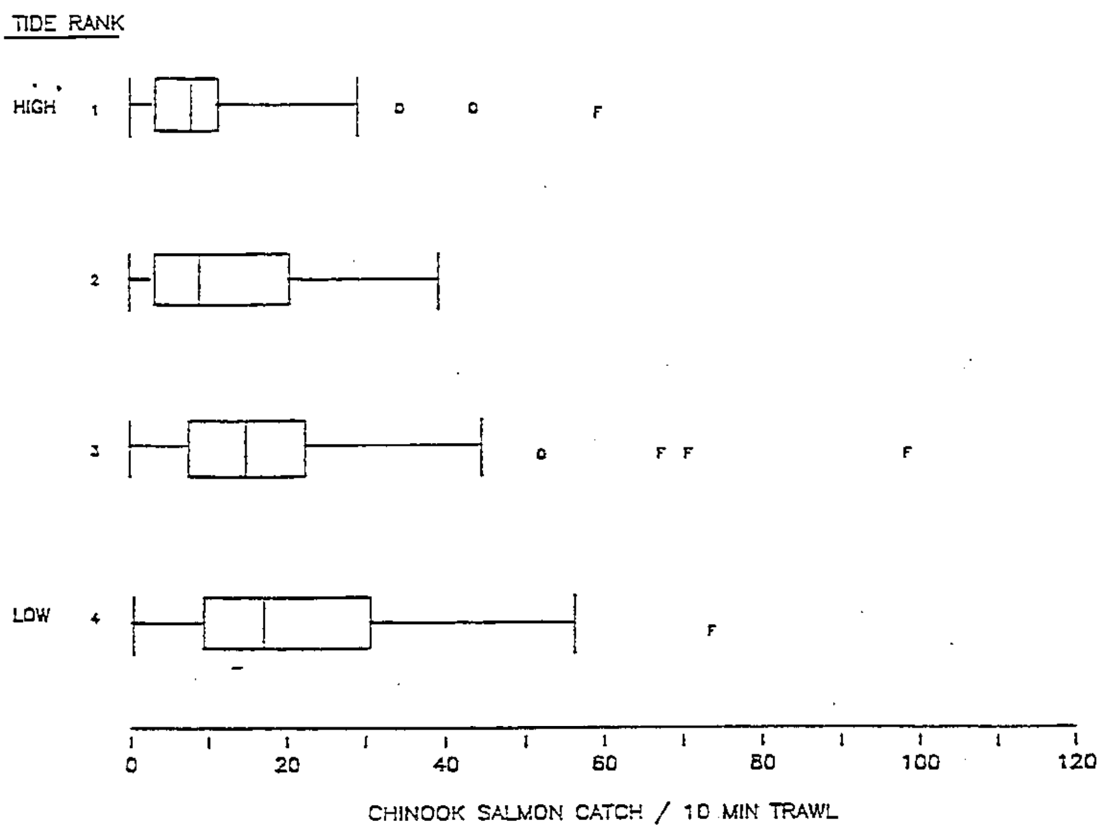
The mean catch per trawl was significantly different ($p = 0.05$)

for each tidal rank. Further analysis using the a posteriori technique of Least Significant Ranges (LSR), showed that each tidal rank was significantly different from the adjacent rank ($p = 0.05$).

Therefore, data from our first year of this evaluation suggest:

<u>On average, significantly more salmon moved past our Mossdale trawl site during ebb tide transitions under the 1987 April-May streamflow conditions. If further study confirms this result then new alternatives to improve juvenile survival through the south Delta may exist.</u>

Figure 11. Catch per tow of chinook salmon in Kodiak Trawl Surveys at Mossdale on the San Joaquin River, April and May, 1987

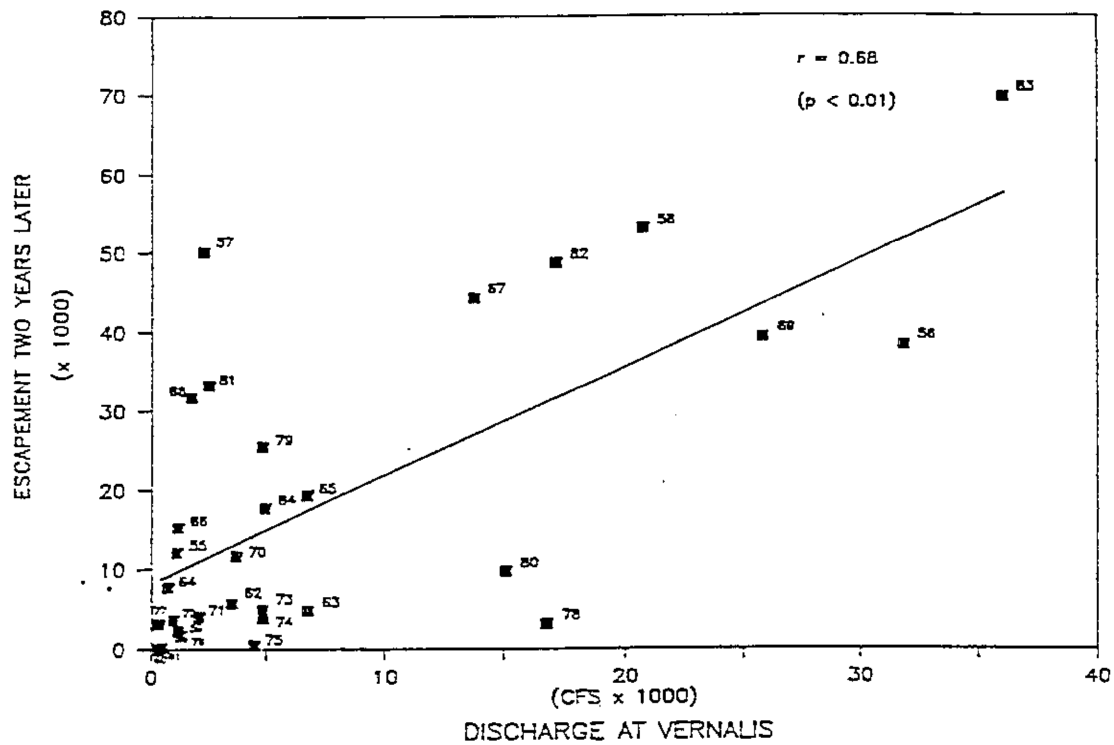


Flow at Vernalis and Adult Numbers

In 1972 the Department of Fish and Game submitted an Exhibit and testimony to the SWRCB regarding the New Melones Project which concluded that spring flows were the most important factor controlling the size of salmon populations in the Stanislaus River, with survival being proportional to flow (DFG, 1972; Appendix 9). A similar relationship existed on the Tuolumne River at that time. Most juvenile salmon emigrate to the Delta in the spring months and the majority of adults resulting from this emigration continue to return to spawn during the fall two calendar years later.

The relationship of streamflows at Vernalis and the total escapement in the drainage continues to indicate that spring flows are still a key factor determining the number of adults produced in the San Joaquin River tributaries (Figure 12). The range of correlation coefficients are from 0 for no relationship to 1.0 for a perfect relationship. The coefficient for the relationship between Vernalis flows and adult escapement 2 years later is 0.66 ($p < 0.01$). Considering all the potential factors affecting juvenile salmon numbers in the tributaries (e.g. streamflow fluctuations, stranding or lack of May rearing flows) in the San Joaquin River (e.g. May temperature stress), the Delta (e.g. predation and water export losses), the estuary and two years in the ocean, an "r" value of 0.68 ($p < 0.01$) indicates that substantial spring flows in the San Joaquin River override most other constraints to salmon production.

Figure 12. Flows in the San Joaquin River during Juvenile Emigration (1955-1984) and Corresponding Adult Spawning Population 2+ Years Later




Based on this information:

<u>The number of San Joaquin drainage salmon produced is largely determined by the spring flows in the San Joaquin River at Vernalis when the young emigrate to the ocean.</u>

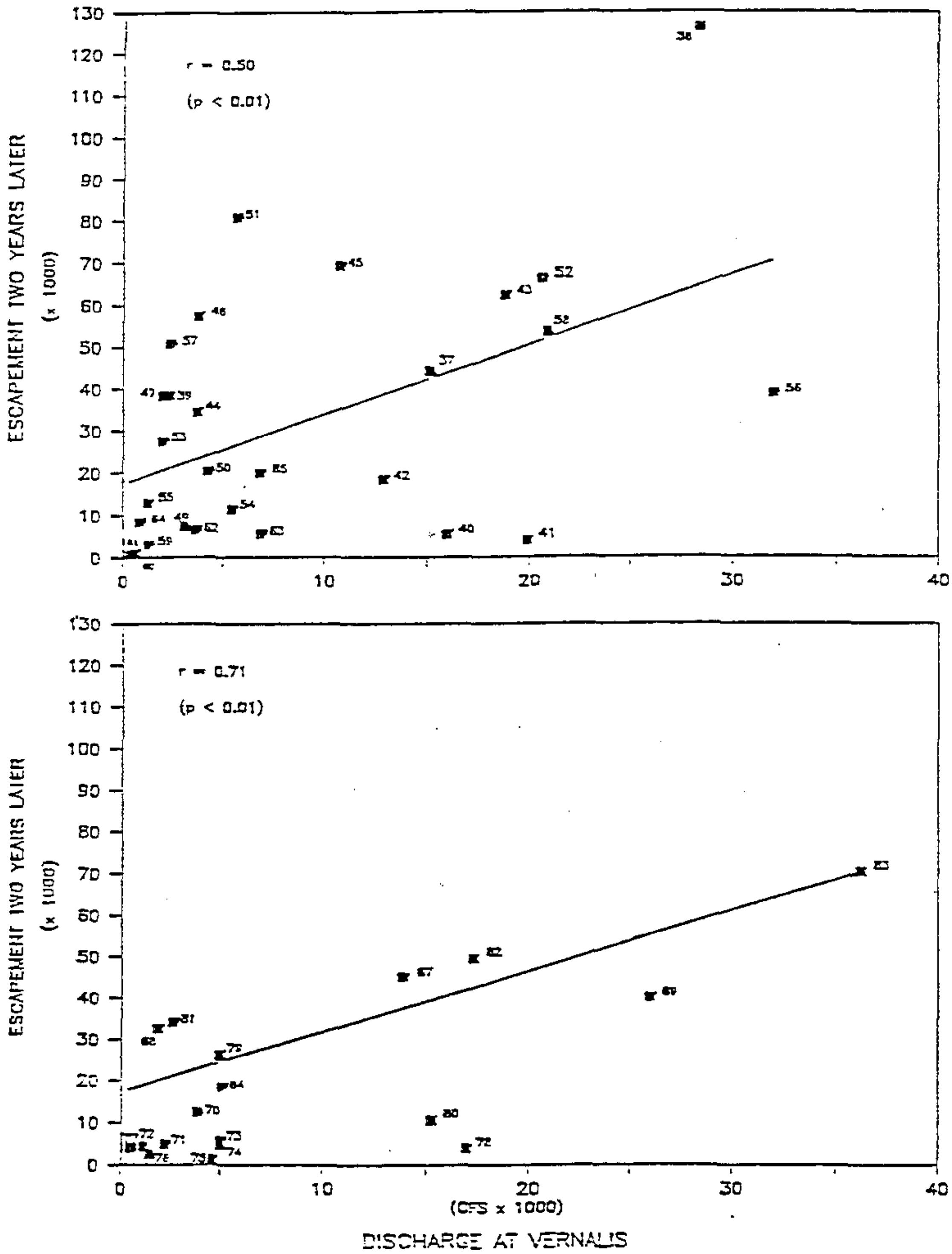
We implemented a long-term smolt survival study in 1985 which has as its goal a determination of San Joaquin River and tributary streamflows needed for acceptable salmon production. This program dovetails with a similar program in the Delta and the Sacramento and Mokelumne Rivers through the Interagency Study Program. A summary of the first three years of San Joaquin drainage smolt survival information will be described along with the Sacramento River and Delta smolt survival information for continuity and better understanding.

Additional evidence on recent changes in the escapement vs. Vernalis flow relationships helps clarify the importance of resolving the need for improved spring flows in the San Joaquin drainage (Figure 13). There is a subtle but distinct reduction in the regression slope since 1967 suggesting that for a given escapement level (40,000 indicated by the dotted lines) it now requires 16,000 cfs instead of 14,000 cfs. This indicates that even under the established streamflow programs implemented in conjunction with recent water storage enlargements on the tributaries (Table 2) the combination of San Joaquin River and Delta impacts has gotten worse for salmon. In the absence of measures which improve smolt survival in the Delta, increased flows would be required at Vernalis to yield the desired effect.

Based on this information:

The number of San Joaquin drainage adult salmon produced is largely determined by the spring flows in the San Joaquin River at Vernalis during the period young salmon emigrate to the ocean.

Figure 13. Relationships of Total Escapement in the San Joaquin Drainage and Vernalis Flows Before (Upper) and After (Lower) the Existing State Water Project in the South Delta and Major Storage Increases in the San Joaquin Drainage.

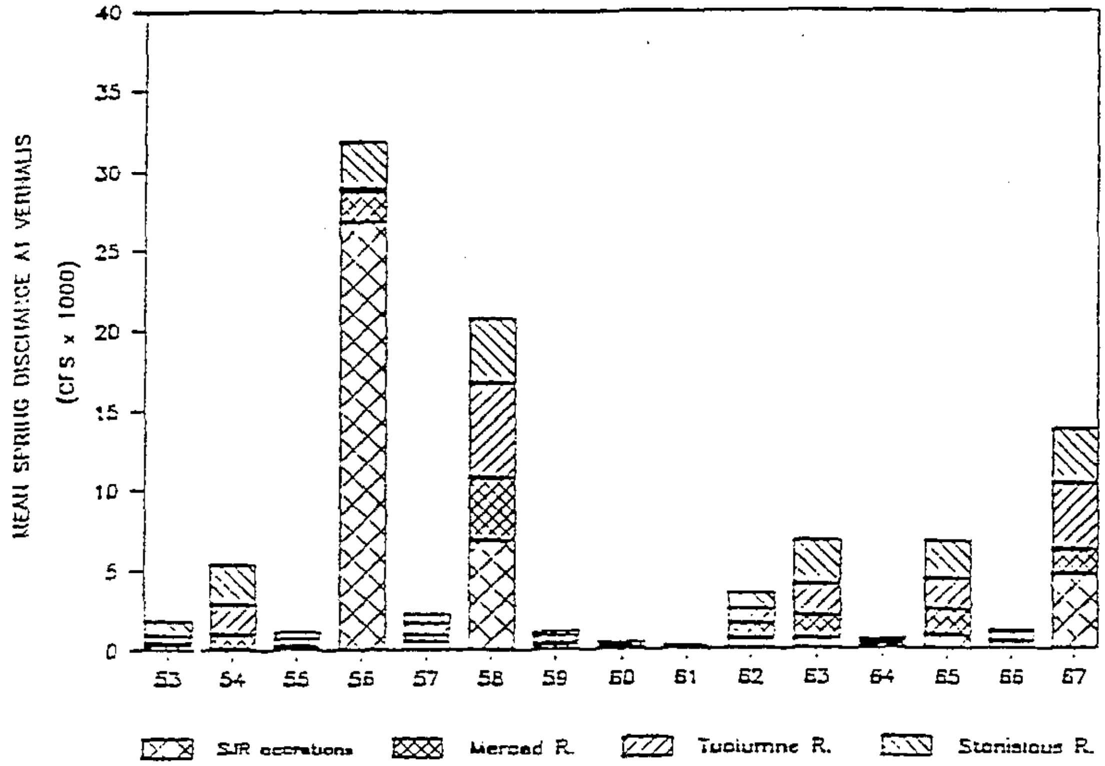


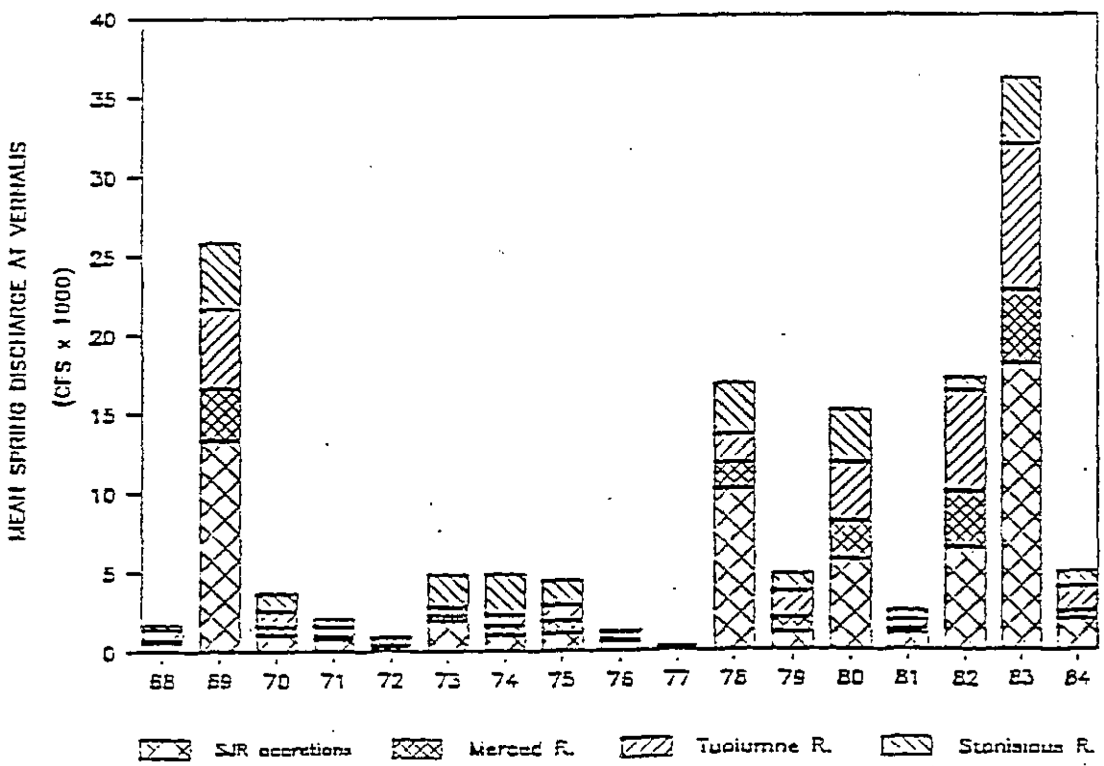
The habitat conditions resulting from limited tributary contributions and impacts in the south Delta are such that it takes approximately 10-15% more spring flow to result in similar escapements which occurred prior to 1967.

Tuolumne River Flow and Adult Numbers

Obviously the spring Vernalis flow is only a composite of the flows leaving the tributaries upstream. An example of the recent contributions is provided in Figure 14.

Figure 14. Tributary Contributions to the Spring (March through May) San Joaquin River Flows at Vernalis, 1953 through 1967 (upper) and 1968 through 1984 (lower)



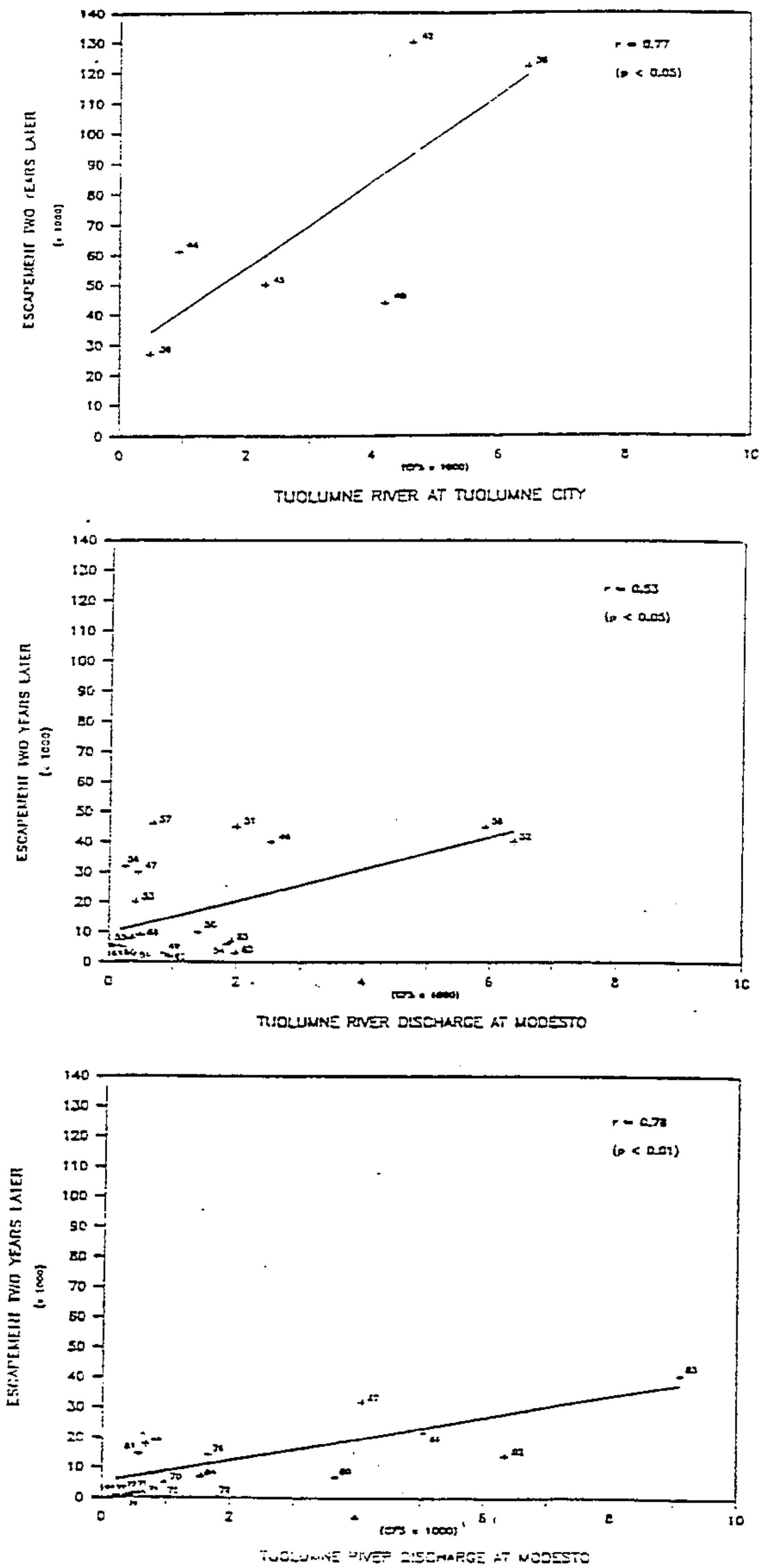
Recall that Table 2 listed the major changes in storage capacity in 1967 (Merced River), 1971 (Tuolumne River) and 1978 (Stanislaus River, filled in 1981). San Joaquin River accretions include the Kings River, Delta-Mendota Canal, Mud and Salt Sloughs, the Eastside Bypass, Berm Check and other minor sources of water.

Escapement estimates and streamflow data for the Tuolumne River are available back to 1938 (Appendix 2; USGS records at Tuolumne City and Modesto). A comparison of the relationships between escapement and mean spring flow in the Tuolumne River during three time intervals more clearly defines how chinook salmon production has responded to changes in spring flows and water exports in the South Delta (Figure 15).

The Tuolumne River escapement generally represents 40% to 50% of the average total escapement in the San Joaquin drainage and therefore provides a fair indication of salmon needs. The declining trend in the slopes of these three relationships in Figure 15 is even more dramatic than similar relationships at Vernalis (Figure 13) and a reduced frequency of escapements exceeding 30,000 adults has occurred. The predicted Tuolumne River spring flows required to produce 30,000 adults has increased from approximately 1,000 cfs (exceeded in all but dry year scenarios during 1938-1945) to 6,000 cfs (now exceeded only in wet years) in 1967-84.

The decline in frequency of escapements exceeding 30,000 adults was 83%, 35% and 11% during these three periods, respectively.

Figure 15. Relationships of Tuolumne River Escapement to Spring flows prior to Delta water developments (top), after CVP development in the drainage and after the SWP and additional storage development in the drainage (bottom)



Based on this and previous information provided:

A. In the absence of improved habitat conditions in the San Joaquin River and Delta, the full potential of Tuolumne River salmon production will only be in wet years when the Tuolumne River mean spring outflow exceeds 6,000 cfs.

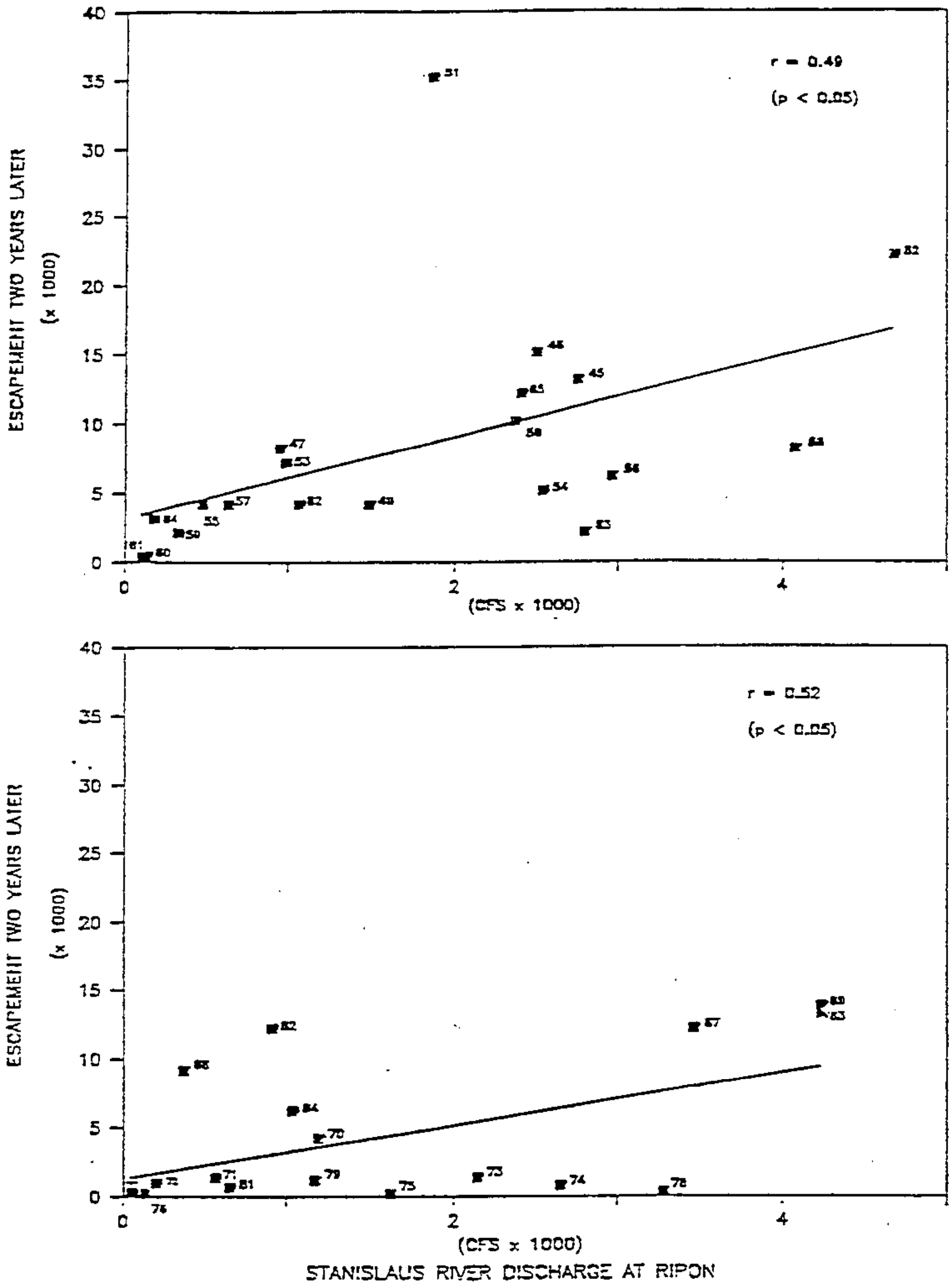
B. Improved tributary flows during the smolt emigration period are important to salmon survival in the tributaries but factors downstream have diminished the positive effects of incremental increases in spring flows.

C. Improvements in emigration flows from the Tuolumne River would also benefit smolts from the Merced and Stanislaus Rivers.

Stanislaus River Flow and Adult Numbers

A similar decline in slope in the relationship between mean spring flow and escapement 2 years later has occurred in the Stanislaus River salmon run (Figure 16). The dotted lines on this Figure indicate that escapements near 10,000 salmon resulted from mean spring outflows near 2,300 cfs at Ripon prior to 1967. After that period, the same escapement results from twice the mean spring flow.

Figure 16. Relationships of Stanislaus River Escapement to Spring Flows Before (upper) and After (lower) the existing State Water Project in the South Delta and Major Storage Enlargements in the San Joaquin Drainage



Based on this and previous information provided:

A. In the absence of improved habitat conditions in the San Joaquin River and Delta, the full potential of Stanislaus River salmon production will only be met in wet years when mean spring flows exceed 4,500 cfs.

B. Improvement in emigration flows from the Stanislaus River could also benefit smolts from the Merced and Tuolumne Rivers travelling down the San Joaquin River if acceptable streamflow requirements were established.

Merced River Flow and Adult Numbers

We found no relationships between mean spring flows at Cressey on the Merced River and escapement 2+ years later. The channel dimensions of this river are most similar to those of the Stanislaus River. Therefore, we assumed that mean spring flows which were predicted to obtain the potential salmon production on the Stanislaus River would also be adequate for the Merced River.

Tributary Flow Requirements

As stated earlier, recovery and maintenance of Merced, Tuolumne and Stanislaus River salmon production relies both on streamflow requirements in the spawning and nursery areas, and reasonable conveyance flows into and through the Delta for emigrants enroute to the ocean. Several Agreements, Licenses, or Decisions which provide the existing fishery streamflow requirements are listed below. All but the SWRCB Decisions are provided in Appendix 5.

| River | Documents Including Mitigative Measures for Salmon |
|---|---|
| San Joaquin below Friant | None |
| Merced | Davis-Grunsky Agreement<br>FERC[1/] License #2179 |
| Tuolumne | FERC License #2299 |
| Stanislaus | SWRCB D1422 plus a recent Civil Agreement between USBR and CDFG |
| Lower San Joaquin River | SWRCB D1485 |

1/ FERC = Federal Energy Regulatory Commission previously know as the Federal Power Commission

A brief synopsis of streamflow limitations on the tributaries is helpful at this point.

| | |
|---|---|
| Upper San Joaquin River | . no salmon streamflow requirements |
| Merced River<br>1. Davis-Grunsky Agreement<br>2. FERC License #2179 | . Pre-spawn flushing flow discretionary<br>. Spawning flow 180 to 220 cfs in the spawning area starts too late, November 1 to April 1. Gauge site not reflective of entire spawning area.<br>. 75 cfs provided by FERC #2179 until May 31. June 1 through October 15 is 25 cfs (see "Dry Year" scenario in Appendix 5).<br>. Provision for "Downstream Migration" Flushing Flow for juveniles need to be implemented. |
| Tuolumne River<br>1. FERC License #2299<br>2. Recent Study Amendment | . Refer to Article 37, FERC #2299 for Normal (Schedule A) and Dry (Schedule B) Schedules (Appendix 5). April and May flows result in high temperatures and poor smolt conveyance to the San Joaquin River and Delta. June through September flow is 3 cfs.<br>. Recent Amendment to FERC #2299 to add studies and revise Schedules A and B. Some flushing flow, spawning flow and egg incubation flows shifted to study spring needs for smolt outmigration.<br>. Additional commitment of 2,000 cfs spring flow (60,000 ac.ft. block) for smolt survival evaluation in 2 of 6 study years.<br>. Fluctuation criteria in Article 38 do not provide adequate protection for fry life stage. |

| | |
|---|---|
| Stanislaus River<br>1. D1422<br>2. Recent Civil Agreement<br>3. Pending SWRCB Decision | . Civil Agreement between DFG and USBR for between 302,100 ac.ft. and 98,300 ac.ft.<br>. 1250 cfs limitation with provisos<br>. March 1-15 flows determined by DFG using February forecasts. Preliminary annual schedule developed using March runoff forecast.<br>. DFG to provide USBR with final streamflow schedule for the period April through February by no later than April 15th each year. |
| Lower San Joaquin River<br>1. D1485 | . No salmon streamflow requirements. |

The net effect of the existing streamflow requirements are that:

A. Adequate flow fluctuation criteria are lacking on the Merced and Tuolumne River during the spawning and fry rearing periods.

B. Streamflows on the Merced and Tuolumne River are severely reduced in April, May and early June just when smolts are emigrating to the ocean. Spring flows on the Stanislaus are only interim and will be determined annually during the defined study period.

C. When Merced River flows reduce to 75 cfs on April 1 and Tuolumne River flows reduce to 100 or 200 cfs on May 1 (3 cfs in Schedule A and B of Article 37, FERC #2299) the juvenile salmon remaining suffer high mortality and production is lost.

D. Only the Stanislaus River has a requirement for streamflow releases to help meet total dissolved solid standards in the lower San Joaquin River at Vernalis.

E. <u>Streamflows released in the San Joaquin tributaries are not guaranteed to improve fish habitat conditions on the San Joaquin River or South Delta in the absence of specific streamflow requirements at Vernalis and other key points.</u>

<u>Flow Improvements Needed</u>

The 1986 revision of FERC License 2299 for the New Don Pedro Project for the Tuolumne River salmon study includes a "Smolt Survival" aspect and a requirement for 60,000 acre-feet of additional water during 2 of 6 study years (Appendix 5). The purpose is to evaluate the affect on smolt survival of short duration high amplitude flushing flow of 2,000 cfs during average runoff conditions on the San Joaquin. The evaluation includes the use of coded wire tagged (CWT) salmon smolts so that survival indices can be developed while they travel through the emigration route. Ultimately ocean returns (2, 3 and 4 years later) provide the final measure of smolt survival. A similar study aspect has been incorporated in the New Melones Fish Study Program on the Stanislaus River under the terms of the recent Agreement between USBR and CDFG (Appendix 5).

Similar studies of smolt survival in relation to flushing flows in the Columbia River System suggest that (1) smolts, if ready, can be stimulated to emigrate with relatively small increases in total discharge (i.e. 25% change), and (2) smolt movement rates were strongly correlated to both river velocity and a measure of turbidity (Scully et. al. 1983; Fish Passage Center, 1986). We believe this approach to water management may provide substantial improvement in habitat conditions for San Joaquin drainage salmon runs.

Based on the relationships between the Tuolumne and Stanislaus Rivers and Vernalis mean spring flows vs. escapement two years later (Figures 15 and 16), we also believe that low spring flows in the San Joaquin River combined with the effects of South Delta exports can negate the benefits of "flushing flows" or only moderately improved tributary flows.

Assuming no change in San Joaquin River and the South Delta conditions in April, May and early June, the following are estimated minimum amounts of water (Table 3), in excess of those provided in current License or Agreement terms, that are needed from the San Joaquin River tributaries to recover and maintain at least 70% of 1940, 1944 and 1945 salmon production.

Table 3. Estimated Spring Flows Required to Recover 70% of Historic (1940, 1944 and 1945) Salmon Production Assuming Current Water and Fisheries Management Program.

| | Period | Estimated Spring Flow-cfs | Existing Minimum Fishery Flows cfs |
|---|---|---|---|
| San Joaquin River below Friant | All year | 6,500 | 35 |
| Merced River | Apr-June<br>October | 2,000<br>220 | 75/25<br>25 |
| Tuolumne River | Apr-June | 6,500 | 550/3 |
| Stanislaus River | Apr-June | 2,000 | 900/1200 |
| San Joaquin River @ Vernalis | Apr-June | 16,000 | 0 |

## Literature Cited

Baggerman, B. 1960. Salinity Preference, Thyroid Activity and the Seaward Migration of Four Species of Pacific Salmon (Oncorhyncus). JFRBC 17: 295-322.

Brett, J.R. 1952. Temperature Tolerance in Young Pacific Salmon, genus Oncorhynchus. JFRBC 9(6): 262-323.

Brett, J.R., M. Hollands and D.F. Alderice. 1958. The Effect of Temperature on the Cruising Speed of Young Sockeye and Coho Salmon JFRBC 15(4): 587-605.

Carlin, B. 1968. Salmon Conservation, Tagging Experiments, and Migrations of Salmon in Sweden. Lecture Series of the Atlantic Salmon Association, Montreal.

Coutant, C.C. 1973. Effect of Thermal Shock on Vulnerability of Juvenile Salmonoids to Predation. JFRBC 30: 965-973.

Dickhoff, W.W., L.C. Foman and A. Gorbman. 1978. Changes in Plasma Thyroxine During Smoltification of Coho Salmon (Oncorhyncus kisutch). Gen. Comp. Endocrinol. 36: 229-232.

Fish Passage Center. U.S. Department of Energy. 1986. Smolt Monitoring Program. 1986 Annual Report. Project No. 86-90. 133 p.

Foerster, R.E. 1937. The Relation of Temperature to the Seaward Migration of Young Sockeye Salmon (Oncorhynchus nerka). J. Biol. Board Canada 3: 421-438.

Fry, D.H., Jr. 1961. King Salmon Spawning Stocks of the California Central Valley, 1940-1959. CDFG 47(1): 55-71. (unpublished Admin. Report).

Gilliom, R.J. 1986. Selected Water-Quality Data for the San Joaquin River and its Tributaries, California, June to September, 1985. U.S. Geological Survey Open-File Report 86-74. U.S. Bureau of Reclamation. 12 p.

Grau, G.F., J.L. Specker, R.S. Nishioka and H.A. Bern. 1982. Factors Determining the Occurrence of the Surge in Thyroid Activity in Salmon during Smoltification. Aquaculture 28 (1982): 49-57.

Hamilton. S.J. 1987. In "Selenium IV. Proceedings of the Fourth Selenium Symposium". Berkeley. 1987. (In Press).

Hamilton, S.J., A.N. Palmisano, G. Wedemeyer and W.T. Yasutake. 1987. Impacts of Selenium on Early Life Stages and Smoltification of Fall Chinook Salmon. Trans 51st N.A. Wildlife and Nat. Res. Conf. p. 343-356.

Hasler, A.D. and A.T. Scholz. 1983. Olfactory Imprinting and Homing in Salmon. Investigations into the Mechanism of the Imprinting Process. (Zoophysiology; v. 14). Springer-Verlag Berlin, Heidelberg. 1983. 134 p.

Hoar, W.S. 1939. The Thyroid Gland of the Atlantic Salmon. J. Morphol., 65: 257-295.

_____. 1976. Smolt Transformations: Evolution, Behavior and Physiology. JFRBC 33: 1233-1252

Hughes, R.M., G.E. Davis and C.E. Warren. 1978. Temperature Requirements of Salmonoids in Relation to Their Feeding, Bioenergetics, Growth and Behavior. U.S. Office and Water Research and Technology Research Grant B-041-ORE, Corvalis, Oregon.

Jensen, A. and R. Duncan. 1971. Homing in Transplanted Coho Salmon. Progressive Fish Culturist 33: 216-218.

Knutson, C. 1980. (DFG Memorandum dated 1/15/80) "CWT Chinook Salmon Salvaged at the State and Federal Delta Pumping Plants". Anadromous Fisheries Branch files.

Mighell, J. 1975. Some Observations of Imprinting of Juvenile Salmon in Fresh and Saltwater. 1975 Symposium on Salmon Homing. Natural Marine Fishing Service, Seattle, Washington.

Nagahama, Y., S. Adachi, F. Tashiro and E.G. Grau. 1982. Some Endocrine Factors Affecting the Development of Seawater Tolerance During the Pass-Smolt Transformation of the Amago Salmon, Oncorhynchus rhodurus. Aquaculture, 28: 81-90.

Orsi, J.J. 1971. Thermal Shock and Upper Lethal Temperature Tolerances of Young King Salmon, Oncorhynchus tshawtscha, from the Sacramento-San Joaquin River System. CDFG. Anadromous Fisheries Br. Admin. Rpt. No. 71-11. 16 p.

Palmissano, A.N. 1987. In "Selenium IV. Proceedings of the Fourth Selenium Symposium". Berkeley, 1987 (In Press)
Palmisano, 1986.

Reavis, R. 1983. Annual Report Chinook Salmon Spawning Stocks in California Central Valley. CDFG admin. Rpt. No. 83-2. 39 p.

Rich, A.A. 1987. Report on Studies Conducted by Sacramento County to Determine the Water Temperatures which Optimize Growth and Survival in Juvenile Chinook Salmon (Oncorhynchus tshawytscha) in the Lower American River. In Analysis of River Flows Necessary to Provide Water Temperature Requirements of Anadromous Fishery Resources of the Lower American River. Lower American River Court Reference, Exhibit No. 69-A.

Schreck C.B., A.G. Maule, C.S. Bradford, B.A. Barton, L. Sigismondi and P.J. Prete. 1985. Columbia River Salmonid Outmigration: McNary Dam Passage and Enhanced Smolt Quality. Oregon Coop. Fisheries Research Unit, Covalis Ore. USCOE Contract No. DACW68-84-C-006: 173 p.

Scully, R.J., E. Buettner and C. Cummins. 1983. Smolt Condition and Timing of Arrival at Lower Granite Reservoir. Idaho Department of Fish and Game, 1983. Annual Report. Bonneville Power Administration Project No. 83-323B. 87 p.

Shelbourne, J.E., J.R. Brett and S. Shirahata. 1973. Effect of Temperature and Feeding Regime on the Specific Growth Rate of Sockeye Salmon (Oncorhynchus nerka) with a Consideration of Size Effect. JFRBC 30(8): 1191-1194.

Sharahata, S. and M. Tanaka. 1969. Homing Behavior of Land-locked Sockeye Salmon in Lake Chuzenji. Annual Mtg. of the Japanese Society Fisheries Scientist.

Sokal, R.R. and F.J. Rohlf. 1969. Biometry. W.H. Freeman and Co. San Francisco. 776 p.

Taylor, S.N. 1974. King Salmon Spawning Stocks in California's Central Valley, 1972. CDFG Anadromous Fisheries Branch. Admin. Rpt. 74-6.

Tukey, J.W. 1977. Exploratory Data Analysis. Addison-Wesley Co.

Wedemeyer, G. 1973. Some Physiological Aspects of Sublethal Stress on the Juvenile Steelhead Trout (Salmon gandueri) and Coho Salmon (Oncorhynchus kisutch). JFRBC 30: 831-834.