Plan for Analyzing and Testing Hypotheses (PATH)

Results of the Kah-Nee-Ta Workshop on Retrospective and Prospective Analyses (April 17-19, 1996)

Bonneville Power Administration

Plan for Analyzing and Testing Hypotheses (PATH)

Results of the Kah-Nee-Ta Workshop on Retrospective and Prospective Analyses (April 17-19, 1996)

Prepared for

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

Prepared by

David R. Marmorek, Daniel R. Bouillon, and Ian P. Parnell
ESSA Technologies Ltd.
Suite 300, 1765 West 8th Avenue
Vancouver, BC V6J 5C6

May 22, 1996

Citation: **Marmorek, D.R., D.R. Bouillon, and I. Parnell.** 1996. Plan for Analyzing and Testing Hypotheses (PATH): Results of the Kah-Nee-Ta Workshop on Retrospective and Prospective Analyses, (April 17-19, 1996). Prepared by ESSA Technologies Ltd., Vancouver, BC for Bonneville Power Administration, Portland, OR, 52 pp. and appendices.

Table of Contents

List of Tables

Tables

List of Figures

Figure

PATH Workshop 2
May 22, 1996

Abstract

The PATH group (Plan for Analyzing and Testing Hypotheses) held a three-day workshop from April 17-19, 1996 at Kah-Nee-Ta resort near Warm Springs, Oregon. Participants at the workshop reviewed the progress made to date on retrospective analyses of key hypotheses, and developed a work plan for completing these analyses. They also reviewed the goals for prospective analyses, brainstormed ideas on how to achieve these goals, and developed a work plan to guide PATH prospective analyses over the next four months. Decision analysis was seen as a potentially very useful integrative framework for evaluating both the effects of alternative sets of management actions, and the relative rates of learning expected from alternative choices. The group also developed a preliminary set of research recommendations, based on the outcome of retrospective analyses. The report summarizes all workshop discussions and proposed work plans. Appendices are included that provide more detailed comments on individual chapters of the Preliminary Report on Retrospective Analyses.

Acknowledgements

The authors thank all the PATH participants for their patience, hard work and cooperative spirit during the workshop. All the participants enjoyed the beautiful surroundings and delicious food of Kah-Nee-Ta, which contributed to the productivity of workshop discussions.

1.0 Introduction

The goals and objectives of the Plan for Analyzing and Testing Hypotheses (PATH) have been described in numerous previous reports (Marmorek et al. 1996, Marmorek et al. 1995). The goals for the second PATH workshop were to:

1. develop a workplan for completing the remaining retrospective analyses; and
2. develop a workplan for prospective analyses.

Table 1.1 summarizes the process used to achieve these goals at the workshop; a detailed agenda for the workshop is included in Table 1.2. Chapters 1, 2, and 4 of this report summarize workshop discussions; a number of interesting ideas are in material distributed at the workshop and subsequently (see Appendices). Chapters 3 and 5 summarize the Retrospective and Prospective Work Plan tasks and associated labor allocations, as of May 17th, 1996. These are continually evolving.

Table 1.1: Goals of the second PATH workshop, and methods used to achieve them **.**

Goals	Methods
1. Develop a work plan for remaining retrospective analyses	C Short, strategic overviews of chapters C List technical issues to be discussed at later meetings C Decide potential of each analysis to clarify management decisions C Determine remaining tasks
2 Develop work plan for prospective analyses.	C Review goals for prospective analyses C Brainstorm ideas on how to achieve these C Structure into general work plan C Provide input to research planning

The workshop began with a review of what has been achieved to date. The Preliminary Report on Retrospective Analyses (Marmorek et al. 1996) presented both pilot results and draft final results for a number of different tasks established at the October 1995 workshop, loosely organized around the PATH hypothesis framework. (Table 1.3 provides a quick summary of the intent of PATH's three-level hypothesis framework.). Work included a substantial amount of progress on Level 1 and Level 2 analyses:

- C pilot correlation and cluster analyses of chinook time series;
- C summary of the relationship between stock indicators and climate;
- C analyses of spatial and temporal trends in ln(R/S) for upstream and downstream stocks;
- C pilot multivariate analyses to explain the stressors correlated with patterns of change in stock indicators;
- C a maximum likelihood estimation (MLE) approach to estimating stock productivity (Ricker curve parameters), mortality due to downstream passage through dams, and year effects due

to changes in climate; and

- analyses of the implications of changes in freshwater spawning and rearing as reflected in smolt to spawner ratios.

Table 1.2: Agenda for second PATH workshop **(April 17-19, 1996).**

1. Strategic Review of Retrospective Analyses (Wednesday, April 17th):

Workshop Goal: Develop a work plan for remaining retrospective analyses. Decide potential of various retrospective analyses to clarify management decisions, and their fate.

8:30am Structure for Workshop; Overview of Progress: what we planned to do; what we got done; how much time we have to finish (David Marmorek)

8:45 General responses of reviewers (Larry Barnthouse)

Level 1 and 2 Analyses

9:00 Goals of levels 1 and 2; review aggregate hypotheses for explaining historical changes (Marmorek)

9:15 Short (10 min.) updates from lead authors, incorporating responses to reviewers. Structure:
a) what does each retrospective analysis provide to help reduce current uncertainties in key management decisions? (e.g. useful decision making / hypothesis structure for clarification of key questions; useful analytical approach for answering these questions)
b) limitations of existing data and how to overcome them (if possible);
c) key linkages to other PATH retrospective analyses;
d) proposed next steps for retrospective analysis (given target finishing date of mid-June for retrospective analyses)

{There will not be time to go through detailed presentations / discussions of recent analyses, though material may be handed out for evening reading and subgroup discussions. Focus is on a ***strategic*** *overview; technical meetings will happen in May. Running list of tasks maintained.}*

920 **Chapter 2** *The Snake River in the Context of Broad Scale Patterns of Change in Stock Indicators: A Level 1 Pilot Analysis*

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

9:50 **Chapter 9** *Evaluation of Survival Trends in the Freshwater Spawning and Rearing Life Stage for Snake River Spring/Summer Chinook* [Charlie Petrosky]

10:05 **Chapter 3** *Contrasts in Stock Recruitment Patterns of Snake and Columbia River Spring/Summer Chinook Populations: Draft Pilot Study* [Howard Schaller]

10:20 **BREAK**

10:35 **Chapter 5** *Retrospective Analysis of Passage Mortality of Spring Chinook of the Columbia River* [Rick Deriso]

11:00 **Chapter 12** *Influence of Climate on Fish* **-** *Review* [Jim Anderson]

11:15 **Conclusions on Level 1 and 2 Analyses (joint discussion):**
a) Does this set of level 1 and 2 retrospective analyses adequately address management decisions and original hypotheses, or do we require other retrospective analyses?
b) Given people available and reviews of progress to date, what is fate of each type of analysis? Abandon, postpone, write-up as is, complete as planned, revise plan {consider linkages among

chapters}.

c) Form task groups and develop general timetable {to be refined after Level 3 discussion}.

12noon **LUNCH**

1-2 pm Conclusions on Level 1 and 2 Analyses (joint discussion) cont'd

Level 3 Analyses

2 pm **Integrative frameworks for Level 3 Analyses**: proposed overall decision tree [Chris Toole] {*An extension to Figure 6-1 to include all H's, ideally blessed by CPT before workshop}*

2:10 **Chapter 6** *A Decision Tree for the Columbia River Hydrosystem, and A Proposed Approach To Synthesizing Evidence Relevant to these Decisions* [Toole]

2:25 **Chapter 7** *Quantitative Exploration of Alternative Hydrosystem Hypotheses* [P. Wilson/A. Giorgi]

2:40 **Chapter 8** *Sensitivity Analyses for Mainstem Passage Survival* [P. WILSON; Al Giorgi; Jim Anderson; and/or Paul Weber **-** need to sort out who will present]

2:55 **Chapter 10** *A Decision Tree for Structured Syntheses of Evidence Concerning Changes in Spawning and Rearing Habitat* [Petrosky]

3:10 **BREAK**

3:25 **Chapter 11** *Hypotheses Regarding Hatchery Impacts* [Wilson]

3:40 **Chapter 13** *Hypotheses Regarding Harvest Impacts* [Cooney]

3:50 **Conclusions on Level 3 Analyses (joint discussion)**

a) Does this set of level 3 retrospective analyses adequately address management decisions and original hypotheses, or do we require other retrospective analyses?
b) Given people available and reviews of progress to date, what is fate of each type of analysis? Abandon, postpone, write-up as is, complete as planned, revise plan {consider linkages among chapters}.
c) Finalize task groups and general timetable.

{Complete assignment / priorization of retrospective tasks will not be possible, since it will also depend on prospective tasks, but we can at least get a sense for group opinions on priority}

5:00 **SUPPER BREAK**

7:00 Conclusions discussion cont'd. Integration of Levels 1, 2 and 3 tasks

9:00. End of Session **-** Collapse

2. Prospective Analyses - Thursday (April 18th) and Friday morning (April 19th)

Workshop Goal: Develop work plan for prospective analyses. Review goals for prospective analyses; brainstorm ideas on how to achieve these: structure into general work plan; provide input to research planning.

Possible Goals for Prospective Analysis:

a) Estimate the improvement in life cycle survival required to reach various salmon objectives (survival, recovery, rebuilding), and the uncertainty associated with this estimate.
b) Develop alternative, prospective, aggregate hypotheses about how to achieve survival goals,

building on the analyses of retrospective aggregate hypotheses.

c) Assess the quantitative improvement in survival that is possible through various combinations of changes in 4H's, climate, and the risks to stocks under different management approaches and climate regimes.
d) Assess the ability to distinguish among competing aggregate and life stage specific hypotheses from future information.
e) Advise various institutions on research, monitoring and adaptive management experiments which would maximize the rate of learning and clarify decisions.

Plenary Session on Thursday, April 18th (8:30-10:00 a.m.)

8:30am Summarize goals for prospective analyses. Present example aggregate hypotheses for stabilizing and recovering stocks. *{Marmorek}*

8:40 Use of decision analyses to structure prospective analyses (Randall Peterman)

9:00 Some thoughts on potential uses of the MLE / Bayesian framework and existing models for prospective analyses of alternative aggregate hypotheses (Rick Deriso)

9:15 Discussion

10:00 Charge to subgroups *{Marmorek}* ***Charge!***

1015 **BREAK**

1030 **Subgroup Meetings**

A - hydrosystem, ocean, harvest; *{Peterman and Botsford here; Marmorek facilitating}*

B - hydrosystem, hatchery, habitat *{Deriso in this group; Barnthouse facilitating}*

Step 1) **Develop approaches to goal *a)* -** estimating the improvement in life cycle survival required for achieving different levels of likelihood of survival, recovery, and rebuilding.

Step 2) **Develop approaches to goal *c) and b)* -** How to assess the quantitative improvement in survival that is possible due to actions within each H and due to changes in climate (both ocean and continental), and the risks to stocks under different management approaches and climate regimes. Develop alternative aggregate hypotheses for stabilization/recovery that involve all 4 H's. Explore how to mesh existing tools (e.g. passage models, other quantitative tools or evidence related to harvest, habitat, hatcheries) with Bayesian framework so as to quantitatively assess changes in stock status (e.g. modify prospective projections of passage models by some measure of the 'bias' apparent in passage model estimates of *m* relative to MLE estimates.)

Step 3) **Develop approaches to goal *d) and e)* -** What future research and monitoring activities, adaptive management experiments are required to improve retrospective/prospective analyses, distinguish among competing hypotheses and clarify management decisions? What specifically has emerged from the retrospective analyses? How do these suggestions agree with / differ from current research plans *{distribute summary of research as reference material for participants}*? Roughly how long would it take to get clarification on alternative decision paths? What general experimental strategies might work? How could one quantitatively assess the rate of learning expected from these activities and experiments?

Step 4) **Solidify 1-4 into a set of work tasks.** *{Facilitators synthesize across groups in evening, and very rapidly between 10 a.m. and 10:30 a.m.}*

12 noon **LUNCH**

5:00 **SUPPER** *{informal meetings in evening}*

Friday, April 19th

8:30am **Subgroup Meetings** cont'd

10:15 **BREAK**

1030 **Plenary Presentations**

12:30 **LUNCH**

1:30pm **Work Plan Development**

3 p.m. **Adjourn**

Table 1.3: Description of the three types of hypotheses considered in PATH **. These three levels are a continuum: some hypotheses and associated analytical methods are intermediate between Levels 1 and 2; others bridge between Levels 2 and 3.**

Level 1 Hypotheses

- exploratory analyses to explore differences in trends of standardized abundance, productivity, and associated variance in species, stocks and periods relevant to Snake River stocks.
- identify differences in trends among species/stocks, but do not propose mechanisms to explain those differences.
- may suggest L2/L3 hypotheses
- may "screen" hypotheses; limit L2/3 possibilities

> Example H: Snake River spring chinook stocks exhibit a different trend in productivity than other northwest Pacific stocks.

Level 2 Hypotheses

- explain 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.
- do not propose specific mechanisms to explain life stage changes, but **must** provide inferences on where to focus management actions.
- two types of Level 2 hypotheses: 1) life stage composite hypotheses; and 2) life cycle aggregate hypotheses.

> Example H: Changes in survival during juvenile and adult mainstem migration correspond to changes in overall productivity and abundance, while changes in survival during other life stages do not. (Life cycle aggregate hypothesis)

Level 3 Hypotheses

- explain life-stage specific mechanisms associated with observed trends, for each life history stage identified at Level 2 as closely associated with the population trends.
- for Snake River stocks, Level 3 hypotheses link directly to key management decisions.
- focus on the quantitative strength of hypothesized effects.

> Example H: Decrease in water velocity during spring one mechanism to explain trend in juvenile mainstem survival; certain minimum flows required to maintain sufficient mainstem survival.

Progress was also made on various Level 3 tasks proposed at the October workshop, including: development of a hydrosystem decision tree, syntheses of evidence for and against hypotheses integral to that decision tree, and sensitivity analyses of mainstem transit time and fish survival. Some progress was also made in developing hypotheses for habitat and hatchery impacts, though less time was allocated to these tasks. Syntheses of evidence for and against habitat and hatchery hypotheses was not initiated during the October to April period. In addition, there was insufficient time to make progress on development of hypotheses regarding the impacts of harvest.

Figure 1.1 summarizes the progress made to date on various spring/summer chinook hypotheses. The chapters and chapter sections relevant to particular system components are listed in this figure. Notably absent are quantitative analyses related to harvest, upstream passage, and transportation hypotheses, though transportation is addressed in Chapter 6 . In addition, the analyses of habitat and hatcheries in Chapters 10 and 11 (respectively) are still very preliminary. It is recognized by all PATH participants that progress on these different hypotheses is iterative, and therefore the initial analyses completed do help to frame the next steps that need to be considered.

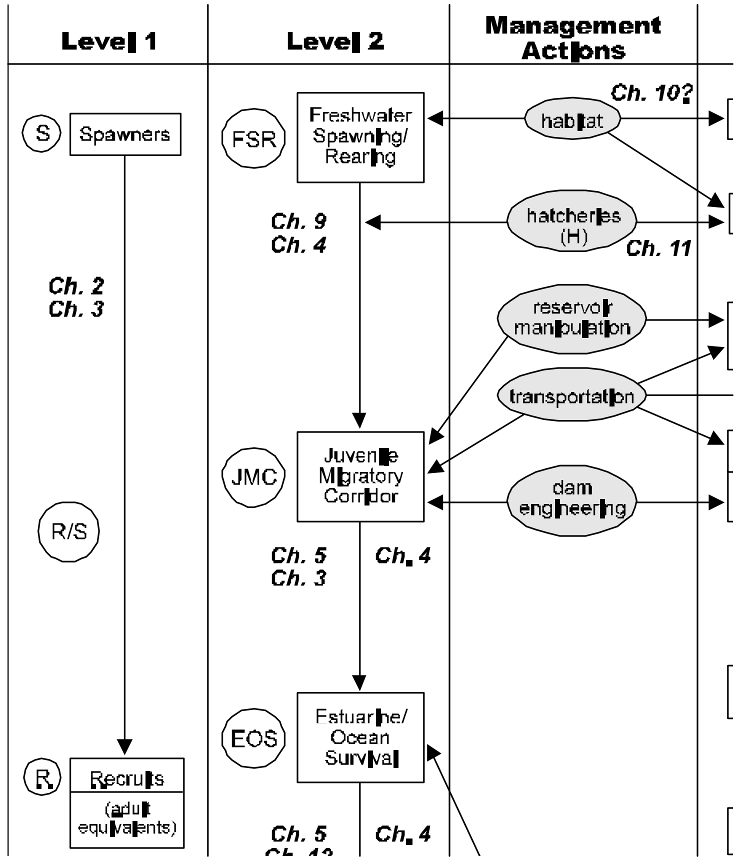


Figure 1.1: Progress made to date on key hypotheses .

A major gap not apparent in Figure 1.1 is the lack of progress on fall chinook. Since harvest is a much more significant component of fall chinook than for the spring/summer stocks, workshop participants agreed that progress on both fall chinook population trends in general, and specifically the impacts of harvest on fall chinook should be a high priority for the next four months' work. This will require additional labor from participating agencies, beyond the current set of PATH participants. Sockeye also has received little attention, but were considered to be lower priority than fall chinook. Phil Munday pointed out that sockeye abundance was very strongly correlated with the loss of access to rearing lakes (Ph.D. thesis by Jeff Fryer, U. Washington), and is probably more sensitive than chinook to the total available habitat, an issued raised by Carl Walters in his review.

Table 1.4 illustrates the intended activities over the next four-month period. Most of the retrospective analyses (with the exception of fall chinook and parts of Chapter 6) are to be completed prior to mid-June, so that many of the PATH participants can focus on prospective analyses. It is recognized that the final report will only include a preliminary set of prospective analyses, though it is hoped that many of the retrospective analyses will be sufficiently far advanced to be published in peer-reviewed literature.

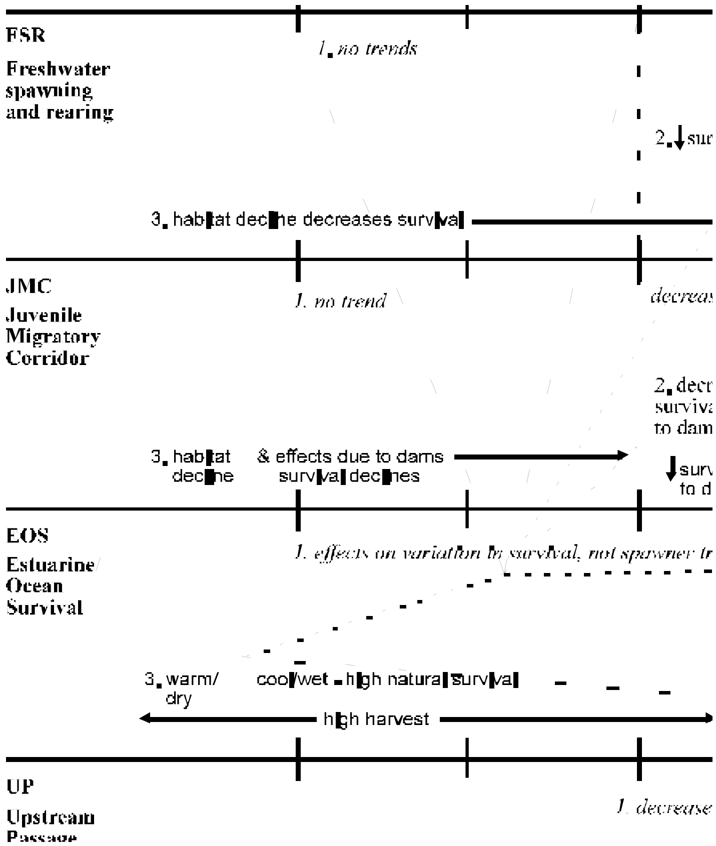
Table 1.4: Next steps **.**

Time	Retrospective Analysis	Prospective Analysis
April Workshop	Develop Completion Plan	Develop Overall Work Plan
Before Mid-June	Do analyses which can be finished by mid-June. 3 draft journal papers + 5 syntheses of evidence	Technical meetings and communications to jump start the prospective analyses (see goals).
Mid-June to Mid-August	Internal Review Fall chinook work, Chapter 6	Complete first set of prospective analyses; Draft report out by Sept. 6/96
September 9-20	Internal and External Peer Review	
Workshop 3 (week of Sept. 24)	Consolidation of results into final report for FY 1995-96	Consolidation of results into final report for FY 1995-96
After September 30, 1996	Next iteration of retrospective analyses.	Next iteration of prospective analyses.

1.1 Summary of Retrospective Aggregate Hypotheses

The aggregate hypotheses serve as an integrative framework for each of the more detailed retrospective analyses. One of the major driving reasons for creating PATH was to make progress on assessing key hypotheses that underlie differences in recommended management actions. Retrospective aggregate hypotheses describe the belief system of what has historically controlled the salmon's life cycle. These hypotheses highlight the relative significance of particular life history stages and associated stressors in determining the historical trends in stock indicators.

Figure 1.2 is an attempt to summarize three alternative aggregate hypotheses to describe the historically observed declines in Snake River chinook. Hypothesis 1 (*in Figure 1.2*) explains most of the recent trends in chinook abundance as a consequence of decreased survival in the juvenile migratory corridor (JMC) stage and upstream passage (UP) stages due to the creation of the Snake River dams. Estuarine and ocean



survival is believed to have affected year-to-year variation in spawner abundance and recruits per spawner, but is not responsible for the overall trends in escapement. Hypothesis 3 attributes the decline in chinook abundance to a number of factors: changes in habitat, the effects of harvest prior to dam construction, the construction of other dams prior to 1970, the effects of dam construction in the 1970s, the effects of spill on survival through increased dissolved gas concentrations in the 1980s, and perhaps most importantly the change in estuarine and ocean survival from a cool/wet period generating high survival to a warm/dry period associated with low/natural survival around 1976 and 1977. Hypothesis 2 is intermediate between Hypotheses 1 and 3; it places more blame on the JMC and UP stage than does hypothesis 3, but also considers estuarine and ocean survival to be the main cause of decline in spawners during the 1980s. Each of these retrospective aggregate hypotheses have different implications for future actions, as described in Chapter 4.

Figure 1.2: Retrospective aggregate hypotheses **.**

An alternative way to consider the retrospective aggregate hypotheses is illustrated in Table 1.5. Here each of the five major forcing factors (hydro, habitat, hatcheries, harvest, and climate) can have low, moderate, or high effects on each of the life history stages (though only habitat and hatcheries are likely to affect FSR). Included in Table 1.5 are examples of some of the data available to evaluate the strength of various components on life cycle or life stage specific estimates of survival. Most of the completed and proposed Level 2 analyses appear on the first row of Table 1.5 describing changes in life cycle survival; the multivariate analyses in Chapter 4 attempt to bridge across all five forcing factors. Any retrospective aggregate hypothesis can be thought of as some combination of low, medium, or high effects on survival from the five components in one or more life history stages. The observed data on spawning and recruitment constrains the number of feasible or credible alternative aggregate hypotheses. The existence of delayed effects, the absence of historical data on life stage specific survivals, and the difficulty of finding index stocks with the appropriate contrast in component effects (to examine changes in R/S) place constraints on how much one can differentiate between alternative retrospective hypotheses.

Table 1.5: Levers on historical / future survival and examples of potential data analyses to quantify effects **. A "—" means that the forcing factor does not affect that life stage.**

Stage	**Forcing Factor**					
	Hydro	**Habitat**	**Hatcheries**	**Harvest**	**Climate**	
Life Cycle Survival	L	L	L	L	L	Chapter 4 Multivariate analysis
	M	M	M	M	M	
	H	H	H	H	H	
	ln(R/S) (Ch 3) Estimated μ (Ch 5)	R/S trends with/ without habitat effects (Ch 10)	R/S trends with/ without hatchery (Ch 11)	Harvest estimates	MLE * (Ch 5)	
FSR	—	L	L	—	—	
		M	M			
		H	H			
		Chapter 9 Smolt: Spawner trends scaled by "a" values				
JMC	L	L	L	—	—	
	M	M	M			
	H	H	H			
	PIT-tag studies NMFS survival estimates (Chs 6,7,8)	PIT-tag studies NMFS survival estimates (Chs 6,7,8)				
EOS	L	L	L	L	L	

	M	M	M	M	M	
	H	H	H	H	H	
	(delayed effects)	(delayed effects)		ocean harvest estimates	CWT data	
UP	L	L	L	L		
	M	M	M	M	?	
	H	H	H	H		
	dam counts	dam counts	dam and rack counts	in-river harvest estimates		

1.2 Review of Preliminary Report on Retrospective Analyses

Larry Barnthouse provided a brief summary of the general impressions of the external reviewers, their impressions of some key results, remaining uncertainties, and a selected summary of recommendations. Larry is currently completing a short summary of the reviewers' comments to be circulated past the PATH Planning Group, the reviewers themselves, and then after revisions to the Implementation Team. In general, the reviewers were very impressed by the work completed to date, as shown in the excerpts contained in Table 1.6. PATH participants also provided some very useful, detailed comments on the Preliminary Report, which are contained in the Appendices.

Table 1.6: Overall impressions of Preliminary Report on Retrospective Analyses from external reviewers **.**

Reviewer	Comment
Carl Walters University of British Columbia	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.
Brian Dennis University of Idaho	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.
Jeremy S. Collie University of Rhode Island	As an outside reviewer, I found this preliminary report on retrospective analyses very informative; by the time I had ploughed through the entire report I knew much more about Columbia River chinook salmon than when I started. I can certainly appreciate the hard work that was required of all the authors to produce the report in the limited time available.

Saul B. Saila University of Rhode Island	It is believed that several important subject areas have not been considered adequately in the development of hypotheses to be tested at the three levels indicated in the PATH preliminary report. Some of these include: 1) hypotheses concerning the nature and levels of physiological changes associated with significant travel delays due to impoundments; 2) hypotheses (models) related to the effects of hatchery stocks on the genetic diversity of indigenous stocks; 3) hypotheses (experiments) related to minimizing the adverse effects of hatchery introductions; 4) hypotheses related to the rate of alteration (degradation) of the existing habitat due to anthropogenic effects; and 5) hypotheses concerning the resource potential of the altered Columbia River system habitat for existing species and stocks with a careful analysis of alternatives.

1.3 Updated List of Participants

In keeping with the fruitful exchange of information among PATH participants, Table 1.7 provides an updated list of participants' addresses, phone and fax numbers and e-mail coordinates.

Table 1.7: List of PATH participants in working groups and workshops **. Attendees at PATH Workshop 2 (Kah-Nee-Ta) indicated with an "*". Please send corrections to David Marmorek.**

Name	Address	Phone # / Fax # / Email Address
Dr. James Anderson*	Columbia Basin Research Puget Sound Plaza 1325 - 4th Avenue, Suite 1820 Seattle, WA 98101-2509	Ph: (206) 543-4772 Fax: (206) 616-7452 or (206) 685-7471 jim@fish.washington.edu
Mr. Dave Askren	Bonneville Power Administration 911 NE 11th P.O. Box 3621, PGIA Portland, OR USA 97208	Ph: (503) 230-5732 Fax: (503) 230-3314 draskren@bpa.gov
Dr. Lawrence Barnthouse*	McLaren Hart Environmental Engineering Chemrisk Division 109 D Jefferson Avenue Oak Ridge, TN USA 37830	Ph: (615) 482-8978 Fax: (615) 576-8543 larry_barnthouse@mclrnhrt.uucp.ne tcom.com
Mr. Ray Beamesderfer*	Oregon Department of Fish &Wildlife P.O. Box 59, 2501 SW First Avenue Third Floor Portland, OR USA 97207	Ph: (503) 872-5252 ext. 5402 Fax: (503) 229-5602 Ray.Beamesderfer@State.or.us
Dr. Lou Botsford*	University of California, Davis Dept. of Wildlife and Fish Conservation Biology Room #1077 Academic Surge Building Davis, CA USA 95616	Ph: (916) 752-6169 Fax: (916) 752-4154 lwbotsford@ucdavis.edu
Mr. Dan Bouillon*	ESSA Technologies Ltd. 300 - 1765 W. 8th Ave. Vancouver, BC V6J 5C6 CANADA	Ph: (604) 733-2996 Fax: (604) 733-4657 dbouillon@essa.com
Mr. Brian Brown*	National Marine Fisheries Service 525 NE Oregon St., 5th Floor Portland, OR USA 97232	Ph: (503) 230-5410 Fax:(503) 231-2318 Brian_Brown@ccgate.ssp.nmfs.gov
Mr. Tom Cooney*	Washington Department of Fisheries Columbia River Fisheries Laboratory 16118 N.E. 219th Street P.O. Box 888 Battle Ground, WA USA 98604	Ph: (360) 576-6073 Fax: (360) 576-6072 tcooney@teleport.com
Dr. Rick Deriso*	2042 De Mayo Rd. Del Mar, CA USA 92014	Ph: (619) 546-7020 Fax: (619) 792-5003 rderiso@ucsd.edu

PATH Workshop 2
May 22, 1996

Dr. Peter Dygert*	NMFS Northwest Regional Office 7600 Sand Point Way NE Bin C15700 Bldg. 1 Seattle, WA USA 98115	Ph: (206) 526-6734 Fax: (206) 526-6736
Mr. Tim Fisher	Beak Consultants 317 SW Alder Street Portland, OR USA	Ph: (503) 248-9507 Fax: (503) 228-3820 beak@teleport.com
Mr. Jim Geiselman*	Bonneville Power Administration 911 NE 11th P.O. Box 3621, PGIA Portland, OR USA 97208	Ph: (503) 230-5732 Fax: (503) 230-4564 jrgeiselman@bpa.gov
Dr. A. Giorgi*	Don Chapman Consultants, Inc. 7981 - 168th Ave. N.E. Redmond, WA USA 98052	Ph: (206) 883-8295 Fax: (206) 869-6387 chapman@nwlink.com
Mr. Josh Hayes	Columbia Basin Research Puget Sound Plaza 1325 - 4th Avenue, Suite 1820 Seattle, WA 98101-2509	Ph. (206) 543-5004 Fax: (206) 616-7452 josh@cqs.washington.edu
Dr. Ray Hilborn	School of Fisheries WH-10 University of Washington Seattle, WA USA 98195	Ph: (206) 543-9026 Fax: (206) 685-7471 rayh@fish.washington.edu
Mr. Rich Hinrichsen	Columbia Basin Research Puget Sound Plaza 1325 - 4th Avenue, Suite 1820 Seattle, WA 98101-2509	Ph. (206) 543-5004 Fax: (206) 616-7452 hinrich@cqs.washington.edu
Mr. Olaf Langness*	State of Washington Department of Fisheries Columbia River Fisheries Laboratory 16118 N.E. 219th Street P.O. Box 888 Battle Ground, WA USA 98604	Ph: (360) 576-6073 Fax: (360) 576-6072 langness@teleport.com
Dr. Danny Lee	US Forest Service Inter-Mountain Research Station 316 E. Myrtle Boise, ID USA 83702	Ph: (208) 364-4386 Fax: (208) 364-4346 /S=D.LEE/OU1=S22L03A@mhs-fsbo.attmail.com
Dr. Jim Lichatowich	ISG 182 Dorey Rd. Sequin, WA USA 98382	Ph: (360) 683-0748 Fax: (360) 681-2938
Mr. David Marmorek*	ESSA Technologies Ltd. 300 - 1765 W. 8th Ave. Vancouver, BC V6J 5C6 CANADA	Ph: (604) 733-2996 Fax: (604) 733-4657 dmarmorek@essa.com
Mr. Mike Matylewich	Columbia River Inter-Tribal Fish Commission 729 NE Oregon Street, Suite 200 Portland, OR, 97232	Ph: (503) 731-1251 Fax: (503) 235-4228

Dr. Chip McConnaha	Northwest Power Planning Council 851 SW Sixth Avenue, Suite 1100 Portland, OR USA 97014	Ph:(1-800) 222-3355 Fax: (503) 795-3370 mcconnah@nwppc.org
Dr. Phil Mundy*	ISG 1015 Sher Lane Lake Oswego, OR USA 97034	Ph: (503) 699-9856 Fax: (503) 636-6335 mundy@teleport.com
Mr. Jim Norris	Columbia Basin Research Puget Sound Plaza 1325 - 4th Avenue, Suite 1820 Seattle, WA 98101-2509	Ph. (206) 685-2182 Fax: norris@fish.washington.edu
Mr. Ian Parnell*	ESSA Technologies Ltd. 300 - 1765 W. 8th Ave. Vancouver, BC V6J 5C6 CANADA	Ph: (604) 733-2996 Fax: (604) 733-4657 iparnell@essa.com
Dr. Charlie Paulsen*	Paulsen Environmental Research 10175 S.W. Barber Boulevard Suite 302B Portland, OR USA 97219	Ph: (503) 245-8186 Fax: (503) 245-8238 cpaulsen@teleport.com
Dr. Randall Peterman*	Simon Fraser University School of Resource and Environmental Management Faculty of Applied Sciences Burnaby, BC V5A 1S6	Ph: (604) 291-4659 Fax: (604) 291-4968 Randall_Peterman@sfu.ca
Dr. Charlie Petrosky*	Idaho Department of Fish and Game 600 South Walnut Boise, ID USA 83707	Ph: (208) 334-3791 Fax: (208) 334-2114 cpetrosk@idfg.state.id.us
Mr. Chris Pinney*	U.S. Army Corps of Engineers Walla Walla District 201 N. 3rd Walla Walla, WA USA 99362	Ph: (509) 527-7284 or 527-7424 (main) Fax: (509) 527-7826 Chris.A.Pinney@NPW01.usace.mil
Dr. John Rhodes	Columbia River Inter-Tribal Fish Commission 729 NE Oregon Street, Suite 200 Portland, OR, 97232	Ph: (503) 238-0667 Fax: (503) 235-4228 fishmgmt@hevanet.com
Dr. Howard Schaller*	Oregon Department of Fish & Wildlife P.O. Box 59, 2501 SW First Avenue Third Floor Portland, OR USA 97207	Ph: (503) 872-5252 ext. 5352 Fax: (503) 229-5602 schaller@eagle.dfw.state.or.us
Dr. Stephen H. Smith*	National Marine Fisheries Service 525 NE Oregon St., 5th Floor Portland, OR USA 97232	Ph: (503) 230-5410 Fax: (503) 231-2318

PATH Workshop 2
May 22, 1996

Dr. Steve Smith*	NWFSC 2725 Montlake Blvd. E. Seattle, WA USA 98112	Ph: (206) 860- 3352 Fax: (206) 860-3267 sgs@tyee.nwfsc.noaa.gov
Dr. Chris Toole*	National Marine Fisheries Service 525 NE Oregon St., 5th Floor Portland, OR USA 97232	Ph: (503) 230-5410 Fax: (503) 231-2318 chris_toole@ccgate.ssp.nmfs.gov
Dr. Randy Tweeten	National Marine Fisheries Service 525 NE Oregon St., 5th Floor Portland, OR USA 97232	Ph: (503) 230-5410
Mr. Earl Weber*	Columbia River Inter-Tribal Fish Commission 729 NE Oregon Street, Suite 200 Portland, OR USA 97232	Ph: (503) 238-0667 Fax: (503) 235-4228 fishmgmt@hevanet.com
Dr. John Williams*	NMFS - NWFSC 2725 Montlake Blvd. E. Seattle, WA USA 98112	Ph: (206) 860- 3352 Fax: (206) 860-3267 jwilliams@sci.nwfsc.noaa.gov
Mr. Paul Wilson*	Columbia Basin Fish & Wildlife Authority Metro Centre 2501 SW First Avenue, Suite 200 Portland, OR USA 97201	Ph: (503) 326-7031 Fax: (503) 326-7033 pwilson@aracnet.com
Mr. Rich Zabel	Columbia Basin Research Puget Sound Plaza 1325 - 4th Avenue, Suite 1820 Seattle, WA 98101-2509	Ph. (206) 685-1132 Fax: (206) 685-1132 rich@cqs.washington.edu

2.0 Discussion of Retrospective Analyses

The first day of the workshop was devoted to the development of a work plan for concluding the remaining retrospective analyses. As apparent from the agenda in Section 1, we completed this discussion in three phases: 1) a review of the chapters dealing with Level 1 and 2 analyses (Chapters 2, 3, 4, 5, 9, and 12); 2) Level 3 analyses (Chapters 6, 7, 8, 10, 11, and 13); and 3) integration of Levels 1, 2, and 3 tasks. Nearing the third part of this discussion, we began to deal with some of the tradeoffs involved in assigning scientists to work on multiple chapters, given the very limited amount of time available. We also discussed integration of various chapters, particularly Chapters 6, 7 and 8.

2.1 Chapter 1

Introduction

This chapter will be updated with a summary of the results of other chapters, and will also include the figures on retrospective aggregate hypotheses presented in Section 1 of this report.

2.2 Chapters 2 and 4

Chapter 2 - The Snake River in the Context of Broad Scale Patterns of Change in Stock Indicators: A Level 1 Pilot Analysis

Chapter 4 - Stressors and Life History Stages Correlated with Patterns of Change in Stock Indicators: A Pilot Demonstration of a Multivariate Analysis Approach

Chapters 2 and 4 were dealt with together in that they both utilize similar data sets, and are strongly linked. Charlie Paulsen summarized the reviewers' comments on Chapter 2 (see Appendix 2), and asked the group which of the various proposed statistical analyses were worth pursuing. The response was that it would be more efficient to form a small technical group to have a detailed discussion of alternative approaches. Charlie Petrosky commented that many of the variables used to characterize habitat were extremely coarse indicators of habitat condition, and that a subjective classification of habitat quality which integrated across many different factors would likely be more accurate. Later in the meeting, it was agreed to classify each of the index stocks according to spawning / early rearing habitat, downstream rearing habitat, and overwintering habitat. Each of the stocks will be ranked on a three-level scale corresponding to high, medium, or low habitat quality:

1. little or no land use impacts with minor degradation;
2. land use impacts with moderate degradation; and
3. land use impacts with heavy degradation.

These classifications will be completed without seeing the quantitative data in Chapter 4 for each of the streams.

In the Retrospective Report, Chapters 2 and 4 explored a number of possible approaches to completing the analysis. Participants discussed various ways of narrowing down the numbers of different combinations of analyses. Randall Peterman recommended choosing a subset of the independent variables to use for the analyses, and to focus only on ln(R/S) as an indicator of stock performance. He also suggested that it was important to select the appropriate spatial scale for explaining the variability among stocks. Charlie Paulsen commented that the data set provided for both within basin contrasts (e.g. Snake River, John Day) as well as between basin contrasts. Howard Schaller commented in turn that the John Day stock should be treated as an aggregate stock, even though there are several index areas.

There was some discussion about the relative importance of the analyses in Chapter 2 compared to the work in other chapters. Given this concern, it was agreed that the most efficient approach for Chapter 2 would be to complete standard approaches (i.e. cluster analyses and discriminant analyses on the complete stock-recruitment data set (or as much as can be completed within the next three weeks), see what the results of these standard approaches are, and then consider whether it is worthwhile completing simulation tests of the influence of uncertainties in stock recruitment data and the use of other analytical methods proposed by the external reviewers. Thus there remain three tasks to be completed:

- Task 2.1- complete the stock and recruitment data assembly;
- Task 2.2 - complete the cluster analyses and discriminant analyses which should be circulated to a larger group; and
- Task 2.3 - assess if the results are deemed worthy enough to do simulation tests and write up as a paper jointly with the results from Chapter 4.

The Chapter 4 analyses were generally felt to be more relevant to management needs than those from Chapter 2, and are thus higher priority. It was agreed that the first priority was to complete the assembly of the remaining data on independent variables including the qualitative habitat ranking discussed above. A meeting will be held May 14 including Danny Lee, John Rhodes, Charlie Petrosky, Les Pinney, Tim Fisher, and Charlie Paulsen. Subsequently regressions and GLIM (General LInear Model) approaches will be applied to the independent variables to explain variation in ln(R/S). Charlie Paulsen agreed to approach Saul Saila to see if Saul is willing to apply some of the less conventional approaches he recommended in his review. The group felt that it was best to concentrate first on “traditional” statistical methods, and see what results they generated. Lou Botsford presented his analyses of the work in Chapters 2 and 4 to account for intra-series correlation. These results are included in Appendix 2. The effect of accounting for intra-series correlation is to substantially reduce the number of significant correlations in the Chapter 2 analyses.

2.3 Chapter 3

Contrast in Stock Recruitment Patterns of Snake and Columbia River Spring/Summer Chinook Populations: Draft Pilot Study

Howard Schaller outlined the proposed next steps for completing Chapter 3. These are outlined in more detail in Section 3, but in summary, they include:

- Task 3.1-completing the run reconstructions for the Entiat, Methow, Wenatchee and Mackenzie (on the Willamette River) by mid-June, and completing the North Fork Umqua (yearling and subyearling), Ho and Queets (Washington, subyearling) after mid-June.

- Task 3.2 - completing the run reconstruction document (Langness et al. 1996) including both general methods and stock details. Stock details should include ocean distribution and genetic information from Chapter 13 (Tom Cooney, hatchery CWT recovery data; Clarabell Hernandez and Mary Anne Johnson). Participants' commented that the ocean distribution information may be biased due to a lack of freshwater recoveries in some missing production groups.

C Task 3.3 - completing statistical tests including formal testing of slopes (ANCOVA, perhaps non parametric), GLIM (reduction in Sum of squares with variable $ versus a constant $ over the whole time period), and a sensitivity analysis of the effects of choice of time periods. These analyses will also include an assessment of the effects of possible errors in the spawner and recruit data.

C Task 3.4 - completing the next draft, and addressing reviewer comments.

Howard Schaller discussed his responses to the reviewers' comments (included in Appendix 3). Howard drew particular attention to the comment on page 2 of his comments regarding the loss of substocks during the 1950 - 1970 period. He made the point that there was not much loss of stocks during the pre-1970 period with the exception of stocks above Hell's Canyon and Panther Creek, neither of which formed a significant proportion of the aggregate stock. Chris Toole raised the question of the sensitivity of the results to the particular choice of years for splitting the data. Howard responded that they would address this question with the aggregate stocks. Charlie Petrosky noted that there was some evidence of impacts in the late 1960s on recruits per spawners; the best approach to assessing these time period issues is to fit the spawner-recruit data to the entire period and then look at residuals. Howard noted that in the subsequent draft the spawning escapements would be normalized to the average escapement for each index stock, rather than to the equilibrium value which led to a rather confusing form of presentation.

One of the interesting comments made by Jeremy Collie was that the recruit per spawner data appeared to imply a strengthening of density dependence, or a reduction in carrying capacity rather than a decrease in productivity in the post 1975 period. Howard commented that the appearance of stronger density dependence is because of the high density independent mortality which occurred in the hydrosystem in the late to mid 1970s, driving down the data points for that period. Howard also addressed the comment of Carl Walters that there could be severe bias in R/S if R had been estimated by proportionally dividing total catch to subbasins as was done for the Fraser River. Howard said this was not a problem in the Columbia as recruitment estimates were performed based on harvest rates rather than total catch. Randall Peterman stressed the importance of including in Chapter 3 a concise description of how recruits and spawners are estimated, since so much of the analyses depend on these data.

Fall Chinook

Fall chinook are an important component of the chinook population in the Columbia and Snake River basins. Fall chinook had not been included in the initial retrospective analyses because of a shortage of time, less availability of useful data, and greater complexity due to a more variable life cycle.

Some of the major groups of fall chinook include:

C mid-Columbia Hanford stock ("Upriver Brights") which range in abundance from 100,000 to 500,000 fish, and have a directed harvest;

- Lewis River stock, a downstream stock ranging in abundance from 10,000 to 20,000 fish;

- the Deschutes stock, ranging in size from 1,000 to 5,000 fish (though estimates are uncertain); and

- the Snake River fall chinook, ranging in abundance from 100 to 500 fish.

- Lyons Ferry Hatchery generates 450,000 yearling fall chinook; another 450,000 yearlings are to be released adjacent to the Snake River. There are approximately 1,000 adults returning from these releases.

There are a variety of ocean distributions among fall chinook. More southern stocks (Cowlitz, Big Creek, Bonneville, Washugl, and Graves) known as "Tule" fish have a significantly different ocean distribution from the mid-Columbia Hanford stock. Ocean harvest of fall chinook remained fairly stable at 45% from the 1970s up until 1991. Subsequently the harvest rate dropped to about 35%, due to both the Endangered Species Act and decreases in other ocean fisheries. The in-river harvest has been driven by harvest rates on the Hanford stock, which increased in the mid 1980s and then subsequently decreased.

Fall chinook experience 55 to 70% mortality rates on upstream passage. The upstream passage mortality is worst in years of low flow. In general, actions for water regulation to assist spring / summer chinook compete with water for fall chinook, both for juveniles and adults. This is an extremely important consideration for the decision analysis to be carried out as part of the prospective analyses.

It was agreed at this workshop that it was important that both retrospective and prospective analyses be carried out for fall chinook, to the degree possible. To meet these goals we developed a draft list of tasks.

- It was agreed that a summary document should be prepared that describes in detail the status of fall chinook stocks in the Columbia River system, including location, availability of run reconstructions, spawner recruit data, historical trends, and ocean / in-river harvest information. The working group for this task is made up of **Olaf Langness** (Leader), Jim Norris, Saan-Yoon Hyun, Howard Schaller, Peter Dygert, Tom Cooney, Mary-Anne Johnson and Dave Gandet (of the Alaskan Department of Fish and Game). They have tentatively planned to meet in Seattle May 31/96 to lay out a plan.

- Carry out run reconstructions for Lewis (downstream), Hanford (mid-Columbia) and Snake River (aggregate) stocks. These reconstructions will be fairly messy because of both the greater complexity / variability of the life history of fall chinook and generally poor quality data.

- Develop Ln(R/S) versus S data for these stocks. Expectations for this analysis should be kept low.

The workshop considered whether later work might include an analysis for fall chinook similar to that done in Chapter 9 of the Retrospective Report (Evaluation of Survival Trends in the Freshwater Spawning and Rearing Life Stage for Snake River Spring/Summer Chinook (Petrosky and Schaller)). It was recognized that the fall chinook analysis would be more difficult for a number of reasons: 1) data are only available since 1991; 2) smolt count information is poorer, 3) fall chinook are not always well differentiated from spring/summer chinook; 4) their behaviour is different; 5) sampling in each year was temporally truncated; 6) PIT-tag data for Snake River rearing area has too low "n" to compute survival; and 7) downstream migration is more complex (temperature driven and quite variable from year to year). On the positive side we do have good predation information (which is much higher on sub-yearlings than on yearlings). As the negative factors far outweighed the positive, the group concluded that there are not enough data to carry out a smolt:spawner retrospective analysis of the type carried out for spring/summer chinook. The PIT-tag data may, however, be useful for estimates of travel time through lower Granite, McNary, Little Goose and John Day (consult Rondorf, Connor and Berggren).

2.4 Chapter 5

Retrospective Analysis of Passage Mortality of Spring Chinook of the Columbia River

Rick Deriso discussed some of the details of the analyses completed in Chapter 5. In particular, simulation testing of the MLE methods showed poor estimation of the X parameter, the constant per dam mortality assigned to each dam not estimated in the upstream/downstream approach to µ. As shown in Table 5.4, the X's and a's tradeoff; scenarios with higher productivities (a) require higher estimated dam mortalities (X) to match the spawning estimates. Rick also pointed out that he had explored the effects of greater levels of error in spawning estimates than a coefficient of variation of 0.25, and higher CVs did not have much effect on the analyses. If the errors in spawning estimates are not important, that is good news for the Chapter 4 analysis which ignores these errors. With respect to the potential bias in estimates (a comment made by a couple of reviewers), Rick commented that the bias was very low to begin with and does not appear to be a problem. Rick also defended the use of a median estimate of µ since some of the distributions looked very skewed, especially those for 1980 and 1981.

Carl Walters had commented that the µ and * (year effect) estimates may be confounded. Rick proposes to follow Carl's suggestion to calculate a year effect specific to different subbasins. This will lead to a range of alternative hypotheses explaining the observed patterns. Rick cautioned, however, that due to the small number of streams, the analysis will eventually blow apart with very wide uncertainty intervals as the number of parameters increases beyond the level that the data will support. It was noted that the year effect estimated by the MLE method generates a pattern somewhat out of phase with the shifts in ocean regimes proposed by Jim Anderson in Chapter 12. Randall Peterman compared the patterns in residuals of ln(R/S) of Bristol Bay sockeye with the year effects estimated by Rick Deriso in Chapter 5. When Randall flipped the graph of Bristol Bay year effects upside down (due to the hypothesis that the Gulf of Alaska regime operates in a manner opposite to that of the Oregon coast) the upside down recruitment anomalies compared surprisingly closely to the * values estimated in Chapter 5 for Columbia River chinook. The comment was made that fish recruitment anomalies are probably a better integration of climate effects than physical oceanographic measurements.

There was considerable discussion regarding the segments of the river for which survival is computed in the MLE method, as compared to the passage models The MLE model computes the incremental life cycle mortality due to Snake River fish having to travel through the migration corridor from Lower Granite Reservoir to John Day Reservoir. The mortality calculated by the MLE method includes habitat effects within this migration corridor as well as the effects of the dams and reservoirs, though habitat effects present prior to 1970 are likely absorbed in the Ricker "a" parameter values. A key point to be checked after the workshop is whether the estimates of mortality generated by the passage models may have included estimates of natural mortality. If this were the case (and it appears that it may be for

CRiSP), then the estimated passage mortalities from the passage models need to be decreased to remove this effect. With respect to the MLE-passage model comparisons, Rick pointed out that it is worth completing a sensitivity analysis of the covariances among streams in patterns of recruits per spawner. This will increase the width of the confidence intervals around the estimates of µ. However, the pre-1970 data does help to ground the estimates of natural survival and productivity.

One of the participants asked whether the MLE estimate of µ needs to explicitly consider transportation. The participant suggested that the calculated X value assumes all fish go through all dams, whereas transported fish do not. It was pointed out by Rick that the MLE estimate calculates the net difference between upstream and downstream stocks, whatever the cause may have been. Therefore some of the transportation benefit ends up lowering the value of µ.

Future analyses to be carried out include the following:

- Task 5.1 - further MLE analyses: analysis of the 1980s' pattern (why was incremental passage mortality so low); examining the pattern of µ versus WTT (water transit time), or potentially adding WTT into the model; examination of * (year effects) against climate indicators; exploring the effect of covariance among streams with respect to confidence intervals around µ; sensitivity analyses of the MLE method (BIC) to help in model selection; examination of µ versus * patterns; and estimates of µ for the pre-1970 time period (which is likely to have wide confidence intervals).

- Task 5.2 - investigation of the connections between the MLE and passage model estimates of mortality (i.e. examination of alternative flow transportation survival relationships as part of the prospective analyses; comparing the confidence intervals around CRiSP/FLUSH projections with the confidence intervals in the MLE method; use of CRiSP/FLUSH estimates of dam mortality back to 1952 to get an estimate of the variation in survival among dams and years [used as indices to derive better estimates of *X*]; and removal of natural mortality from the passage model estimates of µ).

- Task 5.3 - complete the draft journal paper.

Though Chapter 5 is the only chapter which explicitly accounts for uncertainties in S, it was recommended that Chapters 2, 3, 4, and 9 also look at the effects of such errors on their conclusions. The effects of errors in the calculation of recruitment will be part of the prospective analysis.

2.5 Chapter 9

Evaluation of Survival Trends in the Freshwater Spawning and Rearing Life Stage for Snake River Spring/Summer Chinook

Charlie Petrosky's responses to the reviewers' comments are contained in Appendix 4. Charlie noted three changes he proposes to make to the paper:

1. complete non-parametric statistical tests only if the power analysis of the parametric test shows low power;
2. use an ANCOVA instead of a t-test for slopes and intercepts; and

3. revisit the hatchery fry outplant history.

He also proposes to include various caveats related to changes in sampling methods. These changes will be sufficient to convert the chapter into a draft journal paper.

Participants had a few comments on the Chapter 9 analyses. First it was stressed, as stated above, that there should be some analysis of the sensitivity of the conclusions to errors in estimation of S and R. Second, Charlie Paulsen asked whether there might be any transportation effects on smolts, but Charlie Petrosky pointed out that the smolts are collected for transportation after they are enumerated. Third, with respect to the absence of evidence for depensation in the aggregate stock calculations used in Chapter 9, it was pointed out depensation is less likely to be evident in the aggregate stock and more likely to be seen in index stocks. Fourth, with respect to whether the authors of Chapter 9 agreed with Carl Walters that their analyses implied habitat was not an issue, Charlie Petrosky replied that they did not take their conclusions that far. They were merely pointing out that the smolts per spawner were consistent with the 1960s, which is not to say that the 1960s had pristine habitat, and certainly some degradation of habitat had occurred up to that point, going back to the 1900s. Al Giorgi added several comments on Chapter 9 after the workshop, which are contained in Appendix 4.

2.6 Chapter 12

Influence of Climate on Fish: Review

Jim Anderson described the factors responsible for the shift in regimes between the Oregon coast and the Gulf of Alaska. He pointed out that his next steps in improving Chapter 12 would be to: incorporate new information that has come from the recent conference in Newport, Oregon; use the ln (R/S) data of Charlie Paulsen to assess indices other than the NPI (e.g. the recruitment anomalies from Alaska supplied by Randall Peterman, the Aleutian Low Pressure Index); and add the additional information supplied by Rich Hinrichsen (included in Appendix 5). Randall Peterman pointed out that including an index which reflects freshwater climate conditions may absorb some of the variation formerly assigned to ocean indices. Thus, even though it may be potentially more interesting to keep the freshwater and ocean indices separate, the statistical procedure will end up smudging these together.

The group discussed whether or not it was feasible to conduct a broader scale R/S analysis from California to Alaska. Charlie Paulsen felt that there were not enough data to complete this, though there may be data for some hatchery chinook stocks and potentially other salmon species. Jim Anderson raised the possibility of including the ocean indices or residuals from Bristol Bay sockeye data, or Columbia River aggregate stock residuals in the MLE analysis, under the presumption that fish are the best integrators of ocean effects, rather than oceanographic indices. Most people felt that it was better to keep the MLE analyses wholly empirical based on the patterns in recruitment and spawning, rather than incorporating ocean indices directly. Jim raised the point that oceanographic analyses offer alternative interpretations as to what has occurred in the past, and which policies are most appropriate for the future. For example, were the low μ values in the early 1980s due to high flow levels or high upwelling?

2.7 Chapter 6

A Decision Tree for the Columbia River Hydrosystem, and A Proposed Approach to Synthesizing Evidence Relevant to these Decisions

Chris Toole presented an integrative framework incorporating the hydrosystem, habitat, hatchery and harvest effects (Figure 2.1). One participant asked whether the decision tree presented in Chapter 6 was too sequential, causing one to get bogged down in the uncertainties related to transportation and then never move on to other issues. Chris Toole pointed out that these alternatives were only ordered in terms of cost, but actually would be assessed concurrently. The syntheses of evidence completed by Chris, Al Giorgi, and Earl Weber are in fact concurrently assessing each of the various components of the decision tree. Howard Schaller suggested that it would be useful to work experimental programs into the decision tree, so as to promote adaptive management. Randall Peterman pointed out that in traditional methods of decision analysis, the decision tree is not binary, but rather that there are probabilities associated with each pathway (this is discussed further in Section 4).

Chris Toole stressed that fundamental to all the analyses in Chapter 6 (and also to the prospective analyses) is the need for a combined life stage goal either expressed in terms of a percentage, or relative to some previous time period. This then allows one to compute allocations across the different human-induced mortality sources (Figure 2.1). He noted that these allocations must be life cycle mortality estimates to returning spawners, not just survival within the migration corridor. This should condition the types of quantitative analyses completed. Chris highlighted the need to rework the structures of Chapters 6, 7, and 8 as many of the analyses presented in Chapters 7 and 8 do not directly relate to the decision tree, whereas other types of analyses that might be useful have not been addressed yet (e.g. examination of alternative transportation models and their implications). Finally, he thought it would be very fruitful to investigate what alternative hypotheses can explain the difference between the MLE, CRiSP and FLUSH estimates of migration corridor mortality. It was agreed that Chris, Al Giorgi, and Earl Weber would meet during the week of April 22 to rework the structure of Chapters 6, 7, and 8. This meeting (now completed) resulted in a new set of tasks for the integrated Chapter 6 (formerly 6, 7 and 8),

2.8 Chapter 7

Quantitative Exploration of Alternative Hydrosystem Hypotheses

Selected analyses will be completed that are most relevant to the decision tree in Chapter 6, and fit into a revised outline for Chapter 6. It was generally agreed that analyses of travel time were of lower priority than analyses of fish survival. Some simple analyses which could be completed include: deriving a seasonal average FTT from CRiSP to compare with FLUSH; examining evidence of delays at various spots within reservoirs; comparison of a linear relationship with an exponential travel time relationship; and estimates of flow travel time relationships for fall chinook. However, it was agreed that all of these analyses should be postponed until after the mid-June deadline. Appendix 6 contains PATH participants' responses to reviewers' comments on several chapters, including Chapters 7 and 8.

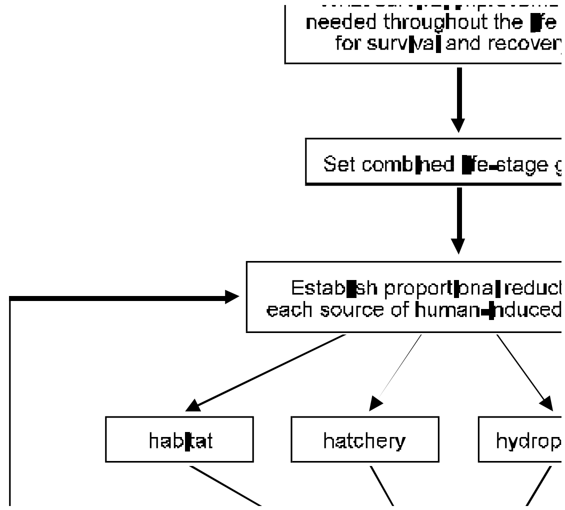


Figure 2.1: Integrative framework incorporating the hydrosystem, habitat, hatchery and harvest effects **.**

2.9 Chapter 8

Sensitivity Analyses for Mainstem Passage Survival

As with Section 7, the components to included in this chapter will be decided after the outline for Chapters 6, 7, and 8 has been developed. Some candidate improvements to Chapter 8 include:

- summarizing the descaling section, which is far too long.

- conduct side-by-side sensitivity comparisons of FLUSH and CRiSP to clarify the effects of differing estimates of fish guidance efficiencies (FGEs), spill efficiency, turbine mortality, survival as a function of descaling, and spill/gas effects. These sensitivity comparisons would use both system survival and reservoir survival as response variables (the existing Chapter 8 has an “apples and oranges” problem). It was also of interest to rerun FLUSH and CRiSP varying survival in 1973 and 1977 to account for descaling, and then with this correction compare the passage model estimates of μ with those generated by the MLE method.

- assess existing information to see if fish are smaller and more vulnerable in low water years.

- examine alternative transportation models (including ocean effects, water transit time, and flow survival) and compare these to the MLE methods.

There are several reasons for undertaking the above analyses. For the FGE estimates, PIT-tag estimates are in the range of 35 to 45%, whereas fyke net estimates are in the range of 50 to 65%, which makes a significant difference in estimated smolt numbers arriving at the dams, and therefore estimated smolt survival (lower FGEs result in higher estimated survivals). Spill efficiency estimates have both poor precision and uncertain accuracy. Turbine mortality estimates range from 7 to 18%, and this range has implications for the improvements gained by both spill and by-pass. Descaling has been discussed for many years as a potential cause of low survival during 1973 and 1977; there are now sufficient data to apply some quantitative estimates to this effect. With respect to spill and gas effects, the estimates of mortality are very dependent on a few experiments that many people believe do not reflect natural conditions. Analyses of changes in fish size and vulnerability in low water years is important for testing the assumption of the independence of survival among dams and years in the MLE method. The CRiSP/FLUSH estimates of dam mortality back to 1952 will also be useful as a relative index of mortality.

As stated above, which of these analyses are actually completed depends on the reframing of Chapters 6, 7, and 8.

2.10 Chapter 10

A Decision Tree for Structured Syntheses of Evidence Concerning Changes in Spawning and Rearing Habitat

Charlie Petrosky led the discussion of this chapter, but indicated that he does not have sufficient time to be its primary author. It was agreed to develop retrospective hypotheses based on a synthesis of questions 1 to 4 in the existing Chapter 10. It was unclear, however, how much useful data could be obtained to document habitat effects historically, and whether those effects for particular stocks could be regionalised using approaches developed by the USFS in the East Side Assessment. During the following day's discussions on prospective analyses in Subgroup B, a number of alternative approaches were suggested. These are described in Table 3.1, presented in the following chapter. Major tasks include qualitative ranking of habitat for each index stock (as described in Section 2.2 of this report), development of aggregate hypotheses, syntheses of evidence from habitat case studies, and exploration of methods to regionalize the results from case studies. The key issue raised by several participants was "How much of the historical reduction in life cycle survival was actually due to habitat changes, and over what time scale did that occur?". Chris Pinney agreed to take over authorship of this chapter, and will attempt to involve a number of other biologists (Danny Lee, Bruce Rieman, John Rhodes, other tribal biologists, Charlie Petrosky, Charlie Paulsen, Ian Parnell and Jim Geiselman). This group met on May 14th, 1996. Reports by Lars Mobrand may also be relevant in examining historical changes in carrying capacity.

2.11 Chapter 11

Hypotheses Regarding Hatchery Impacts

Paul Wilson began the discussion of this chapter by responding to some of the reviewers' comments. He pointed out that it was important to look at hatcheries from a risk management approach; even if hatcheries have had a negative effect historically, we may need them in the future. Paul also pointed out that it is likely to be easier to distinguish hatchery impacts between stocks rather than within stocks. The next step for Chapter 11 is to link the listed hypotheses to management decisions, and conduct a series of fairly simple analyses. These would include comparisons of recruits per spawners for index stocks that have a range of hatchery influence (from zero to high). Some of the independent variables in this analysis would include the number of smolts released (both absolute numbers and relative to the carrying capacity), an index of similarity between the native stock and the release stock (e.g. distance), and intra-specific effects. Paul also thought that it was worthwhile to develop a decision tree specifically for hatcheries.

There are a number of challenges in conducting a synthesis of the literature for and against hatchery hypotheses. One of these challenges is that there are at present many different hypotheses and it is not self-evident how to narrow them down. The second challenge is that the hatchery activity data have not been synthesized over space and time; this is the “big, ugly table” which Carl Walters referred to. Thirdly, since the Snake River hatcheries were constructed as a mitigation for the decline of wild stocks, a negative correlation between hatchery activity and escapement will naturally appear in the data set. Finally, though there may be some pre- and post-supplementation genetic meristic data, interpretation of this information in terms of impacts on life stage specific or life cycle survival will be difficult.

As with Chapter 10 a series of good ideas concerning potential analyses for the hatchery component were developed by Subgroup B on the following day’s discussions of prospective analyses. These are listed in Section 3 under Table 3.1. The first step, linking hatchery hypotheses to management decisions, will involve a meeting with the hatchery group, possibly including Paul Wilson, Dan Bouillon, Charlie Paulsen, Bob Foster, Mark Chilco, Mike Matylewich, Steve Smith (Portland), Olaf Langness, and Reg Reisenbickler.

Finally, an interesting fact supplied by John Williams: in 1970 there were 400 million smolts planted in streams between California and Canada; in 1990 there were 1.4 billion.

2.12 Chapter 13

Hypotheses Regarding Harvest Impacts

Tom Cooney and Peter Dygert proposed to complete a summary of harvest impacts, with the following components:

- time trends in spring/summer harvests in both the ocean and river;
- time trends in fall chinook harvests for the Snake River stocks, coastal stocks along Oregon and Washington, and Canadian stocks;
- a summary section on methods of harvest estimation; and
- development of aggregate hypotheses.

Tom also agreed to supply information on distributions of spring/summer and fall chinook within the ocean, based on coded wire tag collections from hatchery stocks.

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

3.0 Retrospective Analyses Work Plan

Table 3.1 summarizes the major retrospective tasks identified at the workshop, and Table 3.2 provides some preliminary estimates of labor days associated with each task. These estimates are currently being reviewed and revised by the Planning Group.

Table 3.1: Retrospective analyses work plan - task descriptions **.**

Chapter	Task Description
Summary of Peer Reviewers' Comments	**Lead Author: Barnthouse** Summarize peer reviewer comments on the Draft Retrospective Report for the Implementation Team.
Chapter 1	**Lead Author: Marmorek**
	Incorporate reviewers suggestions, aggregate hypotheses figure.
	Modify text for new structure; executive summary.
Chapter 2	**Lead Author: Paulsen**
2.1	Complete spawner and recruit data (see 3.1).
	Assess the effect of errors (especially potential bias) in spawner and recruit data.
2.2	Do cluster analyses and discriminant analyses.
2.3	Assess if the results are interesting enough to do simulation tests and write up as a draft paper (see 4.4).
Chapter 3	**Lead Author: Schaller**
3.1	Finish and distribute run reconstructions data for the Entiat plus three more by June 14th: Methow and Wenatchee (upper Columbia), Mackenzie (on Willamette, two mainstem dams downstream). After June 15th: North Fork Umqua (Oregon, yearling and subyearling), Ho and Queets (Washington, subyearling). Mackenzie and Umqua have lower quality and less data (e.g. age structure) than some of the previous stocks.
3.2	Complete run reconstruction descriptions (Langness et al, 1996), including: a) general methods (higher priority); and b) stock details (including ocean distribution and genetic information from Chapter 13 - Tom Cooney hatchery CWT recovery data - Clarabell Hernandez / Mary Anne Johnson).
3.3	Perform suggested statistical tests: formal testing of slopes (ANCOVA, perhaps nonparametric); GLIM (reduction in SS with variable δ versus constant δ); sensitivity of results to choice of time periods used for comparison. Assess the effect of errors (especially potential bias) in spawner and recruit data.
3.4	Complete next draft. Address reviewer comments.

3.5 (Tasks 3.5-3.8 apply mainly to fall chinook)	Prepare a summary document that describes in detail the status of Fall Chinook stocks in the Columbia River system, including abundance, location, availability of run reconstructions, availability of data, historical trends, recruitment information, etc. Group has tentatively planned to meet in Seattle May 10/96 to layout a plan for Fall Chinook (**Olaf Langness**, Jim Norris, Saan-Yoon Hyun, Howard Schaller, Peter Dygert, Tom Cooney, Mary-Anne Johnson)
3.6	Carry out run reconstructions for Lewis, Hanford and Snake River stocks.
3.7	Develop Ln(R/S) versus S data for these stocks.
3.8	Document the estimated rate of harvest on Spring, Summer and Fall chinook salmon in the Columbia River system.
Chapter 4	**Lead Author: Paulsen**
4.1	Include qualitative ranking of habitat in basins without measured data (see Chapter 10). Complete other data assembly.
4.2	Regressions, GLIM approaches.
	Assess the effect of errors (especially potential bias) in spawner and recruit data.
4.3	Assess other types of analytical approaches (e.g. Saila's pursuit of his own suggestions if possible).
4.4	Complete draft journal paper of Chapters 2 and 4 results (if results worthwhile), addressing reviewer comments.
Chapter 5 MLE	**Lead Author: Deriso**
5.1	**Further MLE Analyses:**
	Further analysis of 1980's pattern.
	Look at μ vs WTT (consider adding into model).
	Look at * vs climate.
	Explore covariance among streams with respect to confidence intervals.
	Explore methods: C BIC C μ vs * (* upstream, * downstream) C pre-1970 μ?
5.2	**MLE - Passage Model Connections:**
	Determine the flow transport survival relations most consistent with μ.
	Sensitivity analysis of MLE using results from sensitivity analyses of CRiSP/FLUSH.
	Test the assumption of independence of survival among dams and among years. For MLE analyses get CRiSP/FLUSH estimates back to 1952 for all 'X's (use as an index of relative mortality).
	Removal of natural mortality from CRiSP μ.
5.3	Complete draft journal paper.

Chapter 6,7, 8 Synthesis - Hydrosystem	**Lead Author: Toole**
Chapter 6	
6.1	Develop structure for integrated chapter 6,7,8 *(Giorgi revising)*
6.2	Incorporate new and updated material for existing Decision Tree and for/against analyses *(See sub-tasks below)*
6.2.1	Complete draft outline for in-river and hybrid decisions; combine with transport outline - this is Section I of integrated report *(Toole)*

6.2.2	Complete draft of Section II of integrated report - quantitative explorations to support decision outline in Section I *(See sub-tasks below)*
6.2.2.1	Summarize recent PIT-tag survival results for Table A1 *(Toole)*
6.2.2.2	Summarize Raymond/S&O survival studies for Table A2 *(Weber)*
6.2.2.3	Illustrate sensitivity of FPE to plausible ranges of FGE and spill efficiency for Table B2a *(Weber)*
6.2.2.4	Illustrate sensitivity of in-river survival past eight dams to plausible range of FPE and turbine mortality for Table B2b; assume reservoir mortality = 0 *(Weber)*
6.2.2.5	Summarize current dam-specific passage configuration, 120% gas saturation spill caps, and site-specific FPE estimates for Table B2c *(Toole)*
6.2.2.6	Illustrate sensitivity of reservoir survival estimates to reach survival estimates (using Tables A1 and A2) and assumptions regarding FPE and turbine mortality (using Table B2b) for Table C1.1 [described as Table C1.3a,b in previous Giorgi outline] *(Weber)*
6.2.2.7	Compare two hypotheses CRiSP and FLUSH characterizations of relationship between fish speed and (WPTT and Flow) for Table C1.2 [described as Table C1.1 in previous Giorgi outline] *(Wilson, Zabel)*
6.2.2.8	Compare two hypotheses CRiSP and FLUSH characterizations of relationship between reservoir mortality and (Fish Speed and WPTT and Flow) for Table C1.3 [described as Table C1.2 in previous Giorgi outline] *(Wilson, Weber, and Zabel)*
6.2.2.9	Prepare table summarizing conclusions regarding possible methods to increase in-river survival *(Not assigned yet)*
6.2.2.10	Prepare summary of information regarding rate of descaling at transport facilities as a function of flow conditions for Table D.1 *(Giorgi)*
6.3	Prepare Appendices to report with narrative discussions containing greater detail regarding reviews of previous research than is contained in Sections I and II *(See sub-tasks below)*
6.3.1	Complete spill efficiency appendix. *(Giorgi)*
6.3.2	Prepare FGE appendix *(Toole to coordinate within NMFS)*
6.3.3	Prepare turbine mortality appendix *(Toole to coordinate within NMFS)*
6.3.4	Prepare bypass mortality appendix *(Toole to coordinate within NMFS)*
6.3.5	Prepare spill mortality appendix *(Toole to coordinate within NMFS)*
Chapter 9 FSR Smolt to spawner survival	**Lead Author: Petrosky**
9.1	**Analyses**
	Assess effect of errors (especially potential bias) in S & R. Assess sensitivity of analyses to break points chosen.
	Other statistical tests (ANCOVA instead of t-tests(slopes and intercepts), or non-parametric if parametric power test shows low power).
	Caveats on changes in methods.
	Revisit hatchery fry outplant history.

9.2	Write draft journal paper.

Chapter 10 Habitat	**Lead Author: Pinney**
10.1	Habitat ranking: Petrosky to classify Idaho stocks (3 days), Beamesderfer to classify Oregon stocks (3 days), Langness et al. to classify Washington stocks (3 days)- completed 14 May 1996, delineate 3 areas/stock (rearing; spawning; over-wintering) on subwatershed maps of Eastside Assessment by 25 May; Billy Connor classify falls (3 days)-post-June 15, 1996. Start with stocks for which run reconstructions have been completed. Use the 3 quality classes of states and correlate to physical variables in Eastside Assessment database - (Lee). Do independent of the quantitative habitat analyses in Chapter 4 and state subjective classifications using amount of management activity. Lee - 1 June 96
10.2	Develop and refine retrospective hypotheses (regional scale) from Questions 1-4 in existing Chapter 10. (Pinney-2 days), 1 June 1996 based upon 14 May discussions. Pinney - 1 June 96
10.3	**'Wait and See' Approach:**
	Complete Chapter 4 analyses (including habitat ranking) for comparison to Chapter 9. If the conclusions are different proceed with further analyses. ("Chapter 9 of retrospective analysis is complete and suggests no change in smolts/spawner during late 60's - mid 80's. Results from Chapter 4 may show a relationship between FSR conditions and recruitment. If so, assess the magnitude of FSR mitigation actions on survival and recovery." (Paulsen-4 days) [from Group B discussions].) Paulsen - 1 June 96
10.4	**Re-evaluate Decision Tree:**
	Tree will be better utilized as an hypothesis tree for management direction linking the Retrospective with the Prospective. Pinney and Parnell - 1 June 96
	Use a hypothesis management approach to look at FSR habitat through Ricker "a" and stock recruit evaluation criteria.
	Clearly link FSR habitat hypotheses to management direction setting a pathway to Prospective analysis.
10.5	**Analyses** (concurrent with Chapter 4) with Evidence For/Evidence Against Matrix based upon Rhodes et al. (1994). Pinney - 14 June 96
	Analyze wild parr PIT tag data, by year and stream. Correlate with habitat quality measures. 5 days. Pinney based on Kiefer, Carmichael, Achord (30 June 96)
	Look for habitat change case studies estimate quantitative change in Ricker "a" and "b", and time scale, and ln(R/S), and mainstem habitat conversions. 4 days. Pinney and Parnell
	Assess for index stocks; then scale up to region using east side assessment. 4 days. Pinney and Lee
	Tell story of life history types that have been lost. Functional versus Physical Attributes of Habitat changes. Irreversible versus Reversible Probabilities of habitat recovery for index stocks or metapopulations by temporal scale for natural vs intervention. 5 days. Pinney consulting on Lee analyses
10.6	Write up results showing benefit to prospective analysis. 3 days. Pinney - 14 June 96

Chapter 11 Hatcheries	**Lead Author: Wilson**
11.1	**Decision Tree**
	Use a risk management approach to look at hatcheries.
	Clearly link hatchery hypotheses to management decisions.
11.2	**Analyses**
	Evaluate wild stocks for which hatcheries came on line during the time period covered by data: Warm Springs (75/77), South Fork Salmon (about 81), Imnaha (mid 80's), Looking Glass (mid 80's). If there has been a decrease in wild stock productivity after the introduction of the hatchery stocks, the difference may be an estimate of: 1) the impact the hatchery had on that wild population; and 2) the potential for recovery of that stock if the hatchery is removed. Compare (R/S) for index stocks affected/not affected by hatcheries. Possible independent variables include: C number of smolts released C [number of smolts released plus strays]/carrying capacity C index of similarity between native stock and released (e.g. distance) C interspecific effects Note: beware of confounding effects. Snake River hatcheries were mitigation for stock declines, therefore a correlation will appear.
	Hatchery literature search: synthesize evidence for/against hypotheses of retrospective hatchery causes of decline in wild stocks (see the Bowles and Leitzinger technical report).
	Obtain pre and post supplementation genetic and meristic data.
	Create Walters' "Big Ugly" hatchery table. The temporal component of this table will be difficult to gather.
11.3	Write up results.
Chapter 12 Climate	**Lead Author: Anderson**
	Synthesize and incorporate new material (e.g. Rich Hinrichsen's work) into Chapter 12.
Chapter 13 Harvest	**Lead Author: Cooney**
13.1	Harvest retrospective aggregate hypotheses linked to Decision Tree.
13.2	Descriptive Information / Write up
	Time trends for spring/summer chinook harvest - ocean/river.
	Time trends for fall chinook harvest - Fisheries: Snake River, coastal, Oregon coast, Washington coast, Canadian.
	Summary section on methods, incorporate into aggregate hypotheses.

PATH Workshop 2
May 22, 1996
Table 3.2: Retrospective analyses work plan - time allocation **(Chapter leads listed in bold) Note: Numbered tasks are separately budgeted for time; subtasks are just listed (about 40 working days until June 15th)**

Task List	Complet'n Date	Person(s) Responsible	Days of Effort	Break down of Effort into Days per Month					
				Apr. 7	May 23	Jun. 20	Jul. 23	Aug. 22	Sep. 21
Summary of peer reviewer comments	**01-May**	**Barnthouse**	**2**	**2**					
Chapter 1 - Introduction (Marmorek) C incorporate reviewer suggestions C revise according to new data and chapter material C executive summary	14-Jun	Marmorek Parnell	3 3			3 3			
Chapter 2 (Paulsen) 2.1 Complete S&R data (Schaller / Langness -- 4 stocks) (Paulsen -- 13 stocks)	10-May	Paulsen Schaller Langness	10 2 5	5 1 2	5 1 3				
2.2 Cluster Analyses & Discriminant Analyses - circulate C Assess the effect of error in S&R data (part 2 of 2)	17-May	Paulsen	5		5				
2.3 Assess if results interesting enough to do simulation tests, and write up as paper (see 4.4)	24-May	Paulsen Botsford	2 1			2 1			
Chapter 3 (Schaller) 3.1 Finish and distribute run reconstructions C Add 1990 brood year	06-May	Langness Beamesderfer Schaller Petrosky	5 5 2 2	5 2 2 2	 3				
3.2a Complete description of methods	15-May	Langness	5		5				
3.2b Complete run reconstruction descriptions including stock details (Langness et al. 1996)	14-Jun	Langness Beamesderfer	7 17		4 10	3 7			
3.3 Complete statistical tests, including effect of error in S&R data	24-May	Schaller	5		5				
3.4 Complete next Draft	14-Jun	Petrosky Schaller	5 5			5 5			
Tasks 3.5 to 3.8 still to be budgeted									
3.5 Summary document for fall chinook [Jim Norris] + Olaf Langness, Saan-Yoon Hyun, Howard Schaller, Peter Dygert, Tom Cooney, Mary-Anne Johnson									
3.6 Run reconstructions for selected stocks.									
3.7 ln(R/S) vs. S analyses									
3.8 Summary of harvest information									
Chapter 4 (Paulsen)									
4.1 Include qualitative habitat ranking (see Chp. 10 task description in Table 3.1) with supportive documentation. Complete other data assembly.	10-May	Paulsen Rhodes Lee Petrosky Pinney Fisher	5 3 3 3 2 20	2 7	3 3 3 3 2 13				

4.2 Regressions, GLIM approaches; C Assess the effect of error in S&R data; independent variables	24-May	Paulsen Botsford	5 2		5 2					
4.3 Assess other types of analyses (e.g. Saila) -- if he is willing	31-May	Saila	10		10					
4.4 Complete draft journal paper (if results worthwhile) with Chpts 2 & 4 results	14-Jun	Paulsen Botsford	10 4			10 4				
Chapter 5 - MLE Analyses (Deriso/Marmorek) 5.1 MLE Investigation C Further analyses of 1980s' patterns C µ versus water travel time C pre-1970 µ C * vs climate C covariance issues, implications for confidence intervals C exploration of method: BIC µ vs *, (* upstream, * downstream)	31-May	Deriso Marmorek Parnell Bouillon	15 15 10 10	1 1	10 10 5 5	4 4 5 5				
5.2 MLE - Passage Model Connections C flow transport survival relationships most consistent with µ C removal of natural mortality from CRiSP µ C complete MLE with CRiSP/FLUSH sensitivity analyses results C CRiSP / FLUSH survival estimates back to 1952 (replace X's)	24-May	Wilson Askren Anderson Schaller	10 10 5 5		10 10 5 5					
5.3 Complete draft journal paper	14-Jun	Deriso Marmorek	5 10			5 10				
Chapters 6, 7, and 8 - Hydrosystem Information Integration (Toole)										
Chp. 6 6.1 Develop structure for integrated Chapter (6,7,8)	01-May	Toole Weber Giorgi	2 1 1	2 1 1						
6.2 Incorporate new and updated material for existing Decision Tree and for/against analyse	14-Jun	Toole Weber Giorgi	20 20 20	5 5 5	10 10 10	5 5 5				
6.2.1 Complete draft outline for in-river and hybrid decisions; combine with transport outline - Section 1		Toole								
6.2.2 Complete draft of Section II - quantitative explorations to support decision outline										
6.2.2.1 Summarize recent PIT-tag survival results		Toole								
6.2.2.2 Summarize Raymond/S&O survival studies		Weber								
6.2.2.3 Sensitivity of FPE to plausible ranges of FGE and spill efficiency		Weber								
6.2.2.4 Sensitivity of in-river survival past eight dams to plausible range of FPE and turbine mortality		Weber								
6.2.2.5 Summarize dam-specific passage configuration, 120% gas saturation spill caps, FPE estimates		Toole								
6.2.2.6 Sensitivity of reservoir survival estimates to reach survival estimates, FPE and turbine mortality		Weber								

6.2.2.7 Compare CRiSP and FLUSH characterizations of relationship between fish speed and (WPTT and Flow)		Wilson Zabel								
6.2.2.8 Compare CRiSP and FLUSH characterizations of relationship between reservoir mortality and (Fish Speed and WPTT and Flow)		Wilson Weber Zabel								
Chapter 9 - FSR Survival (Petrosky) 9.1 Analysis C Assess the effect of error in S&R data C Complete other statistical tests C Place caveats on changes in methods C Revisit outplant history	31-May	Petrosky Schaller	15 5	5	10 5					
9.2 Write up as draft journal paper	14-Jun	Petrosky Schaller	10 5			10 5				
Chapter 10 - Habitat (Pinney) 10.1 Assign habitat quality index to stocks (time under task 4.1)	1-Jun	Pinney Lee Petrosky Langness Connor								
10.2 Retrospective aggregate hypotheses	1-Jun	Pinney	4		4					
10.3 Complete Chapter 4 analyses for comparison to Chapter 9 (wait and see approach)										
10.4 Habitat Analyses (go for it approach; do concurrent with Chapter 4) C Analyses of wild parr PIT tag data C Habitat case studies C Regionalization using East Side Assessment C Evidence for and against	14-Jun	Pinney Lee Rhodes Parnell Weber Geiselman Paulsen	10 5 5 10		10 5 5 10					
10.5 Write up results of analyses	14-Jun	Pinney Parnell	10 5			10 5				
Chapter 11 - Hatchery (all tasks budgeted together) (Wilson) 11.1 Decision Tree	01-May	Wilson Foster	33 5	3	20 5	10				
11.2 Analyses C Genetic and meristic characteristics from Snake River monitoring program C Hatchery literature search - for and against C Evaluate (R/S) for wild stocks for which hatcheries came on line during period of data.	31-May	Chilco Langness Geiselman Smith - Portland Matthews	5 5 3 4 5		5 5 3 2 5	2				
11.3 Write up results C Comments: too unwieldy a team? Paul Wilson overloaded (see totals below)	14-Jun	Reisenbickler Bouillon Matylewich Carremilla	5 10		5 5	5				
Chapter 12 - Climate (Anderson) 12.1 Synthesize and incorporate new information	14-Jun	Anderson	5			5				
Chapter 13 - Harvest (Cooney) 13.1 Decision Trees / Aggregate Hypotheses	05-May	Cooney Dygert Schaller Norris	3 2 2 2		3 2 2 2					

13.2 Descriptive Information / Chapter Writing C time trends for spring/summer and fall chinook harvest C summary section on methods C implications for hypotheses	14-Jun	Cooney Dygert Schaller Norris	10 10 5 10		5 5 3 5	5 5 2 5				

4.0 Prospective Analyses

4.1 Plenary Presentations and Discussions

4.1.1 Goals

Using the new information obtained from the retrospective analyses, and discussions from the workshop, we are now able to begin to consolidate ideas for prospective analyses. At the workshop several goals were proposed for the development of prospective analyses (Table 4.1).

Table 4.1: Goals for prospective analyses **.**

A:	Estimate improvement in life cycle survival required for survival, recovery, and rebuilding goals; and uncertainty in estimates.
B:	Develop alternative aggregate hypotheses (prospective) on how to achieve survival goals, building on analyses of retrospective aggregate hypotheses.
C:	Assess quantitative improvement in survival possible through various combinations of changes in Habitat, Hydropower, Hatcheries, Harvest and Climate, and associated risks.
D:	Assess ability to distinguish among competing aggregate and life stage specific hypotheses from future information.
E:	Advise institutions on research, monitoring and adaptive management experiments which maximize rate of learning, and clarify decisions.

The workshop represented our first real attempt as a group to formulate ideas for the prospective analyses. The development and refinement of these ideas will be an iterative process requiring communication and further small group meetings. As a start to Goal B, David Marmorek presented a preliminary attempt at prospective aggregate hypotheses (Table 4.2). These are the future decision sets most consistent with the alternative retrospective hypotheses presented in Chapter 1 of the Retrospective Report, and illustrated in Figure 1.2 of this report. Though the participants acknowledged the general accuracy of Figure 1.2, some people felt that its structure served mainly to polarize scientists into their traditional “camps”. A decision analysis approach (presented below) was considered more likely to result in progress, since it focuses on specific alternative hypotheses.

4.1.2 Decision Analysis

One of the methods proposed to help develop the appropriate suite of prospective analyses was to structure our problem into a “Decision Analysis” format. The concept of decision analysis is essentially that consensus can be reached by embracing uncertainty, and balance can be achieved even in the absence of an identifiable decision maker (Walters 1986). Randall Peterman briefly described the decision analysis approach in terms of its benefits and essential elements. The benefits include:

1. systematic approach which takes a problem and breaks it into its component parts;
2. explicitly takes uncertainties into account;

3. methods already widely applied in fisheries as well as other fields (e.g. business, refer to references in Appendix 1):
   Sainsbury 1988
   McAllister and Peterman 1992
   Walters 1981, 1986
   Hilborn et al. 1994;
4. helps identify priorities of research; and
5. helps identify management actions which are robust to various uncertainties.

Table 4.2: Implications of aggregate hypotheses for future / actions **(pp. 1-8, 1-9** of PATH Preliminary Report on Retrospective Analyses (Marmorek et al. 1996))

Life Stage	*Hypothesis 1 (pg. 1-8)*		Hypothesis 2
	(pg. 1-9) - HYP. IA/IB/II	*HYP III/IV (pg.1-9)*	
Overall	*No changes in S till {JMC, UP} improves; transportation policy unclear*		Major improvement to JMC in 8 /90's; only modest changes pos now. No change in S till EOS improves
FSR	*Improve habitat to get full benefit of JMC/UP changes*		Improve habitat
JCM	*IA. Reduce reservoir predation* *C increase velocity above Lower Monumental to improve transportation survival.* *C increase velocity through whole system to reduce predation of migrants* *II Improve dam more than reservoir*	*III. Actions in IA and increase water volume to restore natural hydrograph, improve estuary survival.* *IV. Actions in IA and change hatchery operations to minimize interaction*	Continue mixed strategy of transportation / leaving fish in ri improve collection efficiency. Use flow targets.
EOS	*Harvest rate policy consistent with improvements in JMC*		Improve hydrograph to extent possible; wait till ocean change
		III (see JMC)	
UP	*Springs: Breach dams?* *Improve fish ladders?* *Falls: Control temperature and flow (if water available)*		Modest improvements

Legend

PATH Workshop 2
May 22, 1996

Hypothesis 1
Hypothesis 2
Hypothesis 3

ESSA Technologies Ltd.

The eight essential elements of decision analysis include:

1. a list of alternative management actions (i.e. different experimental management plans);
2. management objectives composed of performance measures (e.g. ln(R/S)), which are criteria for ranking management actions;
3. uncertain states of nature (e.g. different hypotheses about key relationships such as flow survival);
4. probabilities of those states;
5. model to calculate the outcomes of each combination of each management action and each hypothesized state of nature;
6. decision tree;
7. rank actions based on the expected value of the performance measures; and
8. sensitivity analyses.

Although decision analyses are not typically applied in such complex situations as in the Columbia River case, the workshop discussions revealed that such an approach would be highly appropriate.

Potential Application of Decision Analysis to the Columbia River

Figure 4.1 illustrates how one might apply the decision analysis concept to the Columbia River endangered species problem. The left-most column represents three alternative management strategies: a) full transportation; b) removal of three Snake River dams; and c) maximizing in-river survival using existing structures. Within each of these three strategies there are many alternative combinations of tactics that could be considered (e.g. A1, A2, A3). The next four columns represent alternative hypotheses about the level of survival through each of four life history stages. For freshwater spawning and rearing (FSR), the alternative hypotheses might be related to low, moderate, or high responsiveness to habitat improvements. For the juvenile migration corridor (JMC), the alternatives might be represented as different passage model structures and parameterizations. Alternative hypotheses regarding future estuarine and ocean survival (EOS) could be represented by different time series of * values, as used in the MLE framework (e.g. repetition of the 1952-1988 pattern, scaling that pattern up or down, and alternative cyclical hypotheses (e.g. Ware 1995)). Upstream passage (UP) could be represented as low, moderate or high levels of survival response to improvements in dams and reservoir conditions for returning spawners.

The final column in Figure 4.1, performance measures, consists of the set of outputs which would be calculated for each possible combination of the various life stage alternative hypotheses. Two obvious performance measures are the probabilities of survival and of recovery, using the definitions established in the Biological Requirements Workgroup Report. Another class of performance measures would be those related to the amount of learning achievable through one set of actions versus another. This could be represented as the change in the posterior probabilities of alternative hypotheses relative to the probabilities as they are at the present time.

Table 4.3 provides a simple example of the change in posterior probabilities with two hypothetical alternative management strategies. Strategy A provides for a greater change in the set of posterior probabilities than Strategy B as compared to the current situation. The actual situation is somewhat more complicated, in that one would be looking at the degree to which alternative strategies help to separate the distributions of posterior probabilities of alternative hypotheses.

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

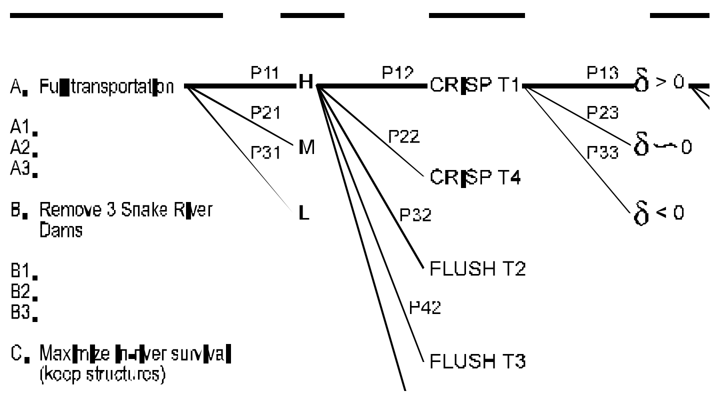


Figure 4.1: Schematic of decision analysis framework for Columbia River decisions **.**

Table 4.3: Change in posterior probabilities with alternative management strategies **. Strategy A provides for greater learning.**

Posterior Probability 1996	**Hypothesis #**	**With Strategy A**	**With Strategy B**
		Posterior Probability in 2006	**Posterior Probability in 2006**
0.2	C-T1	0.25	0.22
0.3	F-T1	0.2	0.31
0.25	C-T5	0.15	0.21
0.25	F-T7	0.4	0.26
3 = 1.0			

Though Figure 4.1 is useful conceptually, there are several unresolved questions which require further thought and discussion:

1. It is not yet clear how to ensure that the decision analysis framework clearly links to key management decisions, as reflected in the decision flowchart within Chapter 6 of the Preliminary Report on Retrospective Analyses. The simplest way to address this problem is to develop greater descriptive richness to the alternative management strategies.

2. In Figure 4.1, the probability of any one combination of hypotheses across the four life history stages is a product of the separate probabilities for each life history stage specific *hypothesis.* This raised some concern at the workshop because life stage *survivals* are generally thought to be correlated. For example, a high level of survival through the FSR probably increases the chance of higher survivals in the JMC and EOS phases. Since the alternatives within each life history stage are meant to represent alternative hypotheses, one way around this problem may be to include the level of dependence on previous life history stages as part of the hypothesis for any specific life stage. For example, specific versions of CRiSP or FLUSH could include in their estimate of passage survival a consideration of the antecedent freshwater spawning and rearing survival. For life stages which do not involve

alternative models, but rather more qualitative responses (e.g. low, moderate or high estuarine and ocean survival), one could change the set of probabilities applied at different branches of the decision tree. For example, if it is thought that better survival in the FSR stage leads to generally better survival in the EOS stage, one could change the components of the decision tree accordingly (e.g. attach a higher probability to high EOS survival in the branch originating from a high FSR survival than in the branch originating from a low FSR survival). A third way of dealing with the issue of covariance in life stage survivals is to represent alternative hypotheses regarding that covariance explicitly in the decision tree.

Dr. Peterman outlined three different approaches generally used in decision analysis to estimate the probabilities associated with alternative hypotheses:

1. *Estimate the probability of occurrence of a particular state of nature directly from time series.* Though this procedure works for well defined state variables such as flows (and indeed has already been applied in previous modelling exercises using long term flow records), it would not work for estimates of life stage specific survivals for which the historical record is very sparse.

2. *Subjectively assign probabilities to the alternative hypotheses.* This is unlikely to work in the JMC, where long-held differences in opinions will preclude consensus on subjectively assigned probabilities. It may be possible, however, for the FSR, EOS and UP life stages, with appropriate sensitivity analyses to reflect the uncertainty in the assigned values.

3. *Estimates of probabilities from data.* This is the only feasible approach for the JMC stage. A procedure for applying this approach is outlined below.

Subgroup A suggested the following approach to the assignment of posterior probabilities of alternative hypotheses, for at least the JMC stage:

1. A Data Evaluation Group would develop alternative data sets to use in assessing the accuracy of passage model predictions, and the relative weights to be assigned to those data sets. These would include: a) stock recruitment data, as integrated into the MLE framework; b) NMFS PIT-tag reach survival studies; and c) NMFS transportation to control ratio studies (see Table 4.4).

2. New versions of the passage models (e.g. CRiSP-T3, FLUSH-T5) would be developed incorporating alternative hypotheses, that attempt to reconcile the various types of evidence.

3. One would compute the probability of seeing the observed data given each alternative model (PR(data*model)). These posterior probabilities would be computed using a weighted log (likelihood) formula that uses the weight assigned in Step 1. One would use all the data for as many years as they are available. In this effort, one would assume uniform priors with respect to the alternative hypotheses, such that the probabilities would be entirely dependent on how well the models matched the different weighted data sets.

4. Complete a sensitivity analysis of the weighting factors. To do this objectively, one would need to develop a set of criteria for assigning the weights (e.g. relative accuracy of

measurements used to calculate survival, relevance of the spatial and temporal window over which survival is calculated).

Table 4.4: Structure of passage model testing proposed to estimate posterior probabilities **.**

Model	**Predictions**	**Data**	**Weighting**
CRiSP-T1 CRiSP-T2 CRiSP-T3 FLUSH-T1 FLUSH-T2 FLUSH-T3	µ ~S (NMFS reaches) ~TCR	1. MLE µ (S; R data)	W1
		2. NMFS PIT-tag studies	W2
		3. TCR studies	W3

In Subgroup B, Olaf Langness proposed to first focus the definition of "prospective aggregate hypotheses" and then apply the eight elements of Decision Analysis as presented by Randall Peterman. These elements would be used to tackle Goal C (i.e. "assess quantitative improvements in survival possible through various combinations of changes in 4 H's and climate, and associated risks to stocks"). He suggested that a prospective aggregate hypothesis was a proposed experimental management plan, with elements derived from belief systems held by the managers in each of the stressor categories (hydro, hatchery, harvest, habitat, and climate). Belief subsystems are contained within each of the five belief systems. As an example, when developing a hydro action package, one would choose actions based on beliefs held about different hydro action arenas (transportation, spill, flow....). When choosing between putting all fish into the barge, all fish into the river, or splitting the risk, one has preconceived notions of how survival will change in each of the life stages effected by hydro actions (JMC, EOS, PS). If you believe "barges kill fish", then you would expect an "all to the barge" transport option would show a drop in survival in the JMC life stage. Depending on your views about delayed mortality, you may or may not expect some decline in survivals in the EOS and/or PS life stages. Thus one might proceed along the path of first identifying the different action arenas for Hydro, Hatchery and Habitat management options. Then, for each of these action arenas, identify proposed or potential options. Finally, one could try to link work being done on the retrospective analysis with each of these options, as it identifies likely survival changes in associated life stages, or the probabilities associated with alternative hypotheses.

Subsequent to the workshop, Chris Pinney developed a much more detailed description of management strategies (Figure 4.2), which begins to move in the direction suggested by Olaf Langness. Further elaboration of action packages is required to harmonize the decision analysis approach with the details of the decision flowchart in Chapter 6 of the Retrospective Report.

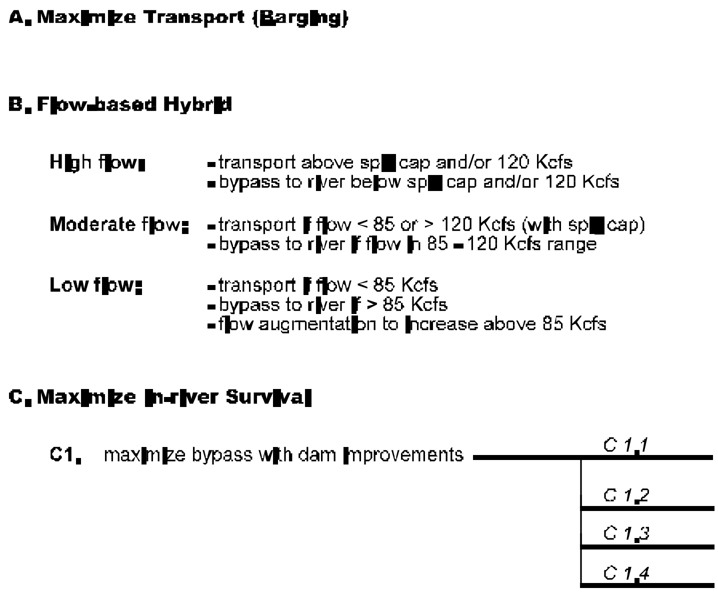


Figure 4.2: Detailed description of management strategies **.**

4.1.3 Maximum Likelihood Estimation Approach for Prospective Analyses

Rick Deriso summarized how the Maximum Likelihood Estimation (MLE) approach could be applied to prospective analyses. The first step is to take the framework used in the retrospective analysis and calculate the relative odds that μ takes on any particular value. This information is then placed in a forward simulating model that would calculate some quantity of interest (e.g. abundance in 30 years versus now). The model would be stochastic in nature to account for year-to-year variation in climate (*) and possibly other factors. Instead of assuming that μ's occurred as they did in the past we would develop a series of different management actions which would change μ in the future.

As discussed above for Decision Analysis, we still need to decide what performance measures will be used to quantify the results of the analyses. Rather than choosing only absolute indicators such as future abundance we could also focus on the probability of an event occurring (e.g. What is the probability that the stock will be above 0.90 of its current size at some specified time in the future?). Since each Columbia Basin stock has a unique recovery goal, separate modelling is required for each stock. A key question is "how much reduction in μ is required for a given stock to achieve its recovery?.

The MLE approach is completely consistent with the decision analysis approach described in Section 4.1.2. The MLE approach helps to set overall survival improvements required for each stock, and is one of the types of "data" used to compute the posterior probabilities of alternative passage models. The passage models themselves are required to implement the details of each management action package.

Some of the other important tasks will be to define what the goals are for recovery and survival of the stocks, and (as in the Decision Analysis approach) to determine which performance measures to use as a measure of goal achievement. For survival goals we may want to determine how many years in a stochastic simulation achieve or exceed some chosen goal, such as a having 95% of all future escapements above some pre-determined level. We may also want to determine the probability of achieving a stock recovery target that is two times pre-dam levels of abundance, and base analyses on 24-, 48-, and 100-year simulations. The Biological Requirements Work Group report sets some useful precedents for these performance measures. The simulation analysis must incorporate the variance associated with positive and negative environmental change. We want to choose recovery and survival goals that are robust to long-term variations in density independent influences (climate, environmental, and ocean changes). We also want to ensure that at some future environmental low the stock is not compromised by the entrenched management process / plan / structure. In general, we want to make sure that any management we do devise is conservative in favour of the fish populations, while at the same time attempting to maximize the rate of learning.

4.2 Sub-Group Discussions

Both subgroups had the same set of objectives. These were to: 1) review the goals for the prospective analyses; 2) brainstorm ideas on how to achieve these; 3) structure the ideas into a general work plan; and 4) provide ideas for future research and monitoring. Some of the key points in subgroup discussions related to the use of Decision Analysis have already been summarized in Section 4.1.2, and are not repeated here. The detailed goals of the Prospective Analyses (Table 4.1) were used to structure subgroup discussions; ideas from the two subgroups are presented together below. The participants of each sub-group are listed in Table 4.5 for reference.

Table 4.5: Participants of each sub-group **.**

Sub-Group A		Sub-Group B	
Dave Marmorek	Dan Bouillon	Larry Barnthouse	Ian Parnell
Howard Schaller	Peter Dygert	Ray Beamesderfer	Olaf Langness
Jim Geiselman	Lou Botsford	Charlie Petrosky	Paul Wilson
Earl Weber	Jim Anderson	Chris Toole	Rick Deriso
Randall Peterman	Tom Cooney	Al Giorgi	Chris Pinney
Brian Brown		John Williams	Charlie Paulsen
Steve Smith (Seattle)		Steve Smith (Portland)	

Though many interesting ideas for prospective analyses were generated at the workshop, a technical meeting needs to be held to further filter and consolidate these into a detailed analytical plan. A possible group for this would include: Dave Marmorek, Charlies Paulsen, Rick Deriso, Paul Wilson, Charlie

Petrosky, Chris Toole, Earl Weber, Howard Schaller, and Lou Botsford. Specific prospective tasks are presented in Chapter 5 of this report.

4.2.1 Goal A: Estimate Life Cycle Survival Required for Survival / Recovery Goals

Prior to conducting any prospective analyses, the MLE approach needs to be revised (i.e. Task 5.1 in Table 3.1). Subsequently there are a number of possible applications of the revised MLE approach to estimate required life cycle survival. These suggestions are listed below, with the subgroup name indicated:

- ***(A1)*** Use 1980 to 1990 µ (dam mortality) and * (environmental signal) by randomly selecting from this time series with replacement. Determine how much µ needs to be lowered to reach the objective survival and recovery goals. Explore different strategies for lowering µ (e.g. constant proportional reduction, avoid worst years by only lowering high µ values). Also consider the logic of pairing µ and * by year.

- ***(A2)*** Use data for µ and * for 1970 to 1990. Some dams were still under construction in the 1970s and operating procedures were less well developed, so there is some question about the appropriateness of the 1970s µ values as an indicator of future conditions. It was suggested that the best approach would be to use both time series (1970-1990 and 1980-1990) so that information about unique environmental conditions (low flow periods) during dam construction is not lost. By assessing the relationship between µ and WTT for both the 1970s and 1980s, one could estimate the µ expected in a 1973 or 1977 low flow year but with 1980s hydrosystem management.

- ***(A3)*** It may be appropriate to develop plausible future time series of * (e.g, increasing, decreasing, periodic with varying periods, and future trends based on the Ware (1995) prediction model, estimates from autocorrelation of * values for 1952-1989.). We must recognize, however, that the conclusions from the study of the influence of environment on fish populations using retrospective data is not definitive, and we cannot confidently predict the important environmental signal into the future. Therefore any management plan to protect endangered stocks must be resilient to unpredictable future changes in the environment. As the duration of environmental time series increases, so does the range of observed conditions; therefore the 1970-1990 time series will underestimate the variation expected over 48- or 100-year future periods.

- ***(A4)*** By incorporating µ as a function of WTT,. one could utilize the full 60-year record of flows and WTT. This would, however, require extrapolation of the µ - WTT relationship beyond the range of 1970 - 1990 data for which µ was estimated, and would add other error (i.e. the residual variation between the actual MLE-estimated µ for a given year and that estimated from a µ-WTT regression line). Alternatively, one could weight each year of the 1970-1990 period according to their frequency in the long-term flow record.

- ***(A5)*** It appears from Chapter 5 that there is a negative correlation between µ and *: high µ values during low flow years are associated with low (negative) * values indicating worse than average year effects. In the prospective analysis, it is important to assess the extremes of conditions (i.e. low μ and high * (best); high μ and low * (worst)).

C *(B1)* An alternative to manipulating μ is to simply project future population trends and variability as a function of changes in *total density independent mortality* (i.e. $a_1 - \mu_t + *_t$). This would provide a more comprehensive assessment of required total survival improvements, wherever they may be implemented. One could then calculate a composite probability comparable to "jeopardy" standards.

C *(B2)* Much work is required to develop data sets for fall chinook before one can apply the MLE method.

C *(B3)* We need to carry out analyses for individual stocks which will determine the probability that we will reach the escapement and recovery goals in some specified period of time. These analyses should be carried out with FLUSH/ELCM, CRISP/SLCM and also the MLE approach. In order to use the MLE technique we would need data on: 1) in-river mortality from recruits (define this) to the time at which they become escaped spawners; 2) age at return information; 3) frequency of water years (look at the relationship of F to water years). Rick Deriso may also be able to bypass the life cycle models with the MLE analyses. We need to compare the two methods of analyses and determine the future time periods of interest. A subgroup should meet to discuss the details of this analysis.

C *(B4)* We need to develop minimum survival goals for stocks that take into account the natural variability that is inherent in salmon populations, and which also recognize that threshold minimum survival goals will vary among different stocks because of their different productivities. The minimum survival goals must be based on a stochastic analysis that incorporates variation in estimates, and quantifies some specified low probability that the stock will not fall below the minimum threshold stock size.

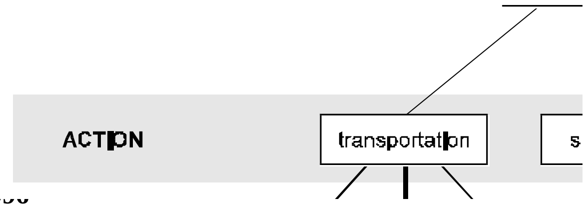
C *(B5)* Some of the approaches used in the Biological Opinion need to be revisited. The following issues have to be reviewed concurrently with the prospective analyses:

- C review criteria and reevaluate index period used in Biological Opinion;
- C thresholds, and probabilities of falling below thresholds;
- C defining recovery levels;
- C method of including depensation;
- C selection of index periods and "noise colour";
- C definition of thresholds and recovery levels for new stocks;
- C operational definition of improvement in survival.

4.2.2 Goals B and C: How to Achieve Survival goals and Estimate Quantitatively Possible Survival Improvements

Hydrosystem

Both subgroups discussed the hydrosystem. Two approaches were proposed to make prospective



projections: the Decision Analysis approach described in Section 4.1.2, and the “pie slice approach (i.e. deciding how much of the overall survival improvement (the pie) is allocated to different hydrosystem factors (the slices)). Both approaches involve selecting and defining a representative set of hydro system management options. The MLE approach described in Section 4.1.3 could be integrated with the Decision Analysis approach. We essentially want to control the mortality rate within the river system by choosing management options which produce different values for μ. From the prospective analysis view we need to ask the question “how much reduction in μ is required to achieve the specific goal for a particular index stock and management strategy?”. Howard Schaller pointed out that many components of both passage models are based on very little data and therefore it will be difficult to establish how much mortality is attributable to each action. The difficulty to this approach therefore becomes deciding how to slice the μ pie, and the complication is that there will be a probability associated with each slice of the pie (i.e. each survival improvement).

The MLE analysis can set the size of the μ pie for the prospective analysis. As a first approach to slicing the pie, we could vary the proportion of transported and non-transported fish, using the MLE method on historical data to get relative odds for each of the various transport hypotheses. One of the options for future actions will be to set μ=0 which is equivalent to removing the five dams (a screening analysis to see what recovery is possible). We can also use Raymond data from the 1960s to get estimates of survival under drawdown conditions. Finally, we can use the passage models and Bayesian analysis to evaluate changes in survival for the different hydro system management options (this is the same as the approach outlined for Decision Analysis in Section 4.1.2).

Though there are many details still to be resolved, we first need to define the family of management actions. Management options for the prospective analysis include:

1. reduce spill and continue transportation;
2. increase transportation only;
3. optimize flow for survival within the current hydro structure;
4. improve habitat;
5. extreme in-river actions (drawdown, dam removal); and
6. hybrid actions.

Figures 4.2 and 4.3 are two ways of categorizing management options.

Figure 4.3: Management options **.**

Harvest

Subgroup A raised the following points with respect to harvest of spring/summer chinook:

- Harvest rates currently stand at about 5-10% for spring chinook and about 2-3% for summer chinook. There is essentially no room to reduce harvest rates because these rates are for subsistence and ceremonial native fisheries. One task should be to document the estimated rate of harvest on spring, summer and fall chinook salmon in the Columbia River system.

- The prospective analyses should simulate non-zero harvest above some threshold abundance (i.e. as abundance increases harvest may increase in steps). The Harvest Group should develop two or three harvest / management scenarios which would incorporate different possible step functions. Although zero harvest may be simulated to determine what effect it may potentially have on rebuilding and survival goals, it is not likely politically feasible to reduce harvest rates below their current values.

- Fall chinook prospective analyses will require a wide range of harvest scenarios; these should evolve from the retrospective analyses in Chapter 13.

Habitat

Though discussion of habitat was specifically assigned to Subgroup B, both subgroups ended up discussing it. Some of the key points raised:

- ***(A1)*** Different stocks are natal to streams which have different potentials for improved habitat. Therefore, potential improvement in survival via habitat restoration will vary from stock to stock. Differences in stock-specific survival will have to be reconciled within the context of the prospective analysis.

- ***(A2)*** The time scale for improvement of habitat will vary from area to area but may be very long (e.g. 20-100 years). We need data from restored watersheds (e.g. change in smolts/spawner [or ln(R/S)]) to estimate the feasible improvements in survival over different time scales. With enough data we could estimate the change in the "*a*" parameter of the Ricker model over time. For example, the estimated Ricker "*a*" value for Poverty Flats is 2.56, a stream with poor FSR habitat conditions (Table 5-4 of Preliminary Retrospective Analysis report). Sulphur Creek and Minam, in excellent habitat, have "*a*" values of 2.95 and 2.91, respectively. Therefore, a very rough estimate of the maximum improvement in "*a*" possible due to habitat improvements is 0.35 - 0.39. This is considerably lower than 1.4, the average value of μ during the 1984-1989 period. Thus, even the best habitat improvements appear insufficient to overcome the mortality in the JMC, though of course every possible action may be required to move towards stock recovery. We should review all case studies of potential relevance, including model watersheds.

- ***(B1)*** If the conclusions drawn from the completed Chapter 4 analyses support the conclusions drawn from the Chapter 9 analyses, we must conclude that we cannot *detect* a habitat effect on survival. Note that intuitively we know that at some scale there must exist an important relationship between quality and quantity of habitat and the survival of salmon. The quality and quantity of data available for the last 30 years, however, may not provide us with the opportunity to determine the nature of this relationship. Note also that this assessment is based on an assumed fixed carrying capacity. Historical reductions in salmon abundance due to habitat loss may not be measurable in many cases because we do not have information on the historical (pristine) survival and carrying capacity of streams.

- ***(B2)*** Habitat conditions for squawfish and other predators are to be considered as part of the hydrosystem analyses, and not part of the Habitat analyses.

- ***(B3)*** Wild parr PIT-tag data (by year and stream) could be correlated with habitat quality measures to assess survival effects.

Hatcheries

- ***(B1)*** The focus of mitigation activities in the Columbia River basin is to increase the abundance of listed wild stocks. The utility of hatcheries to aid in this objective is somewhat limited to supplementation and augmentation of wild stocks. Although experiments with captive brood stock has begun there are no results yet available on adult returns. Traditional hatchery operations would likely only be used as a last ditch recovery plan for an endangered species.

- ***(B2)*** Do hatcheries have a negative effect on abundance of wild salmon and steelhead populations/stocks? The answer is likely to be “yes”. The question becomes “how significant is the effect?”. We need to carry out retrospective analyses before we can really proceed with prospective analyses. For example, where a hatchery has been introduced on a wild stock stream we can compare historical hatchery production to estimates of wild stock spawner abundance. We may determine that hatchery presence is associated with declining abundance of wild stocks. This correlation needs to be carefully interpreted, as hatchery efforts increased in response to declining trends in wild escapement, but it may provide an estimate of the potential improvement due to removing the hatchery.

- ***(B3)*** Prospective analyses should include exploration of new approaches (i.e. benefits of the proper use of supplementation).

- ***(B4)*** Assemble data on hatcheries which came on line during period covered by survival data (Warm Springs (75/77); S. Fork Salmon (~1981); Imnaha (mid-1980s); Lookingglass (mid 1980s)).

4.2.3 Goals D and E: General Ideas for Research and Monitoring

There was only a limited amount of time to discuss research and monitoring priorities at the workshop. Therefore, the following suggestions should be thought of as candidate proposals.

High Priority (next 6-8 years)

- Use decision tree analyses to priorize research and monitoring activities, provide objective rationale for decisions;

- Maintain index stocks throughout the Columbia River Basin;
- Carry out smolt marking: PIT tags for the lower river stocks, and smolt abundance indices for the spring / summer chinook stocks;
- Install safe PIT tag detectors at the Bonneville Dam;
- Calculate spawning and recruitment estimate errors;
- Calculate the survival of fish through the estuary and to adult returns, and the effect of timing of estuary survival on EOS survival;
- Monitor and record estuary conditions using a variety of useful habitat indicators;
- Get best possible estimate of flow-survival, FGEs, FPEs (the latter two for both Snake River and other dams);
- Collect data to determine the mechanism causing differential mortality between upstream and downstream stocks (i.e. differences in ln(R/S));
- Improve the collection efficiency of surface collectors, reducing fall chinook delays in the forebay;
- Carry out steelhead stock reconstructions (this will be more difficult to do than for spring/summer chinook because out-migration and life history is more complex/variable). Also should carry out PIT tag experiments for steelhead, and look at hatchery data to assess corridor survival and ocean survival for steelhead; and
- Look at hatchery data to assess corridor survival and ocean survival for chinook salmon.

Lower Priority

- Develop measures of fish condition (pre- and post-transport, and adult return) using scale and otolith studies (do retrospectively);
- Ensure that environmental variables are monitored in key index streams;
- Carry out more gas bubble disease experiments incorporating the suggestions of the different stakeholders; and
- Investigate the cause of migratory behaviour such as delays in migration in the reservoirs and initiation of migration.

Fall Chinook

In determining research and monitoring needs for fall chinook, participants described what data they would like to have available in 6 and 12 years. The following priorities emerged for the next 6-years:

- Allocate sufficient resources (staff, money) and management priority to update the fall chinook model.

- The installation of safe PIT-tag detectors at the Bonneville dam will eventually lead to survival estimates for the whole system. These estimates will be gathered after several years of operation. Timing information on movement of juveniles down through the system could be gathered over a shorter period of time. As the numbers of wild fall chinook are too low for reliable estimates of survival, it was proposed to use Lyons Ferry hatchery yearlings as a surrogate.

- Need to evaluate the data available for transportation by truck and by barge, and to carry out further analysis of Transportation to Control Ratios (TCR). Since fall chinook come at the end of the run, they are generally transported by truck, which is cheaper than barging, but very likely inferior.

- Coordinated PIT-tag planning needs to be developed among the National Biological Service, NMFS, WFW, and Tribal Groups. There is a need to concurrently evaluate transportation, survival and supplementation. Even with a coordinated plan, only two broods' worth of information will be available in six years.

By the end of 12 years, it would be reasonable to expect to have:

- reasonably clear sense of the controls on survival above the first dam, and the predation effect throughout (some hint of this after six years); and

- locations of freshwater spawning and rearing habitat, and the implications of a shift in timing of migration to a later period.

5.0 Prospective Analyses Preliminary Work Plan

Possible tasks are described in Chapter 4. Table 5.1 provides a preliminary set of tasks, and estimates of labor days associated with each task. Table 5.2 provides a summary of total days allocated to each individual. The task and labor estimates are currently being reviewed and revised by the Planning Group.

Table 5.1: Prospective analyses workplan - time allocation **.**

Task List	Complet'n Date	Persons Responsible	Days of Effort	Break-down of Effort into Days per Month					
				Apr.	May	Jun.	Jul.	Aug.	Sep.
Goal A: Required improvement in life cycle survival									
A1: Produce a written description of the proposed methods for MLE, ELCM, and SLCM.	for July 9 meeting	**Deriso** Schaller Paulsen Marmorek	2 2 2 2				2 2 2 2		
A2: Implement the methods and develop preliminary results for MLE, ELCM, and SLCM. C applied to spring / summer chinook	20-Jul	**Deriso** Schaller Paulsen Marmorek Parnell	10 10 10 10 10				10 10 10 10 10		
A3: Examine the covariance of µ and *. Compare µ versus WTT for the 1970s and 1980s to get low flow µ with 1980s' management. (Task 5.1)	31-May								
A4: Review and re-evaluate survival and recovery criteria and produce a summary useful for the prospective analyses. (Task 6.2) C Develop recovery criteria for new stocks useful for the prospective analyses	31-May	**Toole** Petrosky Langness Schaller	5 10 5 5		5	 10 5 5			
A5: Review and re-evaluate depensation issues and produce a summary of results for prospective analyses	09-Jul	**Paulsen**	5			5			
A6: Preliminary results for fall chinook.		**Langness** Bouillon Schaller Paulsen	10 10 10 10					10 10 10 10	
Goals B and C: Decision Tree, Aggregate Hypotheses of Survival Improvements									
B1: Organize information on potential improvements in habitat and hatchery effects by stock C Compile examples of habitat improvement using estimated changes in the parameters of the Ricker model and the time scale.		**Petrosky** Langness Beamesderfer Wilson Pinney Parnell Geiselman Lee	20 10 10 10 20 20 10 20				10 5 5 5 10 10 5 10	10 5 5 5 10 10 5 10	
B2: Develop alternative future scenarios of marine survival.		**Anderson** Bouillon Botsford	5 5 5					5 5 5	

B3: Document the estimated harvest rates (methods) and develop a wide range of step functions for use in the prospective analyses.		**Cooney** Dygert Bouillon	5 5 5				5 5 5		
B4: Develop alternate passage model parameterizations (CRISP, FLUSH, PAM (?)). C Use the MLE, TCR, and the PIT-tag in-river survival data to calculate posterior probabilities.		**Wilson** Schaller Anderson Deriso Marmorek	20 20 20 5 10				10 10 10	10 10 10 5 10	
B5: Data Evaluation Group develop criteria for weighting data sets and assigning weights.		**Wilson** Schaller Anderson Weber Giorgi	5 5 5 5 5			5 5 5 5 5			
B6: Make revisions to the MLE model and rerun prospective analyses to explore alternative prospective aggregate hypotheses. C Write up Goal B/C results.		**Deriso** Marmorek Bouillon	10 10 5					10 10 5	

Table 5.2: Prospective analyses: summary of effort days by individual **.(#) = approximate working days/months. About 40 working days until June 15.**

Person	Break down of Effort into Days per Month*						
	Total	**Apr. (7)**	**May (23)**	**Jun. (20)**	**Jul. (23)**	**Aug. (22)**	**Sep. (21)**
Anderson	35	0	0	5	20	10	0
Barnthouse	0	0	0	0	0	0	0
Beamesderfer	10	0	0	0	5	5	0
Botsford	5	0	0	0	5	0	0
Bouillon	35	0	0	5	15	15	0
Brown	0	0	0	0	0	0	0
Cooney	5	0	0	0	5	0	0
Deriso	27	0	0	0	12	15	0
Fisher	0	0	0	0	0	0	0
Geiselman	0	0	0	0	0	0	0
Giorgi	5	0	0	5	0	0	0
Langness	25	0	0	5	5	15	0
Lee	20	0	0	0	10	10	0
Marmorek	37	0	0	5	12	20	0
Norris	0	0	0	0	0	0	0
Parnell	30	0	0	0	20	10	0
Paulsen	27	0	0	5	12	10	0
Peterman	0	0	0	0	0	0	0
Petrosky	30	0	0	10	10	10	0
Pinney	20	0	0	0	10	10	0
Reiman	0	0	0	0	0	0	0
Rhodes	0	0	0	0	0	0	0
Saila	0	0	0	0	0	0	0
Schaller	52	0	0	10	22	20	0
Smith - Portland	0	0	0	0	0	0	0
Smith - Seattle	0	0	0	0	0	0	0
Toole	5	0	5	0	0	0	0
Weber	5	0	0	5	0	0	0
Williams	0	0	0	0	0	0	0
Wilson	35	0	0	5	15	15	0

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

6.0 References Cited

Hilborn, R., E.K. Pikitch, and M.K. McAllister. 1994. A Bayesian estimation and decision analysis for an age-structured model using biomass survey data. Fisheries Research 19:17-30.

Langness, O.P., C.E. Petrosky, H.A. Schaller, and R.C.P. Beamesderfer. 1996. Spawner-recruit relationships for spring/summer chinook salmon populations in several Columbia and Snake River subbasins. (in prep.)

Marmorek, D.R., I. Parnell, L. Barnthouse, and D.R. Bouillon. 1995. Plan for Analyzing and Testing Hypotheses. Results of a workshop to design retrospective analyses. Prepared by ESSA Technologies Ltd., Vancouver, BC for Bonneville Power Administration, Portland, 71 pp. and appendices.

Marmorek, D.R., L.P. Parnell, and D. Bouillon (eds.). J.J. Anderson, L. Basham, R. Deriso, L. Garrett, A. Giorgi, O.P. Langness, C. McConnaha, C. Paulsen, C.E. Petrosky, H.A. Schaller, C. Toole, E. Weber, P. Wilson, and R.W. Zabel. 1996. Plan for Analyzing and Testing Hypotheses (PATH): Preliminary report on retrospective analyses. Prepared by ESSA Technologies Ltd., Vancouver, BC.

McAllister, M.K. and R.M Peterman. 1992. Decision analysis of a large-scale fishing experiment designed to test for a genetic effect of size-selective fishing on British Columbia pink salmon. Can. J. Fish. Aquat. Sci. 49:1305-1314.

Sainsbury, K.J. 1988. The ecological basis of multispecies fisheries, and management of a demersal fishery in tropical Australia. In: J.A. Gulland (ed.) Fish Population Dynamics. 2nd Edition. John Wiley And Sons Ltd, pp. 349-382.

Walters, C.J. 1981. Optimum escapements in the face of alternative recruitment hypotheses. Can. J. Fish. Aquat. Sci. 38(6):678-689.

Walters, C.J. 1986. Adaptive Management of Renewable Resources. MacMillan, NY, 374 pp.

Ware, D.M. 1995. A century and a half of change in the climate of the NE Pacific. Fish. Oceanogr. 4(4) 267-277.

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

Appendix 1

Selected and Annotated References on Decision Analysis for Management of Natural Resources

Randall M. Peterman
School of Resource and Environmental Management
Simon Fraser University Burnaby, B.C., CANADA V5A 1S6

20 April 1996

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

The annotated references below constitute only a minor portion of the published papers on the topic of decision analysis in management of natural resources. However, they should provide a useful start into the literature for PATH members. Papers were chosen based in part on their readability for those who have little background on the topic of formal decision analysis.

General Readings on Decision Analysis

Howard, R.A. 1988. Decision analysis: practice and promise. Manag. Sci. 34:679-695.
[a good discussion of merits of decision analysis but not nearly as comprehensive of a paper on the topic as Keeney (1982) below]

Keeney, R.L. 1982. Decision analysis: An overview. Operations Research 30(5):803-838.
[a very good general introduction to decision analysis]

Morgan, G. and M. Henrion. 1990. Uncertainty: A Guide to Dealing with Uncertainty in Quantitative Risk and Policy Analysis. Cambridge Univ. Press, 332 pp.
[covers a wide range of topics concerning uncertainties in decision making]

Render, B. and R.M. Stair. 1988. Decision trees and utility theory. pp. 144-163, In: Quantitative Analysis for Management. Boston: Allyn and Bacon Inc.
[includes a worked out introductory example of decision trees in business and utility analysis (a method for taking several different conflicting objectives into account simultaneously)]

Examples Where Decision Analysis Was Used in Fisheries

Frederick, S.W. and R.M. Peterman. 1995. Choosing fisheries harvest policies: when does uncertainty matter? Can. J. Fish. Aquat. Sci. 52(2):291-306.
[applies decision analysis to choosing the appropriate level of precaution in setting fish harvesting strategies of marine fish populations]

Hilborn, R., E.K. Pikitch, and M.K. McAllister. 1994. A Bayesian estimation and decision analysis for an age-structured model using biomass survey data. Fisheries Research 19:17-30.
[applies decision analysis to evaluating different harvesting regimes for a marine fish population in New Zealand]

Ianelli, J. and J. Heifetz. 1995. Decision analysis of alternative harvest policies for the Gulf of Alaska Pacific Ocean perch fishery. Fish. res. 24:35-63.
[applies decision analysis to choosing harvesting regimes]

Lord, G.E. 1976. Decision theory applied to the simulated data acquisition and management of a salmon fishery. Fish. Bull. 74:837-846.
[one of the early papers to apply decision analysis to a fisheries problem]

McAllister, M.K. and R.M Peterman. 1992. Decision analysis of a large-scale fishing experiment designed to test for a genetic effect of size-selective fishing on British Columbia pink salmon. Can. J. Fish. Aquat. Sci. 49:1305-1314.
[uses decision analysis to choose among several alternative designs of an adaptive management experiment on pink salmon. Should be read after its companion paper McAllister et al. (1992). This paper did not use Bayesian methods to calculated posterior probabilities of the alternative hypotheses but instead used a subjective probability distribution and did a sensitivity analysis on the rank of actions to changes in that distribution.]

McAllister, M.K., R.M. Peterman and D.M. Gillis. 1992. Statistical power analysis of large-scale fishing experiments designed to test for a genetic effect of size-selective fishing on British Columbia pink salmon. Can. J. Fish. Aquat. Sci. 49:1294-1304.
[different designs of adaptive management plans were evaluated based on statistical power, the probability of correctly determining the presence of high heritability in body size, a prerequisite for the size-selective fishing hypothesis to explain the temporal decrease in body size of adult pink salmon]

Parma, A.M. and R.B. Deriso. 1990. Experimental harvesting of cyclic stocks in the face of alternative recruitment hypotheses. Can. J. Fish. Aquat. Sci. 47:595-610.
[evaluates different active and passive adaptive harvesting policies for Pacific halibut given uncertainty about the underlying population dynamics. Uses dynamic programming (an optimization method) to find the optimal policy efficiently, incorporating decision analysis as a subset of the analysis.]

Sainsbury, K.J. 1988. The ecological basis of multispecies fisheries, and management of a demersal fishery in tropical Australia. In: J.A. Gulland (ed.) Fish Population Dynamics. 2nd Edition. John Wiley And Sons Ltd, pp. 349-382.
[shows how decision analysis was used to evaluate several alternative adaptive management schemes before one was finally chosen and implemented. Unfortunately, this paper is extremely dense reading; I do not recommend that you start with it!]

Sainsbury, K.J., R.A. Campbell, R. Lindholm, and A.W. Whitelaw. 1995. Experimental management of an Australian multispecies fishery: Examining the possibility of trawl-induced habitat modification. 16 pp. typed manuscript, in press in the proceedings of the fisheries symposium held in Seattle in June 1994 (still not published as of April 1996).
[shows the benefit of applying the adaptive management scheme identified as the best one in Sainsbury (1988). Since the adaptive management plan was implemented in 1985, new data have been generated and a much higher Bayesian posterior probability has now been calculated for one of the 4 main hypotheses about how the dynamics of the groundfish community work, which in turn led to new management regulations.]

Walters, C.J. 1975. Optimal harvest strategies in relation to environmental variability and uncertainty about production parameters. J. Fish. Res. Board Can. 32(10): 1777-1784.
[one of the first papers to apply decision analysis to a fisheries problem. Uses dynamic programming (an optimization method) to find the optimal policy more efficiently, incorporating decision analysis as a subset of the analysis.]

Walters, C.J. 1977. Management under uncertainty. In: D. Ellis (ed.), Pacific Salmon: Management For People. pp. 261-297.

[applies decision analysis to evaluating different plans for enhancement of salmon populations]

Walters, C.J. 1981. Optimum escapements in the face of alternative recruitment hypotheses. Can. J. Fish. Aquat. Sci. 38(6):678-689.
[uses dynamic programming to evaluate different harvesting policies, including adaptive management schemes, when the stock-recruitment curve is not well known]

Examples Where Decision Analysis Was Used in Forestry, Wildlife, or Other Fields of Resource Management

Cohan, D. S.M. Haas, D.L. Radloff, and R.F. Yancik. 1984. Using fire in forest management: decision making under uncertainty. Interfaces 14(5):8-19.
[a good simple example of decision analysis; I recommend that you start with this one!]

Lave, L.B. and H. Dowlatabadi. 1993. Climate change: The effects of personal beliefs and scientific uncertainty. Environmental Science and Technology 27(10):1962-1972.
[a fairly complex problem but a non-quantitative description of a decision analysis]

Maguire, L.A. and L.G. Boiney. 1994. Resolving environmental disputes: A framework incorporating decision analysis and dispute resolution techniques. J. of Environmental Management 42:31-48.
[a very theoretical but potentially relevant application of decision analysis to resolution of conflicts between interest groups in wildlife management]

Parkhurst, D.F. 1984. Decision analysis for toxic waste releases. J. Environmental Management 18:105-130.
[a fairly readable introductory application of decision analysis to choosing the site of a toxic waste dump].

Reckhow, K.H. 1994. Importance of scientific uncertainty in decision making. Environmental Management 18(2):161-166.
[talks about the importance of taking uncertainties into account in management of water quality]

Bayesian Statistics

Howson, C. and P. Urbach. 1993 (either the 1989 or 1993 edition). Scientific Reasoning: The Bayesian Approach. Open Court Publ. Co., La Salle, Illinois.
[discusses all sides of the debate between Bayesian and classic statisticians; the authors are Bayesians and the book is moderately technical]

Experimental or Adaptive Management

McAllister, M.K. and R.M. Peterman. 1992. Experimental design in the management of fisheries: a review. N. Amer. J. Fish. Management 12:1-18.
[reviews the literature on adaptive or experimental management]

Peterman, R.M. and M.K. McAllister. 1993. A review of the experimental approach to reducing uncertainty in fisheries management-- an extended abstract. Can. Spec. Public. Fish. Aquat. Sci. 120:419-422.
[reviews the literature on the topic of adaptive management and discusses it in the context of reducing risk and uncertainties]

Walters, C.J. 1986. Adaptive Management of Renewable Resources. MacMillan, NY, 374 pp.
[a moderately technical but fairly readable and very comprehensive treatment of adaptive management and Bayesian decision analysis]

Also see the Parma and Derison (1990) and Walters' references above.

Appendix 2: Supplementary Material on Chapters 2 and 4

Chapter 2:
The Snake River in the Context of Broad Scale Patterns of Change in Stock Indicators:
A Level 1 Pilot Analysis

Chapter 4:
Stressors and Life History Stages Correlated with Patterns of Change in Stock Indicators:
A Pilot Demonstration of a Multivariate Analysis Approach

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

Reviewers Comments - Section 2

Methods:

- non-parametric trend testing
  - Mann-Kendall
  - Sen's non-parametric slope estimator
- additional clustering methods:
  - non-parametric clustering methods
    - Multidimensional scaling
    - Analysis of similarity
  - hierarchical methods
- additional correlation methods
  - Nei diagrams of Pearson correlations
  - Kendall's Tau
- additional discriminant analysis methods
  - linear
  - log-linear

Data:

- more details on derivation of data, especially recruitment
  - S_{t+4} vs S_t
  - Ln (S), Ln (R/S), Ricker residuals, detrended (S)
  - check on serial correlations in R

Reviewers Comments - Section 4

Additional/Different Methods:

- "appropriate" transformations, not 0/1
- non-parametric polynomial regression
- neural network approach
- GLM
- recruit-at-age estimates may not be independent; multiple stock model more promising
- inclusion of additional stressors hypothesis-driven

- careful of spurious correlations

Stock-Recruit Data Status

- Reasonably good escapement and environmental data on 42 stocks
- Approximately 25 stocks have completed run reconstructions
- Four stocks promised by H. S. and Co.
- Thirteen run reconstruction looks feasible

Environmental Data Status

Time Series Data:

- spawning and migration flows
  - monthly minimum, mean, maximum
- snowpack
  - monthly total (snow water equivalent)
- dams in and above migration corridors
  - height, capacities, etc. by year
- ocean indices (monthly)
  - upwelling
  - NPI
  - ALPI
  - etc.

Static Data:

- geographic locations
- landforms, geological classifications
- distance from spawning ground to ocean

"Apparently" Static Data:

- irrigation diversions

- mines

Missing Data:

- timber harvest
- grazing
- agricultural use
- etc.

Appendix 3: Responses to Reviewers' Comments on Chapter 3

Chapter 3:
Contrasts in Stock Recruitment Patterns of Snake River and Columbia River Spring / Summer Chinook Populations: Draft Pilot Study

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

Response to comments:

3.0 Contrasts in Stock Recruitment Patterns of Snake River and Columbia River Spring/Summer Chinook Populations

1. *C. Walters review*: Concerned over appearance of density dependence strengthening in latter years.

Comment: The suggestion that density dependence may have strengthened during the latter periods is from inspection of ln(R/S vs S) plots, where the mid-1980s low escapement produced close to expected recruits for the Snake River stocks. These observations tend to increase the slope for the post-1974 data, because all other brood years had substantially poorer than expected recruitment. The apparent strong density-dependence of the post 1970 brood years are driven by high density independent mortality of the hydrosystem in the mid to late 1970s and good conditions in the mid 1980s. This appears consistent with the pattern of mu's from the chapter 5 results. In addition, not all stocks appeared to have expressed stronger density dependence in the post 1974 data (Entiat, John Day, Warm Springs and Aggregate).

Possible severe bias in R/S due to incorrect R (substantial R when no S).

Comment: The R was not obtained by proportionally dividing total catch to subbasins, as described by the problems with the Fraser. Abundance and catch information by stock type is available and lower river fisheries harvest rates were estimated for spring and summer chinook separately. Spring and summer chinook stocks in the Snake Basin showed similar R/S patterns. In addition, over time harvest rates were greatly reduced and R/S ratios at spawning levels declined.

R/S data show no pattern corresponding to habitat change.

Comment: The Poverty Flat stock, which was heavily impacted in the mid 1960s, exhibits a different pattern than the other Snake stocks. For a number of the other index areas, habitat changes occurred long before of R/S data began. A good suggestion to put some visual indicators on the graphs when habitat changes occurred.

Can you assemble some information on ocean distribution?

Comment: There is very little ocean CWT recovery information on upper Columbia Basin stream type chinook. Mid-Columbia and Snake River stream type chinook exhibited less than a 1% CWT ocean recovery rate (Berkson 1992 ESA status review).

Applying AOV will cause a problem in determining degrees of freedom.

Comment: The measure we were investigating was LN(Observed (R/S)/Pred(R/S)) which does not contain the same problems as abundance or spawners. There are problems with the AOV portion of the analysis, but as Lou Botsford states in the end we are not going to prove anything statistically. We are only marshaling another piece of evidence to weigh with the other pieces from the other analyses.

Is it true Snake River stocks maintained productive pre- 1970s? How many substocks have been lost from the upper system 50-70 period?

> *Comment:* The aggregate spring chinook stock showed maintenance of a productive upriver run from 39-69 (Figures 19 and 32). This aggregate would be composed of all spring chinook stocks spawning above Bonneville dam. The aggregate pattern is consistent with most of the Snake River index stocks. The Snake River stocks contributed at a much higher proportion to the Bonneville count in the pre 1970 period. The index populations used in the analysis have consistent spawning escapement trends with a larger set of stocks in the Snake Basin. There was not much loss of stocks during this time period (1960s), with the exception of the stocks above Hells Canyon lost due to blockage to spawning area (not a large component) and Panther Creek due to mine effluent.

2. *S. Saila review:* Concern over using linear fitting procedures for recruitment function.

> *Comment:* The use of a non-linear fitting procedure is a worthwhile task. However, based on previous work using non-linear procedures we don't believe it will have a large effect on the survival indices or change the pattern of survival indices. This would be a bigger concern if we were using this information to set escapement or harvest rate goals.

It is suggested to use formal tests of the slopes of the pre-70 versus post 74 data.

> *Comment:* This is a good suggestion and plan to explore the reviewers suggested approaches.

3. *J. Collie review:* Concern about allocation of harvest to recruitment.

> *Comment:* see comments for C. Walter on same topic.

Replacement level is confusing terminology.

> *Comment:* The only purpose for using the escapement at equilibrium stock size ("replacement") was to standardize the escapement trends among stocks. Nothing more was meant by the assessment and sorry for the confusion. In the next iteration we will provide escapements normalized to average values.

Graphs suggest reduction in habitat since 1970 (change in density dependence).

> *Comment:* Same as for C. Walter about increasing density dependence.
> The suggestion of more fully exploring hypotheses with a general linear model approach is a very good suggestion. This is an area we plan to further investigate and incorporate in our work plan. Also note Collie's review of chapter 9 results, which " seem to refute the suggestions I made on chapter 3..."

4. *Paulsen review:* Marsh Creek and other index stocks were missing redd counts for 5 years or so?

Comment: Not true. There is no missing redd count data for any of the Snake River index stocks. The criteria for selection was no interruption in the escapement time series. (see Petrosky et al. 1995).

Appendix 4
Responses to Reviewers' Comments on Chapter 9 (Charlie Petrosky) and Post-Workshop Comments on Chapter 9 (Al Giorgi)

Chapter 9:
Evaluation of Survival Trends in the Freshwater Spawning and Rearing Life Stage for Snake River Spring / Summer Chinook

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

Responses to Reviewers' Comments on Chapter 9 (Charlie Petrosky)

9.0 Evaluation of Survival Trend in the Freshwater Spawning and Rearing Life Stage for Snake River Spring/Summer Chinook

Review Comments:

1. Dennis review: "great chapter", factual results will significantly constrain future explanations of salmon stock declines. No changes suggested.

2. Saila review: suggested non-parametric 2 sample t-tests, instead of parametric.

   Comment: see Collie and Walters suggested statistical tests also.

3. Paulsen duplicated results for 62-82 (ln(Sm/Sp) vs. Sp). Paulsen Fig. 2 shows x-axis as redds, not potential wild spawners.

   Comment: source of redd counts not identified; but his Fig. 2 on ln(smolts/redd) vs. redd shows similar pattern as chapter 9 analysis. This appears to be corroborating information for independent variable.

   Paulsen Fig. 3 shows linear plot of wild smolts vs. wild spawners. Paulsen suggests that carrying capacity during 1962-82 may not have been limiting [aggregate] production; and that one could reasonably also infer that up to at least 40-50K dam count (well above de-listing criteria).

   Comment: The above inference explicitly assumes the linear fit of smolts and spawners (as opposed to a linear Ricker form). There is a problem with the linear fit of smolts and spawners, in that the intercept significantly exceeds zero (my exploration of this regression yields 807K smolts at zero spawners; 95% CI range, 295K to 1,319K). If the regression is forced through zero, the residual pattern becomes suspect (all escapement values < 15K spawners have positive residuals in this case). These two problems are avoided by use of the Ricker form (ln(Sm/Sp) vs. Sp).

4. Collie review: Quite convincing evidence that there has not been a decrease in survival since FCRPS completion. Could be more formally tested with ANACOVA, test for equality of slopes & intercepts.

   Comment: see statistical suggestions of Saila & Walters.

   Collie states that results refute his chapter 3 suggestion that carrying capacity has changed due to loss of habitat.

   Comment: The chapter 3 suggestion that B may have changed may have come from inspection of the ln(R/S) v. S plots, where the mid-1980s low escapements produced close to expected recruits for the Snake River stocks. These observations tend to increase the slope for the post-1974 data, because all other brood years had substantially poorer than expected recruitment.

Collie also states that there is no evidence for depensatory survival through BY 1993. Is there depensation at other life stage?

Comment: May need to reword the paragraph to state that evidence for depensation is weak up to the 4.8K aggregate spawners. Cautionary notes include the BRWG theoretical basis for depensation (a single adult at LGR or in any tributary will produce zero smolts); need for conservative approaches to risk assessment; and mixed stock phenomena (stronger stocks will dominate the aggregate pattern, and could lose more vulnerable stocks without detection).

Collie also stated that it would be possible and interesting to examine relationship between smolts and returning adults between the two time periods.

Comment: Agree, with certain age structure assumptions. Estimates of spawner-to-smolt and smolt-to-adult survival for the aggregate population would be valuable also in basin-wide M&E program.

5. Walters review: Broad implications regarding role of habitat management in restoration. Surprised to see such high smolt/spawner ratios (>100). Density independent egg- smolt survival doesn't appear to be limiting Snake populations.

Comment: Could this analysis be repeated for other species and stocks around the basin? Could there be a comparable analysis for steelhead?

Comment: Aggregate spawner and smolt data should be incorporated into M&E in the Snake and other parts of basin; separating natural and hatchery fish may prove difficult for existing data. Analysis of steelhead data will be more difficult because smolt age varies by population. All Snake River hatchery steelhead have been ad-clipped since the mid-1980s, so it may be possible to have a longer time series than was possible for spring/summer chinook.

Walters points out an apparent contradiction between chapters 8 and 9: that lower than expected numbers of smolts were reported reaching the upper project in 1973 and 1977, but the pattern is not apparent in chapter 9 analysis. Also, whether there is a flow-dependent bias in smolt estimates?

Comment: 1977 was identified as having lower than expected numbers of smolts at LGR by Sims, Bentley and Johnson (1978), not aware of a similar 1973 citation. There is evidence from this analysis that fewer smolts reached the upper project in the low flow years. Residuals for the 4 lowest flow years (1973, 77, 92, 94) were negative for all but 1973, and 1977 had the largest negative residual. Flow dependent bias in smolt estimates is possible. For example, violation of the homogeneity assumption in Seber-Jolly estimates potentially could be more problematic at lower flows, since fish are moving slower and may exhibit greater physiological change between projects, relative to higher flow years. Estimated detection probabilities were substantially lower in 1994 compared to 1993 and

1995 in periods without spill (it has not been resolved whether the low 1994 detection probabilities were real, or reflect a violation of assumptions). This analysis treated the question by use of a range of FGEs.

Walters questioned whether a full accounting was made of fry outplants (in R) and hatchery withdrawals (in S).

> *Comment:* We'll check on fry outplant numbers. Hatchery withdrawals are accounted for in the TAC estimates, however, the question of adult outplants, or passing hatchery origin fish to spawn in the wild, will be checked into.

Walters question about accuracy of wild smolt proportion for recent years: is misclassification of hatchery smolts as wild likely, due to disproportionate mortality of marked vs. unmarked hatchery fish?

> *Comment:* Not likely. Since 1993, all hatchery smolts were adipose clipped; the proportion estimated at the upper dam does require an assumption about equal survival between marked and unmarked fish. 1992 estimate requires accurate assessment from scale pattern analysis, which appears to have met.

Walters noted that because of the confounding of changes in estimation methods, a pre- and post-1975 statistical comparison isn't warranted, since we cannot decide the cause of any difference."Admit the S/year effect confounding, and restrict further analysis to evaluation of possible problems with recent R data".

> *Comment:* This is a basic question of data qualifiers that has broader implications than chapter 9. It still seems appropriate to test for differences, with t-test (parametric or nonparametric) or ANACOVA (c.f., Saila and Collie reviews). The potential confounding should be noted, but the use of range to bracket recent smolt estimates should satisfy the concern. The basic conclusions were not sensitive to the range of reasonable FGE assumptions.

Walters notes that the analysis not "ambiguous" to depensation at large spatial scales. There isn't any ,or else R/S would show pronounced drop. Recollection of Snake redd count indices showing parallel declines, and no evidence of "crash" following a low escapement.

> *Comment:* See Collie review. Note also that redd count index areas were established for the major populations, which, in theory, would be the among the last to crash from depensatory pressures.

Walters notes evidence of strong compensatory response at low S (S decreased by order of magnitude while R decreased by factor of 2). Can you check pattern using historical data on fry/juvenile rearing density or index density over time within streams? Should see 1) no big drop in rearing density over time within streams and 2) similar rearing density or index densities across streams that have quite different ratios of rearing habitat to spawning area.

> *Comment:* Historical juvenile density information is very limited pre-1985.

> IDFG has monitored juvenile densities since 1985. Densities tend to track with escapements, similar to the pattern seen in the chapter 9 analysis (e.g., Fig. 3 in Petrosky and Holubetz 1986 and Fig. 9 in Scully and Petrosky 1991).

Walters notes problem with the analysis' applicability to stock rebuilding programs in the Snake as a whole. Presumably Snake could: a) produce more than in the 60s; b) a lot of substocks could have been depleted/impacted before; and c) ones which did not show strong compensation would have been eliminated entirely. Analysis should not be interpreted as indicating that there is little capacity to increase/restore juvenile populations sizes in Snake as a whole. Should review evidence concerning loss of sub-stocks and stock structure that may have occurred before 1960.

> *Comment:* This potential problem is with interpretation, not the analysis (chapter 10 may be place to address in more detail?). Agree with points a) and b), and there is historical evidence to support both (e.g., Fulton 1968). Point c) makes sense theoretically; also populations from less productive habitat or "sink populations" would be more vulnerable to density independent mortality. See also chapter 3, R/S for aggregate upriver run.

Walters specific comment, that we should make it top priority to apply methodology to other stocks, particularly steelhead, within the Columbia River Basin. Major implications particularly for habitat management planning and for various ideas about whether hatchery supplementation makes sense.

> *Comment:* First part addressed above. Second implications may be more appropriate in chapters 10 & 11?

Post-Workshop Comments on Chapter 9 (Al Giorgi)

From: Al Giorgi
Subject: Post Workshop Comments on chapter 9
Date: May 5, 1996

Some of these issues we have already discussed, but there are a few more that may require consideration.

I. Prespawning Survival:

As we discussed, their assumption that prespawning survival has remained constant since the 1960s may be a point of discussion. Construction of the Snake River dams began in 1962, with increasing impacts through to completion of the dams in the mid-1970's. If dam passage debilitates or compromises adults, then reasonably prespawning mortality would be expected to increase since completion of the projects. Considering also that pinneped populations have increased substantially and the incidence of strike marks at LGR is pronounced in recent years, then the argument for increased prespawning mortality in recent years seems even more likely. Conversely, if prespawning mortality has indeed remained constant, then it is difficult to argue that adult passage survival has been reduced with the installation of more dams. The conclusion seems to be that adult passage effects are not a concern either in the past or in the future.

With respect to these analyses, if prespawning mortality has increased in recent years, then they overestimated the # of wild spawners and underestimated smolts/spawners. In this situation their relationships depicted in figures 1, 2 would become more pronounced indicating even more intense density dependent effects in recent years.

If productivity (smolts/spawner) has increased in recent years at reduced spawner #s, it suggests that at the spawner levels occurring prior to 1974 may have been approaching k. Does this comport with estimates of carrying capacity reported in the Subbasin Plans?

II. Wild Spawner Estimates:

This is one of several pivotal estimates in this analysis. As described on p 9-3, the TAC estimates of wild fish @ LGR rely on backing out the hatchery run size each year. Rack return # and a prespawn mortality rate are used to estimate the # of hatchery fish @ LGR, a constant 80% prepsawn mortality is applied each year to hatchery fish. However, Chapman et al. (1991) cite a number of investigations that indicate prespawning mortality to be quite variable (see pages C-1 to C-5 that report. It would be instructive if the authors could discuss the ramifications of this to their analyses. Also, is there any evidence that in years when spawners are abundant excess hatchery spawners are locked out, if this occurred it would serve to underestimate the rack returns and overestimate the wild component. It would be instructive to know whether this practice occurred. If this occurred the estimates of smolts/spawner could be affected, particularly in the early period when spawners were abundant. If the estimated wild population was actually comprised of wild fish and hatchery lockouts, and hatchery fish are less productive when spawning and rearing in the wild then the smolts/spawner would be expected to be reduced. I have no idea if lockout occurred, but since it is a possibility it seems like it deserves investigation.

III. Smolt Estimates:

PATH Workshop 2
May 22, 1996

There is a large gap in smolt # estimates, 1983-1989. Why couldn't the authors apply Raymond's method to those years?

Table 7- Given that this estimation method (recent) differs considerably from Raymond's are they really comparable? The 1993 estimate of wild smolts sure seems awfully high. I know that ad clips were enumerated, but is it certain all hatchery fish were clipped each year in the recent period?

Chapman, D., and ten other authors. 1991. Status of Snake River Chinook Salmon. Report prepared for the Pacific Northwest Utility Conference Committee, Portland OR.

Al Giorgi
Don Chapman Consultants, Inc.
Phone (206) 883-8295
Fax (206) 869-6387

Appendix 5: Chapter 12 Addition

Chapter 12:
Influence of Climate on Fish

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

Appendix 6: Responses by Paul Wilson to Reviewers' Comments

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

Response to Peer Reviews of PATH Preliminary Report on Retrospective Analyses

Paul Wilson
4/17/96

Overview

1. *Question / Comment:* Walters: "In particular, perhaps it is time to look harder at the ocean fisheries."

   Response: Since the analyses to date and those planned for immediate future focus on spring/summer chinook, this comment and those immediately following about high harvest rates, which apply to fall chinook, are not particularly relevant for the time being. Same also applies to Walters' recommendations about ocean harvest in Chapter 6.

Chapter 3

1. *Question / Comment:* Walters: "Yet the R/S data show no patterns…corresponding to these habitat changes."

   Response: What about the possibility of a data "sorting" problem with respect to habitat, as suggested earlier for compensation by same reviewer in this chapter's comments?

Chapter 5

1. *Question / Comment:* Walters: "...since they [CRiSP and FLUSH] were parameterized so as to give average effects that agree with SR data."

   Response: As noted in earlier comments on PATH report, neither model is parameterized using SR data. Which isn't to say it wouldn't be a good idea; i.e. we should consider modifying the models to better match results from analyses like this, to incorporate information gleaned from data over longer segments of the life cycle.

2. *Question / Comment:* Collie. "Section 5.6. suggestion that three-parameter depensatory Ricker curve should be used in place of (6)."

   Response: I agree that it makes more sense, if expanding analysis to look at possibility of depensation and not excessively "messing with alternative S-R model forms" (per Walters' comments), to use this form instead. It departs from the standard Ricker form only at very small escapements.

Chapter 7

1. *Question / Comment:* Walters: "Why do fish appear to travel disproportionately slower than the water when water travel time is high [according to FLUSH figures]?" Collie: "Is there a hydrodynamic basis for the exponential relationship between fish travel time and water travel time in the FLUSH model?"

Response: There isn't (as far as I know) a hydrodynamic basis for the form of the fish travel time / water travel time relationships shown. A linear relationship fits the data equally well, and may be considered more parsimonious. Linear forms have been fit to the data for all three reaches, and are in use in some new versions of FLUSH. The difference in predicted fish travel time is small, except in very low or very high flow years.

2. *Question / Comment:* Walters: "...you might want to look into the question of whether water travel time itself can be precisely calculated under alternative flow regimes".

   Response: Good point. We are planning to take a first step toward doing this, by comparing water travel times in CRiSP and FLUSH over the same period in a low and a high flow year.

3. *Question / Comment:* Walters: "…data be presented so as to show clearly how data from different years are interspersed on the relationships.."

   Response: Valid point. Will do for FLUSH (and CRiSP?) for final Retrospective Report.

4. *Question / Comment:* Saila: "(Referring to Table 7.2 and Figure 7.2 for FLUSH fish travel time in one reach)…a coefficient of determination of less than 0.5 suggests that one should not have much confidence in the model."

   Response: It is true that for this reach, the fish travel time vs. water travel time data might lead one to conclude that the relationship is not very strong. However, some procedure for predicting travel time must be put into the model, and we are always limited by the data available. Note also that although the R-square is less than 0.5, the slope is significantly different from (greater than) zero ($p = .001$).

Chapter 8

1. *Question / Comment:* Walters, point (9):

   Response: It should be noted that there is little if any evidence for hypotheses (a) and (b) and the statement that flow management will not work if (a) and (b) are both correct; they are wholly speculative. The analysis presented in Chapter 8 does not provide support for these hypotheses. The reviewer drew the conclusion that it did in part because of some misleading language in the Discussion section (8.3.6.3). It is true that theoretically, with enough fish and enough time, these hypotheses could be tested. However, manipulation of water to intentionally decrease flow during the migration season is unlikely to happen and would be unwise to attempt, given the endangered species context, critically low recent escapements, and the scant credibility of the hypotheses.

2. *Question / Comment:* Collie: "Chapter..is very long and poorly written", "some of the sentences are unintelligible." Dennis: "chapter was long and unfocused."

   Response: Point taken. We should reduce the amount of jargon. If prominent population and stock assessment biologists have trouble gathering the meaning of our prose, we obviously need to try harder to achieve clarity. We also should not have included the long, detailed descaling document in this report, but instead should have referenced it (and included a summary, which would also have addressed available information on relation of descaling to mortality, if time had allowed). Some components of this chapter were undoubtedly difficult to interpret because the reviewers had no context in which to understand the questions and arguments about some issues, for example, flow-survival studies.

Chapter 11

1. *Question / Comment:* Walters: "Perhaps you should be viewing hatchery management issues more from a risk management viewpoint, starting with the assumption that deleterious effects…are the 'default' most likely scenario."

   Response: The first part of the suggestion is worth considering. The second part, which includes the suggestion that we "stop supplementation, period" is not very practical. Although they may have done a lot of damage to some wild stocks, hatcheries are fundamentally different from the hydroelectric dams in several important ways. Dams were never built as mitigation for depletion caused by other sources, nor were they built in an attempt to rehabilitate depressed runs. Many hatcheries were, and the direction and magnitude of effects on wild stocks are likely to be more difficult to tease out. Although few would argue that supplementation is sufficient in and of itself to recover depressed stocks and rebuild them to harvestable levels, given the track record of dealing with other anthropogenic impacts, and the current condition of listed stocks, supplementation and/or captive breeding may be necessary to stave off extinction in the short term, regardless of potential genetic or ecological problems in the long term.

2. *Question/Comment:* Walters (1) "organize questions and hypotheses into two basic categories related to future experimental design opportunities:"

   Response: Suggestion is a good one. Evidence for questions about within-stock impacts may be harder to come by then evidence for questions about between-stock impacts.

3. *Question/Comment:* Walters (2) "the issue is not whether hatcheries affect genetic 'variability' but rather whether they affect the processes of adaptation to local circumstances…"

   Response: I disagree with the statements. Perhaps it was not clear what was intended by language in question I.5. Genetic variation is not *synonymous* with high fitness or local adaptation, but it is crucial for the long term prospects of both. Genetic variability (or variance, a more precise term) is the raw material of natural selection. In fact, the amount of selection (change in fitness) a population will undergo is directly proportional to the amount of additive genetic variance, as well as the intensity of selection. What is an optimum amount of variability, as measured by among-population vs. within-population diversity indices, within a subpopulation in comparison with that experienced by a meta-population is not easy to determine in any particular case. It is true that in animals with population structure such as Snake R. spring/summer chinook, adaptation to their unique environment in individual breeding populations will tend to lead to relatively low within-population genetic variance, with overall variance maintained because of different subpopulations tending to be fixed for different alleles at those loci that are polymorphic in the aggregated population. But some genetic variance must remain in each semi-isolated unit, as migration alone cannot be expected to always provide a sufficient and timely influx of novel alleles as the environment changes. Loss of genetic variation is potentially a serious problem in endangered stocks not simply because of hatcheries, but because of high levels of genetic drift caused by low breeding numbers.

4. *Question / Comment:* Walters (4) and Collie 11.3.

   Response: Reviewers are correct. It is incorrect to say that artificial production may decrease the probability that a wild fish will be caught if annual harvest *effort* is held constant.

5. *Question / Comment:* Walters (5)

   Response: In other PATH tasks, addressing questions about other life stages or impacts, decision trees have been produced. I haven't received much feedback from the members of this task, but that which I have gotten suggests people would find a decision tree useful here. If we are to do a decision analysis for this task, as suggested by several reviewers and as planned for other tasks, a decision tree would be wise. This would involve explicitly linking hypotheses to management questions.

6. *Question / Comment:* Walters (6)

   Response: This is a good idea, if time allows. It would indeed be big and ugly, but it would facilitate evaluation of empirical evidence to test hypotheses.

7 *Question / Comment:* Walters (7)

   Response: I agree to a certain extent with point that computer models do not test hypotheses. However, results of independently validated (to accepted analytical formulas) genetic simulation models may legitimately be considered as evidence, in much the same way that derived analytical formulas may usefully describe likely behavior of certain mechanisms. Note also that SLCM and ELCM are suggested to be used to *explore the implications* of some hypotheses, not test them, per se.

8. *Question / Comment:* Walters (8)

   Response: Although this analysis would be interesting, it does not really belong in this chapter. The performance of hatchery populations is of interest for this task only so far as it impacts wild stocks.

9. *Question / Comment:* Collie (11.4)

   Response: Agree that R/S is the better response variable. Number of smolts released each year in a particular subbasin is one independent variable we could use to quantify degree of hatchery influence. We may want to look at variations of this, however. For example, the absolute number of releases may be less important than the number of releases relative to the estimated carrying capacity of the subbasin. In some subbasins, there are no juvenile hatchery releases, but there are impacts to wild stocks through straying. Fraction of naturally spawning population that is composed of strays or number of strays could serve as independent variable. Or, another independent variable to include in the analysis might be an index of similarity of the donor stock to the native stock.

10. *Question / Comment:* Dennis: "proposed methods for testing them are incomplete and will yield only preliminary information."

    Response: This is true, in a sense. However, for many (if not most) of the hypotheses, there will never be conclusive evidence for or against them. Regardless, some have indicated that a literature review would be useful. I think we should focus on broad (and any narrow) hypotheses that stand a chance of being tested with existing information. Prioritization of these hypotheses according to their potential for testing is a crucial next step, as indicated in the discussion.

Appendix 7

General Comments by PATH Participants on Preliminary Report on Retrospective Analyses

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

To: PATH Participants
From: Charlie Paulsen
Subj: Comments on March 15 Draft Retrospective Analysis

Section 1: Life Cycle Aggregate Hypotheses

Do we intend to formally test these hypotheses, or are they simply an expository or organizing concept? If they are in fact intended to be testable hypotheses, how will they be tested and who is responsible for testing them? If we won't be testing them explicitly, why not call them something else (world views, or something)?

Section 2: Broad Scale Analyses

Mostly agree with Walters:

The Coronado-Hernandes report deserves a close look, to see what this says regarding smolt-to-adult survival of tagged (mostly hatchery) fish. Is it possible to investigate the assumptions that underlie the "R" (i.e., recruit to Columbia River mouth) calculations? As I understand it, we assume that all upriver (above Bonneville) spring chinook have the same harvest and dam conversion/mortality rates. This obviously depends on different stocks being in the same place(s) at the same time, and being equally susceptible to harvest and upstream passage mortality. Can this hypothesis be tested systematically? This seems to me like a good topic for either the next workshop or the post-workshop sessions on retrospective analysis details.

In addition, I think Walters' idea of defining "recruit" as S_{t+4} is a good one: it provides a comparison of recruitment as presently defined, and may enable the use of a larger number of stocks from a wider geographic area. On the other hand, Walters may not have understood how S is calculated: it seems to me that the Petrosky et al. work to date is about as close as one could hope for to independent estimates of spawning escapement (but see comments on Section 3).

A Nei diagram of correlation coefficients may well be a better way of viewing this than the figures in Section 2; I need to try a few and see how they look. I suspect, however, that the discriminant distance interpretation may not be as difficult as Walters' suggests. If it is, one could use a logistic analysis to "explain" the groupings, using the same independent variables as planned for the discriminant analysis.

Section 3: Contrasts in Stock-Recruit patterns

3.2.5 Given Walters' comments on expanding the scope of the analysis, adding the stocks noted in this section assumes particular importance.

3.3 I agree with Saila's comment that the "beak-points" in the time series should be subjected to systematic statistical tests, in addition to the informal graphical approach used in the chapter. For example, some of the difference between observed and "predicted" R/S may in part be an artifact of the period used in the regression relationships.

3.4.1.1 "None of the salmon River stocks…. achieved replacement…" What is the precise definition of "replacement" in this passage?

3.4.1.2 Is there an accepted rule of thumb on how frequently a stock should achieve replacement? I note that the John Day appears to have achieved replacement only 10% of the time in the past 30+ years.

3.4.1.4 The "aggregate upriver spring chinook" material is interesting. Could ODFW/WDFW prepare a detailed report (or distribute copies of Junge 1980, if methods are the same) prior to the next workshop?

Finally, I seem to remember from earlier ESA work that Marsh Creek (and perhaps some other Snake River stocks) was missing actual redd counts for 5-10 years, and that the missing data was filled in via interpolation or correlation with other stocks. Is this correct, and if so, could we see the details on the methods used?

Section 4: Multivariate Analysis of Stressors…

The correct version of Eq. 4.2 is:

STACKALIGN {Ln&` LEFT ((R_1``+``R_{t-1}``+``R_{t-3}) / S_{t-5} RIGHT)~=~ $ _0``+`` $ _1 S_{t-3}``+`` $ _2 DAMS_{t-3}``+ $ _3 UPWELL_{t-3}``+# $_4& MGFLOW_{t-3}``+`` $_5 SPFLOW_{t-4}``+`` $_6 SNOW_{t-4}``+`` $_7 NPI_{t-2}``+`` $_8 NPI_{t-1}``+`` $_9 NPI_t``+`` g_t }

Something evidently was lost in translation from Word to WordPerfect.

Walters and Saila both have some intriguing ideas on how to do the analysis. These should be explored in the next workshop or in subsequent technical sessions.

Section 5: Retrospective analysis of passage mortality…

Dennis' comments on the use of the SIC seems much to the point here, given the somewhat contradictory results from the other comparisons.
A table of the input data would be very useful.
Is there any reason not to split the JDA stocks as was done in sections 2 and 4? This would add considerably to the number of degrees of freedom in the analysis.

5.3 What support is there for assuming that passage survival at BON, TDA, and JDA have been in fact been "Fixed" from 1952-91? Is this really what is being assumed?
For a somewhat less optimistic view of the accuracy of peak redd counts, see Table 1 and Figure 1, for the Wenatchee River spring chinook. The data are not up-to-date, but they do suggest that the Lemhi redd counts may be more accurate than some. One could try a similar approach using aggregated Snake redd counts and dam counts.

5.4 The section on "simulated dam mortality" is unclear. How does one go from simulated dam mortality to total mortality for dam passage?

5.5 Is it possible to separate the downstream mortality from upstream passage mortality of returning adults, using an extension of the methods developed here? I note that μ was "constrained to be positive." What effect does the constraint have on the estimation results? I ask because there are numerous plans for μ in the next section.

5.6 To the best of my knowledge, neither passage model is "tuned" to S-R data. This confusion appears to have propagated through to the reviewers (e.g., Walters) as well. While it is true that "chinook … return to spawn over four or more ages…" the majority of fish for the stocks analyzed return at two age classes. Does this affect the conclusion at all?

Finally, I think Walters' comments on methods deserve serious consideration.

Section 6: A structured synthesis….

I think a much clearer approach would be to "blend" the "Viewpoint A - Viewpoint B" discussions. The way it is presented now is very confusing for the reader. Also, it would be helpful to provide literature citations wherever specific numbers, trends, etc. are mentioned. As an aside, do we plan to develop the decision trees into a formal decision analysis? If so, Saila's comments here are much to the point.

Section 7. Quantitative exploration….

7.3.2 This would be much enhanced by including a table of data used in the analysis.

7.3.3 Ditto.

Section 8. Sensitivity analysis…

8.3.1 No comments

8.3.2 I believe the Jim Anderson has re-visited the spill efficiency issue recently. Perhaps he could contribute something here. The issue has clear management importance given the focus on spill in the current B.O.

8.3.5 I think a comparable detailed calibration section for FLUSH would be very useful.

8.3.6 It is very difficult to follow the analysis here without tables of the actual data, reservoir and dam survivals, etc. In addition, there are many statements (e.g., p. 8-80, "some scientists believe…") that need references/citations.

9.0 Evaluation of survival trends…

I think that Walter's comments on density dependence/carrying capacity limitations (or the lack thereof) are much to the point. To investigate this a bit further, I duplicated Petrosky and Schaller's regression of 1962-82 potential wild spawners (independent variable) on ln (smolts/potential wild spawner), using data shown in Table 2 (from their table 9.5). This results in an adjusted r-square of 0.54 (see Table 3 and Figure 2), the same as reported in table 9.8. As a very informal test of the carrying capacity assumption, I next regressed wild smolts on wild spawners; this resulted in an adjusted r-square of 0.61 (see Table 4 and Figure 3). The fits of the two models are not markedly different. While refinements are clearly desirable, this simple analysis suggests that carrying capacity for the period 1962-82 may not have been limiting production. If that is indeed the case, one could reasonably infer that carrying capacity is not terribly important at spawning escapements (i.e., dam counts) of 40,000-50,000, well above current delisting criteria. Note that this is not to say that carrying capacity is not important for individual sub-stocks (but see Walters' comments on section 3).

Section 10. A decision tree for…

The focus here seems to be on habitat management decisions, rather than hypothesis testing. What would a hypothesis testing framework, as opposed to a management decision framework, look like? How would the authors propose testing the hypotheses, as opposed to deciding on management actions?

Section 11. Hypotheses regarding hatchery impacts.

I agree with Walters' comments on this one. While the list of questions is interesting, I think a very different viewpoint, akin to that outlined by Walters', will be needed to actually test (as opposed to pose) hypotheses regarding hatchery impacts.

Section 12. Influence of climate…

A good review of the literature. Given the PATH focus on survival (R/S), I suspect that the influence of ocean conditions will be somewhat harder to find than the climate/abundance interactions that have usually been done in the past.

Section 13. Hypotheses regarding harvest impacts

When do the authors expect to have something ready for review?

Table 1.

Year	Rock Island Dam Count	Rocky Reach Dam Count	Turnoff (RIS-RR)	Hatchery Runsize	Turnoff-hatchery	Redd Count	Redd Count Expansion	Redd Count / (Turnoff-hatchery)
1975	6,153	3,302	2,851	827	2,024	519	2,675	1.32
1976	8,413	3,354	5,059	1,138	3,921	396	2,041	0.52
1977	18,582	6,211	12,371	3,891	8,480	472	2,683	0.32
1978	19,228	7,317	11911	2,784	9,127	622	3,702	0.41
1979	6,548	2,186	4,362	2,177	2,185	156	804	0.37
1980	7,133	2,023	5,110	3,200	1910	223	1,149	0.60
1981	7,776	3,593	4,183	2,634	1,549	263	1,356	0.88
1982	7,892	2,827	5,065	2,998	2,067	300	1,546	0.75
1983	9,884	3,458	6,426	3,412	3,014	542	2,793	0.93
1984	12,185	4,063	8,122	4,195	3,927	386	1989	0.51
1985	25,848	8,700	17,148	8,038	9,110	747	3,850	0.42
1986	21,001	4,183	16,818	9,189	7,629	441	2,273	0.30

1987	18,883	3,480	15,403	7,573	7,830	545	1,878	0.24
1988	16,212	4,823	11,389	6,265	5,124	491	1,692	0.33
1989	10,690	3,168	7,522	5,134	2,388	493	1,698	0.71
1990	7,721	1909	5,812	4,373	1,439	446	981	0.68
1991	5,781	1,323	4,458	3,934	524	251	552	1.05
1992	15,634	2,714	12,920	11,117	1,803	491	1,080	0.60
1993	19,943	4,128	15,815	12,312	3,503	547	1,203	0.34
1994	2,041	349	1,692	1,118	574	125	275	0.48
							Average	0.59
							CV	0.48

Figure 1.

Table 2.

Brood Yr.	Smolt Yr.	Wild Spring/Summer	Wild Smolts * (10^6)
62	64	51436	2.9
63	65	35263	2.2
64	66	35462	2.8

65	67	20690	2
66	68	42920	2.1
67	69	49455	2.1
68	70	47837	3.2
69	71	52321	2.3
70	72	41366	3.2
71	73	35703	2.9
72	74	36482	2.1
73	75	35771	2.2
74	76	17516	2.5
75	77	17776	0.8
76	78	14483	1
77	79	28367	1.8
78	80	36925	2.8
79	81	7540	1
80	82	4888	0.6
81	83	8697	1.2
82	84	9977	1.2

Table 3. Ricker regression results

SUMMARY OUTPUT				
Regression Statistics				
Multiple R	0.750171			
R Square	0.562756			
Adjusted R Square	0.539744			
Standard Error	0.261655			
Observations	21			
ANOVA				
	df	*SS*	*MS*	*F*
Regression	1	1.674203	1.674203	24.45405
Residual	19	1.300802	0.068463	
Total	20	2.975005		
	Coefficients	*Standard Error*	*t Stat*	*P-value*
Intercept	4.873345	0.127131	38.33327	1.85E-19
Wild Spawners	-1.9E-05	3.78E-06	-4.9451	8.99E-05

Table 4. Linear Regression results

SUMMARY OUTPUT						
Regression Statistics						
Multiple R	0.792552468					
R Square	0.628139415					
Adjusted R Square	0.608567805					
Standard Error	0.502690204					
Observations	21					
ANOVA						
	df	SS	MS	F	Significance F	
Regression	1	8.110177187	8.110177187	32.09441752	1.8403E-05	
Residual	19	4.801251384	0.252697441			
Total	20	12.91142857				
	Coefficients	Standard Error	t Stat	P-value	Lower 95%	Upper 95%
Intercept	0.80657641	0.244243507	3.302345359	0.003745509	0.295368717	1.317784104
Wild Spawners	4.11522E-05	7.26404E-06	5.665193512	1.8403E-05	2.59484E-05	5.6356E-05

Figure 2.

Figure 3.

FROM: Al Giorgi
SUBJECT: Comments on the Preliminary Report
DATE: 3 April, 1996

General Observations:

1. Carl Walters comments were on the mark, by-and-large I concur with most of his assessments.

2.. What conclusions can we now make in PATH? We are conducting numerous time-consuming tasks targeting a multitude of hypotheses. Why? It seems there are only perhaps a handful of assumptions / hypotheses that cause disparate predictions between the two model complexes. Shouldn't we start emphasizing those, and concentrating our effort?

3. What retrospective analyses are worth pursuing? We need to focus on those analyses that will be readily interpretable and lead to some clear conclusions. Analyses that are confounded by numerous correlated variables are a waste of time in my view. What does the group think?

4. If PATH can identify the deficiencies in our historical information bases that would preclude conducting instructive analyses, then we can dispense with unfruitful tasks/analyses. This deserves attention and discussion in the workshop.

Questions @ Preliminary Report:

Chapter 2

1. Is the John Day population is the only lower river reference group available? If so, can we be confident about any inferences that might be drawn from comparisons with this population alone?. The performance of a single population can not be assumed to be representative of the lower river complex of populations. Figure 3, in Chapter 3 illustrates this point.

Chapter 3

3.2.5 The Hoh and Queets chinook are oceantype life histories, as is a portion of the North Umpqua population. It seems inappropriate to use these as a basis of comparison with streamtype Snake River spring/summer chinook.

Chapter 8

What does that descaling paper tell us with respect to why passage models differ in their predictions?

P. 8-69. Table 8-10. The estimated effective FGE at LGR and LGO fluctuated substantially from 1977-1983. Why? Based on what information? What are the implications of FGE differing from the interannual patterns characterized here?

Al Giorgi
Don Chapman Consultants, Inc.
Phone (206) 883-8295
Fax (206) 869-6387

FROM: Paul Wilson
SUBJECT: Comments on PATH Retrospective Analyses

Below are my comments on the report, based on reading Chapters 1,5,6,7, and 8.

Questions/Comments

Chapter 1:

Pg 1-15 Table 1.5. I question whether task 3.1.2a status should be described as "completed". E.g., we haven’t been assigned responsibilities for synthesis of evidence or broken into groups to work on parts of problem.
Table 1.5. We have much more to do on task 3.1.4a. I would not call what was included a "draft final analysis". Perhaps the status should be "P" for pilot/preliminary analysis.

Chapter 5:

Pg. 5-2 Should be Johnson Creek, and Poverty Flats of South Fork= Salmon River. Marsh, not March, Creek.

PATH Workshop 2
May 22, 1996

Pg. 5-3 How is M of 0.2 per year close to M of 4.07 for two years?

Pg. 5-8 1st para. Can NNY and NYY options be run and results presented as addendum or in next version?

Pg. 5-11 3rd para. What does "tuning" of passage models with S-R data mean?

Pg. 5-11 5th para. Suggest list of follow-up analyses include additional passage model runs, as guided by sensitivity analysis (task 3.1.4a), and pre-1970 survival estimates from models.

Chapter 5 graphs and spreadsheet (SRFEB_4.XLS):

Pre-1977 difference in Snake R. survival between transport models 1and 2 in CRiSP suggests they included transport, which was experiment in that period. FLUSH did not include transport. This difference may be important.

Chapter 7:

Title of chapter should be revised to reflect the fact that it is about fish travel time, only. (Also reference to title on page 1-11).

Concur with your correction memo of March 21 that figures 7-1 to 7-3 should be relocated. Could also be inserted in appropriate places in Section 7.3.2; if not, figure place holders and captions should be deleted from that section.

PATH Workshop 2
May 22, 1996

Appendix 8: Responses to Reviewers' Comments on Chapter 10

Chapter 10:
A Decision Tree for Structured Syntheses of Evidence Concerning Changes in Spawning and Rearing Habitat

PATH Workshop 2
May 22, 1996

Response to comments:

10.1 Spawning and Rearing Habitat Decision Tree

1. Rhodes submitted a proposed, more detailed decision tree from recommendations to NMFS on a coarse screening process for proposed activities in salmon producing watersheds. Group needs to decide how to mesh.

2. Much work has gone into East Side Assessment and upper Columbia Basin EIS, that is not reflected in this chapter. Need to get Danny Lee, Bruce Reiman, et al. involvement. Danny has suggested a meeting in early May to incorporate their work.

3. Saila general comment on need for decision analysis expertise (see chapter 6).

4. Collie noted tree is patterned after chapter 6, and raises same fundamental questions:
   a. Should habitat protection measures be implemented?
   b. Could survival be increased by improvements in spawning & rearing habitat?
   c. Could these improvements compensate for decreased survival in other stages?

   Comment: These primarily seem to be PROSPECTIVE questions, and could be worked into an hypothesis framework (see below).

5. Walters questioned the need for habitat decision tree. Also, the PATH should focus on assembling evidence to directly compare population performance in areas that have been impacted in various ways, and that the strengths/weaknesses approach may be largely a waste of time.

   Comment: PATH discussion point? I see value in structuring the evidence in the strengths/ weaknesses format, but have had other priorities. The bigger need seems to be a structuring of the aggregate and component hypotheses into retrospective/prospective questions with management implications, and look for opportunities to examine the hypotheses.

6. Paulsen notes that the focus is on habitat management decisions, rather than hypothesis testing. What would hypothesis testing framework look like? and how would authors propose testing the alternative H_a?

   Comment: Good point. Meeting with Danny in early May?

10.2 Structured Synthesis of Evidence Concerning Changes in Spawning/Rearing Habitat (Spring/Summer Chinook) — Hypotheses

1. RETROSPECTIVE questions from initial list appear to be Q1, Q2a,b, Q3a,b, and Q4. Management links from this list relate primarily to PATH levels 1 and 2: what has been the contribution of FSR habitat degradation in the decline of Snake River salmon? Q11 and Q12 also have some retrospective implications, but probably fit better as prospective questions.

2. PROSPECTIVE questions from initial list appear to be Q5-Q12. Collie's list could be added to these, and used to help structure the section.

3. One possible prospective framework would be:

    I. Watershed Goals
        A. How much increase in FSR survival is possible?
        B. What actions would be needed?
        C. What time frame from implementation to restored/improved condition?

    II. Protection Actions
        A. What are risks of alternative habitat management options?
            1. Species (population) survival and recovery - under status quo and improved hydro system
            2. Stock structure - connectivity to other salmon populations, potential loss of diversity, effects of additional incremental risks to existing high risk, etc.
            3. Steelhead and resident species (bull trout, cutthroat trout, redband trout, etc.)
        B. What are potential benefits of alternative habitat management options?

    III. Restoration Actions
        A. What are risks of alternative habitat management options?
        B. What are potential benefits?

4. Much of suggested framework is beyond scope of PATH. Should be part of Columbia Basin EIA. Need assistance from Danny et al. to integrate.

Appendix 9: Post-Workshop Comments from Randall Peterman

PATH Workshop 2
May 22, 1996

ESSA Technologies Ltd.

Date: 21 April 1996
From: Randall M. Peterman
To: PATH members

Dave Marmorek asked me to write down some further comments about the PATH background document for the April 1996 meeting, covering either matters that were not mentioned at all or that were not covered in much detail.

1. Someone needs to review and document what is known about the ocean distribution of different Columbia River fish stocks, not just in the nearshore environment at the time of harvest (based on coded-wire tags) but also during the previous period between then and when they enter the ocean as juveniles. This will help interpret the results from Chapters 2,3,4,5, and 9.

2. A simulation model can quantitatively evaluate each potential adaptive management action contemplated for the prospective analysis. This model should incorporate the forecasts of each of the major alternative hypotheses about the main processes. This type of prospective evaluation of experiments was used by Sainsbury (1988) and McAllister and Peterman (1992).

3. Everyone in PATH knows that due to confounding and limitations in data, most of the retrospective analyses are useful to only a limited extent for testing different hypotheses about processes affecting salmon. However, the analyses will probably also be useful for generating more alternative hypotheses. One additional benefit that I do not think was mentioned at the meeting last week is that retrospective analyses will provide useful estimates of plausible ranges for parameters to use in the "prospective" simulations.

4. Because you are aiming to publish their work in widely read refereed journals, you should clarify and standardize all terminology to minimize jargon and eliminate ambiguities. Also, use one standard term instead of two or three to mean the same thing. Here are some examples that appeared in the current PATH report:
   - TBR, T/C, transport / benefit ratios, transport / control ratios
   - adult return rates (see below)
   - life cycle model
   - passage model
   - FGE, FPE
   - system survival
   - in-river survival
   - productivity (measured how exactly?)
   - There is often a lack of parallelism in terminology, such as mixing in one sentence "ocean-type chinook" with "spring chinook," etc. It is better to discuss spring/summer and fall chinook, for instance. Also, eliminate nicknames such as "upriver brights."

5. Total and specific rates

   - Just a reminder that one source of misinterpretation that often arises in ecology is that scientists fail to clearly identify whether they are talking about total rates or specific rates. This is a very common problem in PATH documents. For instance, depending on the person, adult return rates can mean **total** number of adults returning, **proportion** of smolts that returns as adults, or even the Ln(recruits/spawner). The latter two are examples of specific rates (i.e. rates of something per individual) and they can lead to very different interpretations than the total rate measure. Use precise terms to avoid this problem.

The same holds for "survival." Most everyone in PATH means proportion surviving by this term, but not always; sometimes it is used to indicate total numbers surviving. I suggest that you use proportional survival rate to indicate, say, proportion of smolts returning as adults.

6. Page 2-2 mentions the bias created in estimates of Ricker a and b parameters by autocorrelation in the time series (Walters 1985). However, it is not clear that Walters' (1990) partial bias correction method is desirable. While it reduced bias in parameter estimates, it only did so under a limited range of conditions and when it did, it increased the variance in the parameter estimates (Korman, Peterman, and Walters, 1995, Canadian J. of Fisheries and Aquatic Sciences 52:2174-2189).

7. I just remembered that I failed to mention one of the most important of the 8 elements of decision analysis in my short presentation on decision analysis at the PATH meeting last week. Element 5 should read "Model to calculate the outcomes of each combination of each management action and each hypothesized state of nature." I incorrectly listed "Criteria for ranking management actions" as the 5th element; that should have been included in the 2nd element, "Performance measures." So much for trying to remember my lecture notes!