SETTING GOALS FOR SALMON SMOLT SURVIVAL IN THE DELTA

and

DISCUSSIONS ON THE PROPOSED EPA SALINITY STANDARD

Prepared for:

California Urban Water Agencies
The Bay Institute of San Francisco
Environmental Defense Fund
Natural Heritage Institute
Save San Francisco Bay Association

Prepared By:

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August 10, 1994

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Preface

The attached reports were prepared to summarize a series of meetings to discuss issues raised in comments on the proposed EPA standards by technical representatives of the California Urban Water Agencies (CUWA) and others. All parties who participated have been given at least one, and in some cases four or more, opportunities to review these reports. Their comments have enabled me to improve and refine the accuracy of the reports, and I am grateful for all the helpful feedback I have received. In addition, I have had numerous lengthy discussions of the issues addressed in these reports and the accuracy to which the degree of consensus achieved has been characterized. I have tried to be even-handed in revising these reports in response to sometimes conflicting comments. Nevertheless, since the subject matter is controversial and topical, some will no doubt disagree with some statements herein. Although I have drawn on the contributions of other participants, the description of the meetings contained herein is my own, and I take full responsibility for any omissions or errors in characterizing the content of the meetings.

SETTING GOALS FOR SALMON SMOLT SURVIVAL IN THE DELTA

Wim Kimmerer

August 10, 1994

Summary Three meetings were held to resolve technical issues raised by California Urban Water Agencies (CUWA) on the Environmental Protection Agency's (EPA) proposed salmon smolt standard[1]. Consensus[2] was achieved on a number of issues. It was agreed that measures for protection of salmon in the delta should be implemented in a timely manner. A goal should be established relating the US Fish and Wildlife Service (USFWS) smolt survival index (SSI) for fall-run chinook salmon to any largely uncontrollable variable, e.g. temperature on the Sacramento River and unimpaired flow on the San Joaquin River. Implementation measures would be devised to achieve the goal, and compliance would be based on whether these measures were actually put into effect. The measures devised for fall-run smolts would be extended over a broad enough period to protect other races. The SSI data would be revisited periodically to assess achievement of the goal, assumptions, and implementation measures, and to improve understanding.

Introduction Meetings were held on 9, 17, and 29 June 1994. The purpose of these meetings was to examine the technical issues raised in CUWA's comments to EPA on the proposed EPA salmon smolt passage standard and to reach consensus on alternative approaches. Specific objectives of the meetings were to answer the following questions:

1. What should a standard consist of?
2. What is the goal of the standard-setting process?
3. How can future levels of smolt survival be calculated for assessment of the implementation program?
4. What implementation measures might be useful?

This report is a summary of the outcome of the series of meetings, rather than a set of minutes of each. The emphasis is on the agreements reached rather than the process or the discussions that took place. Nevertheless, some discussion of the process is included below to reveal how the endpoint was reached. In addition, several key

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[1] There was some confusion and a few semantic arguments over the terms "goal" and "standard". To sidestep these arguments we use the term "goal" to mean the target level of SSI, and "standard" to mean the actions taken or regulations imposed to achieve that goal.

[2] "Consensus" as used here does not refer to unanimity, but to its most usual meaning: a general agreement among members of the group.

technical points are discussed in some detail since their resolution is germane to the consensus that was reached. Notes in square brackets [] throughout the text were added by the author to present additional information or to clarify issues, and may not represent the consensus of the group.

The report is organized with the recommendations, areas of agreement, and unresolved issues presented first for emphasis. That brief discussion is followed by the detailed discussion of technical points.

It was stressed throughout these meetings that the discussion should focus on technical issues only.

Recommendations A goal and a set of implementation measures should be developed based on the fall-run SSI. Pending a revised analysis of the existing data, this could be done according to functions shown in Figure 1. These functions must be filled out by selecting values for the parameters, specifically the amount of improvement over historical conditions. The parameters to be selected are:

- The slope and intercept of Sacramento SSI with respect to temperature
- The minimum SSI in the Sacramento regardless of temperature
- The relationship of San Joaquin SSI to the 60-20-20 index of unimpaired flow

Although participants were willing individually to select values of these parameters, they acknowledged that the basis for any choice was fairly arbitrary, since the choice of parameters entails a choice of a particular "best" value of the SSI goal for a given set of conditions (see discussions below).

Recommendations were also prepared for a program to assess the extent to which goals had been met.

Fundamental agreements Participants unanimously agreed on the underlying purpose of setting standards: salmon need protection. Consensus was also reached on the following statements, some of which are discussed further below:

- Measures are needed to protect and enhance naturally-spawning stocks of salmon
- Smolt survival on passing through the delta is a problem for salmon stocks that is worth considerable effort to solve
- The USFWS SSI may be biased by differences in size of hatchery smolts used in different releases; although other potential sources of bias and error were identified, none was supported by analysis of data to date
- The USFWS SSI is not numerically equal to survival
- The USFWS salmon smolt survival *models* should not be used to set goals
- The USFWS models include many of the environmental variables likely to

influence smolt survival, including temperature, proportion diverted at junctions of certain delta channels, flows, and exports.

- Goals for smolt survival should be based on a selected value or range of values of the SSI for fall-run salmon
- Other races besides fall-run, and other life stages besides smolts, are assumed to be protected by extending the same set of implementation measures to the appropriate times. Data are not now available to evaluate alternative measures or to set numerical goals for these races or life stages
- Goals should vary to account for effects on smolt survival of environmental variables not readily controllable by project operations. This would include the size of smolts used in experiments, temperature on the Sacramento side of the delta, and the 60-20-20 index of unimpaired flow on the San Joaquin side
- Compliance should be based on the degree to which mandated implementation measures were actually carried out
- Several implementation measures were listed. The general consensus was that there would be substantial convergence on the recommended measures among different groups
- Effectiveness of implementation measures and underlying assumptions including those inherent in the SSI should be reevaluated at least every 3 years
- A more detailed research and monitoring program should be developed and implemented which focuses on determining whether goals have been met, and on refining understanding of specific sources and causes of salmon smolt mortality in the delta

Unresolved issues included:

- The statistical reliability of relationships for which CUWA scientists have not examined data; these are taken at face value pending further examination (examples include the ocean survival index and the survival index for wild smolts)
- The utility and statistical reliability of alternative empirical models
- The size of the increase in SSI for each river (i.e. the numerical value for the goal), which cannot be determined on strictly scientific grounds
- Method for calculating baseline values of SSI
- Method of filling in gaps between SSI measurements to assess effectiveness of program
- To what extent survival indices could be improved by different methods, such as more intensive trawling
- Importance and cause of the relationship between smolt size and SSI

Details of technical discussions

The USFWS Smolt Survival Index This index is intended to represent the survival of salmon smolts passing through the delta to Chipps Island. Considerable discussion was held about the index and the possibility that there were flaws in it.

The SSI is calculated as the number of marked smolts recaptured in trawl surveys at Chipps Island, expanded to estimate the number passing Chipps Island, divided by the number released. If the expansion factor were exactly correct, the SSI would be an estimate of survival. Since some parts of the expansion factor are uncertain, this factor becomes merely a correction for trawling effort, and the index is assumed to be proportional to, but not equal to, survival. The proportionality could change with smolt size or other variables, and therefore vary between release groups (see discussion of potential biases below)

USFWS constructed models of the SSI for several reaches in the delta. To represent survival over a pathway consisting of more than one reach, USFWS needed to convert the SSI for each reach to an *estimate* of survival so that survival over the entire migration pathway could be calculated as the product of survival probabilities over each reach. To do this they divided each index by 1.8, which at one time was the largest index, to ensure that the survival estimates did not exceed 1. This practice did not alter the *relative* values of the indices, but it has led to some confusion and disagreement over the nature of the resulting survival estimates. Furthermore, the conversion of indices to probabilities for the purpose of linking reaches is considered statistically unacceptable.

Since it was agreed not to use the USFWS model to set goals, and since the survival index scaled by any constant has the same relationship to environmental values as the raw data, this issue became moot. However, users of the SSI must guard against assuming that this value is actually a survival estimate.

The remaining issue regarding the SSI was whether it was an unbiased index, that is, directly proportional to actual survival of the hatchery fish, and whether it applied equally to survival of naturally-spawned fish. Potential sources of bias identified were:

- Greater duration of migration when longer pathways are taken, resulting in spreading out of the pulse of smolts and consequently reduced probability of detection of smolts passing Chipps Island
- Thermal shock for hatchery-reared fish released in high-temperature delta water that would increase mortality relative to wild fish
- Size of smolts could introduce bias in results of individual releases

Several potential sources of *error* in the SSI were noted, but for none of these did the group conclude that there was *bias*:

- Irregular distributions of smolts in time or distance across the cross-section of the channel at Chipps Island
- Low numbers of smolts recaptured in the trawl, resulting in high variance of recaptures

The duration of migration seemed to be the most likely source of bias. However, USFWS has presented a comparison of SSI with the ocean survival index (OSI), which is determined independently of the trawling effort at Chipps Island. The result was an apparently linear relationship with a correlation coefficient of 0.89 (N=21; WRINT USFWS-9 Figure 7), indicating that the two indices were estimating the same thing and effectively ruling out a *substantial* bias in the trawl recovery data if this analysis is correct. CUWA has not examined this analysis or the underlying data. [Note: since the meetings CUWA biologists have raised questions about the data used in the analysis of ocean survival index. However, these issues were not raised at the meetings and are not discussed further here.]

Smolt survival in the Sacramento side of the delta is negatively correlated with temperature. This correlation could be an artifact resulting from thermal shock or difficulties with acclimation or vulnerability to predation when naive hatchery smolts are dumped from a truck at low temperature into warm delta water. Survival of wild smolts in 1988 and 1989 was negatively correlated with temperature, such that survival was low at temperatures above about 65°F (WRINT USFWS-7 Figures 7 and 8). Since this is qualitatively similar to the results obtained for hatchery smolts, the likelihood of bias seems to be low. Again, this relationship has not been examined in depth by CUWA.

The size of smolts clearly introduces some bias into the results. Survival is negatively correlated with size at release on the Sacramento River. Since there is no apparent relationship between size of smolts and temperature or flow, the correlation of size with survival could be due to increasing net avoidance with increasing smolt size. This could be dealt with by either correcting for size, or by using only releases in a selected size range. [Note: The source of this relationship is unclear. Pat Brandes has stated that the correlation between ocean and trawl indices rules out capture efficiency in the trawls as the cause of the relationship with size. Since the relationship has the opposite slope from what one would expect (i.e. lower survival for larger fish), it could be an artifact of using larger fish later in the season when temperature is higher. She argues that, since it is unexplained, it does not represent bias. However, if there is a real effect of size, and if size is not randomly distributed within release groups, then it could be a source of bias. This issue needs further examination.]

Having acknowledged that there were potential sources of bias in the SSI, Pat Brandes emphasized that the SSI is an *index of survival* that appears to represent patterns of survival for salmon smolts. She presented the correlations between SSI and OSI to demonstrate this. Based on this discussion, the general consensus of the group was that the SSI likely does represent patterns of survival, and therefore could be used as a starting point on which to base a standard. [Note: SSI values have frequently been referred to as if they were survival values in the three meetings, in USFWS reports, and implicitly in the multiplication of adjusted SSI values to estimate a survival index for consecutive reaches.]

The USFWS models These models attempt to explain the variance in SSI on the basis of environmental variables. Models were constructed for several release points, and then the overall SSI models for the Sacramento and San Joaquin rivers were constructed by combining models for different release points representing different reaches of the delta.

Generally the group did not believe that the models are an adequate statistical description of the covariability of the SSI with environmental conditions. Most accepted the statements of John Rice, who stated that the models were too complex and contained too many parameters, and inappropriately converted SSI values to probabilities to calculate survival through successive reaches.

In spite of the general dissatisfaction with the models, the general findings of the USFWS effort seemed to be accepted. For example, participants believed that, in the Sacramento River, increased temperature resulted in lower survival (although the mechanism is not well known), survival in the interior delta is lower than that in the mainstem, and diversion through the cross-channel and Georgiana Slough resulted in lower survival. These are not only outcomes of the model, they can be readily interpreted from the results of paired releases (e.g. above and below the cross-channel) or linear regression analyses.

Most participants were willing to accept that San Joaquin River smolt survival was reduced as exports or diversion of smolts into Old River increased, or as flow through the delta in the San Joaquin River decreased. Data to support this conclusion are limited because only 4 values are from high-flow, high-survival periods, although analyses of adult production estimates apparently give similar results. This acceptance was based as much on biological understanding as on data analysis.

Many participants accepted that the models could be used for guidance *in combination with other information*. Several objected to any use of the models, preferring instead to rely on examination of data. There was general acceptance that expert opinion on the factors affecting salmon survival should be used in setting standards.
To summarize, while the specific numeric output of the models was not believed by

participants, they were ready to agree with some of the qualitative outputs of the model, especially since results of paired releases supported those outputs. These conclusions include:

- The correlation of survival with temperature (particularly for releases at Courtland)
- The reduction in survival of fish that go through the central delta relative to the mainstem Sacramento
- The reduction in survival in the San Joaquin due to the diversion of smolts off the mainstem and the direct influence of export pumping

Use of the SSI as a goal for a standard The goal would be an improvement in survival of salmon smolts passing through the delta. This was recast as an improvement in the SSI, under the assumption that the SSI is the best index of survival now available.

The baseline for improvement was never explicitly stated, although throughout the discussions there was an implicit assumption that the baseline would be determined from all of the applicable SSI data to date. It was generally agreed that the amount of improvement to be achieved could not be fully addressed during the meeting. The reasons for this were discussed briefly at the meetings: 1) Since the actual survival is not known, the necessary improvement cannot be determined; 2) The importance of mortality in the delta can only be assessed in the context of the entire life cycle; and 3) Goals for population size, at least above levels where extinction is a possibility, can only be set by consideration of societal needs.

An approach to basing a standard on the SSI was discussed. According to this approach, the standard would actually be a set of implementation measures designed to provide a specified SSI goal based on prevailing water year conditions, temperature, or other uncontrollable factors. Implementation measures would be devised to achieve that goal. Compliance would then be assessed by comparing the implementation measures actually carried out with those specified. Thus, compliance would not be gauged by whether or not a particular SSI value was achieved. The SSI values would serve as goals, which would be revisited at a minimum of three year intervals to determine the effectiveness of the measures; implementation measures would subsequently be revised or augmented if the SSI were chronically short of the goals, *on average*.

Scaling to uncontrollable variables (Sacramento River) Some variables that are correlated with SSI are not readily controllable. A survival goal should take these variables into account to avoid holding the major water projects responsible for variation over which they have little or no control.

For SSI measurements based on releases on the Sacramento side of the delta, smolt size and temperature are the most important factors explaining variation in SSI. Temperature in the delta can be controlled only to a limited extent, since it is most responsive to meteorological conditions. Therefore the group agreed that some allowance in the goal needed to be made for temperature. For example, it would be unrealistic to expect flow manipulations to achieve a high SSI at a temperature of 75°F, since SSI has always been close to 0 at that temperature.

The goal should be a set increase over the existing relationship between survival index and temperature. The existing relationship for releases at Sacramento or Courtland with cross-channel gates open is (depicted in Figure 1, top, as historical mean):

$$S = \mathrm{MAX}\ \{\ a\ (\ T_x - T\ ),\ 0\ \}, \tag{1}$$

where T is temperature at Freeport (°F), T_x the temperature at which survival goes to zero (approximately 76°F), and a is the slope (approximately 0.05-0.08). Note that this relationship has not been confirmed through analysis of all of the available data, and is presented only as an example of the form the equation might take.

Bruce Herbold suggested that the increase in the goal over the historical value could be either a doubling of survival for a given temperature, or alternatively, an increase in survival corresponding to closing the delta cross-channel gates. Coincidentally, the slopes corresponding to these alternatives come out about the same.

The group recognized that temperature in the delta is controllable to a limited extent, and the above standard could allow some activities that increase temperature, reducing the survival goal. Therefore the group suggested a minimum survival at all temperatures. In addition, EPA, the State Board, and other relevant agencies should re-examine the issue of temperature controllability in the delta, and revise this standard if temperature in the delta increases over the long term through local human actions (i.e. as opposed to global warming).

Thus the goal for the Sacramento SSI would have a functional form similar to:

$$S_1 = \mathrm{MAX}\ \{\ a_1\ (\ T_x - T\ ),\ S_{min}\ \}, \tag{2}$$

where a_1 is the new slope and S_{min} is the minimum survival (Figure 1 top, "goal"). This equation appeared to be the most acceptable of several alternative equations that were discussed.

T_x would be determined from the data, as would the baseline slope a in equation 1. Opinions varied about actual values of the remaining parameters. Bruce Herbold suggested a slope of 0.16, equivalent to a doubling of a, or an increase corresponding to shutting cross-channel gates. The value of S_{min} was somewhat arbitrarily set at 0.25, although opinions on an appropriate value ranged from 0.1 to 0.5.

The group did not recommend setting a separate standard for temperature, because it cannot be controlled to any great extent.

Scaling to uncontrollable variables (San Joaquin River) The response of SSI to flow in the San Joaquin River reflects changes in water year type as indexed by the 60-20-20 index of unimpaired flow. Since that is uncontrollable, it should be accounted for in setting standards.

Susan Hatfield presented an analysis of estimated SSI values representing survival through the delta on the San Joaquin side. Relationships of SSI vs San Joaquin flow at Vernalis showed essentially two groupings of data: one for low-flow conditions during mostly critical years, and the other for higher-flow conditions during wet years. (Because of the way the San Joaquin system is regulated, and because of the recent drought, the data do not include a range of flow conditions). There was some discussion about whether to discard a data point for 1985 in which a different marking method was used, and in which survival was high while flow was low. However, even with that point included, the relationship is highly significant ($p<0.001$, $r^2=0.47$ vs. 0.69 with 1985 deleted).

Susan Hatfield then suggested using either upper quartile or average survival indices doubled for each year type.

Participants preferred a goal that would improve survival in critical years more than in wet years. The historical mean value of SSI is about 0.09 in a limited number of critical years, ($\leq$1.5 MAF) and 0.5 in wet years ($\geq$5 MAF). There was general consensus that the goal for critical years should be a 2- to 3-fold increase over historical values, since populations are more vulnerable during low-flow conditions. The goal in wet years could be set at a value higher than the historical mean, say 0.75. If the goal for the survival index were scaled linearly to unimpaired flow, it would have the following form (Figure 1 bottom):

$$S = 0.05 + 0.14\,Q6, \tag{3}$$

where Q6 is the 60-20-20 index in millions of acre feet.

Implementation measures Although the group discussed implementation measures at various times, the consensus was that other entities (e.g. recovery teams, CVPIA teams) would probably address these in greater detail than would be possible as part of these meetings. Measures identified and discussed briefly by the group included:

- Close delta cross-channel gates from November 1 to June 30 each year, with periodic opening to flush channels

- Limit CVP/SWP exports to about 1500 cfs (daily average) during April-May

- Develop a coordinated CVP/SWP operations plan for other periods to reduce the influence of exports on outmigrating salmon

- Establish minimum flows on the San Joaqin River from 4,000 to 12,000 cfs depending on water year type for April 15 to May 15 or longer

- Install physical barriers at the head of Old River to the extent compatible with management for delta smelt

- Provide minimum net delta outflow of 7,000 cfs, with a minimum flow of 4,000 cfs on the Sacramento, during February-June.

- Develop ramping criteria to prevent stranding in tributaries

- Based on real-time monitoring, limit project and in-delta diversions for an appropriate period following the first storm of each season that produces a smolt outmigration

- Pulse flows had a lower priority than minimum flows

- Some measure of flow balance in the delta is needed. USFWS has used QWEST for this purpose, but most participants believed that QWEST is not real, and should be replaced by some alternative measure.

Compliance monitoring Determining whether goals were being achieved would require considerable effort, presumably by IEP/USFWS, in addition to their research into the factors affecting smolt survival. A practical limit on increasing the total effort is imposed by availability of smolts for release due to facilities constraints. These limit the number of releases that could be devoted to this effort: at present about 8-12 total releases can be made each year. CUWA does not believe that this allows for an adequate number of releases.

<u>How often</u>: Ideally, weekly monitoring when sufficient smolts become available; for the moment, at least 3 releases on each river system or 1/2 - 2/3 of the available release groups. However, this number may not be sufficient to reduce the standard error of the SSI values to the point where achievement of the goal can be reliably assessed. Therefore, the limits on number of releases for this purpose need to be resolved as soon as possible. Expanded capacity for tagging both hatchery and wild fish is also needed.

<u>Where</u>: Locations should include at least Sacramento and Mossdale, but releases at

Port Chicago are important for determining ocean survival

When: Spread out over April to June. The sampling design would need to be devised, but sampling should not alias spring-neap tidal cycles or any other known natural or operational cycles.

Size of fish: Should be standardized to the extent possible.

Determining whether the goal is being met A significant problem in comparing the SSI to the goal is that there will always be gaps in the data, and that conditions could be quite different during these gaps than when survival is measured. The group discussed two alternative approaches but did not achieve consensus on this issue, and considerable analysis would be needed to resolve it.

There are two ways to fill in the gaps. One way is simply to take the results of each release as point estimates, and assume that the sampling scheme assures that these samples are representative with respect to all factors that cause survival index to vary (except smolt size, temperature (Sacramento) and possibly unimpaired flow (San Joaquin), which must be considered explicitly for each release). In this case the goal would be compared with the mean value for several years, using a t-test or other appropriate statistical test to determine whether the goal fell within the confidence limits of the data.

An alternative approach is to use a statistical model of smolt survival index as a function of temperature, flow, exports, smolt size, and anything else that is statistically relevant, and calculate the index for each day on which it was not measured. This would reduce the error variance in the estimates of SSI. However, the mechanism for using a model to fill the gaps was not specified.

[Note: the main concern with using the first method is the difficulty that may be encountered in making the small number of samples representative. One approach to this problem is during the periodic review of the program to compare flow and temperature conditions in the delta during each migration period with the conditions during the releases. If they are reasonably close, then the samples can be considered representative. Bruce Herbold also suggested, after the meetings, that the Ocean Survival Index could be used in combination with timed releases of smaller numbers of fish to integrate over the entire season. An additional point not resolved at the meetings is that the baseline must be the existing SSI data, which were not developed for the purpose of obtaining an annual mean value.]

Application to other races/ages Participants believed that there was insufficient basis for establishing separate standards for other races than fall-run, or for fry. Instead, there was a consensus that measures implemented for fall chinook would probably be sufficient if extended to seasons relevant to other races. In addition, establishing conditions that produce a high SSI should also enhance fry survival for some races and some times.

Research recommendations There was general agreement that improvements could and should be made in the statistical analysis of the data and in the use of these analyses in setting goals and standards. Beyond that, participants were enthusiastic about enhancing the research program to improve knowledge and therefore ability to improve conditions and assess changes.

There was disagreement over the importance of variation in smolt survival in the delta relative to other (mainly upstream) issues, and how to allocate research efforts in these various areas. It was noted, however, that the implementation measures being proposed constrain water uses in the system and that a valid concern exists regarding whether such measures are actually benefitting the resource. A specifically designed program of research and monitoring would address this concern and eventually should lead to the development of refinements to the measures which would better improve overall smolt survival. [Note: the effectiveness of these measures must be assessed in the context of the life cycle of the salmon and the factors limiting their production. Density-dependent mortality in some river reaches could eliminate some benefits of improved delta survival to spawning success; however, these benefits would continue to be felt in improved ocean harvest and in the entire life cycle of winter- spring-, and some fall-run stocks, whose spawning escapements are well below capacity of the rivers.]

Specific recommendations included:

- Continue efforts to refine the SSI, including analyses of assumptions and potential sources of bias and error, and additional covariates such as turbidity, water quality, the temperature difference between the truck carrying the smolts and the receiving water, and the quality of the hatchery source stock.

- Evaluate alternative methods (e.g. larger trawls, increased sampling effort, larger releases) to increase the recaptures and therefore the reliability of the results.

- Test feasibility of using radio or sonic tagging to determine migration pathways and rates and, if possible, locations and causes of excess mortality in the delta

- Make data available in a standard electronic format, and continue efforts to improve statistical reliability of empirical models

- Continue efforts to understand in a more mechanistic (rather than statistical) way how environmental conditions affect smolt survival.

- Continue efforts to develop statistically acceptable models to predict SSI from environmental conditions.
- Tag all hatchery fish rather than a subset [Note: this would be valuable only in analyses of upstream conditions, not in the delta.]
- Improve understanding of effects of toxicity of river and agricultural drain water to salmon smolts and fry

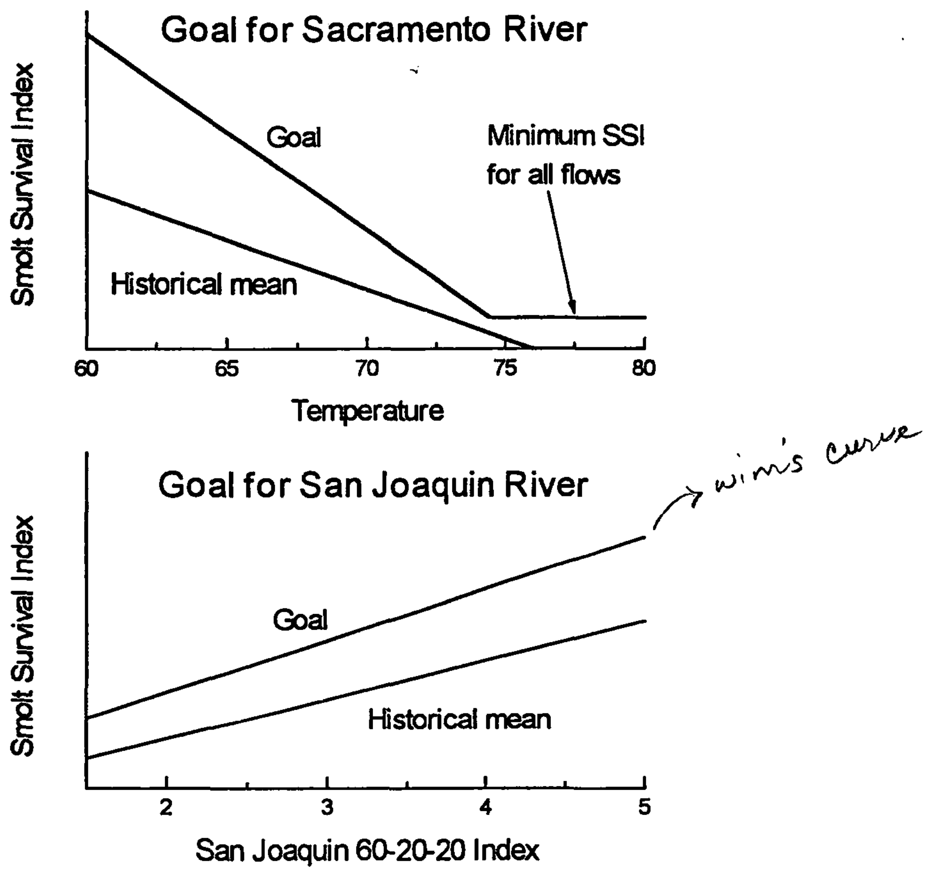

Figure 1. Schematic of possible SSI goals. Top: Goal for Sacramento River is related to temperature, with a minimum SSI for all temperatures. Bottom: Goal for the San Joaquin River is related to the 60-20-20 index of unimpaired flow

APPENDIX. LIST OF PARTICIPANTS IN ONE OR MORE MEETINGS

Randy Bailey	MWD consultant
Gary Bobker	Bay Institute
Pat Brandes	US Fish and Wildlife Service
Heidi Bratovich	State Water Res. Control Board
Jim Buell	MWD consultant
Phyllis Fox	CUWA consultant
Susan Hatfield	USEPA
Bruce Herbold	USEPA
Lyle Hoag	CUWA
Wim Kimmerer	Reporter
Cynthia Koehler	Natural Heritage Institute
Deborah McKee	Fish and Game
Jud Monroe	MWD consultant
Dudley Reiser	CUWA consultant
John Rice	CUWA consultant
Spreck Rosekrans	Environmental Defense Jund
Dan Steiner	SFPUC Consultant
Gary Stern	National Marine Fisheries Service
Lena Tam	East Bay MUD
Patrick Wright	USEPA

DISCUSSIONS ON THE PROPOSED EPA SALINITY STANDARD

Wim Kimmerer

August 10, 1994

Summary This report summarizes a meeting of staff and consultants from California Urban Water Agencies (CUWA), several agencies, and nonprofit environmental and fishery organizations to discuss issues raised in CUWA's reports on the Environmental Protection Agency's (EPA) proposed salinity standard. Major areas of agreement were found, and only a few disagreements, although some were significant. The most significant area of disagreement is the need for and effect of the proposed standard at Roe Island.

Introduction This report describes the results of a meeting held on 31 May 1994, sponsored jointly by CUWA and four environmental organizations, in response to requests by state and federal regulators that stakeholders explore consensus on Bay/Delta water quality standards. The purpose of the meeting was to discuss some of the technical issues raised in CUWA's comments to EPA on the proposed EPA salinity standard. The objective of these discussions was to determine the areas of agreement and disagreement over these issues among the participants (list attached), who included CUWA consultants, federal and state agency staff, and independent scientists. No attempt to resolve disagreements was made.

This report is presented as a summary of the issues raised and areas of agreement and disagreement identified during the meeting. Notes in square brackets [] throughout the text are the comments of the author, intended to present additional information or to clarify issues, but may not represent the consensus of the group.

Areas of disagreement were reduced to a small number, and many areas of fundamental agreement were found that would not have been apparent in a comparative reading of the SFEP workshop report, the EPA promulgation, and the CUWA responses. Participants are to be congratulated for presenting their analyses and making their arguments objectively, and for being willing to listen to each other.

Several areas of agreement formed a premise for these discussions. CUWA representatives have stated explicitly that they agree that:

- There is a problem in the estuary that needs to be addressed
- The salinity standard is a useful way to do this in principle
- A Chipps Island standard for salinity is recommended
- A salinity standard alone is insufficient to restore the estuary

Background on the salinity standards The standards examined in this meeting were those specifying the number of days when X_2 is to be downstream of several control points or, alternatively, when salinity is to be below 2 ppt at those points. This is based partly on the findings of the SFEP workshop, summarized by Schubel (1992; SFEP workshop report) and refined by Jassby et al. (in press, Environmental Management), showing positive relationships between several measures of "health" of the estuary (e.g. abundance or survival indices for estuarine fish or invertebrates, calculated organic carbon input) and X_2. For simplicity these are referred to these below as the "fish-X_2 relationships."

Several participants offered clarification of important points regarding the salinity standards.

1. The standards are based on X_2, defined in the SFEP workshop as the distance from the Golden Gate Bridge to the point at which the daily average salinity is 2 parts per thousand (ppt) near the bottom. X_2 for the period 1968-91 was estimated by interpolation between salinity monitoring stations. For about 10% of the days during 1968-91 (usually when flows were high), or for times earlier and later than that, X_2 was estimated from an autoregressive equation with log of net delta outflow (adjusted for revised estimates of delta consumption from DWR) as an independent variable. Thus, the perception of some that the X_2 values used in the fish-X_2 relationships are derived from outflow is wrong.

2. The participants in the SFEP workshop turned away from discussion of the entrapment zone (EZ) and related phenomena, and chose a simple salinity value as an index to be used in a standard. The reason was not that the EZ and associated processes are unimportant, but that the EZ is difficult to define and locate. Furthermore, participants believed that there were variables that might covary with position of the EZ, but that were not directly related to entrapment phenomena (e.g. abundance of starry flounder). Thus, they believed that use of EZ location might also be misleading. X_2 was recognized as a covariate of a wide range of variables, any of which could cause the observed biological responses. It was not the intent of the SFEP workshop to describe the causative links, nor was it to imply that the actual salinity (2 ppt) was of particular importance to all or even most of the species of concern.

It was suggested and accepted in the 31 May 1994 meeting that discussions of the importance of entrapment phenomena would not be fruitful, and that participants would focus on the salinity standards as stated (and amended by the use of sliding scales, see below).

3. The work done to lay the foundation for the SFEP workshops was done quickly with little opportunity for revision or re-analysis. Several improvements in the methods used for this have been suggested by a number of parties. This suggests that the entire analysis ought to be redone to refine it as a firm basis for the standards. [*Note: I do not believe that a reanalysis of the data will result in qualitatively different conclusions.*]

4. There is an important difference between the standard proposed by EPA and the index recommended by the SFEP workshop. The original proposal was to use the value of X_2 averaged over some period of months as an index on which to set a standard, since that is the independent variable used in the analyses. In addition, SFEP workshop participants strongly recommended that historical variability be somehow preserved in the standard, but

did not offer a means to do this. The EPA chose to use the number of days with salinity below 2 ppt at the three control points. This approach has the advantage of simplicity for monitoring, and also allows the variability in X_2 to be specified.

Most of the scientists at both the SFEP workshops and the May 31 1994 meeting expressed the belief that within-year and between-year variability should be maintained. [*Note: The proposed EPA standards at Roe and Chipps Islands could provide both as follows. Between-year variability would be set by the use of a sliding scale relating the standard for a year to the unimpaired flow for that year. Within-year variability would be established by appropriately specifying the number of days below 2 ppt for each of the three control points. For example, based on data from 1967-91, a mean X_2 at Chipps Island (74 km) for a given 5-month period would be associated with 56% of days with X_2 below Chipps, but also about 17% of days below Roe Island (64 km) and 18% of days above the confluence (81 km; see Kimmerer 1994, sliding scale report to CUWA). Note that setting a standard at Roe Island does not imply that mean X_2 is at that location unless the standard for a given period is for about 50% of the days at Roe, which would occur only under conditions of high unimpaired flow. Similarly, a mean X_2 at the confluence would imply 27% of days below Chipps and 5% below Roe, under historical levels of variability. A mean X_2 at Roe would mean 11% of days above Chipps and 3% of days above the confluence.*]

5. CUWA presented several reasons for their support of the Chipps Island standard but not the Roe Island standard. Briefly, these arguments are:

- Increasing uncertainty in fish-X_2 relationships as X_2 moves downstream
- Potential biases in fall midwater trawl data (see discussion under Issue 6 below)
- Other factors affecting fish abundances
- Loss of habitat for some species, or flushing of nutrients from the estuary, when X_2 is downstream

6. Participants agreed to try to stick to technical issues and avoid unnecessary discussion of economics, water supply, or management. This included discussion of feedback loops from the standards through operations to other biological response variables (e.g. effects of changing carryover storage, resulting from salinity standards, on winter run salmon survival in the upper Sacramento River). This is an area containing important technical issues but was not addressed in this forum because quantitative information on these feedbacks was not available to participants at the time of the meeting. It was also acknowledged that the ultimate selection of standards would include management judgements.

7. X_2 is a useful approximation of position of the EZ. [*Note: The peak abundance of two species of common zooplankton of the entrapment zone, and the peak of turbidity, are close to X_2, and the abundance peak of a third species is slightly upstream (Kimmerer unpublished). Striped bass larvae apparently concentrate at or slightly upstream of X_2 (DFG data). The manifestation of the entrapment zone in terms of particles and at least some organisms is*

therefore close to X_2, not substantially downstream of it.]

8. Most of the concern over changes in the estuary are over estuarine-dependent species, those that must reside in the estuary for all or part of their life cycle, and many of which occur at the low-salinity end of the estuary. These are the species that vary with X_2, and are the subject of the SFEP analyses. There is little concern over effects of freshwater flow on marine species that have extensive habitat outside the bay.

Issues addressed in the meeting The following discussion takes each of the major issues in turn, and summarizes the points on which agreement was achieved or on which differences remained.

Most of the discussion was centered on the fish-X_2 relationships; however, time constraints permitted CUWA to examine only the mid-water trawl data for striped bass, longfin smelt, and delta smelt. They have not performed an exhaustive analysis for the other species. The relationships for these three response variables were therefore the main area of emphasis.

Agreement is indicated where either it was explicitly demonstrated in the meeting, or where there appeared to be no major objections to statements made by one or more participants.

1. What is the qualitative nature of the relationships between X_2 and indices of abundance and survival indices for estuarine species?

Agreement:

a. Relationships between X_2 and indices of abundance or survival are real (although not always strong), and need to be considered in management

b. The fish-X_2 relationships appear to be continuous and monotonic indicating increasing responses as X_2 decreases, except that for Delta Smelt (see Disagreements), and except for some lower abundance indices in 1983 when flows were exceptionally high (low values in 1983 are not included in this discussion except under Issue 3 below). Although several participants (in this meeting and the SFEP workshops) had expected a step or discontinuous function for some of the response variables, these could not be demonstrated statistically.

c. The fish-X_2 relationships describe historical conditions; habitat or other changes in the estuary could cause these relationships to change in the future

d. The fish-X_2 relationships do not imply any causal mechanism; such mechanisms may be different for each species examined

e. Each species examined could be responding to any of the numerous covariates of X_2

f. Delta smelt seem to be very abundant only when X_2 is in Suisun Bay, but X_2 alone is a poor predictor of abundance of delta smelt

Disagreement:

a. What is the strength and significance of the relationship between delta smelt and X_2? Bruce Herbold's linear regression explained 25% of the variance in the delta smelt index, similar to that obtained by John Rice using generalized linear models and weighting the values by a variance function proportional to the mean squared. Phyllis Fox obtained a non-significant relationship using ordinary least squares and an estimate of within-year variance as a weighting factor. [*Note: experts on delta smelt in DFG and elsewhere believe that habitat of delta smelt consists of low-salinity, shallow water. If so, abundance of delta smelt should be higher when X_2 is in Suisun Bay than when it is either in the delta or in Carquinez Strait.*]

2. How should functions be fit to the abundance-X_2 data? The approach used by Jassby et al. (in press, *Environmental Management*) was to apply a generalized linear model with a variance function either proportional to the mean or constant. The choice of variance function was based on exploratory analysis of the annual abundance indices for each species. CUWA consultants used the same techniques and weighted least squares regression but applied variance functions either proportional to the mean squared, or calculated from the standard deviation of the 4 individual months of data, or by error propagation from the standard deviation within each sampling area and month.

Agreement:

a. The overall approach used by both parties is valid, and results do not differ very much qualitatively

b. The two alternative methods used by CUWA do not appear to give very different results from each other

c. All methods indicate that fish-X_2 relationships are statistically significant results, except for delta smelt; the main differences are in the amount of variance explained and the slopes of the lines

d. Variance of the abundance indices increases as the mean increases

e. A log-linear model (i.e. using log-transformed abundance indices) gives a similar result to the generalized linear models

f. The most appropriate variance function could be worked out by examining residuals for each species.

Uncertainty:

a. Is it appropriate to use the months as replicates in analysis of the midwater trawl data, as CUWA has done? This issue was not discussed very much, probably because the alternative error-propagation method gave a similar result.

b. What is the appropriate variance function? Does a constant variance in log-transformed data correspond to variance proportional to the mean, mean squared, or some other relationship?

3. Under what conditions should any years be eliminated from the analysis?

Data from 1967 were not used in the Jassby et al. (in press) analyses, because X_2 interpolated data did not go back that far. 1983 was discarded in some cases because DFG scientists believed that populations of longfin smelt, striped bass 38 mm index, and *Neomysis* shrimp were not sampled adequately. [*Note: the Bay Study data for longfin smelt also show an unexpectedly low abundance index in 1983, and that program samples the entire bay. Therefore abundance was probably low in 1983. Perhaps the best resolution of this is to say that relationships for which 1983 appears anomalously low must be constrained to exclude X_2 values that far downstream, because the data are insufficient to describe how the relationship changes at such high flows.*]

Agreement:

a. 1967 should be included for species for which data were available.

Uncertainty:

a. Should 1983 be excluded for the above-listed species?

4. To what extent do the fish-X_2 relationships allow for alternative interpretations or the influence of other variables than those correlated with X_2?

Jassby et al. (in press) raised this issue in connection with striped bass survival from egg to 38 mm, for which X_2 explained only 36% of the variance. A low proportion of variance explained implies one or more other causative factors, the presence of which could alter the survival-X_2 relationship. This analysis has been used by CUWA to suggest caution in using the results to set standards, particularly at the downstream Roe Island site.

Agreement:

a. Other factors which may not be directly related to X_2 or outflow probably also affect each of the species examined

b. The expected importance of alternative effects decreases as the explanatory power of the X_2 models increases, unless there is substantial collinearity among independent variables in the model

c. For each species abundance or survival could probably be increased through other means in addition to salinity or flow standards

d. Setting standards using salinity does not eliminate the need to continue to improve understanding and management

e. The existence of relationships between indices of abundance or survival of a species and X_2 does not necessarily imply that X_2 *itself* is an important variable, merely that either X_2 or one of its numerous covariates is important to that species

Uncertainty:

a. The variability not explained by the models, but incorporated in the within-year variance estimates, includes sampling variability. The possibility was raised that an analysis of variance components could be used to reduce further the unexplained variability in the annual indices. This was not resolved, although it may be worth exploring later.

5. What alternative analyses might identify benefits and detriments of the X_2 standard?

This question mainly relates to the habitat analyses in which salinity requirements of various species and life stages were transformed into size of habitat, defined as distance from the Golden Gate. This analysis was presented as preliminary, in that it did not take into account other physical attributes of habitat such as width, depth, area, volume, or flow patterns, or any biological attributes. An analysis of "co-abundance" was also presented by CUWA to explore the fish-X_2 relationships.

Agreement:

a. We need to know a lot more about this estuary to make management more effective, although without delaying necessary measures. In particular, sampling needs to include shallow habitat where some of the species of concern are found

b. A habitat analysis could provide information useful in interpreting the results of the statistical analyses, or in extending those results to other species

c. The habitat analysis performed by CUWA is only a preliminary step in determining the amount of habitat available to estuarine-dependent species and should be extended to include other habitat variables

Disagreement:

a. Is the habitat analysis presented by CUWA informative as it stands, or does it need to be expanded? CUWA scientists argued that the habitat analysis as presented is evidence for a harmful effect on some species of downstream locations of X_2. Agency and other scientists argued that there was no evidence for adverse effects in any estuarine-dependent species. [*Note: CUWA has made the point that the potential for adverse impacts of the proposed standards, and the potential for harm to indigenous species by improving conditions for introduced competitors, should be considered by EPA in setting standards. This was not discussed to a sufficient extent to identify areas of agreement or disagreement at this meeting.*]

b. Is the co-abundance analysis a useful tool? There was little agreement that correlation analyses among species gave more information than could be gained by examining the fish-X_2 relationships

6. Are there problems with the Fish and Game monitoring data that might affect the fish-X_2 analyses?

Several of the CUWA documents describe or imply possible biases in the monitoring data that would diminish their utility in the analyses. The principal issue here is not sampling error (which would be uncorrelated with X_2), but bias that causes the population estimate to diverge from the population size as X_2 varies.

Agreement:

a. Although the monitoring program is far from perfect, and potential biases have been identified, no systematic biases have been *demonstrated* in the monitoring data that would affect the fish-X_2 relationships (note that CUWA has analyzed only the midwater trawl data)

b. Weighting the abundance data by area or volume around the sampling station does not make much difference in overall outcome of the analyses

c. Monitoring data should be examined for evidence of a spring-neap cycle or other potential biases

d. Monitoring data should be taken at an interval that does not alias the spring-neap tidal cycle, an important time scale of variability in the estuary

e. Fall midwater trawl monitoring data are more useful for some species (e.g. striped bass) than others (e.g. delta smelt) for which the sampling programs were not designed

f. In particular, abundance indices for splittail should not be relied upon

g. Abundance data should be re-analyzed where appropriate using habitat descriptors (e.g. salinity range) to stratify the data and thereby reduce sampling variance

7. Would a Roe Island standard result in more fish (or better survival) than a Chipps Island standard alone?

This generated more heated discussion than any other topic. [*Note: in these meetings and in many other discussions, there has been confusion about the relationship between the location of the control points, mean X_2, and within-year variability. This is discussed under Point 4 in the Background section of this report. The standards do not establish mean X_2 at the control points; they establish the February-June mean of X_2 at some location, and set the variation in X_2. See the examples given under Point 4 (Background).*]

Agreement:

a. The uncertainty around the regression lines increases as X_2 moves downstream

b. The continuous relationships observed imply an increase on average in abundance or survival with decreasing X_2, except in 1983 for some species

c. There is no "right" number of fish of a particular species as long as the population is large enough to be out of danger of extinction.

d. Therefore there is no "right" location for the long-term mean of X_2 or the number or location of control points. These must be determined from considerations in addition to biology. [*Note: EPA has done this by considering a particular time period in which populations were in better condition than they are now, and attempting to replicate those conditions in terms of salinity. In a similar issue, the Central Valley Project Improvement Act somewhat arbitrarily takes doubling as its goal because there is no "right" number of anadromous fish.*]

Disagreement:

a. Is a standard justified at Roe Island, given that the uncertainty in predictions is higher for downstream than for upstream values of X_2?

b. How large is the uncertainty in the flow-X_2 relationships (see issue 2 above)?

c. Does a monotonic relationship between X_2 and abundance indices imply that moving X_2 from 74 to 64 km will improve abundance *on average,* or does the scatter in the relationships preclude such a statement about mean values?

d Should the standard at the confluence (81 km) be set at 150 days as now proposed, or at some lower figure as implied by the sliding scale analyses? [*Note: This was mostly outside the scope of this discussion, since not all of the participants had been* *at the sliding scale workshop. Most participants seemed willing to accept EPA's proposal to set this standard at 150 days, but CUWA scientists rejected EPA's Roe Island standard and recommended maintaining historical patterns of within-year variability. These three objectives are mutually incompatible, as discussed in Point 4 (Background).*]

e. Is flow released from reservoirs an adequate substitute for naturally-occurring flow for the purpose of reducing X_2 and achieving the anticipated benefits? There was some belief that high natural flows would carry more nutrients and organic matter into the estuary than would reservoir releases. [*Note: If most of the labile organic matter entering the estuary is from freshwater phytoplankton, and nutrient limitation of lower trophic levels is rare, this effect may not be that important.*]

Not addressed

a. What are the quantitative benefits of a Roe Island standard? [*Note: In a memo dated June 3, Phyllis Fox analyzed the predictions of the various fish-X_2 relationships for differences in abundance for a Roe Island and Chipps Island compliance point. The analysis shows that only longfin smelt and striped bass would benefit from a Roe Island standard. This report has not been reviewed by participants in the May 31 meeting, so it cannot be placed in either the "agreed" or "disagreed" category.*]

8. How far geographically should effects of the standards be monitored?

Agreement:

a. The effects of the standards may appear as far upstream as the reservoirs, and as far downstream as (at least) the Golden Gate

Issues not addressed in the meeting

How would wetland species be affected by the standards?

Participants did not have the expertise to discuss this issue.

What is the relationship between entrapment zone phenomena and the observed fish-X_2 relationships?

This was considered an interesting question and one that, if answered, would help to understand the reasons for the X_2-fish relationships, but not central to the issues being discussed at this meeting.

APPENDIX. LIST OF PARTICIPANTS

Chuck Armor	Fish and Game
Randy Bailey	MWD consultant
Gary Bobker	Bay Institute
Jim Buell	MWD consultant
Phyllis Fox	CUWA consultant
David Fullerton	Natural Heritage Institute
Bruce Herbold	USEPA
Lyle Hoag	CUWA
Tom Howard	State Water Res. Control Board
Jerry Johns	State Water Res. Control Board
Wim Kimmerer	Reporter
Jud Monroe	MWD consultant
Austin Nelson	Contra Costa Water District
Randall Neudeck	Metropolitan Water District
Dudley Reiser	CUWA consultant
John Rice	CUWA consultant
Spreck Rosekrans	Environmental Defense Fund