Plan for Analyzing and Testing Hypotheses (PATH)

Preliminary Decision Analysis Report on Snake River Spring/Summer Chinook

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Draft for Scientific Review Panel

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Preliminary Decision Analysis Report on Snake River Spring/Summer Chinook

Prepared for

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and
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Executive Summary

Introduction

The Plan for Analyzing and Testing Hypotheses (PATH) is a formal and rigorous program of formulating and testing hypotheses. It is intended to identify, address and (to the maximum extent possible) resolve uncertainties in the fundamental biological issues surrounding recovery of endangered spring/summer chinook, fall chinook, and steelhead stocks in the Columbia River Basin. This process grew out of previous efforts by various power regulatory and fisheries agencies to compare and improve the models used to evaluate management options intended to enhance recovery of these stocks.

The objectives of PATH are to:

1. determine the overall level of support for key alternative hypotheses from existing information, and propose other hypotheses and/or model improvements that are more consistent with these data;
2. assess the ability to distinguish among competing hypotheses from future information, and advise institutions on research, monitoring and adaptive management experiments that would maximize learning; and
3. advise regulatory agencies on management actions to restore endangered salmon stocks to self-sustaining levels of abundance.

This report describes the methods and results of the decision analysis framework we have used to address the third objective for Snake River spring/summer chinook salmon. The specific purposes of this preliminary decision analysis report are to: 1) test the methods of decision analysis we have formulated over the last two years; 2) provide decision makers with our **preliminary** insights into the range of potential responses of Snake River spring/summer chinook to alternative management decisions; and 3) characterize the magnitude of various uncertainties and demonstrate their relative importance in affecting the outcomes of alternative management decisions.

The preliminary decision analysis builds on the “retrospective” analyses completed to date by PATH under our first objective. PATH retrospective analyses attempt to identify the major spatial and temporal patterns in abundance, productivity, and survival of these stocks over the last 30 to 40 years and to determine the relative contribution of Habitat, Harvest, Hatchery, Hydro, and Climatic influences to these patterns. Results of these analyses were published in the peer-reviewed PATH FY96 Retrospective Analysis report, and summarized in “Conclusions of FY96 Retrospective Analyses”, a consensus document written by PATH scientists in December, 1996. Other retrospective analyses were completed in FY97, and will soon be published. All of the retrospective analyses completed to date are considered in this report.

PATH retrospective analyses have helped to bring a substantial set of empirical information to bear on alternative hypotheses to explain recent declines and have led to considerable improvements in both our understanding and modeling approaches. In addition, there has been considerable convergence on the historical data sets to use in calibrating and testing models, and on many of the assumptions to be made when projecting future population changes.

The PATH retrospective analyses have also highlighted some major uncertainties in past and current conditions that have yet to be resolved because of incomplete data and differences in interpretation. These uncertainties, along with uncertainties in projecting future conditions, imply that a single management action can have a number of possible outcomes, depending on what is assumed about past, present, and future conditions. This range of possible future outcomes of management actions is best captured by modeling salmon populations under a set of alternative hypotheses about uncertain components of the system.

The preliminary decision analysis described in this report looks systematically at the outcomes of management actions under several alternative hypotheses about biological mechanisms that link actions to possible outcomes. This approach was recommended by the SRP and by independent scientists within PATH as a tool for explicitly considering uncertainties in the decision-making process, in recognition that decisions cannot wait for all uncertainties to be resolved. A variety of management objectives can be used to evaluate alternative actions.

We anticipate that review of this preliminary analysis will lead to refinements in methods and consideration of additional alternative hypotheses. These improvements will be incorporated in the final decision analysis report for spring/summer chinook. The final report will also present analyses of additional management actions to those evaluated in this report (including drawdown of John Day Dam), and will endeavor to reach consensus to the maximum extent possible on the relative weights assigned to alternative hypotheses based on the strength of supporting evidence and our professional judgements. We anticipate, however, that lack of evidence will constrain our ability to reach consensus on the relative likelihood of some alternative hypotheses.

For the next four months, PATH intends to focus on completing analyses for fall chinook before returning to spring/summer chinook. We are distributing this preliminary report now rather than wait until the above refinements are made to show what we have been doing and where we are headed. The final report for spring/summer chinook (which will incorporate the above revisions) will be completed by the fall of 1998.

Decision Options

Although many agencies have drafted some very broad goals to help direct decision making, this decision analysis is focused on a narrower question: **To what extent can alternative hydrosystem actions prevent extinction and lead to recovery of stocks either listed or proposed for listing, including wild spring/summer chinook, fall chinook and steelhead stocks in the Snake River and mid-Columbia region?** This preliminary decision analysis considers three alternative hydrosystem actions: A1 (current operations), A2 (maximize transportation without surface collectors), and A3 (drawdown to natural river level of the four Lower Snake dams). We chose this restricted set of options to allow a thorough evaluation of our biological decision analysis and modeling tools by both PATH scientists and decision-makers. We believe that the next options to be evaluated should be B1 (natural river drawdown of both the four Lower Snake dams and John Day Dam), maximizing transportation with surface bypass collectors (A2'), and the in-river option (A6), so as to bracket the potential range of responses of fish populations.

While PATH is only looking at hydrosystem decisions explicitly, the effects of habitat and harvest management actions are being considered in sensitivity analyses. We are also developing approaches to including uncertainties with respect to management of hatcheries, to be added to our final report. The approaches used for all four H's (hydro, habitat, harvest, hatcheries) will be re-examined following peer review of this report.

We also intend to explore options for an experimental management approach, which varies management actions over time and space in a deliberate attempt to test key hypotheses. An experimental management approach has been recommended by some members of the PATH Scientific Review Panel because some of the major uncertainties are difficult to resolve with current information. Though experimentation may pose risks to these stocks, there is risk inherent in any actions, including continuing present operations, as these populations are at dangerously low levels.

How to Assess the Outcomes of the Options

Outcomes of the alternative actions will depend on what is assumed about past, present and future conditions experienced by fish in response to management actions. The previous PATH retrospective analyses have elucidated a great deal, and have also pointed out uncertainties in past conditions due to incomplete data and potentially confounding influences. These uncertainties generate a range of alternative assumptions about historical conditions, which are used in retrospective modeling analyses that generate quantitative estimates of parameters needed to run models into the future. Results from the retrospective analysis are passed to the prospective modeling analysis, which quantifies the range of possible futures. This set of possible futures depends not only on the understanding and parameter estimates gleaned from the retrospective analysis, but also on assumptions about future conditions (such as climate) and the response of stocks to new management actions (such as Snake River drawdown).

The outcomes of alternative hydro management actions are evaluated in terms of various performance measures. These measures are used to rank alternative actions according to how well they meet specified management goals. A variety of performance measures have been developed to assess the biological implications of different management actions. Because our primary goals are to determine the hydrosystem actions that should be taken to prevent extinction and lead to recovery of endangered stocks, we focus here on the National Marine Fisheries Service (NMFS) jeopardy standards that account for each of these goals. These standards are a measure of the ability of actions to increase the spawning abundance of stocks to levels associated with long-term persistence and stability. Survival standards are based on projected probabilities that the spawning abundance will exceed a pre-defined “survival” threshold over a 24 or 100 year simulation period; survival standards are met when that probability is 0.7 or greater. Recovery standards are based on probabilities of exceeding a “recovery” threshold in the last eight years of a 48-year simulation period; this standard is met when the probability is 0.5 or greater.

The standards are applied to the sixth best stock out of the seven Snake River “index” stocks of spring/summer chinook (Imnaha, Minam, Bear Valley/Elk, Sulphur Creek, Marsh Creek, Johnson Creek, and Poverty Flats) to ensure that most of the stocks are able to meet the survival and recovery goals. These seven index stocks are the only ones for which sufficient historical data exist to develop spawner-recruit relationships, required for generating projections of future stock sizes. Further work is required to generalize results from these stocks to all wild chinook populations of the Snake River basin.

Uncertainties in the Response of Populations to Management Actions

There are many uncertainties that can potentially affect the responses of fish populations to management actions. We have focused on twelve of the most important of these uncertainties, and have laid out a range of alternative hypotheses for each. The uncertainties are of two types: uncertainty regarding the future environment, and uncertainty regarding how the system works (i.e., the survival changes caused by management actions). Although the future environment may be beyond human control (e.g., future climate), the uncertainty inherent in projecting it is of potential significance in determining future population sizes. Alternative hypotheses to describe how the system works often hinge on the interpretation of historical information, because the functional relationships in models are based on both general principles and historical data. However, as past information is incomplete, there are differing interpretations of the relative importance of different factors in causing recent declines of Snake River spring-summer chinook.

The twelve uncertainties considered in the preliminary decision analysis were:

1. *Passage assumptions* – uncertainty in direct survival of in-river fish, and the partitioning of in-river survival between dam and reservoir survival.
2. *Fish guidance efficiency (FGE)* – uncertainty in the effectiveness of extended-length screens in diverting fish away from the turbines, relative to standard-length screens.
3. *Turbine/Bypass Mortality* – uncertainty in historical estimates of bypass and turbine mortality for some projects prior to 1980.
4. *Predator Removal Effectiveness* – uncertainty in the effect of the predator removal program (i.e., removal of squawfish for bounties) on survival of salmon smolts in reservoirs.
5. *Transportation assumptions* – uncertainty in the relative survival of transported and non-transported fish after the fish have exited the migration corridor (i.e., below Bonneville Dam).
6. *Stock productivity* – uncertainty in the extent to which Snake River and lower Columbia stocks share common mortality effects.
7. *Extra mortality* – uncertainty in the mortality of both transported and non-transported fish occuring beyond Bonneville Dam.
8. *Future climate* – uncertainty in future patterns in climatic conditions.
9. *Habitat effects* – uncertainty in the biological effects of future habitat management actions.

We also considered the following three uncertainties when projecting the effects of drawdown to natural river of the four lower Snake River dams (option A3):

10. *Length of Pre-Removal Period* – the duration of time between a decision to proceed with drawdown and actual removal of dams (pre-removal period) due to uncertainty in the Congressional appropriations process and the possibility of litigation.
11. *Length of Transition Period* – duration of period between completion of dam removal and establishment of equilibrium conditions in the drawndown section of the river (transition period), reflecting uncertainty in the physical and biological responses to drawdown (e.g., short-term response of predators, release of sediment).
12. *Juvenile survival rate once river has reached equilibrium conditions after drawdown* – uncertainty in the long-term physical and ecological effects of drawdown (e.g., change in density of predators).

We call a particular combination of hypotheses for these twelve uncertainties *a prospective aggregate hypothesis*. Each prospective aggregate hypothesis potentially yields a unique biological response to an action. We have explored 5,148 different aggregate hypotheses in this preliminary analysis. One of our objectives was to determine which uncertainties have limited effects on performance measures and the resulting decision, so that we can focus on the most critical alternative hypotheses. In the final report, we may also develop new variations or combinations of hypotheses that better reflect recent evidence.

We also consider alternative harvest schedules to assess the sensitivity of responses to hydro actions to variations in harvest rate. A number of potentially important factors were not explicitly quantified in the models, although some are considered implicitly in the models to some extent. These include several factors discussed by the Independent Scientific Group in the "Return to the River" report, such as the effects of genetic interactions between populations, and impacts of the hydropower system on conditions in the estuary.

Results

There were five objectives for the results of the preliminary analyses:

1. Explore ways to summarize complex analyses and results into graphs that are easy to understand, interpret, and explain to decision-makers.
2. Provide **preliminary** insights into the relative performance of alternative actions.
3. Identify key uncertainties that affect the results.
4. Test the sensitivity of decisions to the weights placed on key uncertainties, so as to focus the assessment of existing evidence, and the acquisition of additional evidence.
5. Summarize results for some other important performance measures.

Ways to Summarize Results

We have generated predicted outcomes for alternative management actions using each possible aggregate hypothesis. Since there are 5,148 unique aggregate hypotheses, there are 5,148 unique alternative futures that one could examine to evaluate alternative actions. We used two alternative ways to summarize these outcomes. The first was to show a frequency distribution of all outcomes for a single action. This shows the range of possible futures associated with the uncertainties in past, present, and future conditions (an example for Action A1 is shown in Figure E-1). We separated results generated with the CRiSP-T3 passage model and transportation assumptions from those generated with the FLUSH-T1/T2 model because these two models represent fundamentally different approaches to estimating mortality through the juvenile migration corridor and because they are each associated with different assumptions about the relative survival of transported and non-transported fish in the ocean (i.e. CRiSP is associated with transportation assumption T3, while FLUSH is associated with T1 and T2).

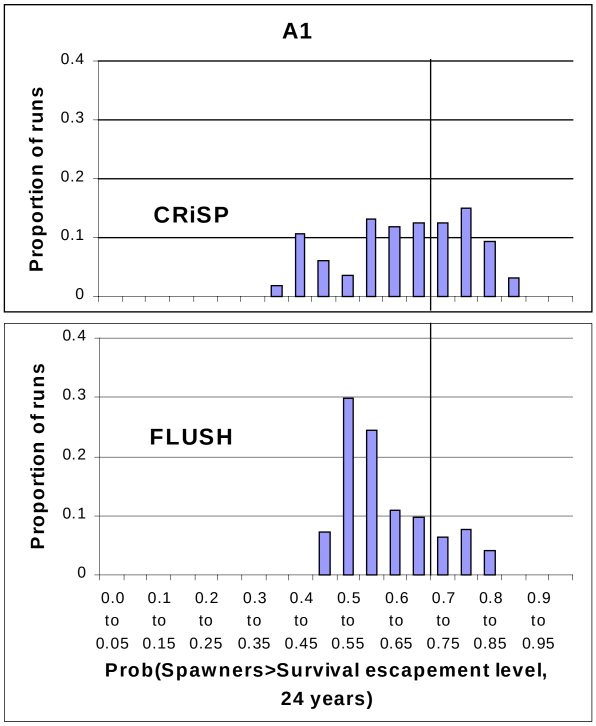


Figure E-1: Frequency distribution of possible future outcomes of Action A1 using CRiSP-T3 (top) and FLUSH-T1/T2 (bottom) passage models and transportation assumptions. Outcomes are measured as the probability of the spawning abundance of the sixth best stock exceeding the survival level of escapement in the first 24 years of the 100-year simulation period. The height of the bars reflects the relative frequency with which a particular outcome is projected. The vertical line at 0.7 represents the NMFS survival standard; outcomes to the right of that line are considered to have met the 24-year survival standard.

The second approach was to calculate the "expected ability" of an action to meet the NMFS survival and recovery goals. This is essentially the weighted fraction of the 5,148 outcomes that met the NMFS criteria for survival and recovery, where the weights reflect the relative degree of belief in one hypothesis over another. In the preliminary analysis, all hypotheses were given equal weights. An example of this type of output is shown in Figure E-2.

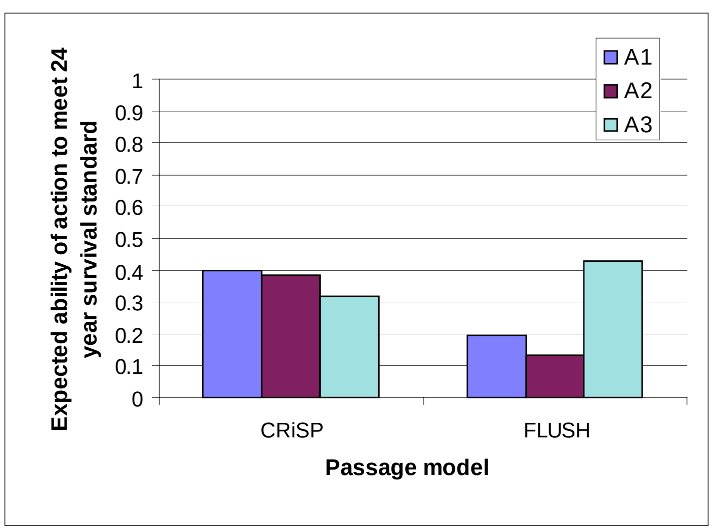


Figure E-2: Expected ability of A1, A2, and A3 to meet the 24-year survival standard. The standard is met when the spawning abundance of the sixth best index stock exceeds the survival escapement level an average of 70% of the time over the first 24 years in the 100-year simulation period.

Relative Performance of Alternative Actions (Preliminary results)

1. ***There is a large variation in outputs, even within models and actions.***

   There is considerable uncertainty in the outcomes of alternative management actions. Probabilities of spawning abundances exceeding survival and recovery escapement levels can range anywhere from very low to very high values, depending on the underlying aggregate hypothesis. For example, probabilities of being above the recovery escapement level generated with the FLUSH-T1/T2 passage model range from 0.15 to 0.85 under A1, while CRiSP-T3 probabilities range from 0.05 to 0.9. For both models, there is greater variation in probabilities associated with recovery escapement levels than in probabilities of exceeding survival escapement levels. CRiSP-T3 results generally have a greater range than FLUSH-T1/T2 runs, particularly under A3. Since this introduces considerable uncertainty into which decision should be made, it is important to identify the individual components of an aggregate hypothesis that have the greatest effect on decisions.

2. ***Relative performance of the management options depends on passage model assumptions.***

   Using CRiSP-T3 passage model and transportation assumptions, A1 or A2 had very similar expected abilities to meet the NMFS standards, while A3 always is the lowest. With FLUSH-T1/T2 passage model and transportation assumptions, A3 always has the highest expected ability, followed by A1 and then A2. Drawdown (A3) represents both improved in-river survival and a reduction in transprotation. Under the CRiSP transportation assumptions, A3 causes a net decline in survival relative to A2; with FLUSH the reverse occurs. This confirms our general expectations based on the structure and application of these models, but our result explicitly quantifies the differences. Such a quantification is extremely important.

3. ***Long-term standards are easier to meet than short-term standards.***

   With CRiSP-T3, the expected ability of action A2 was 0.38 for the 24-year survival standard, but was 0.65 for the 100-year survival standard, and 0.5 for the 48-year recovery standard. The expected ability of action A3 with FLUSH-T1/T2 was 0.42 for the 24-year survival standard, and 1.0 for both the 100-year survival and the 48-year recovery standards.

4. ***With this set of actions, there are few instances in which <u>all</u> of the survival and recovery standards are met with a high expected ability.***

   The highest expected ability to meet all survival and recovery standards using the CRiSP-T3 model is around 0.35 (obtained with action A2), and around 0.4 using FLUSH-T1/T2 (obtained with action A3). We would assume that decision-makers would want the expected ability to meet all of the recovery and survival standards to be high, since that implies a high degree of certainty that these standards will be met. These preliminary results suggest that significantly greater improvements in survival are required beyond those provided by the management actions analyzed here, since none of the current set of actions are able to meet all of the standards with any degree of certainty (at least when the aggregate hypotheses are weighted equally).

5. Alternative standards and harvest schedules affect the outcomes of management options, but not their relative ranking.

The ranking of actions was not affected when we applied weaker (i.e. easier to meet) and stronger (more difficult to meet) jeopardy standards than the informal NMFS definition (0.70 probability of exceeding survival escapement levels, 0.50 probability of exceeding recovery escapement levels), although the expected ability of actions was predictably lower for the stronger standard and higher for the weaker standard. The ranking of actions was also unaffected when we use two more conservative harvest rate schedules than the one based on current management. In one of these alternative schedules, harvest rates are reduced by one-third from their current values. This change had little or no effect on the expected ability of actions to meet survival and recovery standards. In the other alternative schedule, harvest rates of spring-summer chinook are set to 0. Here, the effects were greater than when harvest rates were reduced by one-third; the magnitude of these effects on outcomes depended on the action and passage model assumptions.

Sensitivity of Outcomes and Decisions to Effects of Uncertainties

To assess the sensitivity of outcomes to other uncertainties, we defined two possible criteria for decision-making, both based on the NMFS Jeopardy Standards. The first is a **relative criterion**, in which the preferred action is the one that simply maximizes the expected ability to meet all three NMFS survival and recovery standards. Because the transportation vs. drawdown question seems to be of most interest in the region, we are concerned primarily with the relative ranking of A2 and A3 in this sensitivity analysis. The second possible basis for decision-making is based on an **absolute criterion**. We assume that some minimum expected ability to meet survival and recovery standards is required for an action to be considered acceptable. Since it is not clear at the moment what the minimum expected ability should be, we use 0.7 for illustrative purposes. These criteria are admittedly difficult to meet, since they include the 24-year survival standard (see conclusion #3 above).

6. Within each model, very few uncertainties have significant effects on outcomes and decisions.

The only uncertainty that significantly affects the decision is the uncertainty about the source of extra mortality. Under the "BKD" hypothesis and the regime shift hypothesis, all of the actions fall short of our assumed criterion of 0.7 expected ability to meet all standards. In other words, if post-Bonneville extra mortality remains regardless of hydrosystem actions, the stocks will have a poor ability to recover. However, if extra mortality is related to the hydrosystem, both A1 and A2 (under CRiSP-T3) and A3 (under FLUSH-T1/T2) exceed this minimum level.

In terms of future analyses and monitoring, overall results suggest that many of the uncertainties could be ignored, since they appear to have relatively modest affects on the model results. The main uncertainties to resolve are those associated with passage model assumptions (i.e., estimates of direct in-river survival, and relative survival of transported and non-transported fish), and extra mortality. In some cases, experimental management actions may present the only opportunity for resolving these uncertainties. We plan to have a workshop in 1998 to explore the feasability, benefits, and risks of such experiments.

7. ***The ranking of actions is relatively insensitive to "best" and "worst" case combinations of hypotheses.***

   We looked at the effects of "best-case" and "worst-case" combinations of passage-related hypotheses, drawdown-related hypotheses, and stock productivity and future climate hypotheses. Best and worst case sets of have predictably large effects on results, but they do not significantly affect the relative ranking of actions. A1 or A2 is still always the best with CRiSP-T3, and A3 is still always the best under FLUSH-T1/T2. *In terms of meeting an absolute criterion, the expected ability of actions to meet the survival and recovery standard is below 0.7 for all cases, except for the best-case drawdown scenario.* Under this combination of hypotheses, the expected ability to meet survival and recovery standards for A3 increases to around 0.8 under FLUSH-T1/T2.

8. ***Results are similar using a single stock (Marsh Creek).***

   Results for a single stock (Marsh Creek), using a different standard (0.75 probability of exceeding survival escapement levels over 24 years) show the same general patterns. CRiSP-T3 assumptions tend to favor A2, while FLUSH-T1/T2 assumptions favor A3. Passage model assumptions and extra mortality hypotheses were important in determining which actions met a 24-year survival standard. FGE, PREM, and prospective model alternatives were also important in the results for the single stock.

Sensitivity of Outcomes and Decisions to Weightings on Alternative Hypotheses

PATH will attempt to assign weights to those key uncertainties based on the weight of evidence for and against particular hypotheses. The first step in assigning these weights is to establish just how sensitive the decision is to the weightings that are placed on alternative hypotheses. For example, what is the critical weighting that must be placed on the hydro-related hypothesis for extra mortality before the 0.7 threshold is reached? This information can help to frame the assignment of weights by identifying what the critical weights are. Precise framing of this discussion will be particularly important where there is disagreement among PATH scientists and agencies over what these relative weights should be.

Our results indicate that passage model, extra mortality, and best/worst combinations of drawdown hypotheses had the greatest effects on decisions. Unfortunately, these uncertainties will also likely be the most difficult to assign weightings to, because of firmly-held beliefs about the interpretation of historical data and because extra (post-Bonneville) mortality and drawdown effects are the most difficult to measure. Therefore, we looked at the effects of different weightings on these hypotheses on the expected ability of actions to meet all three survival and recovery standards.

9. ***There has to be a great deal of certainty about passage model assumptions and the hydro extra mortality hypothesis before any of the actions achieve an expected ability to meet all survival and recovery standards greater than 0.7.***

   For A1 and A2, this criterion is only met if we are absolutely certain (i.e. weight=1.0) that CRiSP-T3 and the hydro extra mortality hypothesis are correct. For A3, the 0.70 criterion is met when FLUSH-T1/T2 is assigned a weight of at least 0.8 and the hydro hypothesis is assigned a weighting of 1.0, or when FLUSH-T1/T2 is assigned a weighting of 1.0 and the hydro hypothesis is assigned a weighting of 0.8.

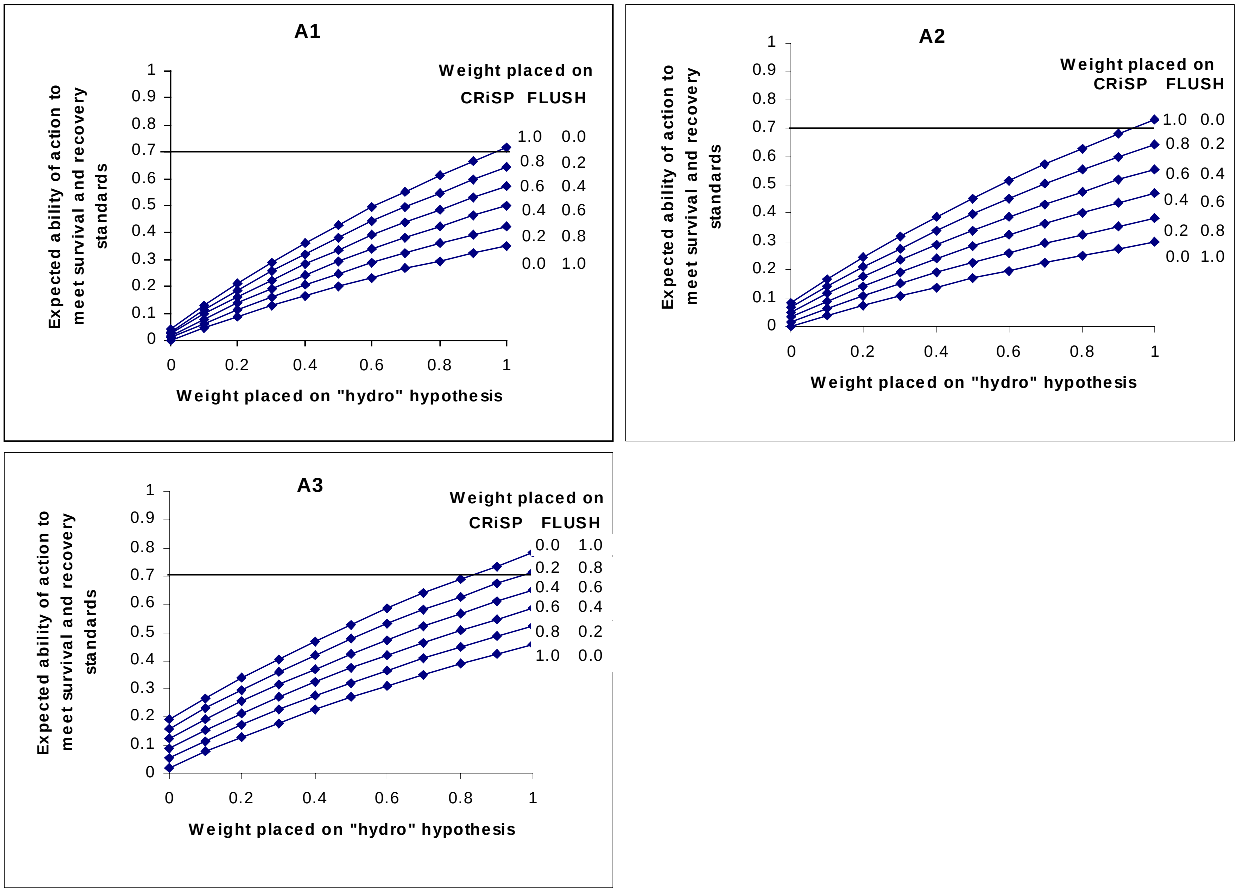


Figure E-3: Sensitivity of expected ability to meet survival and recovery standards to relative weights placed on the "hydro" extra mortality hypothesis and the passage models. Note that the remaining weight placed on the extra mortality hypotheses (i.e., 1 – weight placed on hydro hypothesis) is divided evenly between the "BKD" and the regime shift hypotheses. For example, when the weight placed on the hydro hypothesis is 0.8, the weights placed on the "BKD" and the regime shift hypotheses are both 0.1.

10. There has to be virtual certainty that the pre-removal and transition periods for drawdown to achieve an expected ability to meet survival and recovery standards greater than 0.7, and this only occurs for FLUSH-T1/T2.

A3 under FLUSH-T1/T2 only met the 0.7 criterion when it was certain that the pre-removal period was three years, and the transition period two years. If the weights on these hypotheses are high, then the assumptions about equilibrated juvenile survival rate did not matter.

11. Key uncertainties are unlikely to be resolved with existing data.

There will have to considerable agreement on three key uncertainties (in-river survival, transportation assumptions, and extra mortality) before one of the actions is clearly able to meet the survival and recovery standards. Given the lack of data that gave rise to the uncertainties, and the strongly-held beliefs which fill in dats gaps, this consensus is not likely to be achievable without a well-planned experimental design that is specifically directed towards answering questions about extra mortality and passage model assumptions.

Other Performance Measures

The NMFS jeopardy standards are only one of a number of different measures of performance produced by PATH modeling analyses. In this report, we also briefly report results for two additional measures: projected harvest rates, and Smolt-to-Adult survival rates from the time they pass the upper-most dam as smolts to the time they return to that dam as adults.

12. Projected harvest rates are highly variable.

We showed an example of the trends in mainstem harvest rates for a single stock (Imnaha), and a single action (A1) over time. We showed this for an optimistic aggregate hypothesis and a pessimistic hypothesis. In most years, harvest rates can range from below 0.1 to above 0.35 for a particular scenario. Such uncertainty is important to communicate to decision-makers and to others who will be using this information, such as the economic workgroup.

13. Median SARs of between 2 and 7% are associated with meeting the 100-year survival standard.

This is consistent with the interim SAR goal of between 2 and 6% identified by the PATH hydro workgroup (Ch. 6 in PATH FY1996 Retrospective Report). Note that these 'median SARs' are computed over a 100-year period.

In addition to quantitative performance measures, we would also like to look at how well the alternative management actions do in terms of qualitative measures of performance such as the concepts discussed in the ISG's "Return to the River" report. Such qualitative measures can allow us to incorporate less quantitative but nonetheless important issues relating to the relative health of individual salmon populations, aquatic communities, and entire ecosystems.

Again, we caution that these results are preliminary. We anticipate that review of this preliminary analysis will lead to refinements in methods and consideration of additional alternative hypotheses. These improvements will be incorporated in the final decision analysis report for spring/summer chinook. The final report will also present analyses of additional management actions to those evaluated in this report (including drawdown of John Day Dam), and will endeavor to reach consensus to the maximum extent possible on the relative weights assigned to alternative hypotheses based on the strength of supporting evidence and our professional judgements. We anticipate, however, that lack of evidence will constrain our ability to reach consensus on the relative likelihood of some alternative hypotheses.

Table of Contents

List of Tables

List of Figures

1 Introduction

1.1 Introduction

The Plan for Analyzing and Testing Hypotheses (PATH) is a formal and rigorous program of formulating and testing hypotheses. It is intended to identify, address and (to the maximum extent possible) resolve uncertainties in the fundamental biological issues surrounding recovery of endangered spring/summer chinook, fall chinook, and steelhead stocks in the Columbia River Basin. This iterative process grew out of previous efforts by various power regulatory agencies and state, federal, and tribal fisheries agencies to compare and improve the models used to evaluate management options intended to enhance recovery of these stocks.

The objectives of PATH are to:

1. Determine the overall level of support for key alternative hypotheses, and propose other hypotheses and/or model improvements that are more consistent with existing data;
2. Assess the ability to distinguish among competing hypotheses from future information, and advise institutions on research, monitoring and adaptive management experiments that would maximize learning; and
3. Advise regulatory agencies on management actions to restore endangered salmon stocks to self-sustaining levels of abundance.

PATH has done considerable work on Objective 1. This report focuses on the third objective. We intend to return to Objective 2 in FY98.

PATH analyses thus far have focussed on Snake River spring/summer chinook salmon, and have progressed in two stages: “Retrospective” analyses and “Prospective” analyses. The retrospective analyses attempt to identify the major spatial and temporal patterns in abundance, productivity, and survival of these stocks over the last 40 years and to determine the relative contribution of Habitat, Harvest, Hatchery, Hydro, and Climatic influences to these patterns. Our first major set of analyses was published in the PATH FY96 Retrospective Analysis report, and has received generally favorable reviews by an independent Scientific Review Panel (SRP). Much of this work will be published in peer-reviewed scientific journals in the near future. PATH also summarized the FY96 analyses in “Conclusions of FY96 Retrospective Analyses”, a document that represented the consensus of PATH scientists as of December, 1996. The Conclusions Document assessed the strength of evidence for each conclusion, considered alternative interpretations of historical information, and additional information needs required to strengthen those conclusions. This document was also favorably reviewed by the PATH SRP. Several additional retrospective analyses were completed in FY97 and a report documenting these analyses is planned in the near future. All of the retrospective analyses completed to date are considered in this report.

The PATH retrospective analyses have helped to bring a substantial set of empirical information to bear on alternative hypotheses to explain recent declines (e.g., stock-recruitment information, in-river survival studies, transportation experiments, smolt-to-adult return rates [SARs]) and have led to considerable improvements in both our understanding and modeling approaches. In addition, there has been considerable convergence on the historical data sets to use in calibrating and testing models, and on many of the assumptions to be made when projecting future population changes. Because the future is uncertain, and we are uncertain about several functional relationships in the system, the range of possible futures is best captured through a set of alternative hypotheses about different components of the system.

A given set of alternative hypotheses about all components of the system (stock productivity, downstream migration, marine survival, transportation, future climate, etc.) is referred to as an “aggregate hypothesis”. Historical information can be used to assess the likelihood of alternative hypotheses about one or more system components. For example, the 1996 retrospective analyses concluded that escapement, productivity and survival have been poorer for upriver stocks than for downriver stocks, since 1975. Different aggregate hypotheses address the question of why the performance of upriver stocks is poorer than downriver stocks, and attribute different degrees of influence to the various factors that can potentially explain this pattern. These factors include direct passage mortality within the hydrosystem, delayed passage mortality (after Bonneville Dam) that is related to the hydropower system, and extra mortality that is independent of the hydropower system (due to changing climate, or the effects of hatcheries). These alternatives add uncertainty to decisions about the hydrosystem. A major objective of this report is to understand the effects of these uncertainties on the future condition of populations under different management actions. Aggregate hypotheses are discussed in more detail in Chapter 4.

PATH “prospective” analyses attempt to evaluate the ability of alternative management actions to restore depressed populations of spring/summer chinook stocks. These prospective analyses are based on results of the retrospective analyses, and use this information in two different but complementary ways to assess alternative management actions. The first approach is a weight-of-evidence approach, which synthesizes existing information around specific questions in the decision-making process. This approach was applied in Chapter 6 of the FY96 Retrospective Report, which developed a flowchart of key questions to consider when making decisions about the hydropower system and compiled available evidence to answer those questions.

The second approach to prospective analyses is a formal decision analysis which systematically looks at the outcomes of management actions under several alternative hypotheses about biological mechanisms that link actions to outcomes. Based on their outcomes, actions are then ranked according to specified management objectives. This approach was recommended by the SRP and by independent scientists within PATH as a tool for explicitly considering uncertainties in the decision-making process, in recognition that decisions cannot wait for all uncertainties to be resolved. The biological rationale for alternative hypotheses uses much the same information as the “weight-of-evidence” approach completed in FY96. A variety of management objectives can be used to evaluate alternative actions. This report focuses mostly on the survival and recovery standards used by the National Marine Fisheries Service (NMFS), which are defined precisely in Section 3 and Appendix D. In general terms, the survival standard is the fraction of time during many simulations that the spawning abundance of a stock is above a certain specified low threshold. This fraction is calculated for both 24 and 100 years (about 6 and 25 salmon generations, respectively). The recovery standard is the probability that the spawning abundance exceeds a specified recovery level during the last 8 years of a 48-year period.

Over the past two years, PATH has developed a decision analysis framework and completed a preliminary decision analysis for Snake River spring/summer chinook stocks. The purpose of this preliminary decision analysis report on spring/summer chinook is to:

1. test the methods of decision analysis we have formulated over the last two years both to provide technical guidance to future PATH analyses to acquaint regulatory agencies with these methods, and to obtain feedback on the utility of our approaches;
2. provide decision makers with our preliminary insights into the range of potential responses of Snake River spring/summer chinook to alternative management decisions (while cautioning that these insights may change significantly in future reports); and

3. characterize the magnitude of various uncertainties and demonstrate their relative importance in affecting the outcomes of alternative management decisions, both to aid decision makers in understanding the complexity of decision making, and to focus further efforts of PATH participants and other scientists/managers on those critical uncertainties.

We stress that this is a work in progress. We anticipate that review of this preliminary analysis will lead to refinements in methods and consideration of additional alternative hypotheses. These improvements will be incorporated in the final decision analysis report for spring/summer chinook. The final report will also present analyses of additional management actions to those evaluated in this report (including drawdown of John Day Dam). To the extent possible, we will endeavor to reach consensus on the relative weights assigned to alternative hypotheses based on the strength of supporting evidence and our professional judgements. We anticipate, however, that lack of evidence will constrain our ability to reach consensus on the relative likelihood of some alternative hypotheses.

For the next four months, PATH intends to focus on completing analyses for fall chinook before returning to spring/summer chinook. Therefore, we distributing this preliminary report now rather than wait until the above refinements are made to show what we have been doing and where we are headed. The final report for spring/summer chinook (which will incorporate the above revisions) will be completed by the fall of 1998.

It is important to recognize the different levels of decision making which exist in the Columbia hydropower system. Many agencies have drafted some very broad goals to help direct decision making, for example, the draft Multi-Year Implementation Plan being developed by the Columbia Basin Fish and Wildlife Authority identifies the following goal

> "Restore sustainable, naturally producing fish and wildlife populations to support tribal and non-tribal harvest and cultural and economic practices. This goal will be achieved by restoring the biological integrity and the genetic diversity of the Columbia River ecosystem and through other measures that are compatible with naturally producing fish and wildlife populations."

While the PATH group considers these goals to be of importance, this decision analysis is focused on a narrower question: **To what extent can alternative hydrosystem actions prevent extinction and lead to recovery of stocks either listed or proposed for listing, including wild spring/summer chinook, fall chinook and steelhead stocks in the Snake River and mid-Columbia region?**

In addressing this question, the PATH group adopted several principles:

1. recognize that there are alternative hypotheses regarding the causes of historical population changes, the responses of fish populations to future management actions, and the range of climatic conditions fish may encounter in the future;
2. agree to the greatest extent possible on the set of empirical studies to be used for evaluating alternative hypotheses;
3. develop an analytical framework which can easily and clearly implement alternative hypotheses about different components of the system, as well as aggregate hypotheses that combine component hypotheses;

4. demonstrate the implications and relative importance of alternative hypotheses for future decisions; and
5. document the biological rationale for alternative hypotheses, based to the greatest degree possible on empirical evidence, utilizing previous PATH work and other studies.

Through these principles, the PATH process will ensure that the region has the benefit of the best available scientific methods and information in the analyses supporting efforts to recover and rebuild endangered fish stocks. The focus of PATH analyses will be on spring / summer chinook, fall chinook, and steelhead.

Conclusions on sockeye will be based on very general inferences from the spring / summer chinook analyses, but will not consider the sockeye captive brood stock program and supplementation issues, due to the limitations of both time and information.

1.2 Structure of this Report

The main part of this report is intended to be read by decision makers and their technical advisors. The appendices provide more detailed information on the methods used and the rationale for alternative hypotheses. The structure of Appendices A and B parallels the structure of Sections 4 and 5 of this report. In particular, each of the system components described in Section 4, and the alternative hypotheses associated with these components, are elaborated upon in Appendix A. Appendix B provides more detailed results of our analyses, while Section 5 provides an overview of the main results. For a general overview of the report, we recommend reading Sections 1-3, 4.1, the introductions to Sections 4.2 and 4.3 and Section 5.

Thus, a considerable amount of uncertainty will remain, and we will rigorously assess the implications of this uncertainty.

2 Decision Options

The set of actions currently under consideration for managing the hydrosystem is shown in Table 2-1. This set of actions has been developed by the Implementation Team (I.T.), and draws from previous experience of analyzing a much larger set of options (refs: Biological Opinion; System Operating Review; System Configuration Study). Appendix C describes the hydrosystem operating requirements associated with each option. This preliminary decision analysis only considers three of the options in Table 2-1: A1 (current operations), A2 (maximize transportation without surface collectors) and A3 (drawdown to natural river level of the four Lower Snake dams). We chose this restricted set of options so as to allow us to proceed with a reasonably thorough test of our biological decision analysis and modeling tools, without having to wait for further work by the hydrologic modelers who simulate the flows expected under different scenarios. We believe that the next options to be evaluated should be B1 (natural river drawdown of both the four Lower Snake dams and John Day Dam), maximizing transportation with surface bypass collectors (A2'), and the in-river option (A6), so as to bracket the potential range of responses of fish populations. Later in this report (Appendix A) we refer to historical conditions from 1970 to the present as Scenario A0.

While PATH is only looking at hydro system decisions explicitly, the effects of habitat and harvest management actions are being considered in sensitivity analyses. Sections 4.3.5 and 4.3.7 contain descriptions of how habitat and harvest are incorporated in the analysis, but generally for habitat issues we considered a range of productivities for each stock, while for harvest we explored the effects of more conservative and more liberal harvest regulations. We are developing approaches to including uncertainties with respect to management of hatcheries, to be added to our final report. The approaches used for all four H's (hydro, habitat, harvest, hatcheries) will be re-examined following peer review of this report.

Table 2-1: Hydro system management actions currently under consideration. The in-river improvement option has not yet been quantitatively defined.

Scenario	**Flow Augmentation**		**Drawdown of 4 Snake River dams**	**Drawdown of John Day Dam**
	Columbia	**Snake**		
A1 (Current Operations)	X	X	-	-
A2	Maximize transportation (without surface collectors)			
A2'	Maximize transportation (with surface collectors)			
A3	X	X	Natural River	-
A5	X	-	Natural River	-
A6 (In-river)	No transportation, flow augmentation, surface bypass collection			
B1	X	X	Natural River	Natural River
B2	-	-	Natural River	Natural River
C1	X	X	Natural River	Spillway Crest
C2	-	-	Natural River	Spillway Crest

Some members of the PATH Scientific Review Panel have recommended that, in light of some of the major uncertainties that are difficult to resolve with current information, we focus attention on experimental management options which vary management actions over time and space in a deliberate attempt to test key hypotheses pertaining to the response of fish populations. Though experimentation may pose risks to these stocks, there is risk inherent in any actions, including continuing present operations, as these populations are at dangerously low levels. We intend to explore what kinds of options for experimental management actions may be feasible for the hydrosystem, recognizing that external factors (such as ocean conditions) could confound the results of an experimental change to the hydropower system. We also recognize that there are many people working to develop and analyze alternative actions, and the creation of additional actions could affect the schedule for completion of biological, economic and social impact analyses. This report does not evaluate any experimental management strategies.

3 How to Assess the Outcomes of the Options

3.1 Overview

We have focused our analysis on future decisions, but used the past to develop our understanding of how the hydrosystem and nature interact to affect fish populations. Here we provide a general overview of our approach (Figure 3-1). The previous PATH retrospective analyses have elucidated a great deal (see PATH 1996 Conclusions Document), and have also pointed out uncertainties in past conditions due to incomplete data and potentially confounding influences (Box 1 in Figure 3-1). These uncertainties generate a range of alternative assumptions about historical conditions, such as the mortality of fish at specific dams in past years, or the success of past transportation experiments (Box 2). These alternative assumptions about the past, together with historical flow information (Box 3), are used in retrospective modeling analyses that generate quantitative estimates of parameters needed to run models into the future. This requires running both passage models, which estimate survival from Lower Granite Reservoir to Bonneville Dam (Box 4) and life cycle models (Box 6). Spawner-recruit data and environmental data (e.g. climate indicators) are also used in the life cycle models (Box5). The retrospective modeling analysis quantifies our understanding of the variability in survival rates, and the factors which affect them. Results from the retrospective analysis are passed to the prospective analysis (Box 7). The prospective modeling analysis (Boxes 11 and 13) quantifies the range of possible futures, expressed as specific performance measures (Table 3-1). This set of possible futures depends on:

- the understanding and estimated parameter values gleaned from the retrospective analysis (Box 7);
- the specific future action under consideration (Box 8; scenarios A1, A2, or A3);
- the expected flows associated with this action (Box 10); and
- assumptions about future conditions, including passage survival assumptions (Box 9) such as fish guidance efficiency through bypasses around dams, and non-passage assumptions (Box 12) such as harvest schedules, habitat improvements and future climate.

For the prospective analysis, the alternative hydrosystem management actions described in Section 2 are evaluated by simulating their consequences using a linked set of models in a four-step process to generate performance measures (Figure 3-2):

1. A hydro-regulation model translates each management option into the mean monthly flows which would be observed in the Snake and Columbia Rivers at various locations, (the U.S. Army Corps' HYSER model has been used for the scenarios included in this report). The hydro-regulation model is run for the water years 1929-1988 to generate a representative set of flows, and this information is used as input to the passage models.
2. A passage model translates the projected set of flows and dam configurations and operations for a given year into the estimated passage survival of both transported and non-transported smolts through the migration corridor from the head of Lower Granite Reservoir to the tail-race of Bonneville Dam. The passage models simulate passage survival rates under each management action for the water years 1977-1992, to permit comparison with survival rates in retrospective simulations, and compute the relative improvement in survival. The longer term water record (i.e., 1929-1988) is considered in step 4. We have used two different passage models, CRiSP and Spring FLUSH, which use different approaches to predicting passage survival rates. The primary differences between these two models are described in Section 4.2.1 and Appendix A, Section A.2.1.
3. One of the key pieces of information passed from the retrospective modeling analysis to the prospective analysis are estimates of the ratio of post-Bonneville survival rates of transported and non-transported fish. These ratios are generated by combining estimates of historical passage survival rates with the results of transportation experiments.

4. A life-cycle model generates a range of possible spawner abundances for each stock and year, under each management option. It does this by combining information produced by the passage models (i.e., the projected passage survivals, fraction of fish transported, and post-Bonneville survival assumptions) together with estimates of the other (non-passage) influences on survival (i.e., stock productivity, adult survival during upstream migration and harvest, post-Bonneville mortality, climate conditions, habitat changes, and harvest). The life-cycle model performs a thousand simulations for a given set of passage model inputs to ensure that the full range of possible ways the system works, and thus the full range of possible futures, is adequately simulated, and that the uncertainty in performance measures (Table 3-2) is properly estimated. These simulations randomly select passage model outputs from each of the years 1977-1992 according to how frequently the flow in each year occurred in the long term historical record (1929-1992). For example, an extremely low flow year like 1977 (the lowest flow in the entire 1929-1992 period) is selected much less frequently than a more typical flow year like 1979 or 1985. The life cycle model also considers alternative assumptions with respect to whether upstream and downstream stocks have some common responses to climate fluctuations (DELTA approach) or respond independently (ALPHA approach).

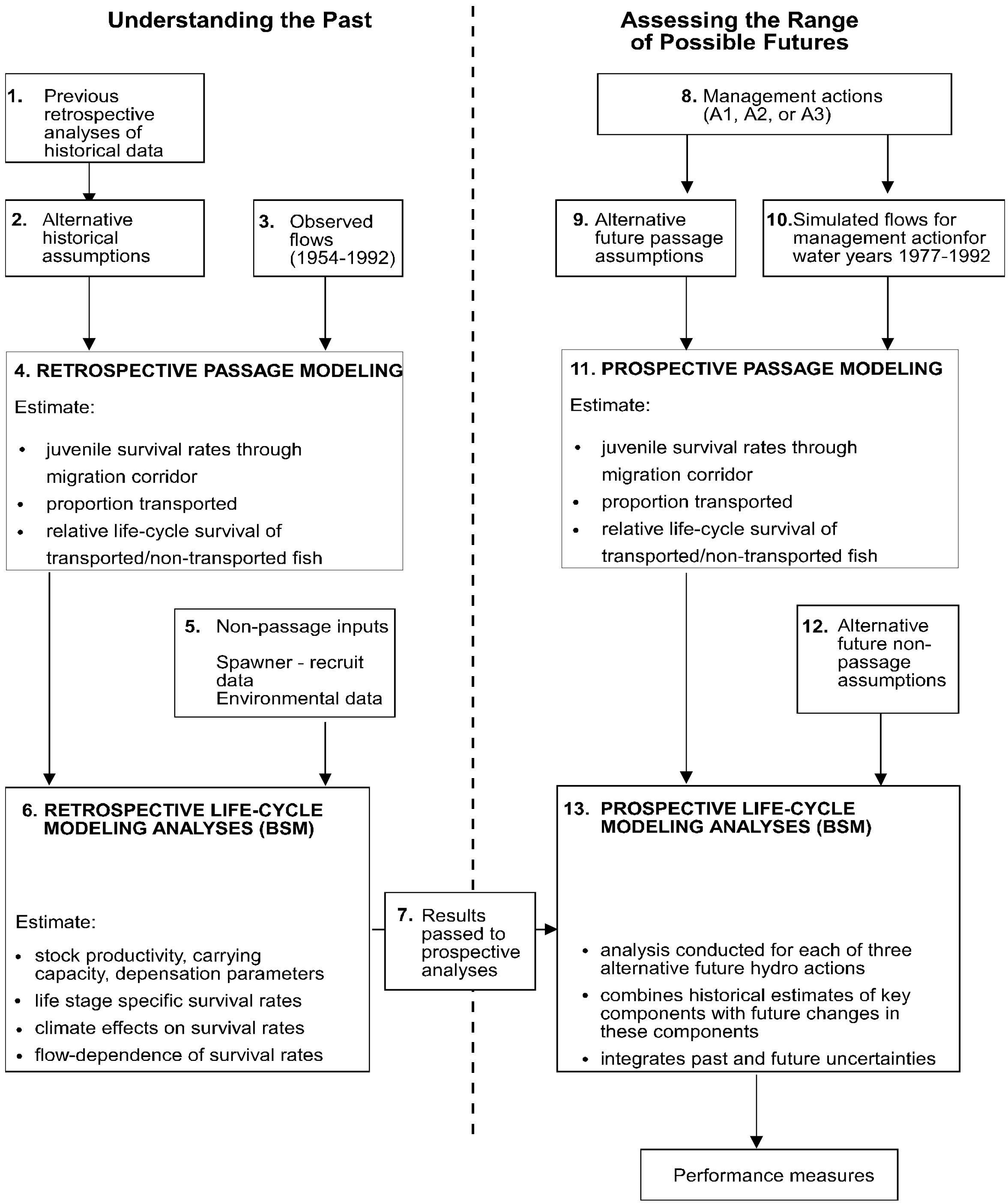


Figure 3-1: General overview of analysis. This diagram oversimplifies the actual analytical approach. More details are provided in Appendix A, section A.1.

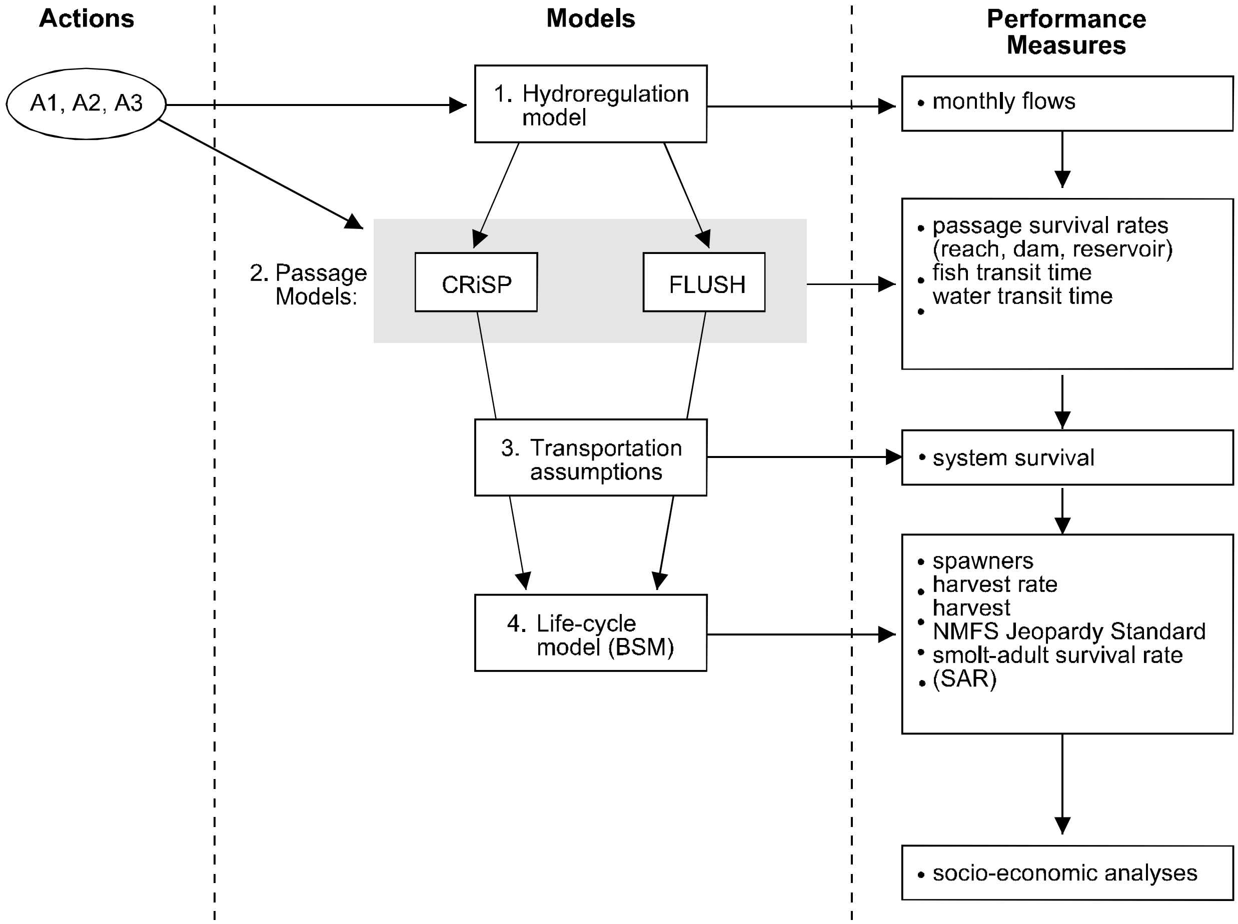


Figure 3-2: Links between the three model types and performance measures. Performance measures are explained in more detail in Section 3.2.

Because there is randomness and uncertainty in the hydrosystem and in salmon populations, as well as in future climate, we need to assess different hydrosystem actions in terms of the chances of various outcomes. To do this, we make many simulation runs, with each run based on a randomly selected value for factors such as future flows and productivity parameters. We then calculate the probability that a certain outcome occurs as the fraction of simulations that produce that outcome. For example, we might want to know the probability that the abundance of a salmon stock was going to become larger than a certain level within 50 years. If we ran 1,000 50 year simulations and the stock exceeded that level in 600 of the simulations, the estimated probability would be 0.6.

3.2 Performance Measures

The outcomes of alternative hydro management actions are evaluated in terms of various performance measures. These measures are used to rank alternative actions according to how well they meet specified management goals. A variety of performance measures have been developed to assess the biological implications of different management actions (Table 3-1). This list of performance measures was generated through discussions with the Implementation Team and analysts working on the socioeconomic analyses associated with the U.S. Army Corps Lower Snake River Feasibility Study. Because our primary goals are to determine the hydrosystem actions that should be taken to prevent extinction and lead to recovery of stocks, we focus here on the NMFS jeopardy standards that account for each of these. These standards are defined below.

Table 3-1: List of performance measures in PATH decision analysis of spring/summer chinook

1. QUANTITATIVE PERFORMANCE MEASURES	
A.	**Direct output from life cycle simulation model (BSM)**
	X Median, 10^{th} and 90^{th} percentiles of number of spawners in each simulated year for each stock [24-, 48- or 100-year simulations that sample entire range of parameter values]
	X Harvest rate (calculated for aggregate Snake River run; assumed to apply to each individual stock)); median, 10^{th} and 90^{th} percentiles of in-river and terminal harvest, by stock
	X SARs (smolt to adult return rates)[1]
	X NMFS Jeopardy Standards
	X other measures of probability of survival
B.	**Diagnostic or Intermediate (produced for verification of results and understanding influences on primary and secondary measures)**
	X estimated dam passage survival rate, estimated reservoir survival rate, reach survival rate
	X Fish Transit Time (FTT), Water Transit Time (WTT)
	X system survival[2] (survival rate through the hydro system, considering delayed mortality)
2. QUALITATIVE PERFORMANCE MEASURES	
	X To what degree is action consistent with normative river concepts (Return to the River)
	X Genetic/life history - does action concentrate total production in a few stocks, or does it improve survival over all stocks (compare changes in distn. of spawners over index stocks)

NMFS Jeopardy Standards

The way in which a specific hydrosystem action affects the chance of an individual spawning stock going extinct is difficult to estimate because it involves population behavior at low abundance, something we have little experience with. The performance measure we use to describe the possibility of extinction here is called a “Survival” standard. This was developed by the Biological Requirements Working Group (BRWG 1994), and has largely been accepted by NMFS for use in Snake River chinook salmon jeopardy determinations (the NMFS’s approach is described in Appendix D). The Survival standard is the fraction of time during many simulations that the spawning abundance of a stock is above a certain specified low threshold. The threshold level used is either 150 spawners or 300 spawners depending on the characteristics of the stock and the stream. These levels were chosen because below these levels, spawner-recruit relationships are poorly known and unpredictable changes in population behavior are likely to occur. This standard is calculated for simulations run over 24 years and simulations over 100 years.

The effect of a certain hydrosystem action on the chance of a spawning stock recovering is described by the

[1] In this document, smolt to adult return rates (SARs) are defined as the percent of smolts counted at Lower Granite Dam which survive to return to Lower Granite Dam. However, SARs can be measured from any smolt counting location (e.g., the smolt trap near the mouth of the Warm Springs River). For clarification on how SARs are computed from BSM, see Appendix A.

[2] System survival is calculated as the number of “in-river equivalent smolts” below BON divided by the population at the head of the first reservoir. The numbers of transported smolts at each collector project that survive to BON are converted into in-river equivalents by considering the relative survival of transported and non-transported fish post-BONN. See Figure 4.2-1 and Appendix A, section A.3.2.

"Recovery" standard chosen by the BRWG (see details in Appendix D). The recovery standard is the fraction of simulation runs for which the average spawner abundance over the last 8 years of a 48-year simulation is greater than a specified level. The specified level of abundance (the recovery level) is different for each stream, and is 60% of the pre-1971 brood-year average spawner counts in each stream. We used the geometric mean abundance of spawners over the last eight years as an index of escapement to reflect the skewed distribution of abundances normally observed over time.[1]

<insert Minam River figure here>

Figure 3-3: Recent trends in Minam River spawning abundance to 1991, relative to survival and recovery levels under NMFS jeopardy standards. Also shown are the 24, 48, and 100-year periods for future projections.

These descriptions are for single stocks, but the overall performance of the system under different options needs to be described in terms of how each option affects a representative sample of all listed stocks in an Evolutionarily Significant Unit (ESU). To apply these performance standards to a number of stocks, NMFS has defined an overall Jeopardy Standard which considers, among other things, these model-derived probabilities as measures of the ability of an action to prevent extinction of an endangered stock. To meet this standard, an action must result in a "high percentage" of available populations having a "high likelihood" of being above the survival threshold level and a "moderate likelihood" of being above the recovery level. "High" and "moderate" likelihoods have been informally defined as being 0.7 for survival standards, and 0.5 for recovery standards. NMFS has defined "high percentage" of stocks as 80% of the available populations. For the cases in which we are focussed on the seven Snake River index stocks, this means that for an action to be considered to have met the overall jeopardy standard, the action must result in six stocks having a probability of 0.7 or greater of being above the survival threshold and a probability of 0.5 or greater of being above the recovery threshold.

[1] We are comparing the geometric mean of simulated future escapements with the arithmetic mean of historical abundances (recovery standard). This difference in summary statistics is recognized, but we use this method because the recovery levels are generally accepted targets, and the geometric mean is an accepted summary statistic for skewed distributions.

3.3 Stocks Considered

This analysis focuses on performance measures for seven index Snake River spring-summer chinook stocks, with comparisons to the same projections for six Lower-Mid Columbia River stocks, which pass a smaller number of dams (Table 3-2). Within these two sub-regions, these thirteen stocks are the only ones for which sufficient historical data exist to develop spawner-recruit relationships, required for generating projections of future stock sizes. The next version of this report will include three Upper Columbia stocks (the Methow, Entiat and Wenatchee). Further work is required to generalize results from these stocks to all wild chinook populations of the Snake River, Lower Columbia and Upper Columbia basins.

Table 3-2: Index stocks used in this report.

River / Region	Index Stock	Brood years with Spawner-Recruit Data
Snake R.	Minam (Snake R.)	1954-1990
	Imnaha (Snake R.)	1949-1950, 1952-1990
	Bear Valley/Elk (Snake R.)	1957-1990
	Poverty Flat (Snake R.)	1957-1990
	Johnson (Snake R.)	1957-1990
	Sulphur (Snake R.)	1957-1990
	Marsh (Snake R.)	1957-1990
Low-Mid Columbia	John Day Mainstem	1959-1990
	John Day Mid Fork	1959-1990
	John Day North Fork	1959-1990
	Warm Springs	1969-1990
	Klickitat	1966-1990
	Wind River	1970-1990

3.4 Limitations of Current Performance Measures

Because the NMFS performance standards which we have used here do not directly address extinction, some qualifications are necessary. First, the jeopardy survival standard is not a probability of a population or a cohort surviving over a certain time period, but rather the probability of a spawning escapement being above a certain spawning abundance. This assumes the population does not go extinct in the time period. Interpreting the survival standard as a probability of survival can lead to apparent inconsistencies. For example, the survival standard over 100 years can be greater than the standard over 24 years under the same conditions, something which would not be possible if it were a true survival probability. A second qualification is that the recovery standard does not explicitly recognize the risks of extinction during the first 40 years (where extinction means that the population falls below a 'quasi-extinction' level that inevitably leads to extinction). This would cause the estimates of probability of recovery given by this performance standard to be biased high. This bias will probably be inconsequential if conditions are constant throughout the 48 years, but in situations where poor initial conditions are followed by good conditions during the last years of the period, the bias may be substantial. This sort of fluctuation is considered under some climate hypotheses (see Section 4.5.3).

While the performance measures we have used provide valuable information regarding the probability of extinction, it has certain drawbacks and we are working on ways of more realistically describing the risks to stocks. For example, when spawner abundances drop below 150 or 300, there is an increasing chance that the spawner-recruit relationship will change in a way that increases the probability of extinction, but such changes are not included in the current life cycle model. Another shortcoming of the current approach is that it focusses on the numbers of spawners in single years. This does not accurately reflect the risk to stocks because a population with low spawning abundance in one year could have large cohorts in the ocean ready to spawn in subsequent years. Conversely, it would be particularly serious if a stock remained below 150 spawners for several consecutive years.[1] We intend to include other performance measures in our next report.

There are other performance measures of potential value for our final report. For example, when a particular action falls short of reaching one or more jeopardy standards, it may be worthwhile to assess how much of an increase in life cycle survival is required to meet the standard. This would help to quantify the magnitude of shortfall in the proposed action.

[1] This is not a hypothetical situation. Out of the last five brood years (1991-1995), numbers of spawners for Snake River index stocks were frequently below their respective threshold levels: Bear Valley/Elk, Marsh, Sulphur, Poverty Flat, Johnson, Imnaha, and Minam populations were less than the threshold 4, 4, 3, 2, 2, 2, and 3 times respectively.

4 Uncertainties in the Response of Populations to Management Actions

> Conscientiously followed, the method of the working hypothesis is an incalculable advance upon the method of the ruling theory; but it has some serious defects. One of these takes concrete form, ... in the ease with which the hypothesis becomes a controlling idea. To avoid this grave danger, the method of multiple working hypotheses is urged. It differs from the simple working hypothesis in that it distributes the effort and divides the affections. … The investigator thus becomes the parent of a family of hypotheses; and by his parental relations to all is morally forbidden to fasten his affections unduly upon any one. … the right use of the method requires the impartial adoption of all alike into the working family. The investigator thus at the onset puts himself in cordial sympathy and in parental relations (of adoption, if not of authorship) with every hypothesis that is at all applicable to the case under investigation. Having thus neutralized so far as may be the partialities of his emotional nature, he proceeds with a certain natural and enforced erectness of mental attitude to the inquiry, knowing well that some of his intellectual children (by birth or adoption) must needs perish before maturity, but yet with the hope that several of them may survive the ordeal of crucial research, since it often proves in the end that several agencies were conjoined in the production of the phenomena. Honors must often be divided between hypotheses. …
>
> (excerpted from: T.C. Chamberlain. 1890. “The Method of Multiple Working Hypotheses”. Science 15:92. Reprinted in R. Hilborn and M. Mangel. 1997. The Ecological Detective. Confronting Models with Data. Princeton University Press. 315 pp.)

4.1 Overview

The hydrosystem management actions under consideration can affect fish populations through changes in juvenile survival past dams and through reservoirs, changes in estuarine and ocean survival, or changes in the survival of adults returning upstream. During the last two years, the PATH group has made considerable progress in clarifying the historical changes that have occurred to spring-summer chinook and standardizing the historical data sets used by models (FY96 PATH Retrospective Report and Conclusions Document*)*. Nevertheless, there do remain many uncertainties which can potentially affect the responses of fish populations to management actions. We have laid out a range of alternative hypotheses for each of these uncertainties.

In this chapter, we provide an overview of the different uncertainties and hypotheses used in the preliminary decision analysis. Further details, the biological rationale, and the mathematical representation of alternative hypotheses are described in Appendix A. Chapter 5 presents the results of our modeling analyses to assess both the range of responses of fish populations to management actions, and the relative importance of these uncertainties in determining those responses. This draft does not attempt to weigh simulation outcomes on the basis of the strength of evidence for alternative hypotheses. In the results presented in Chapter 5, each alternative hypothesis is given equal weight. We do, however, assess the sensitivity of decisions to different weights for key hypotheses. The final report will contain more complete descriptions of alternative hypotheses, provide structured evidence for and against each alternative, and, wherever feasible, assign different weights to these alternatives.

4.1.1 Alternative Futures for Spring Summer Chinook

In exploring the range of possible futures for these stocks under different management actions, we examined twelve different uncertainties, and formulated alternative hypotheses for each one (Figure 4.1-1, Table 4.1-1). Figure 4.1-1 shows the management actions on the left side of the diagram, and then a series of branches to incorporate alternative hypotheses about different components of the system. Each small circle represents a "node" in the decision tree, where a choice of alternative hypotheses is possible. We have not included all of the branches because it would make the figure too cluttered (altogether there are over 5,000 possible paths through the tree). Table 4.1-1 represents the same information as Figure 4.1-1, but in tabular form. It includes a brief description of the differences among alternative hypotheses. These alternatives are outlined in greater detail both within this chapter and in Appendix A.

The uncertainties in Figure 4.1-1 and Table 4.1-1 are of two types: uncertainty regarding the future environment, and uncertainty regarding how the system works (i.e., in the functional relationships we use to predict the future). Though the future environment may be beyond human control (e.g., future climate), it nevertheless is of potential significance in determining future population sizes. We consider our uncertainty in how the system works primarily in terms of the survival changes caused by management actions (e.g., changes in reservoir survival in response to changes in river velocity; changes in juvenile survival after natural river drawdown). Alternative hypotheses to describe how the system works often hinge on the interpretation of historical information, because the functional relationships in models are based on both general principles and historical data. Results from historical transportation experiments, for example, affect the transportation-survival relationships included in the models, which in turn are used to predict the response to a management action. Similarly, uncertainties regarding dam survival in particular past years can, in some models, affect functional relationships that relate river velocity to fish survival through reservoirs. Differences in interpretation of historical data therefore indirectly affect predictions of future states. A final note of caution is necessary with respect to historical data. While reasonable consistency with historical information is necessary to provide some minimum level of confidence in a model, the degree to which a model matches historical information does not determine how well it will perform in predicting the response to new management actions. This is because: 1) future conditions may not be the same as past ones, particularly if there are major changes made to the dams and reservoirs; and 2) the fact that a model fits past data well does not mean that the mechanisms incorporated into that model reflect those which exist in nature, so that the model may not correctly simulate the response of the system to new conditions.

As you move through the different uncertainties in Figure 4.1-1 and Table 4.1-1, the number of combinations of assumptions increases rapidly, just as the branches of a tree increase as you move up the trunk to the crown. A particular combination of hypotheses about how the system works and the future environment is a unique "prospective aggregate hypothesis", like a unique path chosen by an ant climbing from the trunk to the last branch at the top of a tree. Each prospective aggregate hypothesis potentially yields a unique future biological response. One of the objectives of the preliminary analysis reported here is to 'prune the tree', by determining which uncertainties have limited effects on performance measures and the resulting decision, so that we can focus on the most critical alternative hypotheses. We may also develop new variations or combinations of hypotheses that better reflect recent evidence. Model runs were completed for all combinations of the alternative hypotheses in Figure 4.1-1 and Table 4.1-1.

n:\davem\ew788\decana;\modelrun\document\A2treef.cdr

Figure 4.1-1: Decision tree used to incorporate alternative hypotheses in prospective analysis for spring-summer chinook. Further details on the alternative hypotheses are provided in Table 4.1-1. line is an example of a prospective aggregate hypothesis (one of 5,148 possible combinations), which is generally consistent with the retrospective aggregate hypothesis H1, described in Table 4.1-2.

Table 4.1-1: Set of uncertainties and alternative hypotheses considered in this analysis.

Uncertainty (Report section with further details)	Hypothesis Label	Description
Uncertainties / hypotheses related to downstream passage to Bonneville Dam		
In-river survival assumptions —Passage Models (4.2.1)	PMOD1	CRiSP direct survival estimates.
	PMOD2	FLUSH direct survival estimates.
Fish Guidance Efficiency (FGE) (4.2.2)	FGE1	FGE w/ESBS > FGE w/STS. (ESBS = extended length submersible bar screens). (STS = standard length submersible travel screens).
	FGE2	FGE w/ESBS = FGE w/STS.
Historical ∪ Turbine + Bypass Survival (4.2.3)	TURB1	Turbine survival = 0.9. Bypass survival = 0.97 - 0.99, depending on the project.
	TURB4 TURB 5 TURB 6	Various mechanisms for turbine/bypass survival during some historical years. Survival is lowest under TURB4, and highest under TURB5.
Predator removal efficiency (4.2.5)	PREM1	0% reduction in reservoir mortality resulting from predator removal program.
	PREM3	25% reduction in reservoir mortality.
Duration of pre-removal period under drawdown (4.2.6)	PRER1	3 years
	PRER2	8 years
Equilibrated Snake River juvenile survival rate under drawdown (4.2.6)	EJUV1	0.85
	EJUV2	0.96
Transition Period: Juvenile survival (4.2.6)	TJUVa	Survivals reach equilibrated values 2 years after dam removal.
	TJUVb	Survivals reach equilibrated values 10 years after dam removal.
Other uncertainties / alternative hypotheses		
Transportation models (section 4.3.1)	TRANS1 or T1 (FLUSH only)	Relationship established between TCR and FLUSH in-river survival, based on data from **all** transport studies conducted at LGR and LGO dams between 1971-1989. This relationship, and FLUSH in-river survival, used to estimate relative post-BONN survival of transported fish (D) in both retrospective and prospective analyses (see Fig. 4.2-1).
	TRANS2 or T2 (FLUSH only)	TCRs derived from TRANS1 adjusted by 0.83 to reflect poorer survival of transported fish from last dam to spawning grounds.

Uncertainty (Report section with further details)	Hypothesis Label	Description
	TRANS3 or T3 (CRiSP only)	For pre-1980 retrospective analyses, relative post-BONN survival set at average D-value estimated from seven T:C studies in 1970's and associated CRiSP in-river survival rate estimates. Post-1980 retrospective analyses use average D-value estimated from four T:C studies in 1980's, and CRiSP in-river survivals. For prospective analyses, D-value randomly selected from four 1980 values.
Distribution of Extra Mortality (4.3.2)	ALPHA	Extra mortality[1] is specific to each sub-region, and affected by climate variables.
	DELTA	Extra mortality is independent of the common year effects which affect several subregions.
Extra mortality / Future climate (4.3.3, 4.3.4)	EMCLIM1	Extra mortality is here to stay; future climate is sampled from historical distribution with autoregressive properties.
	EMCLIM2	Extra mortality is here to stay; future climate follows cyclical pattern.
	EMCLIM3	Extra mortality is proportional to hydropower-related mortality, future climate is sampled from historical distribution with autoregressive properties.
	EMCLIM4	Extra mortality is proportional to hydropower-related mortality, future climate follows cyclical pattern, with both long (60-year) and shorter (18-year) cycles.
	EMCLIM5	Both extra mortality and future climate follow cyclical pattern.
Habitat Effects (4.3.5)	HAB0	Same management as current.
	HABB	Implementation of all possible habitat restoration or protection.

4.1.2 Differing Explanations of Historic Declines in Spring Summer Chinook and Their Link to Future Projections

We base our future projections on our understanding of the past, much of which is summarized in the PATH FY96 Retrospective Analyses, the PATH FY96 Conclusions Document, and FY97 retrospective analyses (to be published). However, as past information is incomplete, there are differing interpretations of the relative importance of different factors in causing recent declines of Snake River spring-summer chinook. As described above, a set of alternative hypotheses about all components of the system (stock productivity, downstream migration, marine survival, etc.) is referred to as an aggregate hypothesis. We call a set of hypotheses about the future a “prospective aggregate hypothesis”, and a set of hypotheses about the past a “retrospective aggregate hypothesis”. The set of hypotheses about the past is smaller than the set of hypotheses about the future, because though there are many possible alternative futures, there is only one past.

[1] Extra mortality is any mortality that is not accounted for by either: 1) spawner-recruit relationships; 2) estimates of direct mortality within the migration corridor; or 3) for the Delta model only, common year effects affecting both Snake River and lower Columbia River stocks.

Two example retrospective aggregate hypotheses are presented in Table 4.1-2. There are many other possible hypotheses, but these two examples illustrate some key differences in our interpretation of the past which have important consequences for influencing the range of future projections. The H1 aggregate hypothesis proposes that recent declines are primarily due to the hydrosystem, while the H2 aggregate sypothesis attibutes declines primarily to non-hydro factors. The logical consequences of these two *retrospective* aggregate hypotheses are represented by subsets of the *prospective* aggregate hypotheses considered in our future projections. For example, the branches in Figure 4.1-1 involving FLUSH-T1/T2, the DELTA model, and Hydro-related extra mortality are more consistent with the H1 retrospective aggregate hypothesis in Table 4.1-1. The branches involving CRiSP-T3, the ALPHA model, and regime shift or BKD extra mortality are more consistent with the H2 retrospective aggregate hypothesis in Table 4.1-1. Note that for virtually all system components (the rows of the table) there are common areas of agreement (statements which span the H1 and H2 columns in Table 4.1-2). There are, however, some significant differences among these two retrospective aggregate hypotheses; the last column outlines why such differences exist, and what might be necessary to resolve them. The table references various figures which follow, as well as chapters of the PATH FY96 Retrospective Analyses, that help to illustrate the differences among alternative hypotheses for various components. *Readers primarily interested in the prospective analysis may wish to skim Table 4.1-2 and move directly to Section 4.3.*

Table 4.1-2: Examples of two retrospective aggregate hypotheses (H1 and H2). These aggregate hypotheses are two of the many alternative explanations of the 1970-1990 declines in Snake River spring-summer chinook stocks. Both hypotheses recognize that the relative importance of different factors has varied over time. Statements which span the columns for H1 and H2 are common to both hypotheses.

System Component [Abbreviations in Table 4.1-1, Figure 4.1-1; Chapters from FY96 Report; relevant sections in this report]	**Aggregate Hypothesis H1. Mostly Hydro** **BY1970-74: Hydro** **BY75-90: Hydro primary; climate secondary**	**Aggregate Hypothesis H2. Mostly Non-Hydro** **BY1970-74: Hydro** **BY74-90: climate primary; hydro, hatcheries, and habitat secondary**	**Key sources of differences between H1 and H2, and possible ways to resolve them.**
a. Direct Survival of In-River Fish	- poor in-river survival for brood years 1970-74 in part due to inadequate passage facilities at dams		
PMOD, TURB [Ch. 6] [[4.2.1, 4.2.3]	- fish survival rate vs. FTT[1] non-linear - cumulative effect of longer FTT leads to lower reservoir survival per day in JDD -> BONN reach than in other sections - historic dam mortality lower than in H2; reservoir mortality higher	- fish survival rate vs. FTT essentially linear - similar per project survival in JDD -> BONN projects as in other ones - historic dam mortality higher than in H1; reservoir mortality lower	- reach survival data mostly for LGR to MCN; general H1/H2 agreement there. - differences between H1 & H2 caused by lack of PIT-tag studies for lower reaches, and different interpretations of 1973 and 1979 survivals.
b. Transportation TRANS or T	- success of transportation varies both among years and within years		
[Ch. 6] [4.3.1]	- benefits are variable - transport studies from all years (1971-89) are representative of future - transported fish do much worse in the ocean than in-river fish - future D (see footnote 2) values likely to average <0.5 - transport survival not improving over time (SAR data).	- large benefit overall - only studies from 1980's and beyond are representative of future - transported fish do about as well as in-river fish in the ocean - future D values likely to be 0.8 or higher - transport survival improving over time (T:C data, descaling rates)	- no direct way to measure D - all estimates of D are indirect, based on T:C ratios[3] and estimates of in-river survival. - differences in estimated in-river survival (a) magnify differences in transportation benefits (b) - well designed tagging studies to assess the effectiveness of transport - well designed tagging studies to assess the effectiveness of transportation
c. Post-BONN mortality EM; ALPHA/DELTA [Ch. 5] [4.3.2, 4.3.3]	- mostly caused by hydrosystem during brood years (BY) 1970-1974		- not enough CWT recoveries to know if significant upstream-downstream differences exist in marine distribution; differing interpretations of this meager data; no empirical estimates of upstream-downstream differences in marine survival - climate data confirm temporal changes in conditions, but no way to estimate upstream stocks' relative resilience to climate shifts.

[1] FTT=Fish Transit Time, the time it takes smolts to travel from the head of Lower Granite pool (LGR) to the Bonneville tailrace (BONN).

[2] D=(post-BONN survival of transported fish)/(post-BONN survival of in-river fish). If D=1, post-BONN survivals of the two groups are equal. Estimates of D are affected by which T/C studies are used, and estimates of in-river survival. See Figure 4.2-1.

[3] T:C is defined in Figure 4.2-1. The T:C ratio is also referred to as TCR.

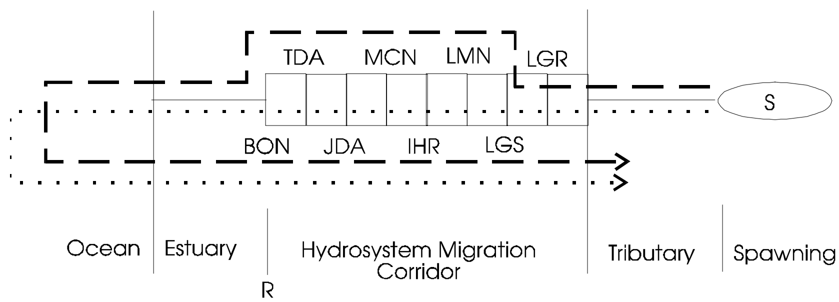
System Component [Abbreviations in Table 4.1-1, Figure 4.1-1; Chapters from FY96 Report; relevant sections in this report]	Aggregate Hypothesis H1. Mostly Hydro **BY1970-74: Hydro** **BY75-90: Hydro primary; climate secondary**	Aggregate Hypothesis H2. Mostly Non-Hydro **BY1970-74: Hydro** **BY74-90: climate primary; hydro, hatcheries, and habitat secondary**	Key sources of differences between H1 and H2, and possible ways to resolve them.
c. Post-BONN mortality (cont.) EM; ALPHA/DELTA [Ch. 5] [4.3.2, 4.3.3]	- mostly driven by hydrosystem in BY75-90, except in BY80-83 [Ch. 5] - overall effect of hydrosystem (m) indicated by differences in (R/S) between Snake R. and lower Columbia R. stocks, after accounting for common climate effects (δ) affecting both groups [Ch. 3 and 5] - post-BONN mortality assumed due to hydrosystem, and estimated from difference between overall effects (m) and direct effects (system survival[1]). - much spatial/temporal overlap of upstream/downstream stocks in estuary and near ocean environments.	- mostly climate-driven for BY75-90 (climate good for 1945-1975, poor for 1975-2005, then good for 2005-2035) [Ch. 12] - upstream-downstream differences in (R/S) caused by factors other than hydrosystem (i.e., different genetic composition, ocean distributions, marine survival, resiliency to climate shifts) - post-BONN mortality (for Snake R. stocks) assumed due to shift in climate regime, estimated from 'step-drop' in (R/S) of Snake R. stocks, and system survival. - upstream stocks distinctly different from downstream stocks.	- resiliency of upstream Snake River stocks may have been lowered by hydrosystem, confounding effects of climate and hydrosystem.
d. Climate	- climate had more negative effects on salmon after the mid-1970's		
CLIM [Ch. 12] [4.3.4]	- climate generally positive for BY 1950-1970; generally negative for BY 1974-1990 (except 1983, 84, 85, 88)	- climate strongly positive for BY1945-1974; strongly negative for BY 1975-2005. - minor 18.5-year cycles on top of major shifts can improve or worsen conditions	- climate regime-shift coincided with major transportation and hatchery programs; - difficult to demonstrate relative contribution of each factor to depressing (R/S) and Smolt-Adult return rates without an experimental change in hydrosystem, transportation, or hatcheries, out of phase with climate changes

[1] System survival = the weighted average of 1) the passage survival of non-transported smolts from the head of LGR pool to BONN tailrace; and 2) the survival of transported fish from the point of collection back to this point. The two survivals are weighted according to the fraction of fish that end up being transported.

System Component [Abbreviations in Table 4.1-1, Figure 4.1-1; Chapters from FY96 Report; relevant sections in this report]	**Aggregate Hypothesis H1. Mostly Hydro** **BY1970-74: Hydro** **BY75-90: Hydro primary; climate secondary**	**Aggregate Hypothesis H2. Mostly Non-Hydro** **BY1970-74: Hydro** **BY74-90: climate primary; hydro, hatcheries, and habitat secondary**	**Key sources of differences between H1 and H2, and possible ways to resolve them.**
e. Hatcheries	- no significant effects of separate hatchery additions in individual streams		
[Ch. 11] [4.3.6]	- effects of aggregate hatchery additions difficult to determine due to overlap in space and time with hydrosystem effects; presumed low [Ch. 11] - important to examine coincidence in timing of ocean entry of hatchery and wild fish for both upriver and downriver stocks to assess if impact is feasible	- Hatchery fish: 1) spread disease to Snake River wild fish, lowering their resilience to climate changes; and 2) compete with Snake River wild fish for food in early ocean period, when productivity lower due to shift in ocean conditions - these effects are greater on Snake River fish than lower Columbia River fish - Snake R. spring/summer chinook declined as hatchery chinook and steelhead releases increased	- changes in disease rates in Snake R. fish insufficient to explain declines without assuming greater sensitivity to disease than lower Columbia stocks - differing interpretations of comparibility of upstream and downstream stocks - impossible to disentangle historical effects of: 1) hydrosystem on early ocean productivity (i.e., blocked nutrients, changed seasonal flow patterns); 2) natural changes in ocean productivity; 3) competition from hatchery fish; 4) impact of hydrosystem on disease transmission (bypasses, barges); and 5) reduced fitness of smolts due to barge transportation - only way to test these effects would be to alter hydrosystem, shut off transportation or hatcheries in successive years, while monitoring 'control' stocks for climate changes (see (d))
f. Spawning and Rearing Habitat	- important factor pre-1975; maintaining habitat is critical to survival of stocks		- scope for increasing productivity and survival rates through habitat improvement - adaptive management experiments to improve degraded habitat
HAB [Ch. 4, 9] [4.3.5]	- not significant in determining rate of 1975-1990 decline - smolts/spawner in 1990's not significantly different from 1960's for Snake R. aggregate stock [Ch. 9] - no significant correlations between changes in land use and trends in (R/S) [Ch. 4]	- reduced resiliency of some stocks in poorer habitat (linked to (h) depensatory mortality) - degradation in some rearing, migratory corridor, estuary, and ocean habitats may have impact on stocks	
g. Harvest	- minor effects on BY 1970-1975		- spring/summer harvest recognized as less significant factor than hydro, climate, hatcheries, and habitat
[Ch. 13] [4.3.7]	- not significant after 1975	- current low harvest rates continue to have some effect	
h. Depensatory mortality and stock productivity [4.3.2]	- generally higher inherent productivities - depensation may have occurred, but not yet observed; could be increasingly important in future	- generally lower stock productivities - low spawning numbers may cause reduced size and fitness of smolts due to lack of carcasses and marine nutrients	- not enough observations at low stock sizes to show reduced (R/S) or reduced smolt size at low numbers of spawners

4.2 Uncertainties/Alternative Hypotheses Related to Downstream Passage

Models simplify reality. The parts of the salmon's life cycle represented in the passage models are shown in Figure 4.2-1. The caption to Figure 4.2-1 also defines some of the terms used to distinguish among alternative hypotheses. Abbreviations for Columbia River dams are defined in Table 4.2-1. As described in Section 4.1, key outputs from the passage models include direct survival of in-river and transported fish, the partitioning of in-river survival between dam and reservoir survival, expected transport:control ratios, and the proportion of fish transported. These outputs feed into a life-cycle model, which is described in Section 4.3.



LEGEND	
. . . .	path of a fish travelling only in-river (not transported)
	Path of a fish collected at Little Goose Dam (LGS), barged to Bonneville (BON) tailrace, and released

Figure 4.2-1: Schematic showing components of life cycle modeled and definition of terms. 'Direct Survival' of in-river juvenile fish (V_n) is survival from head of Lower Granite (LGR) pool to tailrace of Bonneville Dam., including reservoir and dam survival at each project. 'Direct Survival' of transported juvenile fish (V_t) is in-river survival from head of LGR to point of collection (LGS in the example), multiplied by bypass survival at collection project, multiplied by barge survival to BON tailrace. The passage models predict V_n and V_t. Transport:Control ratio (T/C or Φ) is the ratio of survival of transported fish survival to in-river fish survival from juveniles at collection point to adults at the same point (i.e., in example shown, from juveniles at LGS through ocean and back to adults at LGS). Recruits per spawner (R/S) is the number of adult fish returning to BON (R in Figure 4.2-1) , divided by the number of spawners in the parent generation (S). D is the ratio of survival of transported fish measured from BON tailrace through the ocean and back to the point of collection (i.e. $\lambda_{t;}$ excluding downstream migration corridor) to survival of in-river fish, measured over the same interval (λ_n). Putting all these together gives the equation:

[Eq. 4.2-1]

$$(T/C) \text{ or } \phi = [V_t/V_n] * [\lambda_t/\lambda_n] = [V_t/V_n] * D$$

Table 4.2-1: Abbreviations used for Columbia River System Dams

BON	Bonnevile
TDD	The Dalles
JDA	John Day
MCN	McNary
IHA/IHR	Ice Harbor
LMO/LMN	Lower Monumental
LGO/LGS	Little Goose
LGR	Lower Granite

4.2.1 Passage Models

PATH has used two passage models in our analyses of spring/summer chinook: CRiSP and Spring FLUSH (throughout this document, we use "FLUSH" to refer to Spring FLUSH. There is a version of FLUSH for fall chinook called Fall FLUSH). The reason for using two models is that they represent different approaches to modeling reservoir mortality, dam passage mortality and transportation mortality. These are the three main components for which different hypotheses exist within these two models, and different ways of representing these hypotheses mathematically. CRiSP simulates changes to fish populations using a more detailed, mechanistic approach, while spring FLUSH relies on a more aggregated, empirical approach. Peer reviewers have found strengths and weaknesses in both approaches (SRP, 1994, 1996).

We provide here a brief overview of the major differences in structure and hypotheses among these two models; more details are provided in Appendix A.2.1. CRiSP simulates conditions for each day of the juvenile migration season: the monthly flows generated by the hydroregulation models are interpolated to daily flows and velocities; each day's simulated group of fish moves down the river at a speed consistent with that day's velocities and other factors; the dam mortality they experience is determined by the daily proportions of fish passing through turbines, spillways or bypasses; and the reservoir mortality of fish is determined by their daily encounter rates with predators and high gas levels. FLUSH uses a more aggregated approach: the monthly flows generated by the hydroregulation models are converted into the average water velocities and fish transit times experienced by smolts over the migration season; mortalities at turbines, spillways and bypasses are calculated based on the average proportions of fish passing through these alternative routes; and reservoir mortality is based on an empirical relationship between fish transit time and reservoir survival (i.e., the longer the transit time, the poorer the survival). This empirical relationship in FLUSH is based on fits to historical data on reach survivals and assumptions about the level of turbine and bypass mortality before 1980 ('TURB' alternative hypotheses in Table 4.1-1 and Figure 4.1-1). Hypotheses with greater historical turbine and bypass mortality (e.g. TURB4) yield a weaker relationship between reservoir survival and fish travel time. The key difference in the behavior of the two models is that the survival of fish through reservoirs is more responsive to changes in fish transit times within FLUSH than within CRiSP (Figure 4.2-2). FLUSH shows lower total in-river survival than CRiSP during the 1970-1995 period (Figure 4.2-3).

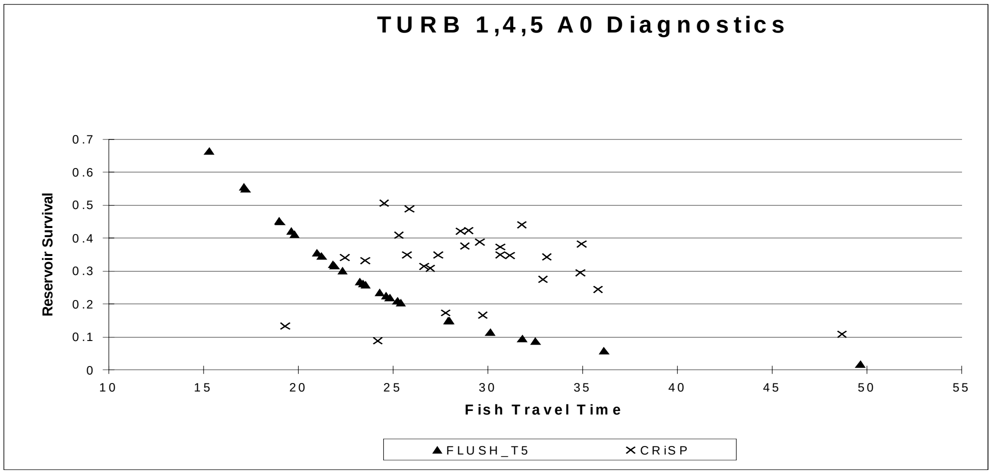


Figure 4.2-2: Model reservoir survival vs. fish travel time relationships in FLUSH and CRiSP. FLUSH example shown is for the TURB5 hypothesis.

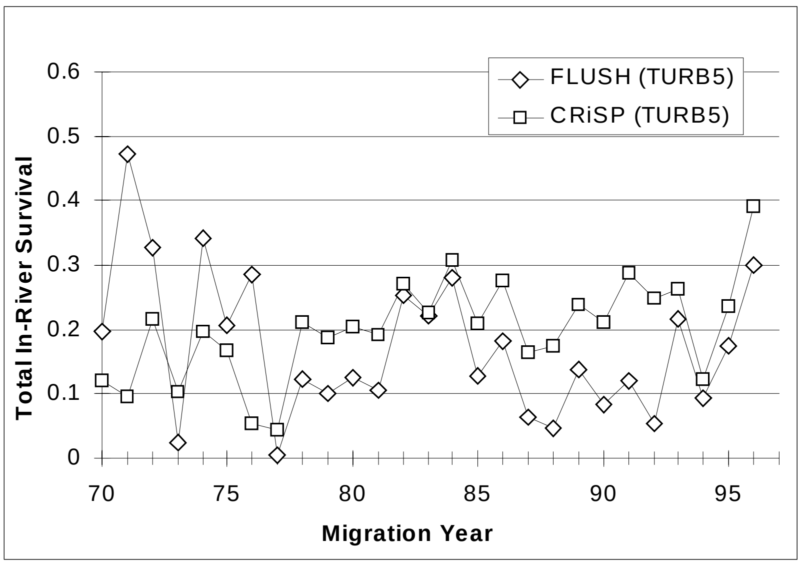


Figure 4.2-3: Comparison of CRiSP and FLUSH estimates of historical in-river survival rates from uppermost reservoir to below Bonneville Dam, not including transported fish.

4.2.2 Fish Guidance Efficiencies and Surface Collectors

Fish guidance efficiency (FGE) is typically defined as the percentage of the (juvenile) fish committed to pass through the turbine intake that, intercepted by special screens, are guided upward into the gatewell and then into a turbine bypass channel (Figure 4.2-4). However, in the past fish could only avoid turbines by swimming through a gatewell salvage system or an ice and trash sluiceway at most of the lower Snake and Columbia River dams. Fish guidance efficiency was estimated for the structural configuration in place at each project during each year of service.

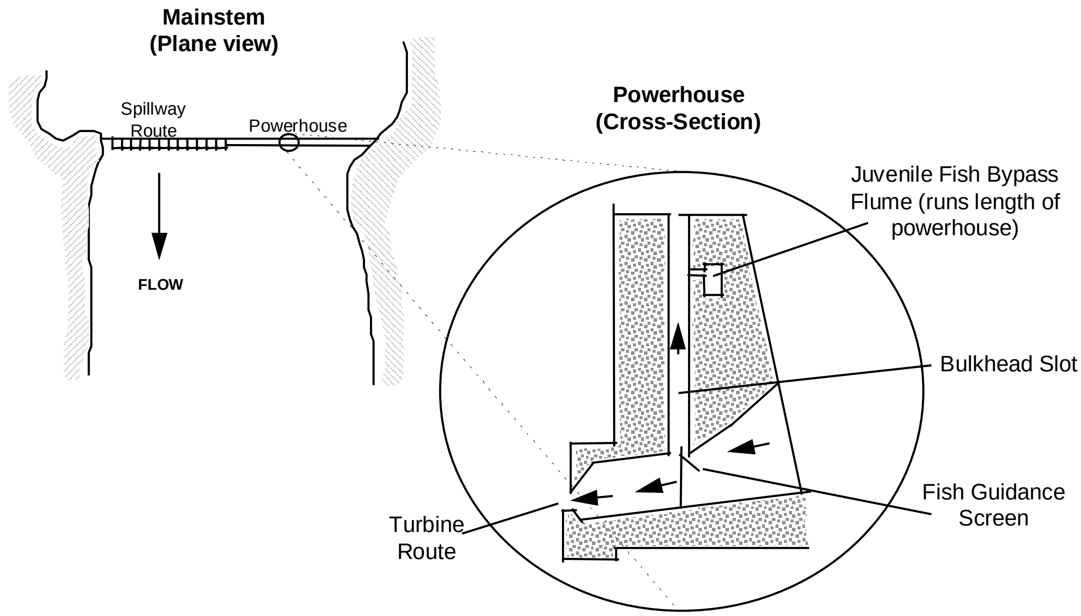


Figure 4.2-4: Spillway, juvenile bypass flume, and turbine routes of passage at a hypothetical mainstem dam.

PIT-Tag versus Fyke Net Estimates of Guidance Efficiencies

Data on fish guidance efficiencies have been obtained with fyke-net tests in the past and with PIT-tag studies in more recent years. Fyke-net estimates of FGE are thought to be biased upward and to characterize the behavior of only a limited portion of the yearling chinook run (see Appendix A, Section 4.2). Therefore, where both types of estimates were available, estimates derived from PIT-tag studies were given precedence (Table A.2.1.1.4-1)

Sensitivity Analysis for the Effect of Extended-Length Screens on FGE

Standard-length submersible traveling screens (STS) have been replaced by lowered STS or extended-length submersible bar screens (ESBS) at Lower Granite, Little Goose, and McNary Dams. Screens were extended to deflect more of the intake flow upward, with the goal of improving fish guidance into the gatewell and the bypass system. Two alternative hypotheses were derived for the effect of extended-length submersible screens on FGE, to assess the sensitivity of outcomes to FGE assumptions.

(FGE1): Assume that extended-length screens have significantly improved FGE. The results of side-by-side (fyke-net, downstream slot) tests of guidance efficiencies for yearling chinook indicate that extended-length screens guide half of the fish that are not guided by the standard-length screens.

(FGE2): Assume that extended-length submersible screens have had no effect on fish guidance efficiency. Recent PIT-tag studies with wild yearling chinook by the Idaho Department of Fish and Game indicate considerable overlap between detection rates at Lower Granite Dam during 1996 (when extended-length screens were in place) versus 1993 (standard-length submersible traveling screens in place), at similar spill levels.

4.2.3 Turbine/Bypass Survival

Standard Estimates

"Standard" estimates represent our best understanding of turbine and bypass survival under current and recent conditions. In determining the standard estimates, greatest weight was given to the most recent data, particularly those derived from PIT-tag studies.

Standard Estimate of Turbine Survival (TURB1): Turbine survival is defined as the proportion of fish surviving direct turbine passage injuries as well as any indirect mortality experienced in the tailrace by fish that passed through turbines, above the tailrace mortality experienced by fish passing through other routes (i.e., spillway or bypass). Based on a review of field studies at mid-Columbia projects and Lower Granite Dam, a value of 0.90 was adopted as the standard estimate of turbine survival. That is, of the fish that pass through turbines, 90% survive.

Standard Estimate of Bypass Survival (TURB1): Bypass survival is defined as survival past turbine intake screens, gatewells, orifices, bypass flumes, and, in some cases, dewatering screens, wet separators, sampling facilities (including holding tanks), and bypass outfall conduits. These estimates also apply to juvenile bypass through sluiceways at The Dalles, Ice Harbor, and the Bonneville Powerhouse One during certain years.

Based on a review of field studies at lower Snake and Columbia River dams, a range of 0.97-0.99 was adopted as the standard estimate of bypass survival, depending upon the bypass method at a given project in a given year.

Non-standard Estimates

Historical estimates of bypass and turbine mortality vary from current estimates for some projects during some years. There was general agreement that, between 1980 and the present, the standard estimate of turbine and bypass applied. However, there is less certainty about survival estimates prior to 1980, so several alternative hypotheses were described.

TURB4: Survival due to passage through these routes is significantly lower than would be predicted based on bypass structure alone. Turbine and bypass survivals are described by an exponential regression function relating passage mortality to the rate of descaling at 0.25, 2, or 6 days after passage.

TURB6: Some additional debris-related mortality occurred during early years but survival was higher than estimated by TURB4. Bypass survival is a function of mortality due to descaling assuming that the rate of mortality is equal to the rate of descaling. The survival of fish passing through the turbine route is the same as that described in TURB1 (i.e., 0.90 ± 0.03). However, there is no time element in the function relating mortality to descaling. All mortality is assumed to occur instantaneously.

TURB5: As described above for TURB6 but assuming that the rate of turbine mortality is equal to one-half the rate of descaling because studies indicated that a large percentage of the descaling was caused by the screens used in those years.

All three non-standard estimates of turbine and bypass survival were incorporated into both the CRiSP and FLUSH models because studies indicated that a large percentage of the descaling was caused by the screens used in those years.

4.2.4 Spill Survival and Spill Efficiency

Spill Survival

A value of 0.98 was adopted as the standard estimate of spillway survival. This estimate is conservative and represents the findings of most of the spill survival studies conducted to date. Uncertainty in this value was explored in initial passage model runs, but it was apparent from these results that different assumptions about spill survival had very little effect on the model output. Therefore, the analyses presented in this report are based solely on the standard estimate of 0.98. We are conducting further sensitivity analyses on the effects of spill efficiency assumptions.

Spill Efficiency

Spill efficiency is defined as a ratio of the proportion of the smolt population passed via the spillway (spill effectiveness) to the proportion (percent) of total flow discharged as spill. The ratio of 1:1 has been adopted as the standard assumed value, at all dams except The Dalles, based upon Steig (1994). The Dalles has an alternative configuration to other projects, with the spillway oriented perpendicular to the natural course of the river and the powerhouse oriented nearly parallel to the river, which should produce higher spill efficiency (e.g., ISG 1996). We applied an equation that set spill efficiency at 2:1 at spill proportions less than 30% of total river flow, decreasing at higher spill proportions until 1:1 is reached at 100% spill (Willis 1982, Giorgi and Stevenson 1995, and Holmberg et al. 1997).

4.2.5 Predator Smolt Removal Efficiency

Predator removal efficiency is expressed as a percent reduction in reservoir mortality. For the preliminary decision analysis, PATH explored two alternative hypotheses. Hypothesis 1 is that the predator removal program (i.e., removal of squawfish for bounties) has no effect on reservoir mortality. Hypothesis 2 states that removal of predator results in a 25% reduction in reservoir mortality. These two values were chosen to represent the extreme bounds of probable effects in the preliminary decision analysis, and follows the approach taken in the 1995 Biological Opinion. A more empirical approach to defining these bounds is described in Appendix A.2.5; this approach or one similar to it could be used in the next round of analyses.

4.2.6 Drawdown

The only drawdown scenario evaluated in the preliminary analysis was to a natural river level drawdown of all four Snake River dams. Although alternative hypotheses for drawdown scenarios were developed by the Drawdown Workgroup, the hypotheses that were actually implemented in the preliminary analysis are slightly different because of modeling constraints. Further revision and specification of drawdown hypotheses will occur after this preliminary analysis.

The group defined four time periods that were important to consider when predicting the effects of drawdown:

1. Pre-removal - the period between when the region decides to proceed with drawdown and when physical removal of dams begins.
2. Removal - period in which engineering work to breach or circumvent the dams is carried out.
3. Transition - period beginning just after the dams are removed and continuing until fish populations attain some equilibrated conditions.
4. Equilibrium - period of time from when fish populations equilibrate to the end of the simulation period.

For each period, we need to estimate: a) the duration of the period, and b) the adult and juvenile survival rates that are expected during this period. Combining these two elements gives a trajectory of adult and juvenile survival rates before, during, and after drawdown that can be used in the model to project spawner abundances over time.

Estimates of duration, juvenile survival, and adult survival for each of the four time periods are summarized in Table 4.2.6-1. Alternative hypotheses were considered for the following elements:

a) Duration of pre-removal period - to reflect uncertainty in the Congressional appropriations process and the possibility of litigation

b) Duration of transition period - to reflect uncertainty in the physical and biological responses to drawdown (e.g., response of predators, release of sediment)

c) Equilibrated juvenile survival rate - to reflect uncertainty in the long-term physical and ecological effects of drawdown (e.g., change in density of predators)

d) Equilibrated adult survival rate – to reflect uncertainty in the long-term effect on adult survival conversion rates (the current model runs include only a single, high conversion rate)

Table 4.2.6-1: Summary of estimates of duration, juvenile survival, and adult survival for each of the four time periods

Time period	Duration (years)	Juvenile survival (over the reach corresponding to 4 Snake River projects)	Adult survival (conversion rates)
Pre-removal	3 years or 8 years	determined by passage models	current estimates
Removal	2 years	no change from pre-removal period	no change from pre-removal
Transition	2 years or 10 years	linear increase from pre-removal survival to equilibrated survival	linear increase from pre-removal to equilibrated value
Equilibrium	determined by length of simulation period	0.85 or 0.96	0.97

Two examples of the juvenile survival trajectory used in the preliminary analysis are shown in Figure 4.2-5. The examples use the same equilibrated juvenile survival rate (equates to a survival rate of 0.85 over the reach corresponding to four Snake River projects) and the same three year pre-removal period, but differ in the length of the transition period between dam removal (which is completed in 2004 in this scenario) and equilibrated levels. In these examples, a regional decision is made in 1999 and removal of dams takes place between 2002 and 2004.

For our next report, we may include sensitivity analyses to explore the potential impacts if drawdown were to concentrate predators during the initial part of the transition period.

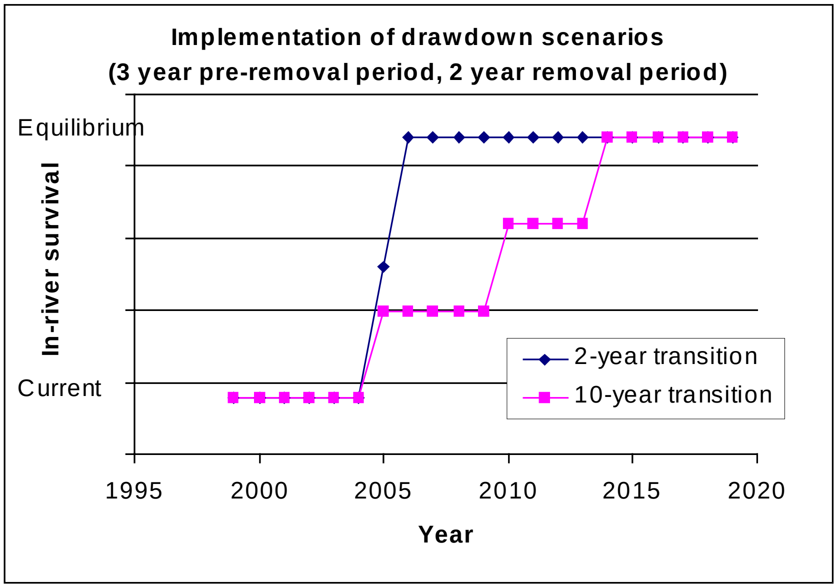


Figure 4.2-5: Example trajectories of juvenile survival following drawdown

4.3 Other Uncertainties/Alternative Hypotheses

There are six system components outside of the downstream passage portion of the life-cycle which can have alternative hypotheses:

- transportation (the processes occurring after the end of the migration corridor);
- stock productivity;
- extra mortality occurring beyond Bonneville (for both in-river and transported fish);
- future of climate conditions;
- the effects of changes in habitat management; and
- the effects of changes in hatchery operations.

Figure 4.3-1 shows the general model structures that are used to represent alternative hypotheses for both passage and non-passage components of the life-cycle. The recruitment of the fish population (R) depends on four components:

1. the basic productivity of the stock, represented by a stock recruitment relationship;
2. the system survival, which includes both the direct survival of in-river fish and the survival of transported fish, converted into in-river equivalents (see Appendix A, Section A.3.2 for more

details);

3. the post-Bonneville survival of non-transported fish; and
4. future climate changes.

Figure 4.3-1 shows how alternative hypotheses can be included for each of these four components. The basic recruitment of the stock depends on the number of spawners present (S), the parameters that define the productivity and carrying capacity of the stock (a and b), and a parameter which affects how productive the stock is at very low numbers of spawners (p). The system survival is determined by three factors: 1) the direct mortality (M, estimated by either CRiSP or FLUSH); 2) D, the ratio of post-Bonneville survivals of transported and non-transported fish; and 3) P, the fraction of smolts at Bonneville which were transported. These terms are described in Figure 4.2-1. The post-Bonneville survival of non-transported fish is affected by whether extra mortality is considered to be unique to each region (ALPHA model), or considered to have common temporal patterns in both Snake River and lower Columbia River stocks (DELTA model). The extra mortality is represented either as a function of changes in hydro system survival, changing ocean regimes, or as something completely independent of hydrosystem actions or climate. The fourth factor, shorter term climate fluctuations, can be represented either randomly or in cycles. Each of these alternative hypotheses for components of the salmon's life history are explained in more detail below.

Future projections of salmon populations are based partly on historical calibrations to spawner recruit data (Box 6 in Figure 3-1). In fitting a basic equation shown in Figure 4.3-1 to historical data, whenever one factor shows a smaller effect (e.g., a higher system survival), one or more of the other factors must "take up the slack" by having lower survival, so that the historical declines of salmon are accurately simulated. Therefore, an alternative hypothesis which postulated higher system survival, would need to also propose either lower stock productivity, lower post-Bonneville survival, and / or lower survival through short-term climate fluctuations in order to generate the observed year-to-year changes in recruitment.

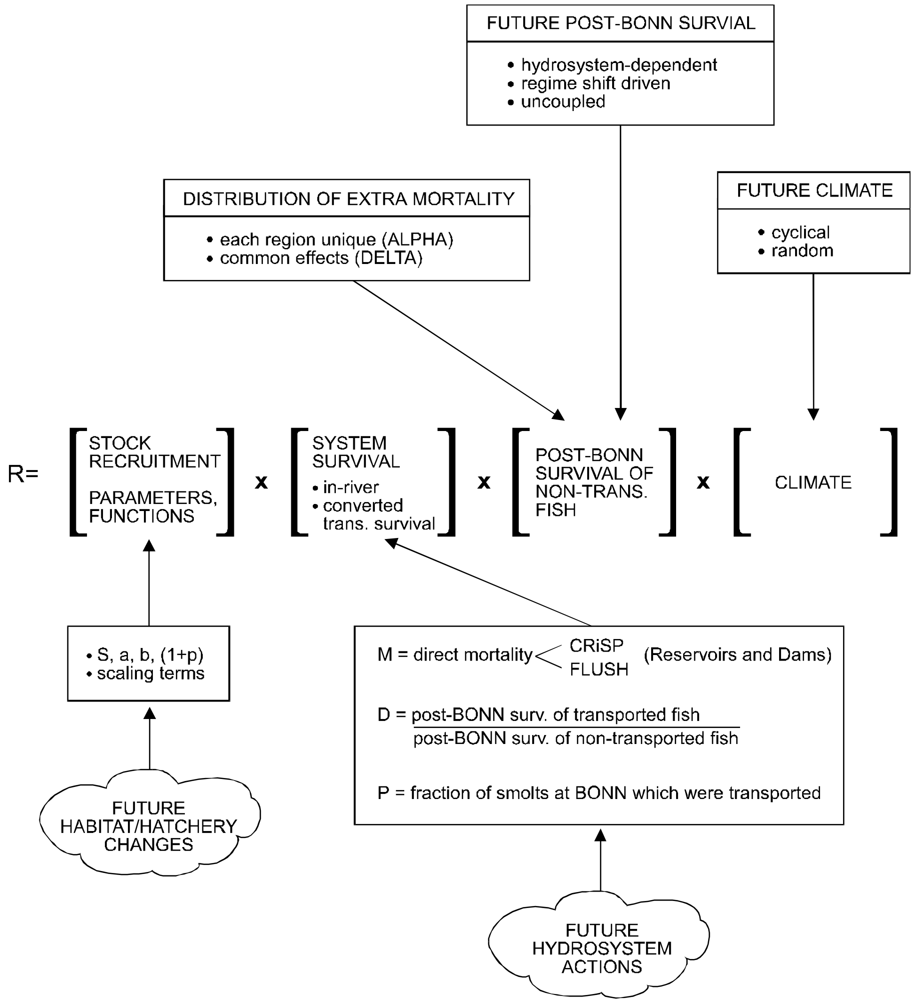


Figure 4.3-1: General structure of the life-cycle model used to integrate alternative hypotheses for all components. Further management actions shown in clouds at bottom. Changes to the schedule of in-river harvests (the only harvest for spring / summer chinoo) do not affect survival of recruits to mouth of Columbia River (R), but do affect number of returning spawners (S) Changes to hatchery operations are not currently modeled.

4.3.1 Transportation Assumptions

Transportation Rules

The proportion of fish transported in the prospective models is determined by the fish guidance efficiencies used in the passage models and the rules for spill and collection under various flows. For scenario **A1**, these rules are based on the seasonal average (April 10-June 20) flows; one of three cases is applied depending on the amount of flow relative to thresholds of 85 kcfs and 100 kcfs (see Section A.3.1 for details). In scenario **A2,** all smolts collected at LGR\LGO\LMO, and MCN Dams are transported (see Table 4.2-1 for abbreviations). There is no voluntary spill at collector projects. In scenario **A3,** no smolts are transported.

Relative Survival of Transported Fish After Bonneville Dam

Uncertainties related to transportation are focussed not on direct survival in the barge (V_t), which is acknowledged to be relatively high (around 98%), but on the relative survival of transported and non-transported juvenile fish between the time they are released below Bonneville Dam and the time they return to Bonneville Dam as adults (Figure 4.2-1).

The alternative “TRANS” hypotheses(i.e. T1, T2, and T3) considered here are different ways to use the available T:C studies to estimate the relative survival of transported fish after Bonneville Dam (“D”; defined in Figure 4.2-1). These alternative hypotheses have been implemented only partially in our current results (i.e., T1 and T2, are used only in combination with FLUSH estimates of in-river survival; T3 is only combined with CRiSP estimates of in-river survival). We intend to conduct sensitivity analyses which explore other combinations (i.e., T1 and T2 with CRiSP; T3 with FLUSH) to understand the importance of transportation hypotheses relative to passage model selection in affecting performance measures.

The effect of these alternative hypotheses is shown in Figure 4.3-2. Because of the importance of this factor, we provide more details in this section of the report below.

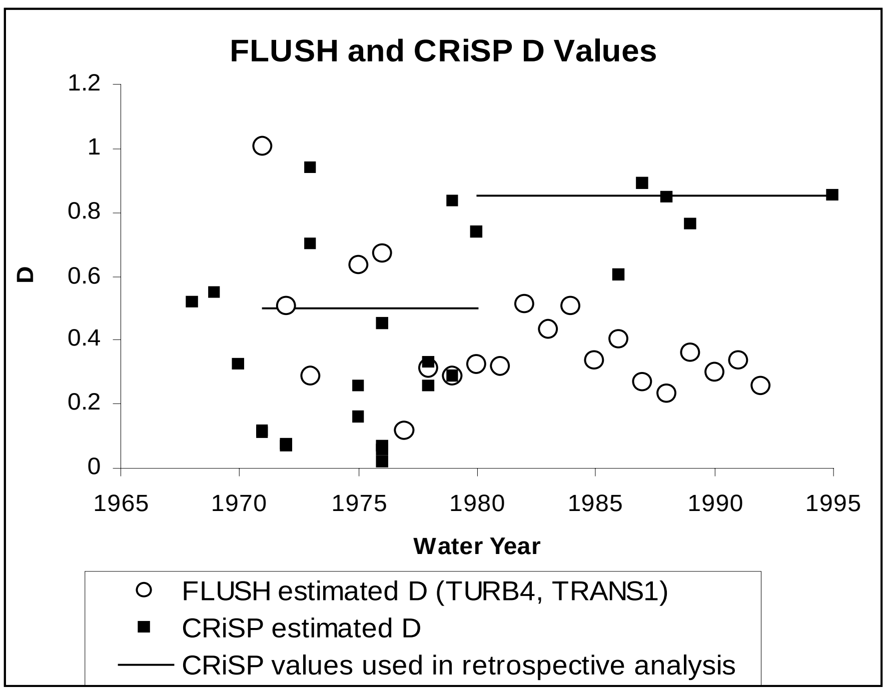


Figure 4.3-2: Comparison of D values estimated from T:C studies and passage models. D is the ratio: (post-BONN survival of transported fish) / (post-BONN survival of non-transported fish). See Figure 4.2-1.

T1and T2 (implemented only with FLUSH)

The ratio of post-Bonneville survivals of transported (8_t) and non transported (8_n) fish is represented by

$$D = (8_t / 8_n) \qquad \text{[Eq. 4.3.1-1]}$$

In T1, retrospective values for *D* are computed for brood year *y* by:

$$D_y = (T/C)_{est} * V_{n,y} / V_{t,y} \qquad \text{[Eq. 4.3.1-2]}$$

where V_n is the estimated survival from the head of Lower Granite to below Bonneville, for non-transported fish. $(T/C)_{est}$ is the T:C ratio estimated from a function in which $(T/C)_{est}$ is inversely related to V_n, and always >1 (see Table 4.3-1, Appendix A.3.1). This function is based on data from all available transport studies conducted at Lower Granite (LGR) and Little Goose (LGO) dams between 1971-1989. A second transport model (T2) adjusts the T/C estimates of transport model 1 by a factor of 0.83 to account for possible differences in T:C values measured at mainstem dams (where most adult recoveries take place) and natal areas (spawning grounds and hatcheries). Under both T1 and T2, prospective values for (T/C) are computed from the (T/C) vs. in-river survival (V_n) function, and the V_n for the simulated future year.

Table 4.3-1: D values estimated by FLUSH.

Water Year	FLUSH D
1971	1.00
1972	0.504
1973	0.285
1975	0.637
1976	0.671
1977	0.116
1978	0.309
1979	0.288
1980	0.321
1981	0.316
1982	0.511
1983	0.434
1984	0.503
1985	0.336
1986	0.402
1987	0.266
1988	0.233
1989	0.357
1990	0.297
1991	0.336
1992	0.254
pre-1980 average	0.476
1980-1992 average	0.351

T3 (implemented only with CRiSP)

In T3, retrospective *D* values were computed from the measured (T/C) for each year with a transportation study (Table 4.3-2):

$$D_y = (T/C)_y * V_{n,y} / V_{t,y} \qquad \text{[Eq. 4.3.1-3]}$$

A D_y value of 0.5 was used for years before 1980, and a value of 0.85 was used for years after 1980. Note that Equation [4.3.1-3] is like Equation [4.3.1-2] except that measured (T/C) values are used instead of estimated ones. For prospective runs, T3 uses:

$$(T/C)_{future} = D_{random} * V_t / V_n \qquad \text{[Eq. 4.3.1-4]}$$

where D_{random} is a randomly selected D_y value from the set of retrospective estimates after 1980.

The CRiSP team is now considering fitting a regression line through D_y to reflect gradual improvements in D_y since 1975, and relating D to estimates of descaling.

Table 4.3-2: CriSP-T3 estimates of D.

Water Year	CRiSP D
1968	0.518
1969	0.548
1970	0.320
1971	0.109
1971	0.118
1972	0.070
1972	0.073
1973	0.702
1973	0.938
1975	0.157
1975	0.254
1976	0.069
1976	0.452
1976	0.018
1976	0.052
1978	0.257
1978	0.326
1979	0.287
1979	0.836 (a)
1980	0.737 (a)
1986	0.603
1987	0.892 (a)
1988	0.847 (a)
1989	0.760
1995	0.852
pre-1980 average	0.321
1980-1995 average	0.782

(a) Estimates based on transport studies at McNary Dam.

It can be seen from the above equations that (T/C) is inversely related to V_n in all the three TRANS hypotheses. In other words, years with lower in-river survival have a higher T:C ratio (and vice versa). Differences in future (T/C) values are related to:

1. Differences between CRiSP and FLUSH in retrospective values for V_n, which affect the estimated historical D values;
2. Differences in the set of years of T/C information used to estimate future T/C's and corresponding *D*'s (i.e., either 1971-1989 (T1 and T2), or just the post-1980 years with transportation studies (T3); and
3. Differences in the smoothing procedure used (i.e., smoothing (T/C) estimates (T1 and T2) or smoothing (averaging or regressing) the *D* estimates (T3).

4.3.2 Stock Productivity

Productivity of spring/summer chinook populations (i.e., the number of recruits per spawner at all spawner densities except very low ones) is quantified based on a generalized Ricker spawner-recruitment model. In some salmon populations, the number of recruits per spawner declines as spawner abundance declines, which is called **depensatory**. The potential for this behavior is of concern because it allows populations to go extinct more rapidly than otherwise expected. The unknown relationship between spawners and resultant recruits at low spawner abundance is a significant source of uncertainty in productivity. This relationship is poorly understood because we have little experience with populations at these low abundances. Though there is no clear evidence for depensatory behavior in these stocks, it is difficult to detect such patterns in data with ageing errors. We account for the possibility of depensatory behavior by including it as a source of uncertainty in the relationship between spawners and recruits, but only insofar as it was evident in the data up to brood year 1990. These data do not show significant evidence of depensation, but more recent brood years (with lower escapements) have not yet been incorporated into the analysis. Further sensitivity analyses to depensation assumptions will be presented in our final report.

The Ricker spawner-recruitment model estimates sub-basin specific rates of intrinsic productivity and population carrying capacity. The prospective analyses include a stochastic relationship between spawners and resultant recruits and admission of uncertainty about fundamental parameters governing modeled productivity. Alternative hypotheses (Section 4.3.5) consider potential changes in each stock's productivity.

There are two alternative representations of chinook population dynamics (the Alpha model and the Delta model). The Alpha and Delta models are described fully in Appendix A.3.2. Briefly, the Delta model is an extension of the model used in Chapter 5 of the PATH FY96 Retrospective Analysis (Deriso et al. 1996). Deriso et al. used spawner-recruit data from Snake River and lower Columbia River stocks to infer both common-year effects due to climate affecting all stocks, and a combined 'direct plus extra' mortality. The prospective Delta model partitions out the direct and extra mortality components by using a passage model for the direct component, but keeps the common year effects as a separate term.

That is, climate effects are separated from both direct and extra mortality. The Alpha model also uses a passage model for the direct component, but does not estimate common-year effects based on similarities between Snake River and lower Columbia River stocks. Rather, the Alpha model treats each stock group independently, with an extra mortality specific to each group that includes both climate effects and any delayed effects of the hydrosystem. Thus climate effects are part of the extra mortality.

The Alpha and Delta models produce comparable rates of average productivity for the 1951-1990 brood years examined (Figure 4.3-3). There were differences, however, between these models for estimates of maximum potential productivity. Maximum potential productivity values are generally larger in the Delta model. However, when adjustments are made to the productivity measure to account for model-specific estimates of post-Bonneville "extra mortality" for non-transported smolts, no single model produces consistently higher estimates of adjusted productivity.

In the final report, we will also compute probabilities of quasi-extinction (see Chapter 3) of this report. Quasi-extinction is simply an abundance of spawners which, for modeling purposes, you assume is equivalent to extinction. One records the number of simulations where this event occurs, along with 'the first crossing time' (i.e. number of years from the start of the simulation until the event occurs).

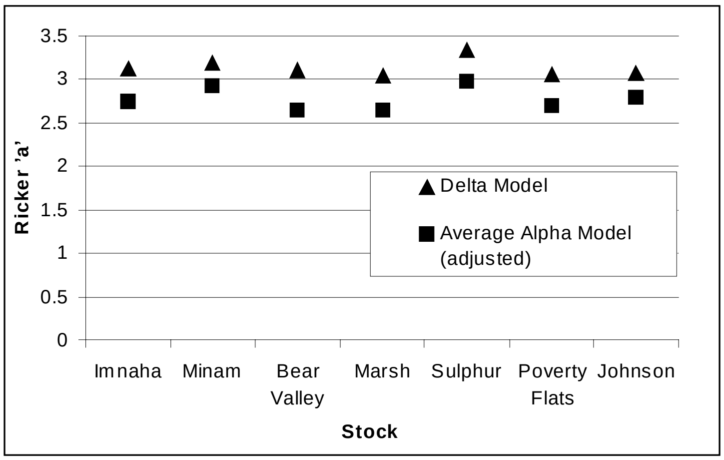


Figure 4.3-3: Comparison of stock productivities as estimated by the Delta and Alpha prospective models.

4.3.3 Extra (Post-BONN) Mortality

Extra mortality is mortality occurring outside of the migration corridor that is not captured by the inherent stock-recruitment parameters for a given stock (i.e., productivity, depensation, and carrying capacity terms). Three alternative hypotheses for extra mortality were considered.

a) Hydro-related

The completion of the Federal Columbia River Power System in the late 1960s through the mid-1970s and its subsequent operation, have increased the direct and delayed mortality of juvenile migrants, resulting in considerably sharper declines in survival rates of Snake River spring and summer chinook stocks (over the same time period), than of similar stocks which migrate past fewer dams and are not transported. This hypothesis follows from Conclusion 3a.2 of the PATH FY96 Conclusions Document:

> *We are reasonably confident that the aggregate effects of the hydrosystem have contributed to reduced survival rates of Snake River stocks (from spawners to adults returning to the mouth of the Columbia River), during the post-1974 period, as compared to the pre-1970 period. Hydrosystem effects include both direct (e.g., turbine mortality) and indirect effects (e.g.,*

delayed mortality, due to such mechanisms as changes in estuary arrival times).

b) "BKD" or Stock Viability Hypothesis

The hypothesis proposes that: 1) the viability of Snake River stocks declined as a direct or indirect result of the hydrosystem construction in the 1970s; 2) current extra mortality is not related to either the hydropower system or climate conditions; and 3) extra mortality is here to stay, even if hydrosystem direct mortality is reduced and / or the climate improves. One hypothesis to account for decreased stock viability is that hatchery programs implemented after construction of the Snake River dams increased either the incidence in the level of bacterial kidney disease (BKD) within the wild population or its severity. In both cases, the mortality increased in juvenile fish after they exited the hydropower system as compared to earlier years (or as compared to downstream stocks for the same time period). Under this hypothesis, it is unlikely that the increased rate of mortality from BKD would change back to a more favorable condition in the near future. Another stock viability hypothesis is that low stock sizes have led to increased predation rates on juveniles, and insufficient nutrients from returning adults' carcasses to support the growth of parr.

BKD is only one possible means by which stock viability may have been reduced. Occasional changes in underlying stock viability may cause some or all of the delayed mortality to remain, even if direct mortality is reduced. The consequence of falling into this category (i.e., "delayed mortality is here to stay") is that it is unknown when or if the impacts will switch back to a less benign state. For modeling purposes, we consider this the worst case, which is that these factors will stay in the present less favorable state.

c) "Regime shift" Hypothesis

Extra mortality is not related to the hydropower system, but is due instead to an interaction with a long term cyclical climate regime shift with a period of 60 years. This regime is believed to have shifted from good to poor during brood year 1975, and is expected to return to above average conditions in 2005. The signatures of a recurring pattern of interdecadal climate variability are widespread and detectable in a variety of Pacific basin climate and ecological systems. These cyclical changes affect ocean temperatures and currents which affect distributions of predators and prey; and broad scale weather patterns over land masses which then affect temperatures, rainfall, snowpacks, and subsequent flows. The changes in conditions could affect various stocks to different degrees with the effect on Snake River stocks being systematically different from lower river stocks. There is nothing that we can do to change these patterns, but they are expected over time to provide more favorable and less favorable conditions to species located in different areas.

4.3.4 Future Climate Conditions

Since climate is a factor that is beyond human control, modeling future climate conditions is necessary to explore the sensitivity of the performance of alternative management actions to good and bad climate scenarios.

Three climate hypotheses were considered in the preliminary analysis (details on the mathematical implementation of these hypotheses are included in Section A.3.4). One hypothesis assumes that climate patterns in the Northeast Pacific follow a cyclical pattern, with changes between good (i.e., increase in productivity) and bad (decrease in productivity) ocean conditions occurring on an 18.5 year cycle. This hypothesis is based on observed historical patterns in various indices of ocean productivity; based on these patterns the last change from good to bad conditions is assumed to have occurred around 1995 in one prospective model (Alpha model; see Section A.3.2) and 1991 in the other prospective model (Delta model), while the next change from bad to good conditions is expected in approximately 2005 in the Alpha model and 2001 in the Delta model. Another hypothesis does not explicitly model an underlying decadal trend. Instead, it samples from estimated climate effects during the period 1950 to 1995, a period which includes both good and bad climatic conditions as measured by various ocean productivity indices. Sampling from this period is done such that good years tend to follow good years and bad years tend to follow bad years (this method is called 'first order autocorellation'). Modeling climate in this way does tend to produce variable cyclical patterns even though no explicit assumptions are made about underlying decadal-scale trends. Finally, a larger-scale climate hypothesis involves a 60-year cycle; this climatic pattern is associated with the "Regime shift" hypothesis for extra mortality discussed in the previous section, and may be superimposed on the shorter period cycles described above. These methods are explained in more detail in Appendix A, section A.3.3 and A.3.4.

The aggregate effects of the FLUSH-T1/T2 passage and transportation models, and the Delta approach to climate effects are shown in Figure 4.3-3 for the historical period. Good climate years help to reduce overall mortality, while poor climate years (most years in Figure 4.3-3) increase it.

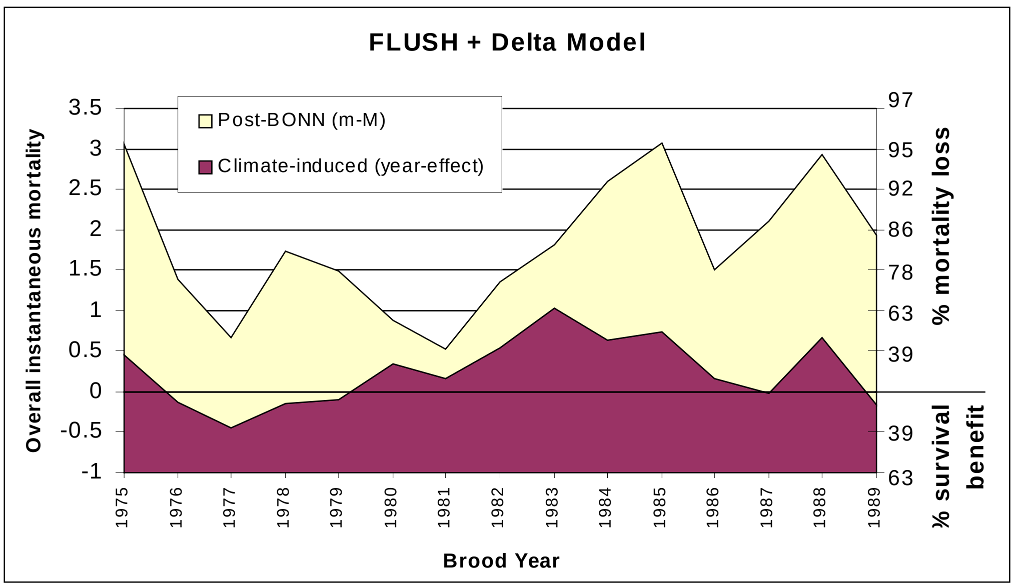


Figure 4.3-4: Comparison of historical estimates of post-Bonneville mortality and climate-related mortality as estimated by FLUSH-T1/T2 passage and Delta prospective models. Years with good climate have less dark area (climate-induced mortality).

< not ready for this draft >

Figure 4.3-5: Comparison of historical estimates of post-Bonneville mortality and climate-related mortality as estimated by CRiSP-T3 passage -transportation assumptions and Alpha prospective models.

4.3.5 Habitat

Future changes to the freshwater spawning and rearing habitats of chinook salmon may have an important influence on stock recovery. While few would disagree that freshwater habitat can be a critical limiting factor or that changes in land use can affect habitat quality and survival, it has not been possible to statistically demonstrate the effects of any given set of habitat conditions or indicators on stock productivity (Appendix A3.5). Instead we have relied upon expert judgments of plausible changes in stock productivity due to habitat management and rehabilitation. The purpose of the habitat approach is to look at how these various assumptions about future spawning and rearing habitat conditions would affect the results of the various hydrosystem management actions. This analysis does not consider the impacts of changes in fish habitat other than for the spawning and rearing part of the life cycle.

Habitat effects were defined in terms of changes to the Ricker *a* parameter, which is a measure of stock productivity at low levels of abundance and should reflect habitat quality. Plausible changes in *a* for each population within a 48-year period (the NMFS recovery time period) were defined as plus or minus 1 unit which is approximately equivalent to a three-fold change in stock productivity (recruits per spawner) or egg-to-smolt survival rates. This range is based on observed ranges of: 1) *a*-values; 2) PIT tag recovery rates; and 3) subbasin planning model smolt production assumptions. To avoid unreasonably high predicted *a* values for some stocks the maximum upward change was constrained to not exceed the highest observed value among all stocks, which was assumed to represent an upper limit for the intrinsic productivity of stocks in an area as determined by physiography and climate.

Using expert judgment we defined the probabilities of: 1) no change; 2) an increase; or 3) a decrease in Ricker *a* values for each population relative to their values estimated from 1952-1990 spawner-recruit data. We developed probability tables for two habitat management options (Table A.3.5-1): (A) status quo habitat management (in which some Ricker *a* values can increase or decrease) and (B) maximum practical effort with respect to habitat protection and restoration (i.e., greater chance of increased *a*-values, and lower chance of declines). The results presented in Section 5 contrast option B to a scenario in which Ricker *a* values did not change from their estimated values. The probability judgments were based on qualitative descriptions of habitat conditions currently affecting each population and habitat group's assessment of the potential for improvement or degradation. Productivity in a relatively pristine habitat was generally deemed unlikely to increase, so a low probability was assigned to the increased *a*-value and a high probability assigned to the no-change option. Conversely, probabilities of increased productivity (higher *a*-value) were greater in habitats degraded by past land use practices. Expert opinion was also used to judge how rapidly changes are likely to occur by assigning probabilities that changes to the higher or lower value would occur by simulation years 12 and 24 given that the change was expected by year 48. The results of the expert elicitation are described in more detail in Appendix A, Section A.3.5.

4.3.6 Hatcheries

As a response to Snake River hydrosystem development, hatchery releases of anadromous fish have increased, both from traditional hatcheries and subbasin supplementation operations (Table A.3.6-1). There are a number of reasons to believe that these releases may have a negative effect on ESA stock survival. These include transmission of disease, increased competition for food and other resources, and increasing available prey for subbasin and mainstem predators (see Chapter 11 of the FY1996 PATH Retrospective Report). These effects could manifest themselves either for stock groups as a whole (e.g., for all Snake index stocks) or at the level of individual index stocks where supplementation releases have occurred. On the other hand, supplementation proponents believe that releases have positive demographic effects by enhancing abundance of returning adults to seed future generations. Although many studies of hatchery-wild salmonid interaction have been conducted, only a handful of studies actually address the effects of hatchery releases on wild stock spawner-recruit survival. Some of these were summarized in Chapter 11 of the PATH FY96 Retrospective Report. We intend to consider hatchery hypotheses in our next report.

4.3.7 Harvest and Upstream Passage

The primary harvest of Snake River spring and summer chinook occurs within the Columbia River, very little of the harvest on these stocks occurs in ocean fisheries. Harvest levels have been significantly restricted since the late 1970s to below 15%. Currently, mainstem harvest of Columbia River spring and summer chinook stocks are managed according to schedules relating annual allowable harvest rates to estimated return levels. The current harvest schedules explicitly address harvest within the range of run strengths observed in recent years and are in place through 1998 (harvest BioOp agreement reference???). For the purposes of prospective modeling exercises, the harvest rate schedule was expanded to cover higher run sizes and tributary harvest. The rules applied at higher run levels reflect the rules included in the Columbia River Fish Management Plan (CRFMP) and historical fishing levels in the tributaries. The allocation of harvest between mainstem and tributaries resulting from application of these rules is essentially arbitrary. In practice, a different mix of the total allowable harvest rate between mainstem and tributary may occur for any indicator stock in any particular year. However, the total harvest rate impacts reflect the intent of the CRFMP. Harvest policies will be reviewed as part of the U.S. vs. Oregon negotiation process for application to 1999 and beyond.

Mainstem harvest rates on spring and summer chinook respectively are determined by the expected Snake River wild returns using harvest rate schedules. The indicator stocks comprise a portion of the total production from Snake River tributaries. For application within the modeling exercise, the run strength parameters determining the applicable harvest rate have been transformed from aggregate Snake River run size into the percent of escapement needed to achieve MSP (escapement level needed to achieve **m**aximum **s**ustained **p**roduction). Transformed harvest schedules for use in the prospective analyses are provided in the following two tables. For any given year in the prospective analysis, the appropriate mainstem harvest rate is obtained through a two step process. The run size projections to the Columbia River mouth are summed across indicator stocks and that total is expressed as a percentage of the sum of the MSP's for those same stocks. A mainstem harvest rate corresponding to that %MSP is applied to each of the individual indicator runs in the simulation. A tributary harvest rate corresponding to the average %MSP is subsequently applied to each of the indicator stocks. A primary assumption of this approach is that variability between actual run strength and the estimated run size used to set harvest rates within a particular year does not have a significant effect on harvest level.

Alternative harvest schedules were developed for the purpose of assessing the impact of variations in the harvest schedule within the prospective analysis (Tables 4.3.7-1 and 4.3.7-2). Given the starting population levels for the indicator stocks, variations in harvest rates at the low to moderate run strengths are the most likely to effect rebuilding. A more conservative harvest schedule was generated by dividing the harvest rates for run strengths below MSP by 1.5. A higher allowable harvest rate alternative was developed by multiplying the same set of rates by 1.5. This approach was applied to both the spring and the summer chinook harvest schedules.

Table 4.3.7-1: Upriver Spring chinook CRFMP harvest rate schedule to be implemented in BSM (Bayesian Simulation Model used to simulate overall life cycle changes)

	Existing Harvest Management		Conservative Harvest Management		Liberalized Harvest Management	
Run Size % of MSP /a /b	C.R. Mainstem Harvest Rate	Tributary Harvest Rate	C.R. Mainstem Harvest Rate	Tributary Harvest Rate	C.R. Mainstem Harvest Rate	Tributary Harvest Rate
< 22%	0.055	0	0.037	0	0.083	0
22%-44%	0.082	0	0.055	0	0.123	0
45%-112%	0.14	0	0.093	0	0.210	0
113%-125%	0.25	0.05	0.25	0.05	0.25	0.05
126%-175%	0.3	0.15	0.3	0.15	0.3	0.15
176%-200%	0.35	0.2	0.35	0.2	0.35	0.2
>200%	0.4	0.25	0.4	0.25	0.4	0.25

a/ run size adjusted for 77-90 average adult passage conversion and 90% prespawning survival
b/ average % of MSP for index stocks

Table 4.3.7-2: Upriver Summer chinook CRFMP harvest rate schedule to be implemented in BSM

	Existing Harvest Management		Conservative Harvest Management		Liberalized Harvest Management	
Run Size % of MSP /a /b	C.R. Mainstem Harvest Rate	Tributary Harvest Rate	C.R. Mainstem Harvest Rate	Tributary Harvest Rate	C.R. Mainstem Harvest Rate	Tributary Harvest Rate
< 25%	0.02	0	0.013	0	0.03	0
25%-49%	0.05	0	0.033	0	0.08	0
50%-99%	0.1	0	0.067	0	0.15	0
100%-129%	0.15	0	0.15	0	0.15	0
130%-149%	0.2	0.05	0.2	0.05	0.2	0.05
150%-169%	0.25	0.1	0.25	0.1	0.25	0.1
170%-200%	0.3	0.2	0.3	0.2	0.3	0.2
>200%	0.35	0.25	0.35	0.25	0.35	0.25

a/ run size adjusted for 77-90 average adult passage conversion and 90% prespawning survival

4.3.8 Other Factors Not Modeled

Any analysis must focus on a particular body or type of information and cannot account for all factors that may bear on decisions. In particular, a quantitative analysis such as this focuses on those issues that can be quantified on the basis of existing information. These missing factors need to be recognized to place the results of the biological analyses in the context of all scientific, social, and economic information that may bear upon important decisions.

The present analysis focuses on those aspects that can be quantified within a stock recruitment type of relationship. However, this does not fully account for all factors that are known to, or have been suggested to, affect the production and success of salmon in the Columbia River Basin. Information that is presently less easily quantifiable may, however, be equally important from a scientific perspective. Examples of factors that have not been included in the quantitative analysis are discussed below. We hope to deal with some of the "unmodeled" factors in our final report.

1. Individual Populations Geographically Isolated

In the modeling of future conditions, each population is treated as an independent entity and isolated from other similar populations. For example, fish do not stray from one population to another. Except to the extent they are affected by common factors, individual populations decline or increase independently. A counter point to this is that though interactions between populations are not explicitly modeled, the effects of population interactions are captured implicitly in historic stock-recruitment data and the fitted parameters of stock-recruitment functions.

In nature, interactions likely do occur between populations in regard to both behavior and productivity. Populations overlap at different life stages affecting dispersion of individuals and perhaps carrying capacity. For example, a population of salmon may spawn in habitat that is considered ideal, but disperse downstream where they intermingle with other populations. These downstream areas may be affected by habitat degredation and have limited capacity as a result. While at the present low levels of abundance such interactions may be less important, within the historical record they could have been important and could be in the future as populations rebuild. In particular, some habitats and environmental conditions could result in low capacities for some life stages. This could affect dispersal, distribution, and survival of populations.

At a level above that of individual populations, the present analysis addresses races (for example, spring chinook) in isolation from other races and species. Regional decisions, however, will be made in the context of their impacts on other races, as well as other salmon, resident fish and wildlife. It is quite possible that assessments of major actions could be different if considered across the spectrum of affected species, races, and populations, rather than for each biological group in isolation. Fall and spring chinook, for example, utilize the mainstem Columbia and Snake rivers at markedly different points in their life cycle. Evaluating management options from the perspective of either group in isolation (perhaps because the biology of one or the other is more easily quantifiable from existing information), may result in a different prioritization of actions than if the actions were evaluating in a broader context of the species (or genus) collectively in an ecosystem context. The final PATH decision analysis will jointly consider the impacts of management actions on spring / summer chinook, fall chinook, and steelhead.

2. Populations are Genetically Isolated

The analysis treats populations as genetic isolates as well. No emigration between populations is explicitly considered. Salmon populations, however, are likely structured into some higher organization reflecting differing degrees of genetic communication. Within this organization, populations may vary asynchronously as a result of local as well as regional conditions. Genetic communication (gene flow) between populations is balanced with selection for local conditions. This organization likely develops as an adaptive trait in response to environmental variation. As such, it would have an impact on survival, rebuilding and sustainability of populations that is not considered in the analysis.

In this analysis, each population declines or increases in isolation from other populations. In reality, conditions may result in increases in some populations and emigration into other populations. The effect of declines in some populations due to local catastrophes or environmental variation may be dampened by emigration from stronger populations. However, a counter argument to this criticism is that in the Snake

River, all naturally spawning spring/summer chinook populations are weak, and varying in synchrony, (i.e., there are no strong populations to serve as effective sources to mitigate the effects of local catastrophes).

3. Populations are Behaviorally Similar for Parts of their Life Cycle

Populations vary within the analysis in regard to basic productivity (Ricker *a* and *b* values) and in regard to their position relative to the number of dams encountered moving to and from the ocean. Fish move through the hydroelectric system at similar times encountering similar conditions and reacting in similar manners. Differences among stocks in basic productivity parameters may reflect differences at any point during the life cycle. Two of the key hypotheses considered in the decision analysis are: 1) whether or not there are common year effects between upriver and downriver stocks; and 2) whether differences in recruitment between upriver and downriver stocks reflect exposure to hydrosystem effects, or differences in marine survival. These alternatives are reflected in the delta and alpha models, and the alternative hypotheses about post-Bonneville survival.

Information upon which to test these alternative hypotheses is limited. The hypothesis of broadscale, regional differences in marine migration and survival of spring chinook, for example, is neither supported nor rejected by the very limited coded wire tag data available from ocean recoveries of hatchery fish. Nonetheless, based on differences observed in some races for which data exist (e.g., Columbia River fall chinook), there is reason at least to carry these alternative hypotheses through the analyses.

4. The Mainstem River is Treated Largely as a Migrational Corridor Affected only by Flow

The mainstem Columbia and Snake rivers are treated largely as reservoirs affected only by flow rate. Quantity and quality of mainstem river habitat for juvenile rearing and migration are not considered. Factors that form riverine environments include geomorphology, physical (and biological) structure, and hydrology. The interaction of these to form salmonid habitat and structure salmonid populations are not considered. The impact of simplification of the mainstem and elimination of habitat is not included except as it is embedded in the historical data. Consideration of future conditions in the mainstem rivers resulting from changed management practices is limited to its effect on fish travel time and migrational survival.

There are legitimate counter-arguments to the above criticisms. First, the key factors for successful mainstem migration are better known for spring/summer chinook than for other species/races. They are passive migrants which historically took less than two weeks to migrate the entire distance. The net effect of any uncertainty of the importance of the mainstem area for spring chinook is much more likely to result in an underestimate of the benefits of a natural river system on chinook survival than in an overestimate.

5. The Impacts of Hydroelectric Operations on the Estuary are not Considered

Within the analysis, changes in hydroelectric operations are restricted to their impacts on flows and velocities within the area above Bonneville Dam. However, there is substantial reason to believe that hydroelectric operations have an appreciable effect on the estuary as well and, therefore, on fish survival. Flow management and the impact of dams on downstream movement of material are known to have changed the timing and magnitude of the spring freshet and the type and quantity of organic and inorganic material delivered to the estuary. The shape and dynamics of the ocean plume are known to have changed as the river has been developed. Biological linkages, especially to salmon production and survival, are difficult to demonstrate, in part because little effort has been directed at this topic. However, because the estuary represents a critical transitional stage for both adult and juvenile salmonids, it is reasonable to suspect a linkage between river development and operations and salmon production. These impacts are not considered in the present analysis because of the lack of quantitative relationships between estuarine physical conditions and salmon. However, some of the empirical relationships incorporated into the models (e.g. common year effects on upstream and downstream stocks in the Delta model, dependence of extra mortality on mainstem flow in the Alpha model) may implicitly capture such effects.

5 Results

5.1 Introduction

PATH analyses to date have made considerable progress towards defining alternative hypotheses about key elements of life-cycle survival and identifying the critical differences between alternatives. This advance in understanding and clarification of differences are described in Chapter 4 and Appendix A of this report.

In this section, we focus on the implications of these alternative hypotheses for decision-making. The objectives of this section are to:

1. Explore ways to summarize complex analyses and results into graphs that are easy to understand, interpret, and explain to decision-makers (Section 5.2).
2. Provide **preliminary** insights into the relative performance of alternative actions (Section 5.3), recognizing that we have only explored a partial set of actions and hypotheses, and that further refinements in our methods are likely to occur.
3. Identify the uncertainties that are most important to assign weights to (if possible) and/or resolve through either continued monitoring and research or deliberate adaptive management experiments (Sections 5.4 and 5.5).
4. Test the sensitivity of decisions to the weights that are placed on key uncertainties (Section 5.6), so as to focus the assessment of existing evidence, and the acquisition of additional evidence.
5. Summarize some other important performance measures (Section 5.7).

5.2 Ways to Summarize Results

We have generated predicted outcomes for alternative management actions (A1, A2, and A3; see Chapter 2 for a description) using each possible combination of models and hypotheses described in Section 4. Each one of these combinations is called an “aggregate hypothesis” or a “run”. Because there are 5,148 unique aggregate hypotheses, there are 5,148 unique alternative futures that one could examine to evaluate alternative actions. This range of outcomes reflects the uncertainty associated with predicting future events from imperfect or incomplete information. We have attempted to boil down this information to reveal the critical uncertainties. Such summarization will be particularly necessary in the final decision analysis report that considers additional actions and species.

To illustrate some of the concepts that follow, example output for 20 out of the 5,148 aggregate hypotheses analyzed is shown in Table 5.2-1. The values shown in Columns 2 to 4 are the probability that the number of spawners for the sixth best stock will exceed a pre-defined “survival” level of spawning abundance for that stock, projected over the next 24 years (this is the 24-year NMFS survival standard; see Chapter 3). Higher numbers indicate larger projected spawning abundances. The 24-year survival standard was selected for this illustration arbitrarily; readers should be aware that there are several different jeopardy standards and performance measures that could be used (see Chapter 3 and Appendix D).

One way to select an action on the basis of the information in Table 5.2-1 would be to simply assume that the "correct" aggregate hypothesis is known with complete certainty, and then select an action based on the outcomes from this single aggregate hypothesis. For example, suppose we assume that aggregate hypothesis #9 is correct. The probability of exceeding the survival escapement level under this aggregate hypothesis is 0.82 for A1, 0.68 for A2, and 0.63 for A3. If we apply the informal criterion that this probability must exceed 0.70 for an action to be considered acceptable (see Chapter 3 for a description of the NMFS jeopardy standards), then the only action that is acceptable is A1, and that is the action that should be taken **(Figure 5.2-1)**.

Table 5.2-1: Example outputs. Probabilities of exceeding Survival escapement level over 24 years, under three management actions, for 20 randomly selected aggregate hypotheses. Weights were assigned randomly for illustrative purposes.

	Probability that the number of spawners exceed the survival escapement level			Is probability greater than 0.7? 1 if yes, 0 if no			
Aggregate Hypothesis	**A1**	**A2**	**A3**	**A1**	**A2**	**A3**	**Weight**
1	0.35	0.65	0.45	0	0	0	0.05
2	0.35	0.65	0.47	0	0	0	0.08
3	0.84	0.85	0.78	1	1	1	0.09
4	0.78	0.85	0.78	1	1	1	0.08
5	0.33	0.63	0.46	0	0	0	0.01
6	0.32	0.65	0.47	0	0	0	0.02
7	0.84	0.84	0.78	1	1	1	0.07
8	0.74	0.85	0.78	1	1	1	0.08
9	0.82	0.68	0.63	1	0	0	0.05
10	0.83	0.71	0.63	1	1	0	0.07
11	0.51	0.53	0.67	0	0	0	0.05
12	0.51	0.53	0.67	0	0	0	0.06
13	0.66	0.60	0.75	0	0	1	0.01
14	0.67	0.61	0.76	0	0	1	0.00
15	0.53	0.52	0.66	0	0	0	0.07
16	0.53	0.52	0.66	0	0	0	0.07
17	0.67	0.60	0.75	0	0	1	0.06
18	0.68	0.60	0.74	0	0	1	0.04
19	0.53	0.50	0.67	0	0	0	0.03
20	0.53	0.51	0.67	0	0	0	0.01
# of aggregate hypotheses resulting in probability >0.7				6	5	8	
Proportion of aggregate hypotheses resulting in probability >0.7				0.3	0.25	0.4	
Expected ability to meet 0.7 standard				0.44	0.39	0.43	

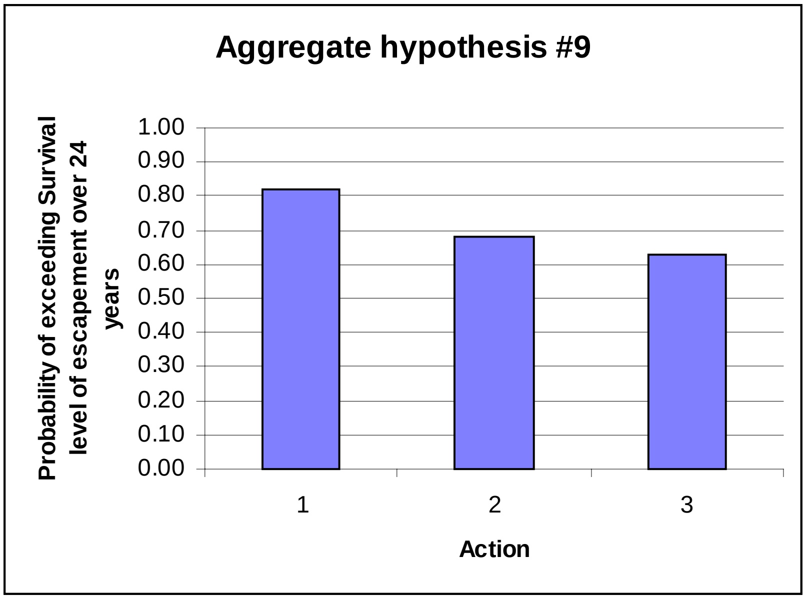


Figure 5.2-1: Display of example results for aggregate hypothesis #9 in Table 5.2-1.

The problem with this approach is that we don't know for certain which aggregate hypothesis is correct, so we have no way of selecting a single hypothesis. Moreover, different aggregate hypotheses can lead to different conclusions. For example, A3 is the only acceptable action under aggregate hypothesis #18 (i.e., the only action with a probability greater than 0.7), while no actions are acceptable under aggregate hypothesis #1 (Table 5.2-1). Therefore, the choice of a particular action will depend on which of the 5,148 aggregate hypothesis is assumed to be correct.

What we need is a way to summarize the results for all aggregate hypotheses in a way that is easy to understand and accurately captures the entire range of outcomes that are possible. There are at least three potential approaches to do this:

1. a frequency distribution of outcomes,
2. calculate the fraction of aggregate hypotheses that meet some criterion
3. calculate the expected ability of an action to meet some criterion

Brief explanations of these approaches are provided below for readers who are unfamiliar with basic principles of probabilities and probability distributions.

5.2.1 Frequency Distribution of Outcomes

One approach is to count the number of aggregate hypotheses that produce an outcome in a given range (or "bin") for a given action. This can be done for a range of bins that covers the entire range of outcomes for that action and shown as a bar chart, which is called a frequency distribution. An example frequency distribution is shown in Figure 5.2-2, based on the outcomes in Table 5.2-1. There are two main features of this frequency distribution. First, the height of the bars reflects the relative frequency with which an outcome in a particular bin is produced. In the example, A1 outcomes in the 0.51 to 0.60 bin are produced with greater frequency than those in other bins. Second, the frequencies show the maximum and minimum limits on the range of outcomes, which provides an indication of the amount of uncertainty in the outcomes. In the example frequency distribution for A1, there are no values less than 0.31 and no outcomes greater than 0.90. Hence, in the four most pessimistic aggregate hypotheses (worst-case scenario), action A1 has only a 0.31 to 0.4 chance of exceeding the pre-defined spawning abundance threshold. The four most optimistic aggregate hypotheses have a 0.81 to 0.9 chance.

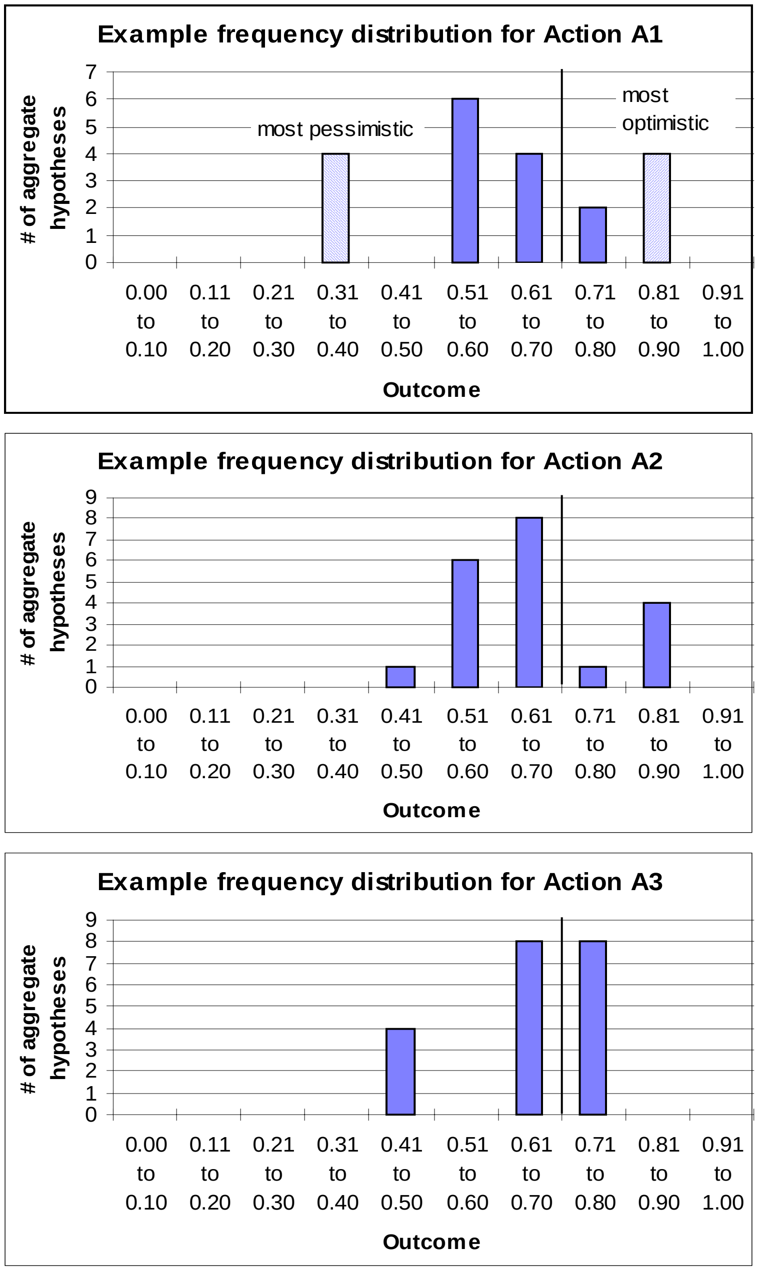


Figure 5.2-2: Example frequency distributions for A1, A2, and A3, based on Table 5.2-1.

5.2.2 Fraction of aggregate hypotheses that meet some criterion

Frequency distributions such as those in Figure 5.2-2 are effective in communicating the degree of uncertainty, but do not clearly show which actions are preferred if an absolute criterion (such as the 0.7 level defined by the NMFS jeopardy standards) is applied. One way to apply this criterion is to calculate the fraction of aggregate hypotheses that result in a probability of greater than 0.7 (i.e., the fraction of aggregate hypotheses that are to the right of the vertical line at 0.7 in Figure 5.2-2). For the example data, six out of 20 (0.30) aggregate hypotheses meet this criterion for action A1, five out of 20 (0.25) for A2, and eight out of 20 (0.40) aggregate hypotheses meet the criterion for action A3. The calculation is also shown in tabular form in Table 5.2-1.

On a relative basis, A3 would be preferred to action A1 and A2. One could also apply some absolute criterion to this fraction to assess the acceptability of actions. For example, an action might be considered acceptable if the NMFS standard were met in more than half of the "possible futures", that is, if the fraction of aggregate hypotheses producing 0.7 probability of exceeding survival escapement levels was 0.5 or greater. Under this criterion, none of the actions in the above example would be considered acceptable.

5.2.3 Expected ability of actions to meet some criterion

One of the drawbacks of a frequency distribution approach is that it implicitly assumes that all aggregate hypotheses are equally likely to be correct, even though there may be information to suggest that some aggregate hypotheses are more likely to correctly represent the way things actually occur in nature. One way to incorporate the relative "belief" in different hypotheses (i.e., the relative possibility that one hypothesis or another most accurately represents the actual conditions) is to weight each aggregate hypothesis by a probability between 0 and 1. The weighting that is placed on a particular aggregate hypothesis reflects the relative belief that the hypothesis best represents the way things work in nature. For example, if one were absolutely certain that a particular aggregate hypothesis represented the way things worked, that aggregate hypothesis would be assigned a weighting of 1, while all of the others would be assigned a weighting of 0. If two aggregate hypotheses were considered to be equally possible, each would be assigned a value of 0.5.

Once these weightings are assigned, we can calculate the weighted fraction of aggregate hypotheses in which the probability of exceeding the survival (or recovery) escapement level is 0.7 (0.5 for recovery). This is also referred to as the "expected ability of an action to meet the survival or recovery standard". In columns 5-7 of Table 5.2-1, we place a "1" if the aggregate hypothesis in the row meets the 24-year survival standard (i.e., probability in columns 2-4 is greater than 0.7), and a "0" if it does not. Then, the expected ability to meet the 24-year survival standard in Table 5.2-1 is calculated by multiplying the weighting for an aggregate hypothesis by the "1" or "0" for that hypothesis, then summing over all aggregate hypotheses (this is equivalent to adding up the weights of only those aggregate hypotheses that meet the standard). If all hypotheses meet the standard, the expected ability to meet the standard is 1.0. Doing this calculation using the example data and weights yields an expected ability to meet the survival standard of 0.44 for A1, 0.39 for A2, and 0.43 for A3. These values imply that given the level of uncertainty indicated by the relative weights, there is less than a 50/50 chance that the actions will meet the 24-year survival standard.

Note that the expected ability to meet a particular standard is a function of both the number of aggregate hypotheses in which that standard is met and the relative weighting placed on that hypothesis. For example, high expected abilities can arise either when a large number of aggregate hypotheses result in that standard being met or when a small number of highly-weighted aggregate hypotheses meet the standard. Because the weighted average probability and the expected ability to meet survival or recovery standards include all possible outcomes and their relative weights, they effectively capture the uncertainty associated with predicting future outcomes from imperfect or incomplete information.

Ideally, weightings assigned to aggregate hypotheses should be based on whatever empirical evidence is available at the time of the analysis. However, in cases where empirical evidence is unavailable or is interpreted differently by different groups, weightings will have to be based on the personal experience and judgement of individuals. In the future, PATH will attempt to reach consensus on the assignment of weightings to alternative hypotheses for those critical uncertainties that drive the results. To facilitate this process, we attempt in this chapter to identify what those critical uncertainties are (Sections 5.4 and 5.5), and to conduct some preliminary sensitivity analyses of the effects of placing different weightings on critical hypotheses (Section 5.6).

Once the critical uncertainties are identified, and weightings assigned, the focus should be on defining specific combinations of hypotheses (i.e., prospective aggregate hypotheses) that are both internally consistent (i.e., the individual hypotheses are based on common assumptions and logic) and are consistent with specific retrospective aggregate hypotheses (Table 4.1-2). For example, a prospective aggregate hypothesis that is consistent internally and with retrospective aggregate hypothesis H1 in Table 4.1-2 is the combination of FLUSH, FGE1, TURB5, PREM3, T1 or T2, Delta model, Hydro-related extra mortality, and "Markovian" climate. A prospective aggregate hypothesis that is consistent internally and with retrospective aggregate hypothesis H2 in Table 4.1-2 is the combination of CRiSP, FGE1, TURB4, PREM3, T3, Alpha model, Regime shift extra mortality, and cyclical climate.

5.3 Relative Performance of Alternative Actions

PATH can provide only very preliminary results on the performance of alternative actions from analyses completed to date. There are three reasons for this. First, not all proposed hypotheses and management actions have been formally evaluated. Second, because we have not yet assigned relative weights to alternative hypotheses, we assume equal weighting for all aggregate hypotheses. Third, further refinements in our methods will likely occur following peer review. **Because of these limitations, the results we present in this section should not be interpreted as implying that one action is better than another. Instead, they should be seen only as an illustration of how these kind of results might be displayed.**

We separate results generated with the CRiSP-T3 passage model and transportation assumptions from those generated with the FLUSH-T1/T2 model. We did this because these two models represent fundamentally different approaches to estimating mortality through the juvenile migration corridor (see Sections 4.2.1 and A.2.1), and because they are each associated with different assumptions about the relative survival of transported and non-transported fish in the ocean (T3 for CRiSP, T1/T2 for FLUSH; see Sections 4.3.1 and A.3.1). Note that although we refer to CRiSP and FLUSH as alternative hypotheses for convenience, it is really their respective underlying assumptions and mechanisms with which we are concerned. Comparison of some diagnostic outputs of the passage models (in-river survival and total direct survival) are shown in Section 5.8. Both passage models were used in conjunction with the BSM life cycle model to project spawning abundances.

To examine the relative performance of the different actions, we use the three official NMFS jeopardy standards[1]. Survival standards are met when the spawning escapement of the sixth best[2] Snake River index stock exceeds the pre-determined survival escapement level an average of 70% of the time over 24 and 100 years. The recovery standard is met when the geometric mean of projected escapement for the sixth best Snake River index stock over the last 8 years of a 48-year period exceeds the pre-determined recovery escapement level an average of 50% of the time (see Section 3 for a description of the Jeopardy Standards). On the frequency distributions below, the jeopardy standards are represented by the vertical lines at 0.7 for survival measures, and 0.5 for recovery. The fraction of runs to the right of these vertical lines indicate the fraction of aggregate hypotheses that meet the survival and recovery standards. Readers should be aware that the probability thresholds defined above (0.7 for survival, 0.5 for recovery) have been debated by some regional entities. Therefore, we explore the sensitivity of our results to different probability thresholds in Section 5.3.2.

5.3.1 Frequency Distributions of Performance Measures

First, to show the amount of uncertainty in outcomes we show frequency distributions of the probabilities that the sixth best stock will have spawner abundances greater than defined survival and recovery escapement. We do this for A1, A2, and A3 (Figures 5.3-1 to 5.3-3). Frequencies are expressed as the proportion of the total number of runs (aggregate hypotheses) for a given passage model rather than the absolute number of runs because there were an unequal number of CRiSP-T3 and FLUSH-T1/T2 runs. This was the case because some hypotheses were specific to certain passage models (e.g., T1 and T2 transportation models in FLUSH), and because a smaller set of passage hypotheses was run by CRiSP. Both passage models ran both best and worst case set of combinations of passage hypotheses that spanned the range of possible outcomes, but not all intermediate combinations were run by CRiSP.

The results show that there is a large variation in outputs, even within models and actions. For example, probabilities of being above the recovery escapement level generated with the FLUSH-T1/T2 passage model range from 0.15 to 0.85 under A1, while CRiSP-T3 probabilities range from 0.05 to 0.9. For both models, there is greater variation in probabilities associated with recovery escapement levels than in probabilities of exceeding survival escapement levels. This is because the recovery standard averages the number of spawners over only one 8-year period at the end of 48 years. While the ability to meet the survival standard is strongly affected by current stock levels and short-term projections (i.e., all runs begin from the same starting point), the ability to meet the recovery standard reflects projected escapement levels in an 8-year period 10 to 12 generations from now. These levels are more affected by the management action and associated hypotheses, and less affected by the starting point. Secondly, with alternative climate hypotheses, these 8 years may contain good conditions for fish in some runs, and bad conditions in others. Ranges do not appear to be sensitive to time periods, except for FLUSH-T1/T2-derived outputs under A3. There, the range of survival and recovery probabilities over the longer time periods (100 years for survival, 48 years for recovery) is smaller than the range of probabilities over 24 years. This is because the implementation and effectiveness of A3 measures are delayed and would have a larger influence on a 24-year probability distribution. Also, the probabilities for the longer time periods are high, and by definition cannot exceed 1.

[1] . Another jeopardy standard, the 24-year recovery standard, was recommended by the BRWG (1994) but has not been officially adopted by NMFS. Although we have not presented results for the 24-year recovery standard in this chapter, some results for this standard can be included in future drafts of this report.

[2] i.e., 5 out of the seven Snake River index stocks performed better than this sixth stock

CRiSP-T3 results generally have a greater range than FLUSH-T1/T2 runs, particularly under A3. Under this action, CRiSP-T3 probabilities of exceeding the survival level of escapement range from 0.15 to 0.95, and probabilities of exceeding the recovery level of escapement range from 0.0 to 0.85. In contrast, FLUSH-T1/T2 probabilities of exceeding the survival escapement level for A3 range from 0.5 to 1 and the probability of exceeding the recovery escapement level ranges from 0.7 to 1.0.

Again, we note that these results are preliminary and will likely change as hypotheses are modified and assigned weights, and analyses are refined. However, they do show that there is considerable uncertainty in the outcomes of alternative management actions. Probabilities can range anywhere from very low to very high values, depending on the underlying aggregate hypothesis. Because this introduces considerable uncertainty into which decision should be made, it is important to identify the individual components of an aggregate hypothesis that have the greatest effect on decisions. This is the objective of Sections 5.4 and 5.5.

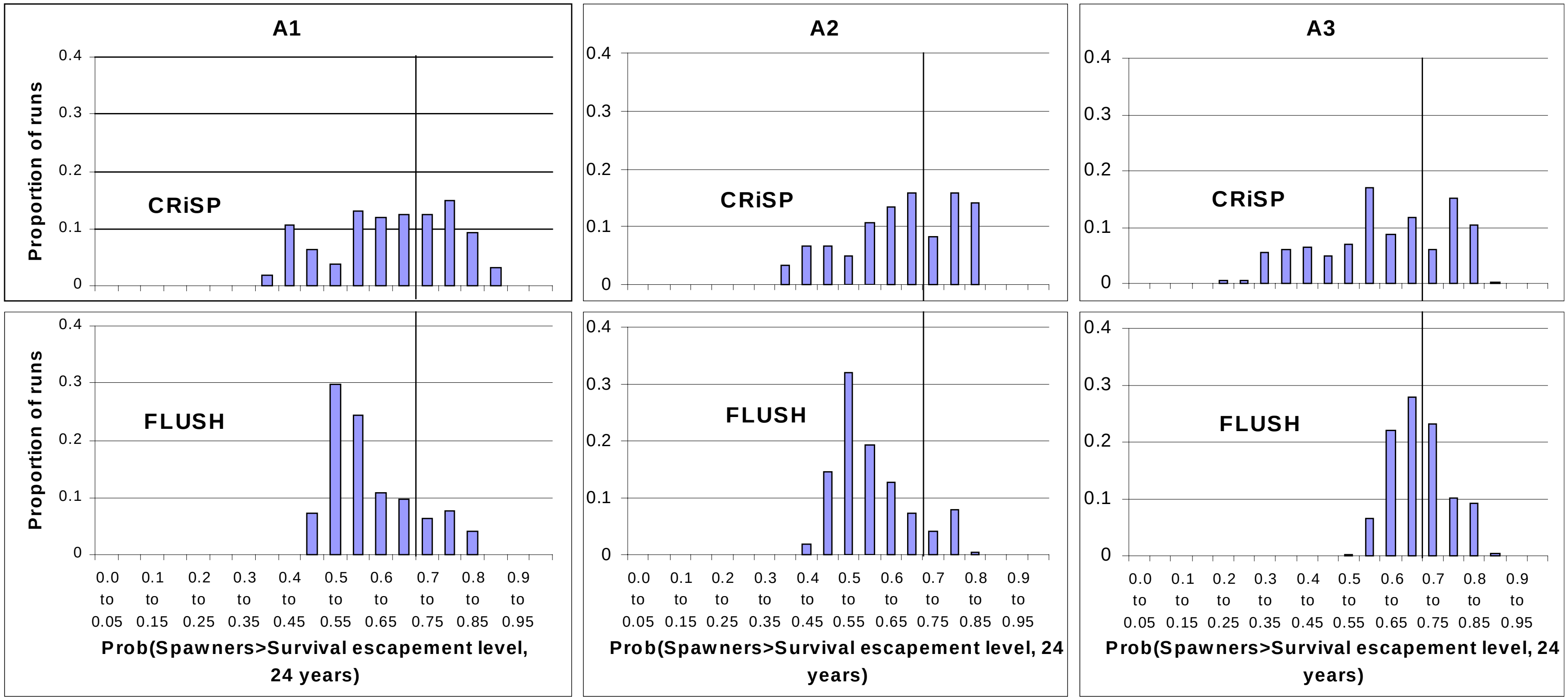


Figure 5.3-1: Frequency distributions of probability of spawners for the sixth best Snake River index stock exceeding survival levels over 24 years. The vertical line at 0.7 represents the criterion associated with the NMFS jeopardy standards.

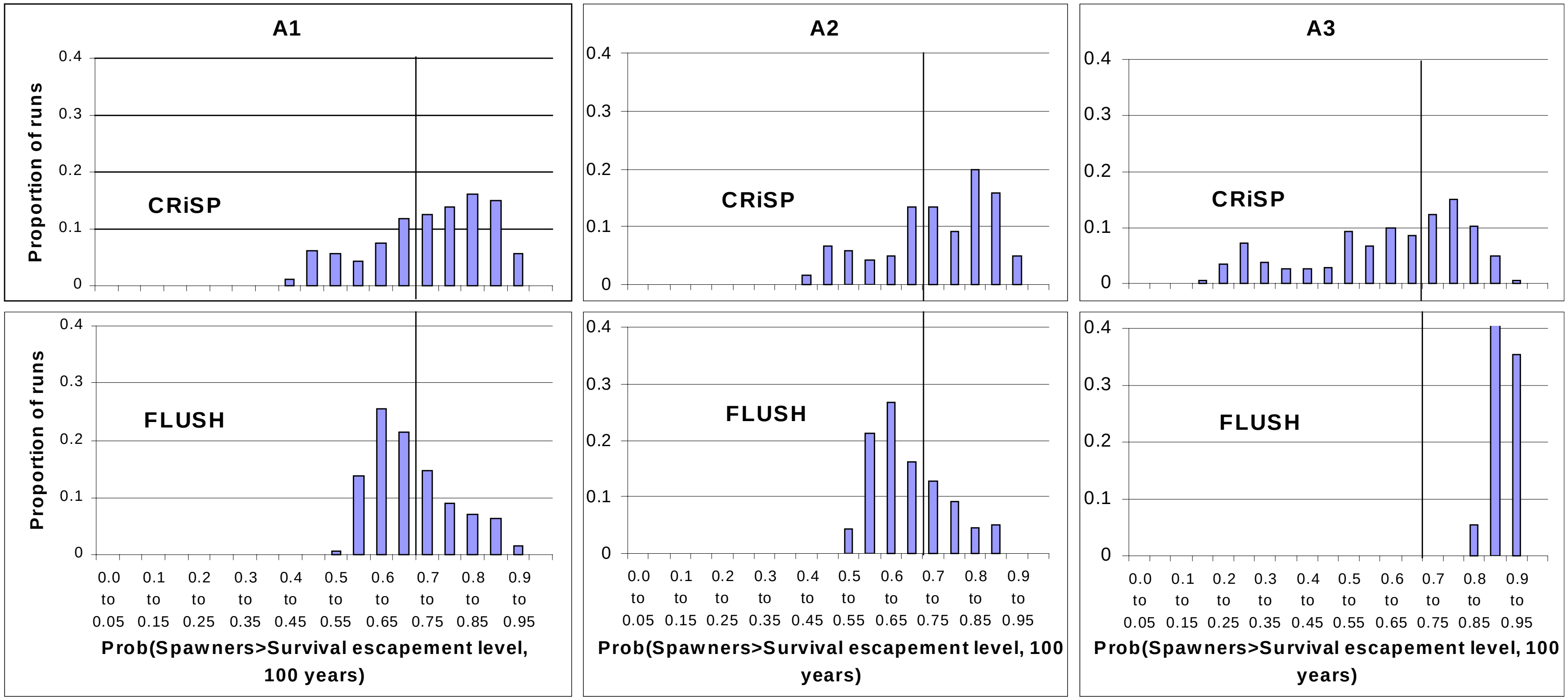


Figure 5.3-2: Frequency distributions of probability of spawners for the sixth best Snake River index stock exceeding survival escapement levels over 100 years. The vertical line at 0.7 represents the criterion associated with the NMFS jeopardy standards.

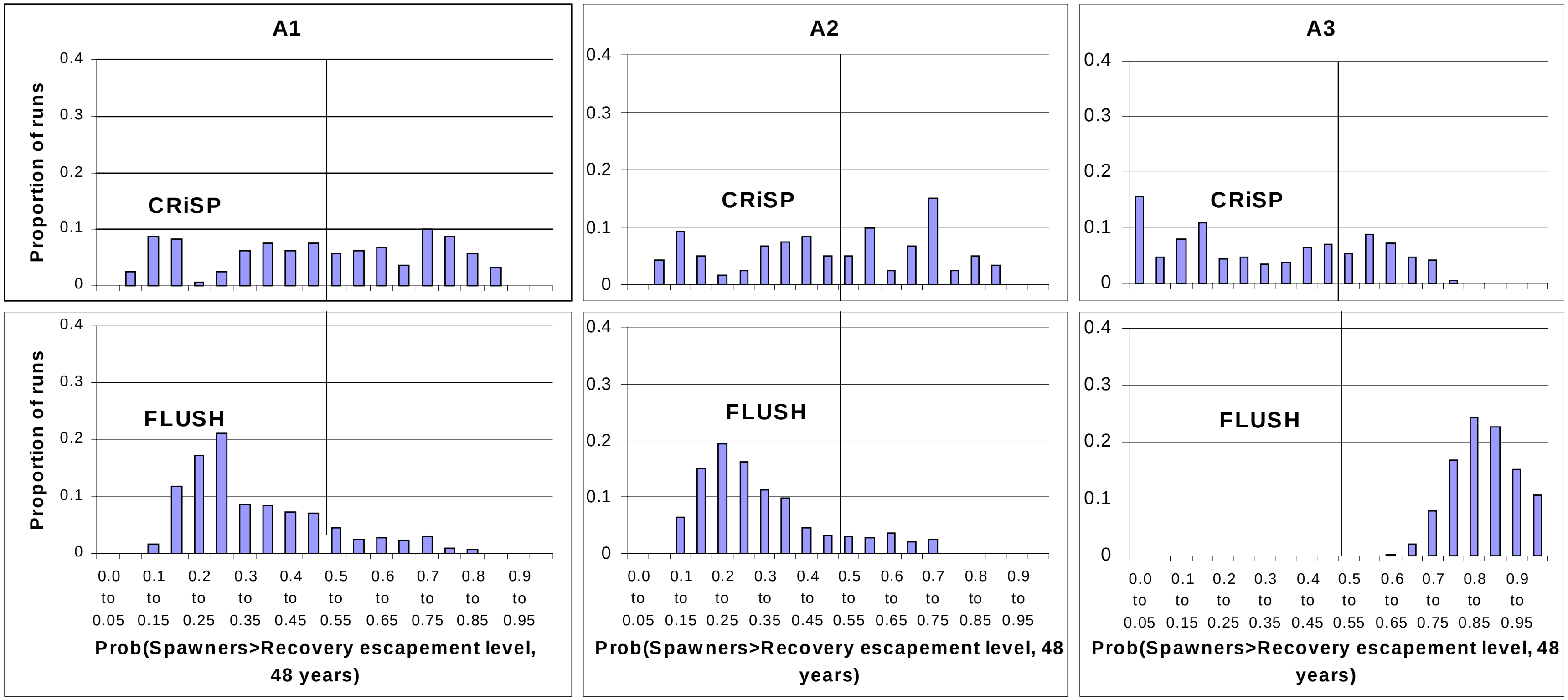


Figure 5.3-3: Frequency distributions of probability of spawners for the sixth best Snake River index stock exceeding recovery escapement levels over 48 years. The vertical line at 0.5 represents the criterion associated with the NMFS jeopardy standards.

5.3.2 Expected ability to meet survival and recovery standards

The weighted fraction, or expected ability of actions to meet survival or recovery standards are compared across actions and passage models in Figures 5.3-4 to 5.3-7.[1] Obviously, higher expected abilities are better than lower values, because they indicate a higher chance of survival and recovery. Using the CRiSP-T3 model, A1 or A2 have very similar expected abilities, while A3 always is the lowest. For FLUSH-T1/T2, A3 is always the preferred option. A1 is next, and A2 always has the lowest expected ability. These patterns hold for all of the jeopardy standards. The expected ability to meet the 100 year survival standard is greater than the 24 year survival standard because there is more time for populations to increase from their current low levels.

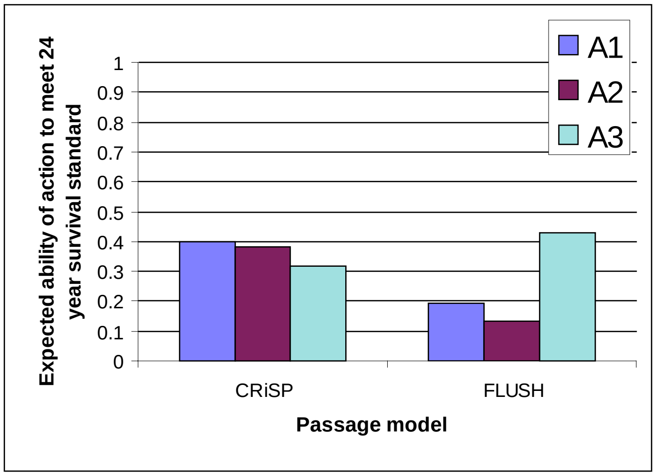


Figure 5.3-4: Expected ability to meet the 24-year survival standard.

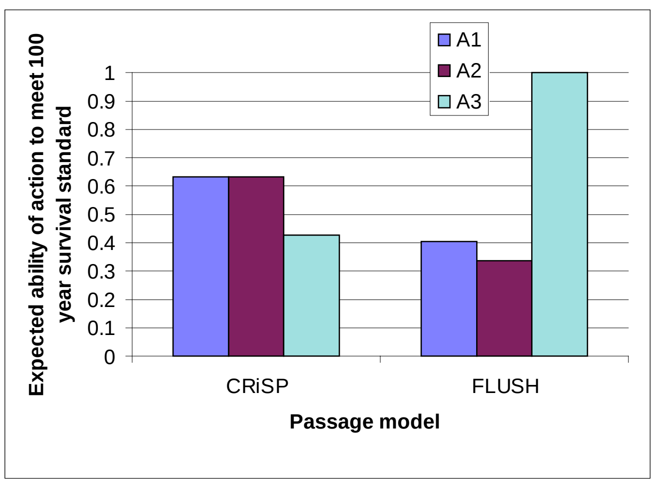


Figure 5.3-5: Expected ability to meet the 100-year survival standard

[1] Within a given passage model, all hypotheses were weighted equally. The weightings were also adjusted to correct for situations where individual hypotheses were not represented equally in the full set of aggregate hypotheses. For example, not all combinations of extra mortality and future climate hypotheses were used. As a result, there were more aggregate hypotheses containing the “cyclical” climate hypotheses than there were containing the “markov” hypothesis

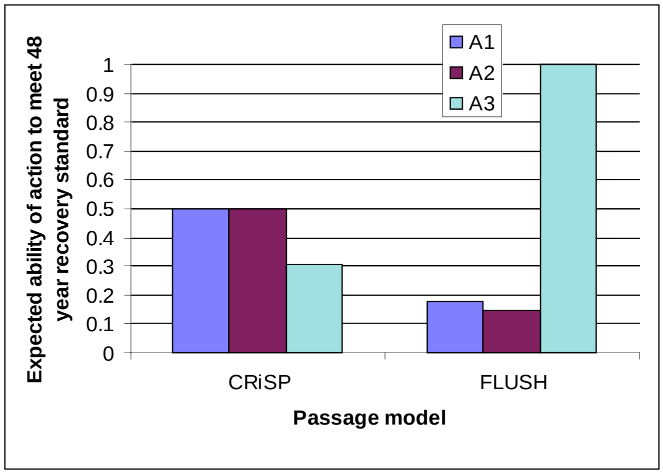


Figure 5.3-6: Expected ability to meet the 48-year Recovery standard

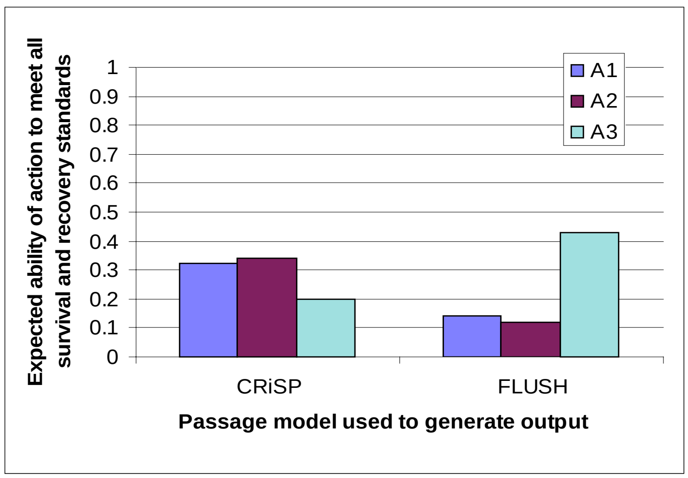


Figure 5.3-7: Expected ability to meet all survival and recovery standards

Sensitivity to weaker and stronger jeopardy standards

The NMFS jeopardy standards are somewhat binary in that an action either meets the standard (e.g., results in a probability of exceeding the survival escapement level of greater than 0.7) or it does not. Therefore, a result that just barely misses the standard (e.g., results in 0.69 average probability of exceeding survival escapement level) is not distinguished from a result that misses the standard by a wide margin (e.g., results in 0.0 average probability of exceeding survival escapement). The result is that the determination of whether an action meets the standard may be quite sensitive to the average probability that is defined as the threshold. We explored the effects of using weaker (i.e., easier to meet) and stronger (more difficult to meet) jeopardy standards than the informal NMFS definition (0.70 probability of exceeding survival escapement levels, 0.50 probability of exceeding recovery escapement levels). As a weaker standard, we assumed that the sixth best stock must exceed the survival escapement level an average of

0.60 of the time, and the recovery escapement level 0.40 of the time. For a stronger standard, we used 0.80 probability of exceeding survival escapement levels and 0.60 probability of exceeding recovery escapement levels. Results are shown in Figure 5.3-8.

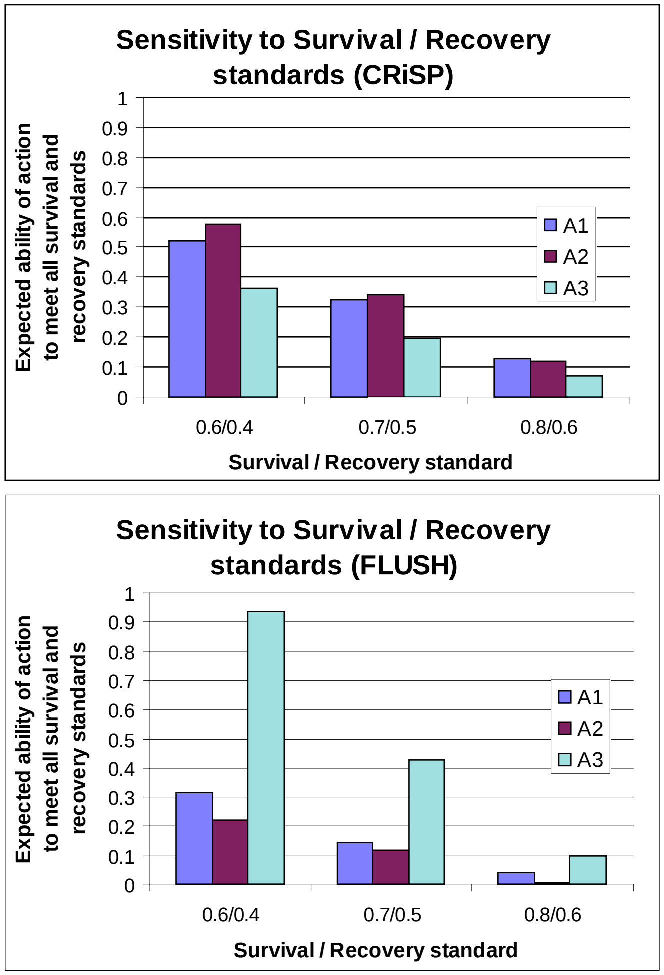


Figure 5.3-8: Sensitivity of the expected ability to meet survival and recovery standards to a weaker (0.6/0.4) and stronger (0.8/0.6) jeopardy standard. The guideline probabilities for the NMFS-defined standard (0.7/0.5) is shown for comparison. A: CRiSP-T3; B: FLUSH-T1/T2.

An alternative approach to assessing the sensitivity of outcomes to the survival and recovery probability thresholds would be to assign each outcome a graduated score from 0 to 1 based on its margin in meeting or missing the standard. For example, an outcome that just barely meets a standard (has a probability of exceeding the survival escapement level of 0.71) might be given a score of 0.5, an outcome that exceeds the probability threshold by a wide margin (e.g., has a probability of 0.90 or greater) might be given a score of 1.0, and an outcome that misses the standard by a wide margin (e.g., has a probability of exceeding the survival escapement level of 0.50 or less) might be given a score of 0.0. The expected ability of an action to meet a standard would then be calculated as a weighted average of these scores over all aggregate hypotheses.

Sensitivity to alternative harvest rate schedules

We also tested the sensitivity of outcomes to lower harvest rates. We considered two additional harvest scenarios. In the first, the harvest rates are reduced by one-third from their values in the current harvest rate schedule (see Tables 4.5.7-1 and 4.5.7-2). In the second, we consider a hypothetical scenario in which harvests of spring-summer chinook are eliminated (i.e., all harvest rates are set to 0). We stress that this "no harvest" scenario is hypothetical only, and is merely intended as a further sensitivity analysis of outcomes to harvest rates.

The results show that the expected ability of actions to meet the survival and recovery standards has either no effect or is only marginally improved when harvest rates are reduced by one third (Figure 5.3-9). The only action and passage model combination in which the expected ability under this harvest scenario differs from the base case (i.e., assuming the current harvest rate schedule) is CRiSP-T3 A3, where the expected ability increases from 0.1 to 0.3, and CRiSP-T3 A2, where surprisingly the expected ability decreases under lower harvest rates. We're not sure why this anomalous result occurs. The effect on outcomes is greater with the "no harvest" scenario. The largest difference is seen for FLUSH-T1/T2 A3, where the expected ability increases from 0.5 under the base case harvest scenario to 0.7 under the "no harvest" scenario.

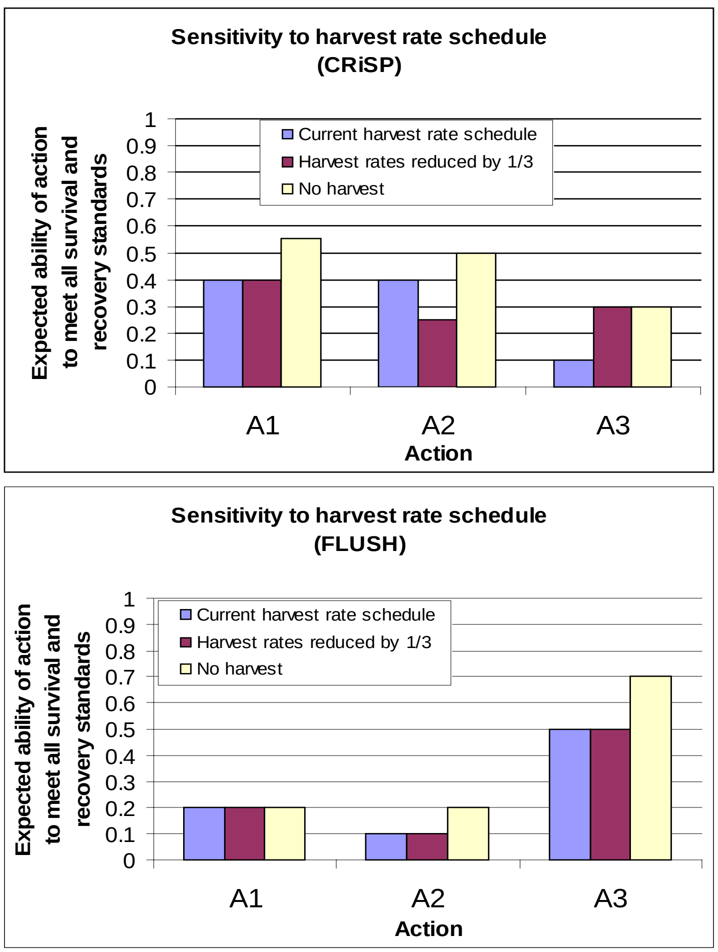


Figure 5.3-9: Sensitivity of outcomes to alternative harvest rate scenarios.

Sensitivity to passage models and associated transportation assumptions

The rank order of actions in this preliminary analysis depends most heavily on the differences in assumptions made by the passage models about mortality through the juvenile migration corridor and the relative survival of transported and non-transported fish in the ocean. FLUSH-T1/T2 favors A3 while CRiSP-T3 favors either A1 (using survival performance measures) or A2 (recovery performance measures). This confirms our general expectations based on the structure and application of these models, but our result explicitly quantifies the differences. Such a quantification is extremely important for focussing debates over their differences.

Sensitivity to survival and recovery standard

The ranking of this set of actions is relatively insensitive to the jeopardy standard that is chosen. Under FLUSH-T1/T2, the ranking of actions was the same regardless of the standard considered. There was a slight change in ranking between survival standards and recovery standards in CRiSP-T3, with either A1 or A2 having the highest expected ability to meet the standards. Although A1 (Status quo) and A2 (maximize transportation) are separate actions, we note that there is relatively little difference between the two in terms of the proportion of fish transported (as calculated by the passage models). For this reason, and because the transportation vs. drawdown question seems to be of most interest in the region, we are concerned primarily with the ranking of A3 relative to A2 and A1 in this sensitivity analysis, and less so with the ranking of A1 relative to A2.

Summary of Results

The results suggest that with this set of actions, there are few instances in which **all** of the survival and recovery standards are met with a high expected ability when all hypotheses are given equal weighting (Figure 5.3-7). The highest expected ability to meet both survival and recovery standards using the CRiSP-T3 model is around 0.35 (obtained with action A2), and around 0.4 using FLUSH-T1/T2 (obtained with action A3). Although a “satisfactory” level has not yet been defined, we would assume that decision-makers would want the expected ability to meet the recovery and survival standards to be high, because that implies a high degree of certainty that these standards will be met.

Actions have a greater ability to meet the longer-term (100 and 48-year) standards than the 24-year standard. The expected ability of A1 or A2 to meet the 100-year survival standard is about 0.6, and about 0.5 for the 48-year recovery standard. Under FLUSH-T1/T2, the expected ability of A3 to meet the 100-year survival and 48-year recovery standard is 1.0.

These preliminary results suggest that significantly greater improvements in survival are required beyond those provided by the management actions analyzed here. This is because none of the current set of actions are able to meet **all** of the standards with any degree of certainty. The 24-year survival standard is the most difficult to achieve, while actions have a higher degree of certainty of meeting the longer-term standards.

This is the case when the aggregate hypotheses are weighted equally. In the final analysis, weightings on aggregate hypotheses will reflect our best joint understanding of the way things work and will not necessarily be equal. Therefore, we explore the sensitivity of the performance of the actions in meeting the standards to the weightings placed on key uncertainties in Section 5.6. First, though, we need to identify what those key uncertainties are. This is the focus of the next section.

5.4 Sensitivity of Outcomes and Decisions to Effects of Uncertainties

We have already noted that the relative outcomes of management actions are greatly affected by the usage of either the CRiSP-T3 or the FLUSH-T1/T2 passage model (Figure 5.3-4 to 5.3-6). In this section we look at the effects of the rest of the uncertainties in Table 4.1-2 on the results of the decision analysis. The primary consideration is the relative effect of each uncertainty on the decision to be made (i.e., how does each factor affect the choice of action to ensure survival and recovery of listed stocks).

Decision Criteria

The process for making decisions about which hydrosystem action or actions to undertake is still being developed, in consultation with many agencies and groups. Here, we use two possible bases for decision-making for the sensitivity analyses in this section, both based on the NMFS Jeopardy Standards. The first is a **relative criterion**, in which the preferred action is the one that simply maximizes the expected ability to meet all three NMFS survival and recovery standards. Actions that result in a large expected ability to meet survival and recovery standards are better.

The second possible basis for decision-making is based on an **absolute criterion**. We assume that some minimum expected ability to meet all of the survival and recovery standards is required for an action to be considered acceptable. That is, we assume that decision-makers will want to be reasonably certain that the action they choose to implement will achieve the 24-year survival standard, the 100-year survival, and the 48-year recovery standard in spite of the presence of uncertainty. Because it is not clear at the moment what the minimum expected ability should be, we use 0.7 for illustrative purposes. The actual threshold minimum may be higher than 0.7, but using a lower value provides a more sensitive test for the significance of the effects of uncertainties.

Although we are looking at a combination of all three of the jeopardy standards in these analyses, we note that the 24-year survival standard is the most difficult to achieve. Therefore, looking only at the longer time periods (100-year survival standard and 48-year recovery standard) will show different patterns in responses. Results of sensitivity analyses for individual standards are shown separately in Appendix B of this report.

Sensitivity of decision to alternative hypotheses

The sensitivity of the decision (using both relative and absolute criteria) to alternative hypotheses can be tested by:

1. assigning weights of 1 and 0 to the alternative hypotheses under consideration (with equal weightings applied to all other hypotheses);
2. correcting for unequal representation of some hypotheses; and
3. comparing the outcomes.

For example, we examine the sensitivity to FGE hypotheses by comparing (Figure 5.4-1):

a) The expected ability to meet survival and recovery standards when FGE1 is assigned a weighting of 1 (with a corresponding weighting on FGE2 = 0, and equal weighting on all TURB, PREM, and other hypotheses)

versus

b) the expected ability when FGE2 is assigned a weighting of 1 (with a corresponding weighting on FGE1 = 0, and equal weighting on all other hypotheses).

Sensitivity to passage-related hypotheses are explored in Section 5.4.1. Sensitivity to other (non-passage) hypotheses are explored in Section 5.4.2. We note that the sensitivity of some hypotheses may be dependent on which other hypotheses are assumed. For example, the ranking of actions may be sensitive to which FGE hypothesis is used only when a particular TURB hypothesis is assumed. Such situations are not be immediately apparent by weighting all other uncertainties equally as we have done in this section, although we are already partially testing for joint sensitivities by doing separate sensitivity analyses for CRiSP-T3 and FLUSH-T1/T2-derived outputs. We explore the sensitivity to combinations of hypotheses in Section 5.4.3.

Summary of Results

Sensitivity analyses are summarized in Table 5.4-1. Details and supporting graphs are provided below.

Sensitivity to passage-related uncertainties

Results presented in Section 5.3 show that the ranking of actions is highly sensitive to which passage model is used in generating the outputs. CRiSP-T3, in general, tends to favor A1 or A2, while FLUSH-T1/T2 favors A3. This pattern holds true regardless of what other hypotheses are assumed, suggesting that the differences in assumptions inherent in the passage model are the main determinants of which action performs the best. Again, this is probably not a surprising result, but these results allow us to explicitly quantify the implications of differences in the passage models.

Within each model, it appears that very few of the uncertainties (at least when looked at independently) have significant effects on either the relative ranking of actions or on the ability of these actions to exceed our decision criterion of 0.7. Although some passage-related hypotheses cause large differences in the ability of some actions to achieve survival and recovery standards, in no cases are the expected abilities significantly greater than 0.5. As we noted earlier, it may be that certain combinations of passage-related hypotheses have effects large enough to change the ranking of actions or to boost the expected ability above our assumed minimum criterion of 0.7. Sensitivity to some of these combinations are explored further in Section 5.4.3.

Sensitivity to other uncertainties

Besides the passage model assumptions, the only other uncertainty that affects the decision is the uncertainty about the source of extra mortality. Under the ”BKD” and the regime shift hypothesis, the expected ability of all of the actions to meet survival and recovery standards falls short of our assumed criterion of 0.7. In other words, if post-Bonneville extra mortality remains regardless of hydrosystem actions, the stocks will have a poor ability to recover. However, if extra mortality is related to the hydrosystem, both A1 and A2 (under CRiSP-T3) and A3 (under FLUSH-T1/T2) exceed this minimum level.

Table 5.4-1: Summary of results of sensitivity analyses. Relative rankings and the absolute criterion are described in the text.

	Sensitivity of decision to uncertainty	
Uncertainty	**Decision based on relative ranking of actions**	**Decision based on absolute criterion of 0.7**
Passage Model	**Sensitive**	Not sensitive
Fish Guidance Efficiency	Not sensitive	Not sensitive
Turbine/Bypass Survival	Not sensitive	Not sensitive
Predator Removal	Not sensitive	Not sensitive
Pre-Removal Period	Not sensitive	Not sensitive
Equilibrated juvenile survival	Not sensitive	Not sensitive
Transition juvenile survival	Not sensitive	Not sensitive
Prospective model	Not sensitive	Not sensitive
Extra mortality	Not sensitive	**Sensitive**
Future climate	Not sensitive	Not sensitive

5.4.1 Effects of uncertainties related to downstream passage

In this analysis we considered four uncertainties related to downstream passage:

1. the effectiveness of extended-length bypass screens in increasing Fish Guidance Efficiencies (FGE1 and FGE2);
2. different hypotheses about the causes of bypass-related mortality during some historical years (TURB1, 4, 5, and 6);
3. the effectiveness of the predator removal program in reducing reservoir mortality (PREM1 and PREM3); and
4. uncertainties related to drawdown
    a) length of the pre-removal period (time between when a decision is made and when removal of dams begins)
    b) juvenile survival rate after river has returned to an equilibrated state
    c) juvenile survival rates during the transition period (time between removal of dams and achievement of equilibrated state).

Analyses for one additional passage-related uncertainty — spill efficiency at Lower Granite, Little Goose, and Lower Monumental dams — were not completed for this draft. Initial results suggest that results may be more sensitive to this uncertainty than to the other passage-related uncertainties. In addition, there may be additional hypotheses for 4 c) (juvenile survival rates during transition period).

Fish Guidance Efficiency

In general, FGE1 (the assumption that FGEs improve using extended-length screens) results in higher overall abilities to meeting survival and recovery standards than FGE2. The relative ranking of Snake River drawdown (i.e., A3 vs. A1/A2) is insensitive to the particular FGE assumption under both FLUSH-T1/T2 and CRiSP-T3.

In terms of an absolute criterion, FGE1 more than doubles the performance of A2 under CRiSP-T3. Even under this higher FGE, however, the expected ability to meet survival and recovery standards is less than 0.7. Expected abilities under FLUSH-T1/T2 are also less than 0.7. If the decision were to be made on the basis of this absolute criterion, no actions would be considered acceptable regardless of the FGE assumption.

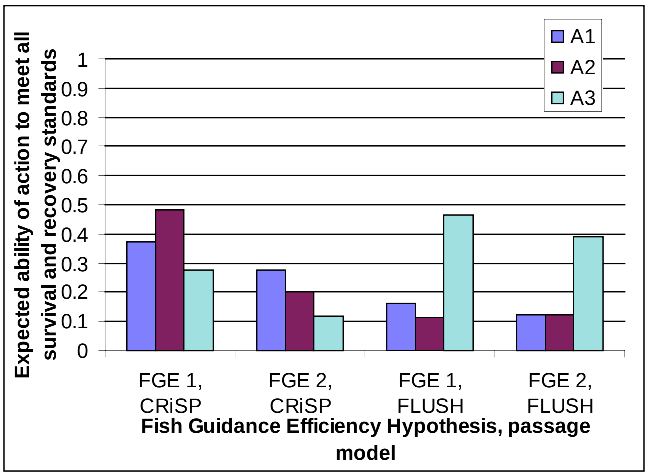


Figure 5.4-1: Expected ability to meet survival and recovery standards under different FGE hypotheses.

Turbine/Bypass Survival

The sensitivity of the model results to the TURB hypotheses was examined in a similar way. Results are shown in Figure 5.4-2. The relative ranking of A3 vs. A1 or A2 is insensitive to TURB hypotheses under both FLUSH-T1/T2 and CRiSP-T3. A1 or A2 have the highest expected ability to meet survival and recovery standards for CRiSP-T3 under all TURB hypotheses, while A3 is always ranked highest under FLUSH-T1/T2.

Expected abilities do not appear to be greatly affected by different TURB hypotheses, and are less than 0.7 in all cases. A decision based on an absolute criterion is therefore also insensitive to the TURB hypothesis.

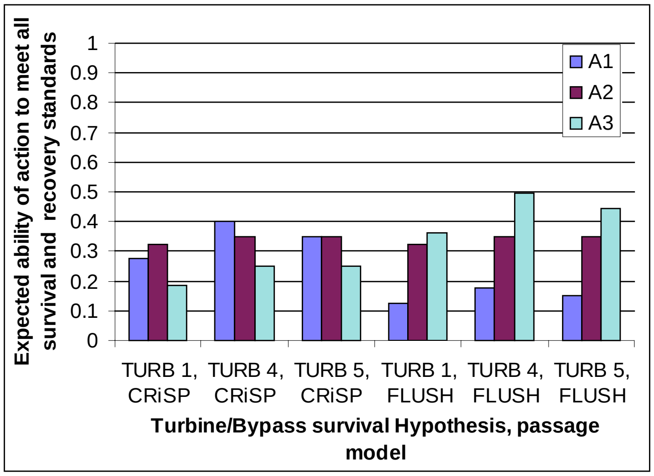


Figure 5.4-2: Expected ability to meet survival and recovery standards under different TURB hypotheses.

Predator Removal

Results using the different Predator Removal (PREM) hypotheses are shown in Figure 5.4-3. Alternative hypotheses have little effect on rankings using FLUSH-T1/T2 (A3 is highest in both cases). With CRiSP-T3, PREM hypotheses affect the rank order of A1 and A2, but do not affect the relative performance of A3.

PREM3 (25% reduction in reservoir mortality due to predator removal program) does tend to result in an increased expected ability to meet the survival and recovery standards, but even this higher PREM assumption does not result in expected abilities greater than 0.7.

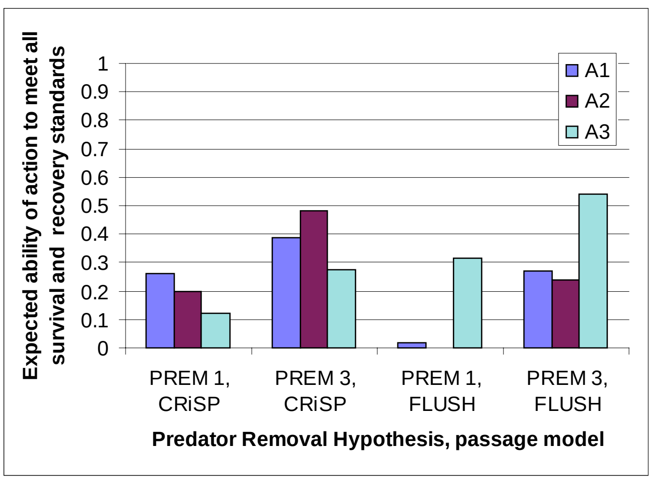


Figure 5.4-3: Expected ability to meet survival and recovery standards under different predator removal hypotheses.

Drawdown

The sensitivity of ranking of actions to uncertainties related to drawdown (A3) is shown in Figures 5.4-4 to 5.4-6. Relative rankings of actions are insensitive to each individual uncertainty. For CRiSP-T3, the expected ability to meet survival and recovery standards under A3 is always lower than the expected ability of A1 or A2, although the higher equilibrated juvenile survival hypothesis (EJUV2) pushes the performance of A3 to close to that of A2 and A1. For FLUSH-T1/T2, A3 is always the best, even under the most pessimistic of assumptions analyzed (e.g., 10-year transition period under TJUVb).

Making pessimistic drawdown assumptions (e.g., long pre-removal (PRER2) or transition (TJUVb) periods), tend to result in substantially reduced performance of A3 under FLUSH-T1/T2. However, the difference is not enough to change a decision based on an absolute criterion of 0.7, because no actions meet this criterion in any case.

Drawdown assumptions are an example where the cumulative effect of certain combinations of drawdown hypotheses may be significant enough to change decisions even though effects of individual hypotheses are not. Sensitivity to combinations of drawdown hypotheses are explored in Section 1.4.3.

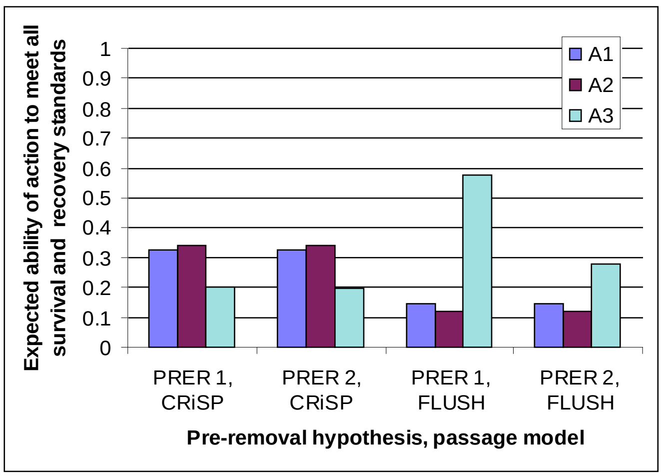


Figure 5.4-4: Expected ability to meet survival and recovery standards under different hypotheses about the length of the Pre-Removal period.

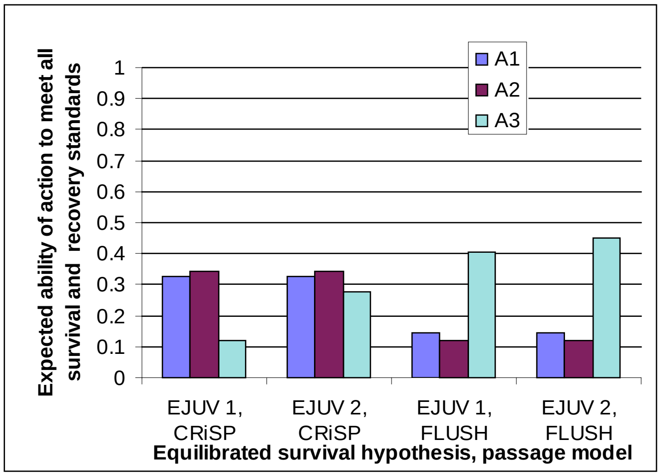


Figure 5.4-5: Expected ability to meet survival and recovery standards under different hypotheses about equilibrated juvenile survival rates after drawdown.

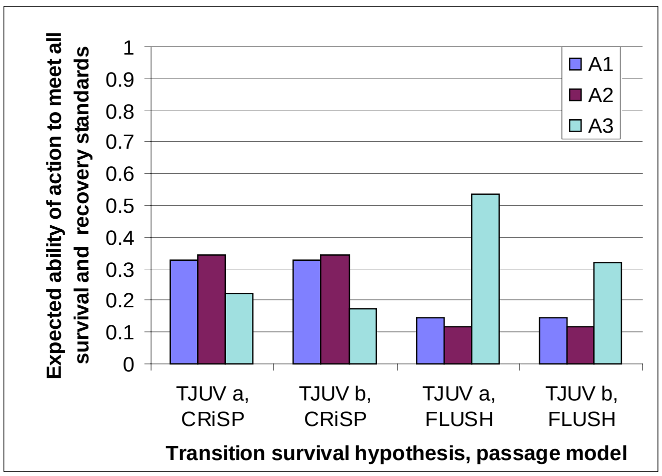


Figure 5.4-6: Expected ability to meet survival and recovery standards under different hypotheses about juvenile survival rates during the transition period between dam removal and equilibrated conditions.

5.4.2 Effects of other uncertainties

In this section, we look at the sensitivity of decisions to "other" uncertainties (i.e., those that are not related to downstream passage). The method for assessing the sensitivity follows that used for passage-related uncertainties in Section 5.4.1. As before, we show CRiSP-T3 and FLUSH-T1/T2 outputs separately. Uncertainties considered in this section are:

1. which prospective model is used: the Alpha model or the Delta model. The two models differ in their assumptions about inherent productivity and the extent to which climate has common effects on upstream and downstream stocks;

2. hypotheses about the sources of "Extra mortality" (mortality that occurs outside of the hydrosystem but is not captured in productivity parameters); and
3. hypotheses about future climate conditions.

Although uncertainties in the response of stocks to future habitat management (i.e., HAB hypotheses in Table 4.1-2) were included in the decision analysis (see Section 4.5.5), we do not look at the sensitivity to habitat effects here because these effects are stock-specific. Because the results presented here are for the sixth best stock, the effects of habitat assumptions are best seen when looking at results for individual stocks. Some of those comparisons are included in Section 5.5.

Prospective Model

Sensitivities of the ranking of management actions to the choice of prospective model (Alpha model vs. Delta model) are shown in Figure 5.4-7. Ranking of A2 vs. A3 is insensitive under FLUSH-T1/T2 and CRiSP-T3. The prospective model seems to have relatively large effects on absolute outcomes, with probabilities generated with the Delta model being higher overall than with the Alpha model, particularly for actions A2 and A3. However, these effects are not large enough to increase the expected ability to meet survival and recovery standards under any passage mode, action, or prospective model to 0.7.

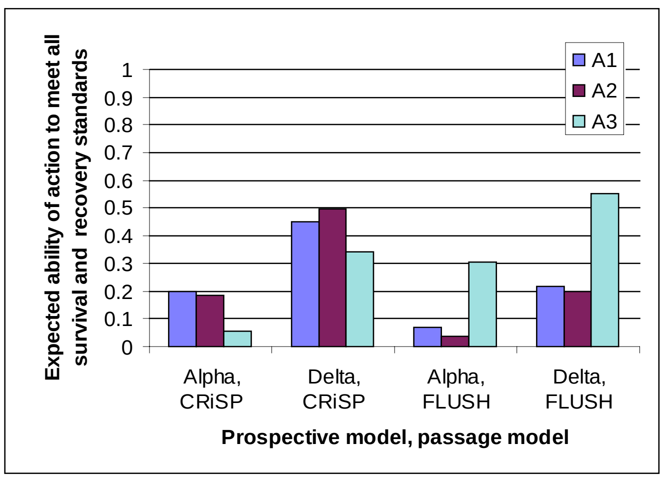


Figure 5.4-7: Expected ability to meet survival and recovery standards using different prospective models.

Extra Mortality

Sensitivity of the ranking of actions to extra mortality hypotheses is shown in Figure 5.4-8. Rankings of actions are insensitive with both passage models. A3 is preferred regardless of what is assumed about extra mortality with FLUSH-T1/T2, while either A1 or A2 is always preferred with CRiSP-T3.

However, extra mortality hypotheses have dramatic effects on decisions that are based on our assumed 0.7 minimum. With both the "BKD" and the regime shift hypotheses, all actions are clearly incapable of meeting this criterion. In fact, with the "BKD" hypothesis there is no chance of meeting the standards with all actions under CRiSP-T3, and with A1 and A2 under FLUSH-T1/T2. The same is true with the Regime shift hypothesis under FLUSH-T1/T2 A1 and A2, and none of the expected abilities for A3 exceed 0.25. Under the Hydro hypothesis, however, both A1 and A2 with CRiSP-T3 and A3 with FLUSH-T1/T2 have a greater than 0.7 expected ability to meet survival and recovery standards.

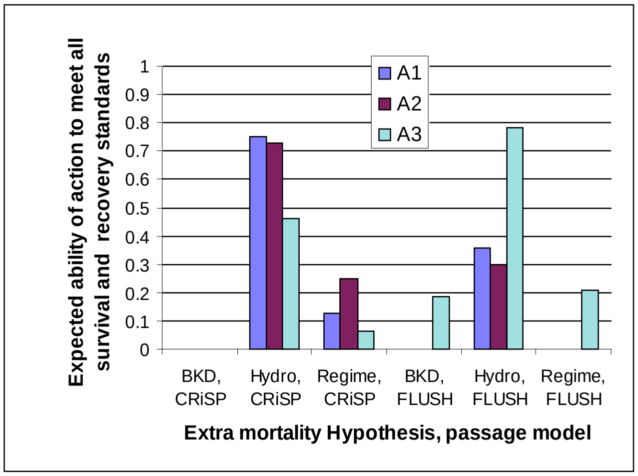


Figure 5.4-8: Expected ability to meet survival and recovery standards under different Extra Mortality hypotheses (A) CRiSP-T3 (B) FLUSH-T1/T2.

Future Climate

Sensitivity of results to uncertainty in future climate effects is shown in Figure 5.4-9. The results suggest that the alternative hypotheses about future climatic effects would not change a decision based on either a relative ranking or on an absolute basis.

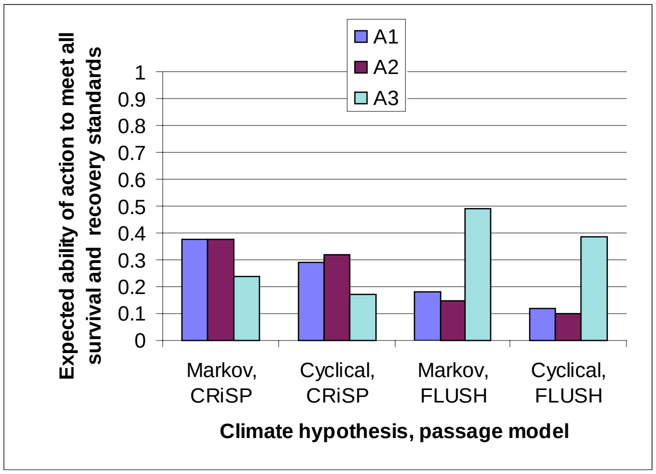


Figure 5.4-9: Expected ability to meet survival and recovery standards under different Future Climate hypotheses.

5.4.3 Effects of combinations of uncertainties

The results in the previous section have looked at the effects of different uncertainties independently of the effects of others. It is possible that uncertainties that independently have minor effects on the results, have significant effects when combined with assumptions about other uncertainties. In this section, we explore the effects of some of these combinations.

We look at the effects of "best-case" and "worst-case" combinations of uncertainties in three major categories (Table 5.4-1). Selection of "best" and "worst" cases are based on the observed effects of each hypothesis on results in Section 5.4.2. The three categories are 1) passage-related hypotheses not associated with drawdown (FGE, TURB, and PREM hypotheses); 2) passage-related hypotheses that are associated with drawdown (PRER, EJUV, and TJUV hypotheses); and 3) hypotheses that are not related to downstream passage, excluding extra mortality hypotheses and habitat hypotheses. Extra mortality hypotheses are excluded because we have already concluded that their individual effects are significant. Habitat hypotheses are excluded because these effects are stock specific and are not likely to show up in results for the 6th best stock. This leaves two uncertainties that are unrelated to downstream passage: the prospective model (Alpha or Delta) and future climate assumptions.

Table 5.4-1: Combinations of hypotheses tested in this section

Category	"Best" Case	"Worst" Case
Passage-related, not associated with drawdown	FGE1 (FGE with extended length screens better than FGE with standard length) TURB4 (relatively high historical dam mortality / relatively low reservoir mortality) PREM3 (25% reduction in reservoir mortality due to predator removal program)	FGE2 (FGE with extended length screens equal to FGE with standard length) TURB1 (relatively low historical dam mortality / relatively high reservoir mortality) PREM1 (0% reduction in reservoir mortality due to predator removal program)
Passage-related associated with drawdown	PRER1 (3 years between decision and dam removal) EJUV2 (96% juvenile survival rate at equilibrated conditions) TJUVa (2 years between dam removal and equilibration)	PRER2 (8 years between decision and dam removal) EJUV1 (85% juvenile survival rate at equilibrated conditions) TJUVb (10 years between dam removal and equilibration)
Not related to downstream passage	DELTA prospective model "Markov" Future climate (climate factors sampled from historical distribution with autoregressive properties)	ALPHA prospective model "Regime shift" future climate (future climate follows a cyclical pattern)

Combination of Passage Hypotheses

Best and worst case passage hypotheses have predictably large effects on results, but they do not significantly affect the relative ranking of actions (Figure 5.4-10). A1 or A2 is still always the best with CRiSP-T3, and A3 is still always the best under FLUSH-T1/T2. In terms of meeting an absolute criterion, the expected ability of actions to meet the survival and recovery standard is below 0.7 for all cases.

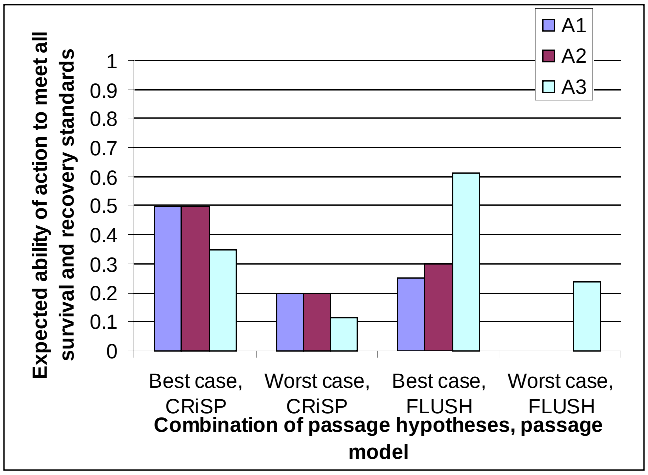


Figure 5.4-10: Expected ability to meet survival and recovery standards under best and worst case combinations of passage-related hypotheses.

Combination of Drawdown Hypotheses

Results are also relatively insensitive to drawdown hypotheses (Figure 5.4-11). For CRiSP-T3, even the best-case set of assumptions about drawdown are insufficient to bring the results for A3 up to A1 or A2, although they are much closer than was seen for any of the drawdown assumptions independently. For FLUSH-T1/T2, even the worst-case set of assumptions is not enough to bring A3 to below A2 or A1. However, the best-case drawdown scenario does elevate the expected ability to meet survival and recovery standards for A3 to around 0.8 under FLUSH-T1/T2. We explore this further in Section 5.6, where we test the sensitivity of meeting the survival and recovery standards to different weights placed on the drawdown assumptions.

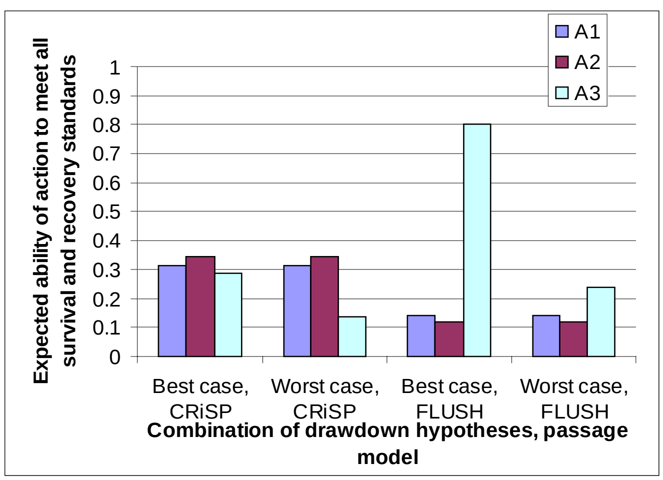


Figure 5.4-11: Expected ability to meet survival and recovery standards under best and worst case combinations of drawdown hypotheses.

Combination of other hypotheses

Results of the sensitivity analysis of best-case and worst-case sets of other (prospective model and future climate) hypotheses are shown in Figure 5.4-12. Rankings are insensitive to non-passage hypotheses for both passage models. A decision based on an absolute criterion of 0.7 is also insensitive, because expected values of meeting survival and recovery standards for any scenario do not exceed 0.7.

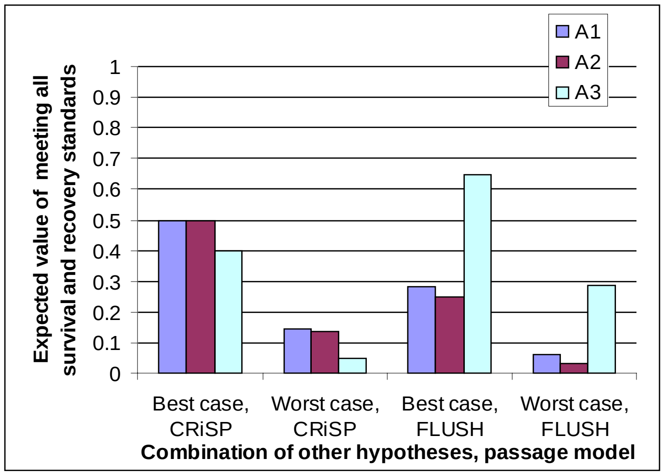


Figure 5.4-12: Expected ability to meet survival and recovery standards under best and worst case combinations of other (non-passage-related) hypotheses.

Summary of Results

Even when comparing best-case and worst-case conditions, a decision based on either the relative performance of alternative actions or on the ability to achieve our 0.70 standard are not greatly affected by passage or by other (non-passage) hypotheses. In all cases, A1 or A2 is still best under CRiSP-T3 while A3 is still best under FLUSH-T1/T2. The results suggest that drawdown assumptions would have to be worse than our worst-case scenario before A2 or A1 was ever better than A3 under FLUSH-T1/T2, and would have to be better than our best-case scenario before A3 was ever better than A2 or A1 under CRiSP-T3. Drawdown assumptions do affect the ability of A3 to meet an absolute criterion of 0.7 under FLUSH-T1/T2, but not under CRiSP-T3.

5.5 Sensitivity of Outcomes for a Single Stock (Marsh Creek) to Effects of Uncertainties

Methods

Previous sections have shown that relatively few combinations of actions and hypotheses actually meet all recovery and jeopardy standards that are based on outcomes for the sixth best stock. This raises the question: “What do you need to do (actions), and what do you need to believe (hypotheses) for a single, weaker stock to meet the NMFS standards?” This section examines actions and hypotheses for a particularly sensitive index stock (Marsh Creek). As a performance measure, we use the probability of Marsh Creek spawning abundances exceeding the survival escapement level (150 spawners) over 24 years.

We chose Marsh Creek because it is one of the weaker index stocks (along with Sulphur Creek). For these analyses, we defined a standard where the average probability of exceeding 150 spawners in any one year must be at least 0.75 (note that this is lightly higher than the NMFS-defined standard, where the average probability must be 0.7 or greater). Although the results of this analysis should be similar to those presented in the previous section, some differences are expected because in these analyses we look at a single stock rather than the sixth best stock, and because we are using a different standard.

Outcomes depend on a combination of three management actions (A1, A2, and A3), and many aggregate hypotheses. Aggregate hypotheses are specific combinations of the individual hypotheses described in Table 4.1-1. Because we needed to use balanced pair-wise comparisons for the analysis, we only looked at aggregate hypotheses that were run for both CRiSP-T3 and FLUSH-T1/T2. The result is a set of 1360 combinations of actions and aggregate hypotheses. More than half of these combinations are for action A3 because there are more uncertainties associated with A3 than with the other actions (i.e., PRER, EJUV, and TJUV hypotheses are only relevant to drawdown). Therefore, the 1360 runs were weighted so that each management action appears in 1/3 of the combinations.

Of the 1360 combinations of actions and hypotheses, only 21.8% (297) meet our 0.75 standard. The question we address in this section is “Of the 297 combinations of actions and hypotheses in which this standard was met, what percentage contained a particular action or hypothesis?” We determine whether this percentage is significant or not by comparing it to the percentage of the entire 1360 combinations that contained that action or hypothesis. If a particular action or hypothesis has no effect on the ability of Marsh Creek stock to meet our 0.75 standard, we would expect that its representation in the 297 “successful” combinations (those that meet the standard) would be the same as its representation in all of the combinations.

Results

Results are shown in Tables 5.5-1 to 5.5-4. For each hypothesis or action, the tables show:

a) the percentage of the total of 1360 combinations that include that hypothesis or action. For example, 50% of the combinations included the FGE1 hypothesis, and 50% included in the FGE2 hypothesis.

b) the percentage of the combinations that meet the 0.75 standard (297) that include that hypothesis or action. For example, out of the 297 combinations that met the 0.75 standard, 220 (74%) included FGE1.

If a) and b) differ substantially, then this indicates that a particular uncertainty has a relatively large effect on the ability to meet our standard of 0.75 average probability of exceeding 150 spawners.

Effects of Passage Models and Actions on Results for Marsh Creek

Effects of different passage models and actions are shown in Table 5.5-1. Overall results suggest that no management action “dominates” the set of results that meet our absolute criterion for Marsh Creek. Of all of the combinations that met the standard, 32% were A1, 31% were A2, and 37% were A3. CRiSP-T3 was considerably more optimistic than FLUSH-T1/T2. Of the 297 combinations that met the standard, 198 (67%) were CRiSP-T3, and 99 (33%) were FLUSH-T1/T2 (versus 50/50 for runs overall). Within CRiSP-

T3, the standard-meeting combinations were broken down relatively evenly into A1 (34%), A2 (34%), and A3 (32%). FLUSH-T1/T2 results were more heavily skewed towards A3; combinations that met the standard broke down into 28% for A1, 25% for A2, and 47% for A3.

What does this mean for decision-making purposes? Essentially, if one places all of the weight on CRiSP-T3 in a weighted analysis, there would be no reason to believe that any action would be more likely than the others to meet the standard (in Marsh Creek). On the other hand, if one placed all of the weight on FLUSH-T1/T2, A3 has a somewhat higher ability to allow the Marsh Creek stock to meet the standard than does A1 or A2. This is consistent with the results presented in Sections 5.3 and 5.4. There, we noted that A3 was consistently better than the other actions under FLUSH-T1/T2, but consistently worse than the other actions under CRiSP-T3.

Table 5.5-1: Percentage of combinations that cause Marsh Creek stock to meet the 24-year survival standard (i.e., spawning escapement is above the survival threshold at least 0.75 of the time), for each of three management actions.

Action	% of all 1360 combinations	% of 297 combinations meeting standard	% of 198 CRiSP-T3 combinations meeting standard	% of 99 FLUSH-T1/T2 combinations meeting standard
A1	33.3	32	34	28
A2	33.3	31	34	25
A3	33.3	37	32	47

Effects of Particular Hypotheses on Results for Marsh Creek

Results for the other hypotheses are shown in Table 5.5-2. Critical uncertainties are those where the representation in the runs that meet the standard is different from the representation in all runs. For example, FGE1 and FGE2 hypotheses are distributed equally (50/50) among all runs, but FGE1 is more highly represented in the runs that meet the survival standard than FGE2 (74% vs. 26%). This suggests that the FGE hypotheses are important in determining whether or not the Marsh Creek stock meets the 0.75 survival standard used here. Distribution of uncertainties is generally not dependant on the action. For example, of the 75% of the runs meeting the standard that included FGE1, 22% were A1, 24% were A2, and 28% were A3 runs. Uncertainties that make a substantial difference are the passage models (PMOD), fish guidance efficiency (FGE), predator removal effectiveness (PREM), the prospective model (Alpha or Delta), and the extra mortality hypotheses. Among those where the results are insensitive are turbine/bypass mortality (TURB), duration of the pre-removal period (PRER), equilibrated juvenile survival (EJUV), juvenile survival during transition (TJUV), future climate, and habitat enhancement (HAB).

It is important to keep in mind what is meant by "insensitive" in this context. If a result is insensitive to equilibrium juvenile survival (EJUV), for example, this means that EJUV hypotheses contribute as much to the 21.8% of the model runs that meet our 0.75 standard as they do to all 1360 runs. In other words, the different hypotheses for EJUV have little if any effect on whether or not the standard for Marsh Creek will be met. This suggests that EJUV is probably not worth worrying about as we consider further model runs (in the short term) or monitoring, small-scale experiments, or large-scale adaptive management experiments (in the longer term).

Table 5.5-2: Distribution of Hypotheses for all combinations and for those meeting the 0.75 24-year survival standard for Marsh Creek. Hypotheses for which the distributions are quite different are highlighted in **Bold**.

Uncertainty	Hypothesis	% of all model runs	% of runs meeting standard			
			Overall	A1	A2	A3
Passage Model & transportation assumption	**PMOD1 – CRiSP-T3**	**50**	**66**	**22**	**23**	**21**
	PMOD2 – FLUSH-T1/T2	**50**	**34**	**10**	**8**	**16**
Fish Guidance Efficiency	**FGE1**	**50**	**74**	**22**	**24**	**28**
	FGE2	**50**	**26**	**10**	**8**	**8**
Turbine/Bypass Survival	TURB1	54	53	14	10	29
	TURB4	23	26	10	11	5
	TURB5	23	22	8	10	4
Predator Removal	**PREM1**	**50**	**25**	**9**	**8**	**8**
	PREM3	**50**	**75**	**23**	**24**	**28**
Pre-Removal Period	N/A (A1 and A2)	67	63	32	31	n/a
	PRER1	17	24	n/a	n/a	24
	PRER2	17	13	n/a	n/a	13
Equilibrated juvenile survival	N/A (A1 and A2)	67	63	32	31	n/a
	EJUV1	17	15	n/a	n/a	15
	EJUV2	17	21	n/a	n/a	21
Transition juvenile survival	N/A (A1 and A2)	67	63	32	31	n/a
	TJUVa	17	22	n/a	n/a	22
	TJUVb	17	15	n/a	n/a	15
Prospective Model	**Alpha**	**50**	**29**	**9**	**8**	**12**
	Delta	**50**	**71**	**23**	**23**	**25**
Extra Mortality	**"BKD"**	**40**	**1**	**0**	**0**	**1**
	Hydro	**40**	**99**	**32**	**31**	**36**
	Regime shift	**20**	**0**	**0**	**0**	**0**
Future climate	"Markov"	40	50	16	16	18
	Cyclical	60	50	16	16	18
Habitat effects	0	50	50	16	16	18
	B	50	50	16	15	19

These results are generally consistent with those in Section 5.4, where we looked at the sensitivity of meeting the NMFS-defined jeopardy standards to the same uncertainties. In that section, we found that the passage models and the extra mortality hypotheses were the most important uncertainties. Here, these uncertainties are also important, particularly the extra mortality hypotheses. FGE, PREM, and prospective model alternatives were also important in these results, but were not identified as such in the previous section. However, we did note in Section 5.4 that these three uncertainties did cause a rather large change in absolute outcomes, but that these changes did not affect either the ranking of actions or the ability of actions to achieve a 0.70 expected ability to meet **all** survival and recovery standards. Because it is probably easier to achieve a single standard such as that used here than it is to achieve all three of the NMFS jeopardy standards, it is not surprising that there are more uncertainties that affect whether this easier standard is achieved.

In terms of future analyses and monitoring, the results suggest that many of the sensitivities could be ignored, because they appear to have relatively modest effects on the model results. Obviously, this interpretation applies only so long as the standard used here (spawning escapement for Marsh Creek exceeds 150 spawners 0.75 of the time over 24 years) is in fact a reasonable facsimile of what managers believe to be an acceptable performance standard. However, additional analyses (not reported here) suggest that other standards (100-year survival, 24-year and 48-year recovery) show similar patterns, and that Marsh Creek is indeed representative of the weaker index stocks. In addition, somewhat higher or lower standards (e.g., 0.8 or 0.7 probability of exceeding 150 spawners) do not seem to make much difference in the patterns noted above.

Effects of Interactions Among Uncertainties on Results for Marsh Creek

The results presented above show the effects of individual uncertainties, and thus do not consider the effects of possible interactions between uncertainties. To investigate this further, we separate the FGE, prospective model, and extra mortality results into CRiSP-T3 and FLUSH-T1/T2 combinations (Table 5.5-3). These results show whether these hypotheses are more sensitive under one passage model than the other. Column 2 of Table 5.5-3 displays the contribution of each hypothesis x passage model combination to the CRiSP-T3 or FLUSH-T1/T2 combinations (680 combinations for each), column 3 displays it for the 198 CRiSP-T3 combinations meeting the 0.75 standard, and column 4 displays it for the 99 FLUSH-T1/T2 combinations that met the 0.75 standard.

Results suggest that the sensitivities noted in Table 5.5-2 to FGE, prospective model, and extra mortality apply regardless of which passage model is used to generate results. The percentage of FLUSH-T1/T2 combinations that meet the standard appears to be more sensitive to FGE and prospective model (Alpha vs. Delta) hypotheses than CRiSP-T3 combinations.

Table 5.5-3: Distribution of passage model x FGE, Prospective model, and extra mortality hypotheses combinations.

Hypothesis	% of CRiSP-T3 or FLUSH-T1/T2 combinations	% of 198 CRiSP-T3 combinations that met 0.75 standard	% of 99 FLUSH-T1/T2 combinations that met 0.75 standard
FGE1	50	67	87
FGE2	50	33	13
Alpha prospective model	50	33	21
Delta prospective model	50	67	79
“BKD” extra mortality	40	0	1
Hydro extra mortality	40	99	99
Regime shift extra mortality	20	1	0

At least one uncertainty that makes a difference – FGE – is probably amenable to relatively short-term, inexpensive experiments. While the measured effectiveness of extended submersible screens differs depending on the method used (PIT tags, fyke nets, etc.), it should be possible to resolve the effectiveness of extended screens by comparing PIT tag and fyke net results in a low-spill year. Given relatively low snow pack so far this year, the spring of 1998 appears to be a year in which such comparisons can be done fairly easily.

The extra mortality hypothesis result is especially disturbing. Results presented in this section and in Section 5.4 indicate that there are several extra mortality hypotheses that cannot be true (or must have a weight of zero) in order to meet a 24-year standard standard. This seems to be true across passage models as well. In particular, if the "BKD" and the regime shift hypotheses are true, then this will prevent the short-term standard being met regardless of the hydro management decision taken. Note that this applies regardless of FGE, predator removal effects, and other sensitivities.

In contrast to FGE hypotheses, it seems unlikely that the extra mortality question can be resolved with conventional, small-scale experiments or monitoring. Perhaps the only way to resolve the extra mortality uncertainty is via large-scale adaptive management experiments, with large contrasts among experimental conditions. For example, one can envision experiments where large-scale management "variables" such as hatchery releases, transportation, or other factors are "switched" on and off in even and odd numbered years. We plan to have a workshop in 1998 to explore the possibilities of such experiments.

5.6 Sensitivity of Outcomes and Decisions to Weightings on Alternative Hypotheses

Research and adaptive management experts should focus on the uncertainties which have the greatest effects on decisions (based either on the ranking of actions or on their ability to meet some minimum criterion). We have scheduled a workshop in 1998 to talk about what kinds of research and experiments might be possible. PATH will also attempt to assign weights to those key uncertainties based on direct empirical evidence from the retrospective analysis, the stated biological rationale, and ecological principles. Because in many cases uncertainty exists because data are either lacking or are interpreted differently, weights will have to be assigned using a structured elicitation of the professional judgment and experience of PATH scientists and/or other experts. However, where strong evidence is lacking, such an elicitation may be unlikely to be able to significantly shift the weights of a set of alternative hypotheses. Once the weights are assigned, focus can then shift to defining those combinations of hypotheses that are internally logical and are consistent with specific retrospective aggregate hypotheses (Table 4.1-2).

The first step in assigning these weights is to establish just how sensitive the decision is to the weightings that are placed on alternative hypotheses. For example, we have already shown that the expected value for action A2 under CRiSP-T3 achieves a 0.7 threshold when the weighting placed on the hydro extra mortality hypothesis is 1 but does not meet this criterion when all of the extra mortality hypotheses are weighted equally. What we would like to know further is how the expected ability to meet survival and recovery standards changes as you change the probabilities within those two extremes. For example, is the 0.7 threshold reached when the weighting on the hydro hypothesis is 0.5? What is the critical weighting that must be placed on the hydro hypothesis before the 0.7 threshold is reached? This information can help to frame the assignment of weights by identifying what the critical weights are. If the expected value is greater than 0.7 as long as the weighting on the hydro hypothesis is greater than 0.6, then the key question to ask when assigning weights to the extra mortality hypotheses is "Is the weight on the hydro hypothesis relative to the "BKD" and regime shift hypothesis greater than or less than 0.6?" This is a much more specific question to answer than "What is the relative weight on the hydro hypothesis for extra mortality?" and can therefore help to focus the discussion when weights are assigned to alternative hypotheses. Precise framing of this discussion will be particularly important where there is disagreement among PATH scientists and agencies over what these relative weights should be.

The purpose of this section is to show how these kinds of analyses can be carried out for the combination of weightings on passage model and transportation assumptions (CRiSP-T3 or FLUSH-T1/T2) and extra mortality. We also look at the effects of different weightings assigned to drawdown assumptions on the expected ability of A3 to meet survival and recovery standards under FLUSH-T1/T2. These uncertainties were selected for these analyses because they were shown to affect either the relative ranking of actions or the ability of these actions to meet some absolute criterion. Moreover, these uncertainties will likely be the most difficult to assign weightings to because of firmly-held beliefs about interpretation of historical data and because extra (post-Bonneville) mortality and drawdown effects are the most difficult to measure. Again, we focus here on the effects of different weightings on the ability of actions to meet all three survival and recovery standards. Effects on individual jeopardy standards are shown in Appendix B.

Sensitivity to weightings on passage models and extra mortality hypotheses

Relative weightings on passage models (and their associated transportation assumptions) and extra mortality hypotheses are jointly represented in Figure 5.6-1. There has to be a great deal of certainty about passage model/transportation assumptions and the hydro extra mortality hypothesis before any action achieves an expected ability to meet survival and recovery standards greater than 0.7. For A1, this criterion is only met if we are absolutely certain (i.e., weight=1.0) that CRiSP-T3 and the hydro extra mortality hypothesis are correct. A2 only satisfies this criterion if CRiSP-T3 is assigned a weighting of 1, and the hydro hypothesis is assigned a weighting of at least 0.9. For A3, the 0.70 criterion is met when FLUSH-T1/T2 is assigned a weight of at least 0.8 and the hydro hypothesis is assigned a weighting of 1.0, or when FLUSH-T1/T2 is assigned a weighting of 1.0 and the hydro hypothesis is assigned a weighting of 0.8.

Sensitivity to weightings on drawdown assumptions

Results for the drawdown assumptions suggest that the length of the pre-removal period (PRER) and the length of the transition period (TJUV) are the most important effects in determining whether the expected ability to meet survival and recovery standards exceeds 0.7 for A3 under FLUSH-T1/T2 (Table 5.6-1). The most optimistic assumptions about these two factors (PRER1 – three-year pre-removal period, TJUVa – two-year transition period) must be assumed to be true with almost complete certainty before A3 meets the 0.7 criterion. If the weights placed on these hypotheses are high, then the assumptions about the equilibrium juvenile survival rate have virtually no effect. For example, if the weighting placed on PRER1 is 1.0, and the weighting assigned to TJUVa is also 1.0, then A3 meets the 0.7 criterion even if the optimistic value for equilibrated juvenile survival rate (EJUV2) has no chance of occurrng.

Table 5.6-1: Combinations of weights placed on PRER, EJUV, and TJUV hypotheses for which A3 under FLUSH-T1/T2 exceeds 0.7 expected ability to meet survival and recovery standards.

Weight placed on PRER1	Weight placed on TJUVa	Minimum weight that must be placed on EJUV2 before 0.7 criterion is met
0.8	1	0.9
0.9	0.9	0.8
	1	0.3
1.0	0.8	0.7
	0.9	0.2

	1.0	0.0

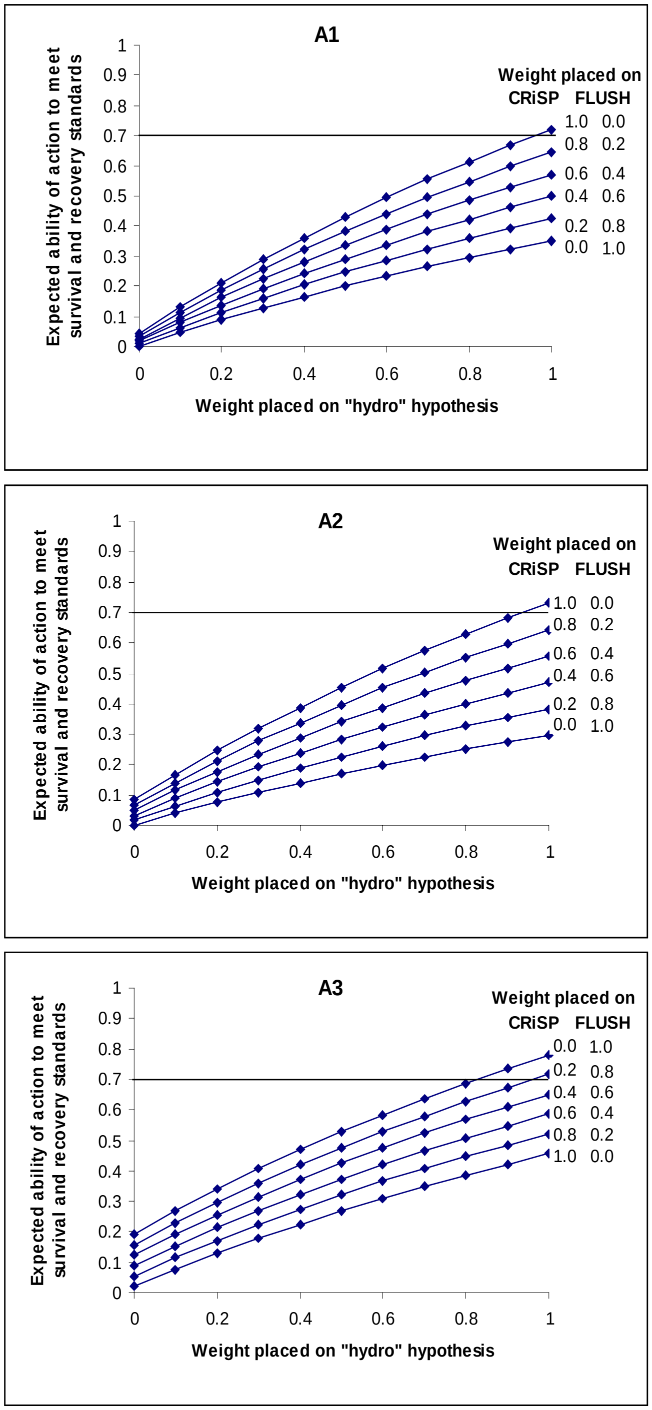


Figure 5.6-1: Sensitivity of expected ability to meet survival and recovery standards to relative weights placed on the "hydro" extra mortality hypothesis and the passage models. Note that the remaining weight placed on the extra mortality hypotheses (i.e., 1 –weight placed on hydro hypothesis) is divided evenly between the "BKD" and the regime shift hypotheses. For example, when the weight placed on the hydro hypothesis is 0.8, the weights placed on the "BKD" and the regime shift hypotheses are both 0.1.

Summary of Results

These results suggest that there will have to be considerable agreement on these key issues before one of the actions is clearly able to meet all of the survival and recovery standards with any degree of certainty. Given the strongly-held beliefs and the lack of data that give rise to the uncertainties, this consensus is not likely to be achievable without a well-planned experimental design. That is, the experimental design must provide a large enough signal to be able to differentiate among hypotheses. The design should be specifically directed towards answering questions about extra mortality and passage model assumptions. Furthermore, it suggests that significantly greater improvements in survival are required beyond those provided by the management actions analyzed here.

5.7 Other Performance Measures

The NMFS jeopardy standards are only one of a number of different measures of performance produced by PATH modeling analyses (see Table 3-1). Other measures include projections of spawner abundances, harvest rates and catch, Smolt-Adult survival rates, and various diagnostic outputs such as survival rates associated with passage through dams and Fish Travel Times. In this section, we briefly report on two of these: projected harvest rates, and Smolt-to-Adult survival rates.

Harvest Rates

Projected harvest rates are important for determining the economic implications of the actions under consideration. This is because harvest rate restrictions for ESA-listed stocks affect the ability to harvest non-listed co-migrating stocks. PATH modeling analyses produce mainstem and tributary harvest rates for all seven Snake River spring/summer chinook index stocks in every 5th year of the 100-year simulation period. Similar computations of harvest statistic for lower Columbia River stocks are planned, but have not been done. These values can be computed for each of the 5,148 aggregate hypotheses, but we have only done so for optimistic and pessimistic aggregate hypotheses in each action. The optimistic aggregate hypothesis was the one that maximized the average spawner abundance for that stock over the 100-year simulation period, while the pessimistic aggregate hypothesis minimized the average number of spawners. Although these are only a small subset of the entire set of aggregate hypotheses, a comparison of optimistic and pessimistic scenarios does give an indication of the range of outputs we can expect given the uncertainties in the analyses. The results presented here are based on the current harvest rate schedules for spring and summer chinook.

We present in this section only a few examples of the types of harvest statistics we could report. First, we show an example of the trends in harvest rates over time (Figure 5.7-1). These results are for mainstem harvest rates for a single stock (Imnaha), a single action (A1), and an optimistic aggregate hypothesis. Because the Imnaha stock is a mixed spring/summer stock, the harvest rate for that stock is estimated by averaging the spring and summer run harvest rates. Time trends in mainstem and tributary harvest rates for Imnaha and Marsh Creek stocks for all three actions are shown in Appndix B. We also show projected time trends in spawning escapement for these two stocks in Appendix B.

The figure can be interpreted as follows. In each year, we produce a frequency distribution of harvest rates rather than a single estimate. This distribution arises from the uncertainty and randomness in the biological and environmental processes underlying salmon population dynamics. To capture this uncertainty, we ran the life-cycle model one thousand times, with each run using a randomly selected value for factors such as future water flows and salmon productivity parameters (this approach is described in more detail in Chapter 3, Section 4.1, and Appendix A.1). Because each simulation run can result in a different harvest rate, the result is that we have a frequency distribution of 1000 possible harvest rates in each year.

This frequency distribution is represented in the figures below by a "box and whisker" plot. The bottom end of the lower line (i.e., "whisker") in each year represents the 10^{th} percentile of the distribution, which means that 10% of the 1000 possible harvests produced by the model for that one year are below that value, and 90% are above that value. For example, the 10^{th} percentile for the CRiSP-T3 output in Simulation Year 35 is 0.12. Therefore, 10% of the 1000 harvest rates simulated for year 35 were below 0.12. The lower end of the box in each year indicates the 25^{th} percentile, the upper end of the box represents the 75^{th} percentile, and the top of the upper line is the 90^{th} percentile. For the CRiSP-T3 output in year 35, this means that 25% of the 1000 harvest rates are below 0.23, 75% of the 1000 harvest rates are below 0.32, and 90% of the harvest rates are below 0.37.

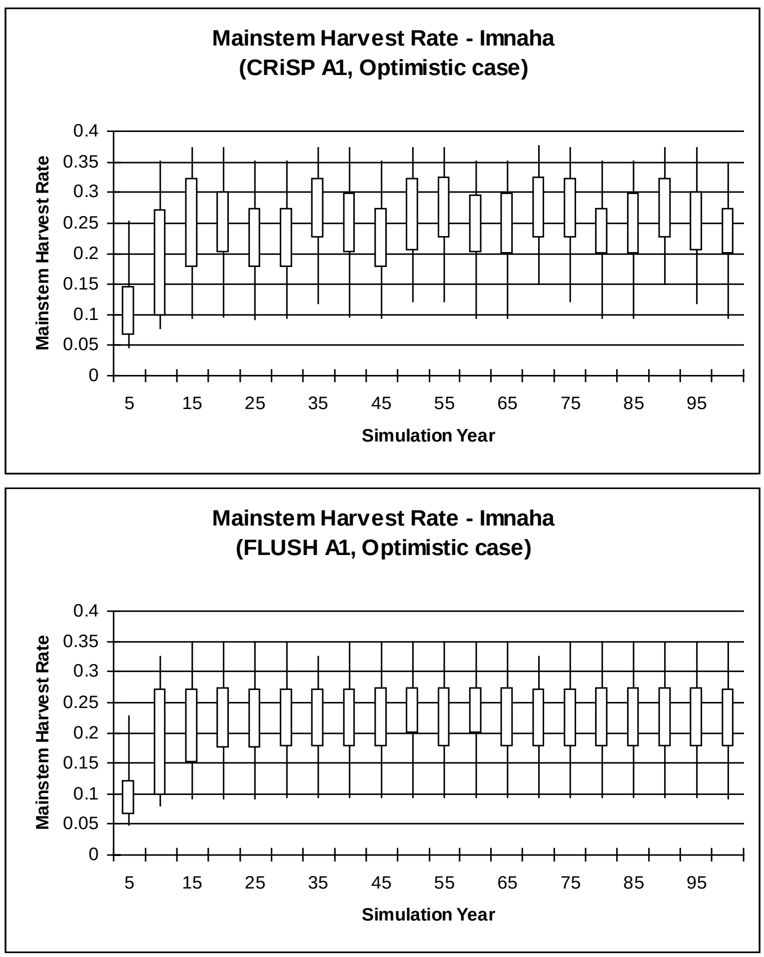
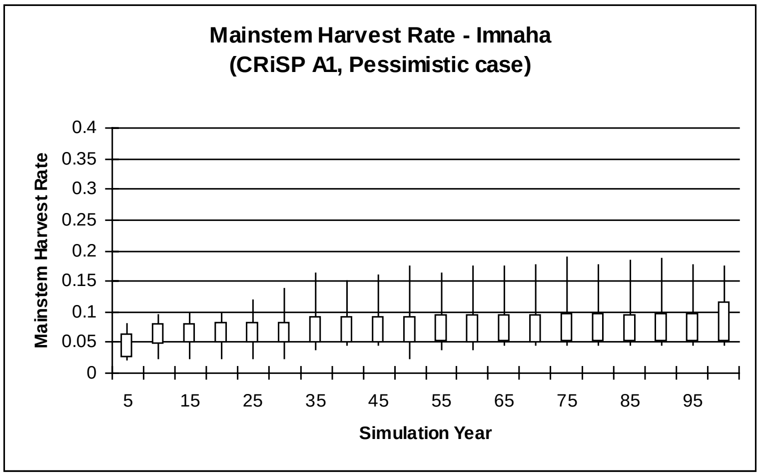


Figure 5.7-1: Mainstem harvest rates under A1 for the Imnaha stock of spring-summer chinook over 100-year simulation period for an optimistic aggregate hypothesis.

The dominant feature in these figures is the amount of variability in annual harvest rates. In most years, harvest rates can range from below 0.1 to above 0.35 for this particular scenario. Note also that these results are only for a single optimistic aggregate hypothesis, and that the degree of variability will be different for different aggregate hypotheses. To illustrate this, Figure 5.7-2 shows the mainstem harvest rates for Imnaha under A1, assuming a pessimistic aggregate hypothesis.



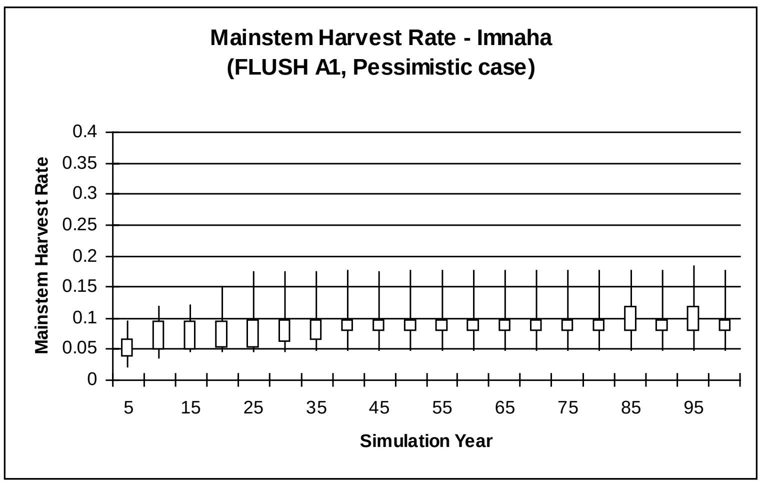


Figure 5.7-2: Mainstem harvest rates under A1 for the Imnaha stock of spring-summer chinook over 100-year simulation period for a pessimistic aggregate hypothesis.

Displays like Figures 5.7-1 and 5.7-2 clearly show trends over time and the amount of variability both within a particular aggregate hypothesis and across aggregate hypotheses. Such uncertainty is important to communicate to decision-makers and to others who will be using this information, such as the economic workgroup. However, these groups of people will also probably want some sort of summary statistic to allow quick comparisons of the harvest implications of different actions and uncertainties. As one example of a summary statistic, we simply calculate the average of the 50th percentile (median) harvest rates in each year over the first 50 years of the 100-year simulation period. Averages for Imnaha are shown in Figure 5.7-3. Another example would be to look at harvest rates in the first and second 25-year blocks. This would distinguish harvest rates experienced during any transition phase from those experienced when the stocks reach some equilibrium.

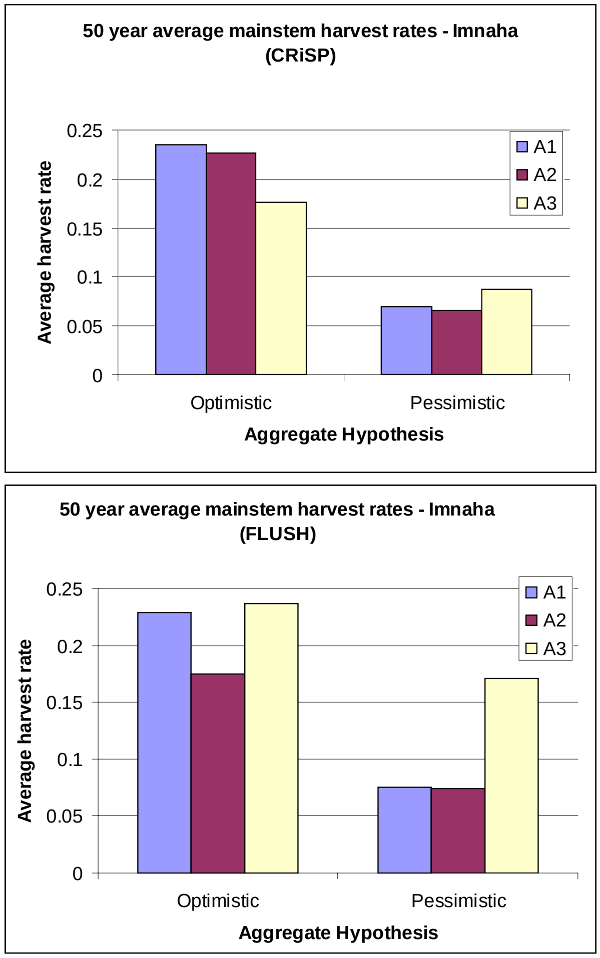


Figure 5.7-3: Fifty-year average mainstem harvest rates for Imnaha stock for CRiSP-T3 (top) and FLUSH-T1/T2 (bottom) model outputs.

Smolt to Adult Survival Rates

Smolt to adult survival rates (SARs) estimate survival rates of fish from the time they pass the upper-most dam as smolts to the time they return to that dam as adults. SARs are estimated in the PATH prospective analyses by relating model estimates of survival over some historical time period to empirical measurements of SARs during that time period, and then projecting that relationship into the future. The model calculates a median SAR over the 100-year simulation period.

Previous analyses by the PATH hydro workgroup suggested that an SAR of between 2 and 6% be used as an interim goal for evaluating whether alternative actions meet survival and recovery standards (Chapter. 6 in PATH FY1996 Retrospective Report). This interim goal was based on Snake River and Warm Springs SARs during periods when those stocks were believed to be healthy, and on theoretical SARs associated with a range of Snake River egg-smolt survival rates from the last three decades.

The purpose of this section of the report is to determine the range of SARs associated with meeting survival and recovery standards. To do this, we show a frequency distribution of the 100-year median SARs for those aggregate hypotheses in which the 100-year survival standard is met (i.e, the 100-year average probability of exceeding the survival escapement level for the 6^{th} best stock is greater than 0.7). We use the 100-year survival standard for this comparison because this time period corresponds to the 100-year time period used to calculate median SARs in the model.

Results show that SARs between 2 and 7% are associated with meeting the 100 year survival goal (Figure 5.7-4). This is very close to the interim goal identified by the PATH Hydro group. In general, CRiSP-T3 SARs for runs that met the 100-year survival standard were lower than FLUSH-T1/T2 SARs. Further analyses of projected SARs are presented in Appendix B.

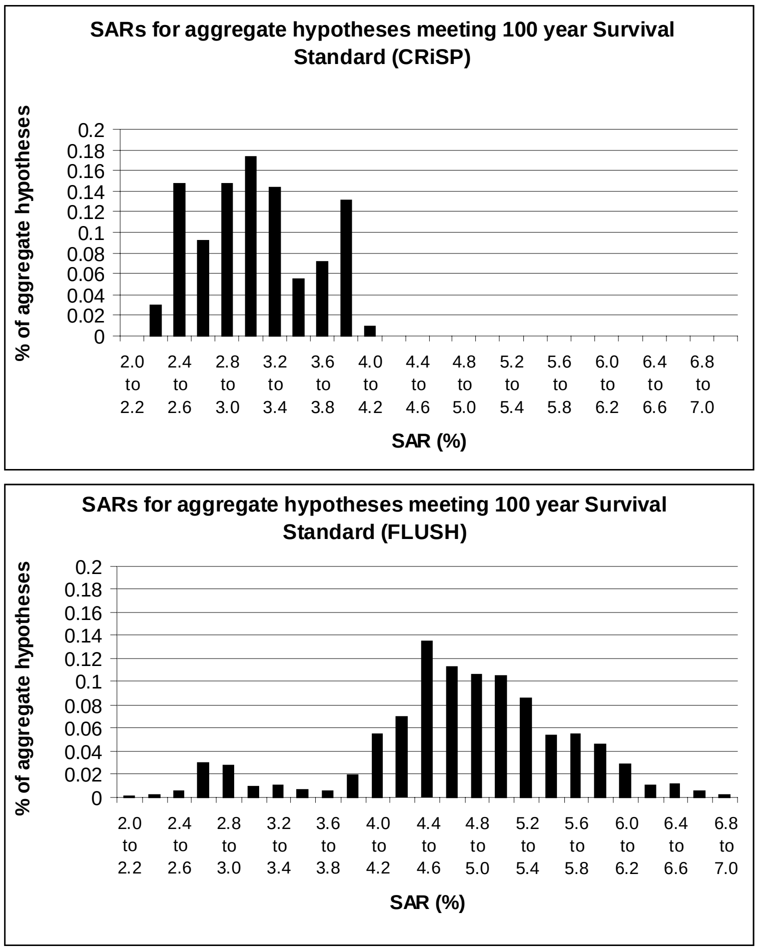


Figure 5.7-4: Frequency of distribution of average SARs for those aggregate hypotheses that met the 100-year survival standard.

Qualitative Performance Measures

In addition to quantitative performance measures, we would also like to look at how well the alternative management actions do in terms of qualitative measures of performance. Such qualitative measures can allow us to incorporate less quantitative but nonetheless important issues relating to the relative health of individual salmon populations, aquatic communities, and entire ecosystems. Many of these issues have been synthesized and discussed in the ISG report "Return to the River". One possible way for PATH to incorporate the Return to the River concepts is to construct a table like that shown in Table 5.7-1. For each indicator of a "normative river" identified in the Return to the River report, we (or members of the ISG) could use some sort of scale to score the conditions created by that action. For example, we can score the actions based on their effects on life history diversity in the Columbia River Basin, which was identified in the Return to the River report as a key element of a healthy ecosystem.

Table 5.7-1: Assessment of Alternative Management Actions based on Qualitative Performance Measures

(Scores of -1 to -3: management action leads to condition inconsistent with normative river (-3 is worst); +1 to +3: management action leads to condition consistent with normative river (+3 is best); 0: action has no effect on this measure).

Measures of Normative River (Based on review of Return to the River)	**Direction of positive effect**	**A1**	**A2**	**A3**
life history diversity within the basin	increased			
proportion of basin accessible to salmon	increased			
seasonal fluctuation in flow	increased			
daily fluctuation in flow	reduced			
smolt condition factor in estuary	increased			
mortality rate (in estuary)	reduced			
time for a downstream migrant to reach estuary	reduced			
mortality rate of downstream migrants	reduced			
water temperature - near shore in main channel	reduced			
area of emergent plant production in estuary	increased			
extent of the (marine) freshwater plume - spring	increased			
period of operation of bypass systems	increased			
mortality rate through bypass systems	reduced			
mortality rate in inter-dam reaches	reduced			
mortality rate in reservoirs	reduced			
fall water temperature in Snake river	reduced			
marine bycatch of immature spring chinook	reduced			
condition of mainstream rearing habitats	improved			
utilization of mainstream rearing habitats	increased			
proportion of stock artificially propagated	reduced			
number of hatchery fish released	reduced			
number of intakes with screens to reduce entrainment	increased			
entrainment mortality	reduced			

5.8 Passage Model Diagnostics

In Section 5.3, we noted that the two models used in these analyses represent fundamentally different approaches to estimating mortality through the juvenile migration corridor (see Sections 4.2.1 and A.2.1), and different assumptions about the relative survival of transported and non-transported fish in the ocean (see Sections 4.3.1 and A.3.1). This section compares diagnostic outputs for one of these differences (mortality through the migration corridor) to show how these assumptions differ between models.

In-River Survival

All passage model runs indicated that, for a given water year, in-river survival was higher under the drawdown scenario A3+EJUV2 at equilibrium than under the maximum transport scenario A2 (Figures 5.8-1 and 5.8-2). (Comparisons for A3+EJUV1 are not available at this time). For scenario A2, FLUSH predicted in-river survival ranging from 4-35%, depending upon historical TURB calibration assumptions, water year, PREM, and FGE assumptions, while the CRiSP model predicted in-river survival ranging from 28-50%. For scenario A3+EJUV2, CRiSP estimates ranged from 59-71%, while FLUSH model estimates ranged from 42-77%. The estimated improvement in reservoir survival with drawdown was greatest with FLUSH TURB1, TURB5, and TURB6 (38-53% in-river survival difference between A2 and A3+EJUV2) and lowest with the CRiSP model (19-26% in-river survival difference). FLUSH TURB4 predicted intermediate in-river survival improvements (approximately 34-40%). Results were relatively insensitive to FGE assumptions, but were quite sensitive to PREM assumptions, especially for FLUSH.

Total Direct Survival

Total direct survival of both transported and in-river migrants to below Bonneville Dam (i.e., not including post-Bonneville mortality of either transported or non-transported fish) was estimated using the PATH Hydro Work Group assumption that survival of transported fish in barges is 98% for scenario A2. Total direct survival under scenario A3+EJUV2 is identical to the in-river survival reported previously because no fish are transported. All passage model runs indicated that, for a given water year, total direct survival was higher under scenario A2 than under scenario A3+EJUV2 at equilibrium (Figures 5.8-3 and 5.8-4). (Comparisons for A3+EJUV1 are not available at this time). Total direct survival estimates of FLUSH (including all four TURB assumptions) and CRiSP overlapped, with both models estimating total direct survival between 70-95% under scenario A2 and 44-78% under scenario A3+EJUV2. CRiSP estimates of the difference between the two scenarios (9-25%) were lower than FLUSH estimates of the difference (14-40%) for nearly all water years.

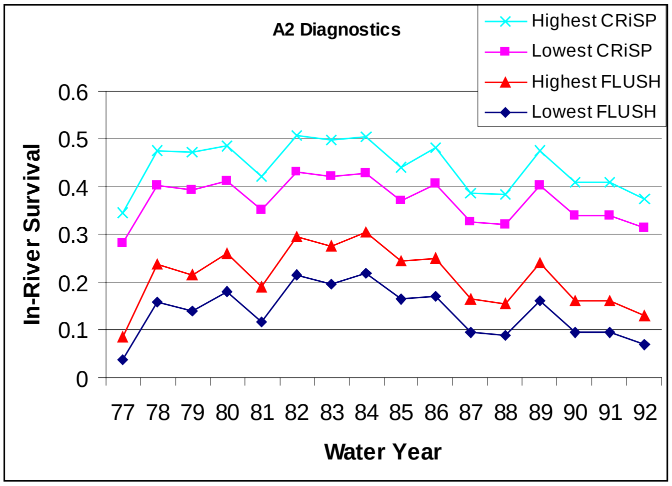


Figure 5.8-1: Range of in-river survival rates estimated by CRiSP and FLUSH for action A2.

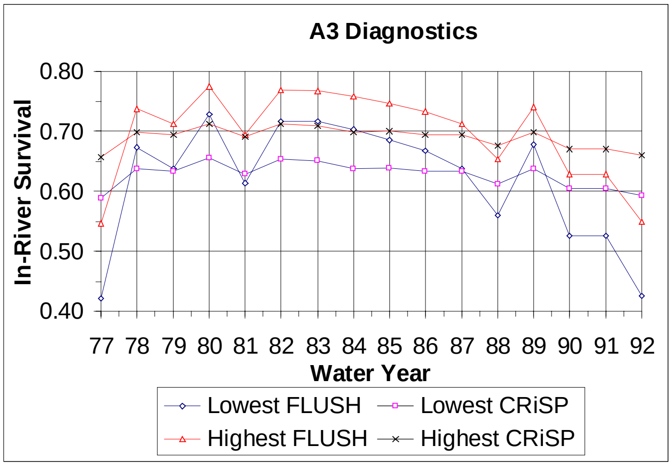


Figure 5.8-2: Range of in-river survival rates estimated by CRiSP and FLUSH for action A3.

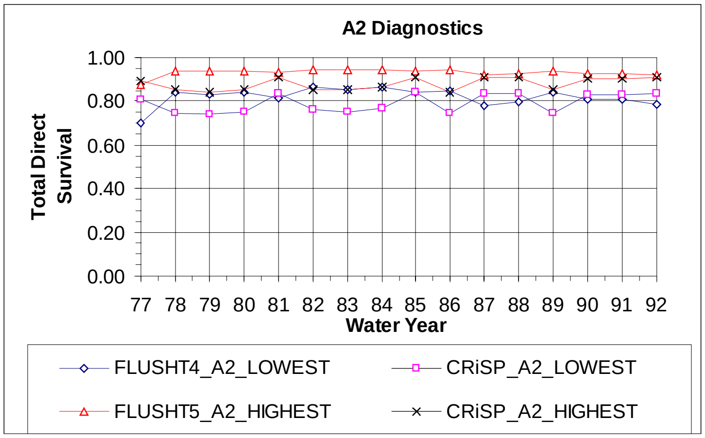


Figure 5.8-3: Range of total direct survival rates estimated by CRiSP and FLUSH for action A2.

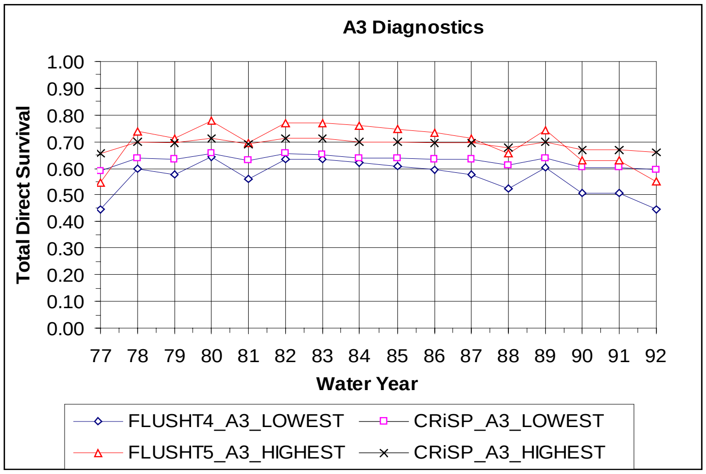


Figure 5.8-4: Range of total direct survival rates estimated by CRiSP and FLUSH for action A3.

6. Glossary

Aggregate hypothesis: A set of alternative hypotheses about all components of the system (stock productivity, downstream migration, marine survival, etc.).

Alpha Model: One of two models of salmon population dynamics used in the PATH prospective analyses. It is based on a Ricker stock-recruitment function, with additional terms for direct juvenile passage mortality and for remaining additional mortality from natural and anthropogenic causes. These two terms are assumed to be specific to the Snake River, Mid-Columbia, and Lower Columbia regions (see Delta Model).

Bacterial Kidney Disease (BKD): A serious salmonoid disease which can cause death or health impairment in both juveniles and adults.

Brood year (BY): The year in which a fish was propagated or spawned.

Coded wire tage (CWT): A tiny tag (1 x 0.25 mm) generally imbedded in the nose cartilage of fingerling or fry while the fish is still in the hatchery. The coded tag allows detailed data on brood year, date of release, and other information to be obtained when the fish is recaptured years later.

D: A parameter used in PATH modeling, equal to post-Bonneville survival of transported fish divided by post-Bonneville survival of in-river fish.

Delta Model: One of two models of salmon population dynamics used in the PATH prospective analyses. It is based on a Ricker stock-recruitment function, with additional terms for direct juvenile passage mortality, an extra mortality factor, and a common year effect. The direct and extra mortality terms are region-specific, while the common year effect acts on all regions (see Alpha Model).

Depensatory: A process that causes mortality rates to increase as abundance decreases. An example of a depensatory process is when the number of individuals removed by predation remains constant as the population abundance decreases.

Drawdown: Releasing water from a reservoir to lower its elevation, thereby reducing surface area and cross-section. This increases water velocity (at any given discharge) in comparison to velocities at higher water levels in the reservoir.

Evolutionary Significant Unit (ESU): A population or group of populations that is considered distinct (and hence a “species”) for purposes of conservation under the ESA. To qualify as an ESU, a population must: 1) be reproductively isolated from other conspecific populations; 2) represent an important component in the evolutionary legacy of the biological species.

Fish guidance efficiency (FGE): The percentage of juvenile fish approaching a turbine intake that are guided into facilities designed to bypass the turbine.

Fish Transit Time (FTT): The time it takes smolts to travel from the head of Lower Granite pool to the Bonneville tailrace.

In-river survival: Direct survival rate of non-transported smolts. The in-river survival rate is estimated from the top of the first reservoir encountered to below Bonneville Dam.

Jeopardy standards: Main performance measures used in this preliminary decision analysis to evaluate alternative management actions and assess sensitivity of outcomes to various uncertainties. The Jeopardy standards are a measure of spawning abundance relative to pre-defined thresholds that are associated with survival and recovery of endangered stocks (see Survival standard and Recovery standard).

Natural river: An option for implementing drawdown of dams where the reservoir is completely drained to create a free-flowing river. This is done either by removing the earthen embankments adjacent to the dam structure, or by building a channel around the dam. In either case, diversion of water around the dam structure results in loss of power-generating capability.

PIT tags: Passive Integrated Transponder tags are used for identifying individual salmon for monitoring and research purposes. The miniaturized tag consists of an integrated microchip that is programmed to include specific fish information. The tag is inserted into the body cavity of the fish and decoded at selected monitoring sites.

Recovery standard: The performance measure used to describe the effect of a certain hydrosystem action on the chance of a spawning stock for recovery; the fraction of simulation runs for which the average spawner abundance over the last 8 years of a 48-year simulation is greater than a specified level (different for each stream).

Ricker *a*: A measure of stock productivity at low levels of abundance.

Spillway crest: An option for implementing drawdown of dams where water levels in the reservoir are lowered to approximately 60-70% of the maximum level. Turbines could continue to operate under this drawdown configuration.

Smolt-to-adult return rate (SAR): Survival rates of fish from the time they pass the upper-most dam as smolts to the time they return to that dam as adults.

Survival standard: The performance measure used to describe the possibility of extinction; the fraction of time during many simulations that the spawning abundance of a stock is above a certain specified low threshold (150 or 300 spawners depending on the characteristics of the stock and the stream).

System survival: The number of in-river equivalent smolts below Bonneville Dam divided by the population at the head of the first reservoir.

Appendix A: Detailed Description of Alternative Hypotheses

"We use models to evaluate hypotheses in terms of their ability to explain existing data and predict other aspects of nature. We use models to combine what we know with our best guesses about what we do not know."

- R. Hilborn and M. Mangel, The Ecological Detective – Confronting Models with Data.

Table of Contents for Appendix A

A.1 Introduction

A.1.1 Structure and Purpose of This Appendix

The purpose of this Appendix is to provide more details about the alternative hypotheses outlined in Section 4 of the report. This Appendix is definitely a work in progress. Once completed, it will provide: 1) detailed biological rationale for alternative hypotheses and evidence against each hypothesis; 2) mathematical means of representing each hypothesis; and 3) ideas on how to resolve differences in future through research, monitoring or adaptive management actions. The first two pieces (biological rationale, evidence for/against and mathematical representation) are only partially complete. We have included comments on the rationale provided for alternative hypotheses, and in some cases, responses to these comments. These comments and responses are indented to separate them from the main text (responses are also italicized). Ideally, each key alternative hypothesis (i.e., those which have a significant effect on the decision) should account for the full set of available evidence. The assessment of alternative hypotheses against available evidence should be presented in a structured manner that facilitates scientific judgements on the appropriate assignment of weights. The third item (research, monitoring, and adaptive management approaches) has not yet been well developed, but will be in our final report.

The structure of the Appendix mirrors that of Section 4. Each sub-section in Section 4 (e.g., 4.2.3), which summarizes a set of alternative hypotheses, has a corresponding sub-section in Appendix A (e.g., A.2.3), which provides the above details for that set of hypotheses. As explained in Section 4.1, a particular combination of assumptions constitutes a prospective aggregate hypothesis, and several of these combinations could be consistent with one retrospective aggregate hypothesis.

A.1.2 Description of Modeling Approach

As we explained in Chapter 3, PATH modeling analyses of projected future effects of hydrosystem management actions are based on previous PATH analyses of historical data. In this section, we provide more details about the flow of information from the PATH retrospective analyses through retrospective and prospective modeling analyses to the performance measures of interest. Figure A.1-1 below, which is based on the simplified diagram shown earlier in Chapter 3 (Figure 3-1), lists and defines the parameters that are passed between the various models and analyses. References for more details on the parameters in each of the boxes are provided in Table A.1-1 below.

Table A.1-1: Reference for more details about parameter estimates used in the models.

Box in Figure A.1-1.	Section in this report with further details
1	PATH 1996 Conclusion Document PATH 1996 Retrospective Report
2	A.2.3
4 and 11	A.3.2
5	Beamesderfer et al. 1997[1]; Anderson and Hinrichsen 1997[2]
6 and 13	A.3.2
8	2
9	A.2.2; A.2.5; A.2.6
12	A.3.4, A.3.5, A.3.7

1 Spawner-Recruit Data for Spring and Summer Chinook Salmon Populations in Idaho, Oregon, and Washington. In: PATH Package #3 for the Scientific Review Panel. July 11, 1997.

2 Prospective analysis for the alpha model. In: PATH Package #4 for the Scientific Review Panel. August 5, 1997.

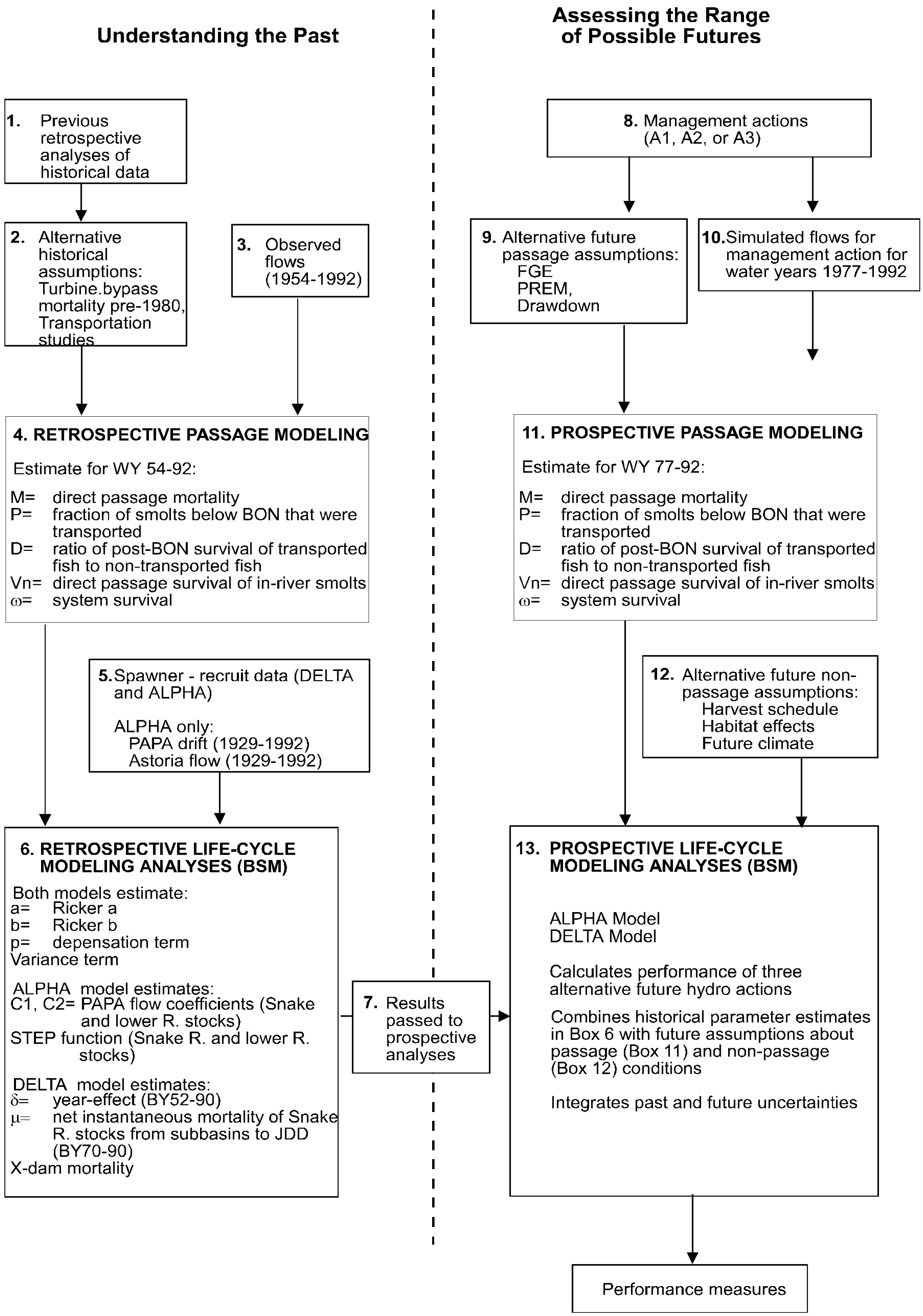


Figure A.1-1: Detailed diagram of analytical approach used in the decision analysis. See Table A.1-1 for sections providing further definition and explanation of parameters.

A.2 Uncertainties / Alternative Hypotheses Related to Downstream Passage

A.2.1 Passage Models

A.2.1.1 Structural Differences in Passage Models

The downstream section considered by the passage models is from the head of LGR pool to below BON dam. PATH has used two passage models in our analyses: CRiSP and FLUSH. The reason for using two models is that they represent different approaches to modeling reservoir mortality, dam passage mortality and transportation (barge) mortality. These are the three main components for which different hypotheses exist within these two models, and different ways of representing these hypotheses mathematically. Assumptions about the post-Bonneville effectiveness of transportation are also important (Section A.3.1); these assumptions are independent of the passage model used. In this analysis, particular passage models have been associated with specific transportation assumptions (i.e., FLUSH with TRANS1 or TRANS2; CRiSP with TRANS3).

The major uncertainties related to hydro actions involve the effects of the hydropower system on juveniles both during their downstream migration (including the effects of both passage through and transportation around the hydropower system) and any residual effects in the ocean. There is a hierarchy of three levels of hydro-related mortality processes as modeled by the two passage models, CRiSP and FLUSH (Figure 1 and Table A.2.1-1). Note that there may be uncertainties not only in the current values or structure of these components, but also in how these components are expected to respond to the hydro actions under consideration.

Level 1: At the most general level (Level 1) are the three major components of passage mortality in CRiSP and FLUSH: reservoir mortality, dam passage mortality, and transportation mortality.

Level 2: Level 1 components are composed of various sub-components of mortality. For example, reservoir survival is a function of flows, WTT, FTT, predation mortality, cumulative effects of stress, bioenergenic loss, injury, disease, and gas-related mortality; dam passage mortality is made up of turbine, spillway, and bypass mortality. These sub-components form the second level of hydro-related mortality. Because Level 2 mortality processes are modeled differently by the two passage models, uncertainties in these processes will also be different.

Level 3: The third level is represented by the precise nature of the functional relationships and equations that are used in the models to represent Level 2 processes of hydro-related mortality. For example, FLUSH uses a relationship between FTT and survival to model reservoir survival rates, while CRiSP uses a series of equations describing gas-related and predation mortality processes. The result is that the survival-FTT relationships are quite different between the two models (Figure 4.2-3).

Uncertainties associated with these Level 3 processes arise from three sources:

1. Uncertainties in the overall form of the relationship (e.g., is the relationship linear, exponential, logistic, etc.)

   These uncertainties should be considered explicitly where the main source of uncertainty is in the overall shape of functional relationship rather than in their exact parameterization, or where the effect of a management action is expected to alter the general form of relationships. Lack of data with sufficient contrast (e.g., insufficient transportation and reach survival studies at very long water and fish transit times) can result in this type of uncertainty. This type of uncertainty is also present when several functional relationships are linked to from a relationship of interest (e.g., a predation function and a gas mortality function combine to generate a reservoir survival-FTT relationship).

2. Uncertainties in the parameters that determine the shape of a given functional form.

   These uncertainties should be considered where the functional form of a particular relationship is generally accepted, but the exact shape of that relationship as described by its parameters or the underlying data set is uncertain, or where the management actions are expected to affect one or more individual parameters in a relationship. Note that uncertainties in the functional form of relationships can be represented by changes in equation parameters to the extent that the relationship is flexible enough to represent the full range of uncertainty in its shape (e.g., changes in the reservoir mortality vs. FTT relationship in FLUSH).

3. Uncertainties about which data sets or points should be used as the basis for estimating/calibrating functional relationships and their parameters.

   These uncertainties should be considered where there are multiple data sets that could be used to fit a relationship (e.g., reach survival studies), or where there is disagreement over particular years of data because of the experimental design, the exact methods used to collect the data, or the potential effects of confounding factors (e.g., low/high flow years, change in hydropower system operations or configuration). The TURB and TRANS alternative hypotheses are good examples.

In general, uncertainties in the decision analysis should be as specific as possible (i.e., Level 3) because (a) alternative hypotheses at this level of detail are easier to represent quantitatively; and (b) it will allow the effects of these uncertainties on the performance measures to be more precisely understood. Obviously, explicitly considering every uncertainty would lead to an overly complex analysis, and we already have over 5000 aggregate hypotheses. Consequently, it is important to only incorporate those uncertainties that are the most critical in determining the model outputs that affect the ranking of management actions. The sensitivity of the decision to other uncertainties that are not incorporated into the decision tree can be dealt with through sensitivity analyses (carefully scoped to avoid prolonged and unnecessary analyses). Note also that each Level 1 component of hydro-related mortality does not necessarily have to be represented in the decision analysis, and that there may be more than one Level 2 and 3 mortality process represented in the tree for a given Level 1 process. The point to emphasize is that only those uncertainties that have the most effect on the performance measures should be represented in the decision tree.

To focus the discussion of uncertainties in the models, we have listed in Table A.2.1-1 below some of the Level 2 and 3 mortality processes associated with each of the Level 1 processes (i.e., reservoir, dam passage, and transportation mortality). Parameter names are shown in *italics* and are based on descriptions of CRiSP 1.5 (CRiSP Manual dated March 20, 1996) and FLUSH 4.6 (Spring FLUSH Version 4.6, Draft Documentation. August 25, 1995). Page numbers in parentheses refer to these documents, except where otherwise noted. This list is not exhaustive but instead attempts to identify the most important processes in each model that corresponds to the different levels of uncertainty. We also refer to Chapter 6 of the PATH FY96 Retrospective Analyses.

Table A.2.1-1: Hierarchy of components of hydro-related mortality

Level 1 Component	Level 2 Component		Representation of Level 3 Component in CRiSP	Representation of Level 3 component in FLUSH
Reservoir Mortality	Flow		flows provided by Hydro reg. modeling group	
	WTT		models to try to use similar WTT	
	FTT		models to try to use similar FTT	
	Survival vs. FTT implicitly includes: gas mortality, predation, cumulative effects of migration delay (e.g., energy depletion, temperature effects, osmoregulation)	Functional form	not directly applicable to CRiSP	Survival vs. FTT relationships for four different TURB assumptions
		Parameters		*A, B* (Ch. 6, App. 5)
		Data	Consistent set of FTTs and survivals used by both models (Toole et al. 1997)	NMFS Reach Survival studies, 1970-1980 (excl. 1971-72) 1994-1996 NMFS PIT-tag data
	Gas mortality	Functional form	Gas mortality function	not directly applicable to FLUSH
		Parameters	a, N_S, N_C, b, H (p. 86)	
		Data	Dawley et al. (1976)	
	Predation	Functional form	Predation mortality function:	Effects of predator removal program implemented through adjustment to *ResSurv* (p. 14)
		Parameters	a, $P(E)$, u, (p. 66)	
		Data used to parameterize/ calibrate	JDA Predation studies, 1984-1986; Predator Index studies, 1990-1993 ; Rieman & Beamesderfer 1990; Predator consumption study (Vigg et al. 1991)	
Dam Passage Mortality	Turbine mortality	Functional form	Turbine survival function	Turbine survival function
		Parameters	p, y, S, F, m_{fo}, m_{tu}, fge (p. 135)	*Spillef, FGE, TurbSurv* (p. 15)
		Data used to parameterize/ calibrate	PATH Hydro Work Group Report (Oct. 1997)	

Level 1 Component	Level 2 Component		Representation of Level 3 Component in CRiSP	Representation of Level 3 component in FLUSH
Dam Passage Mortality (cont'd)	Turbine mortality (cont'd)	Other data sets available	Turbine survival (Ch. 6): PIT-tag estimates 1993, 1995; Balloon-tag estimates 1994-95; pre-1993 turbine survival studies (reviewed by Iwamoto and Williams 1993)	
	Spillway mortality[1]	Functional form	Spillway survival function	Spillway survival function
		Parameters	p, y, S, F, m_{sp} (p. 137)	*Spillsurv, Spillef* (p. 14)
		Data		
	Bypass mortality[1]	Functional form	Bypass survival function	Bypass survival function
		Parameters	p, y, S, F, m_{by}, fge (p. 136)	*Spillef, FGE, Bypasssur* (p. 15)
		Data		
	Spill Efficiency	Functional form	y = Seven possible equations	Currently assumed 1:1 at all projects except Dalles (function of spill proportion at Dalles).Other assumptions to be explored in sensitivity analyses (see Section A.2.4)
		Parameters	a, b (p. 119)	
		Data used to parameterize	Hydro acoustical studies	
		Other data avail.	Ch. 6 Appendix 4: Radio telemetry, experimental digitally-coded tags	
	FGE	Data used to parameterize	Krasnow et al., 1997	
		Other data avail.	Ch. 6: Fyke net estimates, 1989-93	
Transport. Mortality	Barge mortality[2]	Functional form	Constant	
		Parameters	$p, y, S, F, m_{fo}, m_{by}, fge, m_{tr}$ (p. 136)	
		Data avail.	No direct measurements of barge mortality	
	Total mortality		See discussion under Section A.3.1 below	
	Proportion transported	Function of flows, FTOT rules		

1. Chapter 6 of FY96 Retrospective Report suggests that current values of these mortality rates are known with relative certainty (2%).
2. Barge mortality is assumed to be low (2-4%, Chapter 6 of FY96 Retrospective Analyses), but there are few good studies.

Alternative Hypotheses for Reservoir Survival

Assumptions about the processes driving reservoir survival are a major source of differences between the two passage models. Both models have used a common set of reach survival data for comparing their predictions to empirical information (Table A.2.1-2). Alternative hypotheses for reservoir survival can be represented as different values of relevant parameters in the passage models. For FLUSH, these parameters are *A* and *B* in the reservoir survival: FTT relationship; relevant parameters in CRiSP include the predator activity coefficient *a*, and the predator density *p(E)* (Table A.2.1-1).

Alternative Hypotheses for Post-BONN Survival of Transported Fish

CRiSP and FLUSH estimates of passage survival are each associated with different assumptions about the post-Bonneville relative survival of transported and non-transported fish. These assumptions are documented in detail in Section A.3.1, but the main differences can be summarized as:

1. Differences in retrospective values for V_n (direct passage survival of smolts passing in-river);
2. Differences in the set of years for T/C information used to estimate future T/C's and corresponding *D*'s (i.e., either 1971-1989 (FLUSH), or just the post-1980 years with transportation studies (CRiSP); and
3. Differences in the smoothing procedure used (i.e., smoothing (T/C) estimates (FLUSH) or smoothing (averaging or regressing) the *D* estimates (CRiSP).

Differences in V_n are caused primarily by differences in the relationships used to relate fish transit time to fish survival.

A.2.1.2 Detailed Diagnostics to Examine Passage Model Differences in Retrospective Simulations

The FLUSH and CRiSP models contain a series of linked hypotheses regarding direct survival from the head of Lower Granite pool to below Bonneville Dam. In this section, the critical hypotheses are examined first from the perspective of dam passage and second from the perspective of reservoir passage. Note that in this section, Ayear@ refers to the juvenile migration year, which is two years later than the brood year. These comparisons are performed for the historical or retrospective runs of the models, for migration years 1970 to 1997, which we call Scenario A0. The detailed outputs presented below provide examples of differences in behavior of the two passage models, when simulating the 1970 to 1997 period. This is the most comprehensive set of diagnostics produced to date for these two models. Considerable effort has been expended to maximize the consistency of the assumptions made by the two models for retrospective runs. Though this has resulted in a great deal of convergence in the retrospective simulations, some significant differences do remain. We are still exploring the reasons for these differences. *All of the figures referred to below are grouped together at the end of this section. Readers wishing to review critiques of each model should proceed to the text which follows these figures.*

Dam Passage Routing and Survival

Approach To evaluate CRiSP and FLUSH estimates of dam passage mortality, all reservoir survival functions in each model were set to a constant 100% survival. Therefore, the only mortality in these diagnostic runs resulted from dam passage.

Summary Estimates of total dam passage survival ranged from 10 - 77%, varying by year, TURB assumption (for years prior to 1980), and passage model (Figures A.2.1-1a-c, A.2.1-2a-c, A.2.1-3).

Annual Variation: On average, dam passage survival estimates were lower and more variable prior to 1980 than in more recent years. For TURB4 and TURB5 assumptions, 1973 and 1977 survival estimates were much lower than estimates in other years.

TURB Assumption Effects (Pre-1980): Highest dam passage survival estimates with least variation resulted from TURB1 assumptions; the opposite situation resulted from TURB4 assumptions; and TURB5 assumptions resulted in intermediate estimates.

Passage Model Assumptions: Both FLUSH and CRiSP models estimated nearly identical temporal patterns, but the magnitude of survival estimates differed markedly (up to 14% absolute difference) among the two models for some years and some TURB assumptions. For 1980-**1997[?]**, FLUSH estimates were consistently lower than CRiSP estimates. Prior to 1980, relative performance of each model varied by year and TURB assumption. Differences among passage models were not expected and these differences do not represent specific alternative hypotheses. Perhaps the best way to think of the differences in dam passage survival among CRiSP and FLUSH models is as estimation error.

Detailed Results: 1970-1979 We examined three assumptions regarding bypass and turbine survival prior to 1980 (TURB 1,4, and 5) (see Section 4.2.3 for an explanation of TURB assumptions). We expected dam passage survival estimates to differ by TURB assumption but, for a given TURB assumption, to be nearly identical in the FLUSH and CRiSP models. This did not occur. Dam passage survival estimates through all projects ranged from 10-72% (approximately 75-95% mean per-project survival) and varied by year, TURB assumption, and model (Figures A.2.1-1a-c, A.2.1-2a-c, A.2.1-3).

Dam passage survival estimates were generally highest for TURB1 (46-72%, average 59% FLUSH and 60% CRiSP). Both models estimated the highest survival in 1971 and lowest in 1977 and generally paralleled each other in relative ranking of years. CRiSP estimates were <1 to 6% higher than FLUSH estimates in 1970-72 and 1977-80, while FLUSH estimates were <1 to 5% higher than CRiSP estimates in 1973-76. Disparity among models ≥5% occurred in 1970, 1974, and 1975. Possible reasons for the greater disparity among models in these years are being explored. Years with more daily variation in spill should have the largest differences between CRiSP and FLUSH, since CRiSP operates on a daily time step and FLUSH on a seasonal time step.

Dam passage survival estimates were generally lowest for TURB4 (10-72%, average 44% FLUSH and 43% CRiSP). Both models estimated the highest survival in 1971 and lowest in 1977 and generally paralleled each other in relative ranking of years. CRiSP estimates were 4-10% higher than FLUSH estimates in 1970-72 and FLUSH estimates were 2-14% higher than CRiSP estimates in 1973-76 and 1978. Estimates were nearly identical in 1977 and 1979. Disparity among models ≥5% occurred in all years except 1971 and 1977-79. Possible reasons for the great disparity among models in these years (and with this TURB option) are being explored; the most likely explanation is that the TURB4 rules were either implemented differently, or have a greater impact due to time step differences between the two models.

TURB5 dam passage survival estimates were generally intermediate to those of the other turbine and bypass assumptions (36-72%, average 55% FLUSH and 62% CRiSP). As with other TURB assumptions, the relative ranking of years was generally parallel in each model. Unlike the other TURB assumptions, CRiSP estimates were consistently 1-14% higher than FLUSH estimates in every year. Disparity among models ≥5% occurred in all years except 1971 and 1975-77.

Detailed Results: 1980-1992 Specific hypotheses were not proposed for post-1979 dam passage, so estimates did not vary by TURB assumption and nearly identical FLUSH and CRiSP estimates were expected. Passage model diagnostics indicate that 1980-1992 dam passage routing and survival in the two models were similar, but not identical. Dam passage survival estimates ranged from 50-66%, with survival highest in the more recent years (Figures A.2.1-1a-c). Mean per-project survival ranged from approximately 92-94% (Figures A.2.1-2a-c). CRiSP estimates were higher than FLUSH estimates by <1 to 6.4% (mean difference 4%) in all years (Figure A.2.1-3). Disparity among models ≥5% occurred in 1988 and 1990-92.

Detailed Results: 1993-1997 These juvenile migration years were not included in the prospective analysis because adult returns and run reconstructions are not yet complete. However, this information is useful for evaluating model performance We implemented two FGE hypotheses (FGE1 and FGE2), which are related to assumptions about guidance of extended-length screens at three projects in 1996 and/or 1997 (Section 4.2.2).

The 1993-1997 estimates were generally higher than estimates from earlier years, ranging from 63-77%. As with the 1980-1992 estimates, FLUSH 1993-1997 estimates were 6-11% lower than CRiSP estimates each year. The greatest discrepancy was associated with 1994. The FGE assumptions affect the proportion of fish transported, which combines with transportation assumptions (TRANS1, TRANS2, and TRANS3) to affect prospective runs.

Reservoir Passage Survival

To evaluate CRiSP and FLUSH estimates of reservoir mortality, all dam passage survival functions in each model were set to a constant 100% survival. Therefore, the only mortality in these diagnostic runs resulted from reservoir passage. In this section, CRiSP and FLUSH model performance is compared. The underlying functions that contribute to reservoir survival were examined in turn and an attempt was made to document the key hypotheses responsible for different model behavior.

Water and Fish Travel Time

FLUSH and CRiSP water travel time (WTT) estimates closely paralleled each other over time, but CRiSP estimates were consistently slower than FLUSH estimates (Figures A.2.1-4 and A.2.1-5). Discrepancies ranged from 0.4 to 5.4 days, averaging approximately 3.6 days. The greatest discrepancies (>4.9 days) were associated with 1970, 1972, 1984, and 1991 estimates.

The relationship between water travel time and fish travel time (FTT) appeared to be similar in FLUSH and CRiSP (Figure A.2.1-6). While each model uses a fairly complex set of reach-specific equations, the relationships for the full hydropower system can be approximated with the following linear regressions:

CRiSP: $FTT = 9.452 + 0.894*WTT$ $r^2 = 0.971$

FLUSH: $FTT = 4.039 + 1.099*WTT$ $r^2 = 0.986$

While the slopes are similar, the intercepts differ by over five days. On a yearly basis, travel time predictions of the two models were nearly identical for the two longest WTT years (1973 and 1977), but FLUSH estimates were 1.8-6.8 days faster than CRiSP estimates (average 4.4 days) in all other years (Figure A.2.1-7). The greatest discrepancies (>5.9 days) were associated with 1970, 1972, 1983-86, and 1991 estimates.

In summary, the greatest difference in travel time among the two models (average 3.6 days) appears to be associated with estimation of water travel time. However, an additional discrepancy associated with conversion of WTT to FTT increases the average difference in estimates to 4.4 days.

Reservoir Survival In Relation to Fish Travel Time

Two competing hypotheses are embodied in the FLUSH and CRiSP models: FLUSH estimates reservoir survival directly from fish travel time while CRiSP estimates it indirectly from exposure time to predation and total dissolved gas levels.

In FLUSH, the relation between fish travel time and reservoir survival varied with each TURB function because the TURB assumptions affect the partitioning of reservoir and dam mortality in the reach survival estimates to which FLUSH is calibrated (Figure A.2.1-8). The reservoir survival functions estimated with TURB1 and TURB5 were very similar, with a fairly steep exponential decline in survival associated with increased travel time over the range of estimated travel times. In contrast, the TURB4 survival function declined less steeply with travel time and generally resulted in higher survival estimates at a given travel time. When displayed as survival per day, all three FLUSH relationships showed a decreased reservoir survival rate as cumulative travel time through the system increased (Figure A.2.1-9). In other words, in the FLUSH reservoir survival functions, both the cumulative mortality and the daily mortality rate increased with cumulative exposure to the hydropower system.

In CRiSP, reservoir survival did not vary with TURB assumption because the predation and gas mortality functions do not use reach survival estimates in their calibrations. A clear relation between travel time and reservoir survival is not obvious in CRiSP because of the confounding effects of predation and gas-related mortality (Figure A.2.1-8). In some of the earlier years, large amounts of spill occurred with no mitigation from spill deflectors, presumably causing significant gas-related mortality in CRiSP, and the large variability in reservoir survival for a given FTT. We feel fairly confident in ascribing this cause to the results, because the CRiSP reservoir survival vs. FTT function showed very little variability in *prospective* simulations under management action A2, which has little spill (unpublished results, to be included in our final report). We should however confirm this by alternately disabling predation or gas-related mortality functions in CRiSP in the retrospective simulations (Figure A.2.1-10 [Note: CRiSP data not received yet]). Reservoir survival per day remains constant in CRiSP provided that there is no change in temperature. Since the predation rate is temperature-dependent, mortality tends to increase over the spring migration season as the river warms up (Figure A.2.1-11, not received yet).

CRiSP reservoir survival estimates were generally lower than FLUSH estimates between 1970-1976 and were generally higher than FLUSH estimates from 1977 to 1996 (Figures A.2.1-12 and A.2.1-13). Exceptions were a higher CRiSP estimate in 1973, higher FLUSH TURB4 and TURB5 estimates in 1974-76 and 1982-83, and higher FLUSH TURB4 estimates in 1994-95. Discrepancies among models and TURB assumptions ranged from <1% to 53%. At least one FLUSH TURBx estimate varied by more than 5% from CRiSP estimates in all years except 1982. On average, CRiSP estimates were 6.8% higher than FLUSH TURB1 and 3.9% higher than FLUSH TURB5 estimates, while FLUSH TURB4 estimates were 2.8% higher than CRiSP estimates. The greatest discrepancy was associated with 1971: FLUSH estimated 60-66% reservoir survival while CRiSP estimated 13% reservoir survival.

Upstream vs. Downstream Reach Survival (Dam + Reservoir Passage)

If the main difference between the CRiSP and FLUSH reservoir survival vs. FTT relationships is the assumption of an increasing mortality rate with time spent in the system in FLUSH (and the corresponding alternative assumption of a temperature-dependent but more constant rate in CRiSP), then differences in reach survival estimates between the two models should be greater in the lower river reaches than in the upper reaches. To evaluate model differences among upriver and downriver reaches, reservoir survival was estimated from Lower Granite (LGR) reservoir through John Day (JDA) reservoir and from John Day dam through Bonneville (BON) dam. The influence of these reaches on overall FLUSH vs. CRiSP in-river survival estimates was inferred from an exercise in which each model=s LGR-JDA estimate was paired with the other model=s JDA-BON estimate.

Survival estimates for the JDA-BON reach were generally about twice as high as estimates for the LGR-JDA reach (Figures A.2.1-14a to A.2.1-14c). For TURB1 assumptions, absolute differences among models also averaged twice as high (range <1% to 48%; average 3% higher in CRiSP) for the LGR-JDA reach than for the JDA-BON reach (range <1% to 16%; average 1.5% higher in FLUSH) (Figure A.2.1-15a). Absolute differences of ≥10% were associated with estimates in 17 years for the LGR-JDA reach but in only one year for the JDA-BON reach.

When the lower river estimate of one model was paired with the upper river of the other model, there was a very small difference in overall in-river survival estimates during most years (Figure A.2.1-16a). However, for the 1982-84 and 1993-96 periods, the FLUSH upstream estimates coupled with CRiSP downstream estimates more closely resembled the original CRiSP estimates than those of FLUSH. These were years in which the downstream estimates were nearly identical in each model and all of the total in-river estimates were similar.

The combination of the above observations suggests that differences among CRiSP and FLUSH model estimates for the JDA-BON reach explain only a small proportion of the differences among CRiSP and FLUSH total in-river survival estimates.

Comparison With Empirical Reach Survival and PIT-tag Detection Estimates

The PATH Hydro Work Group=s Data Subcommittee identified 19 historical reach survival estimates that were potentially useful for evaluating performance of CRiSP and FLUSH passage models in representing historical passage survival (Table A.2.1-2). Additionally, the subcommittee identified four years of LGR-McNary PIT-tag detection (not survival) estimates that would also be useful for evaluations if there were sufficient time.

Table A.2.1-2: Hydropower reach survival and travel time estimates through longest reaches for which methods described in report can be applied.

Year	Survival	Travel Time (Days) *Distance km)*	Median Arrival Date	Reach	Reference/Comments
1966	0.63	10.0 *229.4*	120.0	IHR Arrivals - TDA Arrivals (Includes free-flowing river through present JDA pool)	Survival: Raymond (1979) Table 11; Travel Time and Arrival Date: Ebel et al. (1973) Table 13.
1967	0.64	9.0 *229.4*	132.0	IHR Arrivals - TDA Arrivals (Includes free-flowing river through present JDA pool)	Survival: Raymond (1979) Table 11; Travel Time and Arrival Date: Ebel et al. (1973) Table 13.
1968	0.62	17.0 *229.4*	139.0	IHR Arrivals - TDA Arrivals	Survival: Raymond (1979) Table 11; Travel Time and Arrival Date: Ebel et al. (1973) Table 13.
1969	0.47	N/A *280.8*	N/A	LMN Arrivals - TDA Arrivals	From multiplication of first two columns in Raymond (1979) Table 11; value reported in third column is incorrect.
1970	0.22	N/A *326.8*	N/A	LGS Arrivals - TDA Arrivals	Raymond (1979) Table 11
1971	0.48	N/A *97.4*	N/A	LGS Arrivals - IHR Arrivals	Raymond (1979) Table 11

Year	Survival	Travel Time (Days) *Distance km)*	Median Arrival Date	Reach	Reference/Comments
1972	0.16	N/A *326.8*	N/A	LGS Arrivals - TDA Arrivals	From multiplication of first two columns in Raymond (1979) Table 11; value reported in third column is incorrect. Slotted bulkheads at LGS.
1973	0.05	22.0 326.8	157.0	LGS Arrivals - TDA Arrivals	Survival: Raymond (1979) Table 11. Travel Time and Arrival: Sims and Ossiander (1981) Table 3.
1974	0.36	12.0 *326.8*	134.0	LGS Arrivals - TDA Arrivals	Survival: From multiplication of first two columns in Raymond (1979) Table 11; value reported in third column is incorrect. Travel Time and Arrival: Sims and Ossiander (1981) Table 3.
1975	0.25	12.0 *326.8*	150.0	LGR Arrivals - TDA Arrivals	Survival: From multiplication of first two columns in Raymond (1979) Table 11; value reported in third column is wrong. Travel Time and Arrival: Sims and Ossiander (1981) Table 3.
1976	0.30	15.0 *348.0*	128.0	LGR Arrivals - JDA Arrivals	Survival : Sims et al. (1977) Table 4. Travel Time: Sims et al. (1977) Table 1. Note: Sims and Ossiander (1981) 17 day estimate (Table 3) is extrapolated to TDA. Arrival: S&O (1981) LGR date + 15.
1977	0.03	36.0 *348.0*	166.0	LGR Arrivals - JDA Arrivals	Survival : Sims et al. (1978) Table 2. Travel Time: Sims et al. (1978) Table 1. Note: Sims and Ossiander (1981) 39 day estimate (Table 3) is extrapolated to TDA. Arrival: S&O (1981) LGR date + 36.
1978	0.47	11.0 *348.0*	131.0	LGR Arrivals - JDA Arrivals	Survival: Raymond and Sims (1980) Table 6, adjusted for LGS transport as in text. Travel Time: Sims et al. (1983) Table 4. Note: Sims and Ossiander (1981) 13 day estimate (Table 3) is extrapolated to TDA. Arrival: S&O (1981) LGR date + 11
1979	0.34	13.0 *348.0*	138.0	LGR Arrivals - JDA Arrivals	Survival: Raymond and Sims (1980) Table 6, adjusted for LGS transport as in text. Travel Time: Raymond and Sims (1980) Table 1 and Sims et al. (1983) Table 4. Note: S & O (1981) 15 day estimate (Table 3) is extrapolated to TDA. Arrival: S&O LGR date + 13
1980	0.36	12.0 *348.0*	127.0	LGR Arrivals - JDA Arrivals	Survival: Sims et al. (1981) Table 2. Travel Time: Sims et al. (1981) Table 4 and Sims et al. (1981) Table 1. Note: Sims and Ossiander (1981) 13 day estimate (Table 3) is extrapolated to TDA. Arrival: Sims et al. (1981) [approx. 115] + 12.
1993	0.74	14.4 *91.0*	121.1	Nisqually John Landing - LGS Tailrace	Survival, Travel Time, and Arrival Date: Muir et al. (1997 - DRAFT), using weighting described in this report. Note: Survival estimates in Iwamoto et al. (1994) have been revised.

Year	Survival	Travel Time (Days) *Distance km)*	Median Arrival Date	Reach	Reference/Comments
1994	0.62	14.7 *143.0*	127.3	Silcott Island - LMN Tailrace	Survival, Travel Time, and Arrival Date: Muir et al. (1997 - DRAFT), using weighting described in this report. Note: Survival estimates in Muir et al. (1995) have been revised.
1995	0.63	17.0 *274.0*	130.0	Port of Wilma - MCN Tailrace	Survival, Travel Time, and Arrival Date: Muir et al. (1997 - DRAFT), using weighting described in this report. Note: Survival estimates in Muir et al. (1996) have been revised.
1996	0.58	9.3 *225.0*	?	LGR Tailrace - MCN Tailrace	Survival, Travel Time, and Arrival Date: Muir et al. (1997 - DRAFT), using weighting described in this report.

Comparison With Empirical Reach Survival Estimates

The 19 reach survival estimates included 2-6 projects with various dam configurations and reservoir conditions, which resulted in a range of survival estimates (Figure A.2.1-17). The original observations can be displayed in various ways (e.g., Figures A.2.1-18a to A.2.1-18d), but interpretation is difficult because dam and reservoir effects are confounded. CRiSP and FLUSH estimates based on the various TURB assumptions partitioned the reservoir and dam effects differently, and results of each assumption are compared. Additionally, model estimates varied by whether the models were used in the Aprospective mode@ (i.e., estimates were based only on the types of information that are available for prospective analysis, such as monthly or bi-monthly flows and seasonal project operations) or the Aretrospective mode@ (i.e., estimates were based on additional information, such as daily flows, temperatures, fish distributions, and project operations that are available from the historical record).

AProspective Mode@ Estimates

The fit, expressed as r^2, of each model under each TURB assumption to the empirical estimates is displayed in Table A.2.1-3. [Note: 1966-69 not provided for FLUSH, so n=15 for FLUSH and n=19 for CRiSP]. The FLUSH travel time estimates fit more closely than the CRiSP travel time estimates, the TURB1 and TURB5 survival estimates had very similar fits, and the CRiSP TURB4 estimates fit more closely than the FLUSH TURB4 estimates. FLUSH fits were nearly identical under each TURB assumption because the FLUSH model was re-calibrated for each assumption. Differences among TURB assumptions was greater for CRiSP, which was calibrated with independent data. The TURB4 assumption provided the closest fit to historical data with the CRiSP model.

When compared with the empirical estimates on an annual basis, the years with greatest discrepancies were dependent upon model and TURB assumption. For years prior to 1980 (Figures A.2.1-19a to 19c), FLUSH discrepancies >10% occurred in 1978-80 for TURB1 and TURB5 and in 1970, 1978, and 1979 for TURB4. [Note: FLUSH fit to 1966-1969 is unknown. This is a period of great interest, since it preceded most of the Snake River dams.] CRiSP discrepancies >10% for this time period occurred in 1972, 1973, 1976, and 1977 for TURB1 and TURB5 and in 1976 and 1978 for TURB4. For years since 1993 (Figures A.2.1-20a to 20c), when empirical estimates are based on PIT-tag mark-recapture studies, FLUSH discrepancies >10% occurred in 1990 and 1995 for TURB1 and TURB5 and in 1995 only for TURB4. CRiSP discrepancies >10% occurred in 1994 for all TURB assumptions.

Table A.2.1-3: Summary of linear regression fits (r^2) to empirical reach survival estimates for CRiSP and FLUSH model estimates. FLUSH retrospective mode implemented observed fish travel times (FTT), so comparisons are not applicable. CRiSP comparisons are for 1966-1980 and 1993-1996 (n=19); FLUSH comparisons are for 1970-1980 and 1993-1996 (n=15).

	FLUSH	CRiSP
Prospective Model		
FTT	0.71	0.69
TURB1	0.73	0.72
TURB4	0.73	0.85
TURB 5	0.74	0.77
Retrospective Model		
FTT	N/A	N/A
TURB1	0.85	0.83
TURB4	0.77	0.87
TURB5	0.86	0.84

We intend to make further comparisons of CRiSP and FLUSH, breaking down reach survival estimates into dam and reservoir survival. Figures to be added:

- Figures 21a-c (CRiSP and FLUSH dam survivals by year for each TURB);
- Figures 22a-c (CRiSP and FLUSH reservoir survivals by year for each TURB); and
- Figure 23a-c (CRiSP and FLUSH reservoir survival vs. FTT for each TURB)

ARetrospective Mode@ Estimates

The fits to empirical estimates are better, due to the more detailed in-season information used from the historical record as input to the models.

Comparison With 1989-1992 PIT-tag Detections

[To be written.]

Table A.2.1-4: Comparison of FLUSH and CRiSP model predictions with Lower Granite Dam to McNary Dam PIT-tag detection rates in 1989-1992.

Observed	FLUSH	CRiSP
1989	0.077	0.076
1990	0.080	0.054
1991	0.054	0.033
1992	0.117	0.034

Proportion Transported and Total Direct Survival to Below Bonneville

This section examines CRiSP and FLUSH model estimates of the proportion of Lower Granite pool arrivals that are transported, which is dependent upon model estimates of both dam and reservoir survival and dam passage routing. This information is then combined with an assumed barge survival constant and previously-described in-river survival estimates to develop estimates of total direct survival to below Bonneville Dam.

Proportion Transported

Each model estimated the proportion of fish arriving at the head of Lower Granite pool which were subsequently transported from all collector projects operating during each year. This estimate differs from the "proportion transported" (P) passed to the prospective models, which represents the proportion of survivors below Bonneville that were transported.

Both models agreed that no fish were transported in 1970 and 1974 and both models showed similar long-term temporal patterns (Figures A.2.1-24a to 24c). In general, the proportion transported was 20% or less through 1975-1976, the proportion increased rapidly from 1975-76 to 1980-81, and has generally fluctuated between about 50-70% since that time. However, the short-term temporal patterns and estimates of the actual proportions transported differed by TURB assumption and passage model (Figures A.2.1-24a to 24c and Figure A.2.1-25). For TURB1 and TURB5 assumptions, FLUSH estimated <1% to 25% higher proportions transported than did CRiSP in all years except 1971, 1972, and 1996. Years with passage model discrepancies >10% for both TURB1 and TURB5 included 1979-1983, 1985-1990, and 1994. In general, TURB4 estimates were lower than TURB1 or TURB5 estimates and the differences among passage models were reduced. For TURB 4, CRiSP estimated higher proportions transported than did FLUSH in 1971-73, 1976-79, 1992, and 1996. FLUSH estimated higher proportions in all other years except 1970 and 1974. Discrepancies >10% occurred only in 1980-83 and 1994.

Direct Survival to Below Bonneville

The PATH Hydro Work Group Data Subcommittee reviewed the limited available information and concluded that mortality on barges was likely very low. An assumption of 98% survival of barged fish was recommended for PATH analyses. When the proportion transported is multiplied by this assumed survival rate and the result is added to the product of estimated in-river survival (Figures A.2.1-16a to 16c) and the proportion not transported, an estimate of total direct survival to below Bonneville Dam is obtained:

$$\text{Total Direct Survival} = [\text{Transport\%} * 0.98] + [(1\text{-Transport\%}) * \text{In-River Survival Rate}]$$

Total Direct Survival is different from "System Survival" (o in Delta prospective model; exp[-M][DP+1-P] in Alpha prospective model), which incorporates assumptions regarding post-Bonneville mortality of transported and in-river migrating smolts. Total Direct Survival corresponds to exp[-M] in each prospective model.

The pattern of total direct survivals (Figures A.2.1-26a to 26c) is very similar to the pattern of in-river survival (Figures 16a-c) prior to 1975, when there was little or no transportation. After this point, the pattern is nearly identical to that of the transported proportion (Figures A.2.1-24a to 24c). TURB assumptions made little difference (generally <5%) in the disparity between FLUSH and CRiSP direct survival estimates, except during 1971-72 and 1977-79 (Figure A.2.1-27). URB4 assumptions generally resulted in greater differences between FLUSH and CRiSP estimates than did TURB1 or TURB5 assumptions. For most years, FLUSH estimates of total direct survival were higher than CRiSP estimates. Exceptions were in 1973, 1992, and 1996 for all TURB assumptions, as well as 1971 and 1977-79 for

TURB4 assumptions. The most striking discrepancies among models were in 1971 and 1994, when FLUSH estimates were 18-33% higher than CRiSP estimates.

Comparisons of Passage Models' Retrospective Simulations: Main Conclusions

Differences Among Models That Do Not Appear to Be Related to Alternative Hypotheses

We expected that the two passage models would produce nearly identical estimates for all functions except the reservoir survival (vs. FTT, predation, or TDG) functions. However, a number of striking differences among the FLUSH and CRiSP passage model estimates were not related to the alternative reservoir survival hypotheses. These include:

a. Dam passage survival estimates: From 1980-96, CRiSP estimates were consistently higher than FLUSH estimates by an average of about 5% (absolute). This represents about a 7-10% relative difference, since the total dam passage survivals estimated during this period were approximately 50-70%. Particularly troubling are the CRiSP vs. FLUSH differences since 1990, when we presumably have the best information on dam configurations and operations, which are all >5% (absolute) apart. Modeling groups should choose a year of great discrepancy, such as 1994, and carefully examine assumptions on a project-by-project basis to determine if the source of disagreement stems from: 1) alternative hypotheses about dam configuration or operations; 2) alternative hypotheses about fish distribution and timing at projects; or 3) errors that can be corrected.

Differences among models for a given TURB assumption also occurred in the pre-1980 period. Because our knowledge of configuration and operation is not as complete for this period, it would not be surprising if alternative hypotheses for dam configuration or operations exist. However, these have not been articulated, except as the three alternative TURB hypotheses, so discrepancies among models for a given TURB assumption are unexpected. Again, modeling groups should carefully examine assumptions on a project-by-project basis for years such as 1975 or 1972 to try to determine the cause of the discrepancies and whether or not this cause can be expressed in the form of alternative hypotheses.

b. Water Travel Time and Fish Travel Time. Differences among models for these estimates were not expected, but FLUSH WTT estimates (in all years) and FTT estimates (in all but two years) were lower (i.e., faster) than CRiSP estimates. The difference in FTT estimates averaged 4.5 days, which would translate into about a 5-10% difference in reservoir survival at 20-35 day total travel times in the TURB 1,4, and 5 FLUSH models (Figure A.2.1-8). This is a very significant proportion of the total FLUSH reservoir survival estimates at those travel times (approximately 5-35% total reservoir survival).

Modelers should carefully examine the functions used to estimate WTT and FTT to determine if the model discrepancies result from error or fundamental differences in hypotheses. If the latter, those hypotheses need to be articulated and, if the former, the models need to be brought into compliance with each other.

Differences Among Models That Are Related to Alternative Hypotheses

We expected that the two passage models would differ in their reservoir survival functions, due to structural difference in the models (Section A.2.1). The differences are substantial, and vary with TURB assumptions about past dam mortality (Figures A.2.1-8 and A.2.1-9). These results need to be generated for CRiSP with no gas mortality and no predation mortality to better understand differences between the models. Large amounts of spill in some past years likely killed fish in the CRiSP model, causing the variation in survival in Figure A.2.1-8. Because both models have similar fits to the original reach survival observations, one cause of the discrepancy may be the manner in which each model partitioned the reservoir and dam mortality in the empirical reach estimates. This was supposed to have been controlled by the TURB assumptions but, as seen with the disparity among dam survival estimates for each TURB assumption (above), the models must either be implementing different configurations and operations or the differences in fish distributions are placing the peak of the run at each project when different conditions are in effect. Until each modeling group produces the 19 reach estimates broken down by dam and reservoir survival, we will not be able to evaluate this possibility. If partitioning of survival is the cause of the discrepancy, we will either have an opportunity to bring the models into greater consistency (if the conditions and operations can be agreed upon) or we will have to look beyond the TURB hypotheses and reservoir survival fitting hypotheses to explain the differences.

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Comments on Section A.2.1 - Passage Models by Williams and Responses by Schaller, Petrosky, Wilson and Weber:

Evidence for the flow-survival relationship used in the models and justification for their use in prospective modeling:

i. FLUSH was based on agreed upon survival estimates from the past, and the curve was the best relationship to fit the yearly data points of survival with concurrent flows. The curve suggests that survival exponentially decreases the longer the fish are in the river. The shape of the exponential decrease is driven by survival estimates in 1973 and 1977.
ii. CRiSP discounts the 1973 and 1977 data as good predictors of reservoir survival because survival in those years was so influenced by problems at the dams. The CRiSP model presumes a constant rate of survival at all flows, thus as travel time increases, overall survival decreases but at a linear rate. The level of daily survival is based on fits of the curve to past data.
iii. Both models presume that they are the best predictor of future reservoir survivals given flows equal to those that occurred in the past.

Response by Schaller et al.: *Statements i and ii mischaracterize the 1973 and 1977 data points. PATH has accommodated the concerns about dam survival versus reservoir survival in those two years with TURB1, TURB4 and TURB5 sensitivities. FLUSH fits to TURB1, 4 and 5 all show a negative relationship between reservoir survival and fish travel time, with and without the 1973 and 1977 data points.*

(Comments by Williams continues):

Evidence against use of the flow-survival relationships used in the models:

FLUSH Model -

i. General agreement exists that there is a relationship between flow and travel time for spring/summer chinook salmon. However, when Figure 18c is redrafted in terms of mean survival per km vs. mean travel time (km/day), and the 1973 and 1977 points are excluded, there is no clear relationship. Survivals were substantially lower in 1973 and 1977 even at speeds that are higher than in some other years. This suggests that conditions in those 2 years do not represent anything that will occur in the future.

ii. The predicted (and agreed upon) in-river survival in 1973 was only 4%. The low value was assumed the consequence of low flows and poor passage conditions at Snake River dams. However, approximately 45,000 adults returned from the 1973 outmigration. If in-river fish had only a 4% survival, then the smolt to adult return (SAR) for fish that made it to below Bonneville Dam was nearly 20%. This is highly unlikely. Adjusting the number of alive smolts to below Bonneville Dam to get a SAR of 6% (high end of historical averages) would then, based upon the number of smolts that started the migration at Little Goose Dam, through the river system, provide a survival estimate of approximately 15% or nearly 4 times higher than predicted. This suggests that the 1973 data point is not a correct one on which to base a flow-survival relationship. It leaves only 1977 as one outlier on which to base the curve.

   Response by Schaller et al.: *The argument that in-river survival in 1973 was not 4%, but rather 15% is flawed in several respects and is undocumented. Williams states that 45,000 adults returned from the 1973 outmigration, but gives no source. Given all the sources of* *variation in an aggregate spring/summer chinook recruit estimate, it doesn't seem that a single year's SAR estimate has any bearing on the survival estimate from mark experiments. Williams states that if in-river fish had only a 4% survival, the SAR for fish that made it below Bonneville was 20% which would be unlikely compared to a high of 6% from pre-1970. However, Raymond (1988) indicates wild adult returns of 11,000 (7K springs and 4K summers) from the 1973 migration, which implies about 5% SAR for smolts below Bonneville, more in line with the pre-1970 estimates.*

iii. Survival of PIT-tagged fish released at the Snake River trap to McNary Dam in 1992 was a minimum of 40% (using techniques that subtract out not only transported fish, but fish "destined for transport", the survival was likely 45-50%.) The FLUSH model predicted survivals of fish to John Day Dam in 1992 of 8-16%. Even presuming that survival in the John Day reservoir was only 75%, the FLUSH survival estimate to McNary Dam averaged only 33% of the true survival. Flows in 1992 were nearly the same as in 1973, the difference was that the peak in 1973 was higher but the timing of the peak was later. Travel time through the Snake River was the same for fish in both 1973 and 1992. This suggests that the model is not a good predictor of survivals that will occur under low-flow conditions.

<u>Response by Schaller et al.</u>: *The comparison of 1973 and 1992 migration years is not as straight forward as implied. The statement that 1992 and 1973 flow years were similar is incorrect, particularly for the peak yearling migration period. In 1992, 90% of the smolts were trapped before May 6 at the Lewiston Trap. This is the population that was PIT tagged. In 1992 these fish would have experienced flows of 54 kcfs (4/15-5/6 average), compared to only 38 kcfs in 1973. During this period in 1992 water temperatures were in the low 50^{o}F range. In contrast, in 1973 survival was measured throughout the season and the temperatures in the Snake and Columbia rivers reached 70^{o}F in the later part of that season.*

iv. Analysis of survivals of PIT-tagged juvenile fish during the 1994-1996 outmigrations found no relationship of within-year flow to survival. No relationship was found even in years when flows varied from as low as 45 kcfs to as high as 180 kcfs for different release groups.

v. Based on the preliminary results from fish released at Lower Granite Dam in 1995, there was no positive relationship between flow experienced by the juveniles and subsequent adult returns. If anything, the lowest SARs occurred during the periods with the highest flows.

<u>Response by Schaller et al to (iv) and (v).</u>: *Williams neglects to identify a key assumption of the within-year analyses of flow and survival: that the different groups were independent. The groups which were assumed independent likely experienced much the same flow regimes but in different river reaches within years. Also, because flow is hypothesized to affect survival indirectly through other factors (water velocity, stage of smoltification, temperature, predation rate,--e.g., ISAB 1996--Return to the River), it is not surprising that a flow-survival relationship was not evident within-seasons. However, there appears to be a flow-survival relationship amongst years (independent groups) for PIT tagged spring chinook.*

CRiSP Model

i. The CRiSP model apparently presumes there is a constant survival per distance traveled, but the value changes with predator densities or consumption rates. The longer the fish are in the system, the lower their survival so any bias relates to the values assumed for constant. If the value for the survival constant in CRiSP is set independent of flow, then it would not be contrary to the empirical results identified (in iv. and v. above under the FLUSH Model).

Response by Schaller et al.: Williams states that the CRiSP model apparently assumes there is a constant survival per distance traveled. We think he meant that CRiSP assumes a constant mortality per day. It is not apparent from the diagnostics that it is constant, because if Figure 8 were expressed for predation only (excluding gas mortality), CRiSP would seem to show a similar relationship as FLUSH TURB4. In fact, predation mortality in CRiSP is temperature dependent and therefore would not be constant over time.

End of comments on Section A.2.1 – Passage Models by Williams and Responses by Schaller et al.

A.2.2 Fish Guidance Efficiency

This section provides a detailed description of the assumptions used for historical and future fish guidance efficiencies.

Guidance Structures

Over time, standard-length guidance screens have been replaced by lowered STS or ESBS at many lower Snake and lower Columbia River mainstem dams. Lower screens deflects more of the intake flow upward, improving fish guidance into the gatewell and the bypass system (Engineering Hydraulics, Inc. 1983 and 1984 and Davidson 1989, cited in McComas et al. 1994). Turbine operating gates have been removed or raised (from the stored position), also to enhance the flow of water up into the gatewell. Although other modifications have been evaluated, these two (lowering or extending the guidance screens further into the powerhouse intake flow and removing or raising the operating gates) have had the largest, most consistent positive effect on FGE.

The configurations of guidance structures at each project over time were determined by reference to a number of sources:

- Time line in the Juvenile Fish Transportation Appendix (C-2, p. 1-16 to 1-24) accompanying Bonneville Power Administration’s (BPA) Columbia River System Operation Review (BPA 1994)
- Memo listing installations of fish guidance screens at Corps of Engineers Dams (John McKern, U.S. Army Corps of Engineers Walla Walla District, memo dated May 9, 1997)
- Detailed Fish Operating Plans for the years 1984, 1986 through 1989, 1991, and 1994 (CBFWA 1984, 1986, 1987, 1988, 1989, 1991, and 1994)
- Fish Passage Plans for 1992, 1993, and 1995 through 1997 (Corps 1992, 1993, 1995, 1996, and 1997)
- Reports of the Fish Transportation Oversight Team for FY 1981 through FY 1992 (NMFS 1982, 1983, 1984, 1985, 1986, 1987, 1988, 1989, 1990, 1991, and 1992)
- Annual Reports for the Juvenile Fish Transportation Program for FY 1993 and 1994 (Corps 1995 and 1996)
- Pers. comm. from the staff of NMFS’ Hydropower Program, Portland, Oregon, the Portland and Walla Walla District offices of the Corps of Engineers, and the Fish Passage Center

PIT-Tag versus Fyke Net Estimates of Guidance Efficiencies

Data on fish guidance efficiencies have been obtained with fyke-net tests in the past and with PIT-tag studies in more recent years. Fyke-net estimates of FGE are thought to be biased upward because these experiments are conducted in the early evening or nighttime hours when juvenile fish are likely to be distributed in the upper water column (see review in *Return to the River*, Chapter 6, Independent Scientific Group 1996). Fish nearer the surface are likely to be guided by submerged screens, leading to higher estimates of FGE (Askren 1995). During other parts of the day, fish passing dams are distributed deeper in the water column and are less likely to be guided, producing lower FGEs. In addition, fyke-net studies usually evaluate the FGE in only one or two turbine units of a given powerhouse. In contrast, data from PIT-tag detections reflect fish guidance levels on a 24 hour/day, project-wide basis and are likely to provide a more representative estimate of FGE. For these reasons, we give precedence to estimates of FGE derived from PIT-tag studies, where available.

Sensitivity Analysis for the Effect of Extended-Length Screen on FGE

Two alternative hypotheses were derived for the effect of extended-length submersible screens on FGE, to be treated as sensitivities in the model runs:

Sensitivity #1: Assume that extended-length screens have significantly improved FGE. We derive support for this hypothesis from side-by-side (fyke-net, downstream slot) tests of guidance efficiencies for yearling chinook with STS and ESBS at McNary, Little Goose, and The Dalles Dams (**Table A.2.2-1**). In each case, the guidance efficiency of extended-length screens, which fish the upper 2/3 of the turbine intake, was significantly higher than that of standard screens, which fish only the upper 1/3 of the intake.

Table A.2.2-1: Comparison of yearling chinook guidance by standard traveling screens and submersible bar screens in side-by-side comparisons.

Dam	STUDY Year	% FGE Sts[1]	Esbs[2]	P-Value	PROPORTION POTENTIAL Improvement[3]	Reference
McNary	1992	61	80	--	(1.00 - 0.80)/(1.00 - 0.61) = 0.49	McComas et al. 1993
The Dalles	1993	44	73	$P < 0.001$	(1.00 - 0.73)/(1.00 - 0.44) = 0.52	Brege et al. 1994
Little Goose	1993	74	90	$P < 0.05$	(1.00-0.90)/(1.00 - 0.74) = 0.62	Gessel et al. 1994

[1]Standard-length traveling screens

[2]Extended-length submersible bar screens

[3]Proportion of fish not guided by standard-length screens that would be guided by extended-length screens (assume that FGE_{max} = 100%).

Project-specific observations of FGE for ESBS were not available for Lower Granite Dam. Therefore, we used the data in **Table A.2.2-1** to derive a function relating the FGE of ESBS to that of STS. Based on these tests, extended-length screens guide between 49% and 62% of the fish that are not guided by standard screens. Using the low (conservative) end of that range (i.e., 50%), we suggest the following relationship between FGE with standard vs. extended-length screens for Sensitivity #1:

$$FGE_{ESBS} = FGE_{STS} + 0.5(1 - FGE_{STS}) \qquad \text{[Eq. A.2.2-1]}$$

where:

FGE_{ESBS} = FGE for yearling chinook with ESBS
FGE_{STS} = FGE for yearling chinook with STS

In effect, extended-length screens guide half the fish that are not guided by the standard-length screens. This adjustment is applied to PIT-tag estimates of FGE for STS (for wild fish) measured at Lower Granite and Little Goose Dams to estimate guidance efficiency under the current configuration (i.e., ESBS). That is, estimates of guidance efficiency for STS, obtained by the "0% spill" method (Smith 1997), are corrected for the effect of extended-length screens.

Sensitivity #2: Assume that extended-length submersible screens have had no effect on fish guidance efficiency. Support for this hypothesis is derived from Russ Keifer's (Idaho Department of Fish and Game) analysis of PIT-tag detections for wild yearling chinook released from Snake River traps during 1993 through 1996 (see Smith 1997). Kiefer's analysis indicates considerable overlap between detection rates at Lower Granite Dam during 1996 (when extended-length screens were in place) and during 1993 through 1995 (standard-length submersible traveling screens in place), at a given spill level. However, because 1996 was a high flow year, Kiefer's method of estimating FGE requires extrapolation of a presumed linear regression function (for probability of detection against % spill) to 0% spill. The 0% spill level is well below the range of observations in data sets for either 1996 or 1997.

Other Correction Factors

Correction for Fyke-Net Position

Until 1992, FGE studies for STS positioned the fyke-net array (used to capture unguided fish) directly under the screen in the bulkhead (upstream) slot. When extended-length screens were introduced, their size precluded direct attachment of the net array. The fyke nets used in ESBS tests were deployed downstream, in the turbine operating gate slot. Subsequent studies conducted by NMFS showed that fish guidance may have been biased upward by the former, upstream position of the nets. Positioned directly under the screen in the upstream slot, the fyke nets may have created a pressure field near the tip of the screen which enhanced guidance into the gatewell (Williams et al. 1996). Positioned in the downstream slot, the nets were less likely to affect the flow field as far forward as the tip of the guidance screen. A correction factor for net position, based on tests conducted at McNary Dam during 1979 and 1992 (Krcma et al. 1980, McComas et al. 1993) was developed. Both of these tests employed a 20-foot long STS attached to a gatewell slot (operating gates fully raised or removed). This correction factor (i.e., the ratio of FGE derived with the fyke-net array in the downstream slot to that derived with the array in the upstream slot) was used to reduce estimates of FGE based on tests conducted with the array in the upstream slot:

$$COR_{pos} = \frac{FGE_{dnstrm}}{FGE_{upstrm}} = \frac{0.61}{0.745} = 0.82 \qquad \text{[Eq. A.2.2-2]}$$

where:

COR_{pos} = correction to downstream position of the fyke-net array
FGE_{upstrm} = FGE with nets in the upstream slot, measured at McNary (1979)
FGE_{dnstrm} = FGE with nets in the downstream slot, measured at McNary (1992)

Correction for Position of the Operating Gate

During tests of a particular guidance configuration at a given dam, the operating gates may have been raised or removed only for the test period; the guidance structures were otherwise operated with the gates stored. Or, the reverse may have been true: the gates were stored during testing and the guidance structures were otherwise operated with the gates raised or removed. Because side-by side fyke-net tests showed that the position of the operating gate can have a significant effect on FGE (**Table A.2.2-2**) a correction for this effect was developed:

- Brege et al. (1988) tested STS with raised operating gates at Ice Harbor. Subtract 15.3% from his reported estimate of FGE to approximate the value that would have been measured if the gates had been stored. This correction was used to estimate FGE for Ice Harbor during 1993 to 1995.
- McComas et al. (1993, 1994, and 1995) tested ESBS at McNary Dam with the operating gates in the stored position. Add 15.3% to his estimates of FGE to approximate values that would have been measured if the gates had been raised. This correction is used to estimate FGE for McNary during 1997. Other tests at McNary were similarly adjusted.

Table A.2.2-2: Effect of raised operating gate on guidance of yearling chinook in side-by-side comparisons.

Test Location	Year	Screen	Net Pos.	% FGE SOG	% FGE ROG	Increase w/ ROG	P-value	Notes	Reference
LGR	1983	STS	upstrm	55.0	74.0	19.0	*?	--	Swan et al. 1984
LGO	1986	STS	"	61.0	73.5	12.5	<0.005	--	Swan et al. 1987
MCN	1982	STS	"	83.0	88.0	5.0	ns?	BFVBS	Krcma et al. 1983
BON I	1977	ESBS	"	18.0	54.0	36.0	*?	65% porosity screen	Krcma et al. 1978
				55.0	77.0	22.0	*?	35% porosity screen	
BON I	1989	ESBS	"	41.0	43.6	2.6	ns	bar screens w/ perf. plate	Gessel et al. 1990
BON I	1991	STS	"	28.9	49.5	20.6	0.01	--	Monk et al. 1992
BON I	1993	STS	"	38.0	43.0	5.0	ns	--	Monk et al. 1993
					Mean Increase w/ ROG =	***15.3***			

Correction for Guidance of Wild Versus Hatchery Fish

With few exceptions, PIT-tagged wild chinook were more likely to be detected than hatchery fish for all combinations of year, release site, and detection site (Table 1 in Smith 1997). Overall, wild fish were 18% more likely to be detected than hatchery fish at Lower Granite Dam, 20% more likely at Little Goose Dam, and 16% more likely at Lower Monumental Dam. This relationship is so consistent that it is reasonable to adjust the FGE of the mixed (hatchery+wild) run, observed during fyke-net tests, to that of wild fish by the overall mean of these probabilities (i.e., 18%).

For three of the lower Snake River dams[1], the FGE for the mixed run is adjusted by the following equation:

$$FGE_{Mixed} = (\%\,Wild)*[1.18(FGE_{Hatch})] + (\%\,Hatch)*[FGE_{Hatch}] \qquad \text{[Eq. A.2.2-3]}$$

where:

FGE_{Mixed} = fish guidance efficiency of the mixed run, observed in the fyke-net test
FGE_{Hatch} = fish guidance efficiency of hatchery smolts
FGE_{Wild} = fish guidance efficiency of wild smolts
%Wild = proportion of the Snake River run made up by wild smolts (from Table 2 in Raymond 1988)
%Hatch = proportion of the Snake River run made up by hatchery smolts from Table 2 in Raymond 1988)

The equation was solved for FGE_{Hatch} and then FGE_{Wild} was calculated as:

$$FGE_{Wild} = 1.18*(FGE_{Hatch}) \qquad \text{[Eq. A.2.2-4]}$$

This method of estimating the FGE of wild fish assumes that the percentages of wild versus hatchery fish at the first dam encountered, as reported by Raymond (1988) in his Table 2, do not change at subsequent Snake River projects.

Raymond (1988) did not present estimates of the proportions of wild versus hatchery fish in runs after 1984. Because new hatcheries continued to come on line through 1992, we assumed that the proportion hatchery fish in years after 1984 could be estimated by the proportions observed during 1981 through 1984 (i.e., 82.2%). A sensitivity analysis showed that increasing the proportion of hatchery fish in the run up to 100% would only change our estimate of the FGE of wild fish by 1%.

For projects downstream of the confluence of the Snake River with the Columbia, we averaged the percent hatchery fish for the Snake run with that for the mid-Columbia River using the data in Raymond's (1988) Tables 2 and 3. The FGE of wild smolts was then estimated using the equation shown above, substituting in the correct percentages and the FGE_{Mixed} observed at each project.

Guidance Efficiencies for Yearling Chinook Salmon

Point estimates of guidance efficiency at each of four lower Snake and four lower Columbia River projects were derived using the data, sensitivities, and correction factors described above. A representative estimate was calculated for each year a project has been in service (service dates from BPA 1991) under the two sensitivities for the effect of extended-length screens. These estimates of yearling chinook guidance efficiency are shown in **Tables A.2.2-3 and A.2.2-4**, respectively.

[1]No fyke-net tests of guidance efficiency at Lower Monumental Dam

Table A.2.2-3: Estimated yearling chinook guidance efficiencies at lower Snake and Columbia River mainstem dams. Sensitivity #1: $FGE_{ESBS} > FGE_{STS}$.

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
Lower Granite	1975-1976	1 of 3 turbines w/ STS, stored operating gate (OG)	46	Fyke-net estimate (50% FGE; Swan et al. 1983), corrected by a factor of 0.8 for fyke-net position, and corrected for percent wild fish.
	1977	3 of 3 turbines w/ STS, stored OG	46	" "
	1978-1990	6 of 6 turbines w/ STS, stored OG	46	" "
	1991-1994	6 of 6 turbines w/ STS, raised OG (ROG)	55	Mean of 1993-1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997)
Lower Granite (cont'd)	1995	1 of 6 turbines w/ ESBS, ROG	55	Assume that FGE for this unit is approximately equal to the mean of the 1993-1995 PIT-tag estimate for this project (wild fish) (Table 2 in Smith 1997).
		5 of 6 turbines w/ STS, ROG	55	Mean of 1993-1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997).
	1996-1997	6 of 6 turbines w/ ESBS, ROG	78	As above w/ Sensitivity #1: assume that FGE for ESBS is approximately equal to the mean of the 1993-1995 PIT-tag estimates for STS (wild fish) (Table 2 in Smith 1997), corrected for ESBS.
Little Goose	1970	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1971-1976	1 of 3 turbines w/ STS, stored OG	55	Fyke-net estimate (61%; Swan et al. 1987), corrected by a factor of 0.8 for fyke-net position and corrected for percent wild fish.
	1977	2 of 3 turbines w/ STS, stored OG	55	" "
	1978-1992	6 of 6 turbines w/ STS, stored OG	55	" "
	1993-1994	1 of 6 turbines w/ ESTS, ROG	64	Mean of 1993 and 1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997).
		1 of 6 turbines w/ ESBS, ROG	64	" "
		4 of 6 turbines w/ STS, ROG	64	" "
	1995	1 of 6 turbines w/ 1 ESTS + 2 ESBS, ROG	64	" "
		5 of 6 turbines w/ STS, ROG	64	" "
	1996-1997	6 of 6 turbines w/ ESBS, ROG	82	As above w/ Sensitivity #1: assume that FGE for ESBS is approximately equal to the mean of the 1993-1995 PIT-tag estimates for STS (wild fish) (Table 2 in Smith 1997), corrected for ESBS.
Lower Monumental	1969-1991	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1992-1997	6 of 6 turbines w/ STS, stored OG	61	Mean of 1994 and 1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997).
Ice Harbor	1961-1966	None	0	
	1967-1979	6-inch orifices to ice and trash sluiceway	3	3% volitional entry from gatewells through 6-in orifices, corrected for percent wild fish.
	1980-1984	6-inch orifices + 1,200 cfs overflow from forebay	30	3% volitional entry plus 23% "guidance" (measured in 1982) for fish skimmed off the forebay (CBFWA 1988), corrected for percent wild fish (acoustic studies with fyke-net capture).
	1985-1992	6-inch orifices + 2,700 cfs overflow from forebay	42	3% volitional entry plus 34% "guidance" (measured in 1987) for fish skimmed off the forebay (CBFWA 1988), corrected for percent wild fish (acoustic studies).
	1993-1995	6 of 6 turbines w/ STS, stored OG	66	Fyke-net estimate (78% FGE; Brege et al. 1988b), corrected by a factor of 0.8 for net position, minus 15% for OG in stored position, and corrected for percent wild fish. During this period,

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
				sluiceway gates opened to create approx. 2,000 cfs skimming flow. Sluiceway guidance
				efficiency approx. equal to 20% (J. McKern, Corps, Walla Walla).
	1996-1997	6 of 6 turbines w/ STS, ROG	71	Fyke-net estimate (78%, above), corrected by a factor of 0.8 for net position (note: new juvenile
				bypass channel open, sluiceway gates closed), and corrected for percent wild fish.
McNary	1953-1978	None	0	
	1979	1 of 14 turbines w/ ESBS, stored OG	79	Sensitivity #1: Estimate of FGE for ESBS derived from fyke-net studies w/ nets in dnstrm slot (81%, 82%, and 87% FGE in McComas et al. 1993, 1994, and 1995, respectively),
				corrected for percent wild fish.
		1 of 14 turbines w/ STS, stored OG	55	Average of fyke-net estimates in Krcma et al. (56% FGE; 1980 and 66% FGE; 1983),
				corrected by a factor of 0.8 for net position, and in McComas et al. (61%; 1993), and
				corrected for percent wild fish.
	1980	6 of 14 turbines w/ STS, stored OG	55	" "
	1981	12.5 of 14 turbines w/ STS, stored OG	55	" "
	1982-1989	14 of 14 turbines w/ STS, stored OG	55	" "
	1990	2 of 14 turbines w/ ESBS, stored OG	79	Sensitivity #1 for ESBS, as above.
		12 of 14 turbines w/ STS, stored OG	55	Estimate for STS as above.
	1991-1992	14 of 14 turbines w/ STS, stored OG	55	" "
	1993	1 of 14 turbines w/ ESTS, stored OG	79	Sensitivity #1 for ESBS, as above.
		1 of 14 turbines w/ ESBS, stored OG	79	" "
		12 of 14 turbines w/ STS, stored OG	55	Estimate for STS as above.
	1994	1 of 13 turbines w/ ESBS, stored OG	79	Sensitivity #1 for ESBS, as above (14th unit out-of-service).
		12 of 13 turbines w/ STS, stored OG	55	Estimate for STS as above (14th unit out-of-service).
	1995	13 of 13 turbines w/ STS, stored OG	55	" "
	1996	6 of 14 turbines w/ ESBS, stored OG	79	Sensitivity #1 for ESBS, as above.
		8 of 14 turbines w/ STS, stored OG	55	Estimate for STS as above.
	1997	14 of 14 turbines w/ ESBS, ROG	96	Sensitivity #1 for ESBS, as above, + adjustment for ROG.
John Day	1968-1984	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1985	9 of 16 turbines w/ STS (no operating gates at John Day Dam)	67	Fyke-net estimate (72% FGE; Fredricks and Graves 1997), corrected by a factor of 0.8 for net position and corrected for percent wild fish.
	1986	12 of 16 turbines w/ STS	67	" "
	1987-1997	16 of 16 turbines w/ STS	67	" "
The Dalles	1957-1974	Ice and trash sluiceway	2	Sluiceway passage efficiency w/ 6-in orifices drilled into the bulkhead between the gatewells
				and the sluiceway.
	1975-1997	Ice and trash sluiceway	46	As above, with addition of overflow from the forebay (40% FGE; from mark/recapture tests
				performed during 1982 and 1983, CBFWA 1984), corrected for percent wild fish. Ice and trash
				sluiceway began operating for fish bypass during 1975 (G. Johnson, Corps, Portland District)

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
Bonneville I	1938-1970	None	0	
	1971-1983	Ice and trash sluiceway	4	Sluiceway passage efficiency w/ 12-in orifices drilled into the bulkhead between the gatewells
				and the sluiceway (G. Fredricks, NMFS, Portland), corrected for percent wild fish.
	1984-1987	10 of 10 units w/ STS, stored OG	72	Fyke-net estimate (71% FGE; Krcma et al. 1980 and 76% FGE; Krcma et al. 1982), corrected
				by a factor of 0.8 for net position, + 4% for sluiceway passage, and corrected for percent
				wild fish.
	1988-1997	10 of 10 units w/ STS, stored OG	41	Navigation lock construction caused change in hydraulic conditions in the forebay which
				coincided with a reduction in FGE. Average of fyke-net measurements (42% FGE; Gessel
				et al. 1990, 38% FGE; Monk et al. 1992, and 39% FGE; Monk et al. 1993), corrected by a
				factor of 0.8 for net position, + 4% for sluiceway passage, and corrected for percent wild fish.
				FGE for sluiceway system reduced from 40% to 4% due to reduced flow to the sluiceway after the installation of STS.
Bonneville II	1982-1988	8 of 8 units w/ STS, stored OG	21	Average of fyke-net estimates (19% FGE; Krcma et al. 1984 and 26% FGE; Gessel et al. 1985),
				each corrected by a factor of 0.8 for net position and corrected for percent wild fish.
	1989-1992	8 of 8 units w/ STS, STR, stored OG	54	Fyke-net estimate by Gessel et al. (59% FGE; 1985), corrected by a factor of 0.8 for net position, and corrected for percent wild fish
	1993-1997	8 of 8 units w/ LSTS, STR, alt TIE, stored OG	43	Average of fyke-net estimates (66% FGE; Gessel et al. 1988, 31% FGE; Gessel et al. 1989, 46% FGE; Monk et al. 1994, and 44% FGE; Monk et al. 1995), corrected by a factor of 0.8 for net
				position, and corrected for percent wild fish.

Table A.2.2-4: Estimated yearling chinook guidance efficiencies at lower Snake and Columbia River mainstem dams. Sensitivity #2 $FGE_{ESBS} = FGE_{STS}$.

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
Lower Granite	1975-1976	1 of 3 turbines w/ STS, stored operating gate (OG)	46	Fyke-net estimate (50% FGE; Swan et al. 1983), corrected by a factor of 0.8 for fyke-net position, and corrected for percent wild fish.
	1977	3 of 3 turbines w/ STS, stored OG	46	" "
	1978-1990	6 of 6 turbines w/ STS, stored OG	46	" "
	1991-1994	6 of 6 turbines w/ STS, raised OG (ROG)	55	Mean of 1993-1995 PIT-tag estimates for wild fish under "no-spill" conditions (see Table 2 in Smith 1997
	1995	1 of 6 turbines w/ ESBS, ROG	55	" "
		5 of 6 turbines w/ STS, ROG	55	" "
	1996-1997	6 of 6 turbines w/ ESBS, ROG	55	As above w/ Sensitivity #2: assume that FGE for ESBS is approximately equal to FGE for STS.
Little **Goose**	1970	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1971-1976	1 of 3 turbines w/ STS, stored OG	55	Fyke-net estimate (61%; Swan et al. 1987), corrected by a factor of 0.8 for fyke-net
				position and corrected for percent wild fish2.
	1977	2 of 3 turbines w/ STS, stored OG	55	" "
	1978-1992	6 of 6 turbines w/ STS, stored OG	55	" "
	1993-1994	1 of 6 turbines w/ ESTS, ROG	64	Mean of 1993 and 1995 PIT-tag estimates for wild fish under "no-spill" conditions (see
				Table 2 in Smith 1997).

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
Little Goose (continued)		1 of 6 turbines w/ ESBS, ROG	64	" "
		4 of 6 turbines w/ STS, ROG	64	" "
	1995	1 of 6 turbines w/ 1 ESTS + 2 ESBS, ROG	64	" "
		5 of 6 turbines w/ STS, ROG	64	" "
	1996-1997	6 of 6 turbines w/ ESBS, ROG	64	As above w/ Sensitivity #2: assume that FGE for ESBS is approximately equal to FGE for STS.
Lower Monumental	1969-1991	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1992-1997	6 of 6 turbines w/ STS, stored OG	61	Mean of 1994 and 1995 PIT-tag estimates for wild fish under "no-spill" conditions (see
				Table 2 in Smith 1997).
Ice Harbor	1961-1966	None	0	
	1967-1979	6-inch orifices to ice and trash sluiceway	3	3% volitional entry from gatewells through 6-in orifices, corrected for percent wild fish.
	1980-1984	6-inch orifices + 1,200 cfs overflow from forebay	30	3% volitional entry plus 23% "guidance" (measured in 1982) for fish skimmed off the
				forebay (CBFWA 1988), corrected for percent wild fish (acoustic studies with fyke-net capture).
	1985-1992	6-inch orifices + 2,700 cfs overflow from forebay	42	3% volitional entry plus 34% "guidance" (measured in 1987) for fish skimmed off the
				forebay (CBFWA 1988), corrected for percent wild fish (acoustic studies).
	1993-1995	6 of 6 turbines w/ STS, stored OG	66	Fyke-net estimate (78% FGE; Brege et al. 1988b), corrected by a factor of 0.8 for net
				position, minus 15% for OG in stored position, and corrected for percent wild fish.
				During this period, sluiceway gates opened to create approx. 2,000 cfs skimming flow. Sluiceway guidance efficiency approx. equal to 20% (J. McKern, Corps, Walla Walla District).
	1996-1997	6 of 6 turbines w/ STS, ROG	71	Fyke-net estimate (78%, above), corrected by a factor of 0.8 for net position (note: new juvenile bypass channel open, sluiceway gates closed), and corrected for percent wild fish
McNary	1953-1978	None	0	
	1979	1 of 14 turbines w/ ESBS, stored OG	55	Average of fyke-net estimates in Krcma et al. (56% FGE; 1980 and 66% FGE; 1983)
				corrected by a factor of 0.8 for net position, and in McComas et al. (61%, 1993), both
				corrected for percent wild fish, w/ Sensitivity #2: assume that FGE for ESBS is
				approximately equal to FGE for STS.
		1 of 14 turbines w/ STS, stored OG	55	" "
	1980	6 of 14 turbines w/ STS, stored OG	55	" "
	1981	12.5 of 14 turbines w/ STS, stored OG	55	" "
	1982-1989	14 of 14 turbines w/ STS, stored OG	55	" "
	1990	2 of 14 turbines w/ ESBS, stored OG	55	" "
		12 of 14 turbines w/ STS, stored OG	55	" "
	1991-1992	14 of 14 turbines w/ STS, stored OG	55	" "
	1993	1 of 14 turbines w/ ESTS, stored OG	55	" "
		1 of 14 turbines w/ ESBS, stored OG	55	" "
		12 of 14 turbines w/ STS, stored OG	55	" "

Dam	Year	Fish Guidance Configurations/Structures	% Fge	Comment
McNary) (continued)	1994	1 of 13 turbines w/ ESBS, stored OG	55	" "
		12 of 13 turbines w/ STS, stored OG	55	" "
	1995	13 of 13 turbines w/ STS, stored OG	55	" "
	1996	6 of 14 turbines w/ ESBS, stored OG	55	" "
		8 of 14 turbines w/ STS, stored OG	55	" "
	1997	14 of 14 turbines w/ ESBS, ROG	72	As above, + adjustment for ROG.
John Day	1968-1984	Gatewell salvage system	2	Embedded pipeline for juvenile passage through 6-in orifices from the gatewells.
	1985	9 of 16 turbines w/ STS (no operating gates at	67	Fyke-net estimate (72% FGE; Fredricks and Graves 1997), corrected by a factor of 0.8
		John Day Dam)		for net position and corrected for percent wild fish.
	1986	12 of 16 turbines w/ STS	67	" "
	1987-1997	16 of 16 turbines w/ STS	67	" "
The Dalles	1957-1974	Ice and trash sluiceway	2	Sluiceway passage efficiency w/ 6-in orifices drilled into the bulkhead between the
				gatewells and the sluiceway.
	1975-1997	Ice and trash sluiceway	46	As above, with addition of overflow from the forebay (40% FGE; from mark/recapture
				tests performed during 1982 and 1983, CBFWA 1984), corrected for percent wild fish.
				Ice and trash sluiceway began operating for fish bypass during 1975 (G. Johnson, Corps, Portland District).
Bonneville I	1938-1970	None	0	
	1971-1983	Ice and trash sluiceway	4	Sluiceway passage efficiency w/ 12-in orifices drilled into the bulkhead between the
				gatewells and the sluiceway (G. Fredricks, NMFS, Portland), corrected for percent wild fish.
	1984-1987	10 of 10 units w/ STS, stored OG	72	Fyke-net estimate (71% FGE; Krcma et al. 1980 and 76% FGE; Krcma et al. 1982),
				corrected by a factor of 0.8 for net position, + 4% for sluiceway passage, and
				corrected for percent wild fish.
				[FGE for the sluiceway system declined from 40% to 4% due to reduced flow to the
				sluiceway after installation of STS (G. Fredricks, NMFS, Portland).]
	1988-1997	10 of 10 units w/ STS, stored OG	41	Navigation lock construction caused change in hydraulic conditions in the forebay
				which coincided with a reduction in FGE. Average of fyke-net measurements (42%
				FGE; Gessel et al. 1990, 38% FGE; Monk et al. 1992, and 39% FGE; Monk et al. a
				1993), corrected by factor of 0.8 for net position, + 4% for sluiceway passage, and corrected for percent wild fish.
Bonneville II	1982-1988	8 of 8 units w/ STS, stored OG	21	Average of fyke-net estimates (19% FGE; Krcma et al. 1984 and 26% FGE; Gessel et
				al. 1985), each corrected by a factor of 0.8 for net position and corrected for percent wild fish.
	1989-1992	8 of 8 units w/ STS, STR, stored OG	54	Fyke-net estimate (59% FGE; Gessel et al. 1985), corrected by a factor of 0.8 for net
				position, and corrected for percent wild fish.
	1993-1997	8 of 8 units w/ LSTS, STR, alt TIE, stored OG	43	Average of fyke-net estimates (66% FGE; Gessel et al. 1988, 31%FGE; Gessel et al.
				1989, 46%FGE; Monk et al. 1994, and 44% FGE; Monk et al. 1995), corrected by a
				factor of 0.8 and corrected for percent wild fish.

A.2.3 Turbine and Bypass Survival

Estimates for Prospective Analyses

These estimates represent our best understanding of turbine and bypass survival under current and past conditions. Greatest weight was given to the most recent estimates, particularly those derived from PIT-tag studies.

Current Estimate of Turbine Survival (TURB1): Turbine survival is defined as the proportion of fish surviving direct turbine passage. The method of measuring turbine survival includes any incremental indirect mortality experienced in the tailrace by fish that passed through turbines, above the tailrace mortality experienced by fish passing through other routes. Indirect mortality was defined as that which could result from predation upon fish that become disoriented or stressed by passage through turbines. Indirect turbine mortality is not explicitly considered in reservoir survival functions, but is implicit in the FLUSH reservoir survival vs. FTT relationship.

Nine turbine survival studies published through 1990 at Snake and lower Columbia River dams have been reviewed by Iwamoto and Williams (1993). Turbine survival estimates studies ranged from 80-98%, averaging 90%. The Independent Scientific Group (ISG 1996) reviewed studies published through 1995, including several from mid-Columbia River projects. Recent studies using PIT-tags indicated high variability. The PATH Hydro Work Group (1996) reviewed studies published through 1996 and attempted to resolve some of the discrepancies among PIT-tag survival studies by examining details of methodology. It was determined that the 1993 estimate of turbine survival at Lower Granite Dam (0.82; Iwamoto et al. 1994) was less reliable than the 1995 estimate at the same site (0.93; Muir et al. 1996). Release methodology was greatly improved for the latter estimate. This review also suggested that direct survival estimates determined from balloon tags are similar to PIT-tag survival estimates when the same release methodology is employed. The PATH Hydro Work Group (1996) concluded that turbine survival appears to be ≥ 0.90.

Based on a review of field studies at mid-Columbia projects and Lower Granite Dam, a value of 0.90 (Table A.2.3-1) was adopted as the current estimate of turbine survival (with sensitivity to a range of 0.87, in TURB2, to 0.93, in TURB3). Note that analyses of TURB2 and TURB3 have been postponed indefinitely.

Table A.2.3-1: Current estimates of dam passage survival and routing parameters.

	Current Estimate
Turbine Survival	0.90 [TURB1] (Sensitivity to 0.87 [TURB2] and 0.93 [TURB3])
Spill Survival	0.98
Bypass Survival	(See last row for TURB1 in Table LK6)
Spill Efficiency - Snake projects and McNary	1.0 [SPILL1] (Sensitivity to Equation [2] at LGR, LGS, and LMN [SPILL2])
Spill Efficiency - The Dalles	Equation (1)
Spill Efficiency - John Day, Bonneville	1.0
Direct Transport Survival	0.98
Reduction in Reservoir Mortality Due to Squawfish Removal Since 1990	Two Sensitivities: 0 [PRED1], 0.25 [PRED2]

Current Estimate of Bypass Survival (TURB1): Bypass survival is defined as survival past turbine intake screens, gatewells, orifices, bypass flumes, and, in some cases, dewatering screens, wet separators, sampling facilities (including holding tanks), and bypass outfall conduits. These estimates also apply to juvenile bypass through sluiceways at The Dalles, Ice Harbor, and the Bonneville Powerhouse One during certain years. Bypass mortality is likely to have varied across projects and over time (i.e., as bypass facilities were modified, **Table A.2.3-2**).

Table A.2.3-2: Description of bypass facilities by project and date (from J. McKern, Corps of Engineers, Walla Walla District) with estimates adopted as the current and post-1979 bypass survival parameters as well as those included for one sensitivity to pre-1980 bypass survivals.

1. **Bonneville Dam**
   a) **Powerhouse One**
      (1) 1938 to 1971 – no bypass provided
      (2) 1971 to 1983 – ice/trash sluiceway and orifice bypass (97%)
      (3) 1983 to present – STS, orifice bypass to sluiceway (97%)
   b) **Powerhouse Two**
      (1) 1982 to present – STS, bypass tunnel (97%)

2. **The Dalles Dam**
   a) 1958 to 1971 – no juvenile bypass provided
   b) 1971 to 1977 – 6-inch orifices were drilled from the bulkhead slots to the ice/trash sluiceway (99%)
   c) 1977 to present – overflow into sluiceway plus orifice flow (99%)

3. **John Day Dam**
   a) 1968 to 1984 – imbedded pipe bypass system (97%)
   b) 1984 to present – STS, tunnel bypass system (98%)

4. **McNary Dam**
   a) 1953 to 1968 – no bypass provided
   b) 1968 to 1979 – orifice, ice/trash sluiceway bypass (98%)
   c) 1979 to 1994 – STS, flume, pressurized pipe bypass (98%)
   d) 1994 to 1996 – STS, flume, nonpressurized pipe bypass (99%)
   e) 1997 to present – ESBS, flume, nonpressurized pipe bypass (99%)

5. **Ice Harbor Dam**
   a) 1961 to 1967 – no juvenile bypass provided
   b) 1967 to 1980 – 6-inch orifices drilled from bulkhead slots to ice/trash sluiceway, converting the sluiceway to a juvenile bypass system (99%, based on subsequent studies at The Dalles)
   c) 1981 to 1984 – ice/trash sluiceway operated as bypass with 1,200 cfs combined orifice and overflow spill (99%)
   d) 1984 to 1995 – ice/trash sluiceway operated as bypass with 2,700 cfs combined orifice and overflow spill (99%)
   e) 1996 to present – fully screened juvenile bypass system (99%)

6. **Lower Monumental**
   a) 1969 to 1991 – imbedded pipe bypass system (97%)
   b) 1992 to present – STS and tunnel bypass, nonpressurized flume (99%)

7. **Little Goose Dam**
   a) 1971 to 1979 – imbedded pipe bypass system (97%)
   b) 1979 to 1989 – STS, tunnel, pressurized pipe bypass system (97%)
   c) 1990 to 1996 – STS, tunnel, nonpressurized flume bypass system (99%)
   d) 1997 to present – ESBS, tunnel, nonpressurized flume bypass system (99%)

8. **Lower Granite Dam**
   a) 1975 to 1995 – STS, tunnel, nonpressurized flume bypass system (99%)
   b) 1996 to present – ESBS, tunnel, nonpressurized flume bypass system (99%)

A minimum estimate of mortality can be determined from observations of dead fish in sampling facilities. Concurrent observations of descaling may also be used for inferring injury. **Table A.2.3-3** summarizes the available information on yearling chinook descaling and facility mortality estimates at juvenile sampling facilities since 1981. Information from 1981-1992 at Lower Granite, Little Goose, and McNary Dams is summarized in Ceballos et al. (1993). More recent estimates at those projects and at Lower Monumental and Ice Harbor Dams are reported in Hurson et al. (1995 and 1996), Baxter et al. (1996), and preliminary Corps of Engineers reports (summarized by John McKern). Information from Bonneville Dam through 1995 is summarized in Martinson et al. (1996). Detailed descriptions of sampling methods and descaling criteria and changes in these methods and criteria over time are included in Basham and Garrett (1996). A review of bypass mortality since 1981 is included in Giorgi (1996a).

Table A.2.3-3: Percent descaling and mortality at juvenile fish facilities during years for which data are available. (John Day Dam not included because estimates are based on gatewell samples.)

Project	Chinook	Yearling Chinook	Hatchery Yearling Chinook	Wild Yearling Chinook
Lower Granite				
1981	15.5(0.7)			
1982	8.8(0.8)			
1983	3(0.7)			
1984	3(0.5)	(0.4)		
1985	3.3(0.3)	(0.3)		
1986	3.7(0.3)	(0.3)		
1987	3.1(1.2)			
1988	2.4(0.5)			
1989	2.3(0.9)			
1990	3.6(0.3)			
1991	2.4(0.2)			
1992	4.7(0.6)			
1993			4.5(0.4)	3.9(0.4)
1994			3.7(0.4)	3.6(0.4)
1995			2.7(0.3)	0.9(.02)
1996			3(0.6)	1.5(0.9)
Little Goose				
1981	15.4(1.3)			
1982	26(6.2)			
1983	18.4(2.7)			
1984	7.1(1.5)			
1985	7.9(1.0)			
1986	8.8(0.9)			
1987	8.6(1.8)			
1988	12.7(1.2)			
1989	9.9(1.5)			
1990	6.5(1.1)			
1991	3.4(0.9)			
1992	4.1(0.4)			
1993			4.7(0.3)	3.7(0.3)
1994			6.3(0.5)	4.4(0.8)
1995			4(0.4)	2.5(0.5)
1996			4.3(0.6)	2.5(1.2)

Project	Chinook	Yearling Chinook	Hatchery Yearling Chinook	Wild Yearling Chinook
Lower Monumental				
1993			9.2(0.1)	5.8(0.1)
1994			6.2(0.3)	6.1(0.5)
1995			4.2(0.1)	3.2(0.2)
1996			4.5(0.2)	4.1(0.4)
Ice Harbor				
1996			4.1(0.5)	4.8(0.0)
McNary				
1981		8.7(0.9)		
1982		17.9(1.8)		
1983		11.6(0.5)		
1984		12.6(0.3)		
1985		6.0(0.4)		
1986		7.0(0.5)		
1987		5.5(0.8)		
1988		7.6(1.4)		
1989		9.8(0.4)		
1990		8.2(1.2)		
1991		8.7(0.7)		
1992		8.8(1.9)		
1993		5.6(0.6)		
1994		8.4(1.1)		
1995		11.1(0.1)		
1996		7.8(0.1)		
Bonneville Powerhouse 1				
1988		4.4(0.2)		
1989		4.2(0.1)		
1990		7.0(0.1)		
1991		9.3(0.1)		
1992		4.6(0.2)		
1993		3.9(0.1)		
1994		2.6(0.2)		
1995		6.7(0.1)		
Bonneville Powerhouse 2				
1988		5.2(2.1)		
1989		4.4(3.1)		
1990		5.3(0.7)		
1991		10.0(0.8)		
1992		10.2(1.4)		
1993		7.2(0.7)		
1994		5.1(1.3)		
1995		6.7(0.6)		

Based on a review of field studies at lower Snake and Columbia River dams, estimates of current bypass survival rates ranging from 0.97 to 0.99 per project were adopted **(Table A.2.3-4)**. Additional mortality may be associated with predation at the bypass outfall at some locations, but insufficient data were available to estimate this increment in mortality for yearling chinook salmon. These bypass survival estimates are likely to encompass any delayed mortality due to passage through this route.

Estimates for Retrospective Analyses

Historical Estimates of Turbine and Bypass Survival

Historical estimates of bypass and turbine mortality vary from current estimates for some projects during some years. There was general agreement that, between 1980 and the present, the current estimate of turbine survival (0.90 in TURB1, with sensitivity to a range of 0.87 [TURB2] to 0.93 [TURB3]), and the estimates of bypass survival for TURB1 in **Table A.2.3-4** applied. However, there is less certainty about survival estimates prior to 1980, so several alternative hypotheses were described.

Williams and Matthews (1995) concluded that 1970's estimates of juvenile reach survival were lower than would occur under similar river conditions in the 1990's, due to significantly higher debris loads at the first three Snake River projects and sporadic turbine operations. Debris, especially when coupled with low flows or reduced turbine operations, clogged bypass systems and increased velocities through unclogged areas, resulting in injury, descaling, and high delayed mortality of fish collected for transportation experiments. The Corps began removing debris from the Lower Granite forebay in 1980 and a permanent debris rake was installed the following year. Presumably, dam passage mortality has been reduced since that time.

Additionally, the slotted bulkheads installed in skeleton bays of Little Goose, Lower Monumental, and Ice Harbor Dams in 1972 were operated during April and early May in an attempt to reduce dissolved gas levels. Mortality of juvenile fish passing through these structures was extremely high (at least 50% mortality in a study at Lower Monumental Dam, Long et al. 1972 and Long and Ossiander 1974). Over 50% of the yearling chinook outmigration was present in this section of the Snake River during 1972 when 38-51% of the river flow was passing through the slotted gates (Table 8 in Ebel et al. 1973), suggesting that if fish routing was proportional to river flow, 9.5-12.8% of the run was killed per project by this structure alone during 1972. The Corps removed the slotted bulkheads and it is unlikely that fish have been exposed to this level of bypass mortality during recent years.

Based on a review of juvenile survival under historical debris loads and with slotted bulkheads in skeleton bays at some lower Snake River dams, mortality as a function of descaling has varied over time. Thus, it is reasonable to consider a range of estimates for turbine and bypass survival at some Snake River projects in the years prior to 1980. Four sensitivity analyses were identified for pre-80s survival at the upper two projects (i.e., TURB1, TURB4, TURB5, and TURB6).

TURB1: Bypass survival is a function of the structure(s) in place. Neither turbine nor bypass survival at a given project has changed significantly over time. Turbine survival is 0.90 and bypass survivals are listed in **Table A.2.3-4**.

TURB4: Survival due to passage through these routes is significantly lower than would be predicted based on bypass structure alone. Turbine and bypass survivals are both mathematical functions of mortality due to descaling alone (**Table A.2.3-3)** where the rate of mortality due to descaling is resolved over a period of six days after passage.

Table A.2.3-4A: Bypass survival estimates used in TURB1, TURB4, and TURB5/6.

		TURB1=McKern version all years; TURB4=Anderson version pre-'80s; TURB6=Weber version pre-'80s														
		LGR	**LGR**	**LGR**	**LGO**	**LGO**	**LGO**	**LMO**	**LMO**	**LMO**	**IHA**	**MCN**	**J DA**	**TDA**	**BON1**	**BON2**
BY	**OUTYR**	**TURB1**	**TURB4**	**TURB5/6**	**TURB1**	**TURB4**	**TURB5/6**	**TURB1**	**TURB4**	**TURB5/6**	**TURB1**	**TURB1**	**TURB1**	**TURB1**	**TURB1**	**TURB1**
1952	1954															
1953	1955															
1954	1956															
1955	1957															
1956	1958															
1957	1959															
1958	1960															
1959	1961															
1960	1962															
1961	1963															
1962	1964															
1963	1965															
1964	1966															
1965	1967										0.99					
1966	1968										0.99	0.98	0.97			
1967	1969							0.97		0.97	0.99	0.98	0.97			
1968	1970						0.9	0.97	0.97	0.97	0.99	0.98	0.97			
1969	1971				0.97	0.65	0.9	0.97	0.97	0.97	0.99	0.98	0.97			
1970	1972				0.97	0.58	0.834	0.97	0.5	0.97	0.99	0.98	0.97			
1971	1973				0.97	0.51	0.804	0.97	0.51	0.97	0.99	0.98	0.97			
1972	1974				0.97	0.89	0.85	0.97	0.98	0.97	0.99	0.98	0.97			
1973	1975	0.99	0.64	0.87	0.97	0.64	0.8	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1974	1976	0.99	0.79	0.93	0.97	0.68	0.885	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1975	1977	0.99	0.41	0.765	0.97	0.44	0.761	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1976	1978	0.99	0.78	0.93	0.97	0.5	0.8	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1977	1979	0.99	0.84	0.947	0.97	0.76	0.92	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1978	1980	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	

Table A.2.3-4A (cont.): Bypass survival estimates used in TURB1, TURB4, and TURB5/6.

		LGR	LGR	LGR	LGO	LGO	LGO	LMO	LMO	LMO	IHA	MCN	JDA	TDA	BON1	BON2
BY	**OUTYR**	**TURB1**	**TURB4**	**TURB5/6**	**TURB1**	**TURB4**	**TURB5/6**	**TURB1**	**TURB4**	**TURB5/6**	**TURB1**	**TURB1**	**TURB1**	**TURB1**	**TURB1**	**TURB1**
1979	1981	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	
1980	1982	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	0.97
1981	1983	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.97	0.99	0.97	0.97
1982	1984	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1983	1985	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1984	1986	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1985	1987	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1986	1988	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1987	1989	0.99	0.99	0.99	0.97	0.97	0.97	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1988	1990	0.99	0.99	0.99	0.99	0.99	0.99	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1989	1991	0.99	0.99	0.99	0.99	0.99	0.99	0.97	0.97	0.97	0.99	0.98	0.98	0.99	0.97	0.97
1990	1992	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.98	0.99	0.97	0.97
1991	1993	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.98	0.99	0.97	0.97
1992	1994	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.99	0.97	0.97
1993	1995	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.99	0.97	0.97
1994	1996	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.99	0.97	0.97
1995	1997	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.99	0.97	0.97

Table A.2.3-4B: Turbine survival estimates used in TURB1/6, TURB4, and TURB5.

		TURB1=McKern version all years; TURB4=Anderson version pre-'80s; TURB6=Weber version pre-'80s															
	LGR	**LGR**	**LGR**	**LGR**	**LGO**	**LGO**	**LGO**	**LMO**	**LMO**	**LMO**	**IHA**	**MCN**	**J DA**	**TDA**	**BON1**	**BON2**	
BY	**OUTYR**	**TURB1/6**	**TURB4**	**TURB5**	**TURB1/6**	**TURB4**	**TURB5**	**TURB1/6**	**TURB4**	**TURB5**	**TURB5**	**TURB1/6**	**TURB1/6**	**TURB1/6**	**TURB1/6**	**TURB1/6**	**TURB1/6**
1952	1954																
1953	1955																
1954	1956																
1955	1957																
1956	1958																
1957	1959																
1958	1960																
1959	1961																
1960	1962																
1961	1963																
1962	1964																
1963	1965																
1964	1966																
1965	1967											0.90					
1966	1968											0.90	0.90	0.90			
1967	1969							0.90			0.90	0.90	0.90	0.90			
1968	1970							0.90	0.97	0.45	0.90	0.90	0.90	0.90			
1969	1971				0.90	0.65		0.90	0.97	0.45	0.90	0.90	0.90	0.90			
1970	1972				0.90	0.58	0.85	0.90	0.5	0.855	0.90	0.90	0.90	0.90			
1971	1973				0.90	0.51	0.85	0.90	0.51	0.855	0.90	0.90	0.90	0.90			
1972	1974				0.90	0.89	0.85	0.90	0.98	0.855	0.90	0.90	0.90	0.90			
1973	1975	0.90	0.64		0.90	0.64	0.81	0.90	0.97	0.81	0.90	0.90	0.90	0.90	0.90	0.90	
1974	1976	0.90	0.79		0.90	0.68	0.83	0.90	0.97	0.83	0.90	0.90	0.90	0.90	0.90	0.90	
1975	1977	0.90	0.41	0.84	0.90	0.44	0.81	0.90	0.97	0.90	0.90	0.90	0.90	0.90	0.90	0.90	
1976	1978	0.90	0.78	0.87	0.90	0.5	0.85	0.90	0.97	0.90	0.90	0.90	0.90	0.90	0.90	0.90	
1977	1979	0.90	0.84	0.79	0.90	0.76	0.79	0.90	0.97	0.90	0.90	0.90	0.90	0.90	0.90	0.90	
1978	1980	0.90	0.99	0.87	0.90	0.97	0.81	0.90	0.97	0.90	0.90	0.90	0.90	0.90	0.90	0.90	

Table A.2.3-4B: Turbine survival estimates used in TURB1/6, TURB4, and TURB5.

		LGR	LGR	LGR	LGO	LGO	LGO	LMO	LMO	LMO	IHA	IHA	MCN	JDA	TDA	BON1	BON2
BY	OUTYR	TURB1/6	TURB4	TURB5	TURB1/6	TURB4	TURB5	TURB1/6	TURB4	TURB5	TURB5	TURB1/6	TURB1/6	TURB1/6	TURB1/6	TURB1/6	TURB1/6
1979	1981	0.90	0.99	0.88	0.90	0.76	0.86	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1980	1982	0.90	0.99	0.88	0.90	0.90	0.86	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1981	1983	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1982	1984	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1983	1985	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1984	1986	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1985	1987	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1986	1988	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1987	1989	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1988	1990	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1989	1991	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.97	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1990	1992	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1991	1993	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1992	1994	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1993	1995	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1994	1996	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90
1995	1997	0.90	0.99	0.90	0.90	0.90	0.90	0.90	0.99	0.90		0.90	0.90	0.90	0.90	0.90	0.90

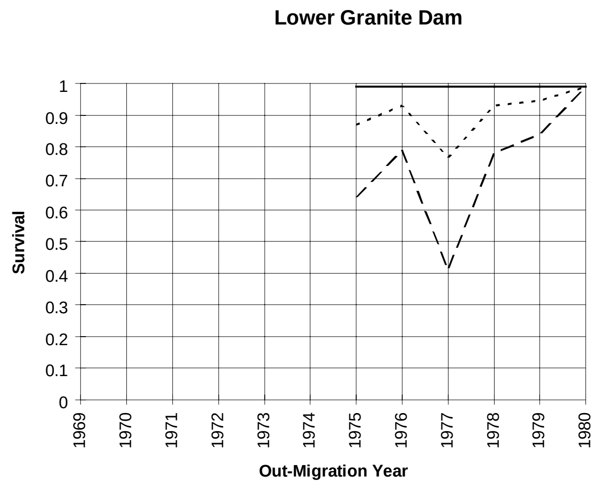

Figure A.2.3-1: Historical dam passage survival estimates for Lower Granite Dam. The solid line (i.e., at 97% survival) represents the standard annual estimates of bypass survival (TURB1). The light dashed line represents the alternative set of estimates designated TURB6. The heavy dashed line represents the set of estimates designated TURB4. See text for descriptions of these alternatives.

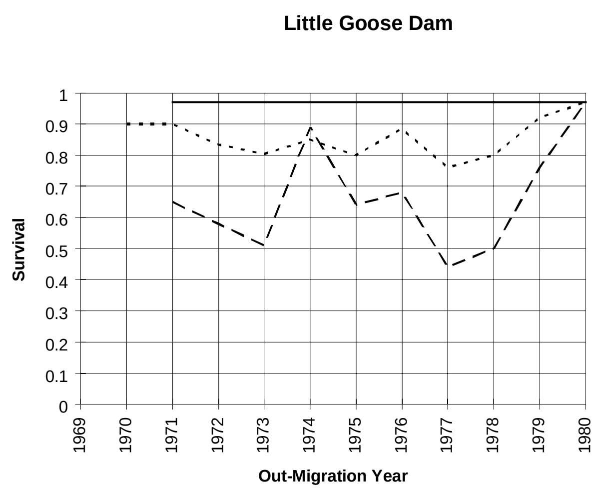

Figure A.2.3-2: Historical dam passage survival estimates for Little Goose Dam. The solid line (i.e., at 97% survival) represents the standard annual estimates of bypass survival (TURB1). The light dashed line represents the alternative set of estimates designated TURB6. The heavy dashed line represents the set of estimates designated TURB4. See text for descriptions of these alternatives.

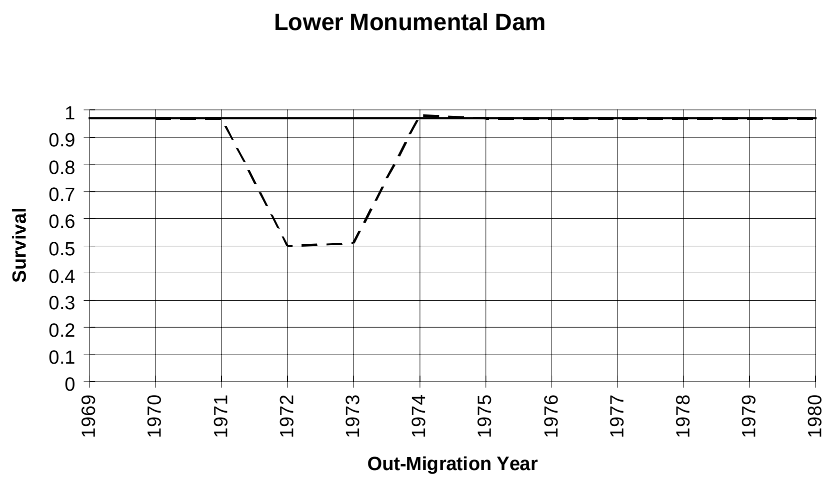

Figure A.2.3-3: Historical dam passage survival estimates for Lower Monumental Dam. The solid line (i.e., at 97% survival) represents the standard annual estimates of bypass survival (TURB1). The dashed line represents the alternative set of estimates designated TURB4. The alternative estimates designated TURB6 were not associated with Lower Monumental Dam. See text for descriptions of these alternatives.

Rationale and Details

Williams and Matthews (1995) stated that descaling due to the presence of forebay trash probably resulted in poor fish survival. Because descaling does not instantaneously kill fish, in order to characterize mortality in studies covering different time periods, the mortality must be expressed by a rate equation of the form:

$$dS/dt = (-r * descaling * S) \qquad \text{[Eq. A.2.3-1]}$$

Where r is the rate of mortality per unit time and percent of the population with greater than 10% descaling, S is the percent of the population alive, descaling is the frequency of occurrence of descaling, and t is the time of the descaling event and observation. Note that, in this equation, descaling is a measure of the stress experienced by fish. Solving Equation (1), the percent alive at a given time, T, is then:

$$S(t) = S0 * \exp(-r * descaling * T) \qquad \text{[Eq. A.2.3-2]}$$

where S0 is an intercept term which adjusts the data and in a sense is the survival that would be experienced if descaling was zero. In general, this term is related to the instantaneous mortality, which is expected to be small because mortality from a stress event is generally delayed. The rate coefficient, r, can be obtained by rearranging Equation (2) as a log-linear regression of the form:

$$\log(100 - mortality) = a + r * descaling * T) \qquad \text{[Eq. A.2.3-3]}$$

where *a*, the intercept term, is the log of the instantaneous survival. Then:

$$\exp(a) = S0 \qquad \text{[Eq. A.2.3-4]}$$

Using Equations [A.2.3-3] and [A.2.3-4], we can investigate the passage/descaling mortality data sets and estimate the mortality rate parameters.

Observations of mortality from dam passage fall into three categories distinguished by the time between passage and observation (short, intermediate, and long). Studies encompassing short periods (i.e., six hours) are available that identify descaling and mortality of fish observed in the collection facilities at Lower Granite and Little Goose Dams between 1981 and 1993. These measurements were collected as part of the Fish Transportation Oversight Team which monitors fish transportation. In general, the observation time in these studies is expected to be within several hours of the stress event, i.e., entering the forebay of a dam. The short period studies have been reviewed by Giorgi (1996).

Intermediate time period studies (i.e., two days) were reported by Williams and Matthews (1995). These involved measurements of descaling and mortality after holding fish for 48 hours before or after truck transport to an area below Bonneville Dam. These observations were made at Little Goose and Lower Granite Dams between 1972 and 1990 and were also reported in Williams and Matthews (1995).

In the long time period studies (i.e., six days), fish were marked and released above and below a dam and the differential mortality in passage was estimated from collections at downstream dams. Raymond (1979), reported mortalities at Little Goose Dam during 1972 and 1973 and more recently (i.e., 1993), turbine survival measurements were reported from PIT-tag studies for Little Goose and Lower Granite Dams (Iwamoto et al. 1994). Descaling observations were not recorded during these long period studies so descaling estimates for the corresponding years have been obtained from Williams and Matthews (1995) and Giorgi (1996).

Statistics for three regressions of mortality against descaling, using the data sets encompassing short, intermediate, and long time periods, respectively, and for a regression combining the data sets, are shown in **Table A.2.3-5.** The regression equation for the combined data:

$$M(t) = 100 - 99.98 * \exp(-0.0058 * descaling * T) \qquad \text{[Eq. A.2.3-5]}$$

assumes a time period of six days to resolve the effects of descaling. That is, the regression provides a coherent and statistically significant fit of data with time factors that vary from a few hours to six days. This suggests that the model can extrapolate mortality observed over several days. That is, the single rate of mortality coefficient, r, fits mortality data over 0.25 to 6 days.

This finding is supported by studies of plasma glucose levels in juvenile chinook after descaling (Congleton et al. 1997). Initial levels prior to descaling or handling were 50 mg/dl. With descaling or simply handling, the levels rose to > 80 mg/dl. Plasma glucose levels in fish handled but not descaled were still above background after two days. Although the fish were handled after four days, plasma glucose levels still had not returned to background after eight days. In further experiments, these authors followed the cumulative mortality of descaled and intact fish exposed to disease. In both conditions, after a five-day induction period, mortality occurred over five additional days.

Table A.2.3-5: Coefficients and significance levels for regressions describing yearling chinook salmon mortality as a function of descaling assuming that mortality takes place over short, intermediate, or long time periods and for all time periods combined. These relationships are the basis for the survival estimates in TURB4.

Time Period	Days(T)	Sample Size (N)	R^2	P-value	Intercept (std err)	Slope (std err)
Short	0.25	26	0.6656	0	4.6067 (0.0023)	-0.0017 (0.0002)
Intermediate	2	30	0.5744	0	4.6145 (0.0291)	-0.0123 (0.0020)
Long	6	4	0.9459	0.0274	4.6217 (0.0767)	-0.0348 (0.0059)
Combined	--	60	0.8308	0	4.6058 (0.0099)	-0.0058 (0.0003)
[F = 284.8748 (1, 58 df), P-value = 0]						

Passage mortalities for the lower Snake River dams during the 1970s were estimated from Equation A.2.3-5 and the observed rates of descaling reported in Williams and Matthews (1995) (**Table A.2.3-6**). Descaling, when combined with other stressors, can have a significant effect on mortality over a period of several days. Because descaling levels of 25% have been associated with mortality rates of 50%, it is reasonable to assume that the measure of descaling is a surrogate of stress in dam passage. *Section A.3.1 (Transportation Assumptions in CRiSP) also discusses trends in descaling rates over time.*

Table A.2.3-6: Estimates of turbine and bypass mortality for TURB4 (see text). "% Descaled column can be compared with similar estimates in Table A.2.3-7, which were used for TURB5 and TURB6

Year	Dam	% Descaled	Turbine/Bypass Mortality (%)	Source
1972	LGS LMO	16 -	42/42 50/50	Eq(6) Data for coho salmon (slotted bulkhead) in Raymond (1979)
1973	LGS LMO	19.6 -	49/49 49/49	Eq(6) and Raymond (1979) Raymond (1979
1974	LGS LMO	- -	11/11 10/2	Using values from Raymond (1979) Assuming same as LGO
1975	LGR LGO	13.0 -	36/36 36/36	Eq(6) Assuming same as LGR
1976	LGR LGO	7.0 11.5	21/21 32/32	Eq(6) Eq(6)
1977	LGR LGO	26.0 23.9	59/59 56/56	Eq(6) Eq(6)
1978	LGR LGO	7.5 20.0	22/22 50/50	Eq(6) Eq(6)
1979	LGR LGO	5.3 8.1	16/16 24/24	Eq(6) Eq(6)
1980	LGR LGO	4.0 -	13/13 10/2	Eq(6) Eq(6)

TURB5: This hypothesis assumes a much lower dam mortality rate than TURB4. During the period of concern, very few fish passed LGR and LGO via the bypass route. Most fish which entered the bypass channel were collected and then transported. Consequently, the high rates of descaling reported for fish encountering screens and blocked or poorly designed orifices have little effect on reach survival estimates of in-river fish.

Because there is some evidence of descaling associated with turbine passage, there is a need to address increased turbine mortality as a sensitivity analysis. Park et al. (1978) indicated that fish sampled in the forebay, which presumably had been swimming in and out of the trash racks, were descaled at a 10-14% rate, approximately half the rate observed for the bypassed fish during 1977. Therefore fish passing through the turbines were assigned a rate of mortality equal to one-half the rate of descaling (**Table 4.2.3-7**) and fish passing through the bypass system (and not transported) have a mortality rate equal to the rate of descaling at a given dam in a given year.

TURB6: Some additional debris-related mortality occurred during early years but survival was higher than would be estimated by TURB4. As bypass conditions improved during the early 1980s, descaling was reduced from double digits to approximately 4%. This rate of descaling continued until recent years. The descaling rates estimated and observed for Lower Granite and Little Goose dams can be applied as the mortality rates for fish that passed these two projects via the bypass route. That is, bypass survival can be estimated as a function of mortality due to descaling, by assuming that the rate of mortality is equal to the rate of descaling. The survival of fish passing through the turbine route would be the same as that described in TURB1 (i.e., 0.90 ± 0.03).

A.2.4 Spill Survival and Spill Efficiency

Spill Survival

Standard Estimate of Spill Survival: The ISG (1996) reviewed estimates of spill survival in the Snake and Columbia Rivers published through 1995. Mortality estimates for 10 of the 13 studies ranged between 0 and 0.022. Estimates from the other three studies were extremely variable (i.e., ranged from 0.04 to 0.275) and should be viewed with caution. In some studies, mortality appears to be higher in spillways with spill deflectors than in those without deflectors, but these differences were generally not statistically significant (e.g., Muir et al. 1995). Additional studies by the Corps are currently underway to resolve this issue.

A value of 0.98 was adopted as the standard estimate of spillway survival. This estimate may be conservative, representing the results of spillway survival studies conducted to with radio-, PIT-, and balloon-tags in spillbays with flow deflectors, operated at discharge rates below 10 kcfs per bay. Survival may decrease as spillbay releases approach flood capacity (about 15+ kcfs), but these conditions have rarely been tested to date. Survival may decrease for juvenile or adult spill patterns adjusted away from the currently slightly-crowned or flat pattern proposed for changing smolt passage distributions for improving spill efficiency. This was the case at The Dalles Dam during 1997, where spill survival was measured at less than 0.8 with a northshore skewed pattern. Uncertainty in spill survival over the range 0.87 to 0.93 was explored in initial passage model runs, but it was apparent from these results that different assumptions about spill survival had very little effect on the model output. Therefore, the analyses presented in this report are based solely on the standard estimate of 0.98.

Table A.2.3-7: Proportion of descaled fish passing through bypass (i.e., "Prop.") used as the basis of the survival rate estimates in TURB5 and TURB6. These can be compared with similar descaling estimates in Table A.2.3-6, which were used for TURB4.

	Lower Granite			Little Goose		
Year	**Units Screened**	**Prop.**	**Comments**	**Units Screened**	**Prop.**	**Comments**
1970	na	na		0 of 3	0.1	Nominal
1971	na	na		0 of 3	0.1	Nominal
1972	na	na		0 of 3	0.166	
1973	na	na		0 of 3	0.196	
1974	na	na		0 of 3	0.15	Mean of range
1975	0 of 3	0.13		0 of 3	0.2	See Note 1
1976	2 of 3	0.07		0 of 3	0.115	
1977	2 of 3	0.235		0 of 3	0.239	
1978	6 of 6	0.07		6 of 6	0.2	
1979	6 of 6	0.053		6 of 6	0.08	See Note 1
1980	6 of 6	0.04		6 of 6	0.08	
1981	6 of 6	0.154		6 of 6	0.135	
1982	6 of 6	0.082		6 of 6	0.26	
1983	6 of 6	0.028		6 of 6	0.199	

Note 1/ No data. Estimate based on ratio of LGR to LGS.

Spill Efficiency

Standard Estimate of Spill Efficiency: Spill efficiency is defined as a ratio of the proportion of the smolt population passed via the spillway (spill effectiveness) to the proportion (percent) of total flow discharged as spill. Steig (1994) reviewed studies at Snake and Columbia River dams published through 1992 and noted that there is considerable variability in daily and weekly spill effectiveness. However, he concluded that most of the results fall around a 1:1 relationship between the proportion of water spilled and the proportion of fish passed in spill (i.e., 1.0 spill efficiency). Giorgi (1996b) reviewed estimates of spill efficiency published through 1993 and pointed out that efficiencies are poorly estimated for most species due to a combination of sparse observations, imprecise estimates, and the reliance of most estimates on hydroacoustic monitoring, which is unable to distinguish among species. He cautioned that the assumption of a spill efficiency of 1.0 could not be justified in most cases. Giorgi implied that a suite of estimates acquired with different methodologies should be considered when deriving species-specific estimates at individual dams. Relying largely on Giorgi's (1996b) review, we concluded that a range of spill efficiencies from 1-2 should be incorporated into sensitivity analyses at dams in the Snake and lower Columbia rivers.

We agree with Giorgi's (1996b) characterization of the uncertainty associated with spill efficiency estimates, but conclude that, if a single estimate must be chosen, most studies support using a factor of 1.0 at all projects except The Dalles (Table A.2.3-1, Figure A.2.4-1). The Dalles Dam has a significantly different configuration than other projects, with a spillway oriented perpendicular to the natural course of the river and the powerhouse oriented nearly parallel to the course of the river. As stated by the Independent Scientific Group (1996), it is not surprising that this project exhibits higher spill efficiency than many other projects. Therefore, a factor of 2.0 was applied at The Dalles Dam at spill levels ≤ 30%. Above 30% spill, the relationship grades from 2.0 to 1.0 according to Equation (1) (Table A.2.3-1, Figure A.2.4-1). This relationship predicts a factor of 1.5 at 65% spill.

(1)

$$P_f = 2.0 * P_w \qquad 0 < P_w \leq 0.30$$
$$P_f = (2.43 - 1.43 * P_w) * P_w \qquad P_w > 0.30$$

where:

P_f = proportion of fish passing over the spillway
P_w = proportion of total river flow passing over the spillway

Spill efficiency, is defined as ($P_f \div P_w$). Support for this relationship comes from several sources. Giorgi and Stevenson (1995) reviewed biological investigations that described smolt passage behavior at The Dalles Dam and discussed implications to future surface bypass and collection research. They cited three investigations that indicated that spill efficiency was near 2.0 when about 20% of the flow passed over the spillway. Included among these studies is Willis (1982), which describes a curvilinear relationship in which spill efficiency is ≥ 2.0 at spill below approximately 30% of total river flow, the efficiency declines to about 1.4 at 60% spill, and declines to 1.0 at 100% spill. A recent radiotelemetry study at The Dalles by Holmberg et al. (1997) supports this general relationship. Spill efficiency for yearling chinook was 2.3 at 30% spill and 1.25 at 64% spill in 1996.

We also suggest application of a sensitivity analysis to the estimate of spill efficiency at Lower Granite, Little Goose, and Lower Monumental dams if there is sufficient time. The suggested sensitivity analysis for Snake River projects relies on a relationship for spring chinook salmon at Lower Granite Dam that is based on radio-telemetry observations (Wilson et al. 1991). Because of the similarity of the three projects we see no reason to assume that the Lower Granite radio-telemetry results are unique to that project. (Similar studies have not been conducted at other Snake River projects). By combining the radio-telemetry observations with assumptions that: (1) 0% of fish pass the spillway at 0% spill and (2) 100% pass at 100% spill, the following relationship (Smith et al. 1993, Figure 1) can be applied:

(2) $P_f = 2.583*P_w - 3.250*P_w^{\ 2} + 1.667*P_w^{\ 3}$

where:

P_f = proportion of fish passing over the spillway
P_w = proportion of water passing over the spillway

Spill efficiency, is defined as ($P_f \div P_w$). As cautioned by the Independent Scientific Group (ISG 1996), the shape of this relationship is highly uncertain outside of the range of observations (20%-40% spill), even though P_f must logically go to 0 and 1.0 at the extremes.

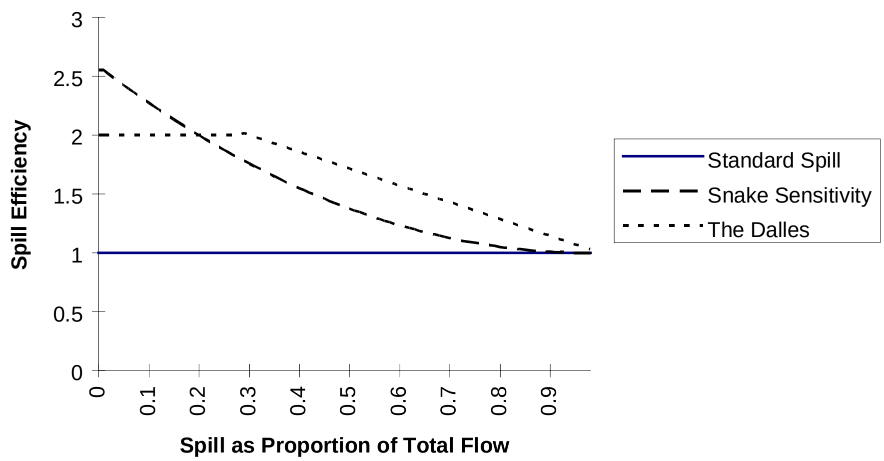

Figure A.2.4-1: Comparison of standard spill efficiency at all projects other than The Dalles, standard spill efficiency at The Dalles, and the sensitivity analysis for spill

A.2.5 Predator Removal Efficiency

Relationships for reservoir survival in FLUSH and CRiSP are based on reach survival estimates that were collected from *?-199?* and on predation research conducted primarily in the 1980s. The survival relationships thus depict the system prior to about *199?*, and system changes that occurred in recent years might not be fully captured in these relationships. Since 1990, the Bonneville Power Administration has funded various programs designed to remove large northern squawfish from Snake and Columbia river reservoirs, and over 900,000 predators have been collected (Beamesderfer et al. 1996). Since northern squawfish are believed to be a major source of mortality for juvenile salmonids (Rieman et al. 1991), changes in predation pressure could have a significant influence on reservoir survival. We estimated the highest and lowest potential effects of squawfish removal on spring/summer chinook survival to use a bounds in sensitivity analyses (25% and 0% reductions in reservoir mortality).

<< next 2 paragraphs need to be edited>>

The higher sensitivity adjustment was estimated first. The average predation -related rate of mortality for salmonids migrating in April-June was estimated to be about 9% of all juvenile salmon entering John Day Reservoir, or 11% as the mean plus 1 SD (Rieman et al. 1991; their Table 6). Early studies suggested that sustained removal *(Reviewer's note: sustained for how long?)* of the northern squawfish from John Day Reservoir would reduce predation-related mortality by 5-% (Rieman and Beamesderfer 1990). Average annual exploitation rates in Columbia and Snake River reservoirs have changed from 9-16% during 1991-1995 (Beamesderfer et al. 1996). Assuming recent removals could be reducing predation by 50%, *(Reviewer's note: Relative to?)* the upper limit for sensitivity adjustment would be 5.5% (0.5 x 0.11 = 0.55). *(Reviewer's note: Methods need to be more clearly stated in this paragraph.) <How do these limits relate to the 0 and 25% used in the decision analysis?>*

For the lower sensitivity adjustment, we first reduced by one-third the average April-June mortality caused by predators (adjusted mortality = 7.5.%), *(Reviewer's note: show equation)* which corresponds roughly to predation loss estimates in Petersen (1994) based on finer subdivision of the reservoir. Next, we estimated exploitation *(Reviewer's note: of squawfish?)* as the average for the lower 95% confidence bound of achieved exploitation rates (10%; Beamesderfer et al. 1996; their Table 2). Assuming a 10% exploitation rate would produce a 25% reduction in predation-caused mortality, the lower estimate for survival change in spring/summer chinook would be 1.9% (0.25 x 0.75). Compensatory responses by the northern squawfish that remain the system, or by predaceous smallmouth bass or walleye, could reduce the effectiveness of the predator removal program (Beamesderfer et al. 1996; J.H. Petersen *unpublished analyses*), so we set the lower limit for sensitivity adjustment to 0% (Table 1). *(Reviewer's note: analysis needs clarification.)*

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A.2.6 Drawdown

Uncertainties related to drawdown were focused on the duration of and survival rates during four time periods:

1. Pre-removal - the period between when the region decides to proceed with drawdown and when physical removal of dams begins.
2. Removal - period in which engineering work to breach or circumvent the dams is carried out.
3. Transition - period beginning just after the dams are removed and continuing until fish populations attain some equilibrated conditions.

4. Equilibrium - period of time from when fish populations equilibrate to the end of the simulation period.

Alternative hypotheses were defined for three uncertainties that the Drawdown workgroup considered to be most important in determining the biological response. These three key uncertainties were:

a) Duration of the pre-removal period
b) Juvenile survival during the transition period
c) Equilibrated juvenile survival rates

a) Duration of the pre-removal period

Uncertainties in the duration of the pre-removal period are related to the Congressional appropriations cycle and the potential for litigation following the regional recommendation. The best-case scenario was considered to be 3 years, which would be the case if there were no litigation and the regional decision was made in time to catch the next appropriations cycle in Congress. The worst-case scenario of 8 years allows for delays due to litigation and potential problems in getting Congressional approval and funding for drawdown.

b) Juvenile survival during the transition period

Batelle's Pacific Northwest Environmental Laboratory is working on modeling timing of physical processes for the Corps, but preliminary results are not expected until Spring 1998. In the meantime, the group made some preliminary estimates of transition timing based on available evidence. Information presented at one of the drawdown meetings included:

Timing of physical responses (Corps estimates):
- sediments in main channel scoured out within 2 years of drawdown
- sediments along banks partially scoured out approx. 10 years after drawdown
- banks stabilized, recolonization by vegetation approx. 10 years after drawdown

Timing of biological responses
- response to removal of dam mortality should be immediate
- redistribution of predators within river could take place relatively quickly
- population responses by predators at least 5 years after drawdown (based on time to maturity for predators), although drawdown is not likely to reduce predator populations

Based on this information, the group decided on 2 years and 10 years as alternative hypotheses about the length of the transition period. The lower bound represents the effects of short-term physical (scouring of main channel) and biological (redistribution of predators) processes. 10 years reflects the effects of longer term physical processes, such as bank stabilization, and predator population responses to drawdown.

Juvenile survival rates could follow any of a number of trajectories during the transition period. Optimistic trajectories would have an immediate increase in juvenile survival following dam removal equal to the amount of dam mortality associated with the 4 Snake River dams. Pessimistic trajectories might involve an initial decrease in survival following dam removal resulting from the release of sediment from the drawndown reservoirs. For the purposes of the preliminary analysis, juvenile survival rates were assumed to simply increase linearly during the transition period, from their initial pre-removal values to their equilibrated values.

c) Equilibrated juvenile survival rates

The group identified two alternative hypotheses about equilibrated juvenile survival rates. These hypothesized rates are based on pre-dam survival estimates and on current estimates of survival in free-flowing reaches of the Snake River above Lower Granite Dam (Table A.2.7-1), expanded to correspond to the 210-km reach encompassed by the four Snake River dams. Although there are numerous caveats involved in using these reach survival estimates to predict equilibrated survival rates (e.g., reach sections in recent estimates have little overlap with reaches in historical estimates and with the reach that would correspond to natural river drawdown), the data in Table A.2.7-1 do provide an empirical basis for defining the bounds of the biological responses we might expect following drawdown.

Based on these data, the group developed 2 hypotheses:

H1: Survivals will return to 0.96, the level observed as they were prior to the construction of the dams. Implicit in this hypothesis is the assumption that predator densities in the free-flowing river following drawdown will return to historical levels and that other changes since 1968 have had negligible effects. This value is based on average survival for the 1966-1968 period, before Lower Granite, Little Goose, and Lower Monumental (Table A.2.7-1).

H2: Survival rates will not return to the same level as they were prior to the dams because of changes since the historical period, including increased shoreline development, the effects of introduced species, changes in upstream water regulation, and permanent changes in predator communities that result from impoundment of the river.

The equilibrated survival rate under this hypothesis is 0.85, which is based on 1993-1996 average survival of wild juveniles for free-flowing reaches between Whitebird and Imnaha traps and Lower Granite (Table A.2.7-1).

Alternative hypotheses were implemented in FLUSH by “hardwiring” the survival rates within the code and bypassing model components related to survival through the four Snake River dams. In CRiSP, equilibrated survival rates were modeled by adjusting the predator densities upward (for the lower hypothesis of 0.85) or downward relative to the current value for that parameter.

Year	Wild/ Hatchery	Survival	Travel Time (Days)	Median Arrival Date	Reach	Downstream Project Survival	Mainly River Survival	km Mainly River	River Survival/km	River Survival/210 km
1966	Unknown	0.85	14	129.0	Whitebird (Salmon R.) - IHR Arrivals	0.950	0.895	339.00	0.999671955	0.9334
1967	Unknown	0.88	16	134.5	Whitebird (Salmon R.) - IHR Arrivals	0.950	0.926	339.00	0.999774244	0.9537
1968	Unknown	0.95	15	132.0	Whitebird (Salmon R.) - IHR Arrivals	0.950	1.000	339.00	1.000000000	1.0000
								Mean Historical Estimate	0.999815399	0.9624
1993	Wild	0.83	11.3	120.0	Whitebird (Salmon Trap) - LGR Tailrace	0.920	0.904	184.00	0.999453730	0.8916
1994	Wild	0.79	13	116.7	Whitebird (Salmon Trap) - LGR Tailrace	0.920	0.857	184.00	0.999158639	0.8380
1994	Wild	0.76	17.2	113.2	Imnaha Trap - LGR Tailrace	0.920	0.826	93.00	0.997947751	0.6496
1995	Wild	0.86	13.3	120.6	Whitebird (Salmon Trap) - LGR Tailrace	0.920	0.938	184.00	0.999652457	0.9296
1995	Wild	0.91	10.5	120.1	Imnaha Trap - LGR Tailrace	0.920	0.988	93.00	0.999870669	0.9732
1996	Wild	0.82	20.3	113.1	Whitebird (Salmon Trap) - LGR Tailrace	0.920	0.893	184.00	0.999388050	0.8794
1996	Wild	0.81	11.1	119.8	Imnaha Trap - LGR Tailrace	0.920	0.884	93.00	0.998671393	0.7564
							Recent Wild Mean:	Whitebird+Imnaha	0.999163241	0.8454
1993	Wild	0.83	9.4	123.1	Clearwater Trap - LGR Tailrace	0.920	0.898	12.00	0.991058639	0.1517
1994	Wild	0.84	13.1	116.5	Clearwater Trap - LGR Tailrace	0.920	0.915	12.00	0.992644381	0.2122
1995	Wild	0.88	12.1	111.2	Clearwater Trap - LGR Tailrace	0.920	0.955	12.00	0.996208143	0.4503
							Recent Wild Mean:	Clearwater	0.993303721	0.2714

A.3 Other Uncertainties / Alternative Hypotheses

This section is structured in a form parallel to Section 4.3 of the main report. It is worth reviewing Section 4.3 prior to reading this section.

A.3.1 Transportation Assumptions

Transportation Rules

The proportion of fish transported in the models prospectively is determined by the FGEs used in the passage models and the rules for spill and collection. The FGEs have been standardized amongst models and outlined in Section 4.2.2 and A.2.2. The rules for collection and spill for scenario A1 are as follows:

Case 1: If the seasonal average (April 10-June 20) flow is projected at <85 kcfs, implement as in Biop: no spill at LGR, LGS, LMN and transport all fish collected at those projects. No transportation at MCN.

Case 2: If seasonal average flow is projected to be >=85 kcfs and <100 kcfs, implement as in the Biop: no spill at LGR and transport all fish collected. At LGS and LMN spill to 12-hour levels described in Biop (80 and 81 % of total project flow, respectively) or to spill cap in 1997 Water Management Plan (50 and 40 kcfs, respectively) and transport all fish collected. No transportation at MCN.

Case 3: If seasonal average flow is >=100 kcfs, manage as in 1997: at LGR, LGO, and LMO spill to the 12-hour levels described in the Biop (80, 80, and 81 % of total project flow, respectively) or to the spill cap in the 1997 Water Management Plan (45, 50, and 40 kcfs, respectively). At LGR, transport all fish collected. At LGS and LMN bypass all fish from the "B" side of separators and transport all fish from the "A" side. No transportation at MCN.

In the passage models, average separator efficiencies for LGO and LMO were used for the prospective simulations.

For scenario A2, transport all fish collected at LGR,LGO,LMO, and MCN. There is no voluntary spill at collector projects.

For scenario A3, there is no transportation.

Transportation assumptions used with FLUSH (TRANS1 and TRANS2)

The transport (T) and control (C) data set used in the FLUSH transport models is summarized in Table A.3.1-1. The raw data represent all available transport studies conducted at Lower Granite (LGR) and Little Goose (LGO) dams, 1971-1989. The quantity T/C (Φ) is defined as follows:

$$T/C = \left[\frac{n_t}{N_t}\right] \Big/ \left[\frac{n_c}{N_c}\right] \qquad \text{[A.3.1-1]}$$

where:

n_t = number of marked adults returning to upper most transport project that were transported as smolts
N_t = number of marked smolts that were transported from the reference project
n_c = number of marked adults returning to upper most transport project that migrated in-river as smolts
N_c = number of marked smolts that migrated in-river

The T/C results from Snake River yearling chinook studies were used to develop a T/C function for use in the FLUSH passage model. Results from all studies were used, with the following exceptions: 1) Studies in which treatment fish were transported in saltwater; and 2) Study fish from 1968-70 were transported from Ice Harbor Dam (where mass transport has never been implemented); hence these studies were not included.

The yearly T/C estimates are the weighted geometric means of the T/C estimates derived from different release groups for that year. The T/C estimates come from the test and control release and recapture numbers in Park (1985), except source documents are used for years when Park presents only pooled numbers from multiple studies (1975 and 1976), and for transport studies done after Park's report (1986 and 1989). The source documents are also used for one freshwater transport release group study in 1978 which Park (1985) omits without explanation.

The T/C estimates are ratios of binomial proportions, with theoretical variances described by

$$\sigma_t^2 = \frac{p_t q_t}{N_t} \text{ and } \sigma_c^2 = \frac{p_c q_c}{N_c} \qquad \text{[A.3.1-2]}$$

for the transport recovery proportion (p_t) and control recovery proportion (p_c), respectively, with q_t = 1 - p_t and q_c = 1 - p_c . The transport and control proportions are given by

$$p_t = \frac{n_t}{N_t} \text{ and } p_c = \frac{n_c}{N_c}, \qquad \text{[A.3.1-3]}$$

respectively. The variance of the natural log of the T/C estimates can be derived by using the delta method of variance approximation for a function of random variables. The resulting variance of the natural log of the T/C estimate is:

$$VAR\left(\ln\left(T/C\right)\right) = \frac{1}{n_t} + \frac{1}{n_c} - \frac{1}{N_t} - \frac{1}{N_c} \qquad \text{[A.3.1-4]}$$

For years in which more than one useable study was performed, a weighted geometric mean T/C for each year was derived as follows:

Calculate the variance of the natural log of each usable T/C estimate for that year (see formula, above).

Calculate a weight for each study as the inverse of the variance:

$$W = \frac{1}{VAR\left(\ln T/C\right)}$$

Multiply the natural log of each T/C estimate by its corresponding weight:

Sum the weighted estimates over all useable transportation studies performed during a given year and calculate the weighted mean ln(T/C) by dividing the sum of the weighted estimates by the sum of the weights:

$$WeightedMean \ln\left(T/C\right) = \frac{\sum_{i=1}^{k} \frac{\ln\left(T/C\right)_i}{VAR_i\left(\ln\left(T/C\right)\right)}}{\sum_{n=1}^{k} \frac{1}{VAR_n\left(\ln\left(T/C\right)\right)}}$$

where there are “k” useable T/C ratios from studies performed within that year.

Calculate the weighted geometric average as the base of the natural logarithm raised to the power of the weighted mean ln(T/C):

$$WeightedGeometricMean\left(T/C\right) = \exp\left(WeightedMean \ln\left(T/C\right)\right)$$

In years where studies were done from both LGR and LGO, the data are combined without making any adjustment to the T/Cs, so the result is an estimate of the T/C for the LGR/LGO aggregate studies.

A primary data limitation is that controls are not fully representative of in-river migrants (IFWTS 1993; Mundy et al. 1994; Ward et al. in press). A key assumption of the transport studies is that the survival of the control fish, which were collected, handled and in most studies transported to their release site, accurately represents survival of in-river migrating fish and that their treatment during the test does not cause significant additional mortality above the level sustained by the in-river migrating population (IFWTS 1993). In an independent review of transportation research, Mundy et al. (1994) state:

> The use of the term, *control*, does not imply that the reviewers consider these individuals to actually be controls. The term is used only for consistency with the nomenclature used by the NMFS. The NMFS control group is considered by the review team to be another treatment group that is used for comparative purposes.

Reported T/Cs for some studies are inflated because control groups were transported upstream substantial distances and had to migrate through the upstream project twice. Controls were released 15-50 km upstream of the transport project in 1971, 1972, 1973, 1975, and 1976 (Table A.3.1-1). Therefore, smolt numbers for in-river controls were adjusted for the mortality of going through a dam twice in 1971, 1972, 1973, 1975,1976 and one group in 1978. Each set of parameter values for historic FGEs, bypass survival, turbine survival and spill efficiency (model runs R1-R8) required a separate adjustment of control fish. An adjusted T/C was then calculated for transport model 1 (Table A.3.1-2).

Table A.3.1-1: Unadjusted T/C data, 1971-1989.

Year	Collection Point	Control Release Point*	Control Releases	Treatment Releases	Control Returns	Treatment Returns	T/C	Source
1971	LGO	LGO[a]	20673	30637	52	119	1.54	Park 1985
1971	LGO	LGO[a]	20673	35252	52	147	1.66	Park 1985
1972	LGO	LGO[a]	32836	54906	25	45	1.08	Park 1985
1972	LGO	LGO[a]	32836	51500	25	44	1.12	Park 1985
1973	LGO	LGO[a]	88170	83606	20	261	13.76	Park 1985
1973	LGO	LGO[a]	88170	57758	20	241	18.39	Park 1985
1975	LGR	LGR[a]	42915	30127	127	145	1.63	Park et al. 1979
1975	LGR	LGR[a]	42915	38423	127	294	2.59	Park et al. 1979
1976	LGO	LGO[a]	27315	36239	7	9	0.97	Park et al. 1980
1976	LGO	LGO[a]	12255	32366	1	16	6.06	Park et al. 1980
1976	LGR	LGR[a]	21711	47507	9	7	0.36	Park et al. 1980
1976	LGR	LGR[a]	2847	25411	1	9	1.01	Park et al. 1980
1978	LGO	LGO[b]	36441	49391	5	5	0.74	Park 1985
1978	LGR	LGR[c]	8249	56546	3	66	3.21	Park 1985, Park et al. 1983
1978	LGR	LGR[c]	8249	43855	3	33	2.07	Park 1985, Park et al. 1983
1978	LGR	LGR[c]	8249	38685	3	5	0.36	Park et al. 1982, 1983
1979	LGR	LGR[c]	25532	27336	3	12	3.74	Park 1985
1986	LGR	LGO[d]	45035	45004	47	74	1.58	Matthews et al. 1992
1989	LGR	LGO[d]	107176	75295	28	46	2.34	Harmon et al. 1993

* Ward et al. 1997:

[a] Trucked 15-50 km upstream from collection site

[b] Released directly into tailrace of dam where collected

[c] Trucked downstream and released into tailrace of dam where collected

[d] Trucked downstream to tailrace of LGO, though collected at LGR

Table A.3.1-2 below contains the annual weighted T/C ratios used to construct a function which predicts T/C ratios from in-river survival. There is a pair of different parameter values for each assumption for historic dam passage survival. In the prospective analysis, the FLUSH reservoir survival/FTT relationship calibration is matched with the T/C function for the same TURB assumption.

Table A.3.1-2: Annual Weighted geometric mean Transport to Control ratios (T/C) for Snake River yearling chinook. The adjusted T/Cs are computed by adjusting the number of control fish for the mortality experienced by being transported upstream of the collector project and migrating through that project twice. Mortality adjustment is based on TURB assumption from passage models.

Year	T/C 1/	T/C 2/	T/C 3/	T/C 4/	T/C 5/
1971	1.60	1.54	1.54	1.52	1.54
1972	1.10	0.95	0.78	0.97	0.93
1973	15.90	14.51	8.43	13.04	14.31
1975	2.12	2.04	1.90	2.01	2.03
1976	0.77	0.73	0.68	0.72	0.73
1978	1.30	1.27	1.14	1.25	1.27
1979	3.74	3.74	3.74	3.74	3.74
1986	1.58	1.58	1.58	1.58	1.58
1989	2.34	2.34	2.34	2.34	2.34

1/ unadjusted geometric mean T/C
2/ adjusted geometric mean T/C (controls adjusted for TURB 1 assumptions)
3/ adjusted geometric mean T/C (controls adjusted for TURB 4 assumptions)
4/ adjusted geometric mean T/C (controls adjusted for TURB 5 assumptions)
5/ adjusted geometric mean T/C (controls adjusted for TURB 6 assumptions)

Controls were collected, handled and marked at the study project, unlike true in-river migrants which experience a combination of collection/bypass, turbine and spill passage routes at this site. Preliminary analysis of return rates of PIT tagged wild smolts from 1994 to 1995 further suggests that delayed mortality of in-river migrants may be related to route of passage through the hydropower system (Weber et al. 1997). Smolts that were detected (i.e., were bypassed) two or more times returned at lower rates than those detected one or zero times (Figure A.3.1-1). The T/C data were not adjusted in either transport model to account for these potential problems.

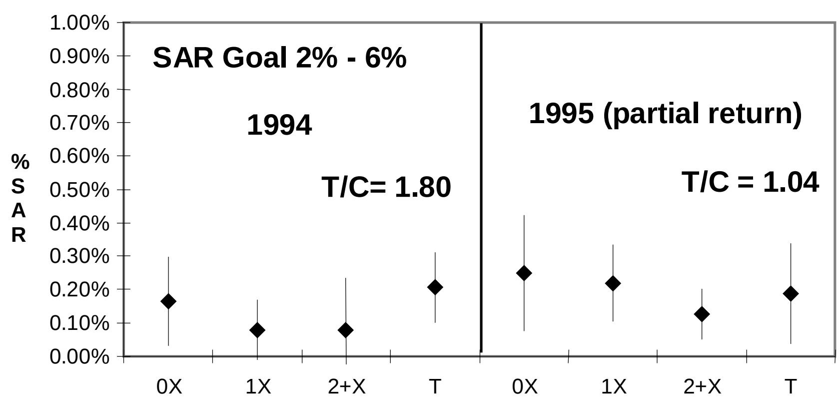

Figure A.3.1-3: Smolt to adult return rates of wild yearling PIT tagged chinook released above lower Granite Dam, 1994-95 (NMFS data). 0X, 1X and 2+X refer to number of times a smolt was detected and bypassed, T refers to transported smolts.

Two transport releases were made in 1975 (Table A.3.1-1), one group was branded and coded-wire tagged and the other was wire-tagged only, to determine whether the additional stress of double marks had a negative effect on survival (IFWTS 1993). Controls in 1975 were double marked with wire-tags and brands. Park et al. (1979) reported that the wire-tagged only transport group had significantly higher T/C than the double marked group (2.60 vs. 1.63; Table A.3.1-1). IFWTS (1993) stated that the more appropriate comparison in 1975 was the reported T/C of 1.63 because of the marking differences. Both reported T/C values were included in the data set for the fitted transport models.

Available T/C estimates do not span the full range of recent passage conditions. No controls and few transported chinook returned from the worst flow year, 1977 (Park 1985; Mundy et al. 1994). Also, transportation studies were not conducted during years with higher flows and spills (e.g., 1982-1984 smolt migrations) in which Snake River chinook stocks experienced overall higher smolt-to-adult return rates (Raymond 1988) and adult-to-adult survival rates (Deriso et al. 1996; Schaller et al. 1996).

The ability of transportation to improve survival to the spawning grounds and hatcheries has not been demonstrated for spring/summer chinook because transport studies were conducted at the mainstem dams (Mundy et al. 1994). Some evidence suggests that T/Cs are lower in natal areas than at the dams (Olney, et al. 1992). Based on recoveries at Lower Granite Dam and natal areas (hatcheries and spawning grounds) from the 1986 and 1989 transport studies, the estimated T/C in the natal areas was 83% of that estimated at the mainstem dams. This pattern was explored in transport model 2 which used a factor of 0.83 to adjust the T/C estimates of transport model 1.

The 1971 through 1979 survival study estimates were expanded to estimate in-river survival below the transport project to Bonneville Dam. Since there were no reach survival studies in 1986 and 1989 estimates were generated through FLUSH. The adjusted T/C (Table A.3.2-2) and in-river survival estimates from Lower Granite (LGR) and Little Goose (LGO) dams on the Snake River were used in FLUSH.

Transport models were fit to the following form:

$$T/C = (1/(1 + \exp(-(s - a) / b))) / s \qquad [A.3.1\text{-}5]$$

where:

s = in-river survival from tailrace of the transport dam; and
T/C = adjusted T/C ratio.

The transport rules described above were used to estimate the proportion of the population of smolts that are transported in the prospective simulations alternative management scenarios. The T/C estimates are used in the prospective passage model simulations to calculate system survivals. The retrospective and prospective λ_n's, along with the retrospective and prospective system survivals are then used in equation A.3.2-20 to derive the prospective river mortality rate (m_y) to use in prospective BSM runs. The annual T/C values are generated in the passage models using the function in equation A.3.1-5 which relates T/C to in-river survival (V_n). Note that the D values (the ratio of delayed survival factors for transported fish to those of in-river migrating fish) are computed by equation A.3.2-13, and are a consequence of the passage model projected V_n and T/C. The important point to note, therefore, is that D values are specific to each passage model and are not an empirical measurement.

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Comment on Transportation assumptions in FLUSH by Williams and response by Schaller et al.:

There is insufficient justification to adjust the transport T/Cs by 0.83, particularly if it is based on conclusions from transport reviews in the early 1990s which only looked at results from 1986 studies. Conclusions are meaningless because of small sample sizes and results from other years, particularly recent ones do not show the same trends.

Response by Schaller et al.: *This was one of two models in a sensitivity; the other assumed no decrease in T/C from the dam to the natal areas. Based on available information, one cannot conclude that T/C ratios do not decrease between the upper dam and the natal areas, therefore the sensitivity is justified. This sensitivity used the geometric mean of two years studies (1986 and 1989):*

Year	*Group*	*Smolts released*	*Adults at dam*	*Adults at natal*	*T/C dam*	*T/C natal*	*natal/dam ratio*
1986	*C*	*45035*	*47*	*28*			
1986	*T*	*45004*	*74*	*29*	*1.58*	*1.04*	*0.66*
1989	*C*	*107176*	*28*	*14*			
1989	*T*	*75295*	*46*	*24*	*2.34*	*2.44*	*1.04*
					Geometric mean =		*0.83*

End of comment on transportation assumptions in FLUSH by Williams and response by Schaller et al.

Comment on transportation assumptions in FLUSH by Anderson and response by Wilson et al.:

The critique of the FLUSH transportation assumptions is developed from an initial Critique (Anderson December 22 1997) and a response (Wilson et al. January 9 1998). The FLUSH transport hypothesis "... does not single out one particular mechanism or index among many to predict direct or delayed mortality" (Wilson 1998). It assumes that in-river survival V_s is a measure of the factors affecting both transport and non-transport fish returns for both retrospective and prospective years. The empirical relationship, which is, they admit, not mechanistic, is written

[15]

where T/C are adjusted, a and b are regression constants, and V_s is the "adjusted" in-river survival from the transport dam to Bonneville Dam tailrace. The adjusted in-river survival is expressed by

[16]

where V_t is survival to the transport dam and V_n is the total in-river survival.
Derivation of the FLUSH hypothesis for D follows from Eq [15] as

[17]

where for simplicity the coefficients α = exp(a/b) and β = 1/b are defined and these constants are extracted from the regression of T/C to V_s. according to Eq [15], and V_T is the direct transport mortality and is generally assumed constant at ~ 0.98. It then follows that D is only a condition of the adjusted in-river survival V_s.

Equations [15], [16] and [17] become a foundation of the delayed mortality hypothesis in FLUSH. The model was derived from retrospective data between T/C and V_s and the same relationship is used to predict prospective T/C from prospective V_s and consequently prospective D values. There are two problems with this approach, one statistical and one conceptual.

The statistical problem involves whether or not Eq [15] is significant. The regression used data from 1971 through 1989 and excluded T/C studies from 1968 to 1970 and the more recent studies in 1994 and 1995. In addition the individual studies within years were averaged so different release and transport conditions were averaged. To gain some idea of the statistical significance Eq [15] was rearranged in a linear form to estimate the regression coefficients α and β. Putting Eq [15] in the form

[18]

the regression results using TURB4 adjusted T/C and V_s were:

Residual Standard Error = 0.2517, Multiple R-Square = 0.5026
N = 9, F-statistic = 7.074 on 1 and 7 df, p-value = 0.0325

	coef	std.err	t.stat	p.value
ln α	-0.2401	0.0272	8.8407	0.00001
β	0.0738	0.0277	-2.6597	0.0325

The regression was significant, but it was confounded by having V_s in both the dependent and independent terms. To assess the actual significance of the regression the STFA (state and tribal fisheries agencies) need to present the statistical details of the regression used to estimate Eq [15].

<u>Response by Wilson et al</u>.:*The function was fit by non-linear routine, so the independent variable (Vs) did not appear in the dependent part of the equation.*

Comment by Anderson continues:

The second problem with the FLUSH delayed mortality hypothesis is the invariant coupling of transport and non-transport delayed mortality with in-river survival. This is best understood from Eq [16] although it is also contained in the T/C definition through Eq [15]. The parameter D is the ratio of mortalities of two groups of fish that have vastly different histories prior to entering the estuary. Expressing their ratio through a single variable, the in-river survival of the non-transport group, is a strong inference that is only achieved in two possible conditions. Either the delayed mortality in one group is constant over all years or there is an explicit relationship between the two groups that is invariant with changes to the hydrosystem or transportation. The STFA claim the latter but since the hypothesis is not mechanistic there is no explanation of its basis.

In particular the hypothesis requires that λ_T will change for any change in V_s irrespective of the transported fish experiencing any of the in-river effect. That is, delayed transport survival will change if changes are made on the hydrosystem downstream of the transported fish collection point. To demonstrate this characteristic we write the incremental change in λ_T for an incremental change in V_s. The equation is

[19]

where all terms are positive. From the regression of Eq [18] $\alpha = 1.27$, $\beta = 0.0738$, $V_T \sim 1$ and from the STFA hypothesis improvements in non-transported fish in-river survival will improve their delayed post-hydrosystem survival. Under these conditions Eq [19] is positive so changes in non-transport fish survival directly changes the delayed survival of transport fish. This result is problematic it is difficult to envision a biological mechanism that would produce this effect. Note that by considering the derivatives as is done in Eq [19] the ambiguities of dealing with the ratio of survivals in Eq [17] are clarified. [Note: Hinrichsen pointed out this result can be simply demonstrated by substituting Eq. [20] into Eq.[17] and arranging the expression in terms of λ_T.]

In the CRiSP descaling hypothesis, x affects transport delayed mortality according to Eq [14] and in-river survival according to Eq [5]. Changes in V_{res} or N do not affect descaling and do not affect delayed transport survival. In the FLUSH hypothesis, changes in any variable that affects in-river survival also affects D. This results in a problematic characteristic in the FLUSH hypothesis. It says that all experiences of the non-transported fish below the collection dam directly affect the fate of the transported fish in the estuary and ocean, even though the transport fish do not pass through the river. For example, under the FLUSH hypothesis removal of McNary Dam would improve the post hydrosystem survival of fish transported from Lower Granite by truck or barge.

Response by Wilson et al. The problem with this conclusion is that this CRiSP approach was not used in the analysis in the Decision Analysis report. However, more important for the approach relating descaling to D is how the T/C experiments were performed. Mundy et al. 1994 point out that "only those fish judged to be in good condition are typically diverted to the marking stations; fish that are diseased, descaled, previously marked, or in poor condition are systematically excluded from the experimental lots (Smith et al. 1981, Matthews et al. 1988)." Note that the D values are estimates specific to each passage model and are not an empirical measurement. It is difficult to follow the logic of the relationship of descaling to D, given the fact that the descaled fish were excluded from the transport experiments which he uses to fit his relationship.

End of comment on transportation assumptions in FLUSH by Anderson and response by Wilson et al.

Transportation assumptions used with CRiSP (TRANS 3)

Transportation effectiveness is characterized primarily by the ratio of the post Bonneville survival of fish that were transported to Bonneville Dam to fish that arrive in the tailrace via an in-river passage route. The relative effect is expressed by D which is defined as:

$$D = \lambda_t/\lambda_n \qquad \text{[Eq. A.3.1-6]}$$

where λ_t is the post Bonneville survival of transported fish and λ_n is the post Bonneville survival of non-transported fish.

On theoretical grounds, D is expected to change with differences in the ability of transported and non transported fish to survive in the post hydropower system life stage. For convenience, we refer to this as the estuary passage stage.

A primary factor determining how D will change over time is the relative condition of transport and non-transport fish as they begin their passage through the estuary. The working hypothesis is that fish condition affects fish survival. Fish condition of the two groups can depend on a number of factors including, but not limited to, stress in collection including descaling and routing into barges, stress in transportation including stress of mixing species in barges and trucks, overcrowding in barges and trucks, and stress at release back into the river below Bonneville Dam. These factors all may have yearly variations resulting changes in hydrosystem and transport operations. In addition fish stress can vary from year to year because of natural variations in environmental conditions prior to and during transportation and in-river migration.

In general we expect D to have changed from year to year as reflected by natural variations in the river system and by improvements in hydrosystem and transportation operations that have reduced fish stress. Perhaps the major improvement to operations was the implementation of a trash removal program in 1980. As was noted by Williams and Matthews (1995), prior to the regular removal of trash, the percent of both transported and bypassed fish with observed signs of descaling was significant and variable. After the inception of the trash removal program descaling declined. This change in the descaling levels significantly improved the condition of fish released from barges. A second stressor that has been reduced over time is due to changes in the mode of transportation. In early years of the transportation program fish were trucked as well as barged, while in the current system fish are barged. Evidence suggests that fish were abnormally stressed during trucking and so the current transport program should be releasing more vital fish below the hydrosystem.

Although these improvement have contributed to increased transport fish survival, in general fish that have passed in-river may, as a group, be stronger because the weaker members were more likely to have been culled from the population than the weaker members of a transport group. In transportation, this culling effect occurs in the estuary, not the hydrosystem, so part of the difference in the survival of transport and non-transport fish in the estuary is a reflection in the differences in culling through the two routes. This initial, or pre-transport, fish vitality distribution is expected to vary from year to year according to environmental conditions prior to and during the early stages of juvenile migration. Under this hypotheses the difference in the vitality of transport and non-transported fish below the hydrosystem will depend on the initial distribution of weak and strong fish at the top of the hydrosystem.

The estimation of D requires information on the in-river survival of non-transported control fish, Vn, an estimate of the barging survival Vt, and the transport to control ratio TCR. In years where a portion of the control fish are transported at a lower dam, D also requires the estimation of the fraction of the control fish that were transported, f.

Applying estimates of these parameters (V_n from CRiSP, V_t of 0.98, and TCRs from Table A.3.1-1) over the years of transport experiments (1968-1995) the D value exhibits an upward trend with high variability prior to 1980 and a more stable distribution after 1980. For retrospective analyses, it was assumed that the D value prior to 1980 was 0.5 and the D value after 1980 was 0.85 (see Table 4.3-2). This indicates that in the early years of the transportation program transport fish experienced more post-Bonneville mortality than in-river passing fish. In the recent period, the differences in survival of the two groups is reduced and is likely to reflect the effect of differences in where weaker fish are culled. The increase in D in more recent years is believed to be related to a reduction in descaling (Figure 4.3.1-2). For prospective analyses, D was selected randomly from the set of post-1980 D values.

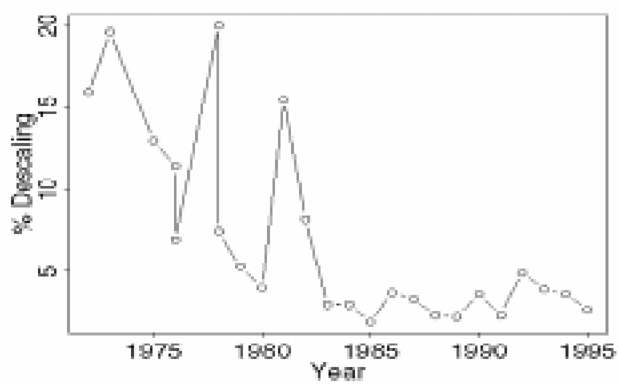


Figure A.3.1-2. Percent descaling at transport dams (LGR or LGS).

Very recent work by James Anderson has developed a new hypothesis in which D is related to descaling estimates, and different hypothesis in which D is related to descaling estimates, and different release times are used to estimate the survival of control fish in T:C studies (i.e., V_n). This results in post-1980 D values of 0.6 to 0.7, rather than around 0.85. However, the results presented in this report used the higher D values that average to 0.85 after 1980.

Comment on transportation assumptions in CRiSP:

> Why were the values used in the retrospective analyses (0.5 before 1980, 0.85 after 1980) different from the mean values for these periods as shown in Table 4.3-2?

End of comment on transportation assumptions in CRiSP.

General comments on Section A.3.1 (Transportation assumptions) by Williams:

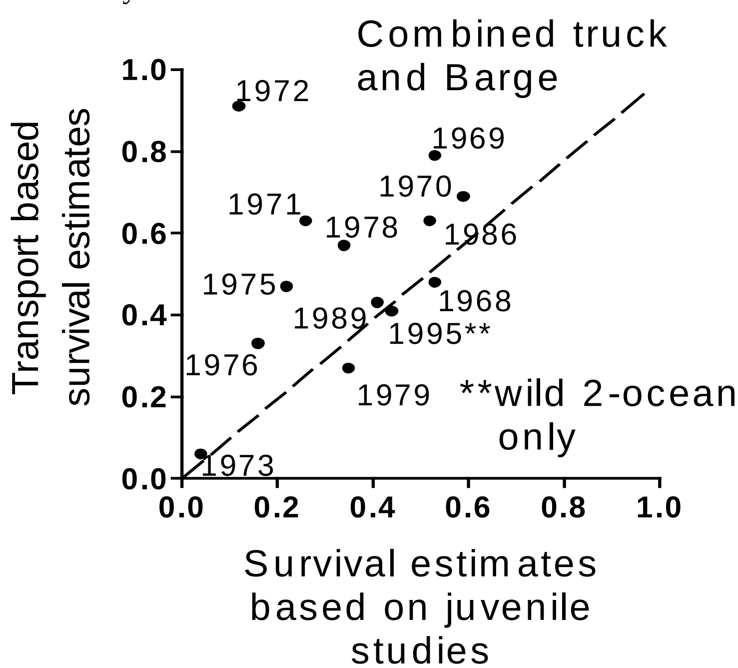
> Transportation studies that used coded-wire tagged (CWT) and branded fish for evaluation were conducted between 1968 and 1989 at Ice Harbor, Little Goose, and Lower Granite Dams. A complete set of the data of estimated in-river survivals through the hydropower system for each year compared to an inverse of the transport to control ratio is presented in the upper figure below. In-river survivals were estimated from past studies conducted during the years of transportation or from more recent studies and adjusted for estimated conditions in the past. Data

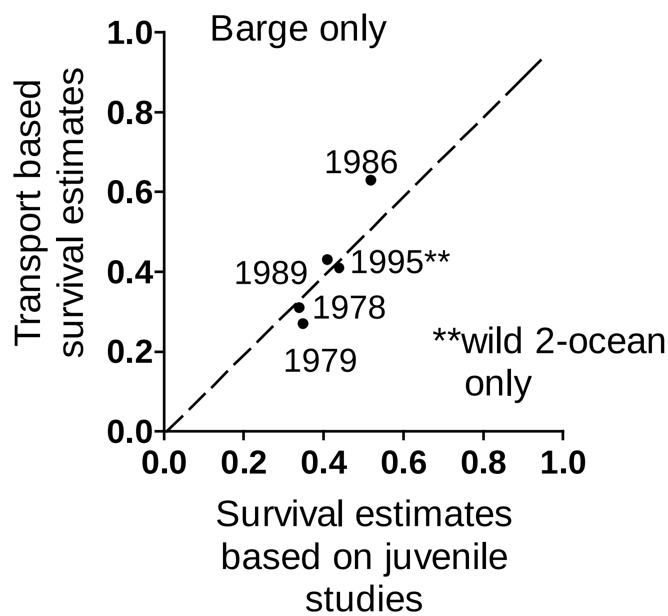
is plotted from the uppermost dam where fish were transported in years when more than one dam were used.

The earliest studies were conducted with fish placed into trucks and released at sites below Bonneville Dam. When the initial research studies indicated that more adult fish returned from juveniles that were transported around the hydropower system than from those that migrated through the hydropower system, a decision was made to maximize the number of fish collected and transported from upper dams on the Snake River. Barges were constructed to haul the fish because the capacity of trucks was too low. Barges are now used to transport nearly all spring migrants; therefore, research studies that were conducted in the past to evaluate transportation that utilized trucks as a means to haul fish to below Bonneville Dam do not represent the present operational mode. The lower graph below presents the data from evaluations where only barged fish were used as a means of transportation.

If concerns exist about the usefulness of data in years when control fish were hauled by truck to below Little Goose Dam from Lower Granite Dam, then only the 1978 and 1979 studies are usable. However, in these 2 years, many of the in-river controls that were released at Lower Granite Dam were collected and transported from Little Goose Dam.

In summary, NMFS does not believe any of the studies where transported fish were trucked to below Bonneville Dam represent conditions that will occur in the future and are therefore usable in prospective modeling. Only studies in 1978, 1979, 1986, and 1989 have complete returns of usable data. Preliminary returns for 1995 studies do not include 3-ocean fish, although likely 80% of the hatchery fish have returned.





End of general comments on Section A.3.1 (Transportation assumptions) by Williams.

A.3.2 Stock Productivity

For prospective modeling, we used two alternative representations of chinook population dynamics.

Delta Model

The Delta model is described in the Aug. 1 memo by Wilson et al. (1997) titled "Draft General Framework for Prospective Modeling, with one proposed hypothesis on delayed mortality". The basic equation is:

$$\ln(R_{t,i}) = (1+p)\ln(S_{t,i}) + a_i - b_i S_{t,i} - M_{t,i} - \Delta m_{t,i} + \delta_t + \varepsilon_{t,i} \qquad \text{[Eq. A.3.2-1]}$$

in which we can write total passage + extra mortality rate *m* as

$$m = M + \Delta m \qquad \text{[Eq. A.3.2-2]}$$

for which,

$R_{t,i}$	=	Columbia River "observed" returns (recruitment) originating from spawning in year t and river sub-basin i
$S_{t,i}$	=	"observed" spawning in year t and river sub-basin i
$a_{,i}$	=	Ricker *a* parameter, which depends on sub-basin i
b_i	=	Ricker b parameter, which depends on sub-basin i
p	=	depensation parameter
$M_{t,i}$	=	direct passage mortality which depends on year and region
$\Delta m_{t,i}$	=	extra mortality rate, which depends on year and region
δ_t	=	year – effect parameter for year t

$\varepsilon_{t,i}$ = normally distributed mixed process error and recruitment measurement, which depends on year t and sub-basin i.
$N_{o,n}$ = number of smolts at the top of first reservoir that are destined not to be transported
V_t - weighted direct passage survival of all transported smolts

A recommended approach for segregating survival in the prospective model uses system survival (ω), which is the output from passage models used in the past for linking passage and life-cycle models. System survival is the number of in-river equivalent smolts below BON divided by the population at the head of the first reservoir. The numbers of transported smolts at each collector project that survive to BON are converted into in-river equivalents by adjusting for differential delayed mortality using the transport/control ratio (Φ) and in-river survival (V_n). System survival is then

$$\omega = P_{0,n}V_n + \Phi V_n(1-P_{0,n}) = V_n\left[P_{0,n} + \Phi\left(1-P_{0,n}\right)\right] \qquad \text{[Eq. A.3.2-3]}$$

Terms and Derivations

Smolts can pass the hydrosystem by one of five routes (subscripts 1,2,3,5, n). The numbers represent dams where collection takes place, in order from the top of the reservoir: 1 = LGR, 2 = LGO, 3 = LMN, 5 = MCN. The subscript 'n' represents smolts which are never transported, i.e., smolts which migrate in-river through the entire hydrosystem.

i = region
j = passage route
t = transported
n = non-transported
b = at Bonneville tailrace

Note: All variables described afterward in this section refer to annual seasonal values: the 'y' subscript is omitted for simplicity.
The following variables are estimated in the passage models:

N_0 = total number of smolts at top of first reservoir in a season
N_j = number of smolts reaching the forebay of dam j
N_b = total number of smolts alive at Bonneville during a season
L_j = cumulative in-river survival from top of first reservoir to dam j
$N_{j,t}$ = number of smolts collected for transportation from dam j
V_n = $N_{b,n}$ / $N_{0,n}$ = direct passage survival of smolts passing in-river
V_j = L_j * bypasssurv * bargesurv = direct passage survival of smolts transported at dam j

$$V_T = \sum_{j=1}^{5} V_j \frac{P_{0,j}}{P_{0,T}} \qquad \text{[Eq. A.3.2-4]}$$

where
$P_{0,j}$ = proportion of smolts at head of first reservoir destined to pass by route j and

$$P_{0,T} = \sum_{j=1}^{5} P_{0,j}$$

and

$P_{0,T}$ = proportion of smolts at head of first reservoir destined to be transported at any project.

Estimates of values for P's, L, and V's come from a passage model.

Derivation of $P_{0,j}$'s:

$$P_{0,j} = P_j \quad \text{if j = 1}$$

$$P_{0,j} = P_j \cdot \prod_{k=1}^{j-1}(1 - P_k) \quad \text{if j > 1, } \neq \text{n}$$

where P_j is the fraction of smolts arriving at dam j that are transported.

If j = n, then $P_{0,j}$ = proportion of smolts destined to pass in-river = $P_{0,n}$
and

$$P_{0,n} = 1 - \sum_{j=1}^{5} P_{0,j} \qquad \text{[Eq. A.3.2-5]}$$

System survival can also be expressed as follows:

$$\omega = \exp[-M][DP + 1 - P] \qquad \text{[Eq. A.3.2-6]}$$

where the P is the fraction of smolts immediately below Bonneville which were transported. The D is the ratio of post-Bonneville survival factors of transported to non-transported smolts (see equation 4.3.1-1). M is direct passage river mortality over a season and is estimated in the passage models as follows:

$$M = -\ln\left(V_n P_{0,n} + \sum_{j=1}^{5} V_j P_{0,j}\right) \qquad \text{[Eq. A.3.2-7]}$$

or can be estimated in the passage models by:

$$M = -\ln(N_b/N_0) \qquad \text{[Eq. A.3.2-8]}$$

The delayed mortality component, given a particular passage model, can be estimated by setting it equal to the MLE estimate of direct and delayed passage mortality (*m* in Chapter 5) minus an estimate of direct passage mortality (*M*, above).

Then,

$$\Delta m = m - M, \text{ with } m = M_d + \mu. \qquad \text{[Eq. A.3.2-9]}$$

where M_d is the direct passage mortality for downriver stocks and μ is net (direct + delayed) instantaneous mortality from the Snake River subbasins to the John Day Dam. Annual estimates of M and M_d would be provided by survival estimates from the passage models without any delayed mortality applied. For downriver stocks, μ by definition is equal to zero; therefore for downriver stocks $m = M$ (= M_d), so Δm = 0.

The delayed mortality factor (Δm) can be expressed as a delayed survival factor as follows:

$$\exp[-\Delta m] = \lambda_n [DP + 1 - P] \qquad \text{[Eq. A.3.2-10]}$$

The system survival (ω) is equal to the ratio of exp(-*m*) to the post-Bonneville delayed survival factor of in-river fish. That is:

$$\omega = \frac{\exp\left(-m\right)}{\lambda_n} \qquad \text{[Eq. A.3.2-11]}$$

The delayed mortality associated with in-river fish, given a particular passage model, can be estimated by setting it equal to the MLE estimate of direct and delayed passage mortality (*m* in Chapter 5) minus an estimate of -ln(ω) (the instantaneous system mortality). This difference is $-\ln(\lambda_n)$, which is the delayed instantaneous mortality of in-river equivalent fish. The post-Bonneville survival factor for non-transported smolts (λ_n as in equation A.3.2-10), is derived by rearranging equation A.3.2-11:

$$\lambda_n = \frac{\exp(-m)}{\omega} \qquad \text{[Eq. A.3.2-12]}$$

The D is the ratio of post-Bonneville survival factors of transported to non-transported smolts and can be expressed as follows:

$$D = \frac{\lambda_T}{\lambda_n} = \Phi\frac{V_n}{V_T} \qquad \text{[Eq. A.3.2-13]}$$

The parameter V_T is direct passage survival of smolts transported at all dams and is estimated through the passage models. The Φ is the quantity referred to as the transport to control ratio (T/C). The T/C ratio is a relative measure of return rates for migrating smolts which have been trucked or barged around the hydrosystem to those smolts which migrated through the hydrosystem. The T/C ratios have been estimated for fish being transported from Snake River dams through mark recapture studies. The T/C values have been estimated in the passage models through a function which relates T/C to in-river survival (V_n). Note that the D values are computed by equation A.3.2-13, and are a consequence of the projected V_n and T/C. The values for V_T and V_n are estimated through the passage models. The T/C values can be estimated by a T/C function as described in Section A.3.1. An alternative prospective method is to plug in T/C values for years data are available into the passage models and then generate D values retrospectively. The resulting retrospective values of D can be randomly sampled to generate T/C values in prospective simulations. This would provide system survivals which can be used in the BSM. Therefore, the important point to note is that D values are specific to each passage model and are not empirical measurements.

Delta Model Sensitivities

The prospective modeling using the delta model relies on the posterior distribution of its parameters, obtained by sampling the likelihood function described in Deriso et al. (1996). The maximum likelihood estimates were tested for sensitivity to the exclusion of various subsets of observations: stocks, brood years, and various other subsets of data, including individual points (Hinrichsen 1998). Differential mortality, dam mortality, and intrinsic productivity estimates were each sensitive to the exclusion of the John Day Middle Fork stock, especially the early part of the record (1959-1973), where three data points (1959, 1964, and 1968) were driving the high influence. These were the three most influential observations in the entire data set. Exclusion of the John Day Middle fork data decreased the differential mortality (μ) and *X*-dam mortality estimates and the intrinsic productivity estimates (Ricker -as) of the Snake River stocks. When the John Day Middle Fork was omitted, (1) the average estimate of mu (1970-

1990) fell from 1.455 to 0.840, (2) the average Snake Ricker-a fell from 3.13 to 2.19 and (3) the X-dam survival rate increased from 0.77 to 0.92 (per dam mortality decreased from 0.26 to 0.08). These represent large shifts in the mean of the parameter distributions. For example, the mean of the dam mortality distribution shifted to 0.08, which was the distribution's 12^{th} percentile when the John Day Middle Fork data were included. Interestingly, the average Snake Ricker-a with the stocks deleted (2.19) was near the average lower river Ricker-a (2.28).

Deletion of data by brood year showed some large changes in the opposite direction. Deletion of brood year 1963, for example, increased mu from 1.455 to 1.675. However, there is no single stock is driving this large increase and, to our knowledge, there is no reason for deleting this brood year based on data quality. It may be possible to estimate a separate X-dam mortality for the year 1963 to reduce its influence, but we have no basis, aside from its high influence, for treating 1963 different than other years.

The early John Day Middle Fork spawner recruit data are of questionable quality and are not representative of the bulk of the lower river spawner-recruit data. Difficulties with using the Middle Fork as a control for the Snake stocks are many. They include: (1) The Middle Fork was treated with rotenone and other chemicals harmful to salmon in 1966, 1973, and 1974. (2) Prior to 1976 a dam, mill and town were located at Bates (river kilometer 107), creating a barrier to salmon migration on the Middle Fork. (3) The index redd counts were not a consistent indicator of spawning in index streams in a 1978-1985 study (Lindsay et al. 1986). One concern we have is that we are not fully aware of the quality of the John Day Middle Fork relative to the other stocks. Another concern is that this stock was scrutinized because of its high influence. Other observations may also have high influence in an opposite direction and similar data quality problems. Another concern we have about conclusions regarding the quality of John Day Middle Fork data is that this particular set of data was examined in more detail only after it was discovered that the MLE estimates were particularly sensitive to three years of that data set.

Further study is needed to determine the extent of the influence of these observations on the prospective results.

References

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Alpha Model

The alpha model is described in Hinrichsen and Anderson, September 4,1997 memo "Passage and prospective model linkage (alpha model)"). The basic equation for the Alpha model is:

$$\ln(R_{t,i}) = (1+p)\ln(S_{t,i}) + a_i - b_i S_{t,i} - M_{t,i} - \alpha_{t,j} + \varepsilon_{t,i} \qquad \text{[Eq. A.3.2-14]}$$

The alpha term can be written as

$$\alpha = \alpha_n - \overline{\alpha}_n - \ln(DP+1-P) + \overline{\ln(DP+1-P)} \qquad \text{[Eq. A.3.2-15]}$$

where

$$\alpha_n = c_1 / F + c_2 E / F + c_3 E^2 / F + STEP \qquad \text{[Eq. A.3.2-16]}$$

as defined in Equation (14) of Anderson and Hinrichsen (1997), except that we've dropped the quadratic term due to some difficulties we've discovered. Note that STEP is a step function which is 0 for brood years 1952-1974.

<<Need to define the period over which the two averages are computed in [Eq. A.3.2-15]>>

Representation of Stock Productivity in the Delta and Alpha models

A number of relationships can be written between parameters of the delta model and alpha model, including

$$\exp[-\Delta m] = \lambda_n [DP + 1 - P] \qquad \text{[Eq. A.3.2-17]}$$

$$\omega = \exp[-M][DP + 1 - P] \qquad \text{[Eq. A.3.2-18]}$$

$$\exp[-m] = \omega \lambda_n \qquad \text{[Eq. A.3.2-19]}$$

in which, ω is system survival and λ_n is post-Bonneville survival factor for non-transported smolts, as defined in Wilson *et al*, and in which *D* is the ratio of post-Bonneville survival factors of transported to non-transported smolts and *P* is the fraction of smolts at Bonneville which were transported, as in Hinrichsen and Anderson's paper.

The last equation given above can be used to write an equation for total passage + extra mortality during any prospective year *y* in terms which involve its coupled retrospective water year *r*:

$$m_y = m_r - \ln[\frac{\omega_y \lambda_{n,y}}{\omega_r \lambda_{n,r}}] \qquad \text{[Eq. A.3.2-20]}$$

The hypotheses described in Section A.3.3.3 allow us to collect terms in the delta model and write directly comparable quantities in the alpha model. The idea is to write the models where components that differ in the prospective analysis can be identified.

With the definitions below, both the delta and alpha model can be written as a Ricker model with 4 specially defined terms,

$$\ln(R) = (1+p)\ln(S) + \textbf{Term1} + \textbf{Term2} + \textbf{Term3} + \textbf{Term4} - bS + \varepsilon \qquad \text{[Eq. A.3.2-21]}$$

The hypotheses in Section A.3.3.3 revolve around the interpretation of **Term3,** that is $\ln(\lambda_n)$. The passage models provide the same standard input **Term2**, that is $\ln(\omega)$, to each of the models. The other terms are in a sense particular ways chosen to estimate intrinsic productivity and common climate variables (common in that they affect equally both transported and non-transported smolts).

Term1: Ricker "*a*" value. Collect all constant terms in each model other than those needed to center climate variables modeled as Markov processes.

In the delta model, this is the term "a"

In the alpha model, this is the term "a - average[ln(DP+1-P)] +average [STEP]

Term2: logarithm of system survival ,ln(ω)

In both models, this is the term "-M +ln(DP +1-P)"

Term3: logarithm of post-Bonneville survival factor of non-transported fish ln(λ_n)

In the delta model, this is the term "-m- ln(ω)"

In the alpha model, this is the term "-STEP"

Term 4: Markov type climate variables, centered so they sum to zero over the brood years 1951-1990

In the delta model, this is the term δ_t

In the alpha model, this is the term, "c_1 (1/F- average(1/F)) + c_2(E/F - average(E/F))"

A comparison of estimates of the Term1 (adjusted Ricker "*a*" values) are shown in Figures A.3.2-1 to A.3.2-3 (Figs. 17-19 from R. Deriso's October paper; not available for this draft). The adjustments on Figure A.3.2-2 and A.3.2-3 for "extra mortality" of non-transported smolts provide a meaningful way to compare productivity measures for the alpha and delta models.

Comment on Section A.3.2 (Stock Productivity) by Williams and response by Schaller et al.:

Snake River and Columbia River stock groupings represent fish from two distinct species as defined under the Endangered Species Act and come from two different Evolutionarily Significant Units (ESUs). Therefore, fish from these two ESUs may have systematic differences in survivals that change independent of each other under varying environmental (both physical and biological) conditions. Though some of these differences are accounted for by different Ricker a and b parameters, such stock differences may confound the interpretation of *m* in the Delta model, which is partly derived from upstream-downstream differences in (R/S)). Does it represent hydrosystem effects, or inherent stock differences in the response to changing ocean conditions? The Alpha model, on the other hand, assumes that all extra mortality for the two stock groupings is independent (i.e., no common year effects). Altogether, the combination of the Alpha/Delta models and the alternative hypotheses for extra mortality (i.e., hydrosystem dependent, regime shift, or BKD / mortality here to stay) account for a wide range of possible interpretations of historical stock-recruitment patterns.

Intrinsic stock productivities may change over time. A recent report by Kiefer, et al. (1997) to the Bonneville Power Administration provided information that egg to smolt survivals from the upper Salmon River from 1988 through 1994 averaged only 2.2%. If levels were this low in the 1960s, it probably would have led toward a quick crash of the stocks. Thus, it seems likely that it is a recent phenomenon and may relate to small spawner sizes in successive years.

Response by Schaller et al.: JGW implies that intrinsic stock productivity may change over time and that it may relate to small spawner sizes in successive years, but does not provide evidence that this has occurred. First, there is no temporal baseline for comparison for the stock that he uses in his example (upper Salmon River); i.e, egg-smolt survival for this stock was not estimated during the 1960s. Also, the statement ignores the evidence from Chapter 9, that changes in Snake River aggregate smolt/spawner productivity and survival rate and between pre-1975 and post 1975 (if any) were small relative to overall declines in adult-to-adult productivity and survival rate.

JGW cites Kiefer et al. (1997) estimates of egg-to-smolt survival rate from the upper Salmon River from 1988-1994 which averaged 2.2%, and states that if levels were this low in the 1960s, the populations would have crashed. However, survival rate estimates reported in the literature are quite sensitive to the life stages indexed, and the methods and assumptions used; extrapolations should be made with caution.

At a typical fecundity (for this stock) of 5,000 eggs per female, 2.2% egg-to-smolt survival rate delivers 110 smolts to the upper end of the hydrosystem, and 2% SAR to the spawning grounds delivers about 2 adults (1 female, i.e., replacement). In their 1994 annual report, Kiefer and Lockhart (1997; p. 38) indicate that average egg-to-smolt survival rate for the headwaters upper Salmon River for brood years 1987 to 1992 was 4.1% or 3.6%, depending on which method was used. At these rates, 180 to 205 smolts would be delivered to the upper end of the hydrosystem, and about 1% SAR to the spawning grounds would result in replacement. SARs of 1%-2% from the upper end of the hydrosystem to the spawning grounds are conceivable for the 1960s based on Raymond (1988) SAR estimates of 3%-6%, which were indexed lower in the system (and accounted for river harvest). Without a careful accounting of method and life stage differences, the recent upper Salmon River egg-to-smolt survival rate estimates seemingly do not provide evidence that the rates have decreased for this stock, nor do they indicate depensation has not occurred.

Source:
Kiefer, R.B. and J.N. Lockhart. 1994. Intensive evaluation and monitoring of chinook salmon and steelhead trout production, Crooked River and upper Salmon River sites. Annual Report 1994. BPA Project 91-073.

End of comment on Section A.3.2 (Stock Productivity) by Williams and response by Schaller et al.

A.3.3. Hypotheses for Delayed Mortality

Extra mortality is any mortality occurring outside of the juvenile migration corridor that is not accounted for by either: 1) spawner-recruit relationships; 2) estimates of direct mortality within the migration corridor; or 3) for the Delta model only, common year effects affecting both Snake River and Lower Columbia River stocks.

A.3.3.1 Extra Mortality is Hydro-Related

Hypothesis:

The completion of the Federal Columbia River Power System in the late 1960s through the mid-1970s and subsequent operation, has increased the direct and delayed mortality of juvenile migrants, which resulted in considerably sharper declines in survival rates of Snake River spring and summer chinook stocks (over the same time period), than of similar stocks which migrate past fewer dams and are not transported.

Rationale:

Several possible mechanisms were identified from the literature in Weber et al. (1997) that may explain delayed mortality of smolts that are transported and those that migrate in-river through the hydropower system. These include: altered saltwater entry timing which is poorly synchronized with the physiological state of the smolts (CBFWA 1991; Fagurlund et al. 1995); stress from crowding and injury (including descaling--Basham and Garrett 1996; Williams and Matthews 1995) during bypass, collection, holding and transport (Mundy et al. 1994); increased vulnerability to disease outbreak (e.g., BKD and fungal infection) due to stress and injury (Mundy et al. 1994; Raymond 1988; Williams 1989); and increased vulnerability to other stressors in the environment or to predation, particularly by northern squawfish (Mundy et al. 1994).

Evidence for delayed mortality due to hydrosystem passage comes in part from the PATH 1996 conclusions on the retrospective analysis (Marmorek and Peters 1996), stock-recruitment comparisons (Schaller et al. 1996) and the MLE retrospective model (Deriso et al. 1996). MLE estimates of Φ, which include direct and delayed passage mortality components, were correlated with water travel times experienced during the smolt outmigration; total mortality of Snake River spring/summer chinook tended to be highest in low flow, low spill years which had higher proportions of smolts transported. Passage models which assumed low delayed mortality of transported smolts (CRiSP T2) had the poorest fit in the MLE (e.g., Fig. 5-5 of Deriso et al. 1996).

The transportation of juvenile salmon through the mainstem Columbia and Snake River hydropower system began as an experimental program in the late 1960s and has been the principal method for improving mainstem survival since the early 1980s (Mundy et al. 1994). Its justification rests on experimental results indicating transported fish return as adults in greater proportions than their non-transported counterparts. Although there are problems with transportation research (Mundy et al. 1994), it appears that transported Snake River spring/summer chinook survive at higher rates than their in-river counterparts in most years studied.

Transportation has been in operation ever since Lower Granite Dam was completed in 1975 and has been fully operational since 1977, but Snake River spring/summer chinook have continued to decline. These declines have shifted focus from comparisons between the relative survival of transported and in-river fish to questions about the absolute survival of transported fish. Recently, two documents (Mundy et al. and Toole et al. 1996) have stressed the importance of evaluating the absolute survival of transported juvenile chinook salmon. Furthermore, the PATH hydro group recommended an interim goal of 2% to 6% smolt to adult return rates (SAR) (Toole et al. 1996).

The estimated SAR rates of transported Snake River wild smolts have been considerably less than the SARs prior to FCRPS completion, and less than the recent SARs of a similar downriver stock, Warm Springs River (Raymond 1988; Weber 1996; Weber et al. 1997). The SARs of transported Snake River wild spring/summer chinook estimated from coded wire tags (1975-1990) and PIT tags (1989-1995) indicate that transported fish rarely, if ever, meet the goal of 2% to 6% SAR (Figure A.3.3.1-1).

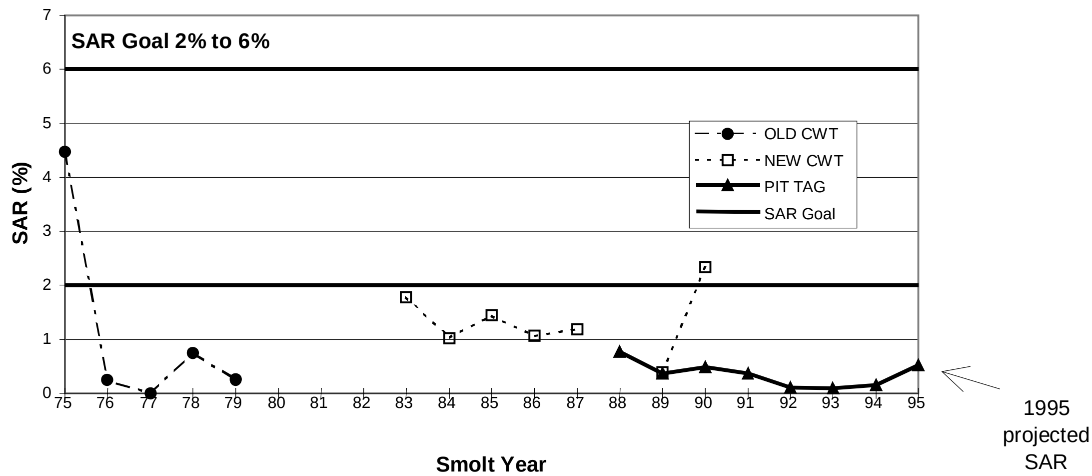

Figure A.3.3.1-1: SAR of Transported Wild Spring/Summer Chinook

Evidence for delayed mortality of transported smolts relative to those that pass in-river through the hydrosystem also comes from a comparison of in-river survival estimates and the inverse of the T/C ratio. If post-hydrosystem mortality were roughly equal between the transported and in-river groups as hypothesized by Williams et al. (1997), the points should scatter around the 1:1 line. However, the scatter of points from the 1968-1979 transportation and in-river survival studies tended to fall to the right and below the 1:1 line (Figure A.3.3.1-2), which supports the hypothesis that delayed mortality was greater for transported fish than for the controls (i.e., that D<1).

Preliminary analysis of return rates of PIT tagged wild smolts from 1994 to 1995 further suggests that delayed mortality of in-river migrants may be related to route of passage through the hydropower system (Weber et al. 1997). Smolts that were detected (i.e., were bypassed) two or more times returned at lower rates than those detected one or zero times (Figure A.3.3.1-3). These wild smolts represented fish that were released above Lower Granite Dam and were not collected for tagging in the fish bypass system at Lower Granite Dam. Based on PATH estimates of direct mortality through bypass, spill and turbine routes, these results suggest that delayed mortality increases as a function of the number of times a fish is bypassed. In addition, the T/Cs estimated using these in-river fish was 1.80 in 1994 and 1.04 in 1995 (partial returns).

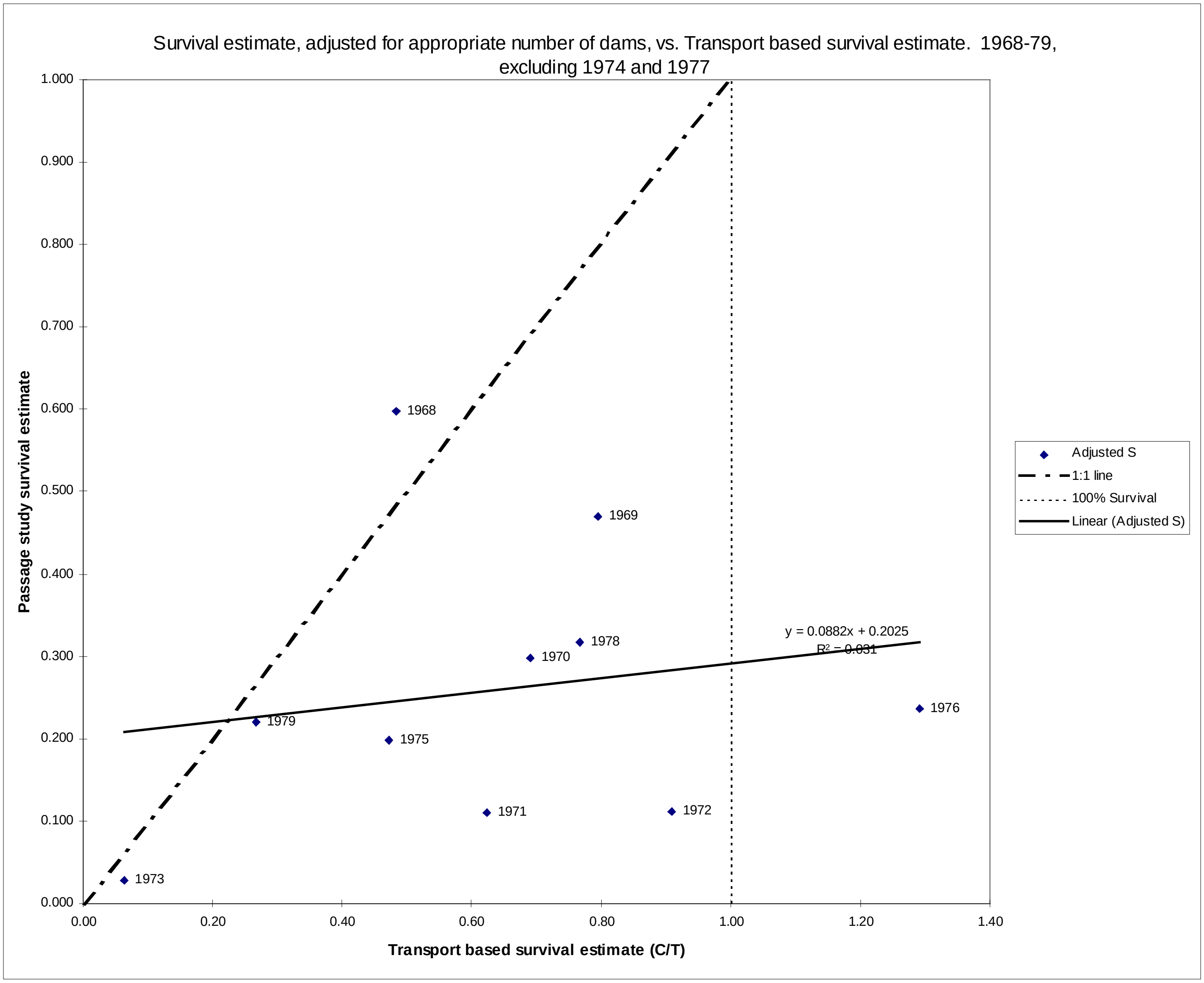

Figure A.3.3.1-2: Survival estimate, adjusted for appropriate number of dams, vs. Transport based survival estimate (1968-1979, excluding 1974 and 1977)

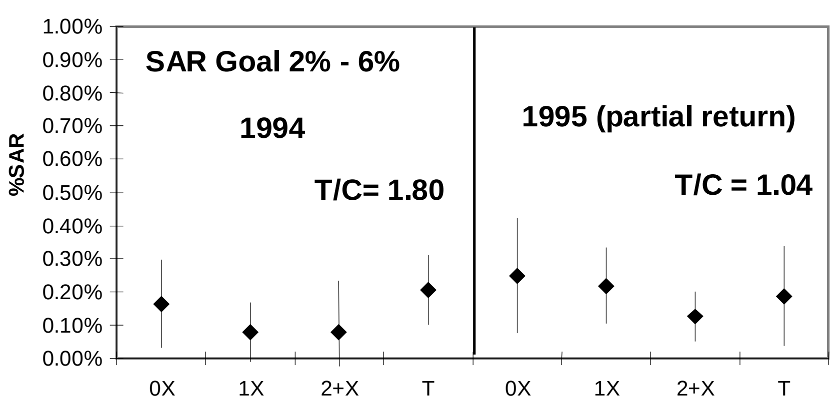

Figure A.3.3.1-3: SAR by detection history, wild yearling chinook.

The use of a common year effect parameter in the MLE analysis (Deriso et al 1996) reflects evidence that the estuary and early ocean conditions do not have a systematically different effect on survival for stream-type chinook stocks across regions of the interior Columbia River Basin. This is reasonable in view of similarity of these stocks, the overlap in time and space of these stocks during their early ocean residence (and beyond), and the broad-scale nature of climatic influences described in the literature.

There are several lines of evidence suggesting that the interior Columbia Basin stocks are exposed to similar estuary and ocean conditions, particularly during the critical first year. Beamish and Bouillon (1993) and others provided evidence that indices of climate over the north Pacific Ocean may play an important role in production of different species of salmon originating over a wide geographic range. In a review paper, Anderson (1996) concluded that a warm/dry regime favors stronger year class strengths of many Alaska fish stocks while cool/wet regime favors stocks on the West Coast of the lower United States. Deriso et al. (1996) found evidence of a common year effect for all index stocks of stream-type chinook from the Snake River and lower Columbia River regions. Of the lower Columbia River stocks in this analysis, at least the John Day River and Warm Springs River spring chinook smolt timing appears very similar to that of Snake River spring and summer chinook. Smolts of these lower Columbia River, Snake River and upper Columbia River stocks migrate through the mainstem to the estuary primarily in late April and May (Lindsay et al. 1986, 1989; Raymond 1979; Hymer et al. 1992; Mains and Smith 1964). Current hypotheses regarding ocean survival of Pacific salmon generally focus on the juveniles' critical first months at sea (Pearcy 1988, 1992; Lichatowich 1993), where juveniles of these index stocks are most likely to overlap in time and space. Year class strength for these spring and summer chinook is apparently established, for the most part, within the first year in the ocean, as evidenced by the ability of fishery managers to predict subsequent adult escapements from jack counts (e.g., Fryer and Schwartzberg 1993).

Although ocean recoveries of coded wire tagged spring/summer chinook are infrequent (Berkson 1991), the few recaptures (62 recoveries from 8 release years) from both Snake River (21 recoveries) and lower Columbia River (41 recoveries) hatchery stocks were widely scattered from California to Alaska ocean fisheries (PSMFC unpublished data). The average annual proportion of CWT recoveries from ocean fisheries north and south of the Columbia River mouth appears to be similar between the Snake and lower Columbia hatcheries (Figure A.3.3.1-4). Since it appears that Columbia Basin stream-type chinook share a common estuary and nearshore ocean environment and a more common ocean distribution than stocks evaluated by Beamish and Bouillon (1993), it seems very unlikely that differential estuary and ocean conditions themselves (i.e., apart from differences in delayed effects due to juvenile migration) would have had a systematically different effect on survival.

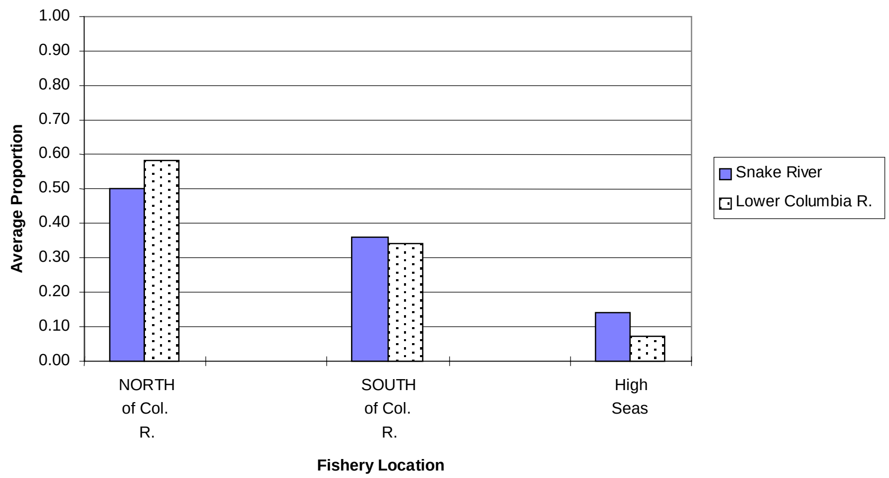

Figure A.3.3.1-4: Observed CWT Ocean Recoveries of Snake River and Lower Columbia River Hatchery Spring Chinook. Source: Weber et al. (1997

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Representation in the Delta Model

Under the hypothesis that λ_n, post-Bonneville survival factor of non-transported smolts, is related to V_n, in-river survival of non-transported smolts, the linkage of passage model output to the BSM is done as follows.

The passage + extra mortality rate m can be converted into a survival fraction, which can be represented as the product of system survival (ω) and delayed survival factor of non-transported fish (λ_n) (as in Equation a5):

$$\exp(-m) = \omega\lambda_n \qquad \text{[Eq. A.3.3.1-1]}$$

With an estimate of m and an estimate of system survival from a passage model, λ_n can be estimated by rearranging the above equation:

$$\lambda_n = \exp\left[-m - \ln(\omega)\right] \qquad \text{[Eq. A.3.3.1-2]}$$

The value of λ_n for a particular retrospective year ($\lambda_{n,r}$) can be estimated from the retrospective BSM model m for that year (m_r) and the passage model system survival estimate for that retrospective year (ω_r). The retrospective annual delayed mortality factor of non-transported fish (M_r^*) is defined as 1 - $\lambda_{n,r}$. To represent this hypothesis about delayed mortality in the BSM, we assume the delayed mortality factor [1-λ_n] for non-transported smolts is proportional to in-river mortality [1- V_n] of non-transported smolts. The prospective annual delayed mortality factor of non-transported fish (M_y^*) is calculated according to

$$M_y^* = M_r^* \cdot \frac{1 - V_{n,y}}{1 - V_{n,r}}$$ [Eq. A.3.3.1-3]

where $V_{n,y}$ and $V_{n,r}$ are prospective and retrospective direct in-river survival, respectively, from a passage model. The estimated prospective delayed survival factor of non-transported fish ($\lambda_{n,y}$) is then

$$\lambda_{n,y} = 1 - M_y^*$$ [Eq. A.3.3.1-4]

The retrospective and prospective λ_n's, along with the retrospective and prospective system survivals are then used in Equation A.3.2-10 above to derive the prospective passage plus extra mortality rate (m_y) to use in prospective BSM runs.

The delta model in prospective mode can be written:

$$\ln(R_y) = (1+p)\ln(S_y) + a - bS_y - m_r + \ln(\omega_y / \omega_r) + \ln(\lambda_{n,y} / \lambda_{n,r}) + \delta_y + \varepsilon_y$$ [Eq. A.3.3.1-5]

The retrospective water year coupled to each prospective year is chosen from brood years 1975-1990. The prospective system survival term ω_y is specified in the input file.

Comment on Representation of Delayed Mortality in the Delta Model by Anderson and response by Wilson et al:

In words the Delta model in-river fish delayed mortality hypothesis is:

"the post-Bonneville survival factor of non-transported smolts is related to the in-river survival of non-transported smolts"

and the mathematical translation of the hypothesis is given as

[20]

The designation *y* and *r* are for prospective and retrospective years, presumably with similar flow regimes. Although the relationship between the FLUSH verbal and mathematical hypotheses may be intuitive, there are uncertainties with the approach. First, there is no clear identification of how retrospective and prospective in-river and estuary/ocean survivals are actually linked in terms of mechanisms. Second, the mathematical form, Eq [20], is not unique since the verbal hypothesis could equally be expressed in terms of survivals leading to the alternative expression

[21]

A problem emerges because Eq [20] and Eq [21], two equally viable models, have different characteristics. While in both cases the sensitivity of extra mortality to in-river survival depends on the retrospective conditions, it does so in exactly opposite ways in the two formulations. For the mortality-based hypothesis mortality, Eq [20], the incremental change in post-Bonneville survival is

[22]

so for a unit increase of prospective in-river survival, $V_{n,r}$, smaller values of the retrospective post-Bonneville survival, $\lambda_{n,r}$, equate to a smaller increases for prospective post-Bonneville survival $\lambda_{n,y}$,.

<u>Response by Wilson et al.</u> *In the description under equation (22), $V_{n,r}$ is misidentified as prospective in-river survival; it is actually retrospective in-river survival. The main point is that $V_{n,r}$ does not increase or decrease with management actions; it is an estimate of past survival for a specific year, and depends only on assumptions about past passage conditions (e.g. TURB1, TURB4, or TURB5).*

Comment by Anderson continues:

For the survival-based hypothesis, expressed by Eq [21], the incremental change in post-Bonneville survival is expressed by the equation

[23]

and this has exactly the opposite behavior of Eq [22]. That is, smaller values of the retrospective post-Bonneville survival, equate to larger prospective survival increases.

The salient point here is that the FLUSH hydro related mortality hypothesis expressed by Eq [20] is a non-unique case with unique consequences as to how information from past conditions alters future predictions. Other equally plausible and simpler expressions of the hypothesis give opposite responses. Thus, the hypothesis as it stands is incomplete in that it is not based on first principles and its consequences depend on the specific but non-mechanistic functional form. If stress were the factor relating the two survivals as expressed by Eq [9] then the relationship based on survivals through Eq [21] would be the more mechanistically correct.

Since the hydro-related mortality factor includes ω which depends on D, and D is problematic because it forces a condition on the transport survival λ_T that is apparently unrealistic, the hypothesis needs to be reconsidered.

End of comment on Representation of Delayed Mortality in the Delta Model by Anderson and response by Wilson et al.

Representation in the Alpha Model

One feature of the alpha model that is different from the delta model is that Hinrichsen and Anderson include in their definition of "extra" mortality (α) climatic variables (E,F) not related to hydro, whereas in the delta model, common year effects are not included in their definition of post-Bonneville survival factors.

Representation of the hydro-related extra mortality hypothesis in the Alpha model is analogous to the approach used in the Delta model. In the Delta model it is assumed that retrospectively 1- λ_n , post-Bonneville mortality factor of non-transported smolts, is proportional to 1- V*n*, in-river mortality of non-transported smolts. In the Alpha model, the *STEP* term is assumed to be proportional to 1-V*n:*

$$1 - \exp[-STEP_r] \propto 1 - \overline{Vn}_r \qquad \text{[Eq. A.3.3.1-6]}$$

where the average is taken over brood years 1975-1990 and during any prospective year *y* the same proportionality coefficient applies to

$$1 - \exp[-STEP_y] \propto 1 - Vn_y \qquad \text{[Eq. A.3.3.1-7]}$$

which we implied that

$$STEP_y = -\ln[1 - (1 - \exp(-STEP_r))(1 - Vn_y) / (1 - \overline{Vn}_r)] \qquad \text{[Eq. A.3.3.1-8]}$$

The alpha model in prospective mode can be written as

[Eq. A.3.3.1-9]

where the α_y is given by Equation A.3.2-15 , except we use Equation A.3.3.1-8 to calculate a prospective value for *STEP***.** The prospective V*n, D,* and *P* are specified in the input file. In prospective years, there are new *F,E* variables according to the particular climate hypothesis.

Comment on Representation of Delayed Mortality in the Alpha Model by Anderson:

> This hypothesis, like the Delta equivalent, has no biological mechanism relating in-river and post-Bonneville survivals.

End of comment on Representation of Delayed Mortality in the Alpha Model by Anderson.

A.3.3.2 "BKD" Hypothesis for Extra Mortality

Hypothesis:

Extra mortality is not related to the hydropower system or climate conditions and is here to stay.

Rationale:

Although the hydropower system causes some direct mortalities that can account for some of the decreased returns since its construction, the direct mortality that results from the hydropower system under the present configuration with bypass systems at most dams is far from sufficient to account for the present low adult return rates. Alternative hypotheses to explain the continued low returns have generally fallen into two categories: 1) the hydropower system is responsible for the low returns because fish, if not dying directly due to the hydropower system, die indirectly after having migrated through or been transported around the system; and 2) some cyclical change in climate that occurred about 1976/77 has created unfavorable rearing and/or ocean conditions which has led to a much lower adult return rate than in the 1950s and 1960s. Both of these general categories of hypotheses presume that the adult returns will increase with changes in the underlying factors that control the stocks. In the case of hypotheses related to hydropower, removal of dams will cause a substantial increase in adult returns. In the case of cyclical climate hypotheses, once the climate changes back to a favorable conditions, adult returns will substantially improve.

The change in stock viability hypothesis (there are likely a number of scenarios by which this could occur) presumes that, at least for the next several decades, stock returns will not substantially increase. Stock viability likely changed as a direct or indirect result of construction of the hydropower system in the 1970s. However, once the viability decreased, no or only slight increases in viability would occur with a favorable change in climate or the hydropower system. This hypotheses provides a pessimistic outlook for the stocks. The following are a couple of scenarios under this hypothesis that suggest how this might occur:

1) Stock sizes have gone so low in some streams that they are not likely to rebound. This could result from a low number of juveniles produced in any one year, but with predator numbers remaining equal to past levels, thus, the predation rate on juveniles is higher. Further, it is possible that adult returns from any one run year are not high enough to return needed nutrients and fertilizer to support the parr in the streams from the previous return years. In a recent report to BPA, Kiefer and Lockhart (1997) estimated the egg to smolt survival of juveniles from the upper Salmon River to the head of Lower Granite Reservoir ranged from 0.5 to 5% from 1988 through 1994 and averaged 2.6%. Based on levels of adult catch and estimated in-river survivals through the hydropower system during the 1960s, egg to smolt survivals would have been much higher than these historically or the stocks would have crashed long ago. This suggests the viability of eggs to smolts is much lower than historically.

2) Disease (most notably bacterial kidney disease [BKD]) was transmitted to wild stocks with the implementation of hatchery programs that began in the Snake River Basin after the initial construction of the Snake River dams. The disease was either new to the stocks or a different and more virulent form that resulted from hatchery practices and it was transmitted to the wild fish from the hatchery fish. Wild fish in the basin now have the new form that is readily transmitted and likely results in a high death rate for fish that have it. The disease affects all fish from the basin and doesn't act on the individuals until they reach the estuary or early ocean. USGS researchers have found BKD at some level in all Snake River basin wild spring/summer chinook salmon stocks. Additionally. USGS researchers conducted studies in 1997 (unpublished) to determine mortalities in groups of fish subjected to either no stresses (control), 1,

2, 4, or 8 cumulative stress incidences that would represent the cumulative passage through 1, 2, 4, or 8 dams. All fish had relatively low survivals. Control fish survival was approximately 50%; whereas the survival of all treatment groups was 10% or less. From these initial studies, they concluded that the type of river conditions that Snake River spring/summer chinook salmon encounter is not likely to affect survivals substantially. If BKD is the causative agent of death, then the fish will likely die with or without the majority of hydropower dams in place.

Comment on Section A.3.3.2 (BKD hypothesis) by Schaller et al.:

> The BKD hypothesis and rationale are incomplete to the extent that they cannot be evaluated or assigned any weight of evidence. There is no literature cited. Proposers of this hypothesis still need to: 1) identify those possible mechanisms that would make Snake River spring/summer chinook stocks *more susceptible* to BKD than lower river stocks, *unrelated to* the stress and crowding which occurs during collection, holding, bypass and transportation; 2) provide a biological rationale for those mechanisms, including literature citations; and 3) provide evidence in support of that rationale.
>
> Until this is made more specific, it seems counterproductive for PATH to continue evaluating this hypothesis.

End of comment on Section A.3.3.2 (BKD hypothesis) by Schaller et al.

Representation in the Delta Model

For the situation where extra mortality is here to stay, $\lambda_{n,y}/\lambda_{n,r} = 1$. The delta model in prospective mode can be written as

$$\ln(R_y) = (1+p)\ln(S_y) + a - bS_y - m_r + \ln[\omega_y / \omega_r] + \delta_y + \varepsilon_y \qquad \text{[Eq. A.3.3.2-1]}$$

The system survival in prospective years, ω_y , will be calculated based on input *M* and *P* values, but the *D* values in the prospective years will be chosen randomly from the 1980 to present water year estimates, which are thought to be representative of current *D* conditions. The retrospective water year coupled to each prospective year is chosen from brood years 1975-1990.

Comment on Representation of BKD hypothesis in Delta Model by J. Anderson:

> The hypothesis that the recent increase in extra mortality is related to the evolution of disease in the Snake River stocks and is here to stay is expressed by the condition $\lambda_{n,y}/\lambda_{n,r} = 1$. Time precludes a complete analysis of this hypothesis but since it contains ω which depends on D and D is problematic because it forces a condition on the transport survival λ_T, the hypothesis is unpredictable and it is possible that it may generate unrealistic results because of its structure.

End of comment on Representation of BKD hypothesis in Delta Model by J. Anderson.

Representation in the Alpha Model

The hypothesis that extra mortality is here to stay is represented in the Alpha model by assuming that the *STEP* component of "extra" mortality is here to stay. In this case the alpha model in prospective mode can be written as in eq. A.3.3.1-9, except that the α_y is calculated with assumption that $STEP_y=STEP_r$. In prospective years, there are new *F,E* variables according to the particular climate hypothesis. The alpha value in prospective years, α_y , will be calculated based on input *M* and *P* values, but the *D* values in the prospective years will be chosen randomly from the 1980 to present water year estimates, which are thought to be representative of current *D* conditions.

Comment on Representation of BKD hypothesis in Alpha Model by Anderson:

> The hypothesis gives a first order relationship of disease on extra mortality. It could be improved by a more detailed formulation of disease dynamics and the vectors of transmission of disease between hatchery and wild fish and between transported and non-transported fish.

End of comment on Representation of BKD hypothesis in Alpha Model by Anderson.

A.3.3.3 Regime Shift Hypothesis for Extra Mortality

Hypothesis:

Extra mortality is not related to hydro, but due instead to an interaction with a cyclical climate regime shift with a period of 60 years, crossing 0 in brood year 1975.

Rationale:
Widespread ecological changes related to interdecadal climate variations in the Pacific have been observed in this century. Dramatic shifts in many marine and terrestrial ecological variables in western North America coincided with changes in the physical environment in the late 1970s (about 1977). The 1977 regime shift is not unique in the climate record nor in the record of North Pacific salmon production. Signatures of an interdecadal climate variability are detectable in many Pacific basin ecological systems (Mantua et al. 1997). Among the salmon species shown to have interdecadal variability were Alaskan sockeye, Alaskan pink salmon, Columbia River spring chinook, and Washington-Oregon-California (WOC) coho. While the climate regime from 1977-present has favored Alaskan sockeye and pink salmon production, it has been associated with decreased production of Columbia River spring chinook and WOC coho (Hare et al 1997).

Comment by Schaller et al.:

> The "regime shift" hypothesis is really a *selective* regime shift hypothesis. As framed, it does not address why the climate regime shift might be *systematically* reducing survival rates more for Snake River stocks than for lower river stocks, *unrelated* to hydrosystem passage.
>
> The point being made here is that climate may affect productivity of salmon stocks *in general*. What would be needed as evidence to support the selective regime shift hypothesis would be evidence that Snake River spring/summer chinook stocks are particularly vulnerable to climate variability, unrelated to their unique hydrosystem experience.

Note also that the stock performance measures being referenced in this rationale are of abundance (catch or run-size). Productivity and survival rate data would provide a stronger basis in support of a hypothesis, since abundance is a function of escapement and exploitation policies, as well as productivity and survival. There is recent evidence that changes in abundance indicators for several Alaskan salmon stocks may be explained by changes in escapement and harvest management policies (Farley and Murphy 1997), rather than climate patterns.

End of comment by Schaller et al.

The PDO is an index of climate based on the North Pacific sea surface temperature pattern since 1900. It is an index that other physical and biological tend to follow on the interdecadal time scale. The signature of PDO appears in the Gulf of Alaska Air Temperature, British Columbia coastal sea surface temperature, Scripps Pier sea surface temperature, Gulf of Alaska stream flow, and British Columbia/Washington stream flow. It also appears in other measures of climate such as the North Pacific sea level pressure.

A regime shift occurs every 25-30 years, whenever the Pacific Decadal oscillation (PDO) makes a polarity switch (positive to negative or visa versa). We simulate the PDO using a square wave with a period of 60 years. In this century, the polarity switches occurred in 1925 (to warm/dry climate), 1947 (to cold/wet climate), and 1977 (to warm/dry climate). These years correspond to Minobe's (1997) analysis of reconstructed continental surface temperatures, which showed interdecadal oscillations (of period 50-70 years) over the last 3 centuries. The next polarity switch is modeled to occur in 2007 (spring chinook brood year 2005).

The biological effects of these regime shifts have been dramatic, changes in catch of Alaskan pink and sockeye salmon decreased by about 57% in 1947, and increased by about 230% during the 1977 climate regime shift (See Table below). Generally speaking Alaskan salmon and Pacific Northwest salmon have run sizes that fluctuate in reverse to one another (Hare et al. 1997). The 1947 shift, which was bad for Alaskan stocks, was good for Columbia River upriver spring chinook, which showed a 49.5% increase.
The 1977 regime shift, though good for Alaskan stocks, was bad for Columbia River upriver spring chinook, which showed a 53.1% decrease (See Figure below).

Table A.3.3.3-1. Percent change in mean catches of four Alaskan stocks and run sizes of Columbia River upriver spring chinook following major PDO polarity changes in 1947,1977. Mean catch levels (run sizes for Columbia stock) were estimated from intervention models fitted to the data, using a 1-year lag for both pink salmon stocks, and 2-year lag for western sockeye, and a 3-year lag for central sockeye, and a 2-year lag for spring chinook. (Alaskan stocks from Mantua et al. [1997]).

Salmon stock	**1947 step**	**1977 step**
Western Alaskan sockeye	-32.2%	+242.2%
Central Alaskan sockeye	-33.3%	+220.4%
Central Alaskan pink	-38.3%	+251.9%
Southeast Alaskan pink	-64.4%	+208.7%
C.R. upriver spring chinook (run size)	+49.1%	-55.5%

Comment by Schaller et al.:

This summary table appears to be evidence that *different species* (sockeye and pink salmon) from *broad geographic areas* (most of Alaska) responded similarly (using mean catch statistics) to climate regime shifts (major PDO polarity). It could be used to support the common year affect of the delta model. Possible mechanisms, rationale and evidence are needed to explain why, under this hypothesis, that very similar, adjacent stock groupings of stream-type chinook would have vastly different responses to climate regimes, unrelated to their unique hydrosystem experience.

End of comment by Schaller et al.:

Figure A.3.3.3-1: Regime shifts are apparent in the run size data of upriver spring chinook of the Columbia Basin (data from ODFW and WDFW 1995). The average levels (dashed lines) were calculated for the various climatic regimes (pre-1947, 1947-1977, and post-1977). Based on the fitted intervention model, the 1947 regime shift resulted in a run that increased by 49.1%, and the 1977 regime shift resulted in a run that decreased by 55.5%. To approximate outmigration year, the year of adult return was lagged by two.

Comment by Schaller et al.:

> This is a very simplistic example. The data used in Figure A.3.3.3-1 are for the aggregate wild and hatchery runsize of upriver spring chinook. No attempt was made here to sort out effects of changing mainstem harvest rates (range 0.03 to 0.86), or of proportion wild fish in the run. If this figure is evidence of the influence of a regime shift on upriver aggregate spring chinook (including both Snake River and lower river index stocks), it does not provide evidence in support of a selective regime shift hypothesis.

End of comment by Schaller et al.:

We used a simple intervention analysis of the upriver spring chinook runsize (1936-1992 outmigration years) to determine whether the regime shifts and anthropogenic effects were important. There are two warm/dry periods with this record: 1936-1947 (REGIME 1) and 1977-1992 (REGIME3), and one cold/wet period 1948-1976 (REGIME2). To estimate the post-1947 anthropogenic effect on 1977-present run size, we compared the average run size during REGIME1 and REGIME3, assuming that the run sizes would be similar during these time periods in the absence of anthropogenic influence. We bounded the effect of the climate regime shift on the run size by estimating a model under two different assumptions: (1) there was no anthropogenic effect during REGIME 2, and (2) the anthropogenic effect during REGIME 2 equaled their effect during REGIME 3. Under (1), we obtain a lower bound for the effect of the climate regime shift, *LCLIM,* the difference in average run sizes during REGIME 2 and REGIME 1. Under (2) we obtain an upper bound on the effect of the regime shift, *UCLIM*, the difference in mean run sizes between REGIME 2 and REGIME 3.

The model used to compute the anthropogenic and climate effects was:

$$RUN_t = REGIME_t + N_t$$
$$N_{t+1} = \phi N_t + \varepsilon_t$$

Where

RUN_t = The Run size corresponding to outmigration year t

$REGIME_t$ = Factor variable containing three runsize levels (*REG1, REG2, REG3*) during the three climate regimes

N_t = Noise following AR(1) process

ϕ = AR(1) coefficient for noise process

ε_t = $N(0,\sigma^2)$ gaussian noise process

The post-1947 anthropogenic effect on the 1977-present run size, *ANTHRO*, was computed as *REG1-REG3*, the lower bound for the climate effect, *LCLIM*, is calculated as *REG2-REG1*, and the upper bound, *UCLIM*, was computed as *REG2-REG3*.

Table. A.3.3.3-2. Parameter estimates of intervention model. The ratio of the climate to hydrosystem effects on spring chinook runsize ranges from 3/2 to 5/2. Number of observations = 56, number of parameters = 4, d.f. = 52.

parameter	**Value**	**Std. Error**	**t-value**	**p-value**
LCLIM, lower bound on effect of climate shift.	57.85	23.43	2.47	0.0168

UCLIM, upper bound on effect of climate shift.	97.47	19.47	5.01	6.71e-6
ANTHRO, post-1947 anthropogenic effect on 1977-present run size.	39.63	26.19	1.51	0.1363
ϕ, AR(1) parameter.	0.388	0.13	2.96	0.0046

The climate effect, estimated between 57.85 and 97.47, was significant at the 0.05-level and it outweighed the anthropogenic effect (39.63) by a ratio of 3/2 to 5/2. The anthropogenic effect, giving a t-value of 1.51, was not significant at the 0.05-level (p-value = 0.1363).

Hare et al. (1997) performed a principle components analysis (PCA) on salmon stocks from Alaska to California which demonstrated the inverse relationship between the catch of Gulf of Alaska stocks and that of West Coast (south of Vancouver Island) stocks. The first principal component explained 69 percent of the variance in the salmon catch. The inverse principle component loadings (expressed as correlations between the salmon catch and principle component score) together with the principal component scores showed interdecadal salmon production regimes within the Pacific basin with an inverse relationship between Northern (Alaska) and southern (West Coast) stocks.

Pearcy (1992) found a relationship between the 1977 regime shift and production of adult hatchery coho. Prior to 1976, there was a positive relationship between smolts released and adult production. From 1976-1986, the relationship became negative. This loss of coho production was accompanied by increased sea level and sea temperatures in the California Current System and reduced southward transport and weak upwelling along the coast (Pearcy 1992).

The marine ecological response to PDO may start with the plankton at the base of the food chain and work its way up to top-level predators such as salmon (Francis et al. 1997). After the 1977 regime shift, there was a zooplankton biomass increase and a re-distribution around the Subarctic gyre, creating favorable feeding conditions for migrant salmon smolts (Brodeur and Ware 1992, Sugimotoa and Tadokoro 1997). Conversely, off the West Coast , there was a dramatic decrease in zooplankton production due to stratification of the California Current waters and loss of advective products from the westwind drift (Roemmich and McGowan, 1995). This relatively barren ocean environment was unfavorable for West Coast smolts (Hare et al. 1997).

Some phytoplankton and zooplankton population models are sensitive to upper-ocean and mixed-layer depths and temperatures, and these are linked with the fluctuations in PDO. One such model has successfully simulated the increase in Gulf of Alaska productivity as a response to 20-30% shoaling and 0.5-1 degrees Celsius warming of the mixed layer (Polovina et al. 1995). Increased PDO generally brings enhanced stream flows and nearshore ocean mixed layer conditions that a favorable to productivity, but generally decreased productivity for Pacific Northwest salmon (Mantua et al. 1997).

There is no agreement on the origin of the decadal climate variability (Ware 1995). Schlesinger and Ramankutty (1994) speculated that the signal might be caused by a low frequency oscillation in the North Atlantic thermohaline circulation. Friis-Christensen and Lassen (1991) suggest that long term global temperatures by respond to lower frequency variation in the solar cycle length, which may indicate change in total solar energy output.

Connection to higher frequency climate fluctuations

The PAPA drift (explained below in A.3.4) appears to make a polarity switch whenever there is a polarity switch in the PDO.

PAPA Drift Crossings (Actual Years)

TO WARM/DRY	1918*	1937	1958	1974*
TO COLD/WET	1929		1946*	1963

The *s correspond to regimes shifts which were estimated by others to occur at 1925, 1947, and 1977 (actual years) (Mantua et al. 1997). The cycle length of the PAPA drift is approximately 18 years – about 1/3 the period of the PDO.

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Comment on Section A.3.3.3 (Regime Shift hypothesis) by L. Botsford:

It is clear that there was a change in the atmosphere in the north east Pacific in the mid 1970s, and considerable evidence that that change had a positive influence on biological productivity in the Gulf of Alaska. There are also mechanistic reasons to believe that there is an inverse relationship between the flows in the Alaska Coastal Current and the California Current. However, whether the change in the 1970s had an effect on salmon stocks in Washington, Oregon and California is just a hypothesis, and evidence regarding that hypothesis must come the area where these fish are found. The Columbia River stocks spend their early life in the California Current, south of the bifurcation of the West Wind Drift, so if that period is important we need evidence from salmon stocks in the California Current.

One piece of evidence an effect of the regime shift on CCS stocks is the decline of Oregon hatchery survivals in the 1970s described Pearcy (1997). However, that same decline is not seen in the Coronado-Hernandez thesis (Fig. 11). Another piece of evidence (though marginal because of the location) is the decline in survivals of chinook stocks in the Georgia Strait (Beamish 199x). There is also a marginal correlation between zooplankton densities off southern California (where there are no salmon), and zooplankton densities in the Gulf of Alaska (Brodeur, et al. 1996?).

Some of the relationships presented in the current document are not rigorous evidence. In the paper by Mantua, et al. (1997) no "correlations" are computed between the PDO and Columbia River stocks nor Washington-Oregon-California coho. There are also no correlations between a variable representing the regime shift and California Current salmon in Francis and Sibley (1991) nor in the Anderson (1996) review. As far as I know, none of the referenced papers contain statistical evidence relating California Current salmon stocks to the regime shift in the Gulf of Alaska.

End of comment on Section A.3.3.3 (Regime Shift hypothesis) by L. Botsford.

Representation in the Delta Model

For this hypothesis, the delta model is written exactly as in eq. A.3.3.2-1, except that the retrospective water year chosen for a given prospective year is one which occurred during the same phase of the cycle. For example, until brood year 2005 the coupled retrospective years are chosen from brood years 1975-1990, then from brood year 2006 for the next 30 years the coupled retrospective years are those chosen from brood years 1952-1974 (1952 is first year of S/R data). The system survival in prospective years, ω_y, will be calculated based on input *M* and *P* values, but the *D* values in the prospective years will be chosen randomly from the 1980 to present water year estimates, which are thought to be representative of current *D* conditions.

Comment on representation of Regime Shift hypothesis in Delta Model by J. Anderson:

The hypothesis has the same problems as other FLUSH/Delta hypotheses in that delayed transport mortality is driven by in-river survival according to Eq [19]. Consequently it is a variant of the hydro related extra mortality model and the hypothesis needs to be further considered.

End of comment on representation of Regime Shift hypothesis in Delta Model by J. Anderson.

Representation in the Alpha Model

The alpha model is written exactly as in eq. A.3.3.1-9, except that the *STEP* value chosen for a given prospective year is one which occurred during the same phase of the cycle. For example, until brood year 2005 the *STEP* is the one applicable to brood years 1975-1990, then from 2006 for the next 30 years the *STEP=0*, which is the one applicable to brood years 1952-1974 (1952 is first year of S/R data). In prospective years, there are new *F,E* variables according to the particular climate hypothesis. The alpha value in prospective years, α_y , will be calculated based on input *M* and *P* values, but the *D* values in the prospective years will be chosen randomly from the 1980 to present water year estimates, which are thought to be representative of current *D* conditions.

General comments on Section A.3.3. (Extra Mortality Hypotheses) by Anderson and Response by Wilson et al:

The model systems are both complex and although they are generally based on the same data sets they are derived from different philosophies. The FLUSH/Delta model system was derived without explicit hypothesis. The CRiSP/Alpha model was derived from a mechanistic basis. A basic assumption of the FLUSH transport mortality hypothesis is that conditions experienced by the in-river fish affect survival of both transport and non-transport groups. In contrast, in CRiSP the survival of each group depends on the passage conditions experienced by each group respectively. Through a test of several hypotheses it was concluded that transport fish delayed mortality depends on stress, which can be indexed by descaling measured at transport collection. Evidence is presented to show that the ratio of delayed mortality of transport to non-transport fish is independent of the number of dams non-transported fish pass and the time of arrival of the groups into the estuary. The hypotheses test also indicates that non-transported fish delayed mortality is manifested while fish are in the hydrosystem passage and is adequately described by the TURB4 dam passage hypothesis which accounts for descaling in dam passage.

It was shown that in the FLUSH delayed mortality hypothesis the delayed survival of transported fish changes in proportion to the non-transported in-river survival. This response is problematic in that changes in the river system below the transport collection site will change the post-hydrosystem survival of the transported fish.

In terms of transport-to-control (*T/C*) ratios the two models are also different. The FLUSH model assumes *T/C* ratios can be expressed by one variable, survival of non-transported fish. In CRiSP the *T/C* ratios are expressed by two variables; the survival of non-transported fish and the descaling experienced by the transported fish. It is interesting to note that by fixing the average descaling at 5% the two models give virtually identical *T/C* vs. survival curves.

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Wilson, P., C. Petrosky, H. Shaller, and E. Weber. 1998. Response to J.J. Anderson "Critiques on transport and extra mortality hypotheses", dated December 22 1997. January 8 1998.

Response by Wilson et al *The claim that CRiSP hypothesis is derived from first principles, while the FLUSH hypothesis is a consequence of a non-mechanistic functional form appears inconsistent. One point is that the "first order mechanisms" describing the CRiSP hypotheses are constantly changing (as outlined above). Principles that would seem central to the CRiSP world view at one stage, such as differential hydrosystem mortality of in-river and transported fish having consequences to later life stage survival rate, are ignored in later incarnations.*

Because of the complexity of the salmon life-cycle and ecosystem, the STFA hypothesis does not single out one particular mechanism or index among many to predict direct and delayed mortality of transported fish. Weber et al. (1997) also noted there are many plausible mechanisms likely influencing survival of transported fish, and cited literature in support:

For example, smolts are subjected to the stress of crowding and injury during bypass, collection, holding, and transport. High levels of descaling have been reported (Williams and Matthews 1995; Basham and Garrett 1996). Stress and injury may trigger disease outbreak (e.g., BKD, fungal infection) and delayed mortality. In-river juvenile migrants from the Snake River now enter the estuary later than they did before completion of the hydropower system, whereas transported juveniles may experience a combination of delayed and accelerated migration. Physiological state and time of saltwater entry may be poorly synchronized on both sides of the "biological window" for transported groups. For example, Fagurlund et al. (1995) cite studies of effects of premature saltwater entry (incomplete smoltification) with coho salmon, resulting in high mortality, and, in many of the survivors, a reduction in or cessation of growth. Several of the above potential mechanisms have been identified in the literature (Mundy et al. 1994, Raymond 1988, Williams 1989).

See Weber et al. (1997) for more discussion of mechanisms causing and support for delayed mortality of transported fish.

Survival of transported fish in FLUSH is expressed relative to that of in-river fish, and has been in regional passage models for many years, because until very recently there were no reliable measures of the isolated smolt-to-adult survival of the transported or in-river component of yearly juvenile migrations. The best way to use the limited historical data available (T/Cs) to estimate absolute values of such things as post-Bonneville survival is by using other data (e.g. stock-recruit data) in combination with the T/C data to try to estimate the parameter of interest for one of the components of the migration (e.g. non-transported fish) and so be able to estimate the other with the relative survival data represented by the T/C studies. One advantage of the STFA T/C model is that it is fit to actual data, on T/Cs and in-river survival, rather than model projections of these quantities (except for 1986 and 1989 T/C studies, where no survival estimates are available).

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Williams, J.G. and G.M. Matthews. 1995. *A review of flow and survival relationships for spring and summer chinook salmon, Oncorhynchus tshawytscha, from the Snake River Basin. Fish. Bull. 93: 732-740.*

End of general comments on Section A.3.3. (Extra Mortality Hypotheses) by Anderson and Response by Wilson et al.

A.3.4 Future Climate

In addition to model components described in Section A.3.2 above, climate variables δ_t (year-effect) in the delta model and the PAPA index in the alpha model are modeled prospectively to reflect different assumptions about future climate. These climate variables are modeled either as a type of autoregressive process or as a combined 18.5 year cyclical/ autoregressive process.

Estimated flow at Astoria was used to investigate post-Bonneville effects of flow. The Astoria flow index in the alpha model showed no significant auto-correlation properties in the exploratory analysis by Hinrichsen and Anderson. A small but significant correlation exists between 1/(Astoria flow) and unregulated water transit time (R=.29). This low level correlation was modeled in BSM by choosing flows from years with above median WTT whenever an above median WTT water year was chosen for a simulated year (and vice versa).

"Markov" (autoregressive) Climate

Rationale:

There is a significant first-order auto-correlation present in the MLE estimates of δ (the year-effects parameters), as seen below in Figure A.3.4-1. Regression of $\delta(t+1)$ versus $\delta(t)$ has an R-square = 0.271 (significant at p=.0008).

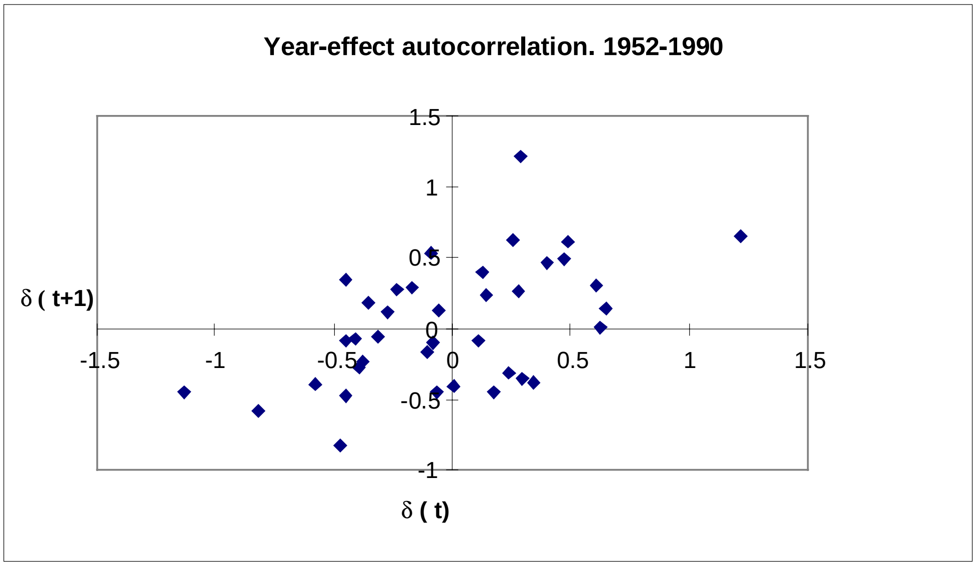


Figure A.3.4-1: MLE estimates of year-effect parameters, the δ's, for 1952 - 1990.

Implementation in Prospective Models:

The autocorrelation apparent in the year-effects parameters was captured in the population projections by a type of Markov process with empirical probability densities. The method implemented in the model consists of the following steps: for each 100 year population projection, a sample of the posterior density of the climate-related variable is made (year-effect parameters for the Delta model, and the PAPA index parameter for the Alpha model). From that sample, MLE estimates are calculated for the multinomial probabilities that characterize the sign of the climate-related variable, such as, $P(x(t)>0 \mid x(t-1)>0)$. Given such a P vector then each simulated climate-related variable follows the multinomial(P) for selection of positive and negative values. The actual positive (negative) values selected are chosen at random from the positive (negative) posterior sample of the variables selected for that particular 100 year projection.

Cyclical Climate

Rationale:

Widespread ecological changes related to interdecadal climate variations in the Pacific have been observed in this century. Dramatic shifts in many marine and terrestrial ecological variables in western North America coincided with changes in the physical environment in the late 1970s (about 1977). The 1977 regime shift is not unique in the climate record nor in the record of North Pacific salmon production. Signatures of an interdecadal climate variability are detectable in many Pacific basin ecological systems (Mantua et al. 1997). Among the salmon species shown to have interdecadal variability were Alaskan sockeye, Alaskan pink salmon, Columbia River spring chinook, and Washington-Oregon-California (WOC) coho. While the climate regime from 1977-present has favored Alaskan sockeye and pink salmon production, it has been associated with decreased production of Columbia River spring chinook and WOC coho.

A regime shift occurs every 25-30 years, whenever the Pacific Decadal oscillation (PDO) makes a polarity switch (positive to negative or visa versa). We simulate the PDO using a square wave with a period of 60 years. In this century, the polarity switches occurred in 1925 (to warm/dry climate), 1947 (to cold/wet climate), and 1977 (to warm/dry climate). These years correspond to Minobe's (1997) analysis of reconstructed continental surface temperatures, which showed interdecadal oscillations (of period 50-70 years) over the last 3 centuries. The next polarity switch is modeled to occur in 2007 (spring chinook brood year 2005).

Implementation in Prospective Models

The approach taken to model the 18.5 year cyclical/autoregressive process is in essence a Markovian process similar to the process described in Section A.3.4.1, except that the underlying Markov probabilities vary dependent on cycle phase. A sine wave crossing 0 in brood year 1975 for the alpha model (and 1980, best fit, for the delta model) with 18.5 year period was used to identify 2 different phases (when the sine>0 and when sine<0). Simulated values of the PAPA index (or year-effect parameter) were selected in each simulation year according to a Markov process estimated from historical data with the same cycle phase as the simulated year. For example if sin[(Y-1980)*2pi/18.5] is positive for simulation year Y then a simulated year-effect in the delta model would be selected from historical estimates which occurred during historical years where the sine is positive. Further, the selection process during this positive sine wave would occur based on Markov probabilities estimated from that sub-set of historical data.

Comment on Section A.3.4 (Cyclical climate hypothesis):

> This description of the Cyclical climate hypothesis is confusing – the rationale is for asquare wave with 60-year period whereas the description of the implementation describes an 18.5 year cycle.

End of comment on Section A.3.4 (Cyclical climate hypothesis).

A.3.5 Habitat

Rationale

Habitat conditions and natural disturbances or management actions which affect habitat have been widely observed to affect salmonid survival during freshwater rearing (Jones et al. 1997 *Retrospective Report revised chapter 10 PATH FY96 Conclusions Document*). In addition, egg-to-smolt mortality rates which typically exceed 90%, suggest that a significant scope exists for habitat-related changes in freshwater rearing survival. However, relationships between population productivity and habitat condition or actions which affect habitat condition are difficult to quantify. Comparisons of stock-recruitment data for spring chinook salmon generally failed to identify significant correlations between landscape or land use variables and index stock productivity (Paulsen 1997 *Retrospective analyses*). Confounding problems included a lack of adequate measures of habitat quality, incomplete datasets on land use, difficulties in defining appropriate spatial scales, and uncertainties in defining lag times for effects. Thus, while few would disagree that habitat can be a critical limiting factor in freshwater rearing or that changes in land use can affect habitat quality and survival, the effects of any given set of habitat improvement activities on stock productivity cannot be predicted. Prospective analyses of the potential effects of habitat changes on future salmon stock performance were therefore based on plausible changes in stock productivity described by the observed range of variability in stock-recruitment parameters among index populations from habitats of varying condition.

We have focused our attention on the Ricker *a* parameter. Our rationale for this choice is that the stocks of interest are generally accepted to be at levels far below their carrying capacities, based on historical estimates of abundance. This implies that habitat changes, while they may in fact affect both *a* and *b* values, are far more likely to affect the probability of stock survival or recovery through their influence on *a*, which directly affects productivity at low stock sizes. The *a* parameter can be thought of as reflecting the quality of the habitat in the area utilized by the stock for spawning and pre-smolt juvenile rearing. The challenge is thus to judge how changes to habitat might affect average egg-to-smolt survival for the stock, translated into a change in the Ricker *a* parameter.

How much of a change in the Ricker *a* parameter is appropriate to consider for each stock? There is no “right” answer to this question. We have chosen to define a range of no greater than a unit increase or decrease in the *a* value from its current value, whatever that might be. A unit change in *a* is equivalent to an approximately three-fold change in stock productivity (or, in other words, in egg-smolt survival), since productivity (R/S) is approximately equal to e^a at low stock sizes, and $e^1 = 2.7$. We offer three reasons for considering this range in *a* to be plausible:

1. For the Snake River basin stocks, the range in current estimated Ricker *a* values is approximately one;
2. A preliminary analysis of PIT tag recoveries showed an approximately three-fold variation in average recovery rates between releases in wilderness areas and releases in managed areas;
3. Smolt production models developed during the sub-basin planning exercise assumed a three-fold range of smolt density capacities between sites classified as having “fair” habitat, and those having “excellent” habitat.

We are not arguing that this evidence provides a convincing reason for believing that habitat management actions will necessarily increase (or mis-management will decrease) productivity three-fold from the current stock condition. We suspect that making such an absolute judgement would be exceedingly difficult, if not impossible. Instead we use this evidence to suggest that three-fold changes in productivity are plausible. Just *how plausible* such a change is for a given stock and management option will affect the assignment of a probability to such a change, as described below.

Before discussing the assignment of probabilities, however, there is a further caveat. We suspect there is an upper limit on the Ricker *a* value for the stocks from a particular region, defined by the intrinsic productivity of the area as determined by physiography and climate. We have thus constrained the plausible increases in the Ricker *a* value to not exceed the maximum *a* value observed for the up-river stocks. In contrast, we do not believe there is a similar constraint on the down-side; stock productivity can reasonably decline by a factor of three, even if it is relatively low to begin with, provided habitat conditions worsen considerably.

We originally chose to examine two contrasting options for future habitat management. The first option (A) can be described as continued management according to existing habitat management plans in the regions of interest (status quo option). In the ICBEMP reports (Quigley et al. (eds) 1996) three options are presented – our Option A is equivalent to Option 1 in their report. The second option (B) is more akin to Option 2 in the ICBEMP report (active restoration of ecological integrity), although we stress the aquatic habitat component more heavily. We characterize this option by the words, “Make every practical effort to restore and protect anadromous fish habitat”. These two options provide contrast in the degree to which habitat protection and restoration will be emphasized – we do not believe an option that reduces emphasis on habitat relative to the status quo is likely for the endangered stocks. The prospective model results in presented in Section 5 consider only Option B, and examine the contrast between this option and the scenario in which no future changes in Ricker *a* values are expected to occur due to habitat management options.

Implementation of Habitat Uncertainty in the Prospective Model

For each stock and habitat option, we judged the probability that the Ricker *a* value would either (1) increase by up to one unit, but to a value no higher than the observed maximum; (2) remain the same; or (3) decrease by one unit, over the next 48 years (the proposed NMFS recovery standard time frame). We also needed to judge how rapidly the changes would occur, should a change occur at all. This is summarized by specifying the probability that *a* will have changed to a new value by year 12 (or 24), **given** that it is expected to change by year 48. If the change is likely to occur very slowly (i.e., a gradual reduction of fines in stream substrates following sediment control, or a slow phase-in of a riparian management option) then the probability of the change occurring in twelve years, even if it does occur after 48 years, is very low. On the other hand, if the change is likely to result from a sudden event (more likely for a negative change due to a catastrophic event) that is equally likely to occur anytime during the next 48 years, the probability of a change by year 12 is 0.25 and by year 24 is 0.5, again **given** that the event has occurred by year 48. These values (.25, .5) could also reflect a gradual but steady progression over time towards the 48-year value.

Our judgments of each of these probabilities for each of the index stocks included in the prospective modeling are summarized in Table A3.5.1. In the prospective model, these probabilities were used to determine whether, for a given run, the Ricker *a* value for the stock being simulated should be modified to reflect a habitat management effect in Year 12, 24 or 48 of the simulation. Again, only Option B was included in the results presented in Section 5.

Using Bear Valley Creek as an example, under Option A we assumed equal probabilities (0.15) of an increase or a decrease in Ricker “a” at 48 years, with the highest probability (0.7) being for “no change” (Table A.3.5-1). If a change was to occur, we judged that an increase in Ricker “a” may occur more slowly than a decrease (see 12 and 24 year probabilities). Under Option B, we judged that an improvement was more likely (0.6) than no change (0.4) or a decrease (0.0).

Table A.3.5.1: Probabilities of future Ricker *a* values for seven Snake River spring/summer chinook stocks given two alternative scenarios of future habitat management. Prob(no change) means the probability that the *a* value does not change from its current (prospective simulation year 1) state by year 48 of the simulation. Prob(increase) and Prob(decrease) are interpreted similarly. The “increase” and “decrease” columns list the percent change in *a* value in the specified direction. Prob(12|increase) is the conditional probability that an increase occurs by simulation year 12, given that it occurs by year 48. The other conditional probabilities – Prob(*year*|*direction*) - have similar interpretations.

Stock	Prob (no change)	Relative Increase in a	Prob (increase)	Prob (12|increase)	Prob (24|increase)	Relative Decrease in a	Prob (decrease)	Prob (12|decrease)	Prob (24|decrease)
Option B									
Imnaha	0.85	12%	0.1	0.1	0.5	29%	0.05	0.25	0.5
Minam	0.85	11%	0.1	0.1	0.5	28%	0.05	0.25	0.5
Bear Valley	0.4	9%	0.6	0.2	0.8	28%	0	0	0
Marsh	0.85	11%	0.15	0.5	0.8	28%	0	0	0
Sulphur	1	6%	0	0	0	27%	0	0	0
Poverty Flats	0.6	14%	0.05	0	0.3	29%	0.35	0.35	0.5
Johnson	0.85	10%	0.07	0.4	0.8	28%	0.08	0.25	0.5
Option A									

Imnaha	0.9	12%	0.05	0.05	0.25	29%	0.05	0.25	0.5
Minam	0.9	11%	0.05	0.05	0.25	28%	0.05	0.25	0.5
Bear Valley	0.7	9%	0.15	0.1	0.4	28%	0.15	0.25	0.5
Marsh	0.85	11%	0.15	0.5	0.8	28%	0	0	0
Sulphur	1	6%	0	0	0	27%	0	0	0
Poverty Flats	0.5	14%	0.05	0	0.3	29%	0.45	0.35	0.5
Johnson	0.85	10%	0	0	0	28%	0.15	0.25	0.5

A.3.6 Hatcheries

Aggregate releases of hatchery fish have increased significantly over the last two decades (Table A.3.6-1). Preliminary work suggests that negative, statistically significant correlations do exist between aggregate hatchery releases and Snake index stock survival. Relationships between subbasin releases and survival of individual index stocks is more ambiguous: sometimes the releases appear to have been beneficial, while for others they were problematic.

However, we did not include these relationships in the preliminary prospective analysis. Although there is an association between increased hatchery releases and decreased survival (see Figure A.3.6-1), interpretation of this association is complex. The releases were intended to mitigate for the effects of the Snake River dams. As such, one would expect that as more Snake River dams came on-line during the late 60's and early 70's (reducing smolt to adult survival of wild fish), this would have been accompanied by more hatchery releases to enhance the fish population. This is indeed what occurred. To the extent that the passage models account for the decrease in smolt-to-adult survival (*m* in equations found in Section 4.3.3) this should be accounted for in the model which includes aggregate releases. It is obvious, however, that PATH is testing many hypotheses which posit that the direct effects of passage (*m*, above) do not completely account for the effects of the hydrosystem. While the increase in hatchery releases may well have played a role, we have not devoted enough time to the hatchery analysis to disentangle the effects of hatcheries from other factors, such as climate and delayed transportation effects. This work will be completed prior to the final spring/summer chinook decision analysis.

Figure A.3.6-1: Snake Yearling Chinook Releases vs. Snake "Alpha", Brood Years 1970 – Present (Preliminary)

Table A.3.6-1: Aggregate Releases of yearling Chinook, Snake and Bonneville-McNary,

Brood Years 1970-Present, Millions (Preliminary). This table does not consider steelhead and subyearling chinook releases.

Brood Year	Snake Releases	Bonneville-McNary Releases
70	3.46	3.86
71	4.9	1.01
72	3.16	2.52
73	6.34	1.45
74	7.27	3.30
75	5.01	2.91
76	7.75	2.15

77	6.37	6.34
78	7.35	5.06
79	4.04	5.80
80	3.82	4.86
81	6.71	4.89
82	8.75	3.87
83	10.9	6.65
84	7.22	4.54
85	11.6	5.73
86	10.9	7.14
87	12	7.14
88	13.5	12.41
89	9.71	11.16
90	11.7	5.69

Appendix B. Detailed Results

This Appendix presents additional results to those presented in Section 5 of the report. Results are presented in graphical form only - we have not provided a narrative describing or explaining these results because of time constraints.

Results in this Appendix are organized into the following sections:

Section B.1. Sensitivity of Outcomes and Decisions to Effects of Uncertainties.
These results are similar to Section 5.4 of the report, but looks at the effects of uncertainties on individual jeopardy standards rather than a combination of all three.

Section B.2. Sensitivity of Outcomes and Decisions to Weightings on Alternative Hypotheses.
These results are similar to those in Section 5.6 of the report, but looks at the effects of different weightings on passage models, transportation assumptions, and extra mortality hypotheses on individual jeopardy standards rather than on a combination of all three.

Section B.3. Projected spawning abundance for Imnaha and Marsh Creek stocks.
This sections shows "Box and whisker" diagrams (see Section 5.7 for an explanation of these diagrams) of simulated distributions of spawning abundance over the 100-year simulation period. Results are presented for an "optimistic" and a "pessimistic" aggregate hypothesis, as defined in Section 5.7 of the report.

Section B.4. Projected mainstem and tributary harvest rates for Imnaha and Marsh Creek stocks.
Supplements results in section 5.7 of the report, showing tributary and mainstem harvest rates for these two stocks for "optimistic" and "pessimistic" aggregate hypotheses.

Section B.5. Further analyses of Smolt-to-Adult Return rates.
Shows average SARs (see section 5.7 for an explanation of how SARs are generated by the life-cycle model) as a function of the probability of the number of spawners for the sixth best stock exceeding the survival escapement level over 100 years. The results are broken out by passage model/transportation assumptions (CRiSP-T3 and FLUSH-T1/T2), prospective model (Alpha or Delta), and action (A1, A2, A3).

Again, we caution that these results are only preliminary. Consideration of additional actions and hypotheses, and assignment of weightings to alternative hypotheses, may cause these results to change in the final report. Therefore, the results we present in this section should not be interpreted as implying that one action is better than another. Instead, they should be seen only as an illustration of how these kind of results might be displayed.

B.1. Sensitivity of Outcomes and Decisions to Effects of Uncertainties.

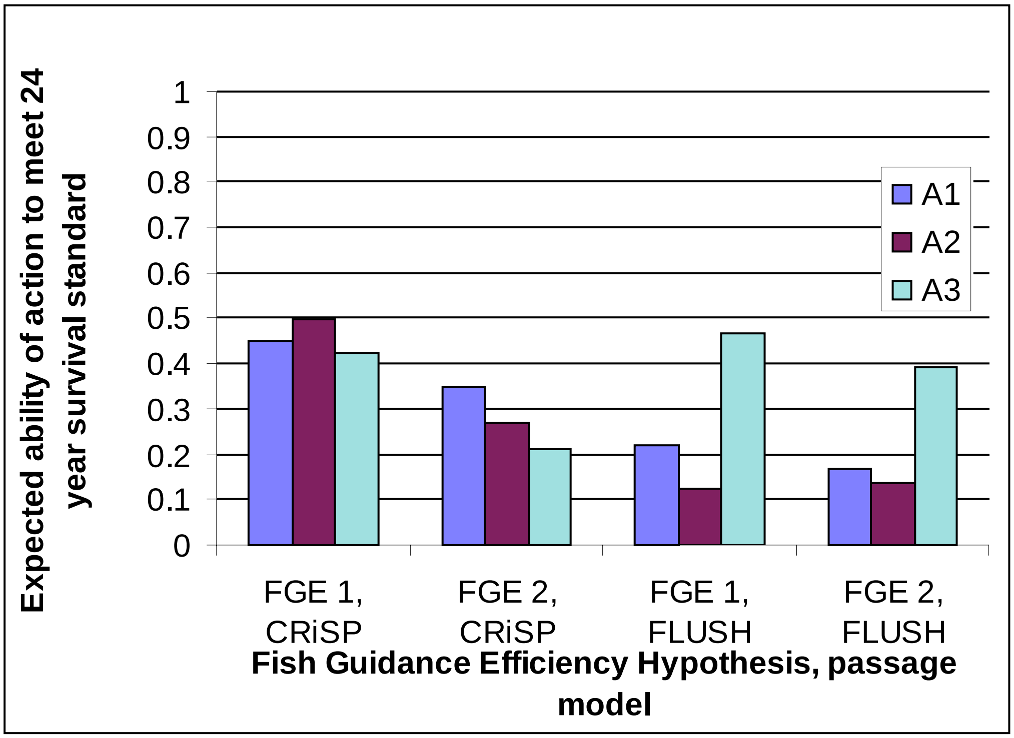


Figure B.1-1. Expected ability to meet 24-year survival standard under different FGE hypotheses. CRiSP passage model outputs are coupled with the TRANS3 assumption about transportation survival; the FLUSH passage model is associated with TRANS1 and TRANS2 transportation assumptions.

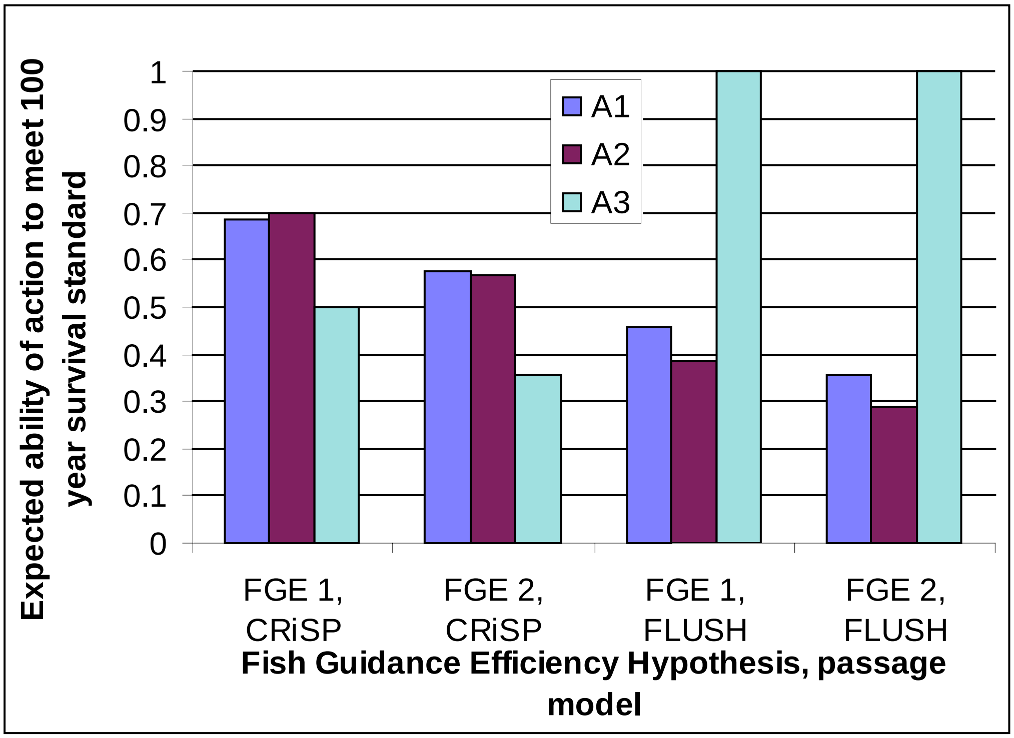


Figure B.1-2. Expected ability to meet 100-year survival standard under different FGE hypotheses.

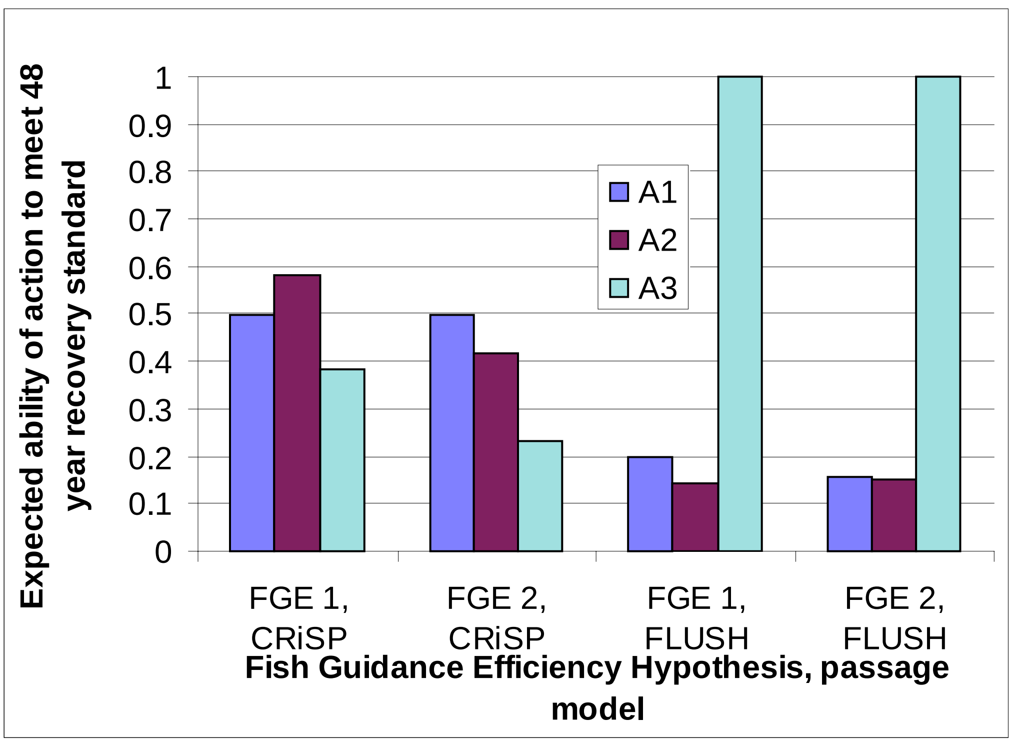


Figure B.1-3. Expected ability to meet 48-year recovery standard under different FGE hypotheses.

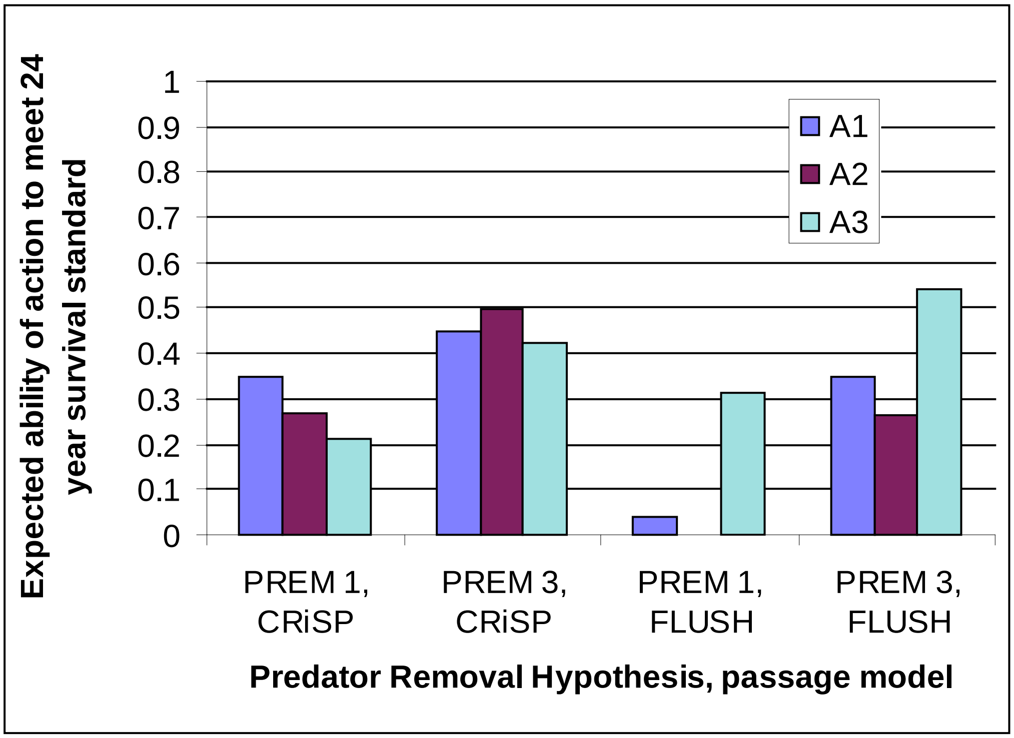


Figure B.1-4. Expected ability to meet 24-year survival standard under different Predator Removal hypotheses.

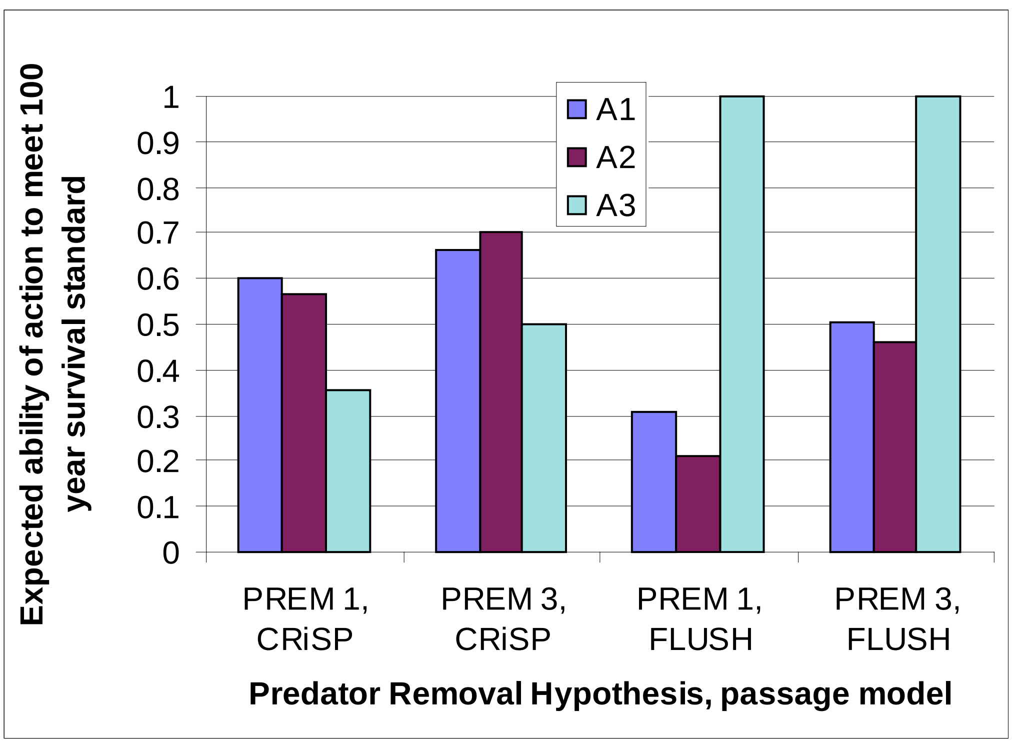


Figure B.1-5. Expected ability to meet 100-year survival standard under different Predator Removal hypotheses.

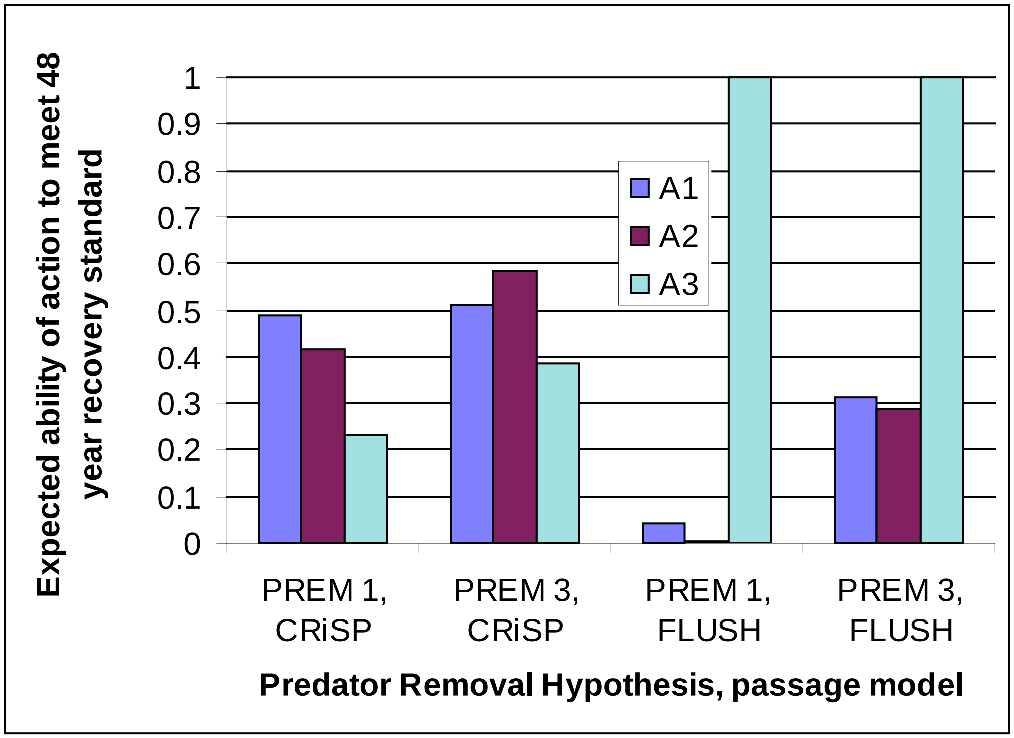


Figure B.1-6. Expected ability to meet 48-year recovery standard under different Predator Removal hypotheses.

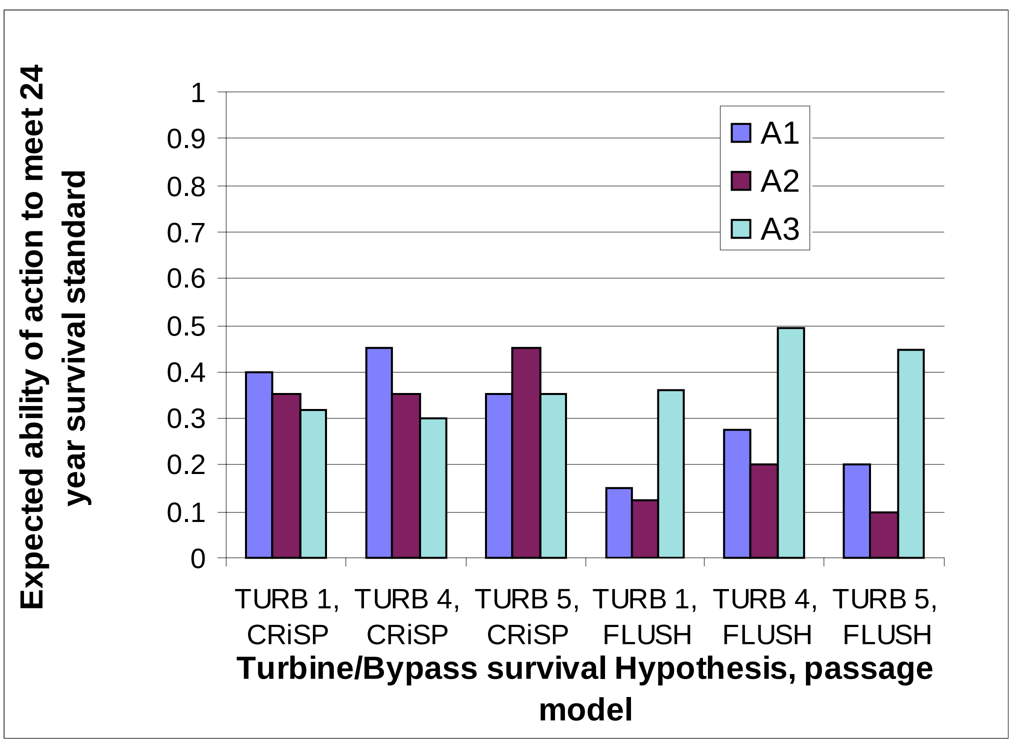


Figure B.1-7. Expected ability to meet 24-year survival standard under different historical turbine/bypass mortality hypotheses.

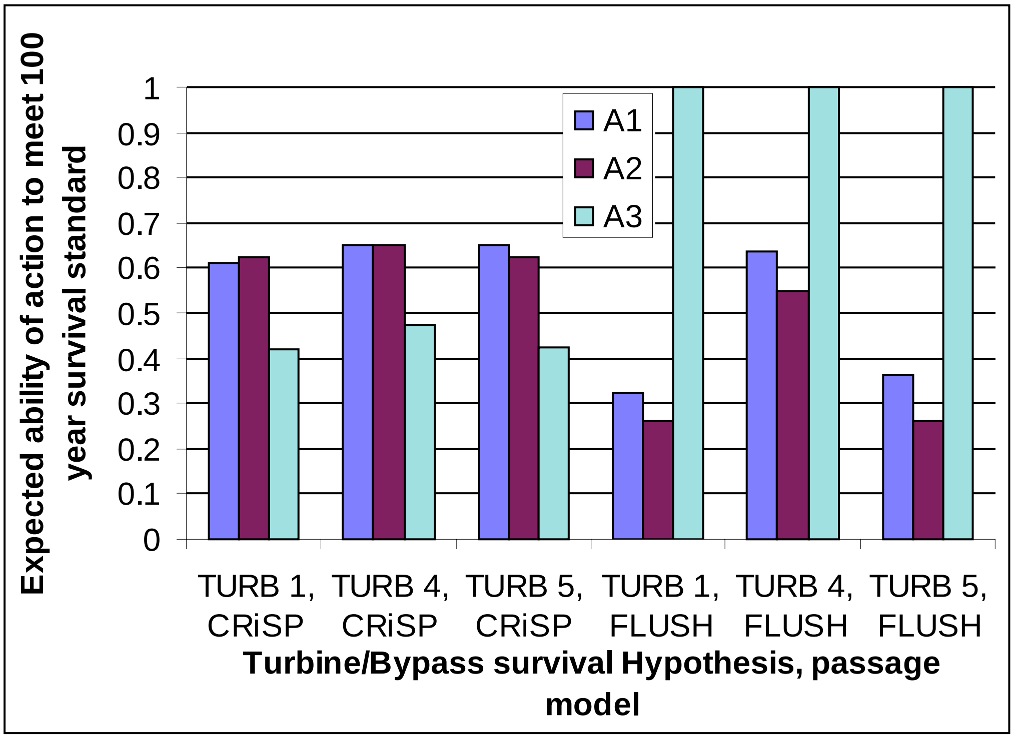


Figure B.1-8. Expected ability to meet 100-year survival standard under different historical turbine/bypass mortality hypotheses.

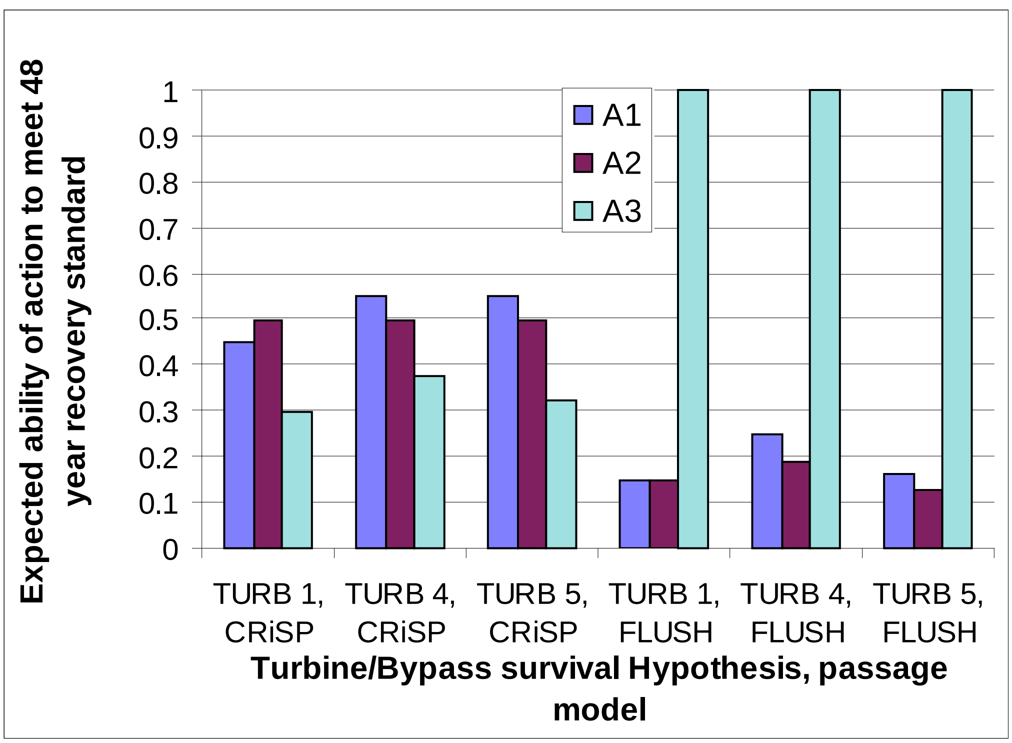


Figure B.1-9. Expected ability to meet 48-year recovery standard under different historical turbine/bypass mortality hypotheses.

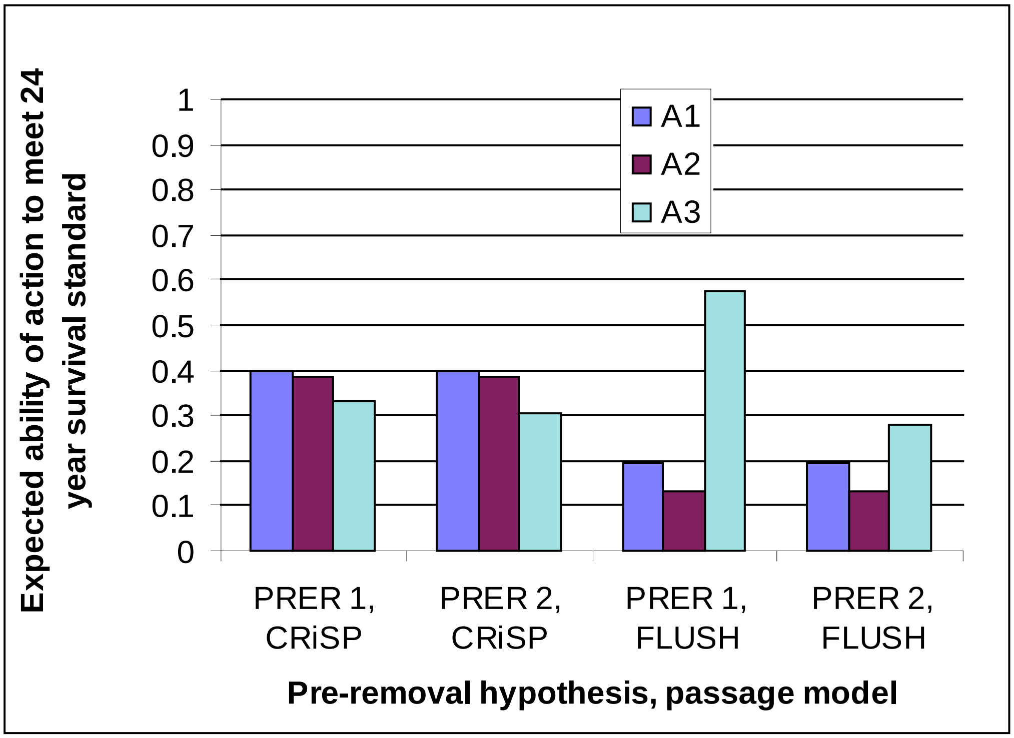


Figure B.1-10. Expected ability to meet 24-year survival standard under different Pre-removal period hypotheses.

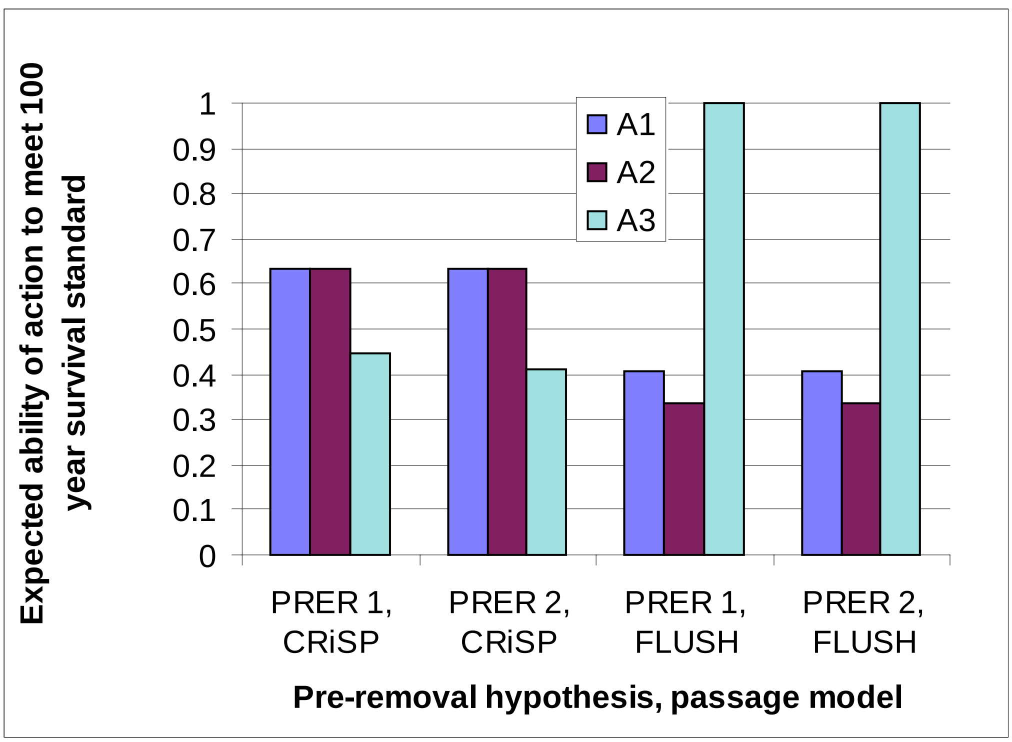


Figure B.1-11. Expected ability to meet 100-year survival standard under different Pre-removal period hypotheses.

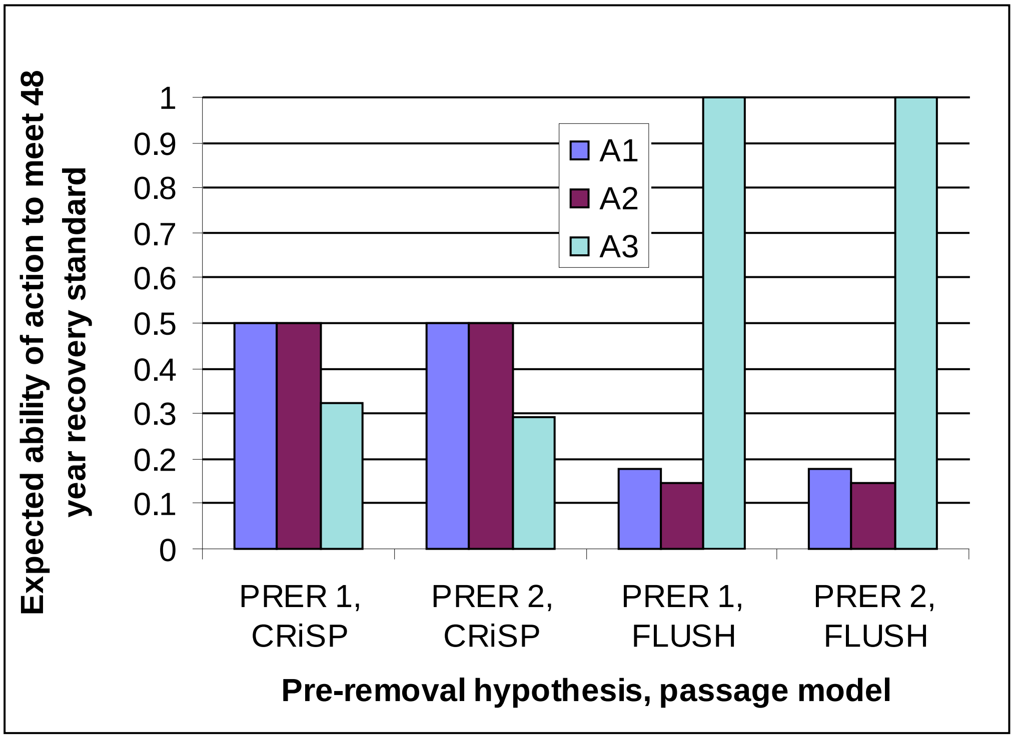


Figure B.1-12. Expected ability to meet 48-year recovery standard under different Pre-removal period hypotheses.

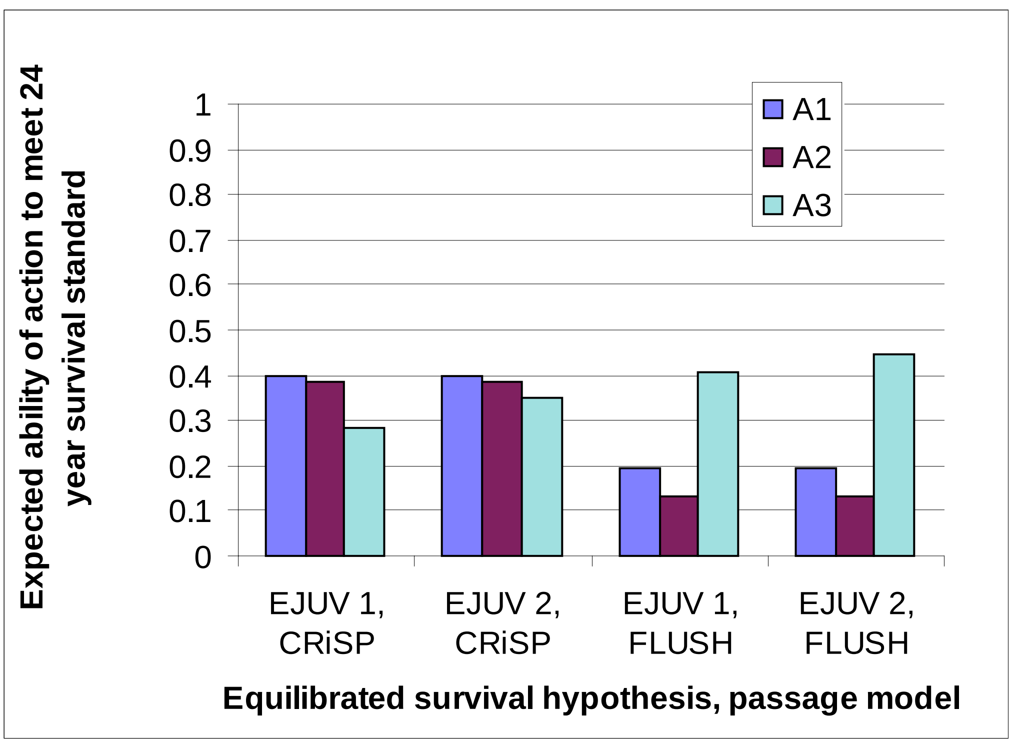


Figure B.1-13. Expected ability to meet 24-year survival standard under different equilibrated juvenile survival rate hypotheses.

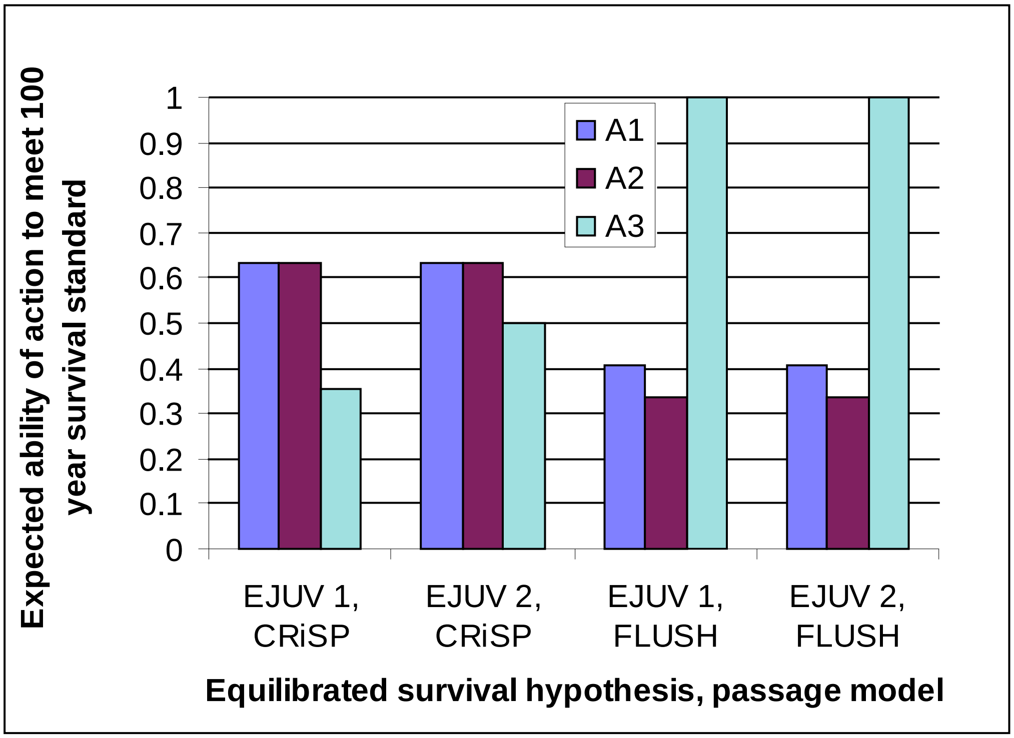


Figure B.1-14. Expected ability to meet 100-year survival standard under different equilibrated juvenile survival rate hypotheses.

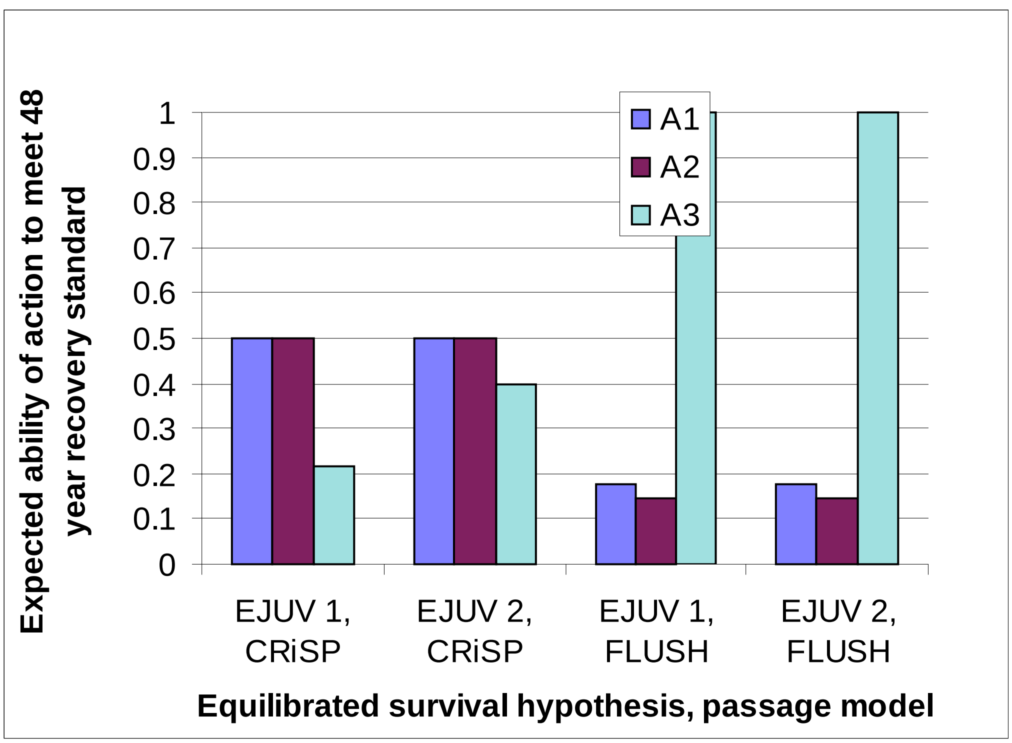


Figure B.1-15. Expected ability to meet 48-year recovery standard under different equilibrated juvenile survival rate hypotheses.

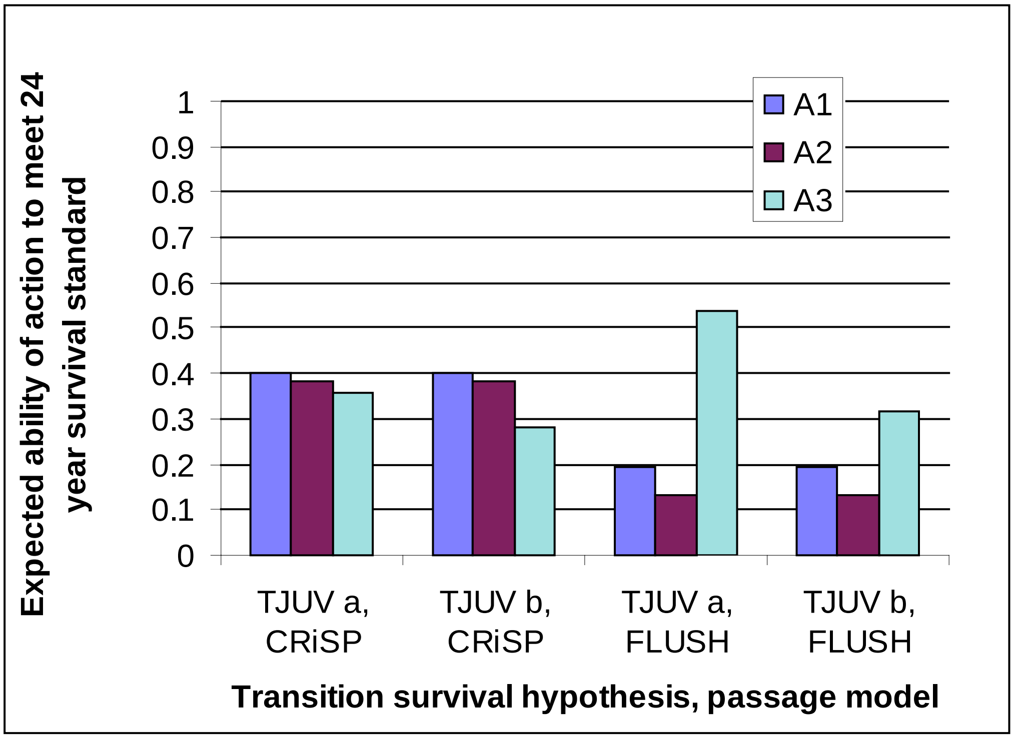


Figure B.1-16. Expected ability to meet 24-year survival standard under different transition period hypotheses.

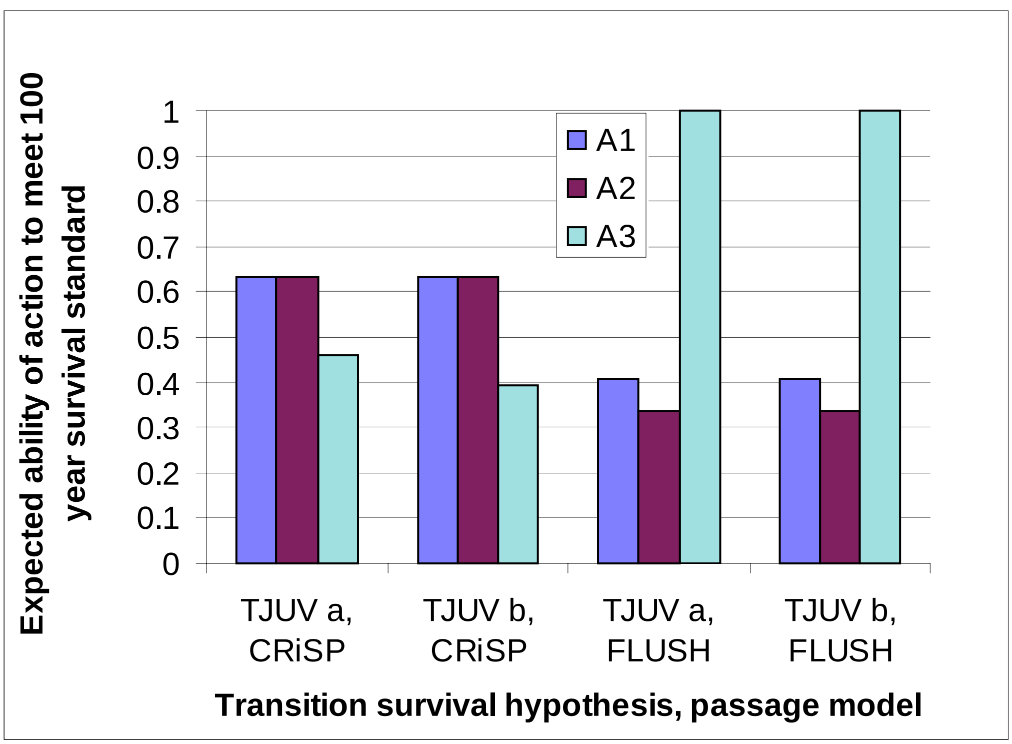


Figure B.1-17. Expected ability to meet 100-year survival standard under different transition period hypotheses.

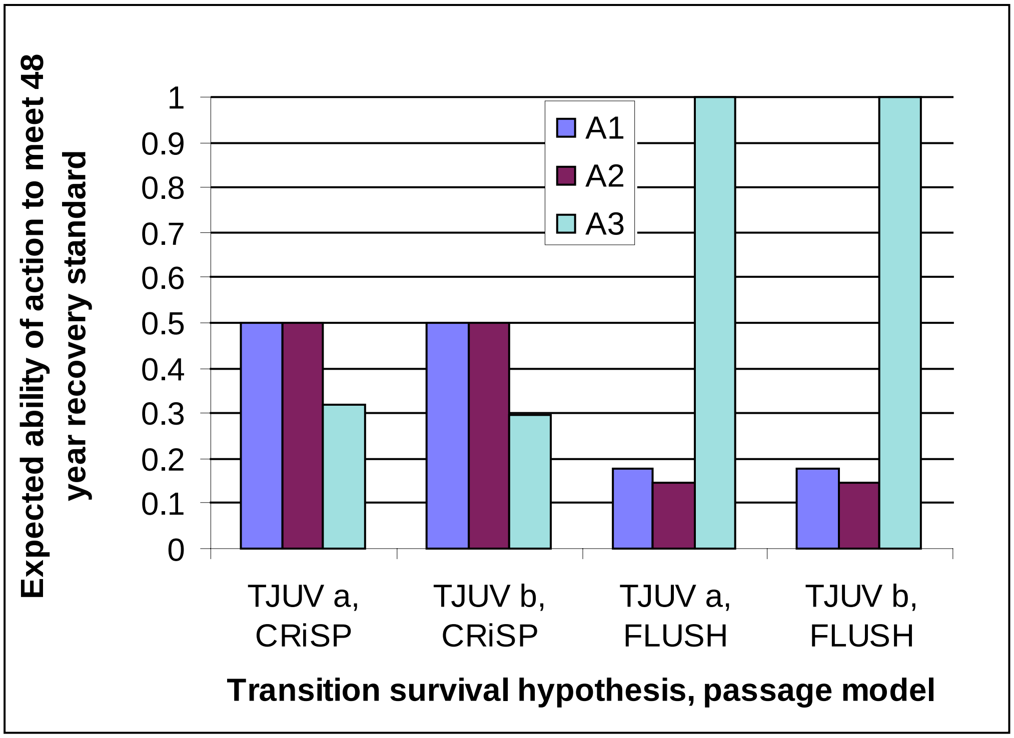


Figure B.1-18. Expected ability to meet 48-year recovery standard under different transition period hypotheses.

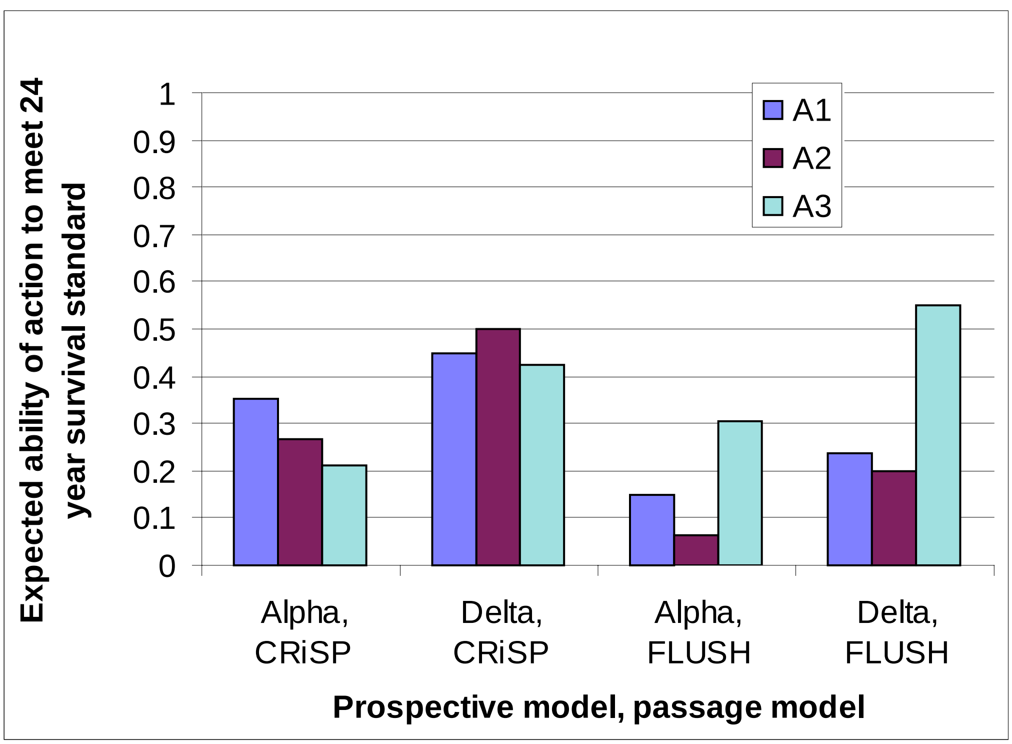


Figure B.1-19 Expected ability to meet 24-year survival standard under different prospective model hypotheses.

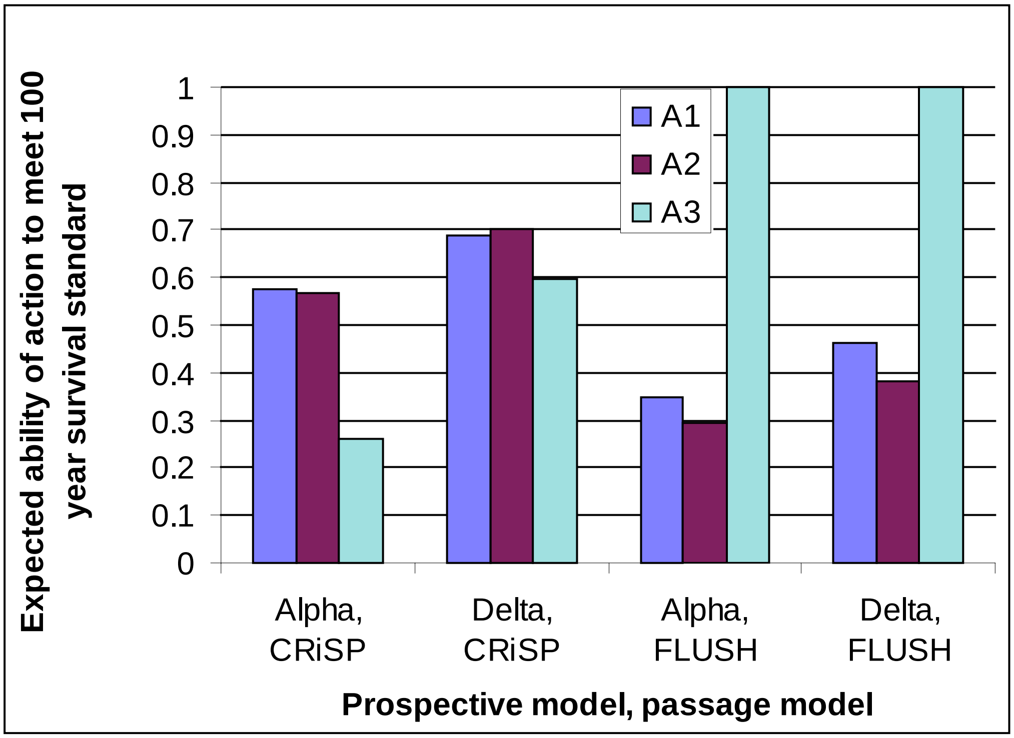


Figure B.1-20 Expected ability to meet 100-year survival standard under different prospective model hypotheses.

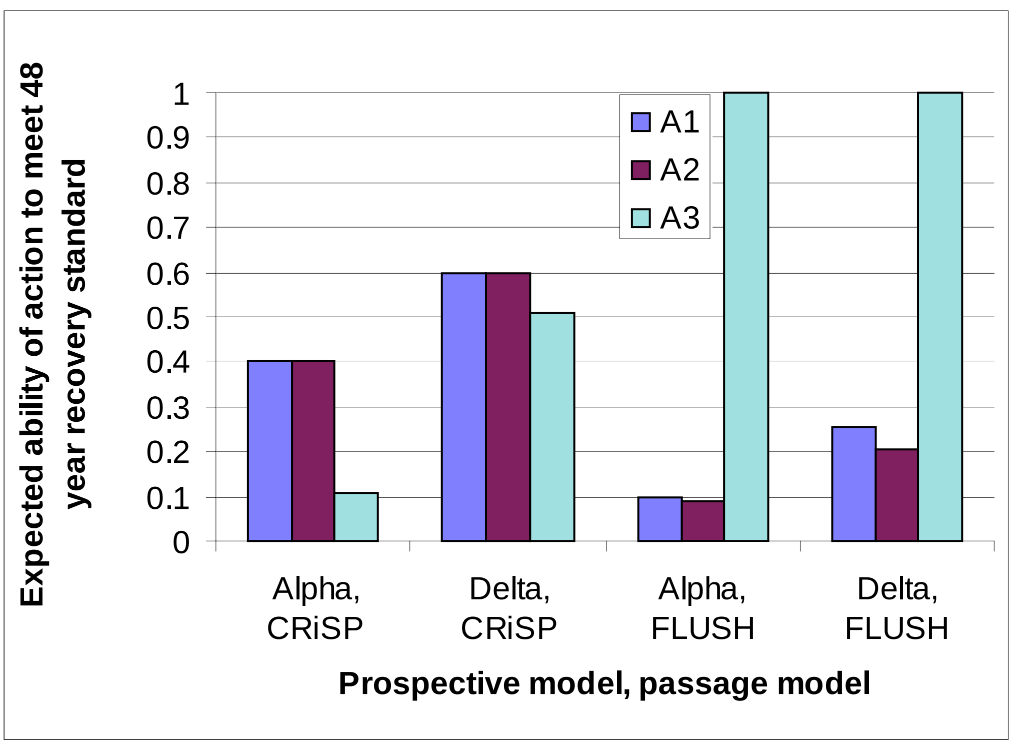


Figure B.1-21 Expected ability to meet 48-year recovery standard under different prospective model hypotheses.

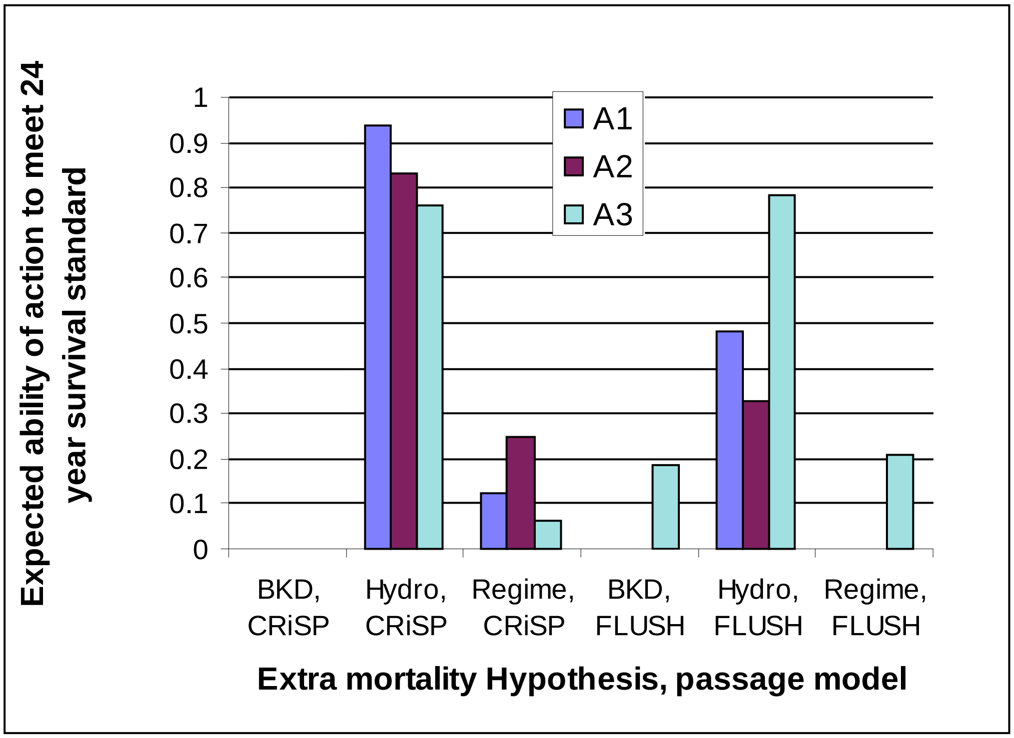


Figure B.1-22 Expected ability to meet 24-year survival standard under different extra mortality hypotheses.

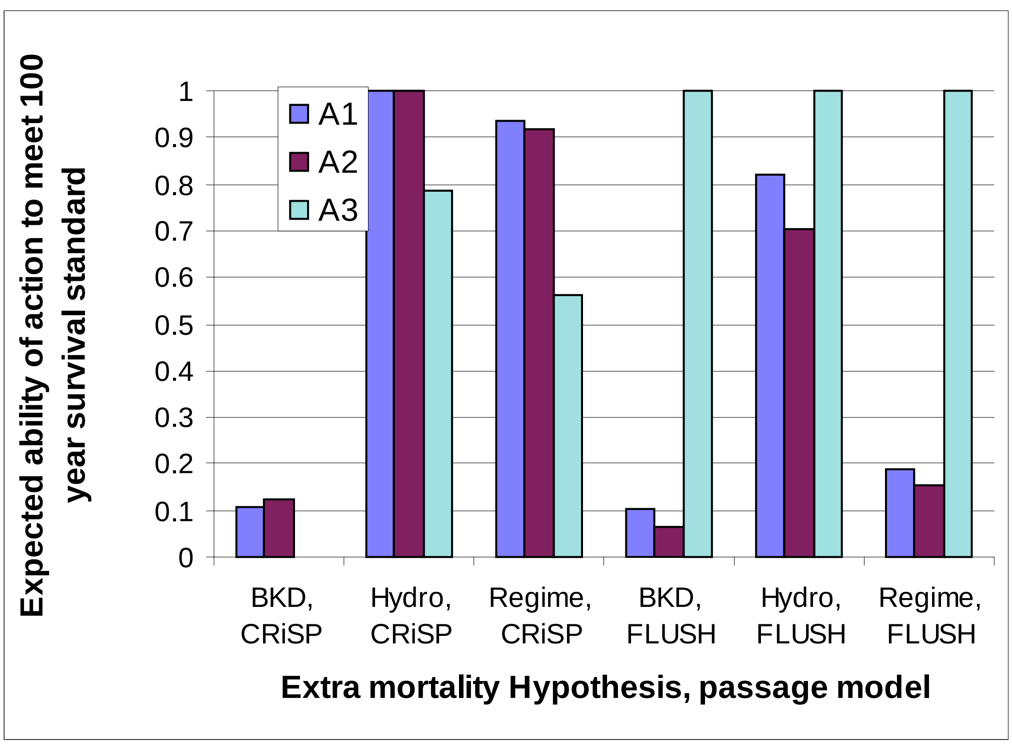


Figure B.1-23 Expected ability to meet 100-year survival standard under different extra mortality hypotheses.

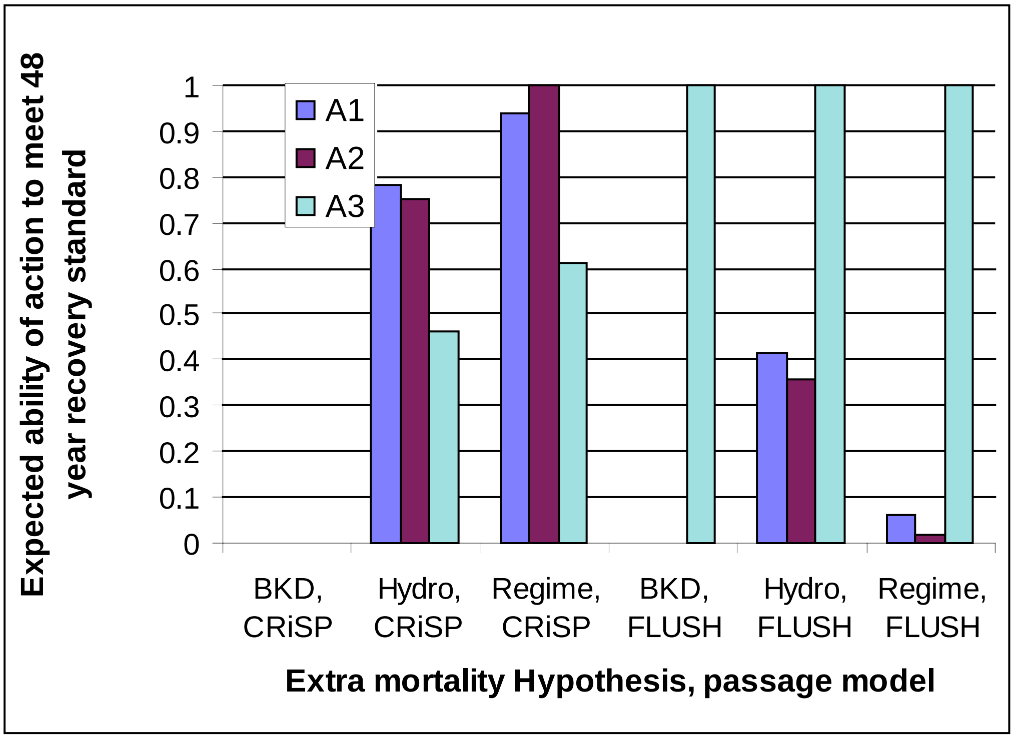


Figure B.1-24 Expected ability to meet 48-year recovery standard under different extra mortality hypotheses.

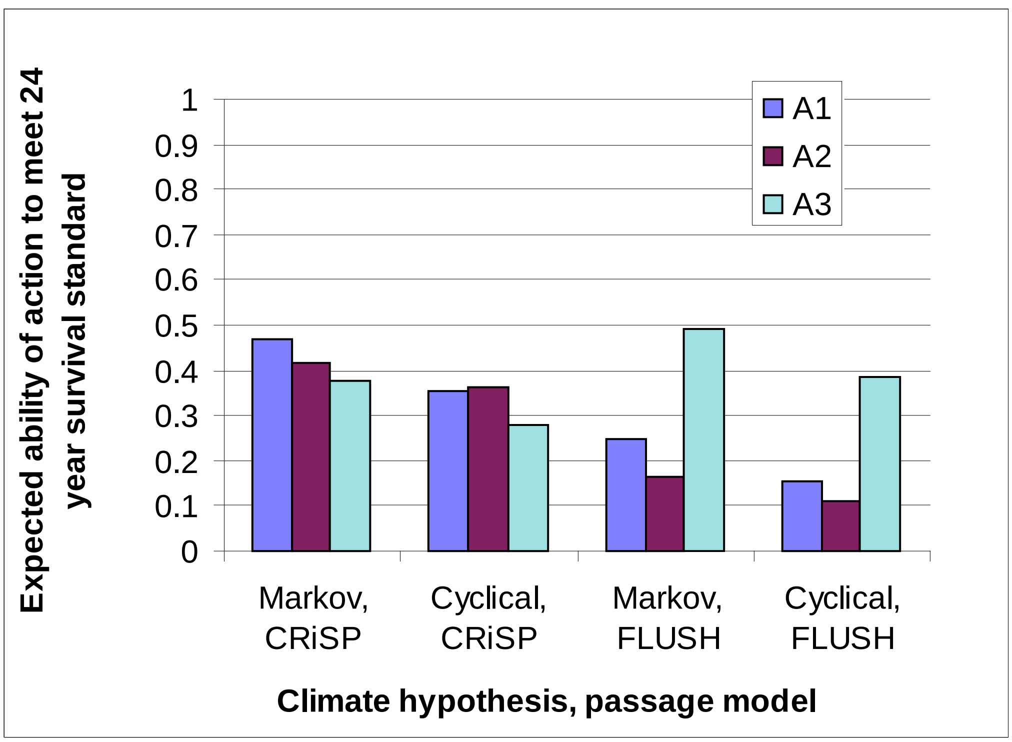


Figure B.1-25 Expected ability to meet 24-year survival standard under different future climate hypotheses.

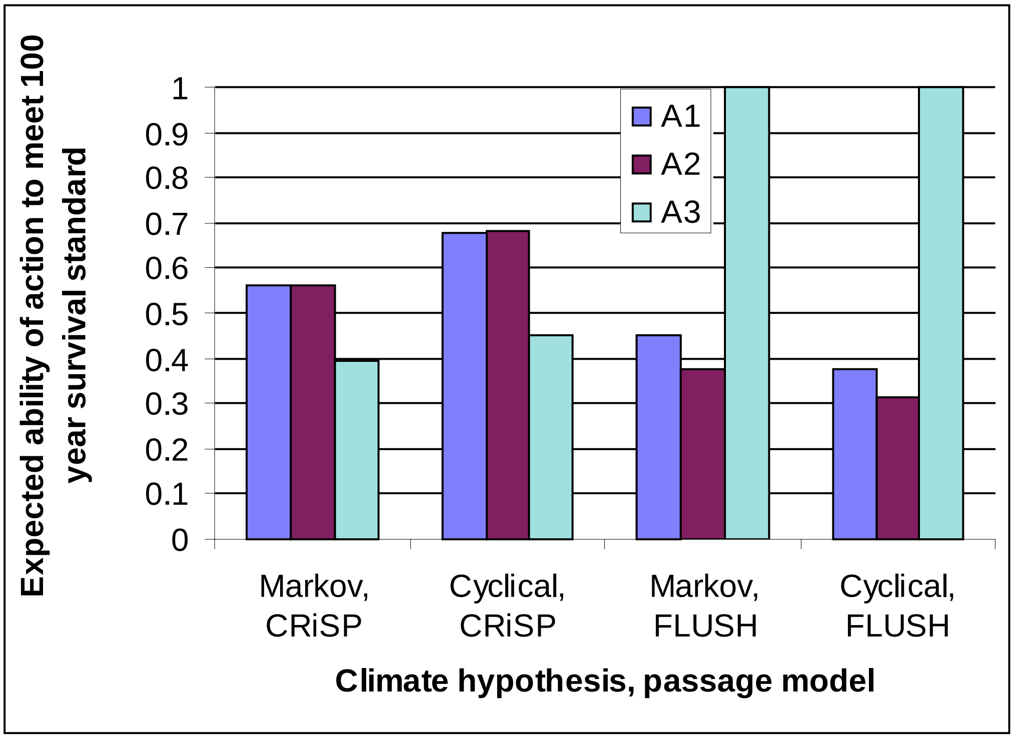


Figure B.1-26 Expected ability to meet 100-year survival standard under different future climate hypotheses.

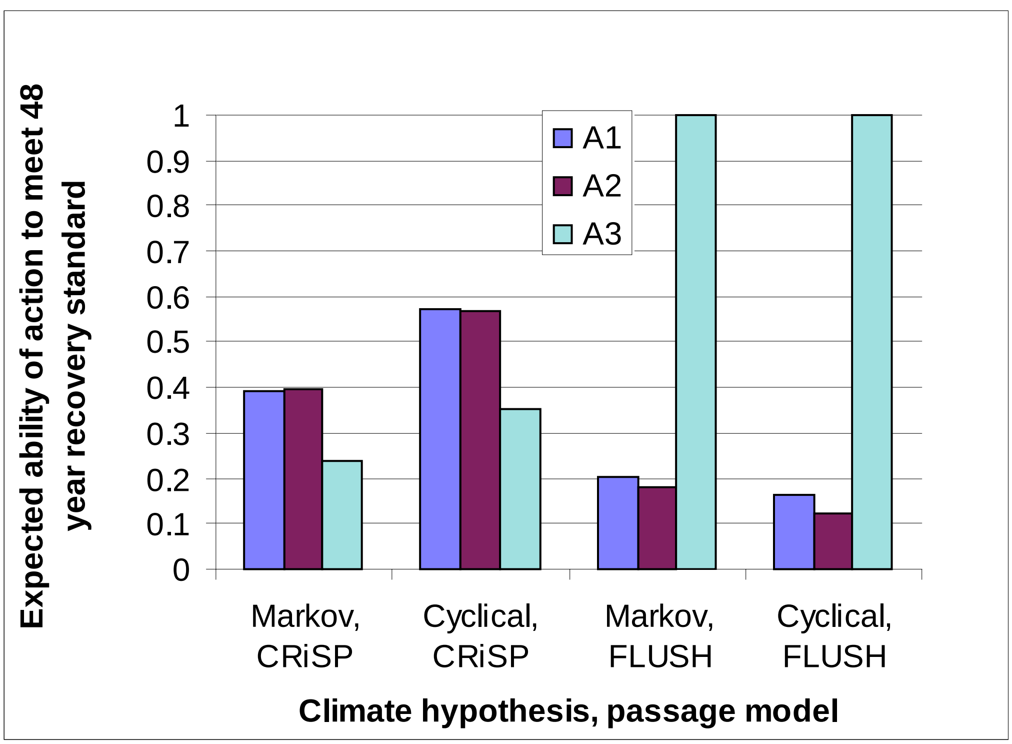


Figure B.1-27 Expected ability to meet 48-year recovery standard under different future climate hypotheses.

B.2. Sensitivity of Outcomes and Decisions to Weightings on Alternative Hypotheses.

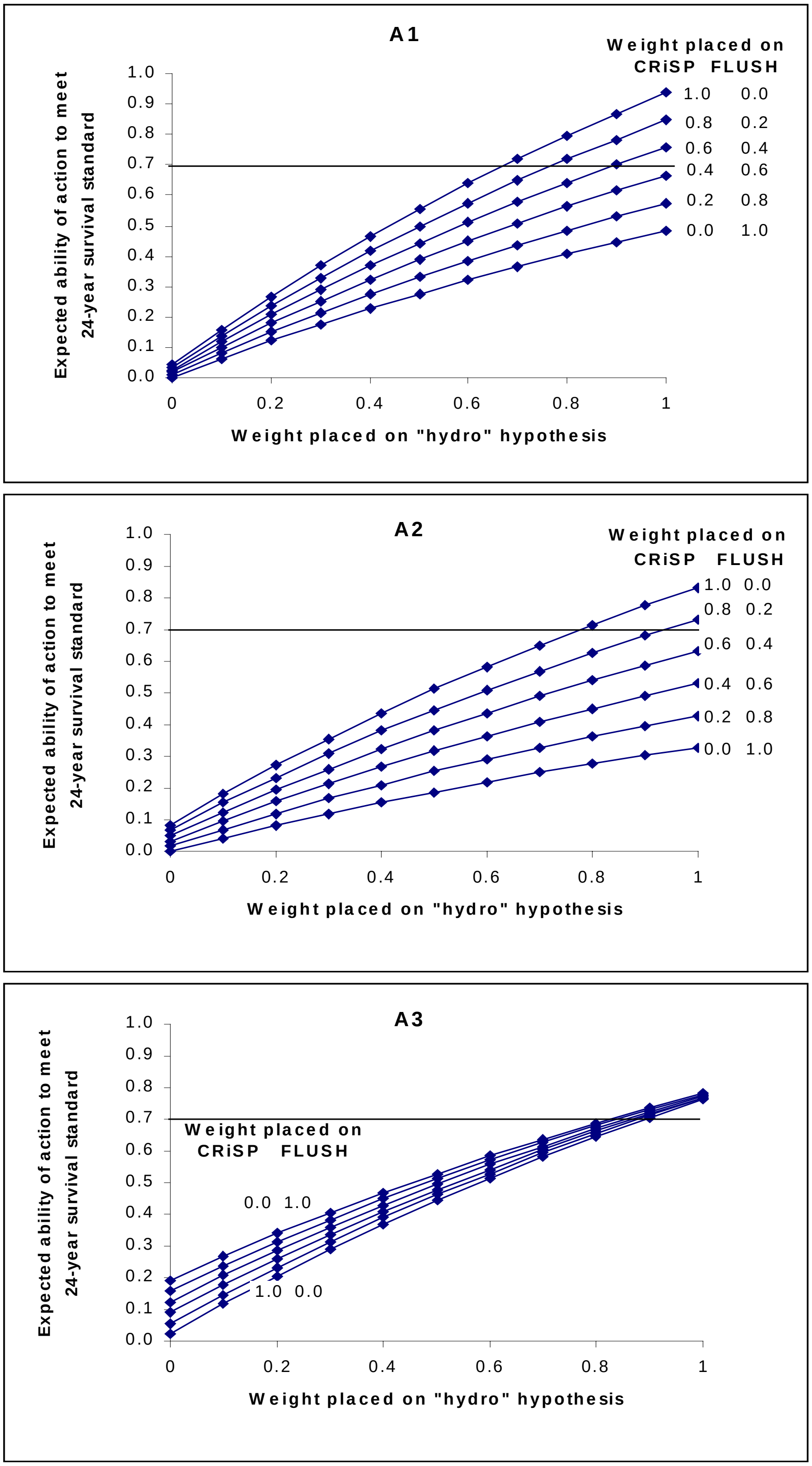
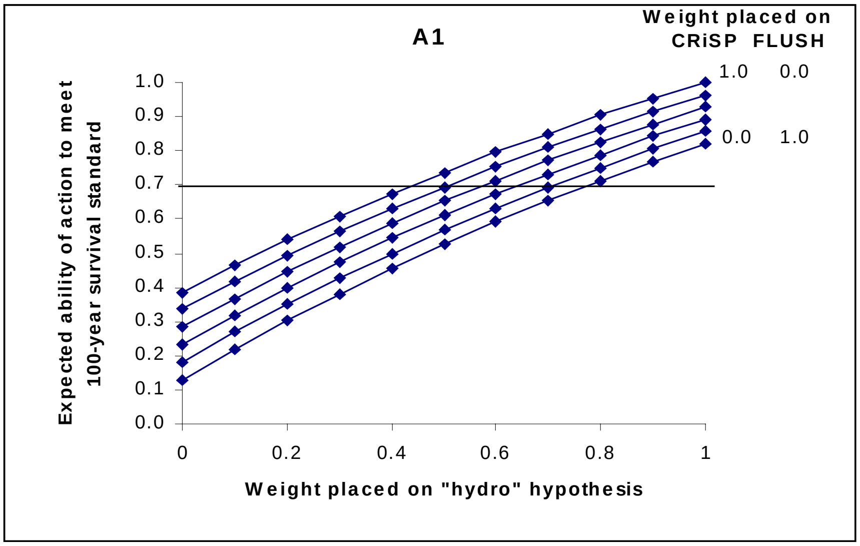
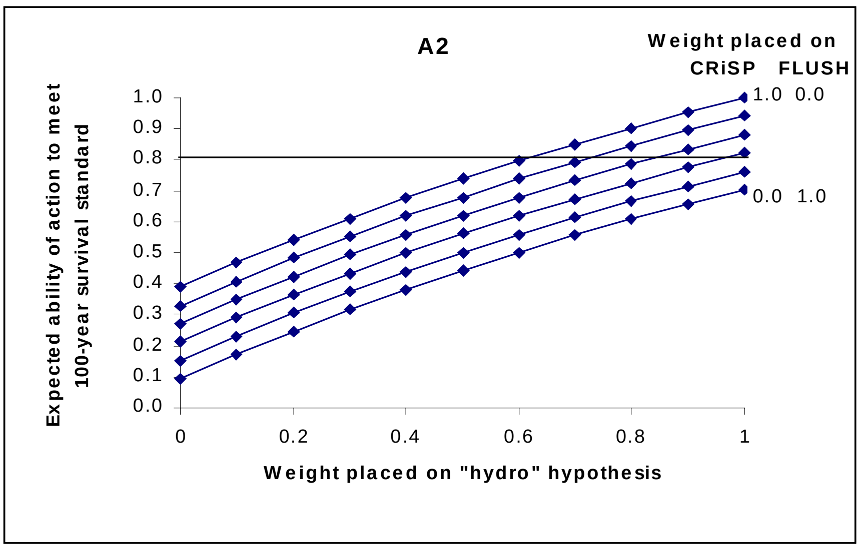


Figure B.2-1. Sensitivity of expected ability to meet 24-year survival standard to relative weights on passage model assumptions / transportation assumptions (CRiSP-T3; FLUSH-T1/T2) and extra mortality hypotheses.





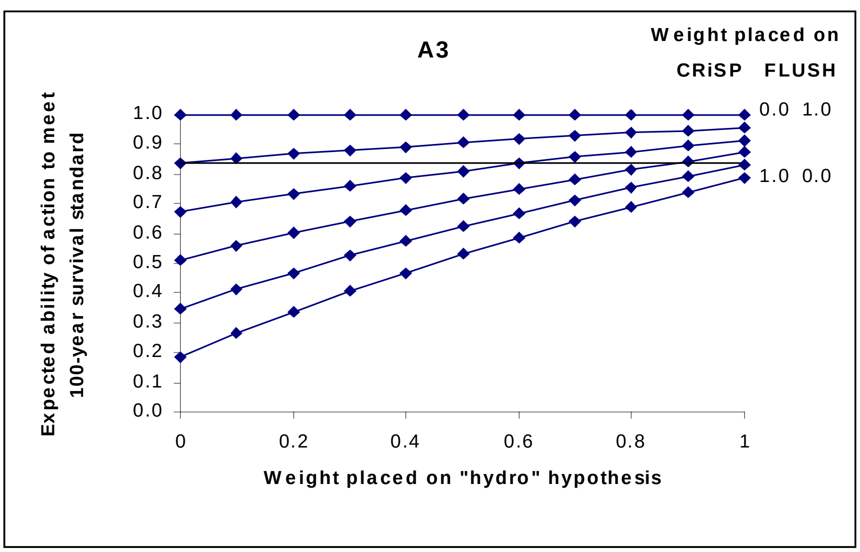


Figure B.2-2. Sensitivity of expected ability to meet 100-year survival standard to relative weights on passage model / transportation assumptions and extra mortality hypotheses.

Figure B.2-3. Sensitivity of expected ability to meet 48-year recovery standard to relative weights on passage model and transportation assumptions, and on extra mortality hypotheses.

B.3. Projected spawning abundance for Imnaha and Marsh Creek stocks.

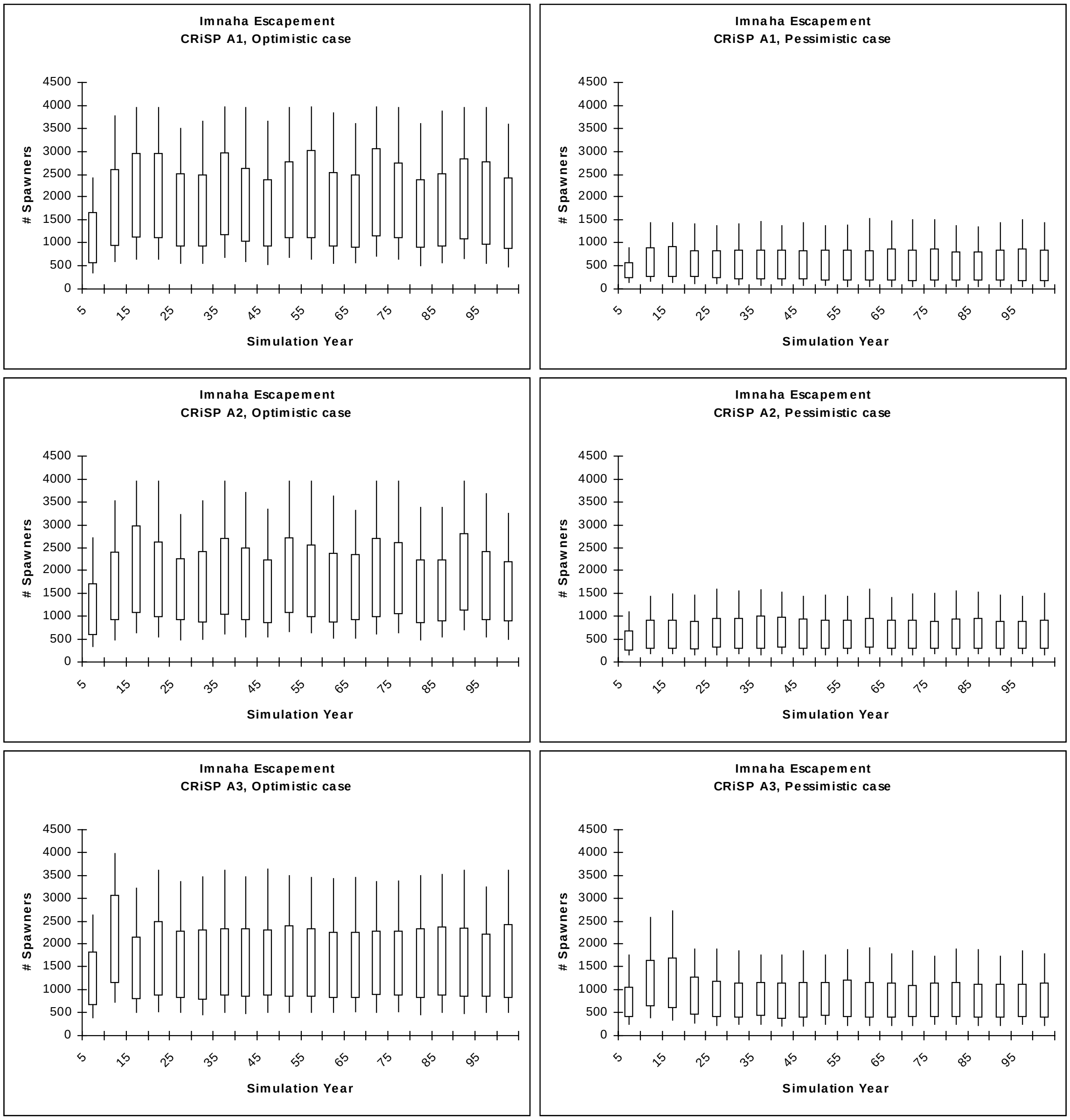


Figure B.3-1. Projected distributions of spawning abundance for Imnaha stock using CRiSP passage model assumptions and T3 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.

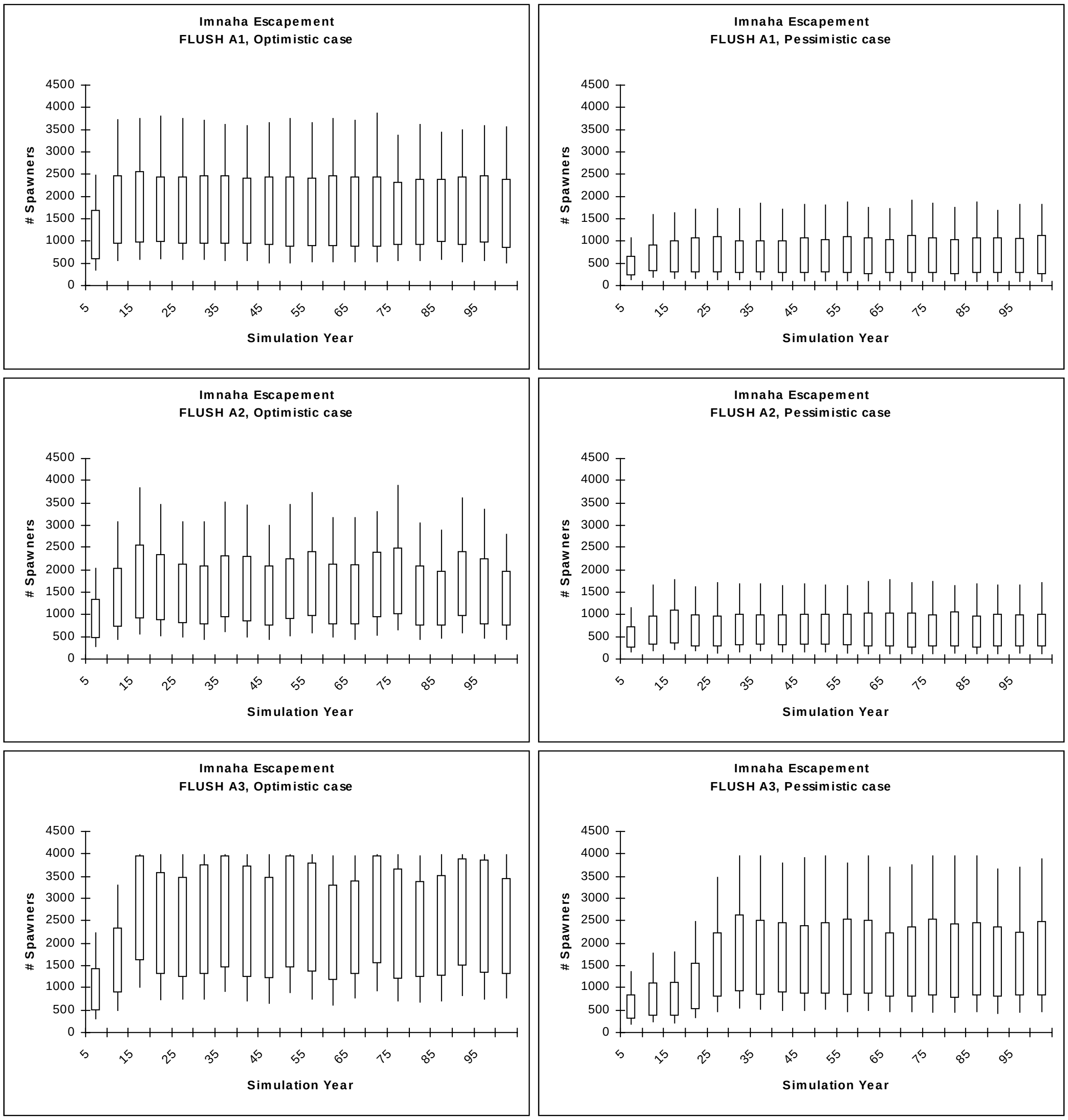


Figure B.3-2. Projected distributions of spawning abundance for Imnaha stock using FLUSH passage model and T1/T2 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.

Figure B.3-3. Projected distributions of spawning abundance for Marsh Creek stock using CRiSP passage model and T3 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.

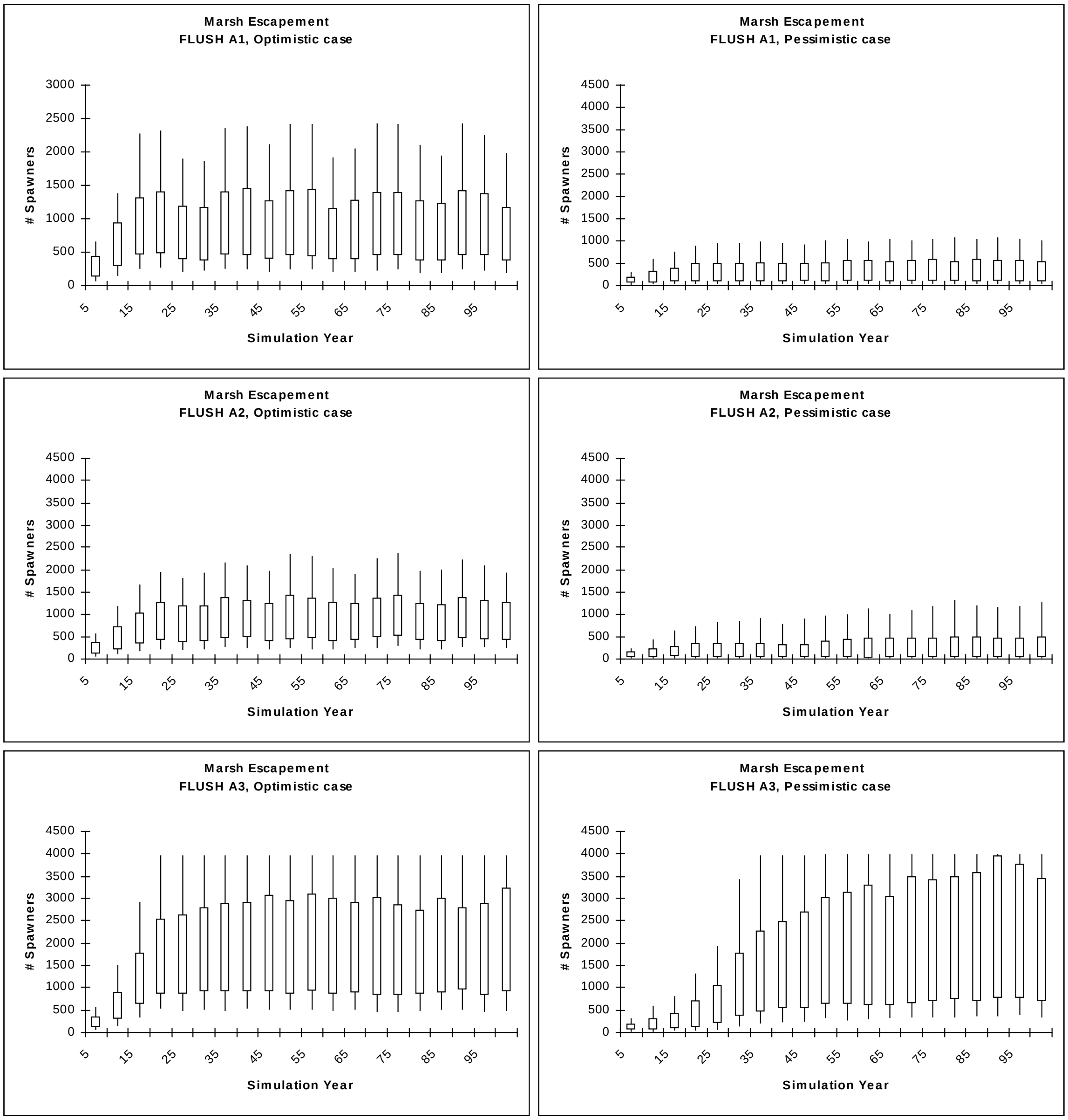
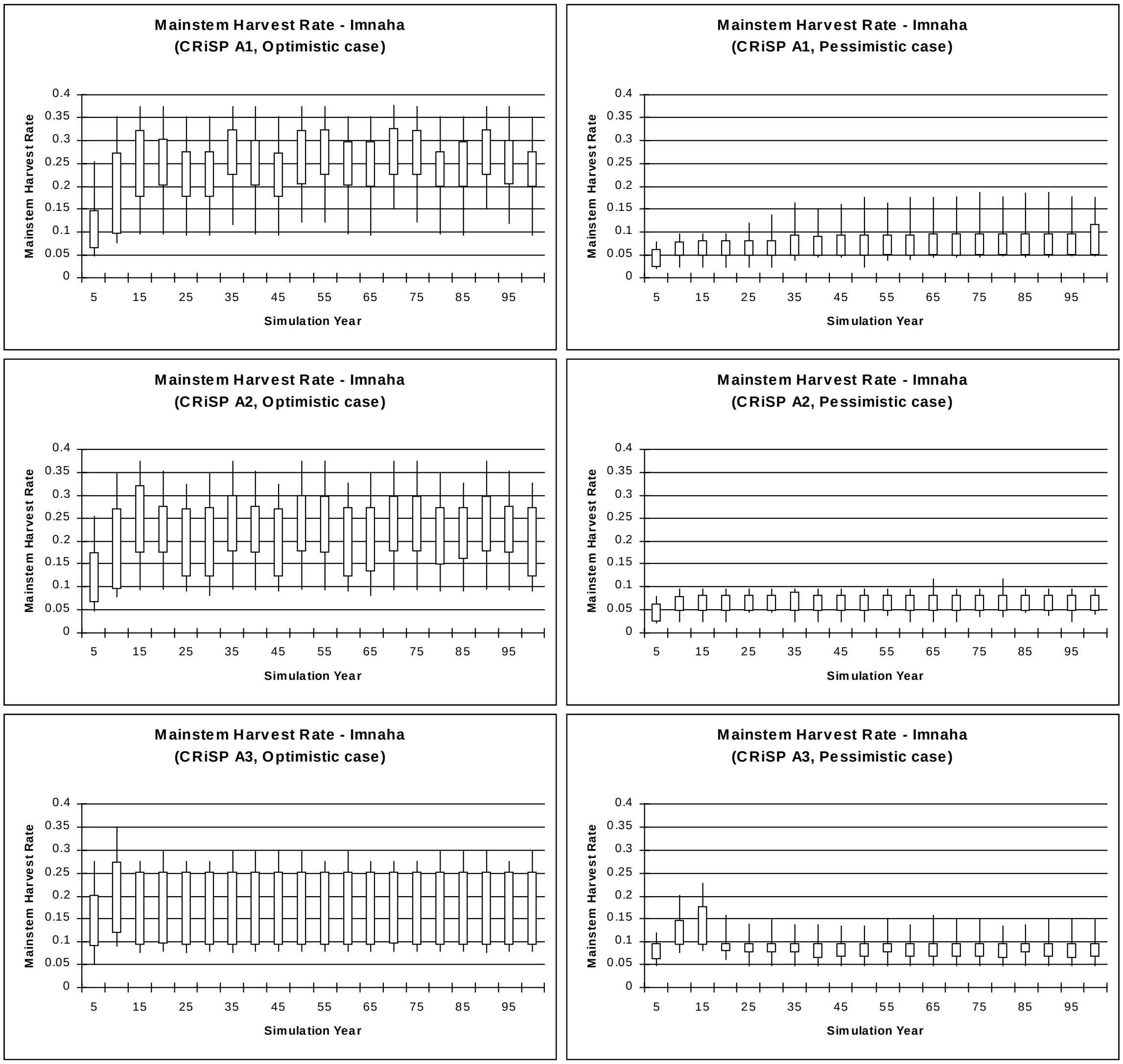
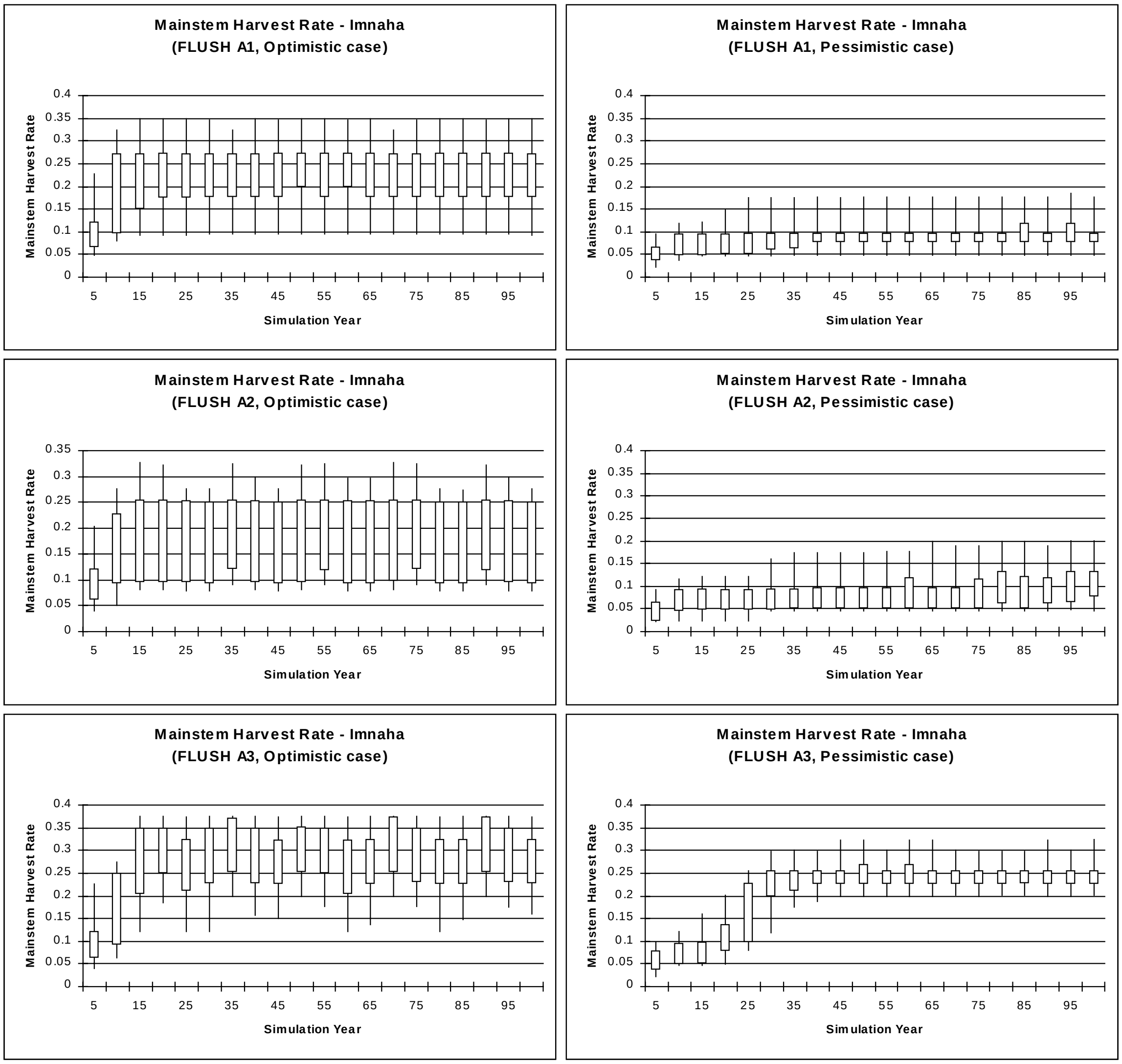


Figure B.3-4. Projected distributions of spawning abundance for Marsh Creek stock using FLUSH passage model and T1/T2 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.

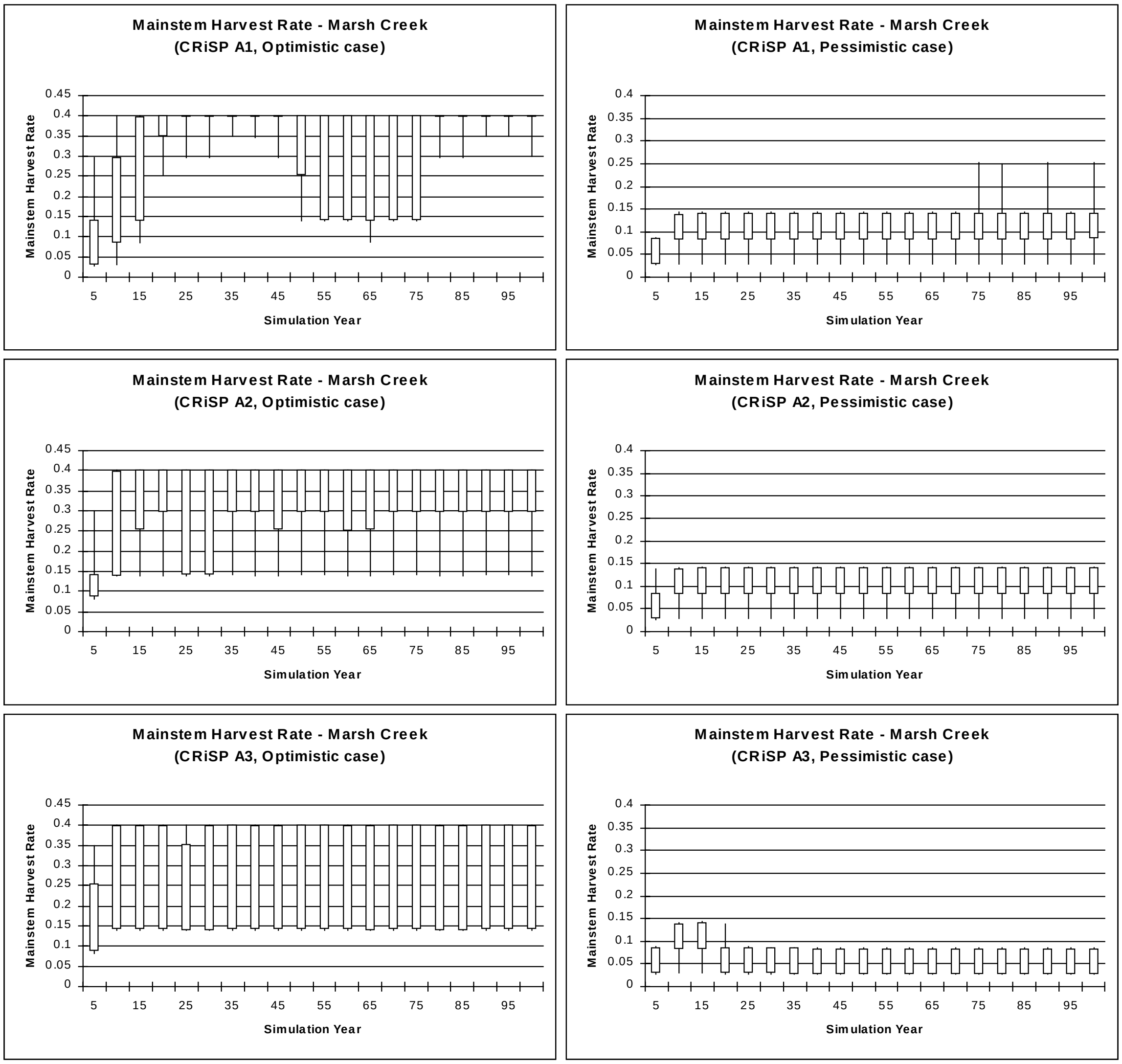
B.4 Projected Mainstem and Tributary Harvest Rates for Imnaha and Marsh Creek stocks



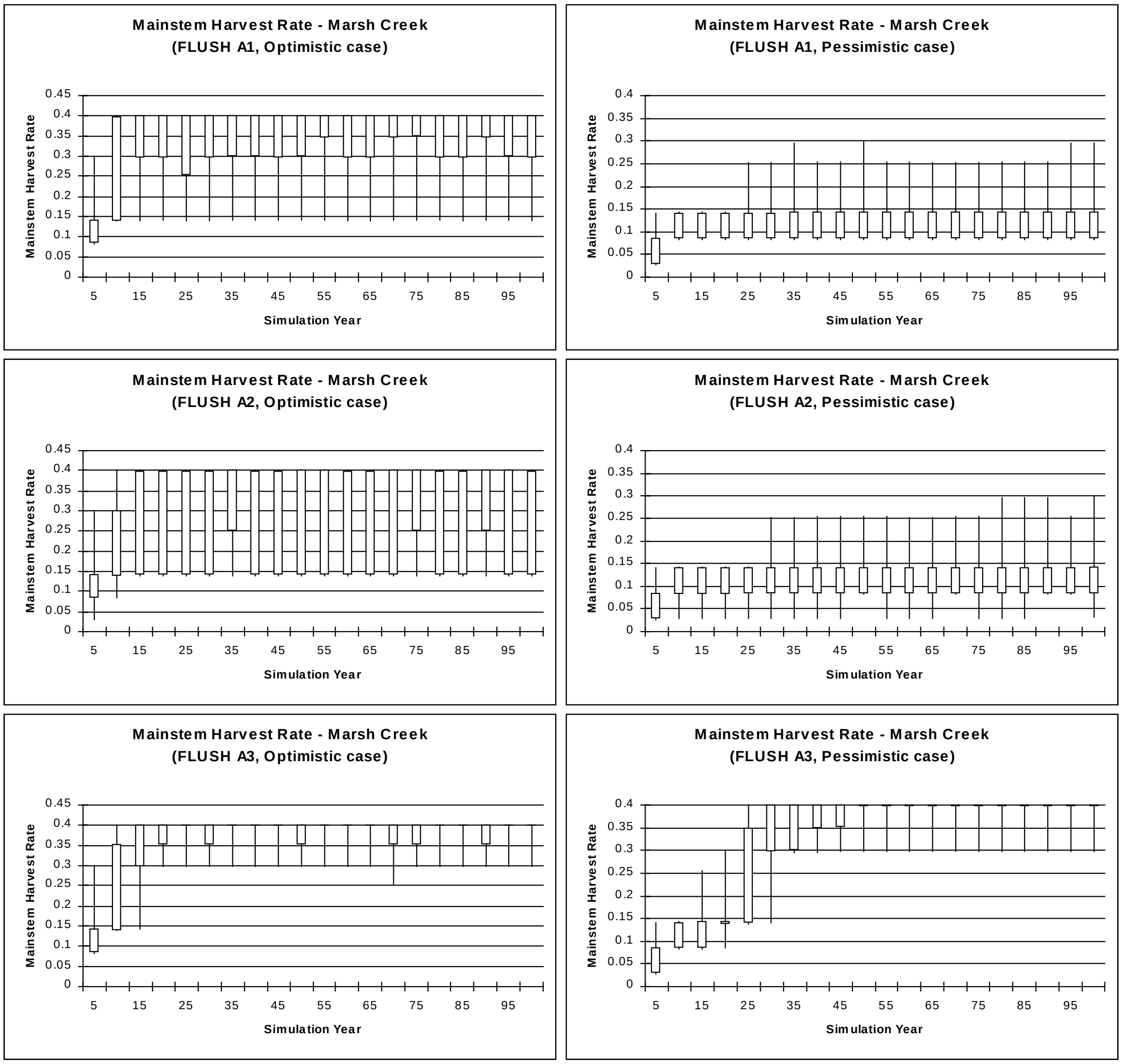
B.4-1. Mainstem harvest rates under A1, A2, and A3 for the Imnaha stock of spring-summer chinook over 100-year simulation period for an optimistic and pessimistic aggregate hypothesis based on CRiSP passage model and T3 transportation assumptions. "Optimistic" and "Pessimistic" cases are defined in Section 5.7.



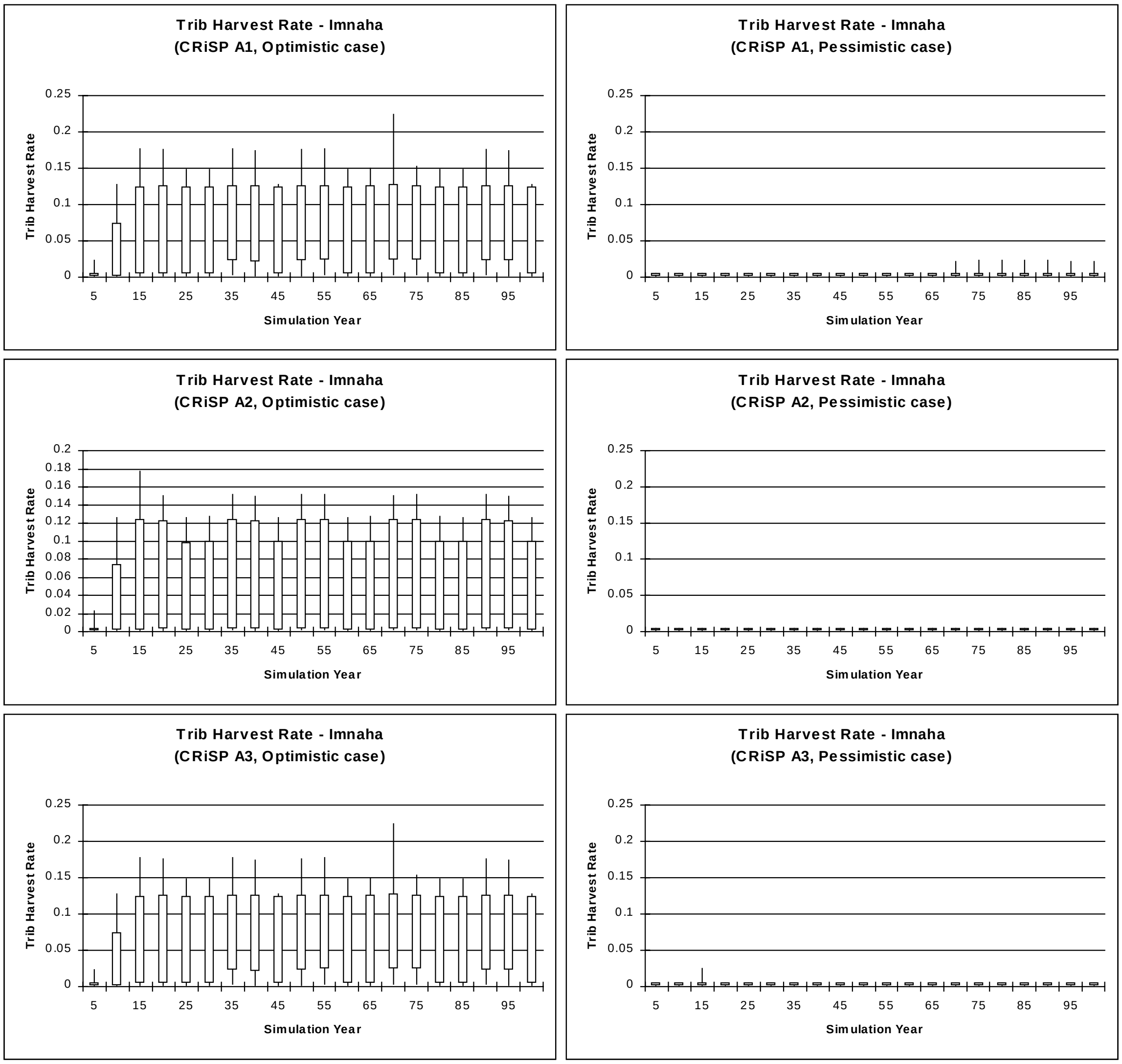
B.4-2. Mainstem harvest rates under A1, A2, and A3 for the Imnaha stock of spring-summer chinook over 100-year simulation period for an optimistic and pessimistic aggregate hypothesis based on FLUSH passage model and T1/T2 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.



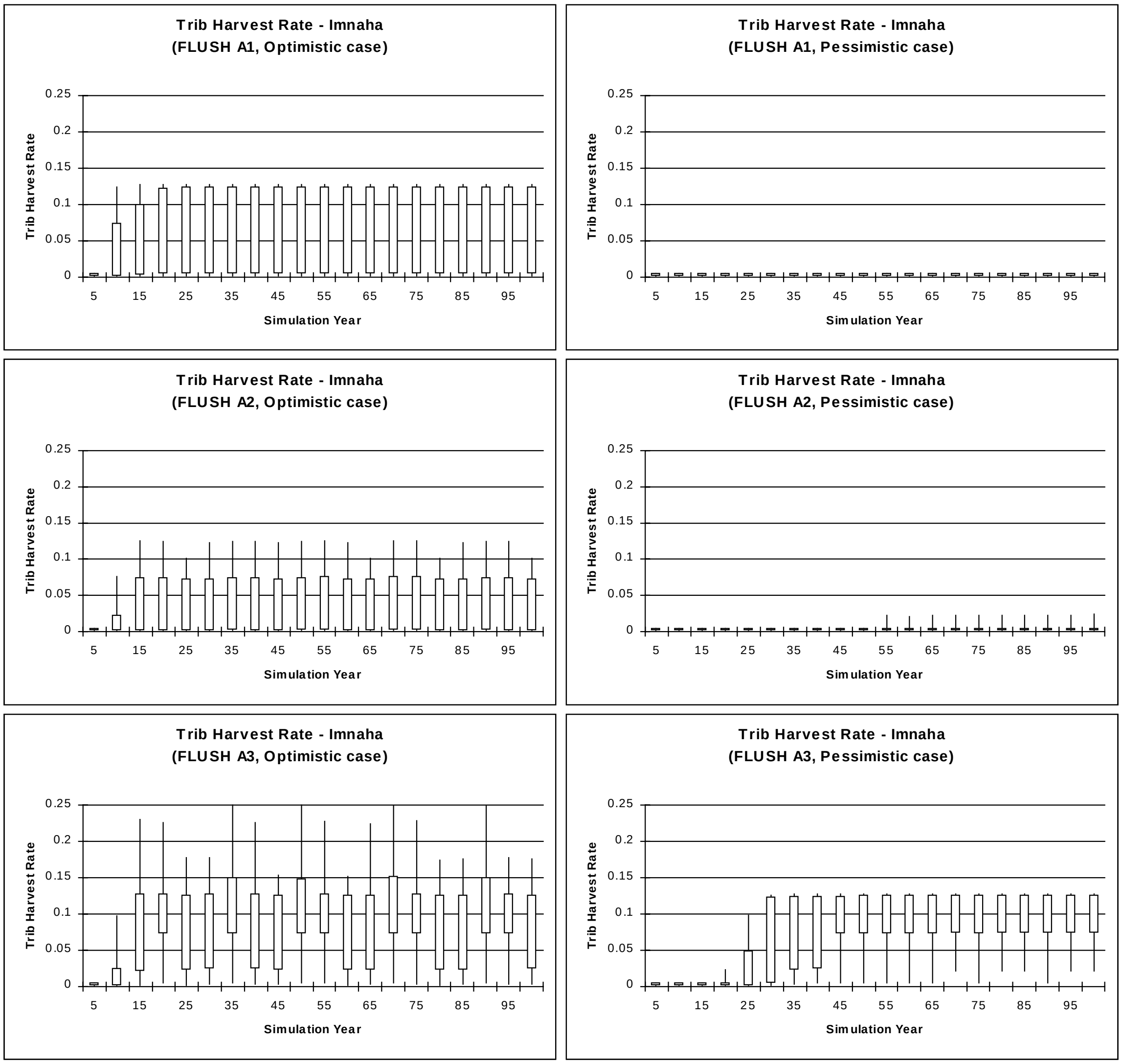
B.4-3. Mainstem harvest rates under A1, A2, and A3 for the Marsh Creek stock of spring-summer chinook over 100-year simulation period for an optimistic and pessimistic aggregate hypothesis based on CRiSP passage model and T3 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.



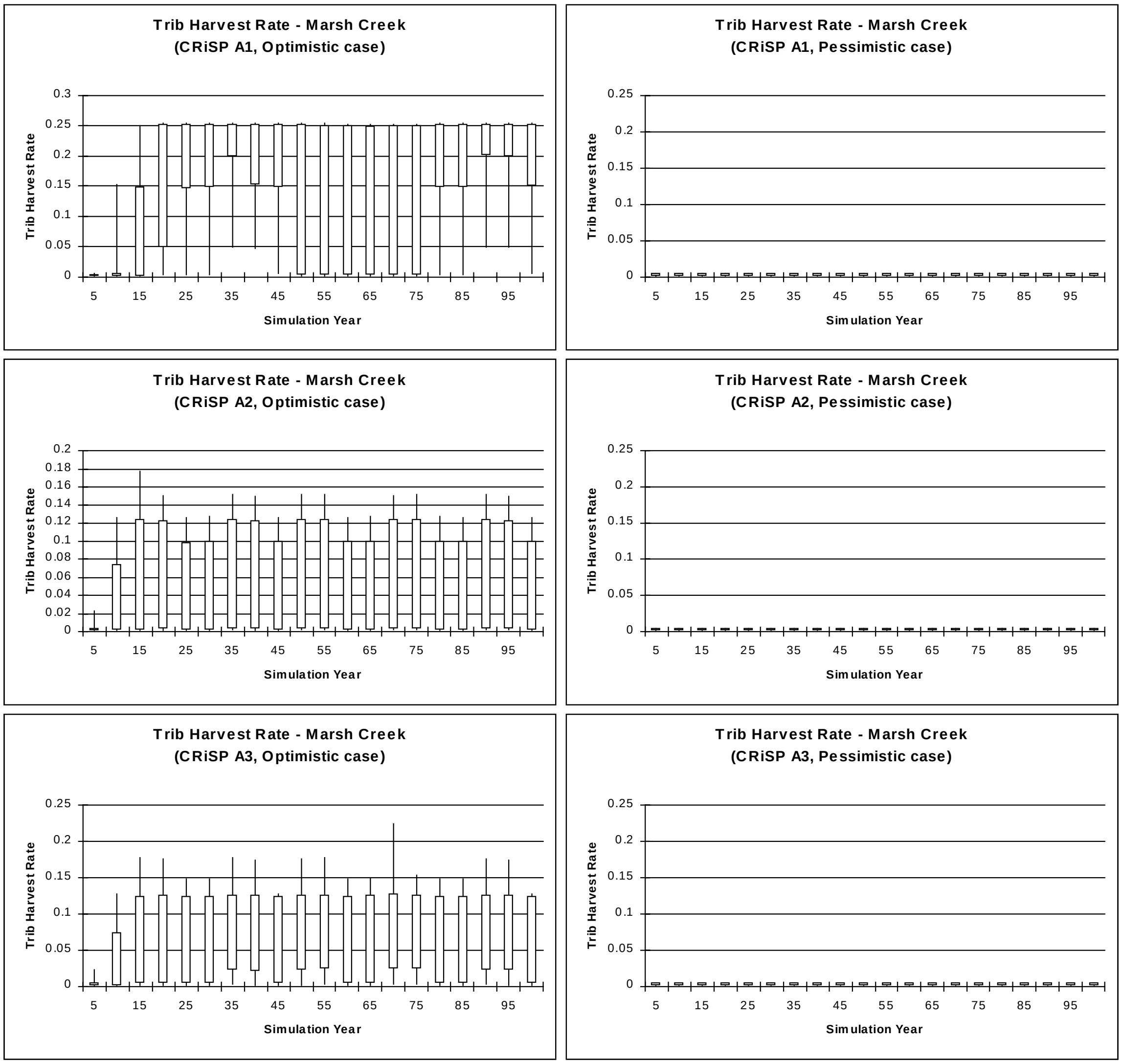
B.4-4. Mainstem harvest rates under A1, A2, and A3 for the Marsh Creek stock of spring-summer chinook over 100-year simulation period for an optimistic and pessimistic aggregate hypothesis based on FLUSH passage model and T1/T2 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.



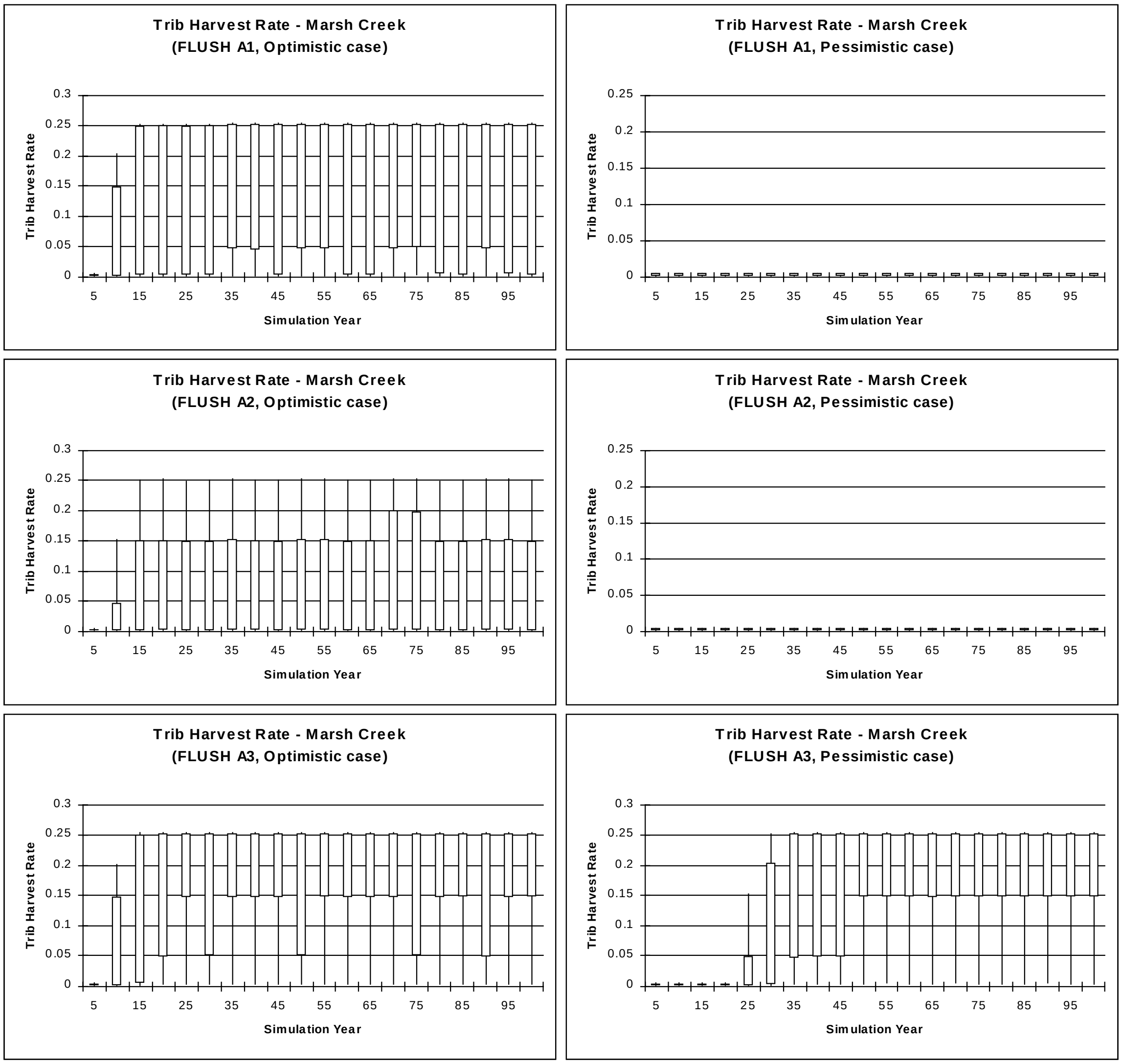
B.4-5. Tributary harvest rates under A1, A2, and A3 for the Imnaha stock of spring-summer chinook over 100-year simulation period for an optimistic and pessimistic aggregate hypothesis based on CRiSP passage model and T3 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.



B.4-6. Tributary harvest rates under A1, A2, and A3 for the Imnaha stock of spring-summer chinook over 100-year simulation period for an optimistic and pessimistic aggregate hypothesis based on FLUSH passage model and T1/T2 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.



B.4-7. Tributary harvest rates under A1, A2, and A3 for the Marsh Creek stock of spring-summer chinook over 100-year simulation period for an optimistic and pessimistic aggregate hypothesis based on CRiSP passage model and T3 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.



B.4-8. Tributary harvest rates under A1, A2, and A3 for the Marsh Creek stock of spring-summer chinook over 100-year simulation period for an optimistic and pessimistic aggregate hypothesis based on FLUSH passage model and T1/T2 transportation assumptions. “Optimistic” and “Pessimistic” cases are defined in Section 5.7.

B.5 Further analyses of Smolt-to-Adult Survival Rates

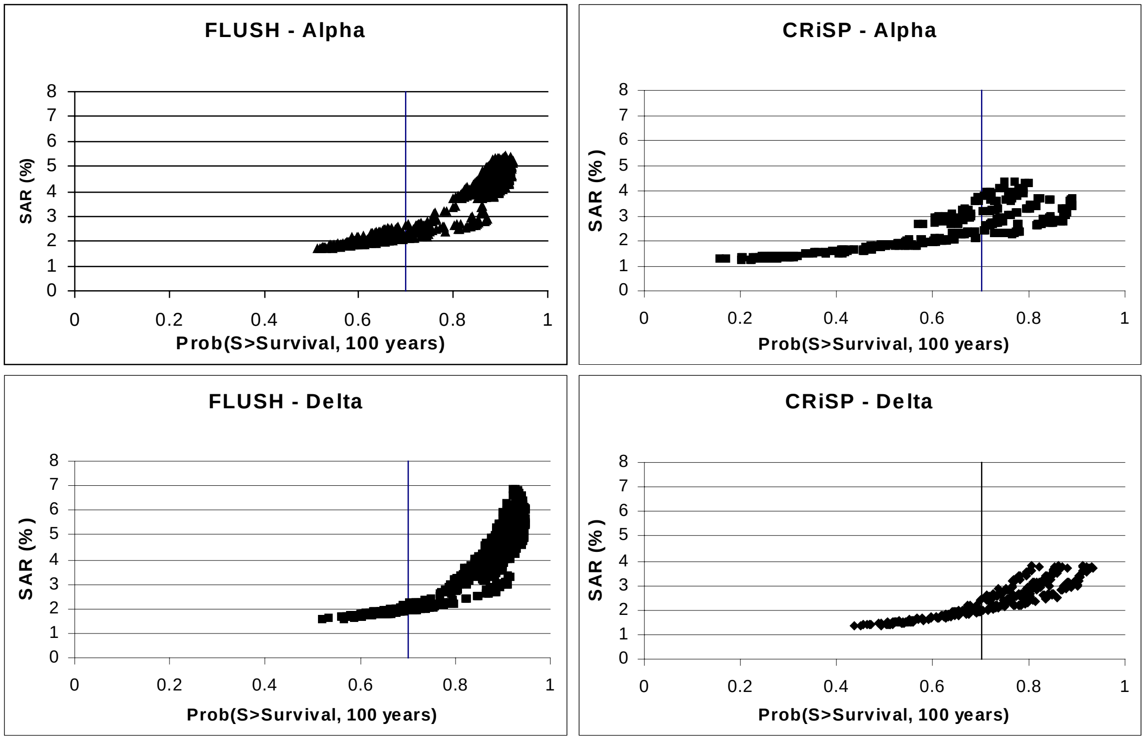


Figure B.5-1. SARs as a function of the probability of the spawning abundance of the sixth best stock exceeding the survival escapement level over 100 years. Data are broken out by passage model / transportation assumptions (CRiSP-T3, FLUSH-T1/T2), and by prospective model. The vertical line at 0.7 indicates the NMFS standard of 0.7 probability.

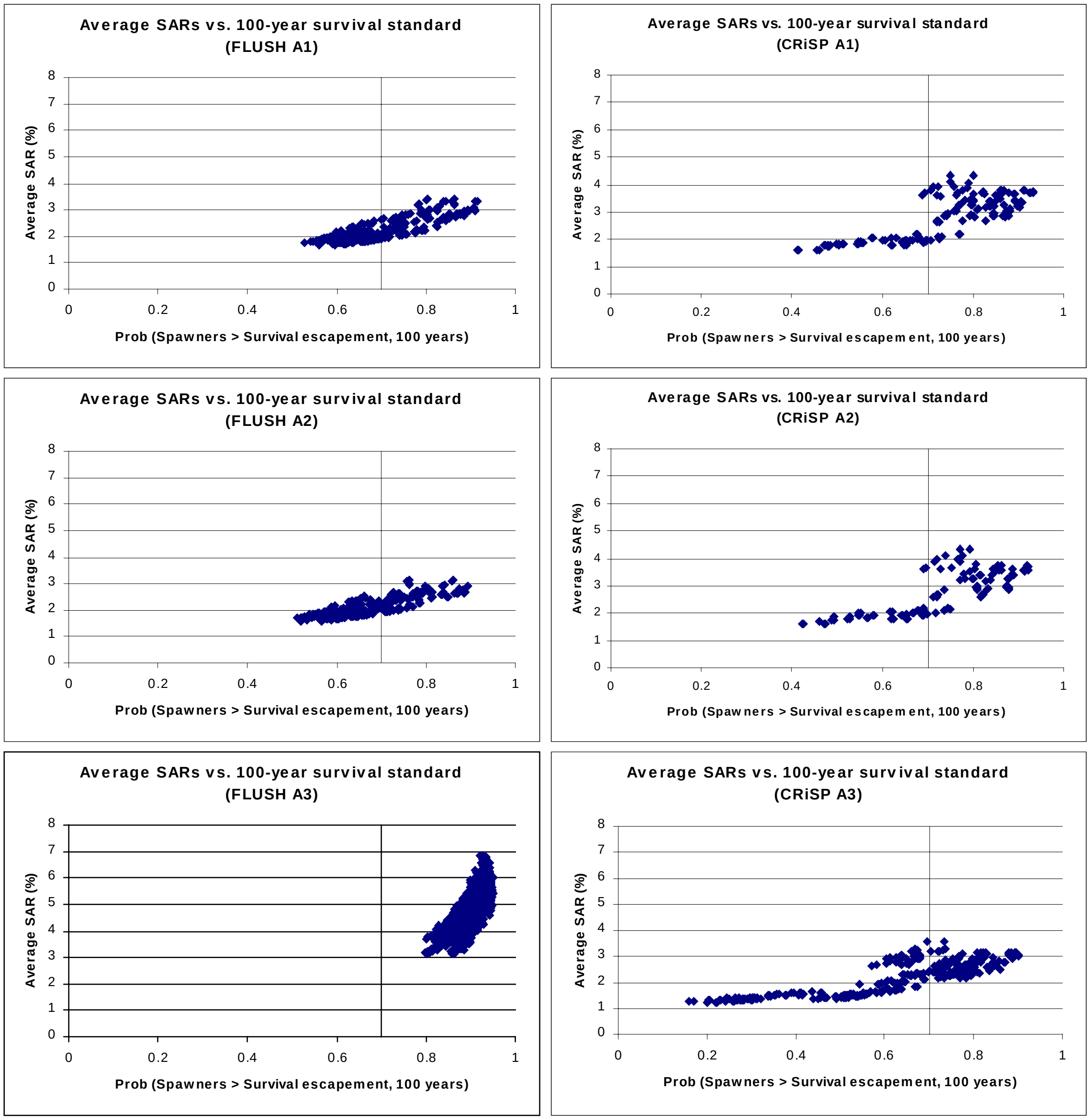


Figure B.5-2. SARs as a function of the probability of the spawning abundance of the sixth best stock exceeding the survival escapement level over 100 years. Data are broken out by passage model / transportation assumption, and by action. The vertical line at 0.7 indicates the NMFS standard of 0.7 probability.

Appendix C: Lower Snake River Feasibility Study
Description of Operational Alternatives

C1. Summary Description

A Group - Lower Snake River Drawdown

Alternative A1(Base Case) – This is the base case as it is today. There is Columbia and Snake River flow augmentation as described in the BiOp.

Alternative A2 – This is the future without drawdown condition. It assumes all fish passage is working with the lower Snake and John Day projects not drawdown. Columbia and Snake River flow augmentation would change to a level which will be identified during the study.

Alternative A3 – This is the alternative which shows the Lower Snake projects drawn down to natural river levels. There is no change in flow augmentation from A1.

Alternative A4 – This is the alternative which shows the Lower Snake projects drawn down to natural river levels and no Columbia or Snake River flow augmentation.

Alternative A5 – This is the alternative which shows the Lower Snake projects drawn down to natural river levels and no Snake River flow augmentation.

B Group - John Day Drawdown to Natural River

Alternative B1 – This is the alternative which shows the Lower Snake and John Day projects drawn down to natural river levels. There is no change in flow augmentation from A1. Or, A3 with John Day drawn down to natural river levels.

Alternative B2 – This is the alternative which shows the Lower Snake and John Day projects drawn down to natural river levels. There is no Columbia or Snake River flow augmentation. Or, A4 with John Day drawn down to natural river levels.

C Group - John Day Drawdown to Spillway

Alternative C1 – This is the alternative which shows the Lower Snake projects drawn down to natural river levels and John Day drawn down to the spillway crest. There is no change in flow augmentation from A1. Or, A3 with John Day drawn down to the spillway crest.

Alternative C2 – This is the alternative which shows the Lower Snake projects drawn down to natural river levels and John Day drawn down to the spillway crest. There is no Columbia or Snake River flow augmentation. Or, A4 with John Day drawn down to the spillway crest.

C2 Detailed Description of Alternatives - Comparison of Operating Requirements using Alternative A1 as the Base Case (16/01/98)

Several alternatives are being considered in the Lower Snake River Feasibility Study ranging from current operations to natural river level drawdown on the lower Snake River and John Day pool with varying amounts of flow augmentation. This paper summarizes the specific operating requirements of the base case alternative, lists the other alternatives and identifies how the requirements for these other alternatives differ in comparison to the base case. For the other alternatives, only requirements that change from the base case are noted.

Alternative A1: – This alternative represents how the Federal system is currently operated under the 1995 Biological Opinions (BO) issued by the National Marine Fisheries Service (NMFS) and the U.S. Fish and Wildlife Service (FWS). It is the base case or "no-action alternative" for this study.

Alternative A1 includes the following operating requirements and provisions (which will be used as the categories of comparison for other alternatives):

Flood Control – Current Upper Rule Curves using forecasted and observed runoff and incorporating a shift of system flood control from Dworshak and Brownlee to Grand Coulee. In addition, the current flood allocation between Mica and Arrow reservoirs.

Initialization of Storage Projects – all projects start the regulation full except for Mica (at July target), Grand Coulee (at 1280 feet), Brownlee (at 2067 feet), Libby (at 2439 feet), Dworshak (at 1520 feet), John Day (at 262.5 feet) and Corra Linn (at 1743.32 feet). Generally, these elevations represent the BO summer draft limits.

Canadian Project Operations – Mica, Duncan and Arrow are operated to the 1997 Assured Operating Plan (AOP) with changes agreed to through the 1997 Detailed Operating Plan (DOP). Arrow will store up to 1 MAF in years when The Dalles volume forecast is less than 90 MAF. The stored water is released from April 16 through June.

Libby Operation – Project is operated September through December to achieve 2411 feet end of December elevation, on minimum flow or for flood control through mid-April, for sturgeon mid-April through July, and up to full powerhouse outflow and to elevation 2439 feet through August in support of McNary flow targets.

Hungry Horse Operation – Project is operated September through December to specific end-of-month elevations (3515 feet by December), on or above Biological Rule Curves through March, on or near flood control through June, and to 3540 feet through August in support of McNary flow targets.

Albeni Falls Operation – Project is operated at 2060 feet in September, at 2055 feet October through April, at 2057 feet in May, and full (2062.5 feet) June through August.

Grand Coulee Operation – Project is operated September through December for power generation, on or above Biological Rule Curves January through mid-April, to the lower of flood control or 1280 to support McNary flow targets through June, and to 1280 feet through August in support of McNary flow targets.

Dworshak Operation – Project is operated on minimum flow or to flood control all months except April through August when it drafts to 1520 feet in support of Lower Granite flow targets.

Upper Snake Operation (including Brownlee) – Flow augmentation of 427 KAF is provided from the upper Snake River. Brownlee is operated to flood control and a maximum of 9 kcfs mid-October through November, operated at 2070 and 2060 feet by end of December and January, respectively, on flood control February through April, operated to 2069 feet in May, attempts to refill in June, drafted to 2069 feet in July, drafted to 2050 and 2048 feet in August 1 and August 2, and operated to 2050 and 2048 feet in September and October.

Lower Snake Project Operations – Projects are operated at Minimum Operating Pool (MOP) April 10 through August for Little Goose, Lower Monumental, and Ice Harbor and through November 15 for Lower Granite.

John Day Pool Elevation – Project is operated at 262.5 feet from mid-April through September.

Peak Efficiency – All lower Columbia and lower Snake projects are operated within 1% of their peak efficiency March through November.

McNary Flow Target – A sliding scale monthly or period flow target from 220 kcfs to 260 kcfs applies from April 20 through June based on January-July runoff forecasts, and a target of 200 kcfs applies in July and August.

Lower Granite Flow Target – A sliding scale monthly or period flow target from 85 kcfs to 100 kcfs applies from April 10 through June 20 based on April-July runoff forecasts, and a sliding scale target of 50 kcfs to 55 kcfs applies from June 21 through August.

Spill – All eight lower river projects provide fish spill during the spring period and non-collector projects provide spill during the summer period. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville, The Dalles and Ice Harbor spill 24 hours a day, remaining projects spill 12 hours per day.

Alternative A2: – This alternative represents Federal system operation without drawdown on the lower Snake River or John Day reservoir, relies on fish transportation as the primary method for fish passage and assumes the current level of development of fish facilities. This alternative eliminates fish spill at fish transportation projects.

All requirements remain the same as in Alternative A1 except for spill.

Spill – Four of the eight lower river projects provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles, John Day and Ice Harbor. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville, The Dalles and Ice Harbor spill 24 hours a day, John Day spills 12 hours per day.

Alternative A3: – This alternative represents Federal system operation with the four lower Snake River projects drawn down to natural river levels on a permanent basis. Flow augmentation, spill, and other requirements remain the same as that provided under the BO and Alternative A1.

Grand Coulee Operation – Project is operated September through December for power generation but is not drafted to below 1280 feet, 1280 feet, 1275 feet and 1265 feet, respectively by month, on or above Biological Rule Curves January through mid-April, to the lower of flood control or 1280 to support McNary flow targets through June, and to 1280 feet through August in support of McNary flow targets.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

Peak Efficiency – Lower Columbia River projects are operated within 1% of their peak efficiency March through November.

Spill – Lower Columbia River projects provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles, John Day and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, John Day and McNary spills 12 hours per day. McNary does not spill during the summer period.

Alternative A5: – This alternative represents Federal system operation with the four lower Snake River projects drawn down to natural river levels on a permanent basis. Flow augmentation at BO levels on the Snake River is eliminated. Flow augmentation on the Columbia River, spill, and other requirements remain the same as that provided under the BO and Alternative A1.

Flood Control – The shift of system flood control from Dworshak and Brownlee to Grand Coulee is eliminated.

Initialization of Storage Projects – Dworshak is initialized at 1600 feet.

Grand Coulee Operation – Project is operated September through December for power generation but is not drafted to below 1280 feet, 1280 feet, 1275 feet and 1265 feet, respectively by month, on or above Biological Rule Curves January through mid-April, to the lower of flood control or 1280 to support McNary flow targets through June, and to 1280 feet through August in support of McNary flow targets.

Dworshak Operation – Project is operated for power generation and flood control from September through May and operated no lower than 1555 feet from June through August. In October, outflow is limited to inflow plus 1300 cfs.

Upper Snake Operation (including Brownlee) – Flow augmentation of 427 KAF from the upper Snake River is eliminated. Brownlee is operated to flood control and a maximum of 9 kcfs October through November per the Idaho Power Company Fall Chinook Plan. The project is operated for power generation and flood control for the rest of the year.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

Peak Efficiency – Lower Columbia River projects are operated within 1% of their peak efficiency March through November.

Lower Granite Flow Target – The sliding scale monthly or period flow augmentation target is eliminated.

Spill – Lower Columbia River projects provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles, John Day and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, John Day and McNary spills 12 hours per day. McNary does not spill during the summer period.

Alternative A6: – This alternative represents Federal system operation in the future without drawdown but reflect operational changes brought about by installation of various fish passage improvements such surface bypass collectors, gas abatement facilities, etc. Flow augmentation is sized to match with the available facilities and appropriate fish passage routes.

[Operating requirements have not yet been determined for this alternative]

Alternative B1: – This alternative represents Federal system operation with the four lower Snake River projects and John Day reservoir drawn down to natural river levels on a permanent basis. Flow augmentation, spill, and other requirements remain the same as that provided under the BO and Alternative A1. This alternative is identical to Alternative A3 except for the addition of John Day drawdown

Flood Control – Flood control remains unchanged because it is assumed that some structure is constructed which would allow filling of the reservoir to the flood control volume currently provided by the reservoir.

Grand Coulee Operation – Project is operated September through December for power generation but is not drafted to below 1280 feet, 1280 feet, 1275 feet and 1265 feet, respectively by month, on or above Biological Rule Curves January through mid-April, to the lower of flood control or 1280 to support McNary flow targets through June, and to 1280 feet through August in support of McNary flow targets.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

John Day Pool Elevation - Project is operated at natural river level year round. Generation is eliminated.

Peak Efficiency - Lower Columbia River projects minus John Day are operated within 1% of their peak efficiency March through November.

Spill - Lower Columbia River projects minus John Day provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, McNary spills 12 hours per day. McNary does not spill during the summer period.

Alternative B2: – This alternative represents Federal system operation with the four lower Snake River projects and John Day reservoir drawn down to natural river levels on a permanent basis. Flow augmentation at BO levels from both the Snake River and Columbia River is eliminated. This alternative is identical to Alternative A5 except for the addition of John Day drawdown and elimination of flow augmentation from the Columbia River.

Flood Control – The shift of system flood control from Dworshak and Brownlee to Grand Coulee is eliminated.

Initialization of Storage Projects – Grand Coulee, Libby and Dworshak are initialized at full pool levels.

Grand Coulee Operation – Project is operated for power generation. The minimum pool elevation for May is 1240 feet. The minimum pool elevation for June through July is 1285 feet. The overall minimum pool elevation year round is 1220 feet to allow for Gifford-Inchelium ferry operation.

Dworshak Operation – Project is operated for power generation and flood control from September through May and operated no lower than 1555 feet from June through August. In October, outflow is limited to inflow plus 1300 cfs.

Upper Snake Operation (including Brownlee) – Flow augmentation of 427 KAF from the upper Snake River is eliminated. Brownlee is operated to flood control and a maximum of 9 kcfs October through November per the Idaho Power Company Fall Chinook Plan. The project is operated for power generation and flood control for the rest of the year.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

John Day Project Operations – Project is operated at natural river level year round. Generation is eliminated.

Peak Efficiency – Lower Columbia River projects, except John Day, are operated within 1% of their peak efficiency March through November.

McNary and Lower Granite Flow Targets – The sliding scale monthly or period flow augmentation targets on both Snake River and Columbia River are eliminated.

Spill – Lower Columbia River projects, except John Day, provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, McNary spills 12 hours per day. McNary does not spill during the summer period.

Alternative C1: – This alternative represents Federal system operation with the four lower Snake River projects drawn down to natural river levels and John Day reservoir to near spillway crest on a permanent basis. Flow augmentation, spill, and other requirements remain the same as that provided under the BO and Alternative A1. This alternative is identical to Alternative A3 except for the addition of John Day drawdown. It is identical to Alternative B1 except for the change in the level of drawdown for John Day reservoir.

Flood Control – Flood control remains unchanged with John Day operated as high as 232 feet to provide current levels of flood control space.

Grand Coulee Operation - Project is operated September through December for power generation but is not drafted to below 1280 feet, 1280 feet, 1275 feet and 1265 feet, respectively by month, on or above Biological Rule Curves January through mid-April, to the lower of flood control or 1280 to support McNary flow targets through June, and to 1280 feet through August in support of McNary flow targets.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

John Day Project Operations – Project is operated from 215 to 220 feet year round. Generation occurs at reduced levels according the lower head.

Peak Efficiency – Lower Columbia River projects including John Day are operated within 1% of their peak efficiency March through November.

Spill – Lower Columbia River projects provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles, John Day and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, John Day and McNary spills 12 hours per day. McNary does not spill during the summer period. John Day's spill percentage and TDG cap remains the same as in Alternative A1.

Alternative C2: – This alternative represents Federal system operation with the four lower Snake River projects drawn down to natural river levels and John Day reservoir to near spillway crest on a permanent basis. Flow augmentation, spill, and other requirements remain the same as that provided under the BO and Alternative A1. This alternative is identical to Alternative A5 except for the addition of John Day drawdown and elimination of flow augmentation from the Columbia River. It is identical to Alternative B2 except for the change in the level of drawdown for John Day reservoir.

Flood Control – The shift of system flood control from Dworshak and Brownlee to Grand Coulee is eliminated. John Day is operated as high as 232 feet to provide current levels of flood control space.

Initialization of Storage Projects – Grand Coulee, Libby and Dworshak are initialized at full pool levels.

Grand Coulee Operation – Project is operated for power generation. The minimum pool elevation for May is 1240 feet. The minimum pool elevation for June through July is 1285 feet. The overall minimum pool elevation year round is 1220 feet to allow for Gifford-Inchelium ferry operation.

Dworshak Operation – Project is operated for power generation and flood control from September through May and operated no lower than 1555 feet from June through August. In October, outflow is limited to inflow plus 1300 cfs.

Upper Snake Operation (including Brownlee) – Flow augmentation of 427 KAF from the upper Snake River is eliminated. Brownlee is operated to flood control and a maximum of 9 kcfs October through November per the Idaho Power Company Fall Chinook Plan. The project is operated for power generation and flood control for the rest of the year.

Lower Snake Project Operations – Projects are operated at natural river levels year round. Generation is eliminated.

John Day Project Operations – Project is operated from 215 to 220 feet year round. Generation occurs at reduced levels according the lower head.

Peak Efficiency – Lower Columbia River projects including John Day are operated within 1% of their peak efficiency March through November.

McNary and Lower Granite Flow Targets – The sliding scale monthly or period flow augmentation targets on both Snake River and Columbia River are eliminated.

Spill – Lower Columbia River projects provide fish spill during the spring and summer period. Specifically, spill at BO levels are provided at Bonneville, The Dalles, John Day and McNary. The level of spill is expressed as a percentage of total flow up to a maximum total dissolved gas cap. Bonneville and The Dalles spill 24 hours a day, John Day and McNary spills 12 hours per day. McNary does not spill during the summer period. John Day's spill percentage and TDG cap remains the same as in Alternative A1.

Appendix D: Summary of Spring/Summer Chinook "Jeopardy Standard"

The following text describes the jeopardy standard used in the NMFS 1995 Biological Opinion on Snake River spring-summer chinook. Variations from this approach in the current PATH analysis are minor, but are indicated below in *italics*.

D1. Survival Standard

a. Set threshold levels for each population. BRWG (Biological Requirements Work Group) estimates were used by NMFS for the following stocks:

Population	Number of Spawners Annually
Bear Valley / Elk	300
Imnaha	300
Marsh Creek	150
Minam River	150
Poverty Flats	300
Sulphur Creek	150

Recently, Johnson Creek run reconstructions were completed, and PATH participants agreed to the following threshold.

Johnson Creek	150

b. Using simulation models, project population levels over 24 years into the future and 100 years into the future.

c. Determine likelihood that each population will be above its threshold level over each of the two time periods. This is determined from the cumulative distribution of all simulations encompassing the time period. For example, if 500 simulations each projected population levels for a 100-year period, the resulting distribution would consist of 50,000 values.

d. Express probability in Step c. as a proportion of probability of being above threshold during a historical period in which stocks were believed to be relatively healthy. NMFS did not define this historical period, but accepted model results based on all available years prior to 1976 for the Biological Opinion. Estimation of the historical probability follows the same process described in Step c., except the simulation model is calibrated only to observations during the historical period.

PATH participants agreed to leave out Step d, for three reasons. First, a ratio of probabilities can be misleading. If both the historical survival standard and the future survival standard have a probability of 0.1, then the ratio will come out to 1.0, which gives a misleading impression that the stock is in a good condition under the particular scenario simulated. Second, the ratio measure was conceived during a time when there were two life-cycle models in use, ELCM and SLCM. A ratio reduced differences between these two models. However, all PATH analyses have been conducted using one life-cycle modeling framework, BSM, which uses a consistent set of *assumptions. Finally, the prospective analyses consider many possible combinations of conditions, as outlined in Figure 4.1-1 and Table 4.1-1. A ratio of survival probabilities would therefore need to define historical scenarios for each of the over 5,000 combinations in Figure 4.1-1, and match these with the appropriate perspective aggregate hypothesis.*

e. NMFS' jeopardy standard is that a "high percentage" of available populations must have a "high likelihood", relative to the historical probability, of being above the threshold level over each time period. NMFS defined "high percentage" as 80% of available populations. The level of 80% does not neatly transfer into a specific number of stocks in the case of the Snake River, where there are seven index stocks. If five of the seven index stocks meet the jeopardy standard, that constitutes 71%; if six of the seven index stocks meet the standard that constitutes 86% of the stocks. *PATH, therefore, applied the standard that six of the seven stocks should meet the NMFS jeopardy standard. That is, we present the probabilities for the sixth best stock.*

NMFS did not define "high likelihood". *PATH has assumed that 70% be considered an approximation of this standard. Some PATH members have suggested reporting results for a range of probabilities between 60%-95%. Results in Section 5 show the actual frequency distribution of probabilities across all alternative aggregate hypotheses, so that one can easily assess the fraction of cases in which the sixth best stock exceeds higher (or lower) probabilities.*

D2. Recovery Standard

a. Set recovery population level. Relevant population recovery goal in NMFS' "Proposed Recovery Plan" is eight-year geometric mean of annual redd counts equivalent to 60% of the pre-1971 brood-year average redd counts.

Although the NMFS draft recovery level is expressed as redd counts, analyses for the biological opinion converted these to estimates of number of spawners (Table D-2). Values used in previous analyses have changed for some stocks as the run reconstruction procedure has been refined.

Table D-2: Recovery levels used for stocks.

Stock	Recovery Threshold (# spawners)
Imnaha	850
Minam	450
Bear Valley	900
Marsh Creek	450
Sulphur Creek	300
Poverty Flat	850
Johnson Creek	300

b. Using simulation models, project population levels 48 years into the future.

This PATH analysis has also looked at population projections 24 years into the future.

c. Determine likelihood that the eight-year geometric mean of each population will be above its recovery level in the 48th year of a simulation (i.e., geometric mean of years 41-48). This is determined from the cumulative distribution of all simulations. *(This analysis also looked at the probability of recovery based on the geometric mean of years 17 to 24.)*

   For example, if 500 simulations each project an eight-year geometric mean population level for the 48th year of the simulation, the resulting distribution would consist of 500 values.

d. Express probability in Step c. as a proportion of probability of being above threshold during a historical period in which stocks were believed to be relatively healthy. NMFS did not define this historical period, but accepted model results based on all available years prior to 1976 for the Biological Opinion. Estimation of the historical probability follows the same process described in Step c., except the simulation model is calibrated only to observations during the historical period. *Step d was not done for the Results in Section 5, for the reasons given under Section D1, part d.*

e. NMFS' jeopardy standard is that a “high percentage” of available populations must have a “moderate to high likelihood” of being above the recovery level within 48 years.

   NMFS defined “high percentage” as 80% of available populations.

NMFS did not define “moderate to high likelihood”. *PATH has assumed that 50% be considered an approximation of this standard. Actual probabilities are listed in the Results section of this report, so that one can assess the effect of raising or lowering the standard. To assess 80% of available populations, PATH used the sixth highest stock, as outlined above in Section D1, part e.*

D3. Considerations for the Simulations

NMFS concluded that survival/recovery probabilities based on simulation models that include depensatory effects are more reasonable than those from models lacking such effects. However, NMFS did not comment on the proper method of implementing depensation in life-cycle models.

PATH implemented a depensatory function in all simulations, calibrated to existing data. See Deriso (1997) for a description of the function and supporting rationale.

References

Deriso, R.B. 1997. Prospective Analysis of Spring Chinook of the Snake River Basin. 33 pp. In PATH Package #1 for the Scientific Review Panel. June 3, 1997.