Plan for Analyzing and Testing Hypotheses (PATH)

First Scientific Review Panel Report

L. Barnthouse (editor)
J. Collie
B. Dennis
S. Saila
C. Walters

Prepared for

Bonneville Power Administration
911 NE 11th
P.O. Box 3621, PGIA
Portland, OR 97208 USA

Prepared by
ChemRisk Division, McLaren/Hart Environmental Engineering Co.
109D Jefferson Ave.
Oak Ridge, Tennessee 37830 USA

May 24, 1996

Table of Contents

1.0 Introduction

This report presents comments by an expert Scientific Review Panel on a draft report on Retrospective Analyses produced by the Plan for Testing and Analyzing Hypotheses (PATH). The PATH group consists of modelers and others who work with the Bonneville Power Administration, the Northwest Power Planning Council, the National Marine Fisheries Service, and various state and tribal resource agencies. The PATH Retrospective Analyses report documents progress on a number of technical tasks related to using historical data to evaluate alternate hypotheses concerning the causes of the recent decline in production of wild salmon stocks in the upper Columbia River basin and the likely effectiveness of the management actions that have been proposed to restore these depleted stocks. The review is intended as a mid-course evaluation, to provide direction to the technical working groups, not a final judgement of the value of the PATH activity.

1.1 Background

For the past several years, the Bonneville Power Administration, the Northwest Power Planning Council, the National Marine Fisheries Service, and various state and tribal resource agencies have been attempting to work together to compare and enhance the models used by all of the agencies to evaluate management options intended to enhance recovery of depleted Columbia River Basin salmon stocks. A number of reports comparing the behaviors of mainstem passage and life-cycle models over a wide range of management scenarios and natural climate conditions have been prepared. These products, together with comments from several external review panels, have helped to clarify the nature of differences among the models and have pointed the way towards helping to resolve them.

Results of these comparisons and reviews have demonstrated that each modeling system has different strengths and weaknesses. Where differences in results exist among the modeling systems, the primary cause has been differences in basic hypotheses and assumptions regarding the impact of recent and potential management actions. A 1994 scientific review panel report concluded that there were three major differences between the modeling systems: 1) the distribution of survival over the life span; 2) the effect of flow on survival, and 3) the benefit of transportation. The panel felt that as long as these differences exist the models were going to give different answers in a fairly predictable fashion. The panel concluded that, rather than continuing with model comparison activities, the modeling groups should attempt to resolve the fundamental issues through hypothesis formulation and testing.

The 1994 review panel report was the stimulus for the development of the PATH project. During the Spring and Summer of 1995 a planning committee identified the following as specific objectives of PATH:

define the management decisions that serve to focus analytical activities;

bound the anadromous salmon ecosystem components that need to be considered;

lay out alternative hypotheses for the functioning of these ecosystem components, in terms of the distribution of survival over the populations' life cycle and the life stages and population responses to management actions under different natural conditions;

compile and analyze information to assess the level of support for alternative hypotheses;

propose other hypotheses and/or model improvements supported by the weight of evidence from these analyses;

identify knowledge and data gaps that could be filled through management experiments, research and monitoring, improving our ability to discriminate among competing hypotheses, and maximizing the rate of learning and clarity of decisions;

provide guidance to the development of regional programs that would stabilize, ensure persistence, and eventually restore depressed salmon stocks to self-sustaining scenarios; and

provide a structure for an adaptive learning approach to development and implementation of a regional salmon recovery program (i.e., iterative evaluation of results of research, monitoring, and adaptive management experiments; assessment of implications for alternative hypotheses and subsequent actions)

A preliminary hypothesis framework was developed, consisting of three nested "levels" of hypotheses (Box 1). At the first PATH workshop, held in October, 1995, this framework was refined and developed into a specific set of hypotheses and tasks. Some of the tasks involve quantitative modeling; others involve qualitative synthesis and evaluation of data. Working groups were organized to address each of the tasks.

The PATH three-level hypothesis framework

Level 1: exploratory analyses to determine if there are differences in trends of abundance and productivity indicators among different Pacific northwest species and stocks. Hypotheses at this level seek to identify differences in trends among species/stocks, but do not propose mechanisms to explain those differences.

Level 2: explanation of trends in stock indicators in terms of spatial contrasts and temporal changes in a) survival during particular life history stages; or b) pressure/stressor indicators associated with survival in one or more life history stages. Hypotheses at this level provide potential inferences concerning life stages on which management actions should be focused.

Level 3: explanation of life-stage-specific mechanisms associated with observed population trends. Level 3 hypotheses link directly to key management decisions.

In March of 1996, a draft "Retrospective Analyses" report presenting preliminary results for each task was prepared. This report was sent for independent review to a Scientific Review Panel consisting of four eminent population biologists with experience in quantitative analysis of fisheries data and applications of modeling in resource management:

- Dr. Jeremy Collie, Graduate School of Oceanography, University of Rhode Island
- Dr. Brian Dennis, Department of Fish and Wildlife, University of Idaho
- Dr. Saul Saila, Graduate School of Oceanography, University of Rhode Island (emeritus)
- Dr. Carl Walters, School of Natural Resources, University of British Columbia

Comments from all four reviewers were received and distributed to the PATH participants for discussion at the second PATH workshop in April, 1996.

1.2 Structure of This Report

This report presents results of the review. Sections 2 and 3 contain, respectively, a summary of the general comments of the reviewers on the Retrospective Analyses report as a whole, and a summary of the reviewers' comments on each of the 12 chapters that have been drafted so far.. The complete text of each review is included as Appendices A-D.

2.0 Summary of General Comments

The reviewers generally endorsed the approach adopted for the PATH project. Two reviewers were especially complimentary:

> "I sat down to review the various chapters of this report expecting a boring rehash of past data and analyses. In the end I found the report fascinating and well worth reading, and I commend the authors on their efforts." (Walters)

> "Overall, I find much to admire in the PATH process and accomplishments. The idea of bringing together many leading scientific players in population biology of Columbia Basin salmonids, and carrying out a process of hypothesis formulation and testing, is a model for other agencies." (Dennis)

Several overall strategic suggestions were, however, made.

Dr. Walters suggested that the scope of the PATH analyses should be broadened to include steelhead as well as chinook salmon and to include stocks outside the Columbia River Basin (e.g., the Fraser River Basin chinook stocks). He provided suggestions for sources of data. He also argued that insufficient attention has been paid to the ocean fishery, in terms of both potential magnitude of impacts and potential future management actions [the PATH Program Plan includes evaluation of hypotheses related to impacts of exploitation, but implementation somewhat behind other tasks]. He further suggested that a better compilation of data relating to historic impacts of habitat degradation is needed.

Dr. Walters and Dr. Dennis both pointed out some lack of coordination among several of the chapters dealing with Level 2 hypotheses, especially chapters 3, 4, 5, and 9. Standardization of terminology and notation for equations were suggested as ways of improving the clarity of the report. Dr. Dennis suggested developing a chapter that would document the major data sets being used in all of these chapters.

Dr. Saila thought the scope of the review was too narrow, and identified what he believed to be several important subject areas that have been inadequately considered to date:

- the nature and levels of physiological changes associated with travel delays due to impoundments,
- the effects of hatchery stocks on the genetic diversity of indigenous stocks,
- minimization of the adverse effects of hatchery introductions,
- the rate of alteration of habitat due to anthropogenic effects, and
- the resource potential of the altered Columbia River system habitat for existing species and stocks (presumably, as compared to the system's potential prior to completion of the hydrosystem).

Dr. Collie suggested that each chapter should end with a discussion of whether the analyses presented support the hypothesis being investigated [such discussions will be included in the final Retrospective Analysis report].

A final recommendation, provided by Dr. Dennis but endorsed by all of the other reviewers as well, was to complete all of the analyses as rigorously and quickly as possible to publish them in the open scientific literature. Publication was seen as providing the greatest possible visibility and credibility to this very important work.

3.0 Chapter-Specific Comments

Chapter 2: The Snake River in the Context of Broad Scale Patterns of Climate Change in Stock Indicators [C. Paulsen]

All four reviewers found this approach to be potentially valuable.

Dr. Walters suggested investigating a larger number of stocks and evaluating additional indicators of stock condition. He raised questions concerning the statistical methods being employed, noting that spurious correlations can easily be introduced due to the methods used to assign catches to stocks. He also suggested that relatively simple clustering methods (e.g., Nei diagrams) would be sufficient to identify relationships among the stocks.

Dr. Dennis called for a clearer explanation of (1) data structure employed in the cluster analyses, and (2) all aspects of the correlation analyses. He expressed confidence, however, that if completed in a thorough manner the chapter would yield interesting and useful information. His opinion was that the "factual content" provided by these analyses (i.e., statistically valid patterns in stock characteristics across time and space) could rule out some of the proposed hypotheses.

This chapter presents a pilot study of different oaches to testing PATH hypothesis L1.1:

There has been a similar trend in the state ators for anadromous salmonid species/stocks that n in a variety of geographic locations in the Pacific ıwest.

mber of multivariate analytical methods are used to ify geographic patterns in historic spawner-recruit for upriver and downriver salmon stocks. minary results of correlation and cluster analyses resented. The authors intend to explore a rminant analysis approach in later versions of the '. If all stocks were to show the same pattern in pement, recruitment, or other important stock icteristics, then some common environmental factor, likely related to ocean conditions or broad-scale atic events, would be indicated as a major factor in ecline of Snake River basin stocks. If, to the ary, there are systematic differences between basins ıong substocks within the major basins, then basin-fic or stock-specific causes are more likely to be ciated with the observed historical patterns.

Dr. Saila proposed a number of alternatives to the statistical methods used in this chapter. He suggested that many of the assumptions required by the regression method used in the trends analysis (ordinary least-squares regression) are violated by the data being used, and therefore that a non-parametric trend testing procedure should be used. He also suggested some alternative clustering methods. His argument was not that the clustering method used in chapter 2 is invalid, but that there is no *a priori* reason for choosing one approach over another and therefore that several alternative approaches should be examined. Dr. Saila also suggested several approaches to discriminant analysis that might be employed [discriminant analysis, although planned for the future, was not included in the chapter].

Dr. Collie, like Dr. Dennis, noted that cluster and correlation analyses do not directly test hypotheses in a statistical sense. Instead, these analyses produce patterns that can be used as evidence for or against alternative hypotheses. Like Dr. Walters, Dr. Collie cautioned against potential spurious correlations that could be introduced by the methods used in the run reconstruction process. He suggested that if this is, in fact, a problem that its effects could be minimized by aggregating spawning stocks by subbasin. He found the detail provided concerning the clustering methods to be inadequate.

3. *Contrasts in Stock Recruitment Patterns of Snake and Columbia River Spring/Summer Chinook Populations* **[H. A. Schaller, C. E. Petrosky, and O. P. Langness]**

All four reviewers accorded a high degree of importance to the analyses described in this chapter, although different reviewers drew different conclusions from them. A planned Analysis of Variance exercise, which would be included in a later version of the Chapter, was recommended against by several reviewers in favor of a more in-depth explanation and evaluation of the current analyses.

Dr. Walters interpreted the results as showing clear evidence for a decrease in survival in the Snake River stocks during the post-dam period. Some of the results might also be interpreted as showing a high-degree of density-dependence within the Snake River stocks, however, Dr. Walters suggested that this pattern might be a result of biased estimates of recruitment. Another possibility raised in the review was that the Snake River stocks for which long time-series of data are available are an unrepresentative sample of the stocks that were present before hydropower development; they may have survived only because they exhibit more compensatory capacity than typical Spring chinook stocks. Dr. Walters suggested that differences in survival during ocean rearing might explain some of the observed patterns; maps of the ocean distribution of the various stocks might be developed and included with a map of spawning locations.

Dr. Dennis argued that chapter 3 makes a very strong case for an influence of dams on Spring chinook stocks in the Snake River basin. He suggested that more complete definitions of the quantities being studied (i.e., recruitment and spawning stock size) should be provided. He recommended that the analysis should be completed and published in the open scientific literature as quickly as possible.

Dr. Saila suggested a number of alternative methods of analyzing the stock-recruitment data. Ordinary least-squares regression of the linearized Ricker model was used by the authors; Dr. Saila recommended that a non-linear estimation procedure applied to the untransformed model

This chapter focuses on examination of time trends in stock indicators for upriver and downriver/coastal stocks, before and after hydropower development. The analysis is relevant to hypotheses at all three PATH levels. Contrasts in stock indicators between upper and lower river stocks, before and after hydropower development, are used to infer the impacts of hydropower development on upriver stocks. If no changes in relative recruitment patterns (relative to downriver stocks) occurred during the post-dam vs. Pre-dam periods, the implication would be that hydrosystem development had a relatively small influence on the Snake River Basin stocks (and therefore changes in the system would have a relatively small benefit); if large differences exist, the implication would be that hydrosystem development had a large influence and, consequently, changes in the system might have relatively large benefits.

should be used for comparative purposes. He also recommended (1) using a non-parametric trend analysis procedure to test for statistical differences in time trends between upriver and downriver stocks and (2) performing formal slope-comparison tests of the pre-1970 vs. Post-1974 recruit-per-stock data

Dr. Collie found the approach adopted for Chapter 3 to be very useful for detecting patterns in chinook salmon productivity and attributing those patterns to hydroelectric development, but he raised a number of methodological concerns. As in his review of Chapter 2, he questioned whether spurious correlations introduced by the run reconstruction process could be influencing the results. He also suggested some clarifications in terminology, especially with the terms "replacement level" and "stock productivity." He found figures 2-5 to be confusing and inadequate for demonstrating the failure of the Snake River stocks to achieve replacement levels.

He interpreted the results presented in figures 7 to 19 as showing that there has been a reduction in available habitat for most of the Snake River stocks during the post-1970 period. For a few stocks, he inferred that productivity, rather than habitat, had been reduced. He suggested that these hypotheses should be more fully explored using the general linear model (GLM) technique.
Dr. Collie noted that chinook stocks throughout the Columbia River basin, but especially in the Snake River basin, achieved unusually high recruitment per stock during the early 1980s. He suggested three alternative explanatory hypotheses:

(1) density-dependence; per capita survival was high because of low escapements;

(2) Ocean survival was high;

(3) juvenile survival of Snake River stocks was especially high because of favorable juvenile rearing conditions.

He noted that the data presented in Chapter 3 provides partial support for all three hypotheses.

4. Stressors and Life-History Stages Correlated with Patterns of Change in Stock Indicators: A Pilot Demonstration of a Multivariate Analysis Approach **[C. Paulsen]**

This chapter describes an analysis of the influence of "environmental stressor" variables on stock indicators for stocks in the Middle Fork Salmon River and John Day River basins. The environmental variables employed are those thought to be influential on salmon survival in different life stages. The idea is to use multiple regression and other multivariate techniques to identify which variables, and therefore which life-stages, have contributed the greatest variability to the variation in stock and recruitment indicators for the different stocks. This was strictly a pilot analysis, intended to illustrate the method. The intent in later versions is to modify the analytical techniques and apply them to a larger number of stocks.

Dr. Walters found the objectives and overall approach to this Chapter to be good, but raised a number of technical issues concerning methodology. He cautioned that many of the presumed "independent" observations are correlated, and therefore the true number of degrees of freedom is less than had been assumed in the pilot analysis. Unless these correlations are accounted for, estimates of the significance of the results would be artificially inflated. He recommended presentation of the correlation matrix between independent variables, with a statistical review of the effects on regression error of including/excluding various independent variables. Dr. Walters found the preliminary results in Table 4.7 "unpromising," in the sense that relatively few significant effects were found and some of those were contradictory with each other or with results presented in Chapter 5.

Dr. Dennis found Chapter 4 to be "confusing." He requested a more complete description of the data, particularly the estimates of stock and recruitment, and a more explicit explanation of the relationship of the proposed environmental variables to the salmon life cycle.

Dr. Saila recommended that a different analytical approach be used. The method employed in Chapter 4, which involved expressing six of the environmental variables as binary dummy variables, in his opinion results in a substantial loss of information. He recommended two alternative forms of nonlinear analysis: the Abductive Information Modeler and neural-network analysis.

Dr. Collie thought that the multiple regression approach was "logical," but raised some concerns about its application in Chapter 4. He recommended making *a priori* predictions concerning how the stressors being examined should affect recruitment. His opinion is that correlation among the independent variables probably is not a serious problem for this analysis, but that it would increase the difficulty of identifying causes for observed effects. Like Dr. Saila, Dr. Collie objected to conversion of independent variables to binary dummy variables. He was confused by equation 2 and thought that there might be an error in notation. Of the several different methods used to define dependent variables, Dr. Collie questioned the validity of the multiple-age-class method. His opinion is that this approach introduces lack-of-independence among the observations, resulting in an overestimate of the number of degrees of freedom. As in his review of Chapter 3, he recommended use of the GLM approach. He found the preliminary regression results to be inconsistent, except for the effects of (1) spawners, (2) dams, and (3) ocean upwellings. He questioned the value of including a large number of additional environmental stressors in the analysis.

5. Retrospective Analysis of Passage Mortality of Spring Chinook of the Columbia River **[R. Deriso, D. Marmorek, I. Parnell]**

This chapter presents draft final results of retrospective modeling conducted to assess the overall effects of mainstem passage down the Columbia River. In contrast to the linear regression methods used in Chapters 2-4, Chapter 5 employs a Maximum Likelihood Estimator It examines the difference in incremental mortality between upriver (Snake River system) and downriver (Lower and Mid Columbia) stocks, using spawner and recruit data from 11 rivers. The resulting estimates of in-river mortality provide an independent check on results obtained from the commonly-used downstream passage models. The results will provide an additional perspective on the influence of flow and transportation on instream survival and therefore on optimal instream management regimes

.Dr. Walters noted that, despite differences in notation, the underlying stock-recruitment model used in Chapter 5 is the same Ricker model used in chapters 3 and 4. He recommended standardizing the notation used in all of these chapters. He made a number of technical suggestions concerning the “first-order bias correction” step, and suggested that estimates of effects of time and mortality effects may be confounded. He suggested that the correlation structure for the time effect should be included and discussed.

An especially noteworthy result, according to Dr. Walters, is that the 1971 brood year, previously thought to have exhibited anomalously low survival throughout the Snake River basin, does not appear as an outlier in the Chapter 5 analysis. He recommended that, instead of examining the value of alternative stock-recruitment model forms for prospective analysis, the authors should develop better models for shared or local environmental effects and for sources of variation in time effects.

Dr. Dennis recommended immediate completion and publication of Chapter 5. He expressed confusion concerning the notation and the nature of the data being used, and suggested developing a spreadsheet that provides the actual numbers used for the various quantities (with column headings that correspond to the equation symbols). He also recommended using a model selection index to (specifically, the Schwartz information criterion or the consistent Akaike information criterion) sort out the various model structures. He disagreed “on scientific principles” with using Bayesian approaches for prospective analysis, and attached a draft manuscript detailing his position.

Dr. Saila wondered, since Chapter 5 was characterized as a “draft final paper,” how the analysis could accommodate new inferences drawn from possibly significant changes in earlier chapters. He raised four questions concerning Chapter 5, which he believed were not sufficiently answered in the current draft:

(1) Have correlations and dependencies among input parameters and outputs been adequately treated I the Monte Carlo simulations?

(2) Are input parameter distributions available?

(3) Is the mathematical structure of the model realistic?

(4) Has input parameter uncertainty been adequately recognized and propagated throughout the calculations?

He suggested that uncertainties due to model structure (as distinct from parameter uncertainties) might be investigated using “fuzzy arithmetic.”

Dr. Collie noted that the method used in Chapter 5 is an extension of the GLM approach he had recommended for the earlier chapters. He endorsed the chapter’s emphasis on passage mortality, with other environmental factors being subsumed in year effects and error terms. He found the two-level parameterization of passage mortality to be “confusing to the naive reader.” Like Dr. Walters, Dr. Collie questioned the bias correction approach. He also suggested that the model may be overparameterized, given the large umber of parameters being estimated. He suggested performing a simplified analysis in which measurement errors are ignored.

Dr. Collie found the results to be quite interesting, particularly the very low estimates of passage mortality from 1980-83 (Figure 1). He noted that this apparently anomalously high survival may, in fact, not be related to passage mortality at all but may instead be due to some other feature unique to the Snake River basin. He also noted that peaks in the year effects appear to correspond to El Niño years and that the year effects in general seem to mirror the decadal pattern of variability in the northeast Pacific.

6. A decision Tree for the Columbia River Hydrosystem, and a Proposed Approach to Synthesizing Evidence Relevant to these Decisions **[E. Weber, A. Giorgi, C. Toole, C. McConnaha]**

This chapter presents a prototype decision tree for evaluating management options for the Columbia River federal power system. It was developed by individuals from the three agencies with plans for rebuilding Columbia River salmon stocks: the National Marine Fisheries Service, the Northwest Power Planning Council, and the Columbia River Inter-Tribal Fish Commission. A three-tiered approach was employed involving first, a decision tree, second, a table providing evidence for and against each decision choice, and third, a narrative that develops the evidence in the table. The decision tree identifies a logic stream for making hydropower decisions and assures the management questions addressed later are those of interest to decision makers.

Dr. Walters questioned the hierarchical structure of the decision tree on the grounds that it may miss "mixed action" options (e.g., small increase in transportation + reduced ocean harvest) that could be more cost-effective than any set of actions involving only one dimension of the system (e.g., only hydropower operations). He suggested that the "arguments for and against" tables should be simplified and should include only arguments for which empirical data or credible model results can be cited. He also recommended (1) examination of data from other river systems (e.g., the Fraser River) for possible evidence supporting or refuting the arguments, (2) avoidance of either/or arguments when there are many possibilities, and (3) summarizing major sources of uncertainty that are common to many arguments.

Dr. Dennis declined to comment on Chapter 6, on the grounds that it is outside his area of expertise.

Dr. Saila endorsed the decision tree/decision alternatives table approach as being a rational approach to decisionmaking for complex problems. He suggested that quantitative as well as qualitative decision analysis methods could be employed. He suggested three quantitative approaches and provided references for each:

- Bayesian decision analysis or Bayesian belief networks
- Dempster-Schafer theory of evidence, and
- Fuzzy decision making.

Dr. Collie also expressed strong support for the decision framework as a logical way of structuring the evidence on juvenile salmon survival. He stated, however, that many of the viewpoints expressed in the current version of the framework are incompletely developed and employ excessive abbreviations and jargon. He suggested revision of the first two boxes in Figure 6-1 to isolate hydropower effects on survival from other sources of variability.

He also suggested paying particular attention to the drawdown sections of the decision tree, given the apparently small potential benefit to be gained from incremental changes in transportation or dam operations.

7. Quantitative Exploration of Alternative Hydrosystem Hypotheses **[P. Wilson, A. Giorgi, R. W. Zabel]**

The goal of this Level 3 analysis is to explore the data on smolt travel time as a function of river flow (or water travel time) and to examine the implications and evidence for equations relating fish travel time in the different passage models. The objectives include cataloguing existing data sets regarding smolt travel time in the Columbia Basin, explaining the development of the fish travel time relationships in CRiSP and FLUSH, and comparing the sensitivity of the travel time relationships in the two passage models to hydrological factors and to uncertainty in the parameters defining them.

Dr. Walters did not comment on Chapter 7.

Dr. Dennis noted that the chapter presents "straightforward regression analyses of fish travel time as predicted by water travel time," but identified no problems and had no specific comments.

Dr. Saila noted, in discussing the analysis of travel time from Lower Granite to McNary Dam, that the coefficient of determination is low enough that little confidence should be placed in the results. He also suggested that an assessment of which parts of reservoirs impose major migration delays would be helpful.

Dr. Collie noted that no hypotheses are explicitly mentioned in the chapter, and inferred that the alternative hypotheses are contained in the two models of smolt travel times (CRiSP and FLUSH). He noted that there is extensive data on migration speed, but only one NMFS study providing a direct linkage between travel speed and survival.

He questioned whether there is a hydrodynamic basis for the exponential relationship between fish travel time and water travel time in FLUSH, and noted that the travel-time aspects of CRiSP cannot be understood from the explanation provided. Both models fit the observed travel times fairly well in the examples provided. Dr. Collie recommended that, until there is evidence linking travel time to survival, further development of travel-time models should be accorded a low priority.

8. Sensitivity Analysis for Mainstem Passage Survival **[P. Wilson, L. Basham, L. Garrett, A. Giorgi, R. W. Zabel, and E. Weber]**

Dr. Walters recommended development of information on the cumulative pattern of descaling during downstream passage. If descaling rates in returning adults were also examined, the relative survival rates of descaled juveniles could be assessed.

He noted that the CRiSP model explains "surprisingly little" of the variation in survival under average conditions, and suggested that this may be due to lack-of-independence between different sources of mortality. Fish more likely to die ant any one dam probably also have higher probability of dying at all dams. If this is true, then detailed modeling of dam passage, under the assumption of independence of risks, will have little value.

The goal of this Level 3 task is to examine the sensitivity of estimates of smolt survival to representations of uncertainty in data relating to descaling, turbine mortality, spill effectiveness, nitrogen mortality, and collection efficiency. This task involves deriving and agreeing upon plausible ranges for parameters representing these factors, and producing and summarizing outputs of survivals from CRiSP and FLUSH. If the relative influence of uncertainties in these different parameters could be identified, then priorities for reducing uncertainty through monitoring and experimentation could be developed.

Analysis of cumulative descaling pattern downstream is needed

Examine descaling rates in returning adults; would permit assessment of relative survival rate of descaled fish.

CRiSP explains surprisingly little of the variation in survival under average conditions

Mortality risks are not independent. Fish more likely to die at one dam probably also have a higher mortality risk at all dams. Implies that detailed modeling of dam passage, assuming independence, has little value. Moreover, there may be a habitat/climate/dam interaction effect, so that in years where rearing conditions are poor, increasing flows won't improve survival, and when rearing conditions are good, fish are health and survival is high even without flow. If such interactions exist, then management options involving increased spills during low-flow years will not increase survival to the expected extent.

Dr. Dennis found Chapter 8 to be "very long and unfocused," but provided no specific comments.

Dr. Saila noted that the results presented in the chapter appear to show that descaling and resultant mortality are substantially higher for hatchery fish than for wild fish.

Dr. Collie found Chapter 8 to be very difficult to review because of its length and characterized it as "poorly written." He found that the chapter contains useful information but recommended that it be condensed and extensively rewritten. He made a few specific comments on the various subsections included in the chapter:

Although very long, the section on descaling does not attempt to relate descaling to subsequent survival. The data in Tables 1-4 should be plotted a means of illustrating patterns.

Most of the spill efficiency results are consistent with a 1:1 relationship between the passage fish and the passage of water through spillways

In the discussion of CRiSP model parameters for nitrogen mortality, Figure 2 does not seem to follow from Table 8.5.

In the CRiSP sensitivity analysis, it is not clear how the model was calibrated to all the independent variables. The sensitivity analysis could be useful for suggesting relationships between environmental variables and survival. The figure references no not seem correct, and the flow-survival curve (Figure 1) appears linear rather than concave up.

9. Evaluation of Survival Trends in the Freshwater Spawning and Rearing Life Stage for Snake River Spring/Summer Chinook **[C. E. Petrosky and H. A. Schaller]**

This chapter examines evidence for, and tests whether, a net decrease in survival in freshwater spawning and rearing life stage has occurred since completion of the hydropower system. Such a change could partially explain the decline of Snake River spring/summer chinook. Numbers of wild spring/summer chinook spawners and smolts were indexed at the uppermost dam from available data sets for brood years 1962-73, 1962-82, and 1990-1993. A strong density-dependent relationship in smolt survival was found. Smolt survival has been much higher in recent years than in earlier years when spawner abundance was higher. The results provide no support for the hypothesis that spawner-to-smolt survival is the primary life stage responsible for the decline of Snake River spring/summer chinook.

Dr. Walters found this chapter to have “broad implications.” He assigned completion of the analysis a “top priority.” He found that it “has major implications for habitat management planning” and recommended that the method be applied to other stocks, especially steelhead.

Especially noteworthy, in Dr. Walters’ opinion, is the fact that the relationship between recruits per spawner and spawning stock size, appears not to have changed with time. He noted an apparent contradiction between Chapters 8 and 9. According to Chapter 8, low numbers of smolts reached the dam system during the low-flow years 1973 and 197. However, the analysis in Chapter 9 shows no such pattern. He recommended checking the data for any flow-dependent bias that could result in inflated smolt abundance estimates during low-flow years.

Additional observations from Dr. Walters include:

Outplanted hatchery fry should be included as a covariate in the Ricker model (as a density-dependent term, like S itself). Estimates of wild smolt abundance in recent years could be biased, if hatchery smolts are being incorrectly classified as wild.

Recruit-per-spawner observations for the 1990s vs. The 1970s may be confounded with changes in assessment methods. If this is the case, statistical significance tests should not be performed.

The analysis in Chapter 9 provides no evidence for depensation at any spatial scale.

Results suggest a strong compensatory increase in egg-smolt survival at low stock sizes. This result should be checked against historical data on fry/juvenile rearing densities in specific streams.

The results may not be applicable to Snake River as a whole. Stocks with low compensatory capability may already have been eliminated.

Dr. Dennis praised the analysis as a "great chapter" and called it a "cornerstone scientific publication." He had no specific comments.

Dr. Saila suggested the use of non-parametric, two-sample tests for comparisons that are currently treated with t tests.

Dr. Collie called the chapter "well-planned, executed, and written." He found it to provide conclusive evidence that survival during freshwater spawning and rearing has not decreased in the stocks examined. In fact, there has been a strong, density-dependent increase in survival. He suggested performing a formal Analysis of Covariance test, but characterized this recommendation as a "technicality" that would not be expected to change the conclusions.

He noted that these results apparently refute a conclusion he himself drew from Chapter 3, that loss of habitat might account for the change in form of the stock-recruitment relationship documented in that chapter. He also observed that Chapter 9 provides no evidence for depensatory survival during the spawning and rearing stage. If depensation is occurring, as is suggested by some of the plots in Chapter 5, it must be explained by changes in survival during some other life stage.

10. A Decision Tree for Structured Synthesis of Evidence Concerning Changes in Spawning and Rearing Habitat **[C. Petrosky]**

This chapter presents a draft decision tree and management questions for spawning habitat effects analogous to the hydropower decision tree presented in Chapter 6.

According to Dr. Walters, a decision tree for habitat change may not be needed. Evidence from previous chapters, especially Chapter 9, suggests that survival rates during spawning and rearing are already high and are not being widely influenced by habitat factors.

His opinion is that Section 10.2 should not focus on documenting change in spawning/rearing habitat. Instead, it should instead assemble evidence that could be used to compare population performance in areas with differing degrees and types of habitat modification. He called the Strength/Weakness evaluation "waste of time" and recommended that an objective comparison of populations subjected to differing degrees of habitat modification be performed.

He recommended that habitat management efforts be focused on developing access for fish to quality habitats from which they are now excluded by natural or man-made barriers.

Dr. Dennis did not comment on Chapter 10.

Dr. Saila repeated his recommendation, also made for Chapter 6, that formal decision analysis techniques be applied to habitat management decision-making. He suggested that a workshop organized and guided by a leader in decision analysis theory and procedures would provide a means of focusing this activity.

Dr. Collie thought that the decision tree in Chapter 10 raised some fundamental questions:

- Should habitat protection measures be implemented?
- Could survival be increased by improvements to spawning/rearing habitats?
- Could these improvements compensate for decreased survival during other stages?

Some questions posed in Chapter 9 appeared to him as "rhetorical" given the results presented in Chapter 9. Questions 5 and 9, which deal with the contribution of localized habitat degradation to extirpation of some geographically isolated stocks, appeared to him to be the most pertinent. He thought that it should be possible to identify the watersheds where protection and restoration will be most effective.

Dr. Collie thought that, although it is unlikely that current survival in the spawning and rearing life stage could be increased by habitat protection measures, such measures may be necessary in the future to accommodate increased numbers of spawners that result from successful implementation of recovery plans.

11. Hypotheses Regarding Hatchery Impacts **[P. Wilson]**

Dr. Walters noted that, although the hatchery working group had been quite thorough about listing concerns relating to hatchery impacts on wild salmon, none of the listed concerns are new. In view of the difficulty encountered in attempting to answer these questions, he recommended performing experiments to assess direct, competitive interaction effects and effects related to stimulation of fishing effort and harvest mortality. He also recommended application of formal decision analysis to the problem designing experiments to test hatchery impact hypotheses. In his view, such an analysis would probably demonstrate that some potential hatchery management experiments (and, by implication, some current hatchery management practices) are unacceptably risky.

The goals of this task are to develop hypotheses concerning the effects of hatcheries on Columbia River salmon and steelhead stocks, and to synthesize information regarding the hypotheses for use in testing them. The chapter is a preliminary attempt to describe and place in context a set of management questions and hypotheses. Questions about artificial propagation are listed and categorized. Hypotheses concerning the effects of hatchery production on wild stocks are then listed.

Additional specific comments from Dr. Walters included:

- Organize hypotheses into (1) questions about within-stock consequences of rearing under hatchery conditions, versus (2) questions about between-stock consequences of competition, etc.
- In question 1.5, the issue is not whether hatcheries affect genetic “variability,” but rather whether they affect the processes of adaptation to local circumstances.
- More attention should be devoted to the potential impacts on wild populations of direct removals of wild fish to seed hatcheries.
- It is incorrect to assert that if annual harvest effort is held constant, then hatchery fish may buffer wild fish against harvesting,
- In the discussion, don’t wast time linking each hypothesis to management questions. These links are obvious. Proceed directly to a summary of evidence for and against the hypotheses.
- Summarize available data on each hatchery stock in tabular form.
- In section 11.4, computer models are not an analytical approach to hypothesis testing. Approach 2, comparison of wild population performance among areas affected differently by hatchery practices, is much more promising.

Perform population performance analyses analogous to those performed in Chapters 2-4 for wild stocks should be performed for hatchery stocks.

Dr. Dennis found the hypotheses in this chapter to be “sharp and clear,” but thought that the proposed methods for testing them are incomplete and will yield only preliminary information.

Dr. Saila felt that his comments on Chapters 6 and 10 (i.e., a formal decision analysis would be highly beneficial) were equally applicable to Chapter 11.

Dr. Collie found this chapter difficult to review objectively because of his own bias against siting hatcheries on rivers with wild populations. He could think of no additional hypotheses to add to the list. Like Dr. Walters, he thought that the hypotheses regarding beneficial impacts of hatcheries on fishing mortality in wild populations had been stated incorrectly. The availability of hatchery fish can decrease fishing mortality on wild populations only if the fishery is managed to achieve an annual harvest quota. Again like Dr. Walters, Dr. Collie recommended comparing performance indices (e.g., recruits per spawner) between stocks with and without hatchery influences.

12. Influence of Climate on Fish **[J. J. Anderson]**

> This chapter summarizes recent studies concerning the influence of decadal-scale climatic/ocean fluctuations on environmental variability and salmon production throughout the Pacific Northwest. Results of studies correlating fish production and climatic fluctuations are discussed together with proposed mechanisms for explaining these influences. A regional shift indicator (the Pacific Northwest Index) is proposed that is a composite of both marine and continental climatic conditions.

Dr. Walters thought that Chapter 12 provides a good summary of the available literature. He thought that the apparent differences in effects on stocks inhabiting downwelling vs. upwelling domains was especially important for design of future studies. He cautioned that some of the published studies employ harvest data rather than recruit-per-spawner data and therefore may confound climate influences with stock size influences. He also suggested that, for future experimental designs, treatment-reference comparisons should be defined on the smallest practical spatial scales, because of potential differences in responses to marine conditions by fish spawned in different geographic areas.

Dr. Dennis found the chapter “fascinating” but provided no specific comments.

Dr. Saila described the chapter as “interesting and important,” and suggested placing more emphasis on the role of mammalian predators, which have increased in abundance due to increased protection.