PATH Scoping of Candidate Research, Monitoring and Experimental Management Actions:

Concurrently Reducing Key Uncertainties and Recovering Stocks

Working Draft

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Concurrently Reducing Key Uncertainties and Recovering Stocks

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Prepared by:

David Marmorek, Ian Parnell, Calvin N. Peters,
and Clint Alexander (compilers / editors)
ESSA Technologies Ltd.
1765 West 8th Avenue, Suite 300
Vancouver, BC V6J 5C6

Contributors:

Jim Anderson
Phaedra Budy
Tom Cooney
Rick Deriso
Al Giorgi
Rich Hinrichsen
Bill Muir
Charlie Paulsen
Randall Peterman
Jim Petersen
Charlie Petrosky
Chris Pinney
Howard Schaller
Steve Smith
Chris Toole
John Williams
Earl Weber

October 22, 1999

Citation: **Marmorek D.R., I. Parnell, C.N. Peters, and C.A.D. Alexander (compls./eds.)** 1999. PATH: Scoping of candidate research, monitoring and experimental management actions: concurrently reducing key uncertainties and recovering stocks. Working draft prepared by ESSA Technologies Ltd., Vancouver, BC, 232 pp.

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Executive Summary

ES.1 Background and Purpose of this Report

One of PATH's original objectives is to assess the ability to distinguish among competing hypotheses from future information, and advise institutions on adaptive management experiments, monitoring, and research that would maximize learning. In the PATH Final Report for Fiscal Year 1998, we set out a plan for addressing this objective (Table ES-1). Following consultation with the Implementation Team (I.T.) early in 1999, PATH established an Experimental Management Workgroup to more clearly define experimental management and generate a list of potential experimental management actions (i.e., the first three tasks in Table ES-1). This report summarizes the progress on these tasks by the experimental management workgroup.

This report is a working draft. We are requesting review and feedback on the candidate approaches from the I.T. The list of candidate options we have developed covers a broad range of possibilities, but is by no means exhaustive. In addition, we have completed only preliminary quantitative analyses of these actions, in keeping with the scoping nature of the report. Our intention is to develop a "short-list" of alternative actions through dialogue with other regional groups, catalyzed by the ideas in this report. Once this short-list is developed, PATH intends to complete more comprehensive quantitative analyses of the risks and benefits of the alternative actions (Tasks 4-7 in Table ES-1).

Table ES-1: Experimental management (ExpM) objectives of PATH. Refer to Appendix A for brief definitions of acronyms (e.g., A1, A2, etc.).

Task	Task Description	Completed
ExpM1	Clarify ExpM approach recommended by SRP	✓
ExpM2	Describe ExpM options as variations to A1, A2, A3, etc.	✓
ExpM3	Detailed description of ExpM options with review from the PATH Scientific Review Panel (SRP), I.T., NWPPC	***This report***
ExpM4	Develop tools for quickly evaluating ExpM options	
ExpM5	Evaluate proposed experimental management actions - risk to stocks versus amount of learning possible	
ExpM6	Evaluate proposed experimental management actions across populations	
ExpM7	Using results from ExpM evaluation, develop a research, monitoring, and evaluation plan to support the 1999 decision	

ES.2 Definition and Objectives of Experimental Management

Experimental management is an explicit commitment to reducing key uncertainties that, because of their significance, are preventing the identification of better management policies. In experimental management, short-term experimental actions are used to learn about the system, and this information is used to guide decisions about long-term management actions. The key feature of experimental management is that the short-term experimental actions consist of deliberate changes to a system to provide contrast in treatments (Walters 1986). These actions are implemented in an experimental design that will reduce the confounding of management effects with other simultaneous events such as climate change. Large-scale management experiments often face challenges and limitations caused by a lack of suitable controls, lack of replicates, lack of baseline information, or difficulty in randomly assigning treatments to experimental units (some important traits of good experiments). These limitations often make it impractical to fully desegregate effects of management actions from other influences. In spite of these limitations, an experimental management approach produces a substantial improvement in the reliability and efficiency of information-gathering, compared to more passive management regimes (Walters 1986).

Because we are concerned with ESA-listed salmon stocks, PATH recognizes that experimental management actions must both **maximize the ability to achieve conservation and recovery objectives** and **generate information to guide selection of better long-term management actions**. There is not universal agreement within PATH about the relative priority of these two potentially conflicting objectives. Consequently, some of the candidate actions described in this report are more oriented towards meeting conservation and recovery objectives, while others focus on the learning objective. However, in recognition that the relative priority of these two objectives is a judgement that must be made at the policy level, we provide a preliminary description of the learning benefits and the biological risks of all candidate actions. More quantitative analyses of these relative risks and benefits are planned for the next Fiscal Year.

E.3 Key Uncertainties

Description of Hypotheses

PATH scientists have identified two key uncertainties[1] that most affect the survival and recovery of Snake River spring/summer and fall chinook under the set of management actions analyzed to date:

1) **Extra mortality of non-transported fish.** Extra mortality is defined as any mortality occurring outside the juvenile migration corridor that is not accounted for by the other terms in the life cycle model used for retrospective and prospective modeling (i.e., terms for stock productivity and carrying capacity, mortality in dams and reservoirs, and estuarine/ocean mortality affecting all salmonid populations). Because many of the changes that may account for historical patterns in extra mortality all happened around the same time (e.g., completion of the hydropower system, a change in ocean conditions, and an increase in hatchery production), there is uncertainty about which of these factors (or mix of factors) influences extra mortality. Therefore, PATH has formulated three alternative hypotheses about the source of this extra mortality:
    a) Hydro – extra mortality is related to the experience of smolts that pass through the hydropower system (e.g., delayed effects of stress).

    b) Regime Shift – extra mortality follows a 60-year cycle that is related to long-term cycles in ocean conditions. There are no actions that can be taken to reduce extra mortality, but extra mortality will eventually go down when ocean conditions improve.

    c) Stock Viability (Here to stay) – extra mortality is due to some phenomenon that will not be affected by any hydrosystem action or regime shift (i.e., interaction with hatchery fish, presence

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[1] Other uncertainties also affect results (e.g., juvenile survival rates after dam breaching), but have less influence on outcomes.

of diseases such as BKD, or reduction in nutrients associated with historical declines in spawning stock)

Extra mortality can only be inferred from other measured quantities, it cannot be directly measured. This makes it difficult to monitor changes in extra mortality resulting from an experimental action, and thus to test alternative hypotheses about extra mortality. Nevertheless, extra mortality is still an important construct because a) it helps to design experimental management actions that address potential causes of extra mortality, and b) it will be needed for simulating the range of effects of alternative experimental actions to assess their relative risks and benefits.

2) **The relative post-Bonneville survival of transported fish compared to post-Bonneville survival of non-transported fish.** The ratio of these two values is known as "D" in the PATH modeling framework. Like extra mortality, D cannot be directly measured but must be inferred from other measured quantities (e.g., Transport:Control ratios and in-river survival estimates from transportation studies for spring/summer chinook). Differences in the assumptions used to estimate D have led to alternative hypotheses about both historical and future D values, for both spring/summer and fall chinook (Table ES-2 – see Section 2.2 of the main report for a description of the alternative hypotheses).

Table ES-2. Alternative D hypotheses for Snake River spring/summer and fall chinook.

Alternative Hypothesis	**Historical D value**	**Future D value**
Spring/summer chinook		
FLUSH	0.34	0.48
CRiSP	0.174 before 1980; 0.633 after 1980	0.67
NMFS	0.8	0.8
Fall chinook		
D1	0.10	0.24
D2	1.0	1.0
D3	0.10	0.10
D4	0.20	0.20

Implications of Key Uncertainties for Selecting a Long-term Management Action

In general, the ability of transportation to recover stocks depends directly on D (i.e., more likely to recover stocks when D is high, less likely when D is low). Drawdown actions are forecast to recover stocks over a wider range of D values. The ability of both drawdown and transportation to recover stocks also depends on the extra mortality hypothesis – both actions are more likely to recover stocks with the hydro hypothesis than with the regime shift or stock viability hypotheses.

Reducing these key uncertainties can help to identify the long-term management action that is best able to recover the stocks. There is an interaction between extra mortality hypotheses and the D value: forecasts of recovery are generally more sensitive to the extra mortality hypotheses if D is a high value[2]. This

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[2] If D is high, fewer transported fish die below Bonneville Dam. Other factors causing extra mortality of all fish are then required to explain historical declines in overall survival rates. If D is low, post-Bonneville survival of transported fish is sufficient to explain most of the observed historical declines in overall survival rates, and extra mortality factors affecting all fish become less important.

suggests that we should not measure D without also narrowing down the extra mortality hypotheses, and vice versa.

ES.4 Variables to Monitor

Any experimental management action requires a concurrent monitoring program to monitor responses to the experimental actions over time. There are many variables that could be monitored in conjunction with experimental management actions for this purpose. The key consideration is that monitoring variables must provide information about both the learning and conservation objectives of experimental management actions, so that we can assess the trade-offs between these two objectives.

Monitoring variables can be organized into three general levels:

Level I. Level I variables measure overall survival rates of salmon. Level I variables do not allow us to directly test extra mortality hypotheses, but they can be feasibly monitored and are useful for addressing more general hypotheses about the overall effects of actions on salmon survival.

Level II. Level II variables are those that directly address the extra mortality and transportation (D value) hypotheses. It appears to be feasible to monitor D, although there are disagreements about how precise these D estimates actually are. It appears from our qualitative analyses thus far that it will be very difficult to obtain good estimates of the changes in extra mortality resulting from experimental management actions. Further quantitative analyses are required to assess how feasible is it to detect changes in extra mortality.

Level III. Level III variables include in-river survival rates, fish condition, disease profiles, and other variables that are measured at finer temporal and spatial scales. These variables are generally relatively easy to monitor, and are useful for identifying mechanisms by which actions affect fish. However, they do not provide direct information on extra mortality or transportation, nor do they provide information for assessing the effects of actions on overall survival rates.

These three levels of variables are complementary in that all are needed to address both the conservation and the learning objectives of experimental management. For example, Level I variables are most useful for assessing the conservation objective, while Levels II and III are most useful for assessing the amount of learning possible from alternative actions. Therefore, in most cases we would like to monitor all three types of variables if possible. Some of these variables must also be monitored for lower Columbia River stocks to provide an indication of regional climatic changes affecting all stocks.

ES.5 Description of Candidate Actions

The PATH Experimental Management Workgroup has identified ten candidate actions for reducing key uncertainties. Eight of these are experimental management actions, while two are research programs that would provide supplemental information to the data collected from the management experiments. The ten actions, with their expected effects on survival and learning, are summarized in Table ES-3. Key points for each of the actions are provided in the text below the table.

Table ES-3: Candidate research, monitoring, and experimental management actions to concurrently reduce key uncertainties and recover stocks.

Candidate Approach	*Experiment*	*Research / Monitoring*	*Possible Survival Effects*	*Hypothesis Tested*
1. Current hydro system operations / Measure D		✓	None beyond current	D
2. Modify smolt transportation and Measure D	✓		Post-BONN survival of transported fish	D
3. Transportation / No Transportation	✓		Direct passage survival; Post-BONN survival of transported fish	D
4. 2 reservoir drawdown	✓		Passage survival; post-BONN survival; upstream survival	Hydro
5. 4 reservoir drawdown	✓		Passage survival; post-BONN survival; upstream survival	Hydro
6. Carcass introductions / stream fertilization	✓		Egg-to-smolt survival; other life stages	Stock viability - nutrient
7. Manipulate hatchery production	✓		Passage survival; post-BONN survival	Stock viability - hatchery/disease
8. Predator removal	✓		Passage survival	Hydro
9. Explore mechanisms for delayed mortality		✓	n/a	D
10. Regime shift monitoring		✓	n/a	Regime shift

Action 1. Continue current operations, measure D

Experimental Action: Continue transport evaluation studies in the Snake River using PIT tags for both yearling chinook salmon and steelhead. Conditions for in-river migrants would be optimized by maximizing spill at downstream projects during the migration.

***Benefits and Amount of Learning Possible**:* Another five years of marking fish along with data that will accrue from present marking programs should provide sufficient information to determine the benefits of transportation across the migration season and annually between seasons. One would need to tag between 63,000 and 2 million smolts, depending on survival rates and the level of precision required.

Risk to Stocks: If transportation and/or the hydropower system have large impacts on fish, continual operation of the hydropower system and transportation will increase the risks that stocks will not recover. Direct risks to stocks would be minimal since recent studies on spring/summer chinook have shown a benefit from transportation from Lower Granite Dam. Furthermore, by maximizing spill for in-river migrants, not all fish would be transported which would spread the risk between in-river migration and transportation as called for in the current Biological Opinion.

Action 2. Modify transportation system, measure D

Experimental Action: During the past couple of years, PATH participants have discussed various changes in methods of transportation that could potentially improve the survival of transported fish. Examples include changes in the timing of delivery of smolts to the estuary, and barging rather than trucking fall chinook.

***Benefits and Amount of Learning Possible**:* Clarifying the effects of ocean entry timing, interaction with other stocks during collection and transport, and the method of transport (barge or truck) will reduce the uncertainty about *D* and extra mortality for both transported and non transported fish. Between 22,000 and 6 million smolts will have to be marked to estimate D, depending on the true value of D and the desired level of precision.

***Risks to Stocks**:* Efforts to improve survival of transported fish, using only experimental PIT-tagged fish, would most likely not increase risk to the general population. Survival improvements may not be sufficient to attain recovery.

Action 3. Turn transportation on / off, measure D

Experimental Action: Vary the intensity of transportation. In some years, most fish would be bypassed, dewatered, and transported, while in others nearly all fish would be bypassed but not dewatered or transported.

***Benefits and Amount of Learning Possible**:* Because the experiment would alternate years when most fish are transported with years when (almost) none would be loaded into barges, it should be possible to observe greater contrast in survival rates of transported and non-transported fish. This should greatly reduce the current uncertainties associated with the estimation of D for the run at large.

Risks to Stocks: The obvious risk is that if transportation is beneficial, eliminating it for the run-at-large half of the time will increase mortality. On the other hand, if we had complete certainty about the effects of transportation, we would not carry out the experiment in the first place.

Action 4. Breach two dams on the lower Snake River

Experimental Action: Breach two of the four lower Snake River dams. There are several possible combinations of which dams to breach (e.g., breaching Ice Harbor and Lower Monumental Dams, or breaching Ice Harbor and Little Goose Dams). Use upstream, unbreached dams to regulate flow magnitude and shaping at breached dams (a Special Regulating Plan may be necessary to lower unbreached dams below minimum operating pool levels).

Benefits and Amount of Learning Possible: A two-dam breach coupled with regulated treatments of flow, volume and shaping from the unbreached dams could provide more information on ecological effects of dam breaching. This information could help to resolve key uncertainties, decrease the time required to justify four-dam breaching to the region, and avoid potential litigation or deferment through the congressional authorization process.

Risks to Stocks: It may be more difficult to detect the effects of dam breaching on salmon survival with only a two-dam breach than a four-dam breach, because of less contrast in the effects. Also, a two-dam breach could have less survival benefits for Snake River stock than a four-dam breach.

Action 5. Breach four dams on the lower Snake River

Experimental Action: Breach Snake River dams, stop transportation, evaluate regional stock responses to help guide John Day drawdown decisions for listed Upper Columbia stocks. Hatchery production could be either pulsed or kept constant under this approach.

***Benefits and Amount of Learning Possible**:* This is not an experimental action for Snake River drawdown decision; it is a long-term management action. However, implementation of this action would aid decisions on whether to restore natural river conditions in the John Day pool reach for listed salmon and steelhead in the Upper Columbia River. The staggered decision points for Snake River drawdown and John Day drawdown lend themselves to a staircase design, if implementation follows the same temporal pattern. Delaying Snake River actions while studies are conducted on John Day would negate this time step. Quantitative assessment of likely power to detect effects should be determined in FY2000.

***Risks to Stocks**:* According to the PATH FY98 and Fall Chinook Decision Analysis reports, four-reservoir drawdown options (A3/B1) have the lowest risk, and highest biological benefits of any of the experimental actions proposed. Transportation-based actions had lower probabilities of meeting survival and recovery standards, and were less robust to uncertainties. The decision analysis indicates that recovery is generally likely for natural river options, regardless of which extra mortality hypothesis is correct. This approach would also help restore ecosystem function and benefit native lamprey, white sturgeon, and resident fish and wildlife, and non-listed anadromous stocks from above John Day pool.

Action 6. Carcass introductions / Stream Fertilization

Experimental Action: Introduce salmon carcasses or introduce chemical fertilizers to increase stream nutrient levels.

***Benefits and Amount of Learning Possible**:* As nutrients increase, then parr-smolt mortality, and perhaps "extra" spawner-recruit mortality will decrease. Parr in about 30 rearing areas are already PIT-tagged, about 16 of which have data for six of the past seven years. The availability of spatial control rearing areas suggests that the power of this experiment to detect changes in parr-smolt survival could be quite high. For example, if 7 of the 16 sites are treated and 9 used as controls, power could range from 0.33 to 1.0 after only 3 years of the experiment, depending on the size of the actual effect on parr-smolt survival.

***Risks to Stocks**:* Disease spread is possible if carcasses are used, and there may not be enough disease-free carcasses to conduct the experiment.

Action 7. Manipulate Hatchery Production

Experimental Action: Manipulate Snake River hatchery steelhead production to reduce exposure of wild Snake River spring/summer chinook juveniles to levels at or below those experienced in the 1970's. Exposure of spring/summer chinook juveniles to hatchery steelhead could be reduced by decreasing the number of steelhead smolts released, reducing the size of steelhead smolts at release, or delaying steelhead smolt releases until late in the migration season.

***Benefits and Amount of Learning Possible**:* Determine (1) if there is support for the stock viability extra mortality hypothesis (hatchery sub-hypothesis), and (2) if reducing or eliminating exposure of wild Snake River spring/summer chinook migrants to hatchery steelhead can reduce total "extra mortality" of spring/summer chinook in the future, without breaching four Snake River dams. By simultaneously monitoring variables used to estimate D, and/or by simultaneously conducting transportation experiments, one could estimate the relative impacts of hatchery steelhead production on transported vs. non-transported spring/summer chinook. The results of such a study could help determine which combinations of hydropower actions and hatchery management scenarios are most likely to result in achieving recovery goals for Snake River spring/summer chinook.

***Risks to Stocks**:* Steelhead releases in the Snake River in 1998 totaled 12.2 million, of which approximately 3 million were used for conservation and/or restoration of native or local stocks. This leaves a possible maximum reduction in hatchery steelhead releases of 9.22 million without impacting conservation/restoration programs. Reductions should also consider the ability to maintain hatchery broodstock.

Action 8. Reduce Effects of Non-native Fishes

A. Reduce Effects of American shad

Experimental Action: Limit the upstream passage of adult American shad at Columbia River dams to reduce the abundance of juvenile shad in the system. Modifications could include elimination of overfall weirs in sections of adult fish ladders, or the use of specially designed overfall weirs to attract shad into terminal capture areas. Harvest offers another alternative to reduce the number of adult American shad, but it has been generally under-utilized.

Benefits and Amount of Learning Possible: Estimate and reduce the effect of American shad on survival of juvenile salmonids in the lower Columbia River. Studies of predator populations would need to be conducted for several years to detect a change in the impacts of predators. Because American shad are an introduced species, reducing their abundance in a portion of their current range would seem prudent. At present, they provide limited benefit for commercial and recreational fishermen above Bonneville Dam. Furthermore, any modification that would reduce their numbers would be reversible.

***Risks to Stocks**:* Modifications to fish ladders could cause some interference with migrating adult salmonids. Potential interference could be minimized by conducting additional research on adult shad passage prior to ladder modification, and by getting input from researchers who are experienced with shad passage on the Atlantic coast.

B. Reduce Abundance and Effects of Non-native Predators in the Lower Snake River

Experimental Action: Reduce the number of predatory fishes (northern pikeminnow, smallmouth bass, walleye, channel catfish) in the lower Snake River, so as to improve survival rates of salmon smolts[3]. Means of reducing predators might include intensive fishing (commercial and/or sport) and collection at dams in the lower Snake River.

***Benefits and Amount of Learning Possible**:* Estimate and reduce the effect of non-native predators in the lower Snake River. Estimate survival of PIT-tagged juvenile salmonids through the lower Snake River reservoirs. Monitor predator populations (variables currently being monitored by the Northern Pikeminnow Management Program include size structure of populations, density, predation rates, exploitation rates, diets). Studies would need to be conducted for several years to detect changes in survival rate or predator populations.

***Risks to Stocks**:* Several commercial fishing techniques that might be used, such as gill netting or trawling, could capture adult salmonids of endangered or threatened species. Careful management of techniques or fishing seasons would be necessary. By-catch of other species such as white sturgeon might also be a problem. Compensatory responses by un-exploited fish populations, especially species that could be potential predators on salmonids, might reduce the effectiveness of this action or even increase predation mortality.

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[3] These predators have much greater effect on the passage survival of fall chinook smolts than on spring/summer chinook smolts.

Action 9. Explore Causes of Delayed Mortality (Research)

Research method: Collect PIT-tagged fish that have had different migration histories at a downstream dam (Bonneville or John Day) using the sort-by-code system and rear in saltwater for an extended period to explore possible mechanisms for delayed mortality.

***Benefits and Amount of Learning Possible**:* From each group of fish, monitor and compare physiology, disease, predator avoidance and escape response, cause of death, and survival rates. Reducing the uncertainty about the extent and cause of delayed mortality for both transported and non transported spring/summer chinook salmon will be beneficial to the region in making informed decisions on operation of the hydropower system.

***Risks to Stocks**:* If this experiment is conducted concurrently with other experiments with the hydropower system in place, there will be little additional risk to stocks because sample sizes needed will be relatively small.

Action 10. Regime Shift monitoring (Research)

Research method: Monitor various indices of ocean climate conditions to detect shifts in ocean climate regime.

***Benefits and Amount of Learning Possible**:* One of the challenges that face experimental management is the ability to separate changes in salmon production that are due to management and those that are due to climate regime shifts. In experimental management designs it is difficult to control for climate effects because reference populations cannot be guaranteed to behave like treatment populations. By monitoring ocean variables, we can determine whether a regime shift has occurred during an experimental management action. Preliminary analyses suggest that it could take between 5 and 12 years to measure such a shift, depending on the strength of the effect.

***Risks to Stocks**:* None.

ES.6 Coordination of Actions

Design of management experiments in the Columbia Basin is made more challenging by the need to take into account a myriad of factors that simultaneously affect the region’s salmon populations such as climate variation, mixed stock fisheries, migration efficiency past dams, hatchery production, and the effectiveness of barging fish. Given the various potential interactions among candidate approaches, ***learning*** will require careful coordination of actions to ensure that the population responses can be related to particular actions.

There also some potential efficiency gains to be realized by combining multiple experimental actions into an overall strategy. For example, hatchery, transportation, and carcass/fertilization experiments could be run for a series of years until enough data is collected to determine whether dam breaching is required. If it is, a two-dam breach could be completed prior to deciding whether another two dams needed to be breached. Other examples are described in the main report. Again, these strategies must be viewed in light of both the conservation and learning objectives of experimental management. Adopting a strategy such as the example described above may enhance learning, but may not enhance conservation because it would take at least 15 years to collect enough data in the initial phase to determine whether dam breaching was required.

ES.7 Evaluation of Experimental Actions

Because of the inherent trade-offs in objectives, PATH needs to assess the performance of each experimental management design with respect to both conservation objectives (e.g., probability of survival and recovery), and learning objectives (e.g., ability to distinguish between hypotheses). Not all of the possible experimental designs will be equally informative, biologically effective, or even feasible. We discuss several measures that can be used by decision-makers to compare different experimental designs and identify the one(s) that best satisfies trade-offs among objectives.

ES.8 Next Steps

The purpose of this report was to solicit feedback from the I.T., NWPPC, and other regional managers on the PATH experimental management work completed thus far. Specifically, we ask the following questions:

- Are there any actions on the PATH list of candidate actions that are obviously infeasible because of legal/political/practical constraints?

- Are there any actions that should be added to the PATH list of candidate actions?

Clearly there is more work to do, particularly in terms of developing overall strategies, building quantitative assessment tools, and completing the analyses of the relative risks and benefits of alternative experimental actions (i.e., Tasks 4-7 in Table ES-1). However, this report is only the first round of creative exploration of experimental actions. The immediate next step is to narrow the list of potential experimental actions further before proceeding with further quantitative assessments.

1.0 Introduction

1.1 History and Objectives of PATH

PATH grew out of previous efforts by various power regulatory agencies and state, federal, and tribal fisheries agencies to compare and improve the models used to evaluate management options intended to enhance recovery of Snake River salmon listed under the Endangered Species Act (ESA). By 1994, an independent Scientific Review Panel concluded that rather than further analyzing model behavior, it would be more useful to test key hypotheses, particularly those related to the distribution of survival over the life span; the effects of flow on survival; and the benefit of transporting smolts from upriver collection projects to below Bonneville Dam. This conclusion was formalized in the National Marine Fisheries Service 1995 Biological Opinion on the Federal Columbia River Power System (page 124, recommendation 17).

PATH was therefore structured as a rigorous program of formulating and testing hypotheses. It is intended to identify, address and (to the maximum extent possible) resolve uncertainties in the fundamental biological issues surrounding recovery of endangered spring/summer chinook, fall chinook, and steelhead stocks in the Columbia River Basin. PATH's objectives are to:

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

PATH has not devoted a lot of effort to assessing the ability of different actions to distinguish among competing hypotheses and thus, to reduce key uncertainties (objective 2). However, PATH has done a considerable amount of work on objectives 1 and 3, important precursors in designing adaptive management experiments. In past reviews of PATH analyses, the members of the Scientific Review Panel (SRP) have commented repeatedly on the need for an experimental-management approach to resolving key uncertainties (FY96, FY97, and FY98 SRP reviews; Appendix F)). At the Weight of Evidence workshop held September 8 to 10, 1998, the SRP members strongly advised against delaying the 1999 decision on modifications to hydro-system management because of uncertainty and again recommended using an experimental management approach (SRP 1998). They described two strategic experimental management alternatives and discussed potential tools for the evaluation of experimental management designs. Though the SRP provided their best judgements to weight (i.e., quantify their relative degrees of belief in) alternative hypotheses on key uncertainties, they stressed that these weightings were not intended to replace experimental management actions, monitoring, or basic research that if properly conducted, would generate data that would narrow these uncertainties further.

PATH retrospective, prospective and decision analyses have clearly defined key management uncertainties, synthesized evidence for a range of alternative hypotheses, and provided a consistent set of data that can be updated and used to evaluate management experiments. This information is essential in the design and evaluation of adaptive management experiments (Walters 1986). Ultimately, it is through experimental management that *learning* can be added to the set of criteria already being used to evaluate proposed management actions.

This report is a working draft. Our purpose in releasing this draft is to get feedback on the candidate approaches from the I.T., the Northwest Power Planning Council, and other regional managers. The list of candidate options we have developed covers a broad range of possibilities, but is by no means exhaustive. In addition, we have completed only preliminary quantitative analyses of these actions, in keeping with the scoping nature of the report. Our intention is to develop a "short-list" of alternative actions through dialogue with other regional groups, catalyzed by the ideas in this report. Once this short-list is developed, PATH intends to complete more comprehensive quantitative analyses of the risks and benefits of the alternative actions.

In the remainder of Section 1 of this report, we define experimental management both generally and specifically how it relates to the Columbia River. In Section 2, we explain some the key uncertainties we are trying to address through experimental management, and describe some of the key considerations in designing experiment management actions. Section 3 provides a detailed description of the candidate experimental actions. Section 4 discusses how individual actions could be combined into an overall experimental strategy, and outlines some examples of such strategies. Section 5 describes some possible tools and approaches for evaluating and comparing actions based on the amount of learning possible and the biological risks. Section 6 outlines the next steps in the PATH experimental management work.

1.2 Definition of Experimental Management

Adaptive management is an explicit commitment to reducing key uncertainties that, because of their significance, are preventing the identification of better management policies. In its most effective form, an experimental approach is used to test clearly formulated hypotheses about the behavior of the important, but uncertain, components of the ecosystem. This produces a substantial improvement in the reliability of information, and lessens the slow, random, less efficient accumulation of knowledge under passive management regimes (Walters 1986). This requires a willingness to treat actions as experiments and to structure actions to address important uncertainties.

Adaptive management recognizes that uncertainties are unavoidable and that action cannot wait for uncertainties to be eliminated. Adaptive management can proceed in a *passive* or an *active* manner. The two approaches differ in the way information is acquired. Policies that just periodically update key management information are called passively adaptive policies, while those that include deliberate manipulation (i.e., creating contrasts in management actions to test alternative hypotheses) are called actively adaptive policies (Walters 1986).

In an "active adaptive" or "experimental" management framework, resource managers implement actions as experiments. Experimental management differs from research because it focuses on the outcome of experimental actions (as determined by monitoring) rather than the mechanistic details about how nature works. Research can help with hypothesis generation and the explanation of experimental response patterns, but it cannot substitute for actually seeing the responses to be explained, responses that follow deliberate contrast in management actions over space and time. Planned experimental actions provide the contrast in treatments necessary to test or refine key management hypotheses about system dynamics. Proper planning and design ensures the experimental actions are implemented in a spatial and/or temporal pattern that will reduce, as much as possible, the confounding of management actions with other uncontrolled events that occur simultaneously, such as climate change. This approach increases the probability of detecting effects of interest if they exist and increases the accuracy of estimated effects. This leads to stronger inferences about the outcomes of management actions and increases the rate at which managers learn about the system and can improve or change management practices to best meet management objectives. In summary, one can have research and monitoring without experimental management – this is generally what has occurred in the Columbia River Basin. Under experimental management, one must have monitoring to learn the consequences of experimental contrasts, and one has an opportunity for better research due to greater contrasts in conditions.

The PATH experimental management work seeks to find actions that both **maximize the ability to achieve conservation and recovery objectives**, and concurrently **learn something about key uncertainties to select long term management actions.** In the context of the PATH decision analysis, learning can be thought of as changes in the probabilities assigned to key alternative hypotheses (i.e., from roughly equal weights, to very unequal weights), which narrows the range of possible outcomes, and makes for a clearer selection of optimal actions. The basic tradeoff is between two strategies: 1) making a decision now (and potentially making the wrong decision); or 2) completing management experiments, research and monitoring to change the probabilities of alternative hypotheses, and making a decision later. The second strategy may reduce but will not eliminate the risk of making the wrong decision, and does incur added risk due to delay, and unintended effects on other species and life stages. As an example, Figure 1.2-1 shows a decision tree to assess the merits of alternative experimental designs to test the hypothesis that extra mortality (see definition in Appendix A) is strongly affected by hatcheries.

Unlike "active" adaptive management, "passive" adaptive management waits for unplanned phenomena to generate contrast in treatments. Managers usually assume a correct model, estimate management parameters based on historical data and update these estimates as new information is collected. For example, a periodic review of monitoring data may result in new model parameter estimates that require a change in management policy to meet management objectives. The new management policy is a new level of treatment, but it does not occur in a deliberate and planned manner. This introduces an inefficiency into the learning process because it may take many years to generate the range of treatments necessary to achieve strong tests of hypotheses. Furthermore, the data used to estimate parameters may co-vary with other uncontrolled environmental factors. As a result, these passive management policies will be confounded with other factors (e.g., was it the habitat restoration work or an open ocean regime shift that produced the increased survival rates?). Thus, a passive approach may increase the time required to detect significant changes and also result in a confounding of controlled and uncontrolled factors. In the case of endangered salmon stocks, the additional time spent under a suboptimal management regime may increase the chances of extinction and the duration of time for stock recovery.

The phrase "adaptive management" has often been used in Columbia Basin planning documents (e.g., the 1994 NPPC Fish and Wildlife Plan). However, implementation of adaptive management within the region has to date adopted a very passive approach:

> Unfortunately the key precepts of adaptive management were lost rather quickly; the idea of learning by doing became a rationale for virtually any action that might offer the possibility of learning (pg. 416, McConnaha and Paquet 1996).

As a result, management actions have generally been taken to improve stocks without a strong means of evaluating their effectiveness, or formally evaluating the key uncertainties that determine that effectiveness (McConnaha and Paquet 1996). Because PATH was structured as a rigorous program for formulating *and testing hypotheses*, a more active approach is needed. Thus, this report focuses on the merits of an *active* or *experimental* approach to adaptive management in the Columbia Basin. However, we recognize that the degree of manipulation will depend upon the particular situation. There is a range from passive adaptive to active adaptive approaches for any experimental action.

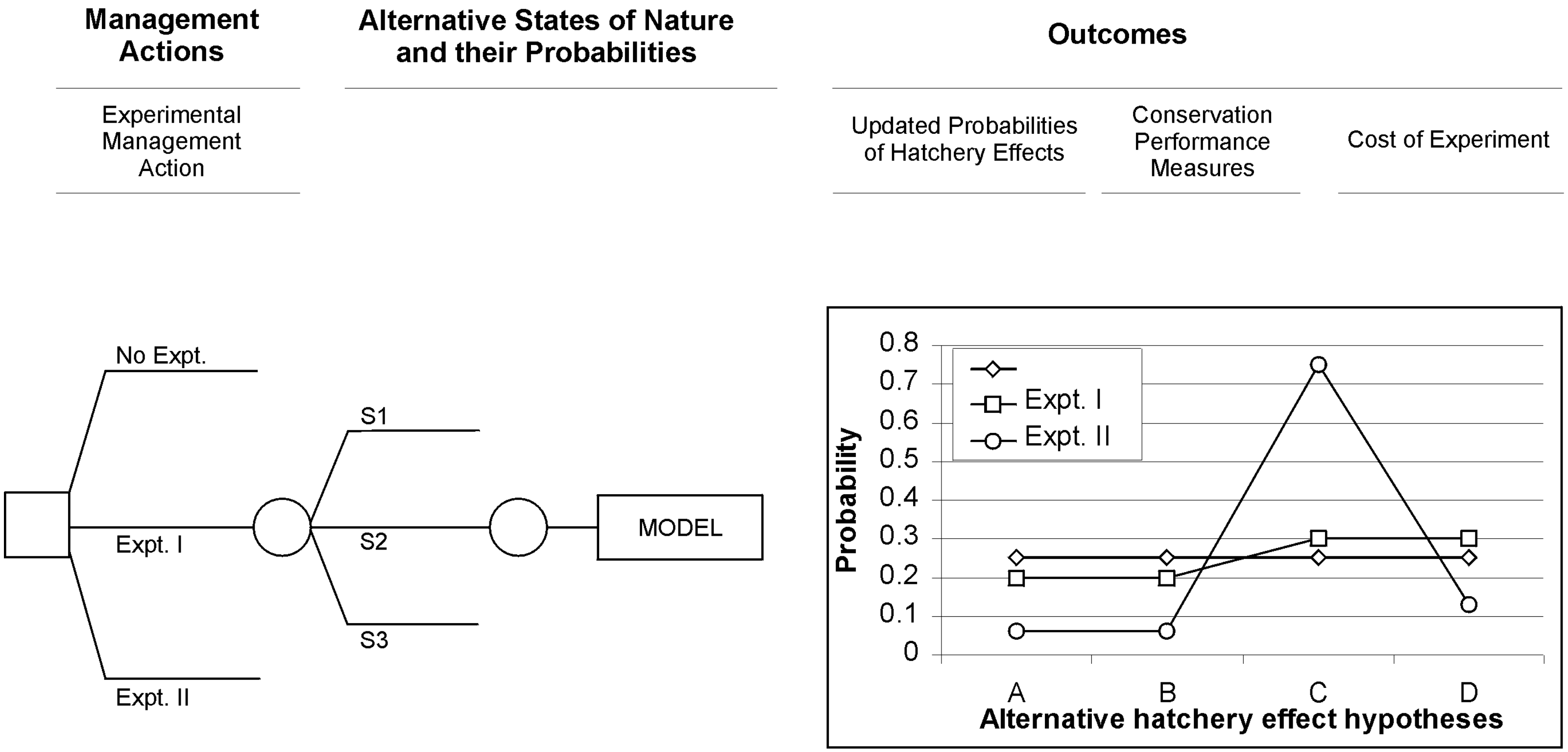

Figure 1.2-1: Decision tree to assess alternative experimental designs to test the hypothesis that extra mortality is strongly affected by hatcheries. Different designs could involve both different experimental actions, and different levels of investment in monitoring to improve the detection of effects. The true state of nature (i.e., S1, S2 or S3 in the diagram) is the actual amount of extra mortality associated with hatcheries. Note that there are three sets of performance measures: probabilities of alternative hypotheses, conservation measures, and cost. In this example, experiment II provides the cleanest distinction among alternative hypotheses.

1.3 Experimental Management of Columbia River Salmon

In the specific context of the Columbia River, experimental management actions are relatively short-term actions that deliberately manipulate one or more of the 4 H's — hydro, habitat, hatcheries, and harvest — to determine which factors exercise the most influence over production of salmon. The purpose of taking these experimental actions is to determine which long-term action is best, both relative to each other, and relative to absolute criteria for survival and recovery of Snake River spring/summer chinook, fall chinook, steelhead, and sockeye salmon. Although much of the current focus is on distinguishing among long-term hydrosystem actions (i.e., transportation vs. drawdown), results of the experimental actions could imply that a non-hydro action may be at least part of the optimal strategy if evidence is found to support certain hypotheses. Implications of the results of experimental actions for deciding on long-term actions are explored further in Section 2.2.

1.4 Objectives and Scope of PATH Experimental Management Work

Chapter 6 of the FY98 PATH report (Marmorek et al. 1998) briefly introduced experimental management as it relates to PATH, provided some examples of how this approach could be helpful to the region, and described what work remains to be done. The challenge is to determine if experimental management actions can reduce key uncertainties in the choice of long term management actions, while at the same time meeting conservation objectives. The FY98 report outlined seven tasks (see Table 1.4-1).

The primary purpose of this report is to make progress on ExpM task 2 and 3 (Table 1.4-1) and get some candidate options on the table that can be broadly reviewed by the Implementation Team (IT), the Northwest Power Planning Council, and other regional management groups (e.g., hatchery and harvest managers). Work on Tasks 4-7 will proceed following review of this report.

Table 1.4-1: Experimental management (ExpM) tasks of PATH in FY99.

Task	Task Description
ExpM1	Clarify the experimental management approach recommended by the Scientific Review Panel (SRP) (see Chapter 6 of FY98 final report (Marmorek et al. 1998)).
ExpM2	Describe the experimental management actions as variations to A1 (current management), A2 (maximize transportation), A3 (natural river drawdown of 4 lower Snake R. dams), etc.
ExpM3	More detailed description of experimental management options with review, input from SRP, Implementation Team, Northwest Power Planning Council, and regional managers.
ExpM4	Develop tools (modifying models, developing simpler models, compare simpler models to existing ones) for quickly evaluating experimental management options.
ExpM5	Evaluate experimental management actions in terms of risks to stocks versus amount of learning possible.
ExpM6	Evaluate proposed management actions with/without experimental management across populations (e.g., spring/summer and fall chinook).
ExpM7	Using results from ExpM evaluation, develop a research, monitoring, and evaluation plan to support the 1999 decision.

Because of ESA requirements, evaluations of experimental actions must consider effects on all stocks and species. Therefore, the scope of this initial development of candidate experimental management opportunities includes all Snake River stocks affected by proposed PATH management actions:

spring/summer chinook, fall chinook, steelhead and sockeye. We can identify three categories of stocks, based on the degree of existing information:

Category 1: Stocks that have been intensively analyzed (e.g., spring/summer/fall chinook), including decision analyses, for which we can identify explicit hypotheses to test.

Category 2: Stocks that have received much less quantitative analysis (Snake R. sockeye and steelhead; mid-Columbia). For these stocks, we need to ask, "what would be the effects on these stocks of actions taken to explore hypotheses for Category 1 stocks and what could be learned?" We want to minimize the biological risk to these stocks.

Category 3: "Control" stocks – stocks that would not necessarily be affected by any proposed actions but are needed to show differential effects (e.g., lower, mid/upper Columbia as controls for Snake River stocks).

2.0 Considerations in Designing and Evaluating Experimental Management Options

2.1 Management Objectives

The PATH experimental management work seeks to find actions that both **maximize the ability to achieve conservation and recovery objectives** and concurrently **learn something about key uncertainties to select long term management actions.**

Conservation Objectives

The primary goal of PATH is to determine the actions that should be taken to prevent extinction of endangered salmon stocks and promote their recovery. Thus, experimental actions need to be assessed on a suite of biological performance indicators. For example, the objective could be to select the experimental design that: 1) maximizes the chance of recovery; 2) minimizes the risk of extinction; and/or 3) best helps to resolve key uncertainties. In light of the Endangered Species Act, it is likely that conservation objective 3 will remain secondary to number 1 and 2. Performance measures for the conservation objective would be the same as for the current PATH decision analyses (e.g., the probability of meeting the National Marine Fisheries Service (NMFS) survival and recovery standards).

Learning Objectives

In this category, PATH must consider the statistical properties of the monitoring programs that will track the response of the salmon to experimental actions. To achieve broader learning objectives, the experimental management and monitoring design that maximizes the probability of detecting a “true” response to the management action is most desirable. (For example, the probability of accurately detecting a true 25% increase in the Snake River spring/summer chinook smolt-to-adult survival rate (SAR) in ten years.) The ideal experimental and monitoring design would have a high probability of detecting “true” changes over a range of system responses and background climatic conditions.. An additional objective may be to minimize the time needed to detect an effect of a particular magnitude. For example, for a particular experimental action a monitoring design could be selected based on how quickly it could detect a true 25% increase in SARs.

Economic Objectives

It is important to consider the economic implications of the experimental actions, the monitoring and research during the experimental period, and the long-term decisions based on experimental results. An experiment should provide a net benefit in the longer term (in terms of both conservation objectives and cost), compared to the option of making a long-term decision now. In this case, the management objective could be to maximize the expected value of experimentation (i.e., the expected outcomes of the best action with additional information minus the expected outcomes of making a decision with current information). A full evaluation of the economic performance of experimental actions is beyond the scope of PATH.

Tradeoffs Among Management Objectives

With endangered species, conservation is a primary objective. Given that learning and economic objectives are also important, there will be tradeoffs among the three categories of objectives. For example, actions that meet conservation objectives (e.g., those actions that minimize the risk of extinction) will be preferred to those actions that maximize learning (generate a larger signal) but would incur a higher risk of extinction. We come back to this point in Section 2.3.1.

2.2 Key Uncertainties

Key uncertainties must be precisely defined to develop useful adaptive management experiments. Fortunately, the existing PATH decision and sensitivity analyses have clearly revealed the most influential uncertainties for determining the expected outcomes associated with particular management actions. These uncertainties are summarized in Table 2.2-1; note that some uncertainties only pertain to some management actions (e.g., draw down-related uncertainties).

Table 2.2-1: Key uncertainties for spring/summer and fall chinook. Extra mortality hypotheses are more important at higher values of D. Alternative hypotheses are described further in the PATH FY98 Report (spring/summer chinook), and the PATH Decision Analysis Report on Fall Chinook.

Spring Summer Chinook (from Table 2.2.4-1 in Marmorek et al. 1998.)	
Uncertainty	*Alternative hypotheses*
Relative post-Bonneville survival of transported and non-transported fish (D)	~0.3: FLUSH transportation model ~0.6: CRiSP transportation model
Extra Mortality	1 – Hydro 2 – Stock Viability (here to stay) 3 – Regime Shift 4 – Hatcheries
Life-cycle models (existence of common year effects, and ability of downstream stocks to act as controls for upstream stocks)	Alpha (no common year effects) Delta (common year effects)
Length of Transition Period following drawdown	2 years 10 years
Equilibrated Juvenile Survival Rate through free-flowing reach following drawdown	0.85 (spring/summer chinook) 0.96 (spring/summer chinook)
Fall Chinook	
Relative post-Bonneville survival of transported and non-transported fish (D)	~0.1: life cycle model and (R/S) data ~0.2: 1995 PIT-tag data for Snake River fish and passage model estimates of in-river survival ~1.6: 1978-83 T:C studies for Hanford Reach fish and various in-river survival estimates
Extra Mortality	1 – Hydro (here to stay unless dams go) 2 – Stock Viability (here to stay) 3 – Regime Shift

The purpose of this section is to: a) explain the key uncertainties to be addressed through experimental management; b) describe the alternative hypotheses for these uncertainties and their implications for selecting a long-term action; and c) provide a brief introduction to possible experimental actions that could help to distinguish between these alternative hypotheses.

2.2.1 Extra Mortality – Definition and Historical Patterns

In this report we focus mostly on extra mortality and transportation uncertainties because they have been demonstrated to produce the largest effects on the ability to meet standards for all actions. Extra mortality is any mortality occurring outside the juvenile migration corridor that is not accounted for by the other terms in the life cycle model used for retrospective and prospective modeling (i.e., stock productivity and carrying capacity, mortality in dams and reservoirs, and estuarine/ocean mortality affecting all salmonid populations). More specifically, extra mortality is not accounted for by: 1) productivity parameters in the index stocks' spawner-recruit relationships (*a*, *b* and *p*); 2) estimates of direct mortality within the migration corridor (*M*); 3) common year effects influencing both Snake River and Lower Columbia River stocks (δ)[4]; and 4) random effects specific to each stock in each year ($\varepsilon_{t,i}$). Extra mortality is modeled in the PATH models by estimating post-Bonneville survival factors of non-transported fish (λ_n), and for transported fish (λ_t). These factors can be thought of as (1-extra mortality) for non-transported and transported fish, respectively.

2.2.1.1 Spring/summer chinook

λ_n is estimated from historical spawner-recruit data by both the Alpha and the Delta life-cycle models, although in slightly different ways. In the Delta spring/summer chinook model (Figure 2.2-1), extra mortality is estimated in each year as the difference between spawner to recruit survival and components of overall survival that are estimated from other information (i.e., passage survival is estimated from passage models, D values from transport:control ratios, and climate effects from yearly effects that are common to Snake River and lower Columbia stocks). In the Alpha spring/summer chinook model (Figure 2.2-2), extra mortality is modeled as a STEP function, where the post-Bonneville survival factor is 1 prior to 1976, then assumes some estimated STEP value after 1976. Like the Delta model, the STEP value is estimated after inclusion of other estimated components of overall survival (i.e., passage survival, D values, climate effects estimated from climate indices). 1976 was selected as the STEP year because that was the year in which there was an ocean regime shift, the last Snake River dams were completed, and hatchery outputs increased. In both models, the historical patterns in extra mortality of transported and non-transported fish depend on assumptions about other historical passage mortality and D (Figures 2.2-1 and 2.2-2 show results for D=0.3 and D=0.8 as a representative range of D values). Generally, extra mortality is greater (i.e., post-Bonneville survival is lower) with CRiSP estimates of passage survival than with FLUSH, and with higher D values. This is because extra mortality has to be greater when mortality is lowered in some other part of the life cycle (e.g., passage mortality is lower in CRiSP than in FLUSH) in order for the estimated overall survival to remain consistent with historical estimates of recruitment.

With both models, there is an increase in extra mortality (decrease in survival factor) during the 1970's, which suggests that whatever mechanism is causing extra mortality can be traced to some process or event that began in that time period. There are at least four processes/events that changed after 1976: completion of the final lower Snake River dam, full-scale transportation of smolts, a shift in ocean conditions to one that is hypothesized to be poor for Columbia river salmon, and a substantial increase in the number of hatchery smolts released. To account for these alternative explanations for extra mortality, we have identified four alternative extra mortality hypotheses. In the life cycle modeling, these extra mortality hypotheses are applied to λ_n, the post-Bonneville survival of non-transported fish. The extra mortality of transported fish is then calculated from the extra mortality of non-transported fish by the 'D' parameter (the relative post-Bonneville survival of transported and non-transported fish; i.e., $\lambda_t = D \times \lambda_n$). Because there is also substantial uncertainty in the D parameter, we have also identified alternative hypotheses about what that value is. These alternative hypotheses for D and extra mortality are described more fully in the following section.

It is important to note that there are no direct measures of λ_t , λ_n or D. These terms are all estimated indirectly from other measurements (e.g., T/C ratios) and passage survival estimates, and the errors accumulate (for ease of reference, all of the aforementioned variables are defined in Appendix A). Thus one can estimate changes in passage survival or proportion transported, but it will be more difficult to get

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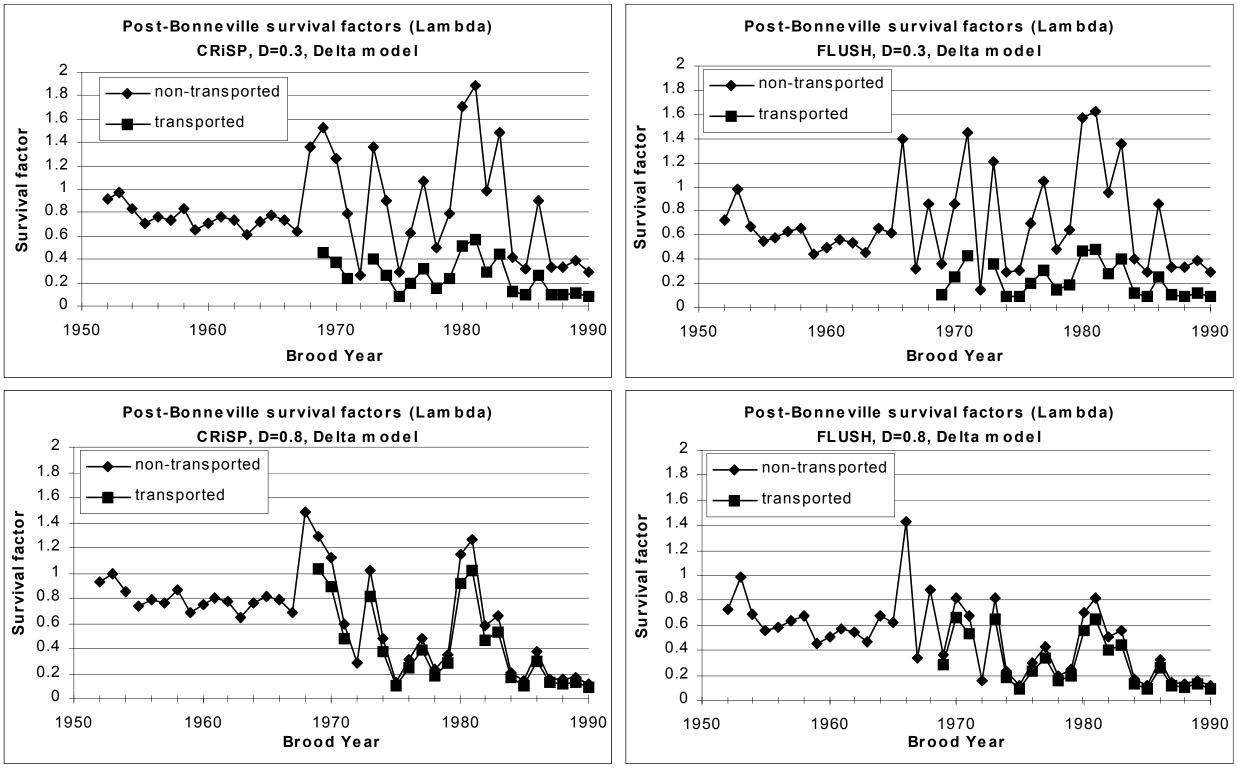
[4] common year effects apply only to the Delta version of the life cycle model.

a precise estimate of changes in λ_n, which relate directly to the alternative extra mortality hypotheses. Even if measurements taken after the action are more precise than those in the past, the uncertainty in historical measurements will affect the ability to estimate changes.


Post-Bonneville survival factors (Lambda)
CRiSP, D=0.3, Delta model

Post-Bonneville survival factors (Lambda)
FLUSH, D=0.3, Delta model

Post-Bonneville survival factors (Lambda)
CRiSP, D=0.8, Delta model

Post-Bonneville survival factors (Lambda)
FLUSH, D=0.8, Delta model

Figure 2.2-1: Historical patterns in extra mortality as estimated in the Delta model for spring/summer chinook.

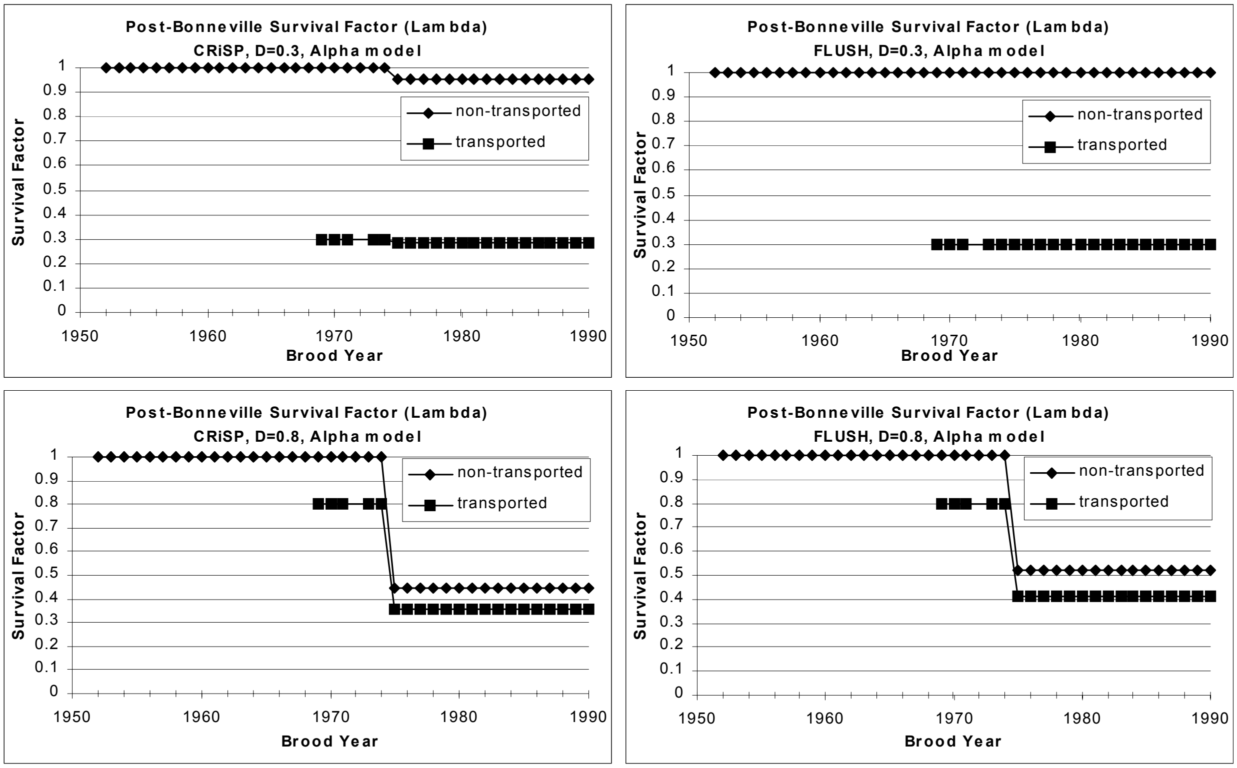


Figure 2.2-2: Historical patterns in extra mortality as estimated in the Alpha model for spring/summer chinook.

2.2.1.1 Fall chinook

Extra mortality in the fall chinook model is estimated in a similar fashion to the Alpha spring/summer chinook model; a STEP function where the post-Bonneville survival factor is 1 prior to either 1970 or 1976, then assumes some estimated STEP value after this year. As in the Alpha spring/summer chinook model, the estimated value of STEP depends on assumptions about other historical passage mortality and D (Figure 2.2-3 shows results for a representative range of D values, D=0.2 and D=1.0). We have developed alternative hypotheses for extra mortality and D, analogous to the hypotheses developed for spring/summer chinook (described in next section). Two STEP years were considered – 1970 corresponds to completion of Lower Monumental and Little Goose dams, and 1976 corresponds to a shift in ocean conditions.

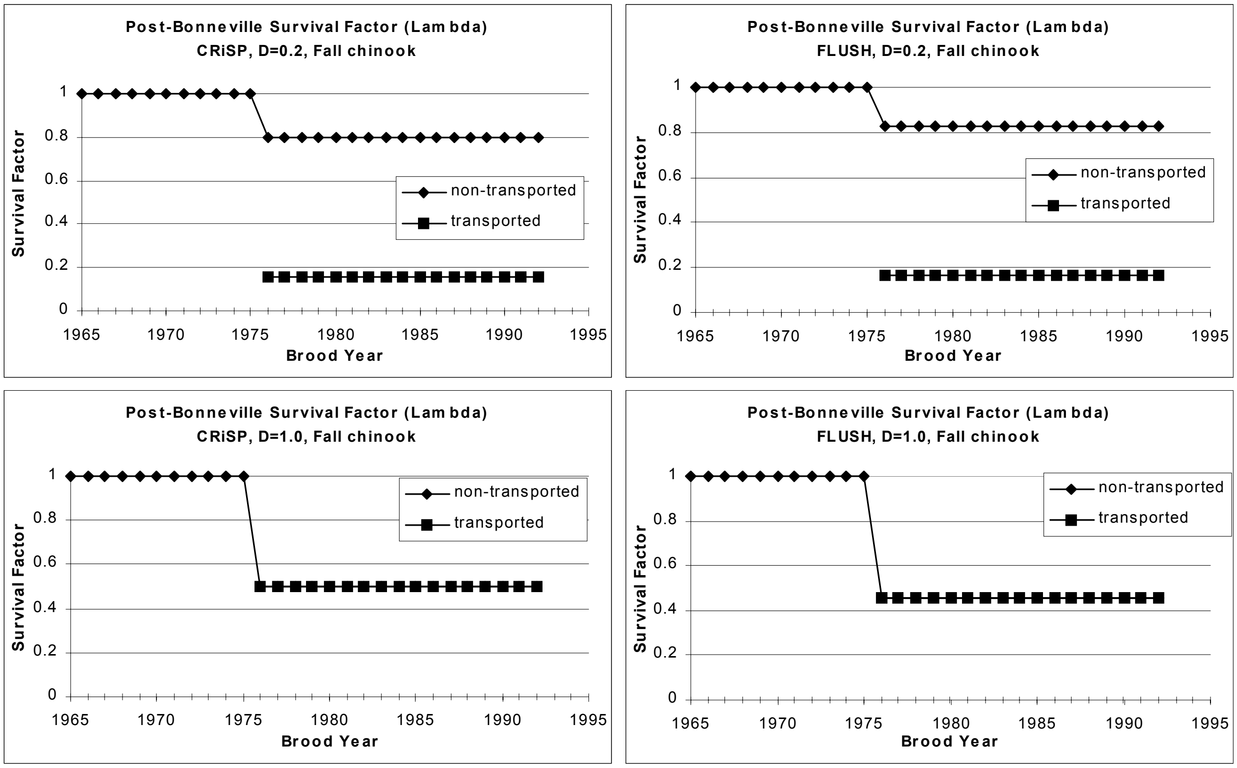


Figure 2.2-3: Historical patterns in extra mortality as estimated for fall chinook. Only one STEP year (1976) is shown in these graphs.

2.2.2 Extra Mortality Hypotheses

2.2.2.1 Spring/summer chinook

A. Description of hypotheses

As explained earlier, the overall decrease in post-Bonneville survival of non-transported Snake River fish in the mid-1970's points to several possible explanations. We have developed three overall hypotheses to explain these patterns in extra mortality:

d) Hydro – extra mortality is related to the experience of smolts that pass through the hydropower system (e.g., delayed effects of stress).

   We have developed two variations on this hypothesis. In Hydro(I), post-Bonneville survival is proportional to the in-river survival rate of non-transported fish (Vn). That is, extra mortality is a related to the entire hydrosystem. Hydro(II) was developed after Hydro(I) in response to critiques of Hydro(I) in the PATH Weight of Evidence process, and comments from the PATH Scientific Review Panel. In this variation, extra mortality is assumed to be here to stay unless the 4 lower Snake River dams are breached (extra mortality is related only to those 4 dams). Although we have not completed a full set of runs with the Hydro II hypothesis, we can approximate those results quite closely by:

   i) Using Hydro(I) results for A3 as a close approximation of Hydro(II) results. A previous sensitivity analysis showed that the two formulations of the Hydro hypothesis produced very similar outcomes under dam breaching (A3) (PATH Weight of Evidence Report, Appendix H)

    ii) Using Stock Viability results for non-breaching actions (A1 and A2) as a close approximation of Hydro(II) results (Hydro(II) says that extra mortality is "here to stay" hypothesis unless dams are breached).

  The Hydro(I) hypothesis suggests that extra mortality can be reduced by any action that increases in-river survival. With Hydro(II), only the breaching of dams will reduce extra mortality.

e) Regime Shift – extra mortality follows a 60-year cycle that is related to long-term cycles in ocean conditions. There are no actions that can be taken to reduce extra mortality, but extra mortality will eventually go down when ocean conditions improve.

f) Stock Viability (Here to stay) – extra mortality is due to some phenomenon that will not be affected by any hydrosystem action or change in ocean conditions. There are three sub-hypotheses:
    i) SV-Hatchery – extra mortality is due to interactions between Snake River spring/summer chinook and hatchery fish
    ii) SV-BKD - extra mortality is due to prevalence of disease
    iii) SV-Nutrients – extra mortality is caused by the reduction in nutrients associated with historical declines in spawning stock of Snake River stocks

  Although the modeled outcomes of each sub-hypothesis is identical, each of these sub-hypotheses suggests a different long-term action. The SV-hatchery hypothesis would suggest that altering hatchery production could reduce extra mortality. This may also be true with the SV-BKD hypothesis, if hatchery fish were a major disease vector for wild fish. With the SV-Nutrients hypothesis, a stream fertilization or carcass introduction program would reduce extra mortality.

B. Implications of hypotheses for selecting a long-term action

Alternative extra mortality hypotheses have different implications for selection of a long-term action. PATH has generally used two criteria for evaluating the biological benefits of alternative actions. The first criterion is a comparison of the jeopardy probabilities of one action relative to another action. Here we use the 48-year recovery probability as the primary standard for comparison. The second criterion is a comparison of the survival and recovery probabilities for different actions to pre-defined standards, corresponding to the NMFS Jeopardy Standards. For the survival probabilities, this standard is 0.7 (i.e., an action is said to have met a standard when the survival probability equals or exceeds 0.7), and 0.5 for recovery probabilities.

The effects of extra mortality hypotheses depend primarily on what D values are assumed. Table 2.2-2 shows 24-year survival and 48-year recovery probabilities for the three extra mortality hypotheses, using the three existing D hypotheses (FLUSH/TRANS1, CRiSP/TRANS4, NMFS) as one set of samples from the entire range of D values (see Section 2.2.3 for an analysis of a wider range of possible D values).

Table 2.2-2: Effects of extra mortality hypotheses on 24-year survival and 48-year recovery probabilities for spring/summer chinook. Probabilities that exceed the standards are in **bold**.

Extra Mortality Hypothesis	D Hypothesis	24-year Survival		48-year Recovery	
		A2	A3	A2	A3
Hydro(I)	FLUSH / TRANS1	0.63	**0.73**	0.44	**0.88**
	CRiSP / TRANS4	**0.80**	**0.82**	**0.69**	**0.88**
	NMFS	**0.74**	**0.75**	**0.61**	**0.82**
Hydro(II)	FLUSH / TRANS1	0.49	**0.73**	0.22	**0.88**
	CRiSP / TRANS4	0.66	**0.82**	0.46	**0.88**
	NMFS	0.61	**0.75**	0.40	**0.82**
Regime Shift	FLUSH / TRANS1	0.50	0.63	0.25	**0.87**
	CRiSP / TRANS4	**0.71**	**0.73**	**0.74**	**0.84**
	NMFS	0.67	0.65	**0.71**	**0.78**
Stock Viability	FLUSH / TRANS1	0.49	0.63	0.22	**0.85**
	CRiSP / TRANS4	0.66	0.69	0.46	**0.65**
	NMFS	0.61	0.57	0.40	0.46

The implications for a long-term decision are summarized in Table 2.2-3. The primary implications of extra mortality hypotheses are on the ability of actions to meet standards — the relative ranking of actions under each D hypothesis is the same for all extra mortality hypotheses. A3 recovery probabilities exceed A2 probabilities by 0.05 or more regardless of the extra mortality and the D hypothesis. A3 survival probabilities exceed A2 survival probabilities with the Hydro(II) hypothesis. With the other extra mortality hypotheses, A3 survival probabilities exceed A2 survival probabilities with the FLUSH/ TRANS1 D hypothesis, but the two actions are approximately equal (survival probabilities within 0.05) with higher D values (CRiSP and NMFS).

Extra mortality hypotheses have a large influence on the ability of actions to meet standards:

- With the hydro(I) hypothesis, A3 meets both standards regardless of the D assumption, while A2 meets the standards with higher D values (CRiSP and NMFS), but not with lower D values (FLUSH).
- With the hydro(II) hypothesis, A3 meets both standards but A2 does not meet either standard, regardless of the D assumption.
- With the regime shift hypothesis, A3 only meets the survival standard with CRiSP/TRANS4, but meets the recovery standard with all D assumptions. A2 also meets the survival standard with CRiSP/TRANS4 only, and meets the recovery standard under both CRiSP/TRANS4 and NMFS.
- With the stock viability hypothesis (the most pessimistic of the three hypotheses considered), neither action meets the survival standard under any D hypothesis. A2 also does not meet the recovery standard under any D hypothesis, while A3 meets the recovery standard with FLUSH/TRANS1 and CRiSP/TRANS4, but not with NMFS.

Table 2.2-3: Implications of extra mortality hypotheses for long-term decisions on spring/summer chinook.

Extra Mortality	D Hypothesis	24-year Survival		48-year Recovery	
		Comparison of actions	**Ability to meet the standard**	**Comparison of actions**	**Ability to meet the standard**
Hydro (I)	FLUSH / TRANS1	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	CRiSP / TRANS4	A2 ≈ A3	Both actions meet standard	A3 > A2 by at least 0.05	Both actions meet standard
	NMFS	A2 ≈ A3	Both actions meet standard	A3 > A2 by at least 0.05	Both actions meet standard
Hydro(II)	FLUSH / TRANS1	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	CRiSP / TRANS4	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	NMFS	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
Regime Shift	FLUSH / TRANS1	A3 > A2 by at least 0.05	Neither action meets standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	CRiSP / TRANS4	A2 ≈ A3	Both actions meet standard	A3 > A2 by at least 0.05	Both actions meet standard
	NMFS	A2 ≈ A3	Neither action meets standard	A3 > A2 by at least 0.05	Both actions meet standard
Stock Viability	FLUSH / TRANS1	A3 > A2 by at least 0.05	Neither action meets standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	CRiSP / TRANS4	A2 ≈ A3	Neither action meets standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	NMFS	A2 ≈ A3	Neither action meets standard	A3 > A2 by at least 0.05	Neither action meets standard

C. Experimental actions to test hypotheses

A variety of experimental actions have been proposed to test these extra mortality hypotheses. To test the hydro hypotheses, both a 2-dam breach (Section 3.4A) and a four-dam breach (Section 3.4B) have been proposed. There is no experimental action to test for the regime shift extra mortality hypothesis. However, there are monitoring approaches that could be used to test for changes in ocean conditions (Section 3.7). For the stock viability hypothesis, various actions have been proposed to test different sub-hypotheses. Experimental manipulations of hatchery releases is an option to test for the SV-hatchery sub-hypothesis. This may also provide a test for SV-BKD if hatchery fish transmit diseases to wild fish, although diseases are likely to remain in the wild population for some time. For the SV-nutrient sub-hypothesis, we describe a carcass introduction experiment (Section 3.5).

2.2.2.2 Fall chinook

A. Description of hypotheses

Because the decrease in post-Bonneville survival for fall chinook occurred at roughly the same time as spring/summer chinook (early to mid-1970's), we identified the same possible mechanisms. Alternative hypotheses for fall chinook are therefore analogous to those developed for spring/summer chinook[5]:

a) Hydro – extra mortality is related to the experience of smolts that pass through the hydropower system (e.g., delayed effects of stress). Only the Hydro(II) variation (extra mortality is assumed to be here to stay unless the 4 lower Snake River dams are breached) was implemented for fall chinook
b) Regime Shift – same as spring/summer chinook
c) Stock Viability (Here to stay) – same as spring/summer chinook

B. Implications for long-term decisions

We evaluated the effects of the extra mortality hypotheses on fall chinook survival and recovery probabilities using two fixed D values, D=0.2 (hypothesis D4) and D=1.0 (hypothesis D2). In both cases, the D value was assumed to be the same both retrospectively and prospectively. Figure 2.2-4 shows these effects. Survival probabilities are high for all hypotheses and are insensitive to the extra mortality hypothesis. Recovery probabilities are sensitive to extra mortality hypotheses only with D=1.0. As shown in Figure 2.2.3, extra mortality is larger with higher D values, as occurred with spring/summer chinook.

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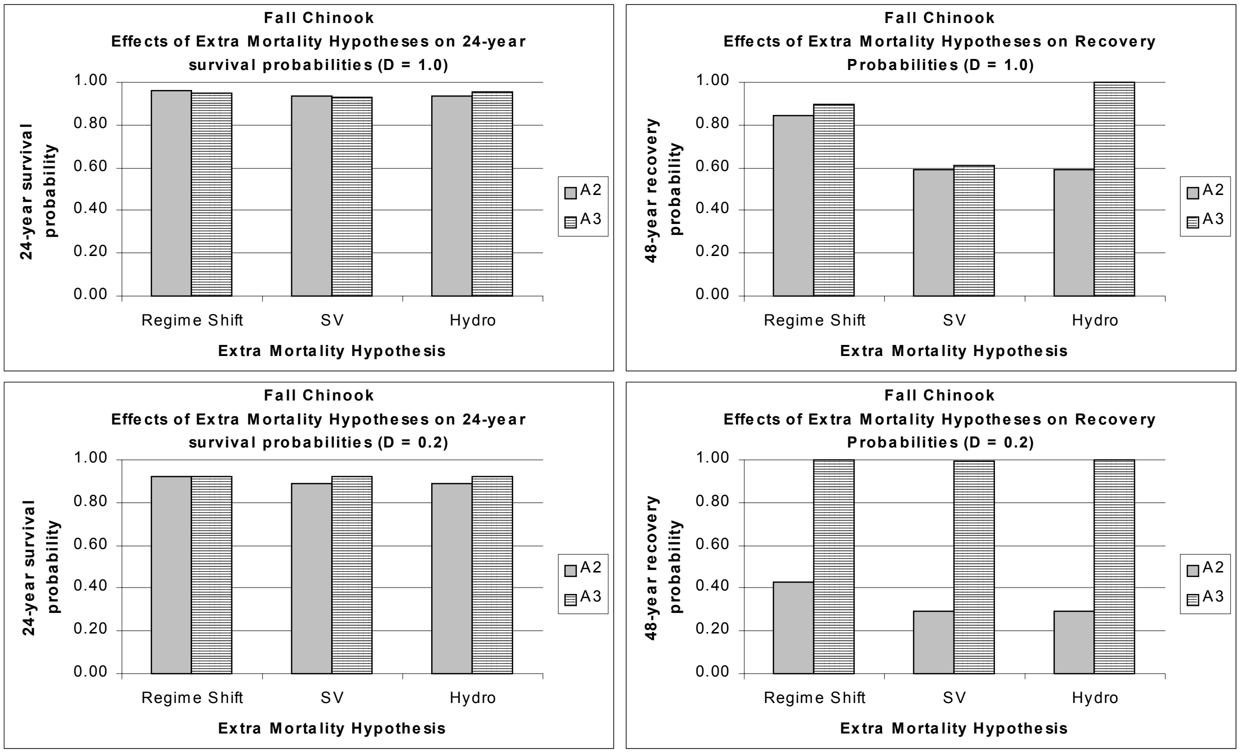
[5] The quality of the spawner-recruit data for Snake River fall chinook is such that either the retrospective D or the STEP (extra mortality term) can be estimated from these data, but not both simultaneously. As a result, STEP can be estimated (and therefore the extra mortality hypotheses can be evaluated) only when D_R is assigned a fixed value. Conversely, D_R can only be estimated when STEP is assigned a fixed value (when estimated, STEP is generally close to zero). Therefore, extra mortality hypotheses were implemented only with two of the four D hypotheses (D2 and D4) because these were the only hypotheses in which D_R was fixed at a constant value (see description of transportation hypotheses in sections 2.2.3.1).



Fall Chinook
Effects of Extra Mortality Hypotheses on 24-year survival probabilities (D = 1.0)

Fall Chinook
Effects of Extra Mortality Hypotheses on Recovery Probabilities (D = 1.0)

Fall Chinook
Effects of Extra Mortality Hypotheses on 24-year survival probabilities (D = 0.2)

Fall Chinook
Effects of Extra Mortality Hypotheses on Recovery Probabilities (D = 0.2)

Figure 2.2-4: Effects of extra mortality hypotheses on survival and recovery probabilities for fall chinook. Results are averaged over the two passage models.

Implications of extra mortality hypotheses for long-term decisions on fall chinook are summarized in Table 2.2-4. All extra mortality hypotheses imply that A2 and A3 survival probabilities are similar and achieve the standard. At D=0.2 A3 recovery probabilities achieve the standard and are greater than A2 probabilities, which do not achieve the standard. At D=1.0, both the hydro and the regime shift hypotheses result in A3 recovery probabilities that exceed A2 probabilities, with both actions achieving the standard. With a D=1.0 and the Stock Viability hypothesis, however, both actions produce similar recovery probabilities, and neither action meets the standard.

Table 2.2-4: Implications of extra mortality hypotheses for long-term decisions on fall chinook.

		24-year Survival		48-year Recovery	
Extra Mortality	**D Hypothesis**	**Comparison of actions**	**Ability to meet the standard**	**Comparison of actions**	**Ability to meet the standard**
Hydro	D=0.2	A2 ≈ A3	Both actions meet standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	D=1.0	A2 ≈ A3	Both actions meet standard	A3 > A2 by at least 0.05	Both actions meet standard
Regime Shift	D=0.2	A2 ≈ A3	Both actions meet standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	D=1.0	A2 ≈ A3	Both actions meet standard	A3 > A2 by at least 0.05	Both actions meet standard
Stock Viability	D=0.2	A2 ≈ A3	Both actions meet standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	D=1.0	A2 ≈ A3	Both actions meet standard	A2 ≈ A3	Both actions meet standard

C. Experimental actions to test hypotheses

Some but not all of the experimental actions that address extra mortality hypotheses for spring/summer chinook also apply to fall chinook. For example, the 2 and 4 dam breach to test the hydro hypothesis would apply to fall chinook but the hatchery action may not because fall chinook smolts generally leave the system later in the year than hatchery smolts. The stream fertilization would also not apply to fall chinook, and attempts to do something similar for fall chinook would be less practical because fall chinook are mainstem spawners.

2.2.3 Transportation Hypotheses

2.2.3.1 Spring/summer chinook

A. Description of hypotheses

D hypotheses are expressed in terms of D values that were assumed to have occurred in the past (retrospective D or D_R), and values that are assumed to occur in the future (prospective D or D_P). Three hypotheses have been proposed to date:

a) FLUSH/TRANS1 – retrospective and prospective values based on all transport studies conducted at Little Goose and Lower Granite dams between 1971 and 1989, coupled with FLUSH estimates of survival of control fish.
D_R = varies between years, average = 0.34
D_P = varies between years, average = 0.48

b) CRiSP/TRANS4 – retrospective values based on all transport studies from Little Goose, Lower Granite, and Ice Harbor dams between 1968 and 1995, coupled with CRiSP estimates of survival of control fish. Prospective values based on post-1980 transport studies.
D_R = 0.174 until 1979 (median of 1968-1979 transport studies
0.633 after 1979 (median of 1980 – 1995 transport studies)
D_P = selected randomly from post-1980 transport studies, average = 0.67

c) NMFS A-Fish Appendix – based on 1994 and 1995 transport studies, coupled with PIT-tag estimates of survival of control fish

$D_R =$ 0.8
$D_P =$ 0.8

B. Implications of hypotheses for selecting a long-term action

To thoroughly investigate the implications of alternative D values, we ran an intensive sensitivity analysis of D values in which a subset of model runs were completed for all combinations of retrospective and prospective D values ranging from 0 to 1.0 (increments of 0.1). The implications of different combinations of retrospective and prospective D values for these two criteria are summarized in Figures 2.2-4 and 2.2-5 (Tables of complete results are provided in Appendix E). Only those combinations where $D_P \geq D_R$ are shown because no D hypotheses have been proposed that assume the D will get worse in the future than it was in the past. D_P exceeds D_R by the largest margin in the upper left corner of the figures (i.e., the area furthest away from the diagonal line where D_P =DR). A higher ratio of D_P to D_R results in higher forecast population sizes.

Because of the interaction between D and the extra mortality hypotheses, we show separate results for the SV and the two Hydro hypotheses (results for the regime shift hypothesis are generally intermediate to the others). Results are averaged over the Alpha and Delta life-cycle models, because those two models produced very similar results. Figure 2.2-5 shows a comparison of actions with different combinations of D values. Three cases were considered:

- 48-year recovery probability for A3 exceeds that of A2 by at least 0.05 (labeled "A3>A2" on the figure)
- 48-year recovery probabilities for A2 and A3 are within 0.05 of each other ("A3 ≈ A2")
- 48-year recovery probability for A2 exceeds that of A3 by at least 0.05 ("A2>A3")

Figure 2.2-6 shows the ability of actions A2 and A3 to meet the 48-year recovery standard, for different retrospective and prospective D values.

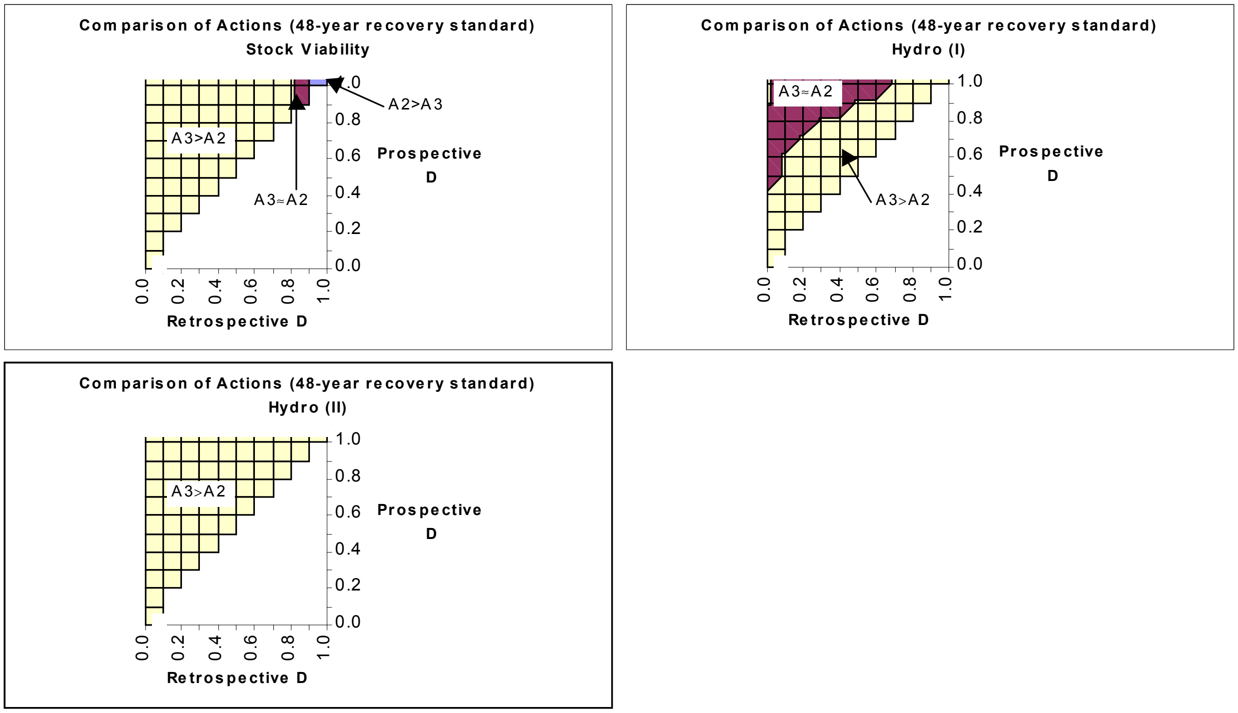


Figure 2.2-5: Effects of D values on comparison of 48-year recovery standards for A2 and A3 for spring/summer chinook. Only those combinations where prospective D ≥ retrospective D are shown because no D hypotheses have been proposed that assume the D will get worse in the future than it was in the past.

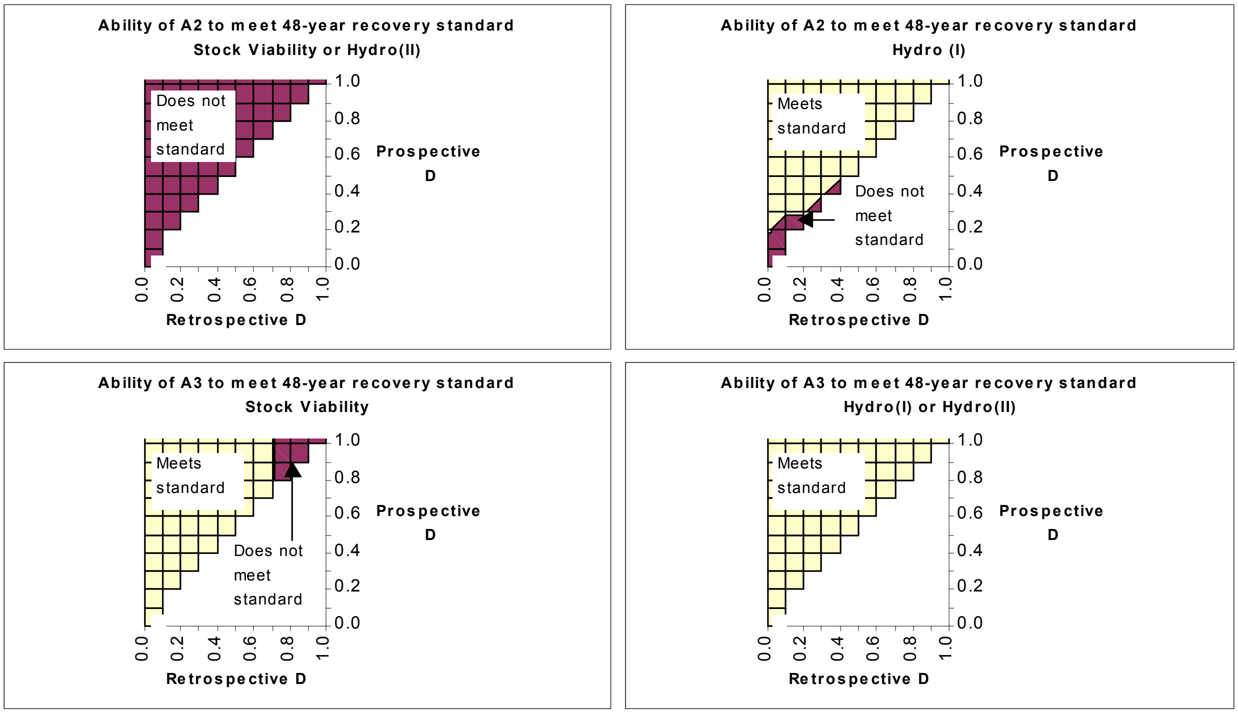


Figure 2.2-6: Effects of D values on ability of A2 and A3 to meet 48-year recovery standards for spring/summer chinook. Only those combinations where $D_P \geq D_R$ are shown because no D hypotheses have been proposed that assume the D will get worse in the future than it was in the past.

Using these figures, we can draw some conclusions about critical ranges of retrospective and prospective D values that have implications for the selection of a long-term action (Table 2.2-5). Because of the interaction between the effects of D and the extra mortality hypotheses, we show these results separately. In general, with the SV hypothesis any retrospective D less than 0.8 results in recovery probabilities for A3 exceeding those of A2, with A3 meeting the standard but not A2. At D_R values >= 0.8, neither A2 nor A3 meet the recovery standard. A2 and A3 are approximately equal with $D_R = 0.9$, and A2 exceeds A3 with a D_R value of 1.0. With the Hydro (I) hypothesis, assuming a low retrospective D and a high prospective D would lead to the conclusion that A2 and A3 produce approximately equal recovery standards, with both actions meeting the standards. If one assumes a low D_R and a low D_P, the recovery probability for A3 meets the standard and exceeds that of A2, which does not meet the standard. If one assumes a high D (>0.7) A3 again exceeds A2, with both actions meeting the standards. With the Hydro(II) hypothesis, recovery probabilities for A3 exceed those of A2, with A3 meeting the standard but not A2.

Table 2.2-5: Critical ranges of D and implications for long-term decisions on spring/summer chinook.

Critical range of Retrospective D (D_R)	**Critical range of Prospective D (D_P)**	**Comparison of Actions**	**Ability to meet the recovery standard**
Stock Viability Extra Mortality			
$D_R = 0.0$ to 0.7	Any $D_P >= D_R$	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
$D_R = 0.8$	Any $D_P >= D_R$	A3 > A2 by at least 0.05	Neither action meets standard
$D_R = 0.9$	Any $D_P >= D_R$	A2 ≈ A3 (within 0.05)	Neither action meets standard
$D_R = 1.0$	Any $D_P >= D_R$	A2 > A3 by at least 0.05	Neither action meets standard
Hydro (I) Extra Mortality			
$D_R = 0.0$ to 0.4	$D_P = D_R$	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	$D_P > D_R$ by 0.1 to 0.3/0.4	A3 > A2 by at least 0.05	Both actions meet standard
	$D_P > D_R$ by more than 0.4	A2 ≈ A3 (within 0.05)	Both actions meet standard
$D_R = 0.5$ to 0.7	$D_P = D_R$ or $D_P > D_R$ by 0.1 to 0.0.2	A3 > A2 by at least 0.05	Both actions meet standard
	$D_P > D_R$ by more than 0.2	A2 ≈ A3 (within 0.05)	Both actions meet standard
$D_R = 0.8$ to 1.0	Any $D_P >= D_R$	A3 > A2 by at least 0.05	Both actions meet standard
Hydro (II) Extra Mortality			
All D_R	Any $D_P >= D_R$	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard

The three D hypotheses (FLUSH/TRANS1, CRiSP/TRANS4, NMFS) can be approximated by the following combinations of retrospective and prospective D values[6]:

FLUSH/TRANS1:	$D_R = 0.3$, $D_P = 0.5$
CRiSP/TRANS4:	$D_R = 0.6$, $D_P = 0.7$
NMFS:	$D_R = 0.8$, $D_P = 0.8$

Using the information in Table 2.2-5, we can deduce the implications of each of these hypotheses for long-term decisions (Table 2.2-6).

Table 2.2-6: Implications of D hypotheses for long-term decision on spring/summer chinook.

D Hypothesis	**Comparison of Actions**	**Ability to Meet the Recovery standard**
Stock Viability Extra Mortality		
FLUSH/TRANS1	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
CRiSP/TRANS4	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
NMFS	A3 > A2 by at least 0.05	Neither action meets standard
Hydro(I) Extra Mortality		
FLUSH/TRANS1	A3 > A2 by at least 0.05	Both actions meet standard
CRiSP/TRANS4	A3 > A2 by at least 0.05	Both actions meet standard
NMFS	A3 > A2 by at least 0.05	Both actions meet standard
Hydro(II) Extra Mortality		
FLUSH/TRANS1	A3 > A2 by at least 0.05	Both actions meet standard
CRiSP/TRANS4	A3 > A2 by at least 0.05	Both actions meet standard
NMFS	A3 > A2 by at least 0.05	Both actions meet standard

C. Experimental actions to test hypotheses

Three experimental actions have been proposed to better estimate current levels of D:

a) Continue current transportation operations and monitor D (described in Section 3.1)
b) Modify transportation operations and monitor D (described in Section 3.2)
c) Turn transportation on and off in alternating years (described in Section 3.3)

An important point here is that in many cases the retrospective D appears to have greater implications for long-term decisions that the prospective D. However, continued monitoring of D and/or experimental manipulations of transport to get better estimates of current D levels tell us little about what D was in the

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[6] The FLUSH and CRISP D hypotheses do not directly correspond to results in these figures because these results are based only a subset of runs, and because CRiSP and FLUSH specified different D values in each year. However, these hypotheses can be approximated by taking the weighted average of D values specified in each year (weighted by the frequency with which each year is selected in prospective modeling), then rounding the weighted average to the nearest increment of 0.1.

past, and thus give us little direction for making long-term decisions. Historical transport:control and in-river survival data is available, but has been applied in different ways using different assumptions to estimate retrospective D values. PATH has recently initiated work to lay out these assumptions in a common framework to allow a more direct comparison of different historical estimates of D and, to the greatest extent possible, resolve differences in these estimates.

2.2.3.2 Fall chinook

A. Description of hypotheses

There is much less information with which to estimate D values for fall chinook than spring/summer chinook because no transportation studies have been conducted for Snake River fall chinook. However, a number of indirect estimates of D have been made (PATH Fall Chinook Decision Analysis Report). Based on these indirect estimates, PATH has modeled a set of 4 D hypotheses:

D1. $D_R = 0.10$ (est.), $D_P = 0.24$ (fixed)
D2. $D_R = 1.0$ (fixed), $D_P = 1.0$ (fixed)
D3. $D_R = 0.10$ (est.), $D_P = 0.10$(est.)
D4. $D_R = 0.20$ (fixed), $D_P = 0.20$ (fixed)

B. Implications of hypotheses for selecting a long-term action

We have not completed an intensive sensitivity analysis of fall chinook D values where model runs were produced for all combinations of retrospective and prospective D values, as we did for spring/summer chinook D values. To show the implications of D, we show a set of results (24-year survival and 48-year recovery probabilities; 100-year survival probabilities are very similar to 24-year survival probabilities) for each of the four D hypotheses (Figure 2.2-7). For the two D hypotheses where STEP was estimated (D2 and D4), we show the results for each extra mortality hypothesis separately.

D hypotheses D1, D2, and D4 have essentially the same implications for selecting a long-term action in terms of the 24-year survival standard: A2 and A3 probabilities are essentially the same (within around 0.05 of each other), and all actions are above the 0.7 standard. With hypothesis D3, 24-year survival probabilities for A3 exceed those of A2 by more than 0.05, but both actions meet the standard. With the 48-year recovery standard, D3 and D4 (with all extra mortality hypotheses) give essentially the same result: A3 recovery probabilities exceed A2, with A3 meeting the standard but not A2. With D1 and D2 (Hydro extra mortality hypothesis), A3 again exceeds A2, but both actions meet the standard. With hypothesis D2 plus the regime shift and here to stay extra mortality hypotheses, there is no difference in probabilities between the actions, and both actions meet the standards. These conclusions are summarized in Table 2.2-7.

Table 2.2-7: Implications of D hypotheses for long-term decisions on fall chinook.

		24-year Survival		48-year Recovery	
D Hypothesis	**Extra Mortality**	**Comparison of Actions**	**Ability to Meet the Standard**	**Comparison of Actions**	**Ability to meet the standard**
D1	N/a	A2 ≈ A3 (within 0.05)	Both actions meet standard	A3 > A2 by at least 0.05	Both actions meet standard
D2	Regime	A2 ≈ A3	Both actions meet standard	A2 ≈ A3	Both actions meet standard
	Here to stay	A2 ≈ A3	Both actions meet standard	A2 ≈ A3	Both actions meet standard
	Hydro	A2 ≈ A3	Both actions meet standard	A3 > A2 by at least 0.05	Both actions meet standard
D3	N/a	A3 > A2 by at least 0.05	Both actions meet standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
D4	Regime	A2 ≈ A3	Both actions meet standard	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	Here to stay	A2 ≈ A3	Both actions meet standards	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard
	Hydro	A2 ≈ A3	Both actions meet standards	A3 > A2 by at least 0.05	A3 meets standard A2 does not meet standard

The critical distinction here appears to be between hypotheses where D is low and constant between retrospective and prospective periods (D3 and D4), and hypotheses where D is either constant and high (D2) or improves from the retrospective period to the prospective period (D1). With D3 and D4, there is a larger difference between actions, both relative to one another and relative to the standards, than with hypotheses D1 and D2.

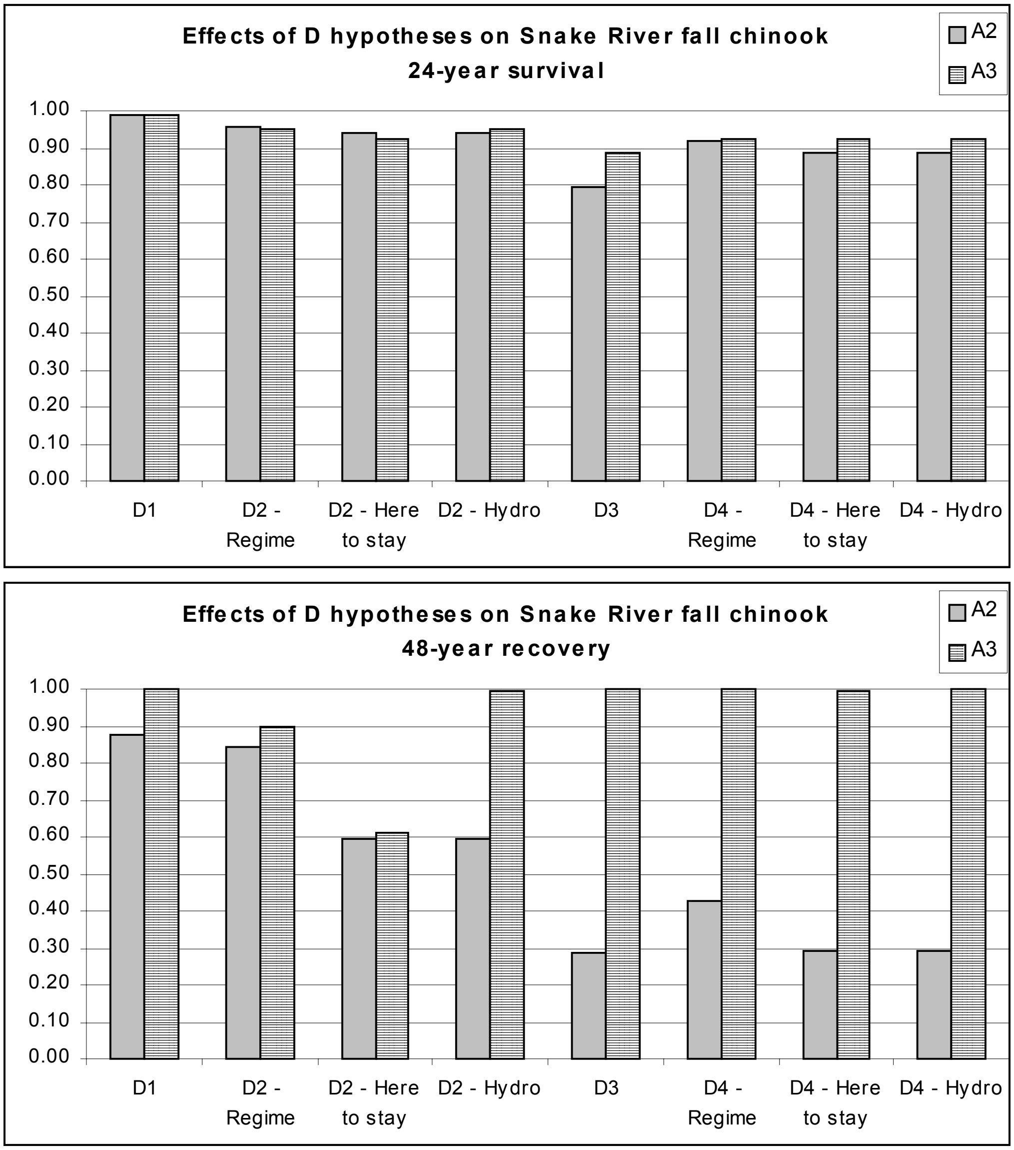


Figure 2.2-7: Effects of D hypotheses on survival and recovery probabilities for Snake River fall chinook.

C. Experimental actions to test hypotheses

The experimental actions that have been proposed to better estimate current levels of D for Snake River fall chinook are the same as the actions proposed for spring/summer chinook:

a) Continue current transportation operations and monitor D (described in Section 3.1)
b) Modify transportation operations and monitor D (described in Section 3.2)
c) Turn transportation on and off in alternating years (described in Section 3.3)

The three experimental actions for transportation will provide information on the current level of D, which will help to narrow down the possibilities by distinguishing between hypotheses D3 (relatively low prospective D), D1/D4 (moderate prospective D) and hypothesis D2 (relatively high prospective D).

However, if the prospective D turns out to be a moderate value (i.e., around 0.2), experimental actions will provide little information on historical levels of D, and therefore will not help to distinguish between hypotheses D1 and D4. These two D hypotheses have quite different effects on the relative ability of the actions to achieve the 48-year recovery standard, although both result in A3 recovery probabilities exceeding A2.

2.2.4 Interactions Between Extra Mortality Hypotheses

A potential drawback of focusing on single hypotheses one at a time is that there may be interactions between them (Table 2.2-8). This can lead to incorrect conclusions about the causes of observed effects. These interactions create confounding if multiple actions are undertaken simultaneously or in an uncoordinated fashion, yet multiple actions may have greater benefits to stocks (the tradeoff between conservation and learning objectives). Table 2.2-8 provides a starting point for identifying some of the potential interactions between extra mortality hypotheses.

Table 2.2-8: Possible interactions among extra mortality hypotheses that could cause confounding.

	Hydro	**Stock Viability**	**Regime shift**	**Hatcheries**	**Birds**
Hydro					
Stock Viability	Dam passed fish are more susceptible to disease. Dams reduce adult returns which leads to reduced nutrients in natal streams.				
Regime shift	Hydro-weakened fish are more vulnerable to worsening of ocean conditions.	BKD weakened fish are more vulnerable to worsening of ocean conditions.			
Hatcheries	Crowding at dams, barges and estuary with hatchery fish leads to more stress. Hatchery fish caused numerical response in reservoir predators. Hatchery fish swamp reservoir predators reducing predation rate on wild smolts.	Overlap in mechanisms (e.g., transmission of BKD from hatchery fish to wild fish)	Increased competition for food in estuary due to both more hatchery fish and less food in ocean.		
Birds	Rice Island created from reservoir dredging (i.e., an indirect hydro-system effect). Barged fish may be more vulnerable to birds.	Less viable fish (e.g., due to disease, genetics, or reduced nutrients in rearing streams) more vulnerable to bird predation.	Less food for birds elsewhere, or estuary makes salmon a larger part of bird diet.	Increases in hatchery releases have stimulated the increase in number of birds. Hatchery fish may swamp avian predators in estuary	

2.3 Experimentation vs. Monitoring

Ideally, management actions should be instituted under a strict experimental design to ensure that the population responses can be related to the chosen action (treatment) with reasonable confidence. In addition, this management experiment requires a monitoring program to generate the data needed to evaluate the experimental action(s).

The purpose of this section is to provide a general discussion of some of the challenges in designing good management experiments.

2.3.1 Aspects of Good Experimental Design

Good experimental design includes:

- a clear statement of goals or hypotheses to be tested;
- a thorough description of how the data will be analyzed;
- an explicit description of the experimental unit and the variables to be measured;
- an explicit definition of the effect size of importance and agreement on the criteria that will determine whether the objectives have been achieved;
- an assessment of statistical power;
- a randomized assignment of treatments to (randomly selected) experimental units;
- spatial and temporal replication of treatments;
- independence among experimental units;
- contrasts in treatment strength;
- the use of controls;
- the use of blocking/stratification; and
- the interspersion of treatments in space and time (i.e., all treatments do not start at the same time to avoid confounding with large-scale environmental trends such as changes in ocean conditions).

These components reduce confounding and increase the probability of drawing the correct conclusion at the end of the experiment. As a consequence, experiments that follow all of these principles stand a very high chance of generating useful evidence for management decisions. Failure to consider these issues can ultimately lead to bad decisions. The rationale for designing experiments that have the aforementioned characteristics is very well documented (see Green 1979; Hurlbert 1984; Schwarz 1998; Hairston 1989 and references therein).

Challenges in Designing Management Experiments

When conducting large-scale manipulations of natural systems, it is extremely difficult to adhere to all of the standards of judicious experimental design. In particular, it is rarely possible/practical to spatially replicate treatments that involve large-scale manipulations. Other possible limitations include:

- lack of suitable controls;
- lack of baseline information;
- difficulty in randomly assigning treatments to experimental units; and

- irreversible treatments/actions (are more likely to get confounded with large-scale environmental changes).

For example, with the exception of carcass introductions and/or stream fertilization (which could be implemented for some Snake River salmon natal streams with others as controls) all of the actions in Table 2.3-1 involve applying the same treatment to all Snake River index stocks. Thus, true replication of many experimental treatments cannot occur over space and time, but only in time. Yet some actions (e.g., dam breaching) obviously can not be switched on and off for reasons of cost, which increases the chances of confounding with climate variations. Other actions (e.g., transportation, hatchery operations, changes to bird habitat, flow) could be changed from year to year, and therefore potentially reduce confounding. Yet getting a clear signal from these actions can also be challenging. In his review of Chapter 6 of the FY98 report, Carl Walters noted (see Appendix F) that:

> for reversible treatments (transportation, hatchery, flow, etc.) confounding of treatment responses with other factors possibly causing Z change [total mortality rate] can only be avoided by interspersing (blocking) treatment and reference comparisons, i.e., by regularly operating the system under a reference treatment option in order to measure changes over time in Z due to factors other than the management treatment. This requirement for temporal reference comparisons greatly increases the time needed for effective experimentation… (Marmorek et al. 1998).

Table 2.3-1: Major categories of actions (individual actions described in more detail in Section 3). Definitions for acronyms are provided in Appendix A.

Uncertainty	Hypothesis	Action (Report section that describes action in more detail)
Transportation		Continue current operations and measure D (3.1) Modify transportation and measure D (3.2, 3.3)
Extra Mortality	Hydro	Breaching of Snake River dams 2 dams (3.4) 4 dams (3.5)
	Stock Viability	Carcass introductions / stream fertilization (3.6)
	Hatcheries	Manipulate hatchery production or operations (3.7)
	Birds	No action considered
	Regime Shift	No specific action; monitor ocean conditions and productivity of various stocks, or vary % of fish transported, timing of transportation, method of transportation (3.10)

Despite limitations such as these, it is still important to design the best experiment possible to improve confidence in the inferences drawn at the end of the experiment and in turn improve management decisions based on those inferences

One particularly useful approach is the Before-After-Control-Impact paired design (or BACI-P) wherein control and treatment stocks are monitored both before and after some management intervention, and one examines the difference in performance indicators over time (Figure 2.3-1). The advantage of this design is that the control and treatment stocks do not need to have exactly similar characteristics, as one is looking for 'changes in the differences', as illustrated in Figure 2.3-1. 'Replicate tributaries and stocks' don't exist anyway, and this design can help get around that problem. Schwarz (1998) has an excellent discussion of experimental designs for adaptive management studies.

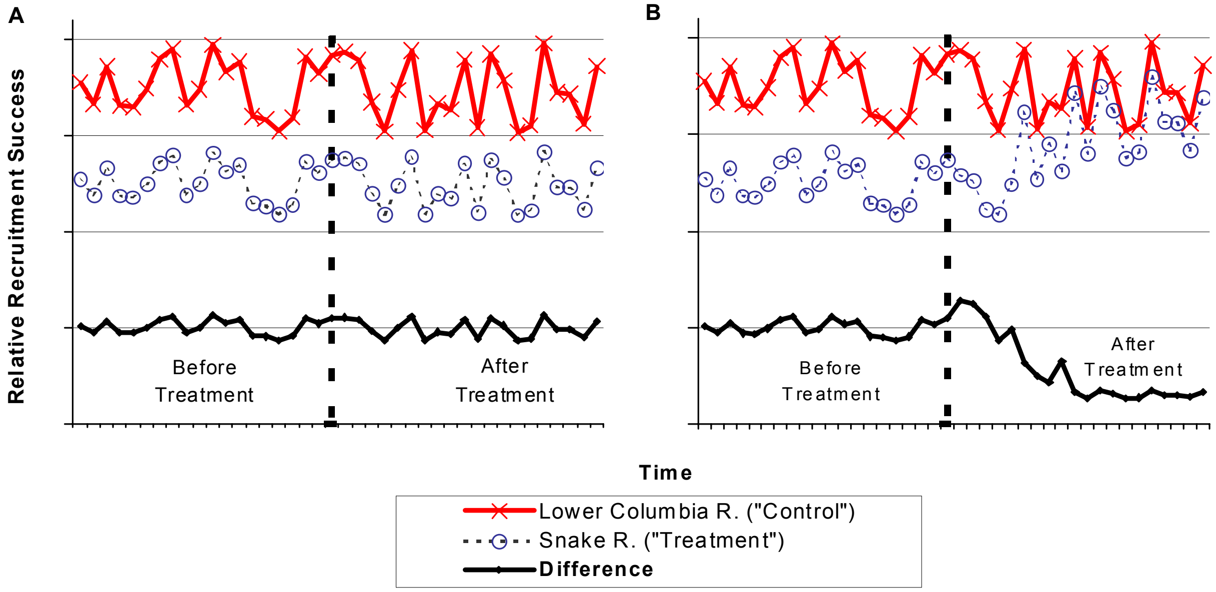


Figure 2.3-1: The BACI-P design. The change in a measured variable from multiple random sampling (before and after the impact) in both control and impact sites. Panels A and B represent two alternative outcomes of the same experiment. In panel A, there is no impact and the mean level of the difference (bottom-most line) is constant over time. In panel B, the treatment has an impact, and the mean level of the difference (bottom-most line) changes over time. With adaptive management experiments, one would hopefully see a smaller difference after treatment, *relative to controls*, rather than an increasing one for survival or abundance based indicators (the opposite would be true for mortality based indicators). Adapted from: Schwarz (1998).

Simple BACI-P designs do not account for transient time effects (Schwarz 1998). This can be dealt with by pairing surveys starting at several different time points (e.g., the target population is exposed to treatment A for some period, treatment B for the next period, and then to treatment A again); (Schwarz 1998). However, this important design refinement is only practical for "reversible" management actions (e.g., such as fertilization experiments that can be turned "on" and "off").

Design of adaptive management experiments in the Columbia Basin is made more challenging by the need to take into account a myriad of factors that simultaneously affect the region's salmon populations such as climate variation, mixed stock fisheries, migration efficiency past dams, hatchery production, and the effectiveness of barging fish. Under these conditions, the adaptive management plan will need to be formulated using multi-stage (e.g., nested and staircase) methods to permit as clear a separation of the effects as possible (Walters and Holling 1990; and see Schmitt and Osenberg 1996 on multi-stage designs).

Errors of Inference

Of particular concern in adaptive management experiments are two errors of inference that may occur at the end of an experiment. These "errors in inference" are shown in (Table 2.3-2).

Table 2.3-2: Errors of inference. The "Null" hypothesis is that which states that some factor has no effect on some other variable. P(β) = the probability of committing a Type II error; P(α) = the probability of committing a Type I error. Adapted from Peterman (1990).

	Outcome	
Actual State of Nature	**Accept Null Hypothesis**	**Reject Null Hypothesis**
Null Hypothesis True	Correct; $P(1-\alpha)$	*Wrong* (**Type I error**); $P(\alpha)$
Null Hypothesis False	*Wrong* (**Type II error**); $P(\beta)$	Correct; $P(1-\beta)$

One error occurs when the researcher concludes that the alternative hypothesis is true when it is not. In classical terms, this is known as the Type I error, or the probability of incorrectly rejecting the "null" hypothesis (e.g., wrongly convicting an innocent suspect). The other error occurs when the researcher concludes that the alternative hypothesis is wrong when it is correct. This is known as Type II error, or the probability of failing to detect a true effect (e.g., wrongly acquitting a guilty suspect).

In resource management, the cost of these two errors may be unequal and the costs may be borne by different groups. For example, suppose we conduct an experiment to test the hypothesis that hatcheries cause most (>50%) of the extra mortality of Snake River chinook. At the end of the experiment the data are analyzed and some form of hypothesis test is conducted. Decisions about both hatchery and hydro-system operations will be based upon the results of this test. Now suppose the hatcheries in fact do not cause this much extra mortality, yet due to some confounding effect the results of the experiment suggest that they do and hatchery operations are changed. This is a Type I error. The groups that operate and rely on these hatcheries would suffer financially and socially.

The costs of Type II errors (which have traditionally been given less attention) may be even greater, and affect the ecosystem in ways that (by definition) are undetected. To continue with the previous example, suppose hatcheries do in fact cause more than 50% of the extra mortality of the Snake River chinook, but an experiment does not detect this (Type II error). Hatcheries would then be allowed to continue their current operations, and the consequences would include both the effects of extra mortality on Snake River chinook, and the future costs of correcting the damage (if possible) when the error is discovered.

Statistical power analysis is the technique used to quantify the probability of Type II errors (Peterman 1990). Statistical power is defined as the probability of correctly detecting some specified effect size, given a particular situation (the term '$1-\beta$' in Table 2.3-2). Power is a function of four factors: α (which is usually 0.05), the true effect size, sample size, and sample variance (Peterman 1990). Power increases with increasing α, effect size, and sample size, but decreases as sample variance increases because the greater level of "noise" tends to mask the true effect. In deliberately designed manipulative experiments on Columbia River chinook, researchers can, in theory, change all four factors to increase power. For instance, all else being equal, a larger effect size (e.g., more fertilizer, or greater reduction in numbers of hatchery steelhead smolts released) will make it easier to detect the "signal," or response to the manipulation, amidst the background variation. Similarly, larger sample size (e.g., more tagged groups of fish involved in a modified transportation regime) will increase the precision in estimates of changes in survival rates. Researchers can reduce residual (unexplained) variance by choosing a more rigorous design than simply a before/after setup (e.g., an enhanced BACI-P design with several stocks in different spatial locations sampled at several times both before and after the experiment starts). Such a design will improve the estimate of the effect of the experimental manipulation by permitting a separate estimate of the portion of total variance in survival rates that is attributable to spatial differences among stocks within years, to differences among years within stocks, etc. Finally, α values of 0.05 have become fairly ingrained. But in most situations, increasing it to 0.1 makes very little difference. Appendix B outlines the steps of an *a priori* (before the fact) power analysis.

Trade-offs

PATH's challenge is the design of management experiments that have both high statistical power and acceptable probabilities of achieving conservation objectives. Thus resource managers must explicitly consider the value or cost associated with both Type I and Type II errors (see Peterman 1990). However, the experimental ideal of high statistical power must be compared to the economic and environmental cost of obtaining that level of power. In the context of PATH, the tradeoff that must be considered is that between *learning* objectives and *conservation* objectives. A particular experimental design may give high power to detect effects, and thus learn a lot about the relative probability of alternative hypotheses, but the duration or magnitude of the treatment effect required to achieve this power may put stocks at a lower probability of survival or recovery over the short run than another experiment of lower power. In the end, the best compromise will likely be the management experiment that provides the best balance between the cost of the experiment and the improvement in the odds of achieving conservation objectives created by reducing probabilities on improbable hypotheses (see Walters and Green 1997). This narrows the range of possible outcomes and thereby reduces the chances of making incorrect long-term decisions. Thus, if the only feasible experiment (in terms of short-term conservation risks and cost) turns out to have "low" power, it may still be worthwhile (Walters and Green 1997).

Assuming a good experimental design has been used, conservation and monitoring performance objectives can be used to explore tradeoffs between management and learning. For each experimental design and underlying set of hypotheses linking actions to effects (ideally using simpler models), PATH would estimate the probability of survival and recovery and the probability of detecting a true effect of a desired magnitude (this involves simulating future data collection, with process and observation error). Table 2.3-3 illustrates how this tradeoff might occur for an evaluation of four hypothetical experimental options. We emphasize that this example is hypothetical and is not meant to imply what PATH will find. One design (Exp1) shows a high probability of meeting jeopardy standards, but the monitoring analysis show that it has a poor ability to generate detectable effects. A second design (Exp4) meets the conservation standards equally as well as Exp1, but it has a higher ability to detect experimental effects. Exp2 has high ability to detect effects, but does not meet conservation objectives as well as Exp1 or Exp4. Exp3 is rated last for all three categories. Description and examples of more detailed quantitative analyses are provided in Section 5.

Table 2.3-3: Rating of how well four hypothetical designs meet management and learning objectives. "Exp" = experiment.

	Management		**Learning**
Experimental Design	**Pr(recovery)**	**Pr(survival)**	**Ability to detect effects**
Exp1	high	high	low
Exp2	medium	medium	high
Exp3	low	low	low
Exp4	high	high	high

2.3.2 Monitoring

It is pointless to consider doing adaptive management experiments without a considerable commitment to monitor responses to the experimental actions over time (Walters 1986). To infer cause-effect relationships in the population/system's response, one must estimate changes in the experimental units to which the alternative treatments were applied. By controlling *what* is measured, *how frequently* these variables are measured, and the precision of the methods/instruments used to measure these variables, managers can determine whether they will be able to detect effect sizes of interest with the desired probability and cost. Table 2.3-4 summarizes what life stage survivals and other key performance measures are currently being monitored, for different stocks and regions. [Table needs to be updated/completed.]

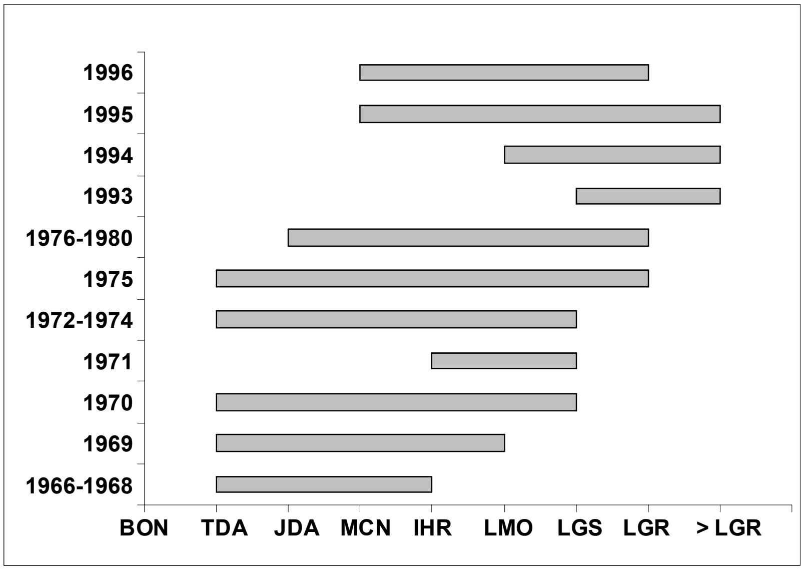


Figure 2.3-2: 1966-1996 Reach Survival Data for Spring-Summer chinook. This figure supplements the information in Table 2.3-4.

Table 2.3-4: Spatial extent and frequency of general monitoring information collected for different stock groups. Does not include detailed project-specific measurements (e.g., FGEs, bypass survival, spill survival). Year in brackets is earliest year of data. [NOTE: Table is incomplete.]

Variable	**Spring/Summer chinook**			**Fall chinook**			**Steelhead**		
	Lower Columbia	*Snake*	*Upper Columbia*	*Lower Columbia*	*Snake*	*Upper Columbia*	*Lower Columbia*	*Snake*	*Upper Columbia*
Escapement	6 stocks: Wind (1973), Klickitat (1966), Warm Springs (1969), 3 John Day (1959) {Annual redd or weir counts}	7 stocks: Minam (1954), Imnaha (1952); since (1957): Poverty Flat, Johnson Ck., Sulphur Ck . Bear Valley, Marsh Ck {Annual redd or weir counts}	3 stocks: Entiat (1955), Methow (1960), Wenatchee (1958)	2 stocks Lewis (multiple survey peak counts since 1964), Deschutes (Mark recapture + redd counts since 1977)	1 stock: (dam counts since 1964)	1 stock Hanford (Interdam adult count differences since 1964)	?	LGR dam	mainly dam counts
Smolt to adult survival (SAR)	Warm Springs smolt trap since ?	Dam counts (1964); PIT-tags (1993– some just hatchery fish)	Priest Rapids dam counts; hatchery SARs	?	mostly hatchery; some wild after 1992	?	Yakima?	LGR dam count	Priest Rapids dam count
T:C Ratios	n.a.	Periodic transportation expts (now with PIT tags)	No data?	n.a	No transportation expts. Some PIT-tag data for 1995	Some transportation expts (1978-1983)	?	?	?
In –river reach survival through hydro-system	?	Various studies (see Fig 2.3-2)	?	?	1995-98 PIT-tag studies from LGR to LMO	1998 PIT-tag study (MCN to JD)	?	?	?
Parr density/size	?	since 1984	?	?	USFWS?	?	?	since 1984	?
Barge/truck survival	n.a.	No data	No data	n.a.	No data	No data	n.a.	No data	No data

Limitations of Monitoring in Resolving Key Uncertainties

Expanded PIT-tag monitoring may provide improved estimates of several variables, including reach survival (from LGR to BON), T:C ratios, "D" and extra mortality of in-river fish. The value of D affects the sensitivity of results to extra mortality hypotheses (greater sensitivity at higher D values). However, monitoring alone is insufficient to determine what factors **control** extra mortality. Another five to ten years estimates of the extra mortality of in-river fish and 'D' values will only shed light on the key driving factors behind these variable (e.g., hydro, hatcheries, climate) to the extent that there are contrasting variations in these factors over time and/or space. The confounding of extra mortality factors during the post-1975 period is evidence for this assertion: ocean conditions deteriorated, hatchery output and transportation both increased, and the four Snake River dams were established (Figure 2.3-3). Despite extensive monitoring of salmon over this period, and upstream-downstream contrasts, we are unable to conclusively identify the relative importance of each factor (PATH Weight of Evidence Report, SRP 1998).

The ability to learn is greatly enhanced when current or expanded monitoring is coupled to experimental management actions. Hydro-system and hatchery operations, predator abundance and/or stream nutrient levels could be deliberately manipulated to both recover endangered stocks and create contrasts in conditions to understand the importance of these factors. As stated, the hydro-system extra mortality hypothesis ("here to stay until the Snake River dams are removed") appears to only be testable by breaching one or more dams. While ocean conditions may be indirectly affected by human activities (e.g., anthropogenic atmospheric emissions may change ocean conditions via global warming), ocean conditions are clearly not amenable to direct experimental manipulation, unlike the other factors listed. Thus the regime shift hypothesis can only be tested passively. It is important, therefore, that experimental management actions and associated monitoring attempt to create a signal that is unlikely to be confounded by natural climatic fluctuations. There are also potential tradeoffs between doing multiple simultaneous actions to maximize the potential for recovery (to meet the conservation objective) and increasing the chances that several experimental management actions will confound the interpretation of which factors are most important (to meet the learning objective).

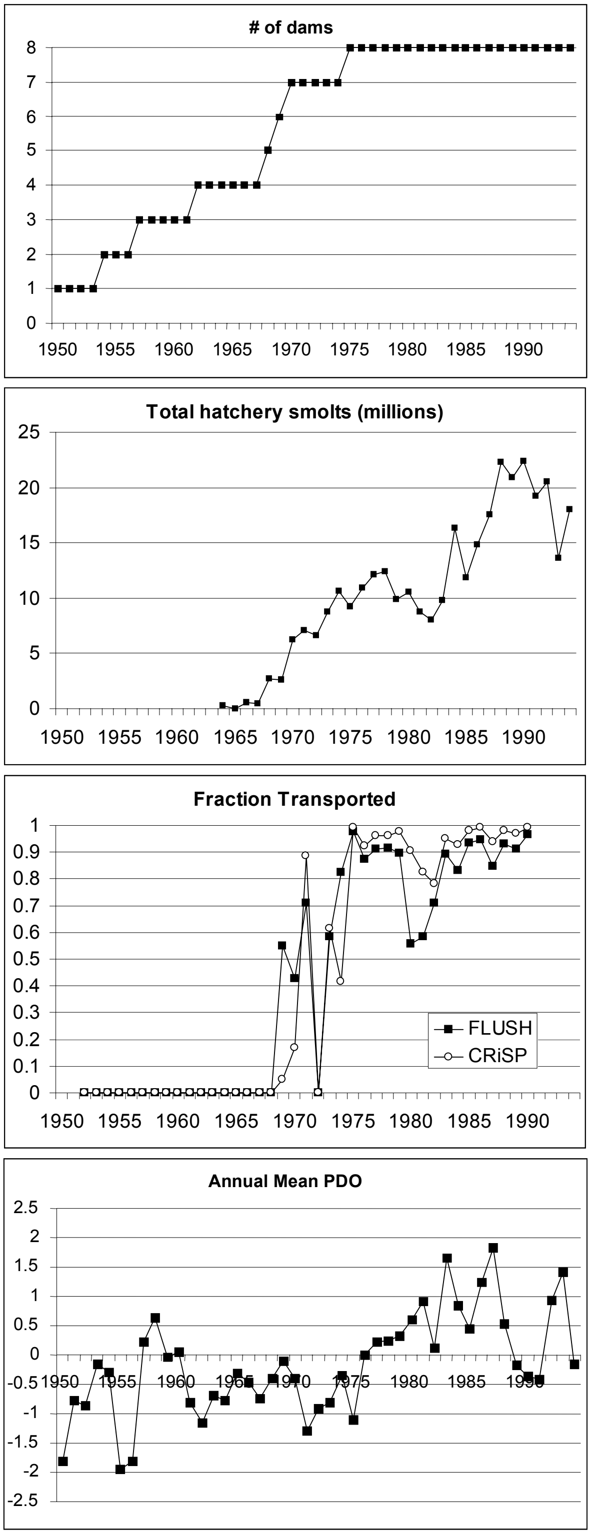


Figure 2.3-3: Historical changes in # dams passed by Snake River fish, total hatchery output, fraction of fish below Bonneville that were transported and Pacific Decadal Oscillation (PDO).

Variables to Monitor

Ideally, the variables that are monitored in conjunction with an adaptive management experiment will have the following attributes:

- link directly to the hypothesis being tested, and at least indirectly to the models being used to forecast future conditions;
- be relevant to management actions as well as sensitive to them;
- have a tendency to change in a consistent manner with respect to the type and magnitude of treatment; and
- provide the ability to be estimated from sampling that is precise and cost efficient (McAllister and Peterman 1992).

For large-scale ecological issues, there is a tendency to identify a large number of variables and try to monitor them all. This can create difficulties in two ways: 1) a myriad of indicators can deliver a contradictory signal with no clear message; or 2) monitoring budgets are exceeded, leading to a loosely planned prioritization that may drop important indicators in favor of those more easily measured. For these reasons, the aforementioned attributes are worth keeping in mind when selecting monitoring variables for use in PATH adaptive management experiments.

The purpose of this section is to collect and summarize descriptions of variables that might be used in experiments or monitoring. This information should include: variable name or symbol, definition, how is it estimated (direct or indirect), how often it is estimated, and how is it used (what information can it provide to directly test a specific hypothesis). Representative examples are described below.

R/S (recruits/spawner): The number of mature fish returning to the point of recruitment (R) divided by the number of spawners in the parent generation (S). R is estimated indirectly from catch, upstream mortality estimates, % hatchery adults on spawning ground and escapement information (see Beamesderfer *et al.* 1997). S is estimated from weir counts, or redd counts (sp/sum) or dam counts (fall chinook).

Residuals from graphs of ln(R/S) vs. S (RRS): This is the difference between the observed and expected R/S at a given spawning density when the data are fit to a model of the form: ln(R/S) vs. S. Variation in residuals will influence the ability of a monitoring design to detect changes in R/S. Time-series patterns in residuals can also provide information about influences on a stock over time. Using multiple stocks to estimate common year effects can filter out temporal variation and make it easier to detect treatment effects.

SAR: Smolt to adult survival rates (SARs) estimate survival rates of fish from the time they pass the upper-most dam as smolts to the time they return as adults. SARs, in conjunction with estimates of V_t (the direct survival rate of transported fish in the barge/truck) and V_n (the direct survival rate of non-transported fish through the hydropower system) allow inferences on ocean survival. There is value in measuring SARs as well as R/S and RRS since R/S and RRS alone cannot differentiate between different life history stages.

T:C : The Transport: Control ratio is the ratio of transported fish survival to in-river fish survival from juveniles at the collection point to adults at the same point. T:C is estimated through tagging experiments. It is essential for estimating D.

V_n : the direct passage survival of in-river juvenile fish, measured from the head of Lower Granite pool (spring/summer chinook) or the face of Lower Granite dam (fall chinook) to the tailrace of Bonneville dam, including reservoir and dam survival at each project.

λ_n post-Bonneville survival factor for non-transported smolts. For spring-summer chinook, this variable is estimated indirectly, and depends on many other variables: the total mortality (*m)* including both passage and extra mortality; the direct mortality estimated from passage models (*M*); the fraction of fish below Bonneville which were transported (*P*); and *D*. This factor will be very strongly affected by assumed D values as long as smolt transportation continues. For details see the PATH Preliminary Decision Analysis report (pg. A-92), *PATH Weight of Evidence Report* (pg. 80) and the Delta model description attached to the AFISH Appendix.

μ (Delta model only): the incremental total mortality between Snake River Basin and the John Day project in a specific year. This variable is estimated indirectly – see Deriso *et al.* 1996.

m (Delta model only): total direct mortality rate of Snake River spring/summer chinook, including both passage and extra mortality, but excluding year effects that affect both lower Columbia and Snake River stocks. **m** is estimated from spawner-recruit data for lower Columbia and Snake River stocks.

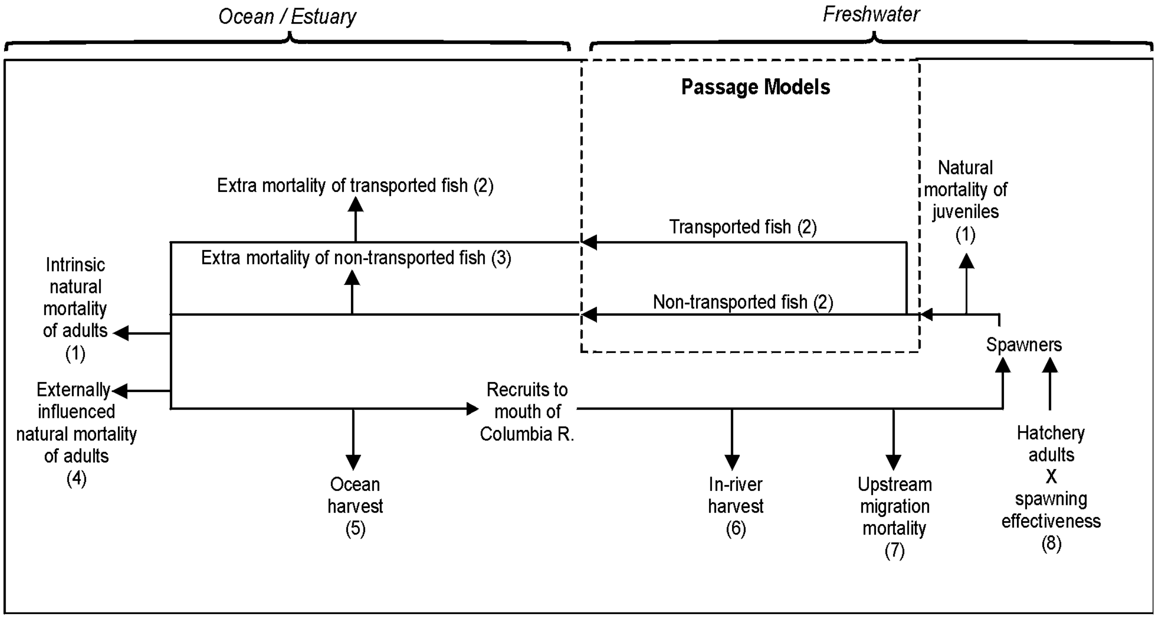
$\Delta \lambda_n$: the change in the post-Bonneville survival factor for non-transported smolts after an action (i.e., $\lambda_{n,\ \text{after action}} / \lambda_{n,\ \text{before action}}$). This variable is potentially useful for differentiating among extra mortality hypotheses.

Which of these candidate variables are appropriate and feasible to monitor? To answer this question we need a framework for assessing how these variables might change in response to management actions, and how these variables interact to affect the recruits returning to the mouth of the Columbia River, and the number of fish returning to their spawning area. Figure 2.3-4 provides such a framework; we summarize each of the terms in this figure, indicating how each might be affected by various management actions, and then consider what monitoring is both appropriate and feasible.

The following numbered paragraphs refer to Figure 2.3-4.

1. The *stock recruitment function* contains several parameters:
    - the stock's intrinsic productivity (Ricker *a* parameter, which reflects natural productivity and mortality),
    - the spawning level generating maximum recruitment (*1/b*), and
    - a parameter to potentially account for less recruitment at low spawning levels (*p*).

    Habitat restoration actions could affect both stock productivity (*a)* as well as the spawning level generating maximum recruitment *(1/b)*. Mainstem actions (e.g., dam breaching, changes in hatchery operations, changes in transportation) are accounted for in different terms.



$$\text{Recruits to mouth of Columbia R.} = \begin{bmatrix}\text{Stock recruitment}\\ \text{Function}\\ (1)\end{bmatrix}\begin{bmatrix}\text{System}\\ \text{Survival}\\ (2)\end{bmatrix}\begin{bmatrix}\text{Post-Bonneville}\\ \text{survival of non-}\\ \text{transported fish}\\ (3)\end{bmatrix}\begin{bmatrix}\text{Climate}\\ \text{Factor}\\ (4)\end{bmatrix}\begin{bmatrix}\text{Ocean}\\ \text{Harvest}\\ (5)\end{bmatrix}$$

$$\text{Spawners} = \underbrace{\begin{bmatrix}\text{Recruits to mouth}\\ \text{Of Columbia R.}\end{bmatrix}\begin{bmatrix}\text{In-river}\\ \text{Harvest}\\ (6)\end{bmatrix}\begin{bmatrix}\text{Upstream}\\ \text{migration}\\ \text{Mortality}\\ (7)\end{bmatrix}}_{\text{Wild spawners}} + \begin{matrix}\text{Hatchery}\\ \text{Spawners}\\ (8)\end{matrix}$$

Figure 2.3-4 Factors influencing the number of recruits returning to the mouth of the Columbia River, and the number of fish returning to their spawning area. The dashed square in the top half of the figure represents the part of the life cycle considered by passage models. The equation at the bottom half of the figure is meant only to illustrate the factors which combine to influence computed recruits and spawners. The actual equations for spring-summer chinook are in the PATH Preliminary Decision Analysis Report (March 1998) and for fall chinook in the PATH Decision Analysis Report for Snake River Fall Chinook (September 1999).

2. *System survival* estimates the overall survival of smolts through the hydrosystem, from the head of the first reservoir to below Bonneville Dam, but also including the post-Bonneville survival of transported fish. As illustrated in the top half of Figure 2.3-4, system survival is affected by:

   - the survival of in-river smolts from the first reservoir to Bonneville (*Vn*), estimated from passage models and/or PIT-tag studies;
   - the survival of transported smolts in barges or trucks (*Vt)*, assumed to be very high (e.g., 0.98);
   - the proportion of smolts below Bonneville which were transported in each year (*Pb_t*), which depends on the overlap in timing of smolt migration and transportation programs; and

- an estimate of the extra mortality of transported fish, which depends on an estimate of *D* (the ratio of post-Bonneville survival of transported smolts to post-Bonneville survival of non-transported smolts).

Changes in mainstem hydrosystem and hatchery operations can affect system survival.

3. *Post-Bonneville survival of non-transported fish (λ_n).* This factor is estimated indirectly and depends on many other variables (see above description of λ_n). It's what's left over after accounting for everything else. The extra mortality hypotheses described above (Hydrosystem, SV, Regime Shift) are different explanations of how λ_n changed in the past, and might change in the future.

4. The *climate factor* accounts for changes in survival other than those due to the stock recruitment function, system survival, and post-Bonneville survival of non-transported fish. These climatic variations could affect any life history stage, though in Figure 2.3-4 we show only the example of changes in estuary and ocean conditions. PATH has used downstream index stocks as measures of regional climatic variations also affecting Snake River stocks (e.g., 6 lower Columbia River spring/summer chinook stocks; the Deschutes River fall chinook stock) or considered oceanographic and flow indices as climate indicators. Columbia River management actions do not directly affect climate, but changes in climate concurrent with changes in management actions can make it difficult to interpret the true benefits of the actions.

5. *Ocean harvest* is important for fall chinook, but is negligible for spring/summer chinook. Harvest actions can affect the number of spawners but do not affect the number of recruits as harvest fish are included in total recruits. Ocean harvest rates are estimated primarily through recoveries of coded wire tags from hatchery fish.

6. *In-river harvest* is important for both fall and spring/summer chinook from the Snake River. Estimates of fish harvested in-river are also included in total recruits.

7. Historical estimates of *upstream migration survival* rates (also called conversion rates) are used in the run reconstructions to estimate total recruits. Dam breaching could potentially improve upstream survival.

8. For fall chinook, the *effective number of spawners* is the sum of the wild spawners and successful hatchery spawners (i.e., those that contribute to future generations). The effectiveness of hatchery spawners (and therefore supplementation actions) is an important factor for fall chinook. Though there are hatcheries for spring/summer chinook, they do not overlap significantly with the areas used by wild spawners.

Selecting Appropriate Monitoring Variables

Consider a mainstem experimental management action, such as a change in transportation, dam breaching, or changes in hatchery operations. What would you monitor to assess the effects of these actions? This is the most difficult monitoring problem as there are no obvious spatial controls (unlike, for example, stream fertilization experiments, which can be applied to some streams and not to others). We discuss this problem using a mathematical approach in Appendix D, and in a more qualitative way here. Mainstem management actions could affect both system survival (i.e., Vn, Pb_t, D), and the post-Bonneville survival of non-transported fish (λ_n). While Vn and Pb_t can be estimated with reasonable precision, estimating D and λ_n precisely is much more difficult, as these are derived variables that depend on many other measurements (such as T:C ratios and Vn), and the errors accumulate. Thus one can estimate changes in passage survival, but it will be more difficult to get a precise estimate of changes in system survival, and even more difficult to estimate changes in λ_n, which relate directly to the alternative extra mortality hypotheses described earlier in Chapter 2. To estimate changes in λ_n, one needs to know all the terms in the Figure 2.3-4, both prior to the management action, and subsequently. Even if measurements taken after the action are more precise than those in the past, the uncertainty in historical measurements will affect the ability to estimate changes.

What about recruits per spawner (R/S) as a measure of overall benefits of a mainstem management action? As long as no habitat restoration actions occurred concurrently for a given stock, the inherent stock recruitment function is likely to remain unchanged, but climatic variations could either improve or worsen during the experimental period (in either the freshwater or ocean phase). Therefore, climatic shifts could cause a change in recruits per spawner (R/S) to be misinterpreted as a result of the management action, when in fact it was due to changing climatic conditions. We can however partially control for this confounding by measuring changes in (R/S) of lower Columbia River stocks, and assuming that these fish experience the same regional climate effects as Snake River chinook. Another problem with (R/S) as a monitoring variable is that it gradually decreases as the number of spawners increases, due to density dependence. Thus an action which significantly increased the number of spawners but changed nothing else (e.g., major decreases in the harvest rate of fall chinook) would likely also decrease (R/S). This creates some confusion in interpretation.

The residuals from graphs of ln(Recruits/Spawner) vs. S[7] provide a measure of relative recruitment success (RRS) and are probably the most useful indicator of benefits of an action over the course of the life cycle. Estimates of RRS can reduce the magnitude of the above problems. In any year, both upstream and downstream stocks will show variations from their “expected (R/S)”, that is, the (R/S) that would occur in an average year at that spawning level (S). Each stock will have a unique Ricker *a* and *b* value that determines that stock’s expected (R/S). In the absence of habitat actions that affect the “expected” ln(R/S) vs. S line, these variations could be due to changes in either system survival, λ_n, or climate. Looking at the differences between the residuals for Snake River stocks and those for Lower Columbia stocks (i.e., RRS_u – RRS_d) can remove the common climatic effects that affect both Snake River and downstream stocks. Looking at changes in this quantity before and after some management action gives an indication of the net benefit of the action for Snake River stocks, relative to the performance of Lower Columbia stocks. These contrasts are illustrated in Table 2.3-5, and described mathematically in Appendix D. The key monitoring indicator is shown in the lower right corner of Table 2.3-5, the *change in upstream-downstream differences over time*. An additional wrinkle is that one needs to control for any changes in the in-river survival of Lower Columbia River stocks (see Appendix D). This discussion applies mainly to the Delta life-cycle model, but an analogous procedure could be developed for the Alpha life-cycle model.

Upstream-downstream contrasts in SAR (Smolt to Adult Returns) data can be used in a similar manner, and are complementary to RRS in that they exclude the portion of the life cycle before the uppermost dam, or uppermost smolt counting location, as described in Appendix D. Both of these measures require monitoring of lower Columbia River stocks concurrent with Snake River stocks.

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[7] These residuals are a measure of overall survival from spawner to recruit, while factoring out effects of the number of spawners on survival (i.e., survival is generally lower at higher spawner numbers because of crowding and other density effects). Residuals of ln(R/S) vs. S are estimated by fitting a linear function to a graph of ln(R/S) vs. S, then comparing the observed ln(R/S) at a given number of spawners to the ln(R/S) that is predicted from the ln(R/S) vs. S function. The difference between the observed and expected ln(R/S) is the residual.

Conclusions:

Monitoring variables can be organized into three levels:

Level I. At the highest level are variables that measure overall survival rates of salmon. Relative recruitment success (RRS) and SARs are examples of these types of variables. Level I variables do not allow us to directly test extra mortality hypotheses, but they can be feasibly monitored and are useful for addressing more general hypotheses about the overall effects of actions on salmon survival.

Level II. Level II variables are those that directly address the transportation (D value) and extra mortality hypotheses. It appears to be feasible to monitor D, although there are disagreements about how precise these D estimates actually are. Because extra mortality hypotheses are expressed in terms of the post-Bonneville survival of non-transported fish (λ_n), this would be the ideal variable to monitor in an experimental management strategy. To this point we have only completed a preliminary, qualitative analysis of λ_n, but it appears from this qualitative analysis that it will be very difficult to obtain good estimates of the changes in λ_n resulting from experimental management actions. It is even more difficult to estimate λ_n when transportation is occurring because estimation of λ_n then requires an estimation of D. Further quantitative analyses are required to assess how feasible it is to detect changes in λ_n.

Without estimates of λ_n, it will be difficult to test the extra mortality hypotheses directly. This does not mean that we should abandon these hypotheses – they have provided a convenient framework for conducting the prospective simulations, and they have helped to design experimental management actions that address potential causes of extra mortality. In addition, the extra mortality hypotheses will be necessary for simulating the effects of alternative experimental actions to assess their relative risks and benefits.

Level III. Level III variables include in-river survival rates, fish condition, disease profiles, and other variables that are measured at finer temporal and spatial scales. These variables are generally relatively easy to monitor, and are useful for identifying mechanisms by which actions affect fish. However, they do not provide information on transportation or extra mortality, nor do they provide information for assessing the effects of actions on overall survival rates.

These three levels of variables are complementary in that all are needed to address both the conservation and the learning objectives of experimental management. For example, Level I variables are most useful for assessing the conservation objective, while Levels II and III are most useful for assessing the amount of learning possible from alternative actions. Therefore, in most cases we would like to monitor all three types of variables if possible. Some of these variables must also be monitored for lower Columbia River stocks to provide an indication of regional climatic changes affecting all stocks.

Table 2.3-5: Contrasts in RRS (residuals from graphs on ln(R/S) vs. S) to assess the net benefits of a management action.

	Time Period		
Stock Group	**Before Management Action**	**After Management Action**	**Net Change (After vs. Before Management Action)**
Snake River (u= upstream)	$\{RRS_u\}_b$	$\{RRS_u\}_a$	$\{RRS_u\}_a - \{RRS_u\}_b$ Change in upstream stock's RRS over time
Lower Columbia River (d = downstream)	$\{RRS_d\}_b$	$\{RRS_d\}_a$	$\{RRS_d\}_a - \{RRS_d\}_b$ Change in downstream stock's RRS over time
Difference	$\{RRS_u\}_b - \{RRS_d\}_b$ Upstream-downstream differences before management action	$\{RRS_u\}_a - \{RRS_d\}_a$ Upstream-downstream differences after management action	$[\{RRS_u\}_b - \{RRS_d\}_b] - [\{RRS_u\}_a - \{RRS_d\}_a]$ Change in upstream-downstream differences over time.

3.0 Detailed Descriptions of Experimental Actions / Research & Monitoring

Introduction

PATH has developed a list of candidate experimental actions that are designed to address specific hypotheses about extra mortality (Table 2.3-1). The purpose of this section of the report is to define some of these actions in enough detail to allow a thorough discussion with other regional groups and agencies. We emphasize that the list of candidate actions described in this report is intended only to stimulate discussion of experimental management within the region. The list is intended to be added to or modified as a result of further dialogue between PATH and the region. Some of the sections in this Chapter (i.e., 3.1, 3.9, 3.10) describe research and monitoring approaches to key uncertainties, rather than experimental management actions.

For each action discussed in Chapter 3 below, we consider the following topics and issues:

3.X.1 Description of Experimental Action / Research & Monitoring

Rationale

- State the explicit objectives of the experiment.
- Pose a clear testable hypothesis that can be transformed into a statistical hypothesis or an explicit, quantitative decision rule
- State the statistical hypothesis or decision rule.

Spatial and Temporal Components

- How long will the experiment run in time (1 year, 25 years)?
- What is the spatial resolution of concern (Snake River, Columbia River basin, region wide coast wide)?
- What are the experimental units? (stocks, cohorts, streams, years, regions)?
- How are treatments interspersed in space and time?

3.X.2 Monitoring Approach

Variables to Monitor

- refer to Section 2.3.2 for variables; specify for which stocks (e.g., Snake R., lower Columbia) these variables are to be monitored

Duration and Intensity of Monitoring

- Frequency of sampling (annual, seasonal, intra-annual)?

3.X.3 Benefits, Risks, Costs, and Trade-offs

Benefits and Amount of Learning Possible

- What are the benefits this action (e.g., how will results provide learning that improves management, how will these changes benefit the stocks)?
- Quantitative or qualitative assessment of likely power to detect effects.

Risks to Stocks

- for continued transportation, jeopardy probabilities for A1/A2
- for drawdown options involving breaching of all four Snake River dams, jeopardy probabilities for A3/B1
- drawdown options involving breaching of two dams only will require further simulations
- for fertilization, hatchery, bird management actions, estimation of jeopardy probabilities will require some creative thinking about how to implement these new hypotheses into modeling framework (e.g., could use Figure 1 from WOE Submission #1 showing extra mortality vs. hatchery releases)

Costs

- at least consider monitoring costs and rough estimate of implementation costs for novel actions
- placeholder for input of other analytical groups (e.g., Drawdown Regional Economic Workgroup) on relative costs (PATH won't be able to do this)

Trade-offs

- integrate results from previous three sections into assessment of relative benefits/risks/costs

3.X.4 Inferences

- for a particular management experiment and variable, tabulate the observations that will be consistent with the "null" and alternative hypotheses. For each variable, try to quantify the magnitude of change consistent with different conclusions.

3.X.5 Confounding Factors

- list possible confounding factors
- discuss how confounding is reduced by the elements of the specified experimental action (Section 3.X.1) and monitoring (Section 3.X.2)

3.X.6 Practical Constraints

- Legal: Is there legislation in place that would hinder or prevent the experimental action (e.g., environmental impacts under the National Environmental Protection Act, Endangered

Species Act)? What steps would have to be taken to circumvent or comply with this legislation?

- Economical: How much would it cost to do the experiment above ongoing monitoring actions? What are the implications of this action for the regional economy?
- Logistical: Is it feasible to implement the experiment within the stated time frame?

***************************** Candidate Actions **********************

3.1 Continue Current Hydropower Operations and Estimate D

3.1.1 Description of Experimental Action / Research & Monitoring

Rationale

Study Objective: To estimate differential post-Bonneville Dam survival of transported smolts versus those that migrate in the river under current operating conditions.

Description of Hypothesis: Several null hypotheses are possible. One is that post-Bonneville survival of transported fish is the same as those that migrate in the river. However, this null would be somewhat artificial, as there is consensus that post-Bonneville mortality of transported fish is at least slightly elevated. Based on previous PATH decision analysis results and recent analyses of PIT-tag data suggesting that recent and prospective D-values might be higher than previously estimated, the most useful form for the null hypothesis is one-sided:

$$H_0 : D \leq D_0 \qquad \text{[Eq. 3.1-1]}$$

where D_0 is the hypothesized value of D. Thus, rejection of this hypothesis constitutes evidence that the true value of D is greater than D_0. Several values of D_0 are worth considering. For purposes of illustrating required sample sizes, we have used hypothesized values of 0.35 and 0.65.

The results of the decision analyses are sensitive to the value of *D* (see Section 2.4). Methods of transportation have improved, as has survival of downstream migrants (and the means to estimate it), so that estimates of *D* derived from earlier studies, particularly pre-1980, do not apply to the present or in the future. Direct losses in the hydropower system in the 1970s and early 1980s clearly impacted stocks; however, current estimates of downstream migrant survival are similar to survival of fish through the hydropower system prior to construction of John Day, Lower Monumental, Little Goose, and Lower Granite Dams. Transport studies conducted under a range of in-river and ocean conditions would reduce uncertainty about the efficacy of transportation

Experimental Action: Continue transport evaluation studies in the Snake River using PIT tags for both yearling chinook salmon and steelhead. Conditions for in-river migrants would be optimized by maximizing spill at downstream projects during the migration.

Spatial and Temporal Components

Juvenile fish marking to evaluate transportation and downstream migrant survival should occur for another five years (2000 through 2004). Complete adult evaluation will occur by 2007. The work should encompass PIT-tagged fish from all major tributaries of the entire Columbia River Basin above

Bonneville Dam. The experimental units will include some combination of streams, cohorts, and stocks, evaluated within and between years.

3.1.2 Monitoring Approach

Variables to Monitor

For each year of the study, use NMFS methods to estimate D from PIT-tagged smolts. The method has several steps, including estimation of survival through reaches of the lower Snake River and lower Columbia River, estimation of the number of PIT-tagged fish that experienced each possible passage history during juvenile migration, construction of experimental "treatment" and "control" groups of PIT-tagged fish representative of the run at large, and calculation of Bonneville (smolt)-to-Lower Granite (adult) return rates for the two groups. Methods have been described elsewhere.

Additionally, PIT tags provide timing information for all fish. Relationships among travel time, survival, passage history, and environmental conditions (including flow, water temperature, and levels of spill) would also be evaluated. Reach survival would be estimated using the PIT-tag interrogation system now in place in the Snake and Columbia Rivers (with the planned upgrade to the new PIT-tag frequency), the PIT-tag towed array in the Columbia River estuary, and recoveries of tags from bird colonies below Bonneville Dam. If tag detections below Bonneville Dam are sufficient, survival to Bonneville Dam will be estimated directly. Otherwise, survival estimates to Bonneville Dam will be extrapolated from upstream reach estimates.

Sample Sizes Required

Required sample sizes would depend on the desired power of the test, the significance level of the test, the desired minimum detectable difference between the hypothesized and true values of D, and the overall smolt-to-adult return rate.

Using NMFS' methods, variance of estimated "D" is estimated using bootstrap methods, but is approximated by variance of the ratio of probability estimates (Bonneville-to Lower Granite SARs for treatment and control groups) from two independent binomial distributions. The equation for variance of the ratio of two SARs is (Burnham et al. 1987):

$$Var\left(\frac{\hat{p}_1}{\hat{p}_2}\right)=\left(\frac{1}{n_1}-\frac{1}{N_1}+\frac{1}{n_2}-\frac{1}{N_2}\right)\left(\frac{\hat{p}_1}{\hat{p}_2}\right)^2 \qquad \text{[Eq. 3.1-2]}$$

where N_i is the number of juveniles, n_i is the number of returning adults, and p_i is the return rate in group i. Analyses are typically done on the log-transformed scale. The variance of the log of the ratios is:

$$Var\left(\ln\frac{\hat{p}_1}{\hat{p}_2}\right)=\left(\frac{1}{n_1}-\frac{1}{N_1}+\frac{1}{n_2}-\frac{1}{N_2}\right) \qquad \text{[Eq. 3.1-3]}$$

Because return rates are quite low (i.e. $N_i >> n_i$), this expression is dominated by the inverse of counts of returning adults in the two groups. If we plan the study so that the expected number of adults in each group is equal ($n_i = n$), then the right side of the equation reduces to approximately $2/n$ (see Section 3.2.2). For example, if we hypothesize $D_0 = 0.35$, and we wish to have 80% power to detect a difference if the true D is 0.65 or greater with a 0.05-significance level test, then the number of adults needed in each group is (Steel and Torrie 1980):

$$n = \frac{2 \bullet \left(z_{.80} + z_{.95}\right)^2}{\left(\ln(0.65) - \ln(0.35)\right)^2} = 33.$$

[Eq. 3.1-4]

The number of PIT-tagged fish in the system released at or above Lower Granite Dam required to ensure sufficient juveniles in each group for various hypothesized and true D values and expected return rates are given in Table 3.1-1.

Table 3.1-1: Number of PIT-tagged fish in the system released at or above Lower Granite Dam required to ensure sufficient adult returns in each group, assuming 50% survival from head of Lower Granite Reservoir to Bonneville Dam tailrace for control fish. Significance level is $\alpha = 0.05$ and power is $(1-\beta) = 0.80$.

Null Hypothesis	**True D value**	**Expected LGR-to-LGR SAR for Transported Group**					
		0.25	**0.50**	**0.75**	**1.00**	**1.50**	**2.00**
$D_0 \leq 0.35$	0.40	T: 277,600 C: 222,080	T: 138,800 C: 111,040	T:92,534 C:74,027	T:69,400 C:55,520	T:46,267 C:37,014	T:34,700 C:27,760
	0.50	T:39,200 C:39,200	T:19,600 C:19,600	T:13,067 C:13,067	T:9,800 C:9,800	T:6,534 C:6,534	T:4,900 C:4,900
	0.60	T:17,200 C:20,640	T: 8,600 C:10,320	T: 5,734 C: 6,880	T: 4,300 C:5,160	T: 2,867 C: 3,440	T: 2,150 C:2,580
	0.70	T:10,400 C:14,560	T: 5,200 C: 7,280	T: 3,467 C: 4,854	T: 2,600 C: 3,640	T: 1,734 C: 2,427	T: 1,300 C: 1,820
	0.80	T: 7,600 C:12,160	T: 3,800 C: 6,080	T: 2,534 C: 4,054	T: 1,900 C: 3,040	T: 1,267 C: 2,027	T: 950 C: 1,520
	0.90	T: 5,600 C:10,080	T: 2,800 C: 5,040	T: 1,867 C: 3,360	T: 1,400 C: 2,520	T: 934 C: 1,680	T: 700 C: 1,260
	1.00	T: 4,800 C: 9,600	T: 2,400 C: 4,800	T: 1,600 C: 3,200	T: 1,200 C: 2,400	T: 800 C: 1,600	T: 600 C: 1,200
$D_0 \leq 0.65$	0.70	T: 900,800 C: 1,261,120	T: 450,400 C: 630,560	T: 300,267 C: 420,374	T: 225,200 C: 315,280	T: 150,134 C: 210,187	T: 112,600 C: 157,640
	0.80	T: 114,800 C: 183,680	T:57,400 C:91,840	T:38,267 C:61,267	T:28,700 C:45,920	T:19,134 C:30,614	T:14,350 C:22,960
	0.90	T:46,800 C:84,240	T:23,400 C:42,120	T:15,600 C:28,080	T:11,700 C:21,060	T: 7,800 C:14,040	T: 5,850 C:10,530
	1.00	T:26,800 C:53,600	T:13,400 C:26,800	T: 8,934 C:17,867	T: 6,700 C:13,400	T: 4,467 C: 8,934	T: 3,350 C: 6,700

The number of fish shown in the table above for the control group (C) would vary depending on the type of control group desired. If all fish in the control group were required to be those never detected anywhere, then sample size would have to be increased substantially to ensure enough fish in that category. Alternatively, the downstream dams or their operations could be modified by being placed in primary bypass mode or having all guidance screens removed to increase the number of fish in the never detected category.

Duration and Intensity of Monitoring

Results based on complete returns from outmigrations. Monitor all adult returns at dams. By 2001, adult monitors likely at all upstream passage points at Bonneville Dam.

3.1.3 Benefits, Risks, Costs, and Trade-offs

Benefits and Amount of Learning Possible

Uncertainty exists about the benefit of transportation to different stocks of fish as it is presently implemented. Current models rely heavily on transportation results derived from conditions that no longer exist. This continual monitoring will provide the needed information. Another five years of marking fish along with data that will accrue from present marking programs should provide sufficient information to determine the benefits of transportation across the migration season and annually between seasons.

Risks to Stocks

If transportation and/or the hydropower system have large impacts on fish, continual operation of the hydropower system and transportation will increase the risks that stocks will not recover. Direct risks to stocks would be minimal since recent studies have shown a benefit from transportation from Lower Granite Dam. Furthermore, by maximizing spill for in-river migrants, not all fish would be transported which would spread the risk between in-river migration and transportation as called for in the current Biological Opinion.

Costs

Transportation studies and the cost to mark stocks from all of the river basins to compare with upper river stocks will likely cost $1.0 M + annually.

Trade-offs

There are few alternative to present hydropower system operations, other than dam removal. Recent sensitivity analyses showed that results of the decision analysis are quite sensitive to the value of D, while analyses of PIT-tag data suggested that recent and prospective D-values might be higher than previously estimated. Dam removal is a very costly alternative if it turns out that transportation does not currently impact stocks to the degree indicated by these previous estimates.

3.1.4 Inferences

If transported fish return at rates greater than the segment of fish that pass through the hydropower system under the best possible passage conditions and there are little or no delayed affects of transportation, it will indicate that transportation benefits fish. The degree that benefits are higher will affect the D value used in models. However, transportation results used to date are modeled on a basis of seasonal benefits. Data collected in recent years indicates potentially large differences in benefits of transportation within a season. This is most likely due to differential timing of ocean entry for groups of fish both transported and left in-river. If transportation within season varies tremendously, then additional studies may provide some evidence on the mechanisms that cause this differential survival. This may also provide insight into expected benefits from alternative hydropower configurations such as additional flow augmentation.

Even if D turns out to be higher than used in previous model projections, the key issue remains as to what drives the extra mortality experienced by both transported and in-river fish. As shown in Section 2.4 of this report, extra mortality hypotheses have a big effect on the relative performance of A2 and A3, especially at relatively high D values.

3.1.5 Confounding Factors

Measurements of transportation benefits are possibly confounded by numbers of hatchery fish in the system and conditions smolts face when entering the estuary and ocean.

3.1.6 Practical Constraints

There are no practical constraints to the monitoring. To do a complete job, however, will require PIT-tagging fish in downriver tributaries.

3.2 Modify Transportation and Estimate D

3.2.1 Description of Experimental Action / Research & Monitoring

Rationale

Study Objective: To determine the efficacy of modified smolt transportation methods.

Description of Hypothesis: During the past couple of years, PATH participants have discussed various changes in methods of transportation. Some of these have been proposed on the basis that they could improve the survival of transported fish, such as:

- changes in the timing of delivery of smolts to the estuary;
- barging (rather than trucking) fall chinook;
- discontinuing collection of smolts at LMO and McN, because fish transported from these locations have had poorer survival;
- removing screens from some projects, because multiply-detected fish have poorer survival (H1: screens damage fish and reduce survival; H2: screens select for diseased fish);
- transporting different groups of fish in different barges (e.g., no hatchery steelhead mixed in with wild fish), and comparing SARs from this group with regular methods of transportation to assess effects of species interactions.

Other changes to transportation have been proposed as a method to evaluate *D*:

- stop transportation of fall chinook in some years, continue it in others; and
- stop transportation of both spring/summer and fall chinook in some years, and continue it in others

Experimental Actions: Evaluating the proposed transportation modification alternatives would require varying levels of effort. The first alternative (estuarine timing) is already being evaluated to a limited degree by looking at results of ongoing transportation evaluations on a temporal basis (SARs or T:C ratios by date of tagging and estimated date of estuary arrival). The results could be enhanced by examining relationships between transported and nontransported fish survival and ongoing studies in the Columbia River plume looking at salmonid predator/prey interactions and nearshore biotic and abiotic conditions that smolts face during ocean entry.

The second alternative (barging rather than trucking fall chinook) could be evaluated using supplemented subyearling fall chinook salmon from Lyons Ferry Hatchery. In 1999, about 600,000 hatchery subyearlings were released above Lower Granite Dam and releases of this size are expected for the next several years. Fish could be collected and PIT-tagged at Lower Granite Dam or tagged at Lyons Ferry Hatchery and trucked upstream, acclimated, and released for eventual recapture and sorting into transport and in-river groups at Lower Granite Dam. In-river survival could be estimated from the control group. This study would require a minimum of at least three years of tagging. Required sample sizes are discussed in the next section.

The third alternative (discontinue transport from Lower Monumental and McNary Dams) could be evaluated by transporting PIT-tagged smolts or allowing them to migrate in-river during alternate weeks and comparing adult returns to the dam of interest. However, the data demonstrating poorer survival for fish transported from these sites is limited.

The fourth alternative (removing screens to optimize in-river survival) is not directly transport-related. Identifying problem bypass systems, or areas within bypass systems causing mortality might lessen any bypass-related mortality (immediate and delayed). Ongoing studies are addressing this issue.

The fifth alternative (different barges for different stocks) could be accomplished in two ways. The species of interest (wild chinook) could be sorted from other species and stocks at Lower Granite Dam, barged separately, and their SAR compared to the same stock not barged separately in alternate weeks or years. This could be accomplished using the PIT tag sort-by-code system now in place at Lower Granite Dam or by building a new efficient fish separator at this facility. The stock of interest would still have interaction with other species and stocks during river and reservoir migration and collection at Lower Granite Dam. A better study design would be to delay release of hatchery stocks above Lower Granite Dam (some supplementation releases would be acceptable) until early or mid-May. Fish collected prior to arrival of the hatchery stocks would avoid interaction for a good portion of the migration season. Hatcheries could accommodate later release dates if extra feed were provided, especially in years when fewer fish were being reared. This study design would require alternative treatment years since release date would influence SARs.

The last two alternatives (halting transportation for some years) may provide little additional information since we are already doing this on an experimental basis with PIT-tagged fish left in-river. However, as there are some limitations to the precision of PIT-tag estimated survival rates, we also examine a transport — no transport adaptive management experiment (see Section 3.2B).

Spatial and Temporal Components

Juvenile fish marking to evaluate the number of possibilities will take a number of years. Any good research design will minimally require three replicate years for each test condition. Thus, it will take six years of juvenile fish releases to provide three years each of two different conditions. It will take three years from the last juvenile fish release to get complete adult returns. It is difficult to decide at this time how many different treatments one could test within a migration year. Under the most optimal set of circumstances, it will likely take six years of juvenile releases to provide evaluation of treatments. The work should encompass PIT-tagged fish from the Snake River, but depending on transportation conditions, it should include fish marked in the upper Columbia River as well. Depending on analyses, marking of comparison fish from tributaries below McNary Dam might prove valuable. The experimental units will include some combination of streams, cohorts, and stocks, evaluated within and between years.

3.2.2 Monitoring Approach

Variables to Monitor

For each experimental group, measure downstream migrant survival, physical and physiological conditions of the fish, SAR, and T:C ratios (where available). PIT tags provide timing information for all fish.

Sample Sizes Required

Sample sizes for a transport study using transport smolt-to-adult ratios (SARs) relative to in-river SARs (T/I) are determined to assure the desired probability (1-β) that the confidence interval around the estimated T/I (significance level α) will not contain the value 1, or that the confidence interval on the estimated log-transformed T/I, ln(T/I), will not contain 0. (This is equivalent to the sample size required to assure power of (1-β) for an α-level test of the null hypothesis that SARs for the two groups are equal; i.e., $H_0 : T/I = 1.0$).

Therefore, for a desired α and β and specified minimum detectable difference between the true T/I and 1.0, the number of fish needed can be determined from the following equation:

$$E\left[\ln(\frac{\hat{T}}{I}) - (z_{\alpha/2} + z_{\beta}) \bullet se(\ln(\frac{\hat{T}}{I}))\right] \geq 0. \qquad \text{[Eq. 3.2-1]}$$

where $E\left[\ln(\frac{\hat{T}}{I})\right]$ = ln(T/I), and $se(\ln(\frac{\hat{T}}{I})) \approx \sqrt{(1/n_T + 1/n_I)} = \sqrt{2/n}$ if the number of adult returns per treatment is set equal for simplicity (see Section 3.1.2).

Solving the equation for *n*:

$$n \geq \frac{2 \bullet (z_{\alpha/2} + z_{\beta})^2}{(\ln(T/I))^2} \qquad \text{[Eq. 3.2-2]}$$

Of course, the number of juveniles required for the two release groups in the transport/in-river study depends on both the number of adult returns required and the expected SAR of the two groups.

Assuming a = 0.05 and b = 0.20 and a conservative transport SAR of 1% for subyearling fall chinook salmon, the sample sizes needed for various true values of T/I are given in Table 3.2-1 (N denotes the number of juveniles).

For reference, see Table 3.1-1 for indication of the power to test hypotheses regarding D using samples of the same size as in 3.1-2.

Given the high SARs for wild fall subyearling chinook salmon in recent years (>2%) and the poor in-river survival, expected 1.0% SAR for hatchery subyearlings and expected true T/I ratio of 1.4 or greater seem reasonable. Releases of PIT-tagged Lyons Ferry Hatchery chinook salmon above Lower Granite Dam in 1995, at less than optimum size and time of release, have an SAR of about 0.5% to date with 4- and 5-year olds yet to return. For an expected SAR of 0.5%, the numbers in Table 3.2-1 would be multiplied by 2 and for an SAR of 0.25%, multiplied by 4.

Table 3.2-1: Size of release groups needed at Lower Granite Dam for a transport/in-river study of subyearling fall chinook salmon, assuming 1% LGR-to-LGR SAR for transported fish, significance level α=0.05 and power (1-β)=0.80.

True T/I	Number of adults needed (n)	Number of transported juveniles (N_T)	Number of in-river juveniles (N_I) (=N_T*T/I)	Total number of juveniles
1.1	1,728	172,800	190,080	362,880
1.2	473	47,400	56,760	104,160
1.3	228	22,800	29,640	52,440
1.4	139	13,900	19,460	33,360
1.5	96	9,600	14,400	24,000

Releasing tagged Lyons Ferry Hatchery subyearling fall chinook salmon above Lower Granite Dam and using the sort-by-code system to create the release groups would require increasing the number of fish tagged over that shown above to provide sufficient numbers for each group at Lower Granite Dam (assuming 60% survival to Lower Granite Dam and 50% FGE). The transport number released would be multiplied by 3.33 (1.0/(0.6*0.5)) and the in-river number by 1.67 (1.0/0.6). Therefore, with an SAR of 1.0% and a T/I of 1.4, a total of 78,785 fall chinook salmon would need to be PIT-tagged and released above Lower Granite Dam (46,287 transports and 32,498 controls). Different survival and detection estimates would require different multipliers calculated in the same manor.

Table 3.2-2 is provided as a general guide to the required numbers of juvenile fish in transport and in-river groups at Lower Granite Dam to estimate D with various levels of precision. The table assumes 1.0% LGR-to-LGR SAR for inriver fish. For an expected SAR of 0.5%, sample sizes in Table 3.2-2 would be multiplied by 2 and for an SAR of 0.25%, multiplied by 4.

Table 3.2-2: Number of PIT-tagged juvenile fish required in each group at Lower Granite Dam (T=transport; C=control) to obtain given precision (80% probability that the half-width of 95% confidence interval will be equal to or less than ε) of "D" estimate assuming 1.00% LGR-to-LGR SAR for in-river control group and 50% survival from head of Lower Granite Reservoir to Bonneville Dam tailrace.

True D value	Precision				
	0.02	**0.04**	**0.06**	**0.08**	**0.10**
0.20	T: 395,663 C: 155,100	T: 98,913 C: 38,774	T: 43,962 C: 17,233	T: 24,730 C: 9,694	T: 15,827 C: 6,204
0.40	T:789,668 C: 619,100	T: 197,418 C: 154,776	T: 87,742 C: 68,790	T: 49,356 C: 38,695	T: 31,588 C: 24,765
0.60	T: 1,182,143 C: 1,390,200	T: 295,536 C: 347,550	T: 131,349 C: 154,467	T: 73,884 C: 86,888	T: 47,286 C: 55,608
0.80	T: 1,573,023 C: 2,466,500	T: 393,254 C: 616,622	T: 174,778 C: 274,052	T: 98,312 C: 154,153	T: 62,920 C: 98,658
1.00	T: 1,962,245 C: 3,846,000	T: 490,561 C: 961,500	T: 218,027 C: 427,333	T: 122,640 C: 240,374	T: 78,489 C: 153,839

Duration and Intensity of Monitoring

Results will be based on complete returns from outmigrations. Monitor all adult returns at dams. By 2001, adult monitors will likely be installed at all upstream passage points at Bonneville Dam.

3.2.3 Benefits, Risks, Costs, and Trade-offs

Benefits and Amount of Learning Possible

Isolating the effects of ocean entry timing, interaction with other stocks during collection and transport, the method of transport (barge or truck) will reduce the uncertainty about *D* and extra mortality for both transported and non transported fish.

Risks to Stocks

Efforts to improve survival of transported fish, using only experimental PIT-tagged fish, would most likely not increase risk to the general population.

Costs

Costs for evaluating the proposed transportation alternatives would vary, but all would be inexpensive when compared to dam breaching.

Trade-offs

There are few alternative to present hydropower system operations, other than dam removal. Recent sensitivity analyses showed that results of the decision analysis are quite sensitive to the value of D, while analyses of PIT-tag data suggested that recent and prospective D-values might be higher than previously estimated. Dam removal is a very costly alternative if it turns out that transportation does not currently impact stocks to the degree indicated by these previous estimates.

3.2.4 Inferences

Transportation results used to date are modeled on a basis of seasonal benefits for combined stocks. Data collected in recent years indicates potentially large differences in benefits of transportation within a season. Evaluating transportation on a finer scale may provide some evidence on the mechanisms that cause differential survival. This may also provide insight into expected benefits from alternative transportation methods.

3.2.5 Confounding Factors

Measurements of transportation benefits using alternate techniques are possibly confounded by numbers of hatchery fish in the system and conditions smolts face when entering the estuary and ocean.

3.2.6 Practical Constraints

There are no overwhelming practical constraints to the evaluation of proposed modified smolt transportation methods.

3.3 Transport/No Transport Adaptive Management Experiment

NOTE:
Because we could not locate a precise description of the mechanism by which this experiment addresses the hypothesis that never-detected fish are thought to do better than fish detected many times because of the secondary dewatering needed for detection (at least at transport projects) and subsequent transportation. Other mechanisms may also be possible. While the methods described below would work for other mechanisms (e.g., bypass itself, primary dewatering, or lack of imprinting and stress while in the barge) they are to some degree specific to the mechanism just noted.

3.3.1 Description of Experimental Action / Research & Monitoring

Background:

Most unmarked (e.g., non PIT-tagged) Snake River chinook and steelhead smolts bypassed at collection projects are transported. In contrast, most marked fish bypassed at collector projects are returned to the river below the project outfall, to provide additional information for in-river survival studies. Therefore, for calculating SARs, TCR's, and other smolt-to-adult survival statistics, so-called "non-detected" PIT-tagged smolts –those never seen at collector projects-- are often thought to be the best surrogates for the (unmarked) run-at-large for all Snake River ESU's.

Methodologically, this approach is not without potential problems (statistics are developed in Sandford and Smith, 1999, *in review*). First, one must calculate the survival of non-detected fish from Lower Granite Dam (LGR) to each collection project. These calculations are very complex, in contrast to straight-forward Cormack-Jolly-Seber (CJS) survival estimates. In addition, since adult returns are sparse (due to low SAR's and small numbers released in each of many narrowly defined groups), the information content of the estimates of SAR's, TCR's, etc. is rather low. Because non-detected fish sometimes have higher SAR's than fish detected several times, there is the possibility that detection (i.e., bypass, dewatering, and detection in the lower reaches of each bypass system) may influence SARs. Under the hypotheses tested in this section, this long-term effect is assumed to result from secondary dewatering, rather than bypass proper. Finally, because relatively few wild smolts are tagged, and because few wild adults are scanned for PIT tags on the spawning grounds, calculating wild SARs to the spawning ground yields little useful information.

One mechanism by which conventional, PIT-tagged controls in T/C experiments may overstate the benefits of transportation is as follows. The SAR's for non-detected fish are sometimes somewhat higher for fish that are never detected in the bypass systems than for fish that are detected at several projects. This could be due to many factors, including imperfect detection systems. If the difference is real, it is attributed to the long-term effects of the PIT tag dewatering system. The mechanism is related to the treatment that the fish receives once it is in the bypass system. After a smolt is guided by the screens into the bypass system, a "primary" dewatering occurs. If no detection system is in place, the fish are then returned to the river at or below the project tailrace. Because the bypassed fish are in too large a pipe for accurate detection, fish at most projects with both detectors and transport facilities receive a secondary dewatering, and are shunted into a smaller pipe. There, they pass a series of magnetic coils designed to detect the tags. Fish that are to be transported are dewatered further before being placed in a raceway or directly into a barge. The secondary dewatering is thought to have deleterious long-term effects on the fish. Short-term effects (as measured by inriver survival through subsequent projects) are not apparent in studies done to date.

In this section, we develop rationale and an initial design for a large-scale adaptive management experiment to address these issues. If the design is both relevant (measures the intended effects) and sufficiently powerful, it will enable managers to obtain information on the smolt-to-adult survival of transported and non-transported fish without the complications noted above. We may also answer the question of whether or not transport, if fully applied to all Snake River stocks, could help those stocks to recover within a given time frame.

Rationale

- The objective is to determine if the SAR's, TCR's, and "D" values estimated in transport experiments are representative of the wild spring/summer and fall chinook runs at large. The method for doing so is to alternate different means for "treating" the population: in some years, most fish would be bypassed, dewatered, and transported, while in others nearly all fish would be bypassed but not dewatered or transported.
- The hypothesis to be tested is that measures of spawner-to-recruit survival are proportional to TCR's estimated from transport experiments.

3.3.2 Monitoring Approach

The essence of this adaptive management experiment is to extend conventional, PIT-tag based experiments to include "true" controls, that would be more nearly representative of fish migrating in-river with little or no indirect influence of transportation. That is, in years with transport turned off (except for simultaneous transport experiments – see below), fish migrating inriver would by bypassed and primary-dewatered, but not secondary-dewatered. If secondary dewatering reduces subsequent survival, these "control" years should capture and measure the effect, whether it is evident from inriver survival downstream of transport projects, or in SAR's.

In addition, because the experiment would alternate years when most fish are transported with years when (almost) none would be loaded into barges, it should be possible to observe a strong contrast in measures of survival that cannot be detected using PIT-tagged fish. In particular, we can monitor spawner-to-recruit survival across the two sharply different experimental conditions – transport vs. no transport – to see if these differences in survival comport with TCR's and other survival measures from conventional experiments.

Broadly speaking, there would be a "grand" adaptive management experiment – transport vs. no transport – with smaller, conventional transport experiments and inriver survival monitoring nested within the larger study (see Table 3.3-1). The grand experiment would run in alternate years (See Hinrichsen appendix on why one should use even/odd years for this type of experiment). In years when transportation is in operation (call them T, T+2, T+4, …) voluntary spill would be eliminated, all fish bypassed at transport projects would be dewatered and transported, while PIT tagging and detection would continue much like the present, though at higher intensity. In no-transport years (T+1, T+3, …) projects would spill to gas caps, and detection systems would operate only on primary-dewatered fish (see above). Flat-plate detectors would be used to detect fish near the outfalls for these systems. Detection efficiency would likely be lower than at present (Bill Muir, personal communication, Sept. 14, 1999; Jim Ceballos, personal communication, Sept. 15, 1999); indeed, detection efficiency *could* be zero. Alternatively, one might route a portion (perhaps 10%) of the smolts through the secondary dewatering/detection apparatus. Collection/transport/tagging efforts at Lower Granite and other mainstem projects would operate only as needed to conduct transport experiments. Detection at John Day, The Dalles, and Bonneville would continue as at present in all years.

Monitoring would focus on many of the same variables as at present, including SAR's, TCR's, inriver survival (Vn), etc. (See Table 3.3-2). One crucial addition would be that spawner-recruit survival could then be divided into years when fish are transported and years when most fish (transport study fish are the only exception) are transported.

To accurately assess the power of transport/no transport experiments, estimates of some variables may need to be improved. Note that the list is not exhaustive.

1) Recruits. For spring/summer chinook, these are recruits to the Columbia. For fall chinook, this will include ocean harvest. For retrospective work, we did not considered variance in the sub-model that is needed for these, except (for spring/summer chinook) potential errors in spawner estimates. We believe that we should include the recruitment variation, as best we are able, for prospective work. Details on harvest rate calculations, turnoff to tributaries, dam counts (including fallback rate estimates), and background data used to derive them will be required. If more PIT-tagged adults are detected at Bonneville in future, this information could substitute for the laboriously derived conversion rates used retrospectively.
2) Spawners. As with recruits, we believe we will need a more explicit treatment of variation in the estimates for prospective work. As part of this, we will need details on how dam counts (for fall chinook) and redd counts (for spring/summer chinook) are used to calculate spawning abundance. This includes redd-count expansion factors and their derivation, hatchery contributions to natural spawning, aging data and expansions to run-year spawner estimates, pre-spawning mortality, their respective data sources, and assessments of the accuracy of the expansions/extrapolations.
3) Passage parameters (P, Vn, and D). We obviously assume that these will be calculated from PIT tag release/detection information. Analytical or boot-strapped variance estimates can and should be calculated for all these quantities. Explicit assumptions relating PIT tag population estimates to individual stocks will also be required. Finally, for the Delta model analog of Eqn. 3.3.1, estimates of Vn and perhaps SARs will be needed for downstream spring chinook stocks.

Example Analysis: Precision of In-river Survival Estimates [Vn]:

Vn and P(bt) estimates are needed to assess changes in extra mortality, in addition to being of interest in their own right. P(bt) is obviously not a problem in years when only experimental fish are transported: it will probably be close to zero. Vn estimates depend on detections of tagged smolts at dams, and these detections would decrease dramatically in years with reduced secondary dewatering. We assume that the number of fish tagged is roughly equal in years with transport operating and years when only experimental fish are transported. Furthermore, we assume that at each transport project, the same number of fish will be tagged and released for transport experiments regardless of what occurs for the run at large. This is purely for convenience in subsequent power analyses. It may turn out to be a sub-optimal strategy, in the sense that one could gain more information by varying the tagging protocol between transport/no transport years.

In years T, T+2, etc. when transportation is operating, estimates of inriver survival would proceed in much the same way as at present. Most PIT-tagged fish would not be transported, but used to estimate inriver survival. They would therefore be returned to the river at each project. The exception would be fish selected for transport experiments. By assumption, the transport experiments would occur at each project where fish are transported, with roughly half of the fish being tagged (if they aren't already) and placed into barges, and remaining half returned to the river at outfalls below the project.

In years T+1, T+3, etc. estimates of inriver survival will be less precise, since detection efficiency will surely decrease. This will occur because detecting fish in larger primary bypass outlets will be less efficient than in secondary-bypass outlets, if it is even possible. Finally, since spill is increased in no-transport years, a higher proportion of the smolts will be directed away from the bypass and detection facilities. Note that the tagging protocol at transport projects would also need to be changed: since a smaller portion of the run will go into secondary bypass/detection/collection systems, obtaining a fixed number of fish for a transport experiment may take longer than under current operations.

To see how this might affect the variance in in-river survival estimates, we conducted a very simple "experiment" using chinook (from PITAGIS) that were released above (not at) Lower Granite in 1998, with all fish having the tagger-assigned migration year of 1998. The experiment was to randomly reduce detections at Lower Granite, Little Goose, Lower Monumental, and McNary (i.e., all transport projects) by 90%, and see how this affected estimates of the standard errors for in-river survival estimates.

In doing so, we used a very simple approach: spring, summer, and fall chinook of hatchery origin comprised three release groups, while wild fish comprised three additional groups. That is, we did not try to separate fish by day, week, or other sub-seasonal release or detection time. Note that we do not recommend this as a strategy to estimate seasonal average survivals; rather, it is a way of developing a simple example. If, with reduced detections, one could form the approximately the same number of daily or weekly groups as at present, the method we used will overstate the increase in variance associated with a reduction in detections.

Data used for the example are shown in Table 3.3.3. Releases included only fish of known run and origin, from release sites well above LGR. As can be seen, the numbers of releases vary by almost three orders of magnitude, from 162K for hatchery spring chinook to 556 for wild fall chinook. Detections follow a similar pattern. We did not include "detections" at Rice Island, as these tend to be very modest numbers for chinook.

Table 3.3.4 shows CJS estimated survival and detection rates, both for "All" detections and the 90% reduction, as described above. As one would expect, point estimates of survival generally don't change by much – where they do, we believe it's simply due to the randomness in detection rates. We only performed each simulated reduction in detections once; had we repeated it more often, survival rates would have converged to the means for the "All" case. A 90% reduction on detection rates usually increases the standard error (S.E.) of the survival rates by a factor of 10. For example, for hatchery fall chinook, survival from release to LGR was about 0.64. When all detections are used, the S.E. is 0.003. When 90% of detections are ignored, the S.E. increases to 0.027.

While the results for hatchery fish suggest that the precision of survival estimates would still be fairly tight for reaches with good estimates at present, for wild fish the results are more discouraging. For example, for wild spring chinook survival from LGR to LGS, estimated survival using all detections is 0.946, with a S.E. of 0.019. If the detection rate is reduced by 90%, the point estimate changes to 1.121, with a S.E. of 0.227. However, the example likely overstates the problem, at least for spring/summer chinook. Aside from the over-simplified nature of this "seasonal" estimate already noted, spring and summer chinook are usually combined (see last section of Table 3.3.4). In addition, many wild fish are tagged in their subbasin of origin the previous year; a back-of-the envelope estimate from Section 3.5 suggests that about 20K fish were tagged in 1997, and presumably migrated in 1998. If 25% of these fish survived to LGR, it would add at least 5K fish to the available sample. Finally, PIT-tagged wild fish may well be needed for other studies. Assuming that sufficient wild parr can be found, it seems reasonable to assume that the sample sizes used in this example are most likely under-estimates.

In summary, it would appear from this example that detection rates could be reduced substantially without greatly reducing the precision of in-river survival estimates.

Problems Using Historical R/S Models for Power Analyses

If one wishes to be able to estimate the power of tests for changes in R/S between transport and no-transport years, it seems logical to use a model of historic R/S to see how much unexplained noise is in that data. The retrospective model could then be used to assess the power of various experimental designs. For this example, we used the Alpha model, for three reasons:

1) It is simpler than the Delta model, with no year effects;
2) It makes no assumptions about common ocean survival for the upstream and downstream stocks; and
3) It can be translated more easily into a model that could be used for fall chinook.

We used a simple (for PATH) version for spring/summer chinook:

$$\mathrm{Ln}(R) = \mathrm{Ln}(S) - M - \ln(D*P + 1 - P) - b_0 * S + b_1 * \mathrm{Step} + b_2 * 1/F + b_3 * P/F + \mathrm{epsilon} \qquad 3.3.1$$

Where

Ln(R) = Natural log of recruits;
Ln(S) = Natural log of spawners;
M = Total Passage mortality (from summer 1997 versions of CRISP and FLUSH);
D = post-Bonn. survival of transported fish relative to in-river fish;
P = Proportion of fish below Bonneville that were transported there;
S = Spawners;
Step = 0 through brood year 1974, 1 thereafter;
F = Flow at Astoria, in year of downstream migration;
P = Poppa Drift, 1st winter of ocean life;
epsilon is a normally distributed mixed process and measurement error; and
b_0 – b_3 are estimated parameters.

Note that in this formulation, Ln(S), -M, and ln(D*P + 1 – P) are "offsets," with parameters equal to minus one, by definition. As such, they do not "count" in terms of the number of estimated parameters in the model.

As formulated above, the passage models' estimates are assumed to account for both direct (i.e., in-river survival) and indirect (delayed transport) effects of passage through the hydrosystem. We were concerned because retrospective estimates of these terms are derived from passage models. Since prospective estimates will be derived from PIT tag data, we might, in effect, be accounting for the same phenomena (in-river survival, P[bt], and "D") using two different methods. This in turn could affect the power analysis in ways that are difficult or impossible to account for.

However, in the process of developing the retrospective R/S analysis, we have simply assumed that passage terms are important in explaining recruitment. PATH has never systematically tested this assumption. More explicitly, we have never compared the performance of models like 3.3.1 to a slightly simpler model:

Ln(R) = Ln(S) - b_0 * S + b_1 * Step + b_2 * 1/F + b_3 * P/F + epsilon 3.3.2

With all terms as defined above. Before worrying about how to reconcile retrospective and prospective estimates of passage survival, we decided to compare the two. The results are shown in Table 3.3.5.

As can be seen from the table, adding the passage offsets (Eqn. 3.3.1) results in only slightly better Sum-of-Squares (SSE's) and R-squares than the simpler model with no offsets (Eqn. 3.3.2).[8] Given the ≈ 40% of variation that is unexplained by the models (i.e., 1 - R-Square), and the difficulties reconciling retrospective and prospective passage parameter estimates already noted, we conclude that, for experimental management planning, we will be better off simply ignoring the passage models. Furthermore, we conclude that the "historic," pre-PIT-tag era stock-recruit data will not add much information to the transport/no transport experimental management alternative. Instead, we should concentrate on obtaining precise, accurate estimates of data and parameters in future.

SAR Variability Example and a Cautionary Note

As an example of the variability of SARs and TCRs, we examined data for spring/summer chinook tagged at Lower Granite (LGR) in 1995-1996. Data in the top half of Table 3.3.6 are for fish tagged as part of the NMFS transport study in each year, with the last column being 1995 and 1996 combined. The first line displays the number transported, and the second the number returned to the river below the project (fish transported at other projects are excluded from the sample). The next two lines show jack and adult returns to LGR through October 7, 1999. [The 1999 returns should account for most 3-ocean fish that migrated downstream in 1996.] The SAR's in the next lines are simply (adults + jacks) / releases for each group, and their standard errors (Burnham et al 1987, p. 115). The TCR is the ratio of the two SAR's. It's variance is computed using Burnham et al p. 84. As can be seen from the table, the standard errors are fairly tight on all survivals and TCRs. More importantly, SARs for 1995 are 3-4 times higher in 1995 than in 1996.

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[8] Note that this is analogous to results for fall chinook, where adding passage model parameters did not make much difference in goodness-of-fit measures.

Recall that in the transport/no transport experiment, one would like to compare SARs and R/S in years with and without transport. Contrast this with a "within-year," conventional transport experiment. In a conventional experiment, the SARs derived for each group may be thought of as follows.

SAR(in-river) = Vn * (Bonneville to LGR survival)

SAR(Transport) = 0.98 * D * (Bonneville to LGR survival of in-river fish)

Where Vn = in-river survival;
0.98 = assumed in-barge survival of transported fish;
D = Survival differential for transported fish, compared to in-river migrants;
Bonneville to LGR survival = "Common" survival for both groups, Bonneville back to LGR, after allowing for "D". This is similar to method used for PATH retrospective "D" calculations.

It is straight-forward to solve for the common survival for each year. Assuming in-river survival of 0.5, the common survival is about 0.5% for 1995 and 0.25% for 1996. Note that the common survival [SAR(in-river) / Vn] is determined solely by in-river migrants. Because the TCRs and "D" values are ratios containing the common term, [e.g., SAR(transport)/SAR(in-river)], the common survival cancels out for within-year comparisons.

For the transport/no transport experiment, however, one would compare recruits/spawner between years with and without transportation: a between-year comparison. To see what the effect of this might be, in the lower half of Table 3.3.6, we "crossed" the data for 1995 and 1996. In the first column of the second half of the table, we used data for 1995 in-river migrants (as a stand-in for a no-transport year) and 1996 transported fish. In the second column of the second half, we reversed the comparison, using 1996 in-river migrants and 1995 transported fish. The results, as one might expect given very different "common" SARs for the two years (see above) were very discouraging: the pseudo-TCRs were 0.48 and 6.08 for the two cross-year comparisons.

An Example Power Analysis Using 1980-90 R/S Estimates and Assumed T/C Relationships

Given this high variability in Bonneville to LGR survival and the large amount of unexplained noise in the R/S data series noted previously, one may need to carry out between-year comparisons for many years before being able to detect differences in R/S between transport and in-river years. However, if the two-fold difference in SAR's in transport experiments (e.g., 1995 data from Table 3.3.6) carries over to R/S, some more rapid results may be possible. Unfortunately, the R/S data presently available (through brood year 1990 for spring/summer chinook, and brood year 1991 for fall chinook) ends at about the same time that PIT-tag based SAR estimates begin, around 1993-1995. Therefore, we cannot establish whether there is any correspondence between R/S and SAR's based on PIT tags. However, there was a weak correlation between Raymond's SAR estimates and R/S for spring chinook (e.g., PATH FY98 report), so there is some reason to believe that such a relationship should hold at present. One way to test this assumption would be to update the R/S analysis for spring chinook (through brood year 1994, downstream passage year 1996) and perhaps fall chinook (through brood year 1993, passage year 1994), and compare R/S to available PIT-tag SAR estimates. However, because the overlap between R/S and SAR data are very short, one cannot expect too much from such a comparison.

By way of an example with existing data, we have developed a very simple stock-recruit model. It uses spring/summer chinook R/S data from 1980-90, after transport began in earnest. The retrospective model has the following form:

Ln(R_j,t / S_j,t) = YEAR_t + Epsilon_j,t 3.3.3

Where:

R_j,t = recruits, stock j, year t
S_j,t = spawners, stock j, year t
YEAR_t = Year effect factor or class variable, year t, and
Epsilon_j,t is mixed process and measurement error.

No density-dependent term is included, since spawner densities in 1980's were very low.[9]

Now, what one would like to do is see how R/S varies with the proportion of the run transported. Unfortunately, according to passage model output, the Pbt was very high throughout this period: since there is little contrast in the existing data, we cannot use it to test hypotheses about TCR's, etc. Instead, for purposes of an example analysis, we will assume that differences in TCR's do translate into differences in R/S, and perform a simple bootstrap analysis to see how long it would take to detect the (assumed) differences.

More precisely, assume that in future the following relationships hold:

In transport years:

Ln(R_j,t / S_j,t) = YEAR_t + TRANS_T + Epsilon_j,t

While in non-transport years

Ln(R_j,t / S_j,t/TCR) = YEAR_t + TRANS_T + Epsilon_j,t 3.3.4

Where TRANS_T is a dummy variable, 1 when transport is operating and zero otherwise. In the absence of empirical data, 3.3.4 essentially assumes that the TCR's carry through to recruitment: the higher the TCR, the lower R/S should be in years when transport is turned off. The question then becomes: how long would one need to detect such differences, assuming the above relationship is in fact correct?

Table 3.3.7 contains the results from a simple 50-iteration bootstrap test. We have used the conventional 0.8, 0.05 combination often employed in biological power testing: an experiment is sufficiently powerful if it detects the effect of interest 80% of the time with 5% confidence limits. Shaded cells indicate sufficient power as just defined. As one can see, TCR's of 1.2 will not be detectable within 11 years. Note that this is 11 years of different passage treatments, plus 3-4 years for all recruits to return, for a total of 14-15 calendar years from the start of the experiment. Conversely, if the TCR is 2.0, this would be detectable within 3 years 95% of the time.

There are a number of reasons why the above example will overstate the power of the experiment. Firstly, it assumes the relationship that it purports to test: that TCR's are directly related to spawner -> recruit survival. Given the poor fit between passage model survival estimates and R/S (above) this likely overstates the relationship. Secondly, due to time constraints, we have not accounted for some of the variability in R/S and the inter-annual variance in the TCR's themselves. Other problems will doubtless arise following more extensive review of the methods.

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[9] In fact, when a model is estimated that includes spawners, the coefficients are usually significant and positive: more spawners is associated with higher R/S. This lends some support to the notion that spawner densities may be "too low" at present, and that adding nutrients could enhance survival.

On the other hand, the example results may understate the power of the experiment. R/S estimates for 1980-90 are available for at least 9 additional index stocks (data from Ray Beamesderfer, August 20, 1997). Use of this information in models like 3.3.3 – 3.3.4 would probably increase the power of future experiments. In addition, if we can perform T/C experiments concurrently with a transport/no transport experiment, that information may add to the confidence one can place in the experimental management results. This could be done by subsuming SAR estimates for transport and no-transport years (from PIT tag transport and control groups) into models similar to 3.3.3 – 3.3.4.

3.3.3 Benefits, Risks, Costs, and Trade-offs

Benefits and Amount of Learning Possible

The proposed experiment should greatly reduce the current uncertainties associated with the benefits (if any) of transportation. Unless an analysis prior to the experiment shows this to be the case, it will not be implemented.

Risks to Stocks

The obvious risk is that if transportation is beneficial, eliminating it for the run-at-large ½ of the time will be an obvious problem. On the other hand, if we had complete certainty about the effects of transportation, we would not carry out the experiment in the first place.

Costs

Costs should be roughly the same as for current transport experiments. Additional tagging efforts would increase costs, but this will be offset (to some degree) by reductions in spill (i.e., foregone power costs) and transportation in years when these are reduced.

Trade-offs

3.3.4 Inferences

- Short-term operations (3-10 years): If the adaptive management experiment shows that past transport experiments' results (TCR's approximately equal to 2) provide an accurate depiction of the relative benefits of transportation, then current "spread the risk" operations would be replaced by operations that maximize transportation. In contrast, if the adaptive management experiment suggests that "true" TCR's are less than one, one would discontinue transportation.
- Long-term operations (11+ years): if the SAR's from the adaptive management experiment suggest that, in combination with other measures, survival with full transportation is sufficient to lead to recovery, maximize transportation. Otherwise, proceed with 2-dam or 4-dam drawdown.

3.3.5 Confounding Factors

Obviously, any other experiments that were carried out concurrently (e.g., fertilization, reduced hatchery releases) might confound the results. The key to reducing the confounding is to vary the other experiments on a different schedule than that used for the transport experiment.

3.3.6 Practical Constraints

None that are unique to this experiment. So far as we know, it could be carried out under existing regulatory and legislative frameworks.

Table 3.3-1: Operational and monitoring measures- transport-no transport adaptive management experiment

Operational Measures	Transport Years	No Transport Years
Spill	No Voluntary spill	Spill to gas caps
Screening/Bypass in place?	Yes	Yes
Dewatering of bypassed fish at transport projects	Yes	Minimal - just for marking and detection of experimental fish
Transport at Snake Projects (Granite, Goose, Lower Monumental)	Yes	Minimal - just for experimental fish
Transport at McNary?	Yes	Minimal - just for experimental fish
Bypass/detection below McNary?	Yes	Yes
Monitoring/Experimental Measures		
Collection and PIT tagging above Granite?	Yes, maximize	Yes, maximize
Collection and Marking at Granite	Yes	Only as needed for experimental transport SAR/TCR estimates
Collection and marking at other transport projects?	Yes	Only as needed for experimental transport SAR/TCR estimates

Table 3.3-2: Variables to monitor/estimate for transport-no transport experimental management action.

Variable	How to Monitor	
	Transport Years	**No-transport Years**
Vn	As at present	Use flat plates after primary dewatering. Detection probability likely lower
SAR - Project-Ocean-LGR	As at present	As at present. Smolt detection probability likely lower.
SAR - Project-Ocean-BONN	As at present (after BONN adult detectors in, 2001	As at present (after BONN adult detectors in, 2001
Spawner-Recruit Survival	As at present	As at present

Table 3.3.3: Releases and Detections, Chinook Released and Migrating in 1998

Chinook Run	Hatchery/Wild	Event	Number Released or Detected
Spring	**Hatchery**	Released Above LGR	162,021
		Lower Granite Detections	52,532
		Little Goose Detections	35,440
		Lower Mon. Detections	23,756
		McNary Detections	15,165
		John Day Detections	6,563
		Bonn. Detections	4,999
		Towed Array (Below Bonn.) Detections	1,125
Summer	**Hatchery**	Released Above LGR	49,404
		Lower Granite Detections	13,473
		Little Goose Detections	9,506
		Lower Mon. Detections	7,441
		McNary Detections	3,168
		John Day Detections	2,079
		Bonn. Detections	1,473
		Towed Array Detections	228
Fall	**Hatchery**	Released Above LGR	102,596
		Lower Granite Detections	30,002
		Little Goose Detections	26,394
		Lower Mon. Detections	17,028
		McNary Detections	13,902
		John Day Detections	3,988
		Bonn. Detections	1,302
		Towed Array Detections	89
Spring	**Wild**	Released Above LGR	5,511
		Lower Granite Detections	1,451
		Little Goose Detections	1,450
		Lower Mon. Detections	1,090
		McNary Detections	606
		John Day Detections	391
		Bonn. Detections	206
		Towed Array Detections	22
Summer	**Wild**	Released Above LGR	6,110
		Lower Granite Detections	2,316
		Little Goose Detections	2,417
		Lower Mon. Detections	1,595
		McNary Detections	1,063
		John Day Detections	470
		Bonn. Detections	300
		Towed Array Detections	47
Fall	**Wild**	Released Above LGR	556
		Lower Granite Detections	124
		Little Goose Detections	121
		Lower Mon. Detections	68
		McNary Detections	58
		John Day Detections	27
		Bonn. Detections	11
		Towed Array Detections	0

Table 3.3.4: "Seasonal" CJS Survival and Detection Estimates, Chinook Released and Migrating in 1998. Reported survival rates are for the reach indicated in the column header; reported capture rates are at the last dam in that reach. For example, in the first column of numbers, survival rates are from release to LGR, while capture rates are at LGR.

Chinook Run	Hatchery/ Wild	All or 10% Detections		Release to LGR (Capture at LGR)	(S.E.)	LGR to LGS (Capture at LGS)	(S.E.)	LGS to LMN (Capture at LMN)	(S.E.)	LMN to MCN (Capture at MCN)	(S.E.)	MCN to JDA (Capture at JDA)	(S.E.)
Spring	**Hatchery**	**All**	**Est. Survival**	0.728	(0.002)	0.984	(0.006)	0.852	(0.007)	0.886	(0.013)	0.784	(0.025)
			Est. P (Capture)	0.445	(0.002)	0.44	(0.003)	0.386	(0.003)	0.278	(0.004)	0.154	(0.005)
Summer	**Hatchery**	**All**	**Survival**	0.595	(0.004)	0.957	(0.010)	0.861	(0.012)	0.885	(0.027)	0.822	(0.048)
			Capture	0.459	(0.004)	0.482	(0.005)	0.466	(0.007)	0.224	(0.007)	0.178	(0.010)
Fall	**Hatchery**	**All**	**Survival**	0.636	(0.003)	0.777	(0.004)	0.901	(0.008)	0.822	(0.015)	0.77	(0.051)
			Capture	0.46	(0.002)	0.529	(0.003)	0.385	(0.004)	0.382	(0.007)	0.142	(0.009)
Spring	**Wild**	**All**	**Survival**	0.586	(0.009)	0.946	(0.019)	0.942	(0.032)	0.94	(0.068)	0.7	(0.095)
			Capture	0.45	(0.010)	0.494	(0.012)	0.409	(0.015)	0.242	(0.018)	0.223	(0.028)
Summer	**Wild**	**All**	**Survival**	0.781	(0.008)	0.991	(0.015)	0.874	(0.024)	0.89	(0.048)	0.803	(0.098)
			Capture	0.485	(0.008)	0.531	(0.010)	0.414	(0.012)	0.309	(0.017)	0.171	(0.020)
Fall	**Wild**	**All**	**Survival**	0.566	(0.043)	0.639	(0.059)	1.089	(0.160)	0.484	(0.098)	0.48	(0.141)
			Capture	0.395	(0.038)	0.604	(0.045)	0.319	(0.055)	0.563	(0.088)	0.545	(0.150)
Spring	**Hatchery**	**10%**	**Survival**	0.524	(0.023)	0.947	(0.057)	0.796	(0.044)	0.963	(0.060)	0.789	(0.045)
			Capture	0.062	(0.003)	0.046	(0.002)	0.038	(0.002)	0.027	(0.002)	0.142	(0.005)
Summer	**Hatchery**	**10%**	**Survival**	0.415	(0.032)	0.936	(0.094)	0.773	(0.065)	1.026	(0.121)	0.855	(0.098)
			Capture	0.063	(0.005)	0.053	(0.004)	0.057	(0.004)	0.02	(0.002)	0.167	(0.009)
Fall	**Hatchery**	**10%**	**Survival**	0.634	(0.027)	0.788	(0.049)	0.916	(0.069)	0.703	(0.060)	0.859	(0.076)
			Capture	0.046	(0.002)	0.051	(0.003)	0.037	(0.002)	0.042	(0.003)	0.141	(0.009)
Spring	**Wild**	**10%**	**Survival**	0.499	(0.063)	1.121	(0.227)	0.985	(0.249)	0.734	(0.206)	0.799	(0.185)
			Capture	0.059	(0.009)	0.046	(0.008)	0.032	(0.007)	0.032	(0.007)	0.222	(0.028)
Summer	**Wild**	**10%**	Survival	0.84	(0.10)	0.81	(0.14)	0.87	(0.17)	1.05	(0.26)	0.73	(0.16)
			Capture	0.046	(0.006)	0.057	(0.008)	0.042	(0.007)	0.027	(0.006)	0.172	(0.020)
Fall	**Wild**	**10%**	**Survival**	0.265	(0.105)	1.088	(0.674)	1.488	(1.532)	0.269	(0.279)	0.773	(0.422)
			Capture	0.082	(0.039)	0.075	(0.042)	0.029	(0.029)	0.063	(0.043)	0.545	(0.150)
Spring/ Summer	**Wild**	**10%**	**Survival**	0.776	(0.079)	0.775	(0.107)	0.847	(0.118)	1.098	(0.200)	0.702	(0.120)
			Capture	0.041	(0.005)	0.052	(0.006)	0.048	(0.006)	0.026	(0.004)	0.19	(0.016)

Table 3.3.5: Comparison of Goodness-of-Fit (R-Square) for Alpha Models With and Without Passage Model Offsets. CRISP and FLUSH Results Are From August, 1997.

	Deviance (SSE)	**R-Square**
Null Model [Ln(R) = mean Ln (R)]	524.280	0.000
Eqn. 3.3.2 (No Passage Offset)	219.193	0.582
Model 3.3.1 (With Passage Offset):		
CRiSP C1	200.900	0.617
CRISP C3	205.285	0.608
CRISP C4	203.423	0.612
FLUSH F1	203.924	0.611
FLUSH F3	211.210	0.597
FLUSH F4	207.585	0.604

Table 3.3.6: SAR's and TCR's for 1995 and 1996 Spring/summer chinook transport studies at Lower Granite (LGR). Data from PITAGIS; Doug Marsh study groups tagged at LGR. Includes fish detected below LGR. Excludes fish transported below LGR.

	Migration Year		
	95	96	95 & 96 Combined
# Transported @ LGR	101,576	44,799	146,375
# In-river @ LGR	125,070	64,578	189,648
Transported Jacks + Adults @ LGR	516	57	573
In-river Jacks + Adults @ LGR	331	54	385
Transport SAR	0.508%	0.127%	0.391%
Std. Dev.	0.022%	0.017%	0.016%
In-river SAR	0.265%	0.084%	0.203%
Std. Dev.	0.015%	0.011%	0.010%
TCR	1.92	1.52	1.93
Std. Dev.	0.135	0.289	0.127
Between-Year Comparisons			
	95 in-river data, 96 transport data	96 in-river data, 95 transport data	95 & 96 Combined
# Transported @ LGR	44,799	101,576	146,375
# In-river @ LGR	125,070	64,578	189,648
Transported Jacks + Adults @ LGR	57	516	573
In-river Jacks + Adults @ LGR	331	54	385
Transport SAR	0.127%	0.508%	0.391%
Std. Dev.	0.017%	0.022%	0.016%
In-river SAR	0.265%	0.084%	0.203%
Std. Dev.	0.015%	0.011%	0.010%
TCR	0.48	6.08	1.93
Std. Dev.	0.069	0.868	0.127

Table 3.3.7: Example Power Analysis for A Range of TCR and Monitoring Periods. Shaded Cells Indicate Power of at least 80% at 5% Type-1 Error Level. See Text for Details.

Probability of Detecting an Effect @ 5% Type-1 Error Level						
	Years of Monitoring					
Assumed TCR	1	3	5	7	9	11
1	0	0	0	0	0	0
1.2	0	0	0	0	0	0
1.4	0	0	0.16	0.26	0.46	0.8
1.6	0	0.33	0.48	0.67	0.86	0.98
1.8	0.21	0.53	0.75	0.92	0.98	1
2	0.71	0.95	0.98	1	1	1

3.4 Drawdown of Two Snake River Dams

3.4.1 Description of Experimental Action /Research & Monitoring

Rationale

Study Objective: To determine effects of drawdown of lower Snake River reservoirs on overall recruitment success and post-Bonneville survival rates of chinook salmon. To verify estimates of survival rates in the lower Snake River for transition and equilibrated periods following a multiple dam breach, for chinook salmon smolts and adults.

The following Key Uncertainties from Table 2.2-1 could be tested for Snake River spring/summer chinook, fall chinook, and steelhead:

1. Relative Post-Bonneville Survival of Non-transported Fish (or simply in-river fish if a complementary transport-with-drawdown experiment cannot be designed);
2. Extra Mortality Alternative Hypotheses (Hydro I, BKD, Hatcheries);
3. Both Life-cycle Models (Alpha and Delta);
4. Length of Transition Period following Drawdown, and Survival Effects during Transition Period; and
5. Equilibrated Juvenile Survival Rate following Drawdown (including variability associated with water year/flow).

Description of Hypothesis: The Primary Response Variables would be Vn (in-river passage survival) for short-term responses, and changes in SAR or Relative Recruitment Success (RRS) for long-term responses (see Section 2.3 and Appendix D for a discussion of monitoring variables).

H0: The drawdown of lower Snake River reservoirs through multiple dam breaching would have no effect on these response variables (i.e., evidence against beneficial effects from dam breaching).

H1: The drawdown of lower Snake River reservoirs through multiple dam breaching would have a significant positive effect on all of these response variables, indicating improvements in passage survival (evidence in favor of benefits from dam breaching, but evidence against Hydro I extra mortality hypothesis).

H2: The drawdown of lower Snake River reservoirs through multiple dam breaching would have a significant positive effect on all of these response variables, indicating improvements in both passage and post-Bonneville survival (evidence in favor of Hydro I extra mortality hypothesis).

Note that by definition a 2-dam drawdown action cannot test the Hydro II extra mortality hypothesis, which states that post-Bonneville extra mortality will be "here to stay" until all four Snake River dams are removed.

Statistical Hypothesis:

H0: Vn breach = Vn unbreach, variation in Vn in years 1 to 6 following multiple dam breach = variation in Vn between 1992 and 1999, and Relative Recruitment Success breach = Relative Recruitment Success unbreach (or SAR breach = SAR unbreach)

H1: Vn breach > Vn unbreach, variation in Vn in years 1 to 6 following multiple dam breach < variation in Vn 1992-1999. Relative Recruitment Success breach > Relative Recruitment Success unbreach (or SAR breach > SAR unbreach), with the increase in RRS or SAR proportional to the increase in Vn.

H2: Vn breach > Vn unbreach, variation in Vn in years 1 to 6 following multiple dam breach < variation in Vn 1992-1999. Relative Recruitment Success breach > Relative Recruitment Success unbreach (or SAR breach > SAR unbreach), with the increase in RRS or SAR disproportionately larger then the increase in Vn.

One could also examine the change in upstream-downstream differences in RRS or SAR before and after the breaching experiment (as described in Section 2.3 and Appendix D).

Experimental action: The Corps would not have to breach all dams on the lower Snake River at once to measure contrasting effect on salmon survival. The breaching of less than four dams on the lower Snake River could increase smolt survival rates enough to measure the difference between treatment blocks. Increases in survival could occur by:

- directly zeroing out mortality at those dams that are breached;
- decreasing travel time through the warmer waters of the lower Snake River; and
- eliminating extra mortality given to smolts transported by barge.

Concurrently, releases of hatchery fish should be held near their recent levels to test possible density-dependent factors. Hatchery outputs could be reduced after 3–5 years to test this potentially confounding factor. Large woody debris would be routinely passed through the spillways of unbreached dams along with the slugs of increased suspended sediment, and allowed to redistribute downriver to form habitat attributes such as opposing bars.

The Corps could do an experimental breach with two dams to reduce critical drawdown uncertainties (e.g., length of transition period to equilibration of sediment budget, equilibrated juvenile passage survival and annual variation around the median estimate). The rate of learning could be optimized by designing the experiment to provide flow regulation from the unbreached dam immediately upriver from the breached dams. This flow regulation would be used to actively manage instream treatment and restore habitat attributes as soon as possible (the rate at which the river returns to an equilibrated "natural" state is a primary uncertainty in PATH modeling to date). Although the Lower Snake dams have only a small operating interval between full pool and minimum operating pool (around 5 feet), this volume of water could provide flows of up to around 200 kcfs for a week or two during the peak flow of a moderately high to high flow year. If higher flows are needed, a Special Regulating Plan could be obtained that allows limited drawdowns of reservoirs below minimum operating pool (the 1992 physical drawdown tests in Lower Granite and Little Goose dams were conducted using such a Special Regulating Plan)[10].

It may be more difficult to detect the effects of dam breaching on salmon survival with only a two-dam breach than a four-dam breach, because of less contrast in the effects. Also, a two-dam breach could have less short-term (< 5 years) survival benefits for Snake River stock survival if drawdown results in reach survival rates at or near the upper bound survival assumption used in the PATH simulations. However, a two-dam breach coupled with regulated treatments of flow, volume and shaping from the unbreached dams could provide more information on ecological effects of dam breaching. This information could help to resolve key uncertainties, decrease the time required to justify four-dam breaching to the region, and avoid potential litigation or deferment through the congressional authorization process. With a 4-dam breach, experimented flow magnitudes and shapes would have to be regulated at Hell's Canyon Dam. Because Hell's Canyon Dam is not under the Corps ownership, there would be less control of these flows over time.

Experimental dam breaching would be a comparative "before-after" study, or a single treatment experiment without a true paired control. Either case would be subject to Type I and/or Type II statistical error. Unregulated flow treatments, as well as variation in annual runoff volume in before and after treatment years could act to confound results. Direct upriver regulation of flows, which is possible with a two-dam breach, could be maximized by incorporating BiOp and flood control rules under a Special Regulating Plan as the Corps did in 1992 for the physical drawdown test of Lower Granite and Little Goose reservoirs (Wik et al. 1993). This would help to maximize the contrast for detecting a difference between pre- and post drawdown for the two dams breached. A two-dam breach design could provide within subbasin replication both spatially and temporally, depending upon which dams are designated for breaching.

Experimental Units would be blocked out treatments of breached versus unbreached reaches operated under two-flow treatments:

1. NMFS target seasonal average flow of 85-100 Kcfs on sliding scale for spring/summer chinook and 55 Kcfs for fall chinook;
2. mimic natural hydrograph with peaks exceeding near 200 Kcfs for a couple of weeks to scour consolidated substrates and transport the resulting sediment (Richmond et al. 1999).

Figure 2.3 in Richmond et al. (1999) estimates that a flow year of 200+ Kcfs may only occur at a 2-3% exceedence frequency based upon the 62 year flow record (Table 3.4-1). This exceedence flow indicates that a high volume of storage may be required for flow treatment 2 in low flow years, and that the treatment could not be done in consecutive years or low-to-moderate flow years. Spawning habitat

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[10] The 5 foot of reservoir operating space between full (normal) pool and MOP constitutes about 50,000 acre feet. This volume could flow releases of around 200 kcfs for about a week or two during peak flow of a moderately high to high (above average) flow year where inflow is 70-100+ kcfs. Powerhouse flow passage exceeds 100 kcfs without opening a single spill gate, so the concern lies within the nonproduction or control of production of %TDG supersaturation at spillways without adequate abatement when inflows exceed about 140 kcfs. If the experiment does not include breaching Ice Harbor, this one dam alone can pass 200+ kcfs and maintain TDG below 120% regulated by NMFS' interim/temporary water quality waiver (111 kcfs through the powerhouse plus 110 kcfs through the spillway). A near maximum single short-term release for Lower Granite and Little Goose would be 400 kcfs for a few hour blocks, maybe sustained for most of a single day. Such a maximum would pose short duration %TDG supersaturation problems for downriver spillways. The Special Regulating Plan would allow evacuation of reservoir water down to the threshold in elevation for adult ladder exit operation or as far down as spillway crest. This was done in 1992 for the physical drawdown test for Lower Granite and Little Goose dams.

restoration would likely require active mechanical breakup of consolidated substrate and maximum annual channel flows to assist in cleaning and transporting suspended sediment.

Table 3.4-1: Discharge in Kcfs equal to percent flow exceedance in the lower Snake River calculated from the 62 year flow record. Adapted from Table 2.1 and Figure 2.3 (Richmond et al. 1999).

% Time Flow Equaled or Exceeded	Discharge (Kcfs)
2-3%	200.0+
10%	111.5
20%	74.26
50%	31.71
80%	19.9
90%	16.68

Selecting which dams to breach for the experimental design: A two-reservoir drawdown experiment for the lower Snake River could be designed in various combinations. As to recommending which two dams to breach to give the most statistically powerful experiment will require discussion on the tradeoffs versus the pros and cons of each. The first two combinations listed below could be the most scientifically justified.

Table 3.4-2: Design flows and estimated trapped sediment volumes for the lower Columbia, lower Snake, and middle Snake river hydroelectric projects. The lower Snake River downriver of Lewiston, Idaho annually transports about 3-4 million cubic yards (MCYD) of new sediments that have been eroded from its drainage basin above the eco-influence of the Snake and Clearwater rivers. Sediment contributions by lower Snake River basin watersheds downriver of the Clearwater River confluence are not reflected in the sediment volumes listed in Table 3.4-2. Hence, the Lower Monumental reservoir likely has trapped greater than the 3-4 MCYD listed in the table due to input from the highly sediment laden Palouse River and the Tuccannon River.

Dam	**Year Reservoir Filled**	**Reservoir Length in Miles**	**Reservoir Volume (thousand acre-feet)**	**Hydraulic Capacity Power-House (Kcfs)**	**Hydraulic Capacity Spillway (maximum design capacity) (Kcfs)**	**Hydraulic Capacity Total Project (Kcfs)**	**Reservoir Storage for River Flows (Kcfs)**	**Estimated Sediment Volume Trapped to Date in Million Cubic Yards (MCYD)**
Bonneville (BON)	1938	46						
The Dalles (TDA)	1957	24						
John Day (JDA)	1968	76	2,367.0	452.0	2,250.0 (1,644.0 is maximum flood @ full pool elevation)	2,196.0	700.0 Kcfs (500,000 acre-feet)	
McNary (McN)	1953	61	1,350.0	232.0	1,368.0	1,600.0	100.0	27-36
Ice Harbor (IHR)	1961	32	406.5	106.0	850.0	956.0	0	21-28
Lower Monumental (LMO)	1969	29	432.0	111.0	850.0	961.0	0	3-4
Little Goose (LGO)	1970	37	565.2 (50.0 between elevations 738 and 733 feet)	111.0	850.0	961.0	0	15-20
Lower Granite (LGR)	1975	39	483.8 (49.0 between elevations 638 and 633 feet)	111.0	850.0	961.0	0	72-96
4 Lower Snake River Projects (LGR, LGO, LMO, IHR)		137		111.0	850.0	961.0	0	111-148
Hells Canyon (HC) (RM247)	1967	22	167.7	24.0 (peak turbine eff),27.0 (max)	186.0 + 88.0 outlet capacity @ normal full pool, 210.0 + 90.0 outlet capacity @ maximum pool	327.0	0	No estimate available, presumed low volume based upon rocky geology and presence of upriver dam prior to filling
Oxbow (OX) (RM273)	1910 for first structure for mining, 1961	12	58.2	25.0	300.0 @ full pool	325.0	0	No estimate available, presumed low volume based upon rocky geology and presence of upriver dam prior to filling

Dam	Year Reservoir Filled	Reservoir Length in Miles	Reservoir Volume (thousand acre-feet)	Hydraulic Capacity Power-House (Kcfs)	Hydraulic Capacity Spillway (maximum design capacity) (Kcfs)	Hydraulic Capacity Total Project (Kcfs)	Reservoir Storage for River Flows (Kcfs)	Estimated Sediment Volume Trapped to Date in Million Cubic Yards (MCYD)
Brownlee (BR) (RM285)	1958	57	1,420.1	22.2 (@ minimum power head of 176 ft (low flow year), max power head is 227 ft	300.0 spillway + outlet, 175,000 spillway only with 4 gates	322.2+, 450.0 total control capacity for emergency planning through spillway and outlet gates and 5 turbine units	975,318 maximum space for flood control	No estimate available, presumed higher volume than LGR (72-96 MCYD) based on geology, no presence of immediate dam upriver prior to filling, and large agricultural land use base upriver.

Possible Combinations

1. Breach Ice Harbor (IHR) and Lower Monumental (LMO) dams to evaluate effects on connected treatment reaches and use Little Goose (LGO) and Lower Granite (LGR) dams as flow regulating controls for treatments.

Pros

A. Current and future maintenance requirements for IHR and LMO projects are high. Upgrade and/or replacement of turbines at IHR has been on hold since 1996. An eroded stilling basin and additional gas abatement has been studied at LMO since 1997. A turbine rehabilitation study at LMO is scheduled pending the IHR turbine rehabilitation and the 1999 BiOp decision.

B. Diversity of historical (1934) fish habitat is greater in LMO with more deep pools, backwaters, tributary influences, and shallow open waters with sandy bottoms.

C. Estimates of survival derived from PIT-tag detection probabilities are higher in LGO during 1994, 1995, and 1996; but higher in LMO during 1997 and 1998. The survival response could be flow related. Both dams have eroded portions of stilling basin and some bypass problems that continue to receive attention. LGO typically has had slightly higher mortality rates in its juvenile fish bypass facility.

D. In the 1934 bathymetry reconstruction, LMO and IHR appear to be more important for fall chinook spawning (Table 3.4-3). Suitable rearing habitat for fall chinook is 6% of each reach for both LMO versus LGO.

Table 3.4-3: Percent of habitat that is suitable, unsuitable, or unknown for fall chinook spawning under natural river conditions (based on 1934 bathymetry reconstruction).

	Percent of habitat that is ____ for spawning		
	suitable	unsuitable	unknown
LGO	12%	80%	8%
LMO	41%	39%	20%
IHR	31%		
below IHR	33%		

Cons

A. Lyons Ferry hatchery operates within LMO.

B. Historic macroinvertebrate species diversity and composition decreases downriver from LGR to IHR (Edwards 1974; Dorband 1980).

C. Gradient is higher in LGO than in LMO (14 rapids compared to 9), and the macroinvertebrate species diversity and composition is higher in LGO because of increased cobble and gravel interspersed with sand.

D. High cost in dollars and possibly implementation time to retrofit tailrace entrances for adult ladder at LGO only.

E. The ability of IHR to spill 100% flow up to 100 Kcfs while not exceeding 120% Total Dissolved Gas (TDG) would be lost. Spill cap at LGO is the second lowest on the lower Snake River. Additional gas abatement at LGO would be required to maintain flood peak flows (flow treatment 2) below Federal and State Water quality Standards and interim waivers granted to NMFS for the 1995 BiOp and 1998 Supplemental BiOp.

2. Breach IHR and LGO dams leaving a flow control structure above each treatment reach (non-contiguous blocks).

Pros

A. Dam operations on the lower Snake River are not totally independent or mutually exclusive. Breaching every other dam (LGO and IHR, instead of LMO and IHR) provides a way to control for habitat differences between the lower reaches and the upper reaches of the lower Snake River as indicated by the 1934 bathymetry reconstruction.

B. The IHR-LGO breach design could provide replicate reaches separated spatially (but not temporally) and provides better controls within the Snake River basin for differences in-river channel form and function for historic and recent periods. They may also provide controls for possible differences in climatic effects when using outside basins for comparison (e.g., water temperatures are known to be higher in the Snake River subbasin versus other Columbia River subbasins (Waples et al. 1991)).

C. Potential for better statistical replication.

D. Potential for better contrast between upriver and downriver differences in habitat types and geomorphological attributes.

E. LGO has a greater quantity of stored sediment than LMO (Table 3.4-2).

F. Provides an opportunity to use a “regulating” dam immediately upriver of each breached reach to control flow quantity and shaping. Flow treatments would be used to determine the instream flows required to restore the river’s geomorphologic function in the least amount of time.

G. Lyons Ferry hatchery operates within LMO.

H. Gradient is higher in LGO than in LMO (14 rapids compared to 9), and the macroinvertebrate species diversity and composition is higher in LGO because of increased cobble and gravel interspersed with sand.

Cons

A. Historic macroinvertebrate species diversity and composition decreases downriver from LGR to IHR (Edwards 1974; Dorband 1980).

B. Diversity of historical (1934) fish habitat is greater in LMO with more deep pools, backwaters, tributary influences, and shallow open waters with sandy bottoms.

C. Estimates of survival derived from PIT-tag detection probabilities are higher in LGO during 1994, 1995, and 1996; but higher in LMO during 1997 and 1998. The survival response could be flow related. Both dams have eroded portions of stilling basin and some bypass problems that continue to receive attention. LGO typically has had slightly higher mortality rates in its juvenile fish bypass facility.

D. In the 1934 bathymetry reconstruction, LMO and IHR appear to be more important for fall chinook spawning (Table 3.4-3). Suitable rearing habitat for fall chinook is 6% of each reach for both LMO versus LGO.

E. Higher costs in dollars and implementation time than the IHR and LMO breach alternative for retrofitting tailrace entrances for adult ladders at LMO and LGR (two dams versus a single dam).

F. The ability of IHR to spill 100% flow up to 100 Kcfs while not exceeding 120% Total Dissolved Gas would be lost. Spill cap at LMO is the most restrictive on the lower Snake River. Additional gas abatement at LMO would be required to maintain flood peak flows (flow treatment 2) below Federal and State Water quality Standards and interim waivers granted to NMFS for the 1995 BiOp and 1998 Supplemental BiOp.

3. Breach LGR and LGO to evaluate contiguous blocks while allowing for comparison of smolt transport from LMO.

Pros

A. It has been suggested that the first dam encountered by migrating smolts has the most significant influence on mortality. Removing the first dam in the lower Snake River could test this presumption, although removing LGR and LGO would only result in moving the first dam encountered to LMO.

B. Lyons Ferry hatchery operates within LMO.

C. Historic macroinvertebrate species diversity and composition decreases downriver from LGR to IHR (Edwards 1974; Dorband 1980).

D. Gradient is higher in LGO than in LMO, (14 rapids compared to 9) and the macroinvertebrate species diversity and composition is higher in LGO because of increased cobble and gravel interspersed with sand.

E. Estimates of survival derived from PIT-tag detection probabilities are higher in LGO during 1994, 1995, and 1996; but higher in LMO during 1997 and 1998. The survival response could be flow related. Both dams have eroded portions of stilling basin and some bypass problems that continue to receive attention. LGO typically has had slightly higher mortality rates in its juvenile fish bypass facility.

F. Retrofitting tailrace entrances for adult ladders not required at any lower Snake River dam.

G. Potential to run a companion or paired test for smolt transport from LMO.

H. Eliminates the need for long-term dredging of commercial barge transportation channel currently being studied by the Corps.

Cons

A. SARs for transported smolts have been shown to be highest at LGR compared to SARs at LMO, although sample sizes at LMO have been small.

B. Breaching LGR would remove an important collection and index site for PIT-tag and transport experiments.

C. Diversity of historical (1934) fish habitat is greater in LMO with more deep pools, backwaters, tributary influences, and shallow open waters with sandy bottoms.

D. In the 1934 bathymetry reconstruction, LMO and IHR appear to be more important for fall chinook spawning (Table 3.4-3). Suitable rearing habitat for fall chinook is 6% of each reach for both LMO versus LGO.

E. LGR has the largest volume of trapped sediment of the four lower Snake River reservoirs (Table 3.4-2). After breaching LGR and LGO, the bulk of the volume would settle in LMO reservoir. Therefore, the experimental design would need to include a monitoring/regulating plan to control for potentially prolonged periods of turbidity and suspended sediment concentrations that would likely exceed Federal and State water quality standards. Breaching the highest sediment trap (LGR) has considerable risk associated with it based upon uncertainties related to sediment transport and its effects on salmon condition and passage.

F. Lower Granite reservoir has only 3% of the historical suitable spawning habitat for fall chinook contained within the 140 miles of the lower Snake River, compared to 31% in Ice Harbor reservoir (BRD GIS queries 1999).

G. Breaching Lower Granite Dam does not afford immediate upriver control for experimental flows, but relies upon Hell's Canyon regulations and operational constraints currently in effect.

4. Breach John Day (JDA).

Pros

A. Recent PIT-tag detection probabilities (Muir graphs presented to NWPPC, spring 1999) indicate that project survival through JDA alone (0.75) is almost equivalent to the cumulative project survivals through all four lower Snake River projects plus McNary (McN) (0.95^5 = .774).

B. McNary Dam would require few modifications because McNary was constructed before John Day dam, and its tail water structures (e.g., fish ladders) were designed to operate under natural river conditions.

C. The Blalock Island area has historically been important spawning habitat in both quantity and quality, and may have been an important historical metapopulation that seeded stocks in the Columbia and Snake River arms (ISG 1996).

D. Gas abatement and research at JDA is expensive.

E. Provides the best opportunity for learning about predator response to reservoir drawdown because most of the historical species abundance, composition, behavior, and distribution data has been collected in JDA. All other lower Columbia and Snake rivers reservoir predator estimates (except for LGR) are indexed (Bennett and Shrier 1987; Bennett et al. 1988-1997; Dresser 1996; Anglea 1997; Chipps et al. 1997; Connor et al. 1998).

Cons

A. Breaching would likely be considered an operation instead of an experiment and would require Congressional authorization due to an estimated cost in billions of dollars and an implementation date of at least 10 years. Congressional authorization requires commercial barging under a dam breach condition for JDA. The Corps Portland District engineers interpret that as requiring a new navigation lock excavated as a side channel with a new adult ladder prior to any breach activity.

B. Some believe that since inundation, the Blalock Island area has increased the quantity and quality of rearing habitat of Hanford reach subyearlings.

C. Gas abatement structures installed at JDA (e.g., flow deflectors installed since 1996) would be lost or mothballed.

Spatial and Temporal Components

It would be desirable to get at least three years of data per flow treatment (i.e., NMFS flow; natural hydrograph) with each one of those years representing a high, medium, and low runoff year. A single flow treatment would therefore require at least three years of juvenile PIT-tag and radio-tag releases, while two flow treatments would require at least six years of juvenile tag releases, plus three additional years from the last juvenile release to get complete adult return data. Currently, adult PIT-tag detectors are scheduled to be installed at all lower Columbia and lower Snake rivers by YR2010. This schedule should be accelerated by five to six years to satisfy the needs of this two-dam breach experiment proposal. Treatments would be interspersed in space and time by a randomized block design of annual flow treatment over at least six years duration. The following timeline is one possible sequence of events:

1. *Prior to YR1:* Continue monitoring of current or interim actions for before/after comparisons. Complete final engineering design and experimental study design for breach as soon as possible.
2. *YR1:* Breach two dams simultaneously.
3. *YR1 through YR6:* Monthly sampling of sediment transport and deposition. Monthly sampling of macroinvertebrate drift, species composition, and diversity index. Seasonal radio-tracking of adults to accurately account for loss numbers, location, and mechanisms. Weekly release groups of PIT-tag cohorts supplemented by radio-tags for monitoring of specific passage routing and rearing mechanisms (similar to current efforts).
4. *YR2 through YR6:* PIT-tag detection of jacks and adult returns (similar to current efforts).
5. *YR6 through YR12:* If results from of YR1 through YR6 monitoring show conclusively that dam removal has positive effects on juvenile and adult passage survival, then breach other two lower Snake River dams in YR6 and other Basin dams that can be justified. Removal designs for the other dams should have been developed and finalized during YR1 through YR6, and results from original 2-dam breach should be used for justifying authorization.

   If results from YR1 through YR6 monitoring are relatively inconclusive, then continue experimentation with justified modifications (e.g., different flow shapes/volumes, breach a third dam to widen contrast).

   If results from YR1 through YR6 monitoring show conclusively that dam removal does not have the hypothesized positive effects on survival, are negative or inconclusive, then refill the earthen fill sections and refill the reservoirs to an optimum elevation. This would also be an opportunity to implement new turbine and spillway designs. These designs can be studied and authorized for implementation during YR1 through YR6.

Biological Sample Units would be indices of physical condition and survival probabilities of run-of-the-river PIT-tagged fish representing cohorts of:

- the Snake River spring/summer chinook stock composed of the aggregation of the index stream stocks;
- the Snake River fall chinook stock;
- and the aggregate Snake River steelhead stock.

Compare detection probabilities for weekly cohort releases from above Lower Granite reservoir to paired cohort releases within side tributaries of each of the experimental reaches and/or releases for specific routing survival studies, similar to the studies currently performed.

Physical Sample Units would include most physio-chemical parameters currently monitored, such as temperature and Total Dissolved Gas. Current monitoring programs would be supplemented to increase spatial and temporal coverage for sediment transport (e.g., concentrations and distribution of turbidity, bedload and suspended sediment rate of suspension and dropout, and water velocity) and associated contaminant resuspension.

3.4.2 Monitoring Approach

Variables to Monitor

To assess the rate of restoration, habitat transects and/or zonal monitoring would be carried out at least monthly for juvenile salmonid use, macroinvertebrate colonization and diversity indices, and physio-chemical and physio-geomorphological parameters. Physical parameters include sediment constituent concentrations and distributional behavior, settling rates by size class and flow (velocity) for contribution to bedload or suspended sediment. Physio-chemical parameters can be more easily measured continuously through remote sensing programs currently deployed, such as the Corps TDGMSystem.

Smolt and adult salmon and steelhead condition index variables should be measured at least weekly throughout the respective migration seasons.

A primary objective of the two-dam breach experiment would be to assess PATH's assumptions about the duration of the transition period, and the survival of smolts and adults during the transition period. Monitoring of the spawners and estimated recruits for index stocks in the Snake River subbasin before and after the experimental action would be required to calculate or partition RRS and SARs. Contrasts with downstream stocks would also be informative, as described in Section 2.3 and Appendix D.

Predictions of how these variables would change following dam breaching under alternative hypotheses are described in Section 3.4.3.

Sources of variation include:

1. Annual flow year/runoff magnitude, shaping, and timing.

2. Errors in estimating routing proportions (FGE, Spill Effectiveness) and standardized average metrics for routing survivals required in the Cormack-Jolly-Seber single-release model that uses PIT-tag detection probabilities to estimate reach survival.
3. The remaining extra mortality variables not specifically addressed in the drawdown experimental design, such as influence of hatchery fish densities and behavior influencing wild fish, climate, disease vectors and annual exposures, etc.

Duration and Intensity of Monitoring

Frequency of biological sampling would be continual throughout each stock's outmigrant and adult return season based upon weekly releases of tagged fish. Frequency of relevant physio-chemical and physio-geomorphological attribute sampling would be either continual throughout each stock's outmigrant and adult return season for those parameters that can be remotely monitored and logged, and monthly at designated stations for habitat attributes and macroinvertebrates.

3.4.3 Benefits, Risks, Costs, and Trade-offs

Benefits and Amount of Learning Possible

To date, much of the contention with drawdown is the degree of uncertainty around the critical assumptions and the robustness of the methods available for estimating their parameters. The inability to estimate variability around the scant 1994-1998 data relative to the historical data does not allow for a scientifically supportable decision on large-scale (4-dam) drawdown with a high level of confidence. To the best of our knowledge, there are no other actual or proposed drawdowns that can confidently be used as a surrogate for estimating the effects of simultaneously breaching the four 100-foot head lower Snake River dams on salmon survival and viability.

Intuitively, an historical natural river would still be closest to the optimum conditions for salmonid productivity. However, these reservoirs have been in place for 25–40 years, with high accumulations of sediments behind at least two of the four lower Snake River dams. Therefore, a return to an assumed "pristine" state following breaching is not realistic under the current human population and their multiple uses of the river. Estimating the effects of a dam breach on restoring habitat attribute function and connectivity through an active management of flow shaping and magnitude in an experimental design is likely the most reasonable and scientifically supported approach. Unforeseen or poorly predicted effects of breaching of smaller dams throughout the United States, mostly related to sediment behavior on biota, has resulted in highly variable results. These include both positive responses, with rapid evacuation and restoration in 2–5 years, and negative responses, with prolonged bedload migrations and poor quality restoration progress that have not equilibrated in nearly 20 years. These experiences suggest that each breach action may be unique to some critical degree based upon geologic slopes, gradients, soils, biotic resilience, etc.

Based on these experiences, FERC is requiring Sediment Management Plans prior to authorizing the breaching of dams under their jurisdiction. There are considerable uncertainties about the potential short-term lethal or latent sublethal effects of massive sediment transport and contaminant resuspension on salmonid survival. Only a highly controlled experimental breaching of a subset of the four lower Snake River dams, without relying on an irreversible breaching of all four connected high head dams, provides a scientifically sound approach for addressing these uncertainties.

Drawdown experimentation is essentially a "before-after" study without controls, or a single treatment experimental study. Flow treatments for both the before and after treatment years can be designed to infer optimal shaping and magnitude for restoring geomorphological habitat components as quickly as possible. These flow treatments may also act to confound results if not treated and monitored appropriately. However, with the 2-dam breach design direct upriver flows could be managed (using BiOp and flood control rules under a Special Regulating Plan) to maximize contrasts for detecting changes in pre- and post-drawdown conditions for the two dams breached.

Dam operations on the lower Snake River are not totally independent or mutually exclusive. Breaching every other dam (LGO and IHR, instead of LMO and IHR) provides a way to control for habitat differences between the lower reaches and the upper reaches of the lower Snake River as indicated by the 1934 bathymetry reconstruction.

The IHR-LGO breach design could provide replicate reaches separated spatially (but not temporally) and provides better controls within the Snake River basin for differences in-river channel form and function for historic and recent periods. They may also provide controls for possible differences in climatic effects when using outside basins for comparison (e.g., water temperatures are known to be higher in the Snake River subbasin versus other Columbia River subbasins (Waples et al. 1991)).

Risks to Stocks and Costs

A potential biological risk to stocks and cost for the two-dam breach experiment (versus a four-dam breach operation) would be the likely requirement for structural modification of the adult ladder entrances in the tailrace of the unbreached dams immediately above a breached reach. Each lower Snake River dam was designed to operate under a narrow range of tailrace water elevation. Following a breach, the elevation in the tailrace of an upstream dam may be too low and may not meet current operational criteria for ladder entrance hydraulic conditions, stilling basin influence on hydraulics, and possibly even turbine operational efficiency. All of these factors contribute to percent smolt mortality. Conditions would likely worsen during years of high runoff, resulting in higher spill and less control of %Total Dissolved Gas.

Structural modifications can be time consuming and expensive to redesign and implement and thus may constrain the decision about which two of the four dams to breach as experimental treatments. Breaching two connected dams (IHR and LMO dams) or JDA alone could require that tailrace modifications be made only for a single upriver dam (which could take one to two years), whereas breaching two separated dams (IHR and LGO dams) could require tailrace modifications to both upriver dams (which could take two to four years). The decision may also be influenced by engineering parameters, such as which dam has the deepest stilling basin and requires the least expensive structural modification to ladder entrances.

Estimated monetary costs for implementation of breaching only (not including loss of commercial barge transportation, mitigation, or loss of hydropower production) could be:

1. Approximately \$200 million (LMO) to \$300 million (LGR) per dam, or a total of about \$1 billion for partial removal through earthen-fill breach of all four lower Snake River dams; includes all construction activity with shoreline protection throughout all four reservoirs and breach sections. Tailrace structural modifications for adult and juvenile passage are not required because dam structures would be isolated in the dry by coffer dams (Corps 1999, Lower Snake River Juvenile Salmon Migration Feasibility Study-Engineering Appendix- In Review).
2. Approximately \$400–500 million for partial removal through earthen-fill breach of two lower Snake River dams (depending on which two dams are breached); includes all construction activity with shoreline protection throughout two reservoirs and two breach sections (Corps 1999, Lower Snake River Juvenile Salmon Migration Feasibility Study-Engineering Appendix- In Review). Not all of this structural modification to the shoreline may be required prior to an experimental breach. Tailrace structural modifications for adult and juvenile

passage and gas abatement at either one or two unbreached dams located upriver would be required with an estimated cost of about $2-4 million for each of two years (Corps 1999, Dissolved Gas Abatement Study (DGAS), Phase II 60% Draft Technical Report).

3. Cost of data collection and monitoring would be near the current annual costs incurred on the lower Snake River through Corps and BPA research program funding (i.e., Corps Anadromous Fish Evaluation Program (AFEP)). Additional monitoring of geomorphological attributes and sediment migration would be required (currently, only sediment transects in LGR are irregularly monitored). These costs could be covered through reallocation of current funding used for routing and facility survival and efficiency studies at those dams that would be breached.

Any breaching experiment or operation would require an additional Environmental Impact Statement (EIS) under the United States National Environmental Protection Act (NEPA) with inclusion of ESA consultation with NMFS and FWS under coordination with NMFS' Regional Forum. Congress would have to authorize and appropriate funds.

Trade-offs

Because barge transportation of smolts from any lower Snake River collection dam is eliminated with drawdown, it will not be possible to continue to monitor the effects of transportation to see if an improvement in survival is due to drawdown alone or the result of drawdown in addition to the elimination of smolt transport. Any net benefits to smolt transportation (which includes 98% barge survival and post-Bonneville survival) would be lost, and assumed to be replaced by the hypothesized increase in in-river survival and reduction in post-Bonneville mortality. Transport from McNary Dam may have to be simultaneously evaluated. A no transport without drawdown experiment may be required before any experimental breaching experiment (see Section 3.3 for a description of such an experiment).

A four-dam breach is a strict "before-after" comparison that relies on an adequate time series of "before" data. These data have high variability because of informal monitoring, changing operations and structural configurations, and few structured experiments over multiple dams and reaches. Only the post-1994 PIT-tag detection probability data allow for reach estimates of survival that are useful as a comparative base. This same database could be used as a comparison with the 2-dam breach, supplemented by paired comparisons of the breached and unbreached dams and their reaches. The 2-dam breach would also be a "before-after" comparison study, although statistical power would be increased by using experimental treatment pairs of two breached and two unbreached control reaches, all regulated under the same flow regime in the same river environment. The two-dam breach design would reduce the degree of potential confounding caused by using separate rivers or segments either within or outside the subbasin.

Using the reach survivals for the lower Snake River estimated from 1994-1995 PIT-tag detection probabilities, it may be difficult to detect a clear signal from even a four-dam breach, let alone a two-dam breach. As an illustration of this, consider the simplified analysis of reach survivals summarized in Table 3.4-3. Current survival estimates through the lower Snake River from above Lower Granite to below Ice Harbor (with all four dams in place) range from 0.57 to 0.82 based on 1994-1997 PIT-tag data. Survival estimates through this reach with 4-dam drawdown range from 0.59 (Anderson et al. 1999, Table 28 - passage model estimate using flow-survival relationship that includes low flow years) to 0.96 (highest assumption used in PATH). Survival estimates through the lower Snake with a 2-dam drawdown range from 0.59 to 0.88. All of these ranges overlap substantially, suggesting that it would be difficult to detect changes in reach survival following a 2-dam or 4-dam drawdown. Perhaps the best information that can be expected from any dam breaching experiment in the relatively short-term (i.e., a decade) is the effects of flow on balancing the sediment budget. The most beneficial affect of drawdown on salmon viability may be the restoration of geomorphic and habitat attributes as quickly as possible. Uncertainties on length of transition and survival during transition require an active experimental approach to habitat attribute restoration or reconstruction. Instream flow treatments would provide the only means of assessing the rate of recovery and ecosystem stability while controlling for stressors such as sediment transport.

Table 3.4-3: Range of survival estimates under current, 4-dam breach, and 2-dam breach.

	Estimated Survival Rate	
	Lower Snake River	**Per project**
Current[1]	0.57-0.82	0.87-0.95
4-dam breach[2]	0.60-0.96	0.88-0.99
2-dam breach[3]	0.59-0.88	

Notes: [1] Estimated from 1994-1997 PIT-tag data.

[2] Lower bound based on passage model prediction; upper bound based on highest PATH assumption

[3] Lower bound = (lowest current per-project estimate)^2 x (lowest current per-project estimate)^2.
Upper bound = (highest current per project estimate)^2 x (highest 4-dam breach per-project estimate)^2

3.4.4 Inferences

Table 3.4-4: Example inference table for Hydro Extra Mortality Hypothesis.

	Observation and Inference	
Variable	**Observations consistent with "Hydro"**	**Observations not consistent with "Hydro"**
V_n	Must be monitored but not directly relevant to testing extra mortality hypothesis	
SAR or RRS	Increase > improvement in V_n	Increase ≤ improvement in V_n
λ_n	Increase	-*
$\Delta \lambda_n$	>1	≤ 1*

* Critical observations; - ≈ no change

1. **Residuals of ln(R/S) vs. S** (RRS) should increase for the trend sampled for the long-term, although it will vary annually with flow year and routing operations. Sampling methodology must remain stable to account for any error estimation. The difference between upstream and downstream RRS will increase following breaching.
2. **SAR** should increase when other variables are held constant or standardized. Monitoring of climatic variables/indices and other variables determined influential will be required. The difference between upstream and downstream SARs will increase following breaching.
3. **V_n** should increase. V_n must be measured at scales for lower Snake River reach survivals, each reservoir and dam specific survival, and for system survival through the estuary past BON. V_n will be expected to vary as influenced by flow year.
4. **λ_n** should increase, though as discussed in Section 2.3 this change is likely very difficult to detect precisely. The statistical design will need to account for estuary and early ocean influences. The

total mortality (**m**), including both passage and extra mortality, should decrease. Extra mortality would be expected to decrease following a time lag, or at a reduced rate compared to the immediate reduction in direct passage mortalities. Direct passage mortality is predicted to decrease because dam mortality would be assumed *a priori* to decrease to zero for passage through breached dams, and because of improved reservoir passage. The experiment provides an opportunity to test PATH passage model estimates for reservoir survival during and after the transition period. If a positive trend in survival becomes apparent, then passage model predictions of M can be adjusted. The fraction of fish below Bonneville which were transported (P) from either LMO or LGO could be zero depending upon which dams were selected as experimental treatments. Pre-drawdown experimental tests would estimate D for comparison to the drawdown experimental estimates in changes in D, if transportation continues with a 2-dam breach.

6. Appendix D recommends monitoring **m** for both Snake River and Lower Columbia River stocks, before and after the experimental action in the lower Snake River. However, other subbasin regions have stressors other than the number of dams and reservoirs. Therefore, we cannot easily assume that the Mid (Upper by ESA definition) or lower Columbia stocks are representative of the Snake River stocks. While changes in λ_n are more important than changes in **m**, they are more difficult to detect (Section 2.3). Nevertheless, both V_n and λ_n need to increase to be consistent with the hydro extra mortality hypothesis.

   Within-subbasin comparisons provided by a two-dam breach experimental design would provide more definitive information on the direct influences on salmonid survival (flow shape and volume, travel time) and the indirect influences on salmonid viability (habitat restoration, sediment budget) than the upstream-downstream comparisons associated with a 4-dam drawdown. Results from a two-dam breach design would provide better confidence by controlling for the partitioning of effects and by using spatial and temporal replication with paired control reaches to increase statistical power.

7. $\boldsymbol{\Delta \lambda_n}$ (i.e., $\lambda_{n,\text{ after action}} / \lambda_{n,\text{ before action}}$) should be positive dependent upon flow year and operation. This variable, while theoretically useful for differentiating among extra mortality hypotheses, is likely to have a very large variance, for reasons discussed in Section 2.3.

3.4.5 Confounding Factors

1. Changes in climate or other factors following an experimental breach could confound the results. If climate got better and λ_n for all stock groups had improved, it would be difficult to separate climate effects from drawdown effects. Climate indices and other metrics for more regionally based inferences would have to be simultaneously monitored (see Section 3.10).
2. Passage at adult ladder entrances and other modified tailrace structures on unbreached dams could not be tested until after the tailrace was drawn down. This makes it difficult to compare adult and juvenile mortality before and after the breach and separate the effects of modifying the tailrace structures from the true effects of drawdown if juvenile reach survival does not increase, or if juvenile reach survival decreases. To adjust or track such a potential confounding factor, route specific survival studies with radiotagged and PIT-tagged juvenile and adults would have to be simultaneously performed.
3. Elimination of barge transport and redistribution of predators. Drawing down reservoirs through dam breaching eliminates barge transport of smolts and could result in a dramatic change in abundance and/or distribution of in-river predators in the breached reaches. This would make it difficult to attribute the observed response in freshwater survival rate (and subsequent SAR or Ln (R/S)) to a particular treatment. This potential confounding factor requires an experimental design incorporating two-tailed tests for power. In addition, this potential for temporal confounding is one reason why

simple "before-after" comparisons at a single location are not very informative or statistically powerful.

Another design that would be more appropriate with drawdown would be a "before-after-control-impact" (BACI) scheme provided by a two-dam treatment design. This design includes sampling before and after some treatment, as well as comparisons between a control group of fish (or locations) and a treatment group of fish (or locations). The most advanced BACI designs (i.e., those that avoid several sources of confounding and permit wider extrapolation of results) include taking repeated samples both before and after a treatment at two different time scales (e.g., frequently within a year, and across several years). PIT-tag detection probability estimates for lower Snake River reach survivals between 1994-1999 may provide adequate repeated samples and estimates of variation before the drawdown treatments for short-term comparison on smolt survival and adult escapement post-drawdown. However, this time series is probably not long enough to test for estimated equilibrated survival.

Among the criteria used to select among potential management experiments, PATH should include a “do no harm” provision. Experimental actions should only be recommended if either:

1. the probability of meeting survival/recovery objectives is not affected if the underlying hypothesis is false; or
2. the increase in probability of meeting the objectives if the hypothesis is true, is much greater than the decrease in that same probability if the hypothesis is false.

The two-dam breach experiment is less irreversible than the four-dam breach with respect to time to refill or the ability to respond rapidly to unforeseen channel instability and potential detrimental impact on listed stocks. The four-dam breach is less of an experiment and more of an irreversible operation that carries high risk based upon the methods and assumptions used to estimate survival and timing of the transition period, and equilibrated survival (currently “hardwired” without incorporation of variation in PATH model simulations). There is also some potential for chronic and/or catastrophic negative effects of drawdown on salmon smolt survival, adult blockage, and adult spawning condition. Considering these potential effects, the two-dam breach experiment would be less risky by controlling flows immediately upriver.

3.4.6 Practical Constraints

Legal and Legislative

Point: Many agencies and Congressional Authorizations would not call four-dam breaching an experiment but an irreversible action, and would need justification to consider two-dam breaching an experiment. Must design structurally for possibility of refill.

Counterpoint: Any kind of manipulation is an experiment — just that some are poorly design experiments (see Schmitt and Osenberg 1996).

- Drawdown of lower Snake River dams through dam breaching is expensive with respect to funding requirements and time for designing protective measures for salmon passage during the breach activity.
- Loss of commercial transportation for the Ports of Lewiston, Idaho and Clarkston, Washington is the same for either a two-dam or a four-dam breach experiment or action. Loss of increments in hydropower electrical output would be less for a two-dam breach, but

> remains dependent upon which two dams would be selected for treatment. The hydropower dams on the lower Snake River provide about 4–5% of BPA's power, but provide flexibility in adjusting power supply in periods of high and low regional demand.

Literature Cited:

Anderson, J.J., J.H. Hayes, P. Shaw, .N. Beer, and S. Kroop. 1999. CRiSP1 Simulations for Reservoir Drawdown and Other Operation Alternatives for the Snake River Feasibility Study. Report to U.S. Army Corps of Engineers, Walla Walla District, Contract No. DACW68-96-C-0018 by Columbia Basin Research, University of Washington. 65 pp.

Wik, S.J., A.L. Shoulders, L.A. Reese, D.F. Hurson, T.D. Miller, L.L. Cunningham, J.P. Leier, L.E. Mettler, P.F. Poolman, J.A. Buck, C.A. Wolff, and J.S. Smith. 1993. 1992 Reservoir Drawdown Test: Lower granite and Little Goose Dams. U.S. Army Corps of Engineers, Walla Walla District. 141 pp

3.5 Drawdown of Four Snake River Dams

3.5.1 Description of experimental action / research & monitoring

Rationale

Description of Hypothesis: The completion of the Federal Columbia River Power System in the late 1960's through the mid-1970's and subsequent operation, has increased the direct and delayed mortality of juvenile migrants, which resulted in considerably sharper declines in survival rates of Snake River spring and summer chinook stocks (over the same period), than of similar stocks which migrate past fewer dams and are not transported.

PATH retrospective analyses (PATH FY96; Conclusion 3a.1) concluded that the differences in stream-type chinook indicators of productivity and survival rates between upstream (Snake) and downstream (Lower Columbia) are coincident in time and space with development of the hydrosystem (high confidence). PATH also concluded that, on a decadal scale, differences in these indicators between Upper Columbia and Lower Columbia are coincident in time and space with development of the hydrosystem (reasonable confidence, low confidence with regard to specific years).

Snake River fall chinook also declined following Snake River dam construction and operation, whereas similar stocks above fewer dams (Hanford - 4 dams; Lewis – 0 dams) have remained more stable. Snake River steelhead declines were also temporally associated with Snake River dam construction and operation.

The proposed experimental action recognizes that two major hydropower treatments already have been applied to upriver stocks, construction and operation of dams and juvenile fish transportation. This proposed experimental action partially reverses the treatments for listed Snake stocks (consistent with ESA requirements to ensure survival and recovery), and evaluates the magnitude of the response. Regional stock responses would be used to: (1) determine the extent to which dam removal affects survival and recovery of Snake River stocks; and (2) evaluate likely effects for decisions (John Day drawdown) on the listed Upper Columbia stocks.

Experimental action: Breach Snake River dams, stop transportation, evaluate regional stock responses to help guide John Day drawdown decisions for listed Upper Columbia stocks. Hatchery production could be either pulsed or kept constant under this approach (assumed constant in this option).

Explicit Objectives: Recover listed Snake River salmon and steelhead populations, determine consistency of Snake River population response to alternative hypotheses about delayed or extra mortality, and evaluate hypotheses relevant to future management decisions, specifically for recovery of upper Columbia River listed populations.

The stated purpose of experimental management (Section 1.1) is to "…**both maximize the ability to achieve conservation and recovery objectives,** and concurrently **learn something about key uncertainties to improve future management.**" This experimental option proposes reductions in direct and delayed mortality of Snake River stocks using the most risk-averse hydropower action to provide a large contrast in stock response for evaluation of mortality components. The magnitude of the observed change would be contrasted with that projected from alternative PATH hypotheses about extra mortality, to evaluate consistency of hypotheses with empirical data, improving the predictive capability for future management decisions, specifically for listed upper Columbia River populations. The timing and sequence of actions are based on earliest feasibility of implementation assumed in previous PATH analyses.

Testable hypothesis: Following Snake River dam breaching (A3/A5), the measured (estimated) values of R/S residuals, μ and relative change in SAR will best fit those projected by the one of following extra mortality hypotheses: (1) Hydropower, (2) Stock Viability, or (3) Regime Shift.

To test this hypothesis, projections of R/S response, μ and differential SAR specific to each regional contrast (Snake, Lower Columbia, Upper Columbia) would first be made (in FY99) using passage/transport models, which produce different ranges of in-river survival, T/C ratios and D-values. For example, non-hydropower extra mortality hypotheses for spring/summer chinook are expected to project a substantially smaller reduction in the Snake River μ (and also less relative change in SAR) following A3/A5 implementation. The projected values are specific to passage models and estimated D-values (from T/C and in-river survival estimates). A pattern of greatly reduced Snake River μ, relative to change in upper Columbia μ, would be evidence for the hydro extra mortality hypothesis.

Statistical hypothesis or decision rule: A framework is presented in Appendix D to relate future monitoring data to PATH life cycle models to help test hypotheses regarding the magnitude of responses to management actions. Measured responses in R/S residuals (i.e., Relative Recruitment Success (RRS)) and differential SARs would be compared to projected responses to determine which hypotheses best fit the data.

To test R/S response for spring/summer chinook, residuals from the R/S data from upstream and downstream stocks are measurable empirically and correspond to terms in the delta model. Now consider the differences in performance between upstream and downstream stocks. We would like to see if an action changes the performance of upriver stocks relative to downriver stocks. Though it would be nice to know whether an improvement due to some action occurs in system survival or extra mortality, the most important thing to know is that (RRS_u – RRS_d) is positive (i.e., the status of Snake River stocks is improving relative to downriver stocks). To assess the response of the system to implementing a natural river option, for example, we measure total mortality "m" and see how much it changes (see Appendix D for derivation using the delta model). That is,

RRS_u - RRS_d = m - ln(V_d) [Eq. D-9]

Note that equation D-9 is analogous to the parameter ‘μ‘ referred to throughout this section. This formulation would require an estimate of in-river survival (V_d) of smolts from the lower river tributaries (i.e., John Day River).

A comparison of [b]efore versus [a]fter conditions would attempt to measure the changes in the upriver-downriver differences in the residuals, that is:

$$\{RRS_u - RRS_d\}[a] - \{RRS_u - RRS_d\}[b] = m[a] - m[b] - \ln(V_d[a]/V_d[b]) \qquad \text{[Eq. D-10]}$$

Therefore the only thing we need to factor out is changes in the in-river survival of downriver stocks ($V_d[a]/V_d[b]$). Then we can directly measure the net benefit of an action in terms of $m_a - m_b$.

Appendix D also presents equations to address the question of where the net benefits occurred (i.e., improved system survival or post BON survival), but cautions that this becomes more difficult to determine.

We can model SARs in a similar manner. Smolt-to-adult return rates of upriver and downriver stocks would be estimated and contrasted as:

$$\ln(\text{scaled } SAR_u) - \ln(\text{scaled } SAR_d) = m - \ln(V_d) \qquad \text{[Eq. D-16]}$$

This is analogous to eq. D-9, and assumes that upriver and downriver stocks have similar ocean mortality. So tagging should in principle be an alternative way to get at total mortality rate, “m”. The SAR data involve fewer unknown coefficients, since the egg-to-smolt survival is not part of the estimate. Therefore, there is one less source of variation. In addition, observed SARs can be directly compared to the PATH goal of 2% to 6% needed for survival and recovery of Snake River spring/summer chinook (FY98 report).

Spatial and temporal components

Experiment period: Experiment period is 8 years (depending somewhat on the definition). There are actually four periods; pre-1970, 1975-2003 (implement A3/A5), 2004-2012 (evaluate effects of A3/A5), post-2012 (implement B1/B2, depending on results of evaluation).

Spatial resolution: Regions and stock groupings of the interior Columbia Basin.

Experimental units: Regional stock groupings (stream-type chinook, ocean-type chinook, steelhead in the Snake, Upper Columbia and Lower Columbia regions). Index stocks are replicates within the regional groupings.

Treatments Interspersed in Time: Implement A3/A5 for Snake River populations (3 region-species groups) in 2003, and B1/B2 for upper Columbia populations (3 region-species groups) in 2012.

Hypothesized response for Snake River stocks would be large reductions in μ for the 2003-2012 period following implementation of A3/A5 (Fig. 3.5-1), reflecting decreases in direct and extra (delayed) mortality. Implementation of B1/B2 in 2012 would further reduce μ for Snake River stocks. The expected change in μ depends on the alternative hypotheses about extra mortality and D. The hydro hypothesis was illustrated as H1 and Stock Viability hypothesis as H2 in Figure 3.5-1. (note: values are for illustration; to be replaced with PATH results). Similarly, the hypothesized SAR response would be improvements for Snake River stocks beginning in 2003, with an additional increase beginning in 2012 (Fig. 3.5-1). H1 and

H2 project similar increases in in-river survival rate and upstream passage survival rate following A3/A5 and B1/B2 implementation (Fig. 3.5-1).

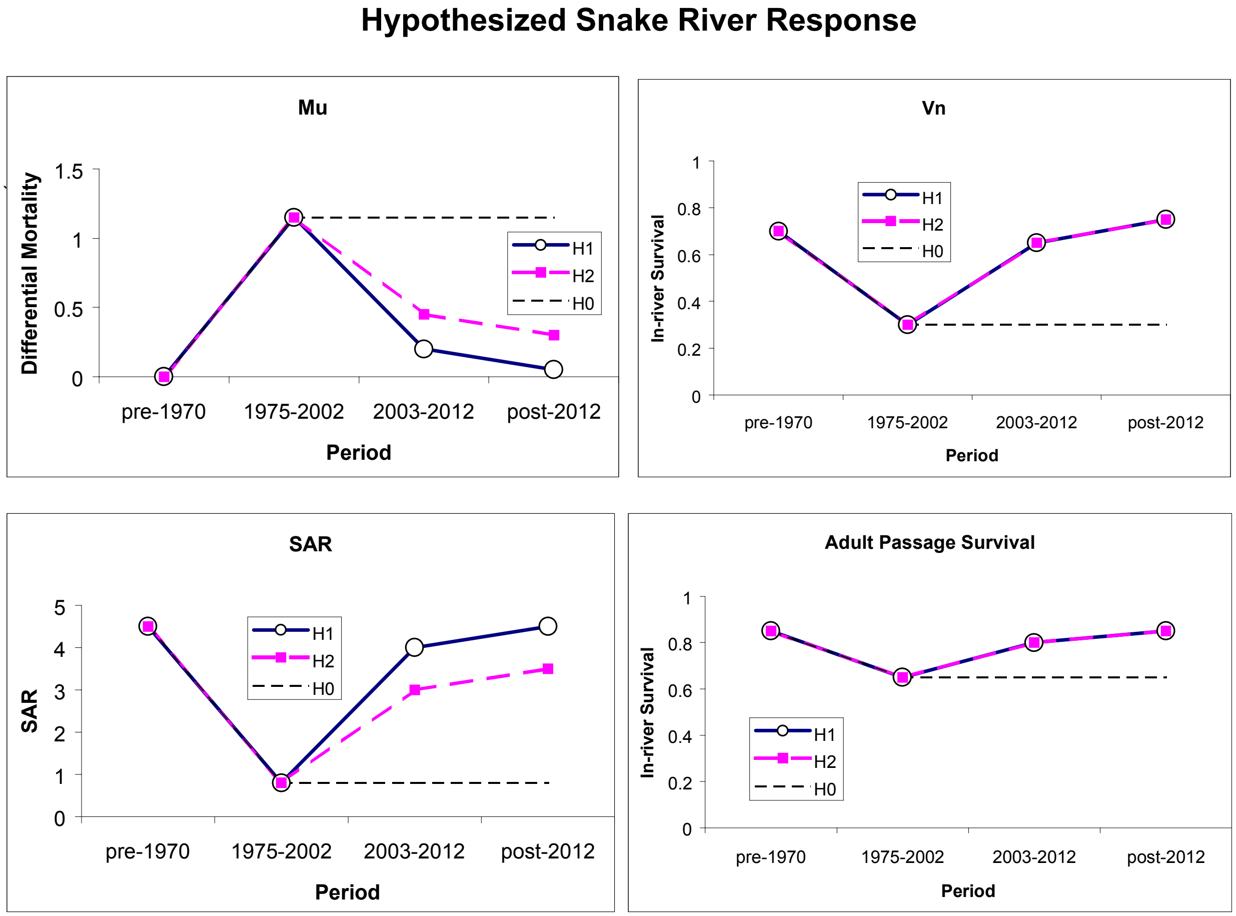

Figure 3.5-1: Hypothesized change for Snake River stocks in differential mortality (mu), SAR, in-river survival (Vn) and upstream passage survival. The null hypothesis (H0) represents no change from base period of 1975-1990 brood years. H1 and H2 represent hydro hypothesis and Stock Viability hypothesis for extra mortality, respectively. Plotted values are for illustration purposes.

Upper Columbia stocks could be incorporated as a third regional block to provide additional spatial and temporal contrast to Snake and Lower Columbia regions. One potential confounding factor is that the first step (A3/A5) restores free-flowing conditions in the lower Snake River *and* eliminates transportation from McNary Dam. Assuming that McNary transportation is neutral to Upper Columbia spring chinook, hypothesized regional stock responses for H0 and H1 would be represented by Figure 3.5-2.

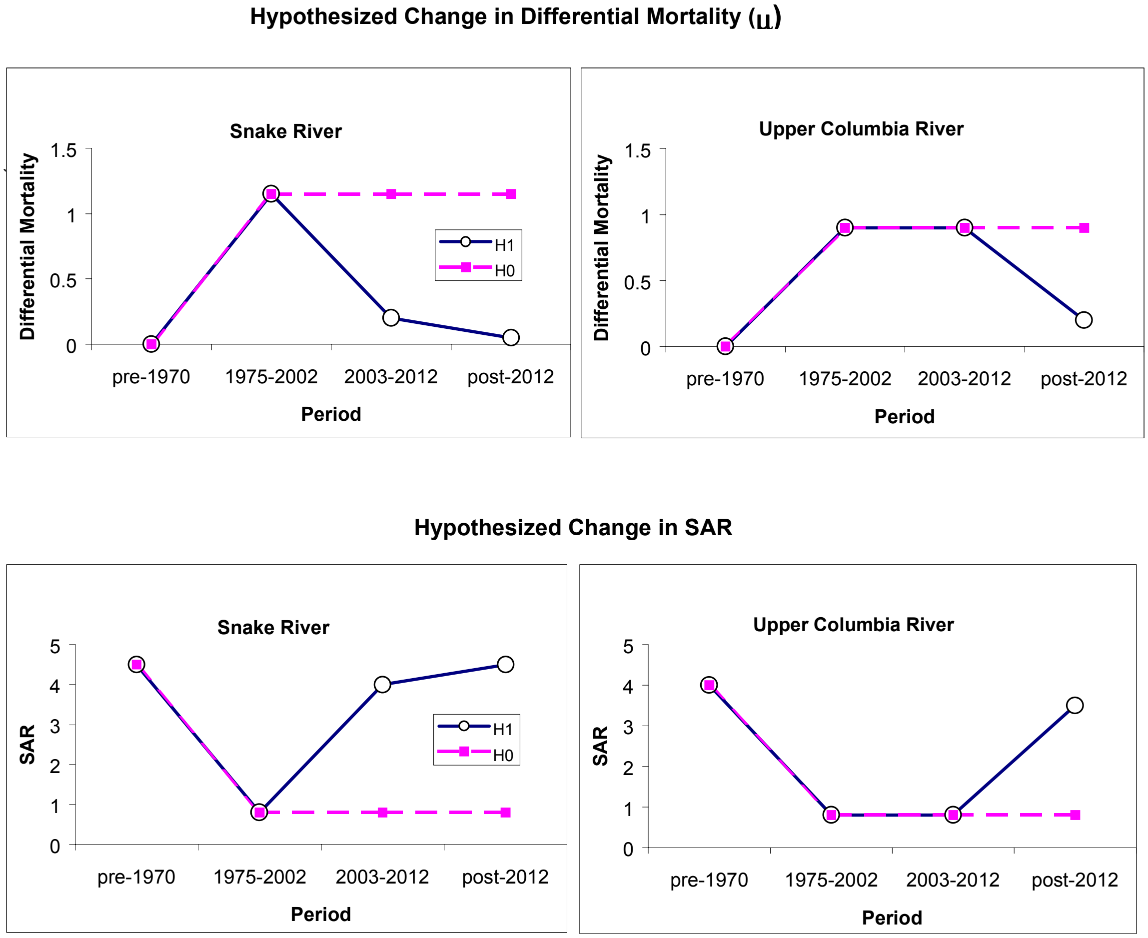

Figure 3.5-2: Hypothesized changes in mu and SAR for stream-type chinook from the Snake River and Upper Columbia in response to implementing A3 in 2003 and B1 in 2012. The null hypothesis (H0) assumes no change from base period of 1975-1990 brood years. H1 is represented by the hydro hypothesis for extra mortality. Plotted values are for illustration purposes.

In this case, no change would be hypothesized under H1 in Upper Columbia mu or SAR for the 2003-2012 period due to implementation of A3/A5, and improvements would be expected to follow John Day drawdown (B1/B2) in 2012 (Fig. 3.5-2).

If McNary transportation is not neutral (i.e., is either beneficial or detrimental) to Upper Columbia stocks, the H1 response in 2003-2012 would be higher or lower than represented in Fig. 3.5-2. Two ways to deal with this potential confounding would be to explicitly hypothesize the effect of ceasing McNary transportation for Upper Columbia stocks, or to experimentally turn on and off transportation from this location.

3.5.2 Monitoring approach

Variables to monitor

Key variables to monitor are R/S (stream-type chinook and ocean-type chinook), and SAR (stream-type chinook and steelhead) for stocks in the three regions (Snake, Upper Columbia, Lower Columbia). R/S data require estimates of age-structured escapement, hatchery fractions on the spawning grounds, upstream passage loss, and harvest rates in the intercepting fisheries. SAR data require estimates of smolt numbers, age-structured adult returns, upstream passage loss, and harvest rates in the intercepting fisheries.

For stream-type chinook, we are interested in changes in the differential mortality between stock groups. From R/S data differential mortality has been expressed as μ (Deriso et al. 1996), and represents both direct and extra (or delayed) mortality. An analogous differential mortality statistic, $[-\ln(SAR_1/SAR_2)]$, could be developed for SAR data from the three regions (where subscripts represent different regions). Available SAR data from Warm Springs River and Yakima River (above 2 and 4 dams, respectively), indicate substantially better survival through this life stage for these stocks than for Snake River stocks.

For ocean-type chinook, we are primarily interested in differential changes in R/S patterns. (μ cannot be estimated because of lack of replication within region). SAR data may be difficult to obtain because of difficult logistics in sampling subyearlings at the same life stage (migration vs. rearing).

For steelhead, we are primarily interested in changes in differential SAR between regions. R/S data are scarce, due to more complex life-history patterns (e.g., variable smolt ages), and difficulty in accurately sampling spawning population sizes. Currently we have historic SAR estimates for aggregate wild runs from the Snake and upper Columbia.

To apply equations D-9 and D-16, an estimate of smolt survival [V_d] for downriver stocks (i.e., John Day) is also needed. To determine *where* net benefits in survival improvement may have occurred for Snake River stocks following dam breaching, would also require estimates of system survival and its components M (direct survival), D (differential survival of transported smolts post-BON), and P[b] for the period before breaching. Retrospective estimates of these parameters have been made in PATH using alternative passage models and hypotheses. Errors in estimating these quantities (particularly D) may make it very difficult to get accurate estimates of system survival.

Duration and intensity of monitoring

Frequency of sampling is annual. A long-term commitment should be made to collect R/S and SAR data throughout the Columbia Basin for this and other experimental management options.

Index stock R/S data need to be continued, and specific recommendations developed to improve future data collection (e.g., age composition, redd expansions, hatchery fraction accounting). A coordinated program would be developed to estimate SAR for steelhead and stream-type chinook index populations throughout the interior Columbia Basin.

3.5.3 Benefits, risks, costs, and trade-offs

Benefits and amount of learning possible

This approach implements of the least risky management action (natural-river restoration), within an experimental framework. The approach directly tests the outcome of implementing the best biological option for Snake River stocks, to apply results to decisions for Upper Columbia stocks.

The natural river options are the most likely to recover listed Snake River salmon, and are less risky than transportation options, according to PATH FY98 analyses. The natural river options exceeded all three standards used by NMFS to determine jeopardy for Snake River spring/summer and fall chinook salmon, with one exception. The likelihood of survival of spring/summer chinook missed the 24-year survival standard by less than one percentage point when breaching was delayed for eight years. In most cases, the natural river options met the standards under the most pessimistic assumptions. None of the transportation options met the recovery standard, except under very optimistic assumptions. NMFS' (1999) A-Fish sensitivity analysis (using PATH results and different assumed values of D) indicates that the natural river options outperform transportation, *except when high D-values are combined with non-hydro hypotheses about extra mortality*. (i.e., a high D-value combined with hydro-related delayed mortality of in-river fish still results in the best option being natural river).

Implementation of this action would aid decisions on whether to restore natural river conditions in the John Day pool reach for listed salmon and steelhead in the Upper Columbia River. The staggered decision points for Snake River drawdown and John Day drawdown lend themselves to a staircase design, if implementation follows the same temporal pattern. Delaying Snake River actions while studies are conducted on John Day would negate this time step.

This approach was previously described in a concept paper (Petrosky et al. 1998) submitted to the Multi-Species Framework Process in November 1998. In addition to benefiting listed anadromous stocks in the Snake and Upper Columbia, this approach would help restore ecosystem function and benefit native lamprey, white sturgeon, and resident fish and wildlife, and non-listed anadromous stocks from above John Day pool (ibid.).

Quantitative assessment of likely power to detect effects should be determined in FY2000. Because the desired effect size is large for total mortality reduction, and was estimable retrospectively, there is reason to believe that proposed monitoring could detect the desired effect. However, PATH has not investigated whether there would be sufficient power to clearly isolate which of the extra mortality hypotheses was more likely, given the future, observed regional stock responses.

Risks to stocks

Snake River spring/summer chinook salmon are at extreme risk. Spawning population numbers since 1980 have been extremely depressed, and some spawning areas (Sulphur, Marsh creeks) have been devoid of spawners in some recent years. A greater concern is the fact that the depressed populations have been in decline since a brief positive trend during the early 1980s. For the seven stocks, the geometric mean of recruits per spawner to the spawning grounds (spawner to spawner ratio) has been less than 1.0 every year from 1984 through 1993 brood years (Fig. 3.5-3). Since 1984, the geometric mean spawner/spawner ratio for the seven Snake River index stocks has been 0.44, that is, each generation has returned less than one-half the spawners of the previous generation. Obviously, populations cannot survive this trend indefinitely.

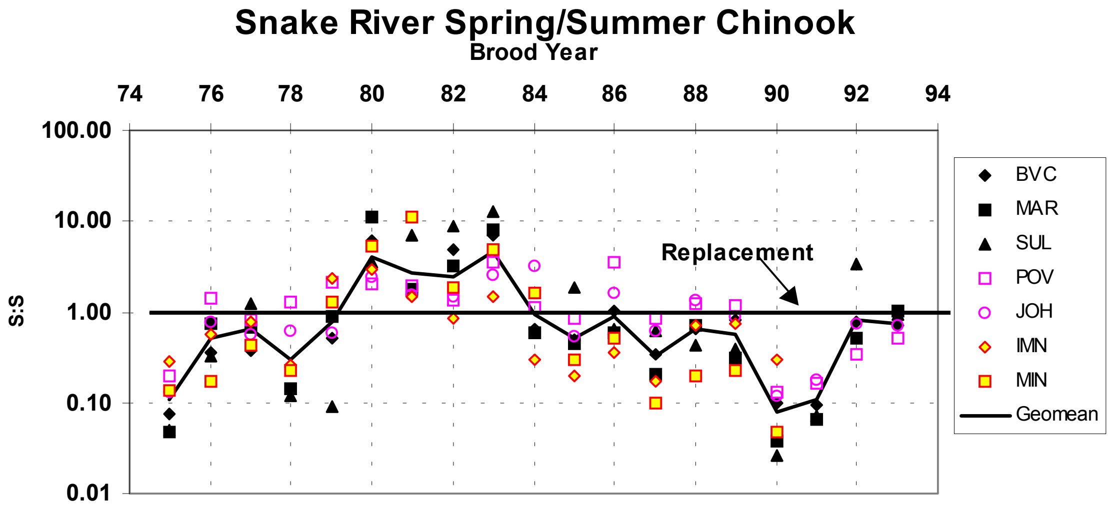

Figure 3.5-3: Spawner to spawner ratios (S:S; natural log scale) for seven index stocks of Snake River spring/summer chinook, 1975-1993 brood years (Beamesderfer et al. 1997; PATH updates for brood years 1991-1993). A value less than 1.0 indicates that the population has declined for that brood year. Stocks are: Bear Valley (BVC), Marsh (MAR), Sulphur (SUL), Poverty Flat (POV), Johnson (JOH), Imnaha (IMN), Minam (MIN). S:S estimates not completed for Imnaha and Minam stocks for brood years 1991-1993.

Experimental management options that propose continuation of status quo hydropower operations, while studying components of extra mortality, need to explicitly recognize this risk. The proposed option prioritizes recovery to listed Snake River populations with the *least risky* hydrosystem alternative, and uses information gained to evaluate feasibility of natural river restoration through the John Day Pool reach specifically for upper Columbia stocks.

According to PATH FY98 the A3/B1 option has the lowest risk, and highest biological benefits of any of the experimental actions proposed. Transportation-based actions had lower probabilities of meeting survival and recovery standards, and were less robust to uncertainties. The decision analysis indicates that there is relatively less risk with the natural river options of falsely assuming the wrong extra mortality hypothesis for Snake River stocks. That is, recovery is likely for natural river options, regardless of which extra mortality hypothesis is correct.

Costs

Implementation costs for A3/B1 or A5/B2 options will be determined by the Drawdown Regional Economic Workgroup (DREW).

Costs of the proposed experimental management program have not been estimated. However, costs of a program to systematically evaluate responses in recruitment patterns and SARs to Snake River dam breaching would seemingly be comparable to current research, monitoring and evaluation efforts. Such a systematic program is needed to assess any of the long-term hydropower operations.

Trade-offs

Relative benefits are high and relative risks are low. Implementation costs for A3/B1 or A5/B2 options will be determined by DREW. Evaluation costs are expected to be similar to the current efforts.

3.5.4 Inferences

Table 3.5-1: Example inference table for Hydro Extra Mortality Hypothesis.

	Observation and Inference	
Variable	**Observations consistent with "Hydro"**	**Observations not consistent with "Hydro"**
μ	Response consistent with that projected by H1 vs. H2 (Figure 3.5-1 and 3.5-2)	Response not consistent with that projected by H1 vs. H2 (Figure 3.5-1 and 3.5-2)
SAR or R/S	Response consistent with that projected by H1 vs. H2 (Figure 3.5-1 and 3.5-2)	Response consistent with that projected by H1 vs. H2 (Figure 3.5-1 and 3.5-2)
V_n	must be monitored but not directly relevant to testing extra mortality hypothesis	
λ_n	Increase	-*
$\Delta \lambda_n$	>1	≤ 1*

* Critical observations; - ≈ no change

3.5.5 Confounding factors

In addition to the action of breaching dams, survival improvements potentially could be attributed to elimination of transportation, climate change, changes in passage survival at remaining dams, and/or changes in hatchery effects, etc.

The issue of confounding, and approaches to reduce it, will be examined in FY2000. In principle, it might be possible to pulse treatments for some of the potential confounding factors, such as hatchery production or transportation from McNary Dam. Confounding also might be reduced with explicit and quantitative, *a priori* statements of expected effects for Snake River spring/summer chinook, fall chinook and steelhead. For example, the hatchery hypothesis for extra mortality presumably does not apply to Snake River fall chinook, since they migrate after the hatchery spring migrants have departed. Potential changes in hatchery production that may tend to confound spring/summer response would not confound fall chinook response. Similarly, it does not seem likely that climate change would be selectively influential for both Snake River spring/summer chinook and fall chinook (compared to lower river stocks), since these stream-type and ocean-type stocks do not share in time and space the same estuary/ocean environments.

3.5.6 Practical Constraints

Implementation of natural river options would require congressional authorization, whether or not the actions are organized into an experimental management design. Assuming that natural river restoration actions would be authorized, there appear to be no serious logistical constraints to a program that systematically evaluates recruitment patterns and SARs from the Snake, upper Columbia and lower Columbia regions. Costs of such a program would seemingly be comparable to current research, monitoring and evaluation efforts.

The initial decision on whether to pursue natural river options for listed Snake River stocks will come with NMFS's biological opinion on the operation of the Columbia Basin hydroelectric system in 1999. A review of the biological, economic and legal case for natural river options (Blumm et al. 1999) concludes that breaching of Snake River dams is economically affordable based on several economic studies, and that this option would produce net social benefits.

While achieving congressional authorization may be difficult, Blumm et al. argue that continuation of the status quo FCRPS operations is "legally unacceptable" on several grounds, and that legal processes in addition to ESA may come into play:

> "Although ESA will dominate the legal landscape during the next couple of years, the Northwest Power Act, the Federal Power Act, the Clean Water Act, Indian treaty fishing rights and the Pacific Salmon Treaty could also affect the drawdown decision." (p. 132).

"Among the largest legal threats to the current status quo in Idaho is the potential demand for water to restore Snake River salmon runs, either to satisfy the ESA, the Clean Water Act, or the Nez Perce Tribe's reserved water rights to the Snake River. Because these claims are quite large, they could jeopardize the water rights of numerous upstream diverters… Settling these claims through enactment of federal legislation authorizing breaching of lower Snake River dams and lowering John Day reservoir offers the best chance of restoring the fishing economy of both the Nez Perce Tribe and the state of Idaho, while preserving irrigation economies of Idaho and eastern Oregon and Washington." (p. 153).

Literature Cited

Blumm, M.C., L.J. Lucas, D.B. Miller, D.J. Rohlf and G.H. Spain. 1999. Saving Snake River water and salmon simultaneously: the biological, economic and legal case for breaching the lower Snake River dams, lowering John Day Reservoir, and restoring natural river flows. Environmental Law 28(4):101-153.

Petrosky, C., H. Schaller, P. Wilson, E. Weber and O. Langness. 1998. Integration of ESA recovery actions and experimental management into a multi-species framework. Multi-Species Framework Concept Paper, November 17, 1998 Workshop. Northwest Power Planning Council. Portland, Oregon.

3.6 Carcass Introductions / Stream Fertilization

3.6.1 Description of Experimental Action / Research & Monitoring

***Description**:* Life cycle survival is reduced because there are too few spawner carcasses to provide adequate nutrients in natal and freshwater rearing areas. This may be manifest as a decrease in parr-smolt survival or spawner(t)-spawner(t+1) survival. Either may be due in part to reduced parr or smolt size.

Experimental Action: Experimental carcass introduction or introduction of chemical fertilizers to increase stream nutrient levels. As nutrients increase, then parr-smolt mortality, and perhaps "extra" spawner-recruit mortality will decrease. Parr in about 30 rearing areas are already PIT-tagged, about 16 of which have data for six of the past seven years. Therefore, there are opportunities for staircase-style experimental designs for both parr-smolt and R/S monitoring (see below).

Evidence against this hypothesis:

- No change in smolts/spawner since 1960's (Chapter 9 of FY96 PATH report);

- No evidence of depensation in spawner-recruit data (estimated depensation parameter, “p” = 0);
- Lemhi (higher nutrients) has shown a rate of decline similar to other stocks.

Counter-arguments:

- The evidence in Chapter 9 depends on two different methods for estimating smolt abundance between the early 1960’s and the 1980’s. This may reduce the confidence one can place in the inferences.
- Even if there is no significant difference in smolts/spawner from 1960’s, there may be physiological effects that cause smolts to be less “fit” since spawner numbers decreased, which affects recruitment. This may be manifest as a reduction in smolt length and/or weight.
- The apparent lack of depensation in stock-recruitment data could occur because of insufficient data points at low spawner abundance.
- Measurement error may conceal depensation (Hinrichsen, *in prep*).

Other evidence:

- Kline et al (1990) show that marine-derived nitrogen and carbon are recycled by stream biota (this is one of numerous examples of similar work in Alaska, British Columbia, and Washington).
- Johnston at al (1990) and Stockner et al (1996) exemplify work on lake enhancement in British Columbia.
- Bilby et al (1996) demonstrated that marine-derived nitrogen and carbon from coho carcasses are incorporated into stream biota, including coho smolts. They also showed that growth rates of age-0 coho doubled following spawning (in their 2nd year of freshwater rearing).
- Bilby et al (1998) concluded that age-0 coho and age-0/1+ steelhead densities increased following the addition of coho carcasses.
- Michael (1995) demonstrated a strong positive correlation between the abundance of pink salmon spawners and recruitment of coho rearing in the same streams in the year the pinks spawned.
- There is an extensive literature on the incorporation of marine-derived nutrients into stream biota, including age-0/1+ anadromous sockeye, steelhead, and coho, and resident trout. Evidence of increases in density and size of parr/smolts also exists, but is not so extensive.
- Michael (1995) is apparently the only study that carries survival through to adult recruitment (but see Schmidt et al 1998 for a more indirect approach using sockeye).
- No similar studies have been done on chinook, although one by Bilby (1999) is starting this year. Because all studies to date are for salmonids other than chinook, the effects (if any) of carcass or nutrient additions are essentially unknown. The one exception is a recent (1998-99?) study in the Grande Ronde (N. E. Oregon, Howard Schaller, personal communication, 7/26/99)

Spatial and temporal components

In about 32 sites in the Snake tributaries, rearing spring/summer chinook parr are already PIT-tagged in the summer and fall (Table 3.6-1). Survivors are detected the following spring at traps and mainstem dams on the Snake and lower Columbia. Many of the sites (e.g., Bear Valley and Elk Creek) are probably too close geographically to use as separate experimental sites (enhancement in one creek would likely have similar effects on both), but a substantial number of well-separated sites should be feasible. At 16 sites, fish have been tagged in 6-7 of the past 7 years (see Table 3.6-2). Mean survival (naïve bootstrap, 5,000 draws from individual tagging/detection records) varies widely among sites and years (Table 3.6.4). Length of tagging is almost always recorded (Table 3.6.3), and the variance in the Cormack-Jolly-Seber estimates of overwintering survival is generally modest. This is true especially in later years (1994-on, with several monitoring sites at mainstem dams and most tagged fish returned to river), and where > 1,000 parr were tagged (Table 3.6.4, Figure 3.6.1). In general, there is a marked decrease in the "range" of survival estimates (defined here as [95th percentile- 5th percentile] / median survival) as the number of fish tagged approached 1000-2000, with much smaller decreases thereafter. From this, we conclude that increasing sampling effort to obtain 2000 +/- fish at each site and year would increase the precision of survival estimates (from tagging as parr in the summer/fall to LGR the following spring), but that samples > 2k would add little additional information. Power analyses are performed (Section 3.6.2.2, below) assuming no increase in tagging effort, however.

Table 3.6-1: 32 Sites with tagging data and Number of fish tagged, 1992-98.

Site	1992 # Tagged	1993 # Tagged	1994 # Tagged	1995 # Tagged	1996 # Tagged	1997 # Tagged	1998 # Tagged
Altulc	368	-	331	-	-	-	-
Bear/Elk	1632	1854	2916	-	-	671	1519
Bigc	758	730	1499	-	-	-	1452
Camasc	1011	215	1527	4	-	-	-
Capehc	205	-	1326	-	-	-	270
Cathec	1091	998	1983	1102	982	1250	1151
Cfctrp	855	1857	2883	359	538	988	2618
Chambc	497	570	1157	-	-	-	-
Crotrp	84	357	1164	40	-	84	273
Fren/Smile	541	892	1103	500	-	-	-
Grandr	915	1909	1853	-	27	724	937
Imnahr	996	2427	1758	2973	1458	4421	5003
Johnsc	633	-	192	-	-	-	5444
Lemhir	560	746	1717	179	269	752	3463
Loloc	923	1503	1639	144	-	620	2003
Lookgc	-	1944	3569	2025	15	1626	2151
Loonc	261	395	964	-	-	-	1030
Lostir	995	721	999	977	1045	997	1172
Marshc	999	7534	4891	275	-	1006	2971
Minamr	935	994	996	988	589	984	999
Pahsir	1072	561	2928	262	101	248	1160
Red/Amer	552	996	2758	634	25	1385	1571
Salref	222	316	1576	108	-	-	960
Salrnf	505	314	519	-	-	-	-

Site	1992 # Tagged	1993 # Tagged	1994 # Tagged	1995 # Tagged	1996 # Tagged	1997 # Tagged	1998 # Tagged
Salrsf	640	5196	3999	1777	2048	2869	3920
Sawtrp	739	99	1132	553	-	116	351
Secesr	-	673	1547	571	260	1176	3033
Sulfuc	710	-	726	-	-	-	442
Valeyc	1026	848	1550	-	-	-	1001
Wenr/Wenrsf	730	995	996	993	62	-	-

Table 3.6-2: Site names, locations, and climate regions for 16 sites with 6 - 7 years of tagging data, 1992-1998.

PITAGIS Site ID	Name	Palmer Drought Severity Index (PDSI) Climate Region
Cathec	Catherine CK - OR	NE_OR
Cfctrp	Crooked Fork Trap - ID	N_Cent_Canyons
Crotrp	Crooked Trap - ID	N_Cent_Canyons
Grandr	Grande Ronde - OR	NE_OR
Imnahr	Imnaha - OR	N_Cent_Canyons
Lemhir	Lemhi - ID	NE_Valleys
Loloc	Lolo Ck - ID	N_Cent_Canyons
Lookgc	Looking Glass CK - OR	Blues
Lostir	Lostine - OR	NE_OR
Marshc	Marsh Ck - ID	Cent_Mts
Minamr	Minam - OR	NE_OR
Pahsir	Pahsimeroi – ID	NE_Valleys
Red/Amer	Red/American - ID	N_Cent_Canyons
Salrsf	Salmon R South Fk - ID	Cent_Mts
Sawtrp	Sawtooth Trap - ID	Cent_Mts
Secesr	Secesh - ID	Cent_Mts

Table 3.6-3: Mean survival from tagging to LGR, 1992-1998.

Site	1992	1993	1994	1995	1996	1997	1998
Cathec	0.18	0.23	0.21	0.31	0.24	0.22	0.19
Cfctrp	0.32	0.3	0.19	0.3	0.25	0.53	0.32
Crotrp	0.41	0.26	0.13	0.09	-	0.27	0.23
Grandr	0.3	0.2	0.18	-	0.16	0.26	0.2
Imnahr	0.14	0.22	0.16	0.28	0.28	0.47	0.3
Lemhir	0.24	0.25	0.34	0.42	0.48	0.52	0.38
Loloc	0.3	0.27	0.22	0.04	-	0.44	0.18
Lookgc	-	0.23	0.14	0.24	0.25	0.29	0.26
Lostir	0.25	0.24	0.22	0.22	0.27	0.39	0.31
Marshc	0.14	0.3	0.21	0.37	-	0.57	0.31
Minamr	0.2	0.3	0.15	0.2	0.22	0.23	0.18
Pahsir	0.15	0.24	0.26	0.33	0.32	0.36	0.35
Red/Amer	0.15	0.29	0.13	0.27	0.28	0.33	0.15
Salrsf	0.31	0.19	0.11	0.16	0.15	0.26	0.15
Sawtrp	0.11	0.11	0.18	0.34	-	0.35	0.29
Secesr	-	0.12	0.13	0.13	0.23	0.32	0.24
Annual Average	*0.23*	*0.23*	*0.19*	*0.25*	*0.26*	*0.36*	*0.25*

Table 3.6-4: Annual average length of fish tagged and annual Palmer Drought Index (PDSI), for 16 sites with 6 - 7 years of tagging data, 1992-1998.

	Length at Tagging, mm.							PDSI, July – December, in year of tagging						
Site	**1992**	**1993**	**1994**	**1995**	**1996**	**1997**	**1998**	**1992**	**1993**	**1994**	**1995**	**1996**	**1997**	**1998**
Cathec	77	80	77	87	87	83	79	-3.70	0.22	-2.41	1.60	1.28	-0.16	2.09
Cfctrp	82	77	70	83	82	84	76	-2.30	0.23	-0.95	4.91	4.30	4.34	3.17
Crotrp	82	83	71	77	-	81	78	-2.30	0.23	-0.95	4.91	-	4.34	3.17
Grandr	75	68	71	-	92	80	79	-3.70	0.22	-2.41	-	1.28	-0.16	2.09
Imnahr	73	83	72	84	89	89	87	-2.30	0.23	-0.95	4.91	4.30	4.34	3.17
Lemhir	127	116	108	114	110	112	104	-1.13	1.00	-1.34	5.85	5.40	5.37	5.66
Loloc	76	82	75	109	-	86	68	-2.30	0.23	-0.95	4.91	-	4.34	3.17
Lookgc	-	86	77	91	92	87	86	-	0.02	-1.18	3.47	4.20	2.38	0.12
Lostir	84	72	72	69	88	96	84	-3.70	0.22	-2.41	1.60	1.28	-0.16	2.09
Marshc	71	83	77	93	-	87	74	-3.50	0.06	-3.56	2.33	-	1.18	1.70
Minamr	82	77	68	81	92	76	75	-3.70	0.22	-2.41	1.60	1.28	-0.16	2.09
Pahsir	99	105	96	113	113	113	101	-1.13	1.00	-1.34	5.85	5.40	5.37	5.66
Red/Amer	74	88	68	81	88	79	72	-2.30	0.23	-0.95	4.91	4.30	4.34	3.17
Salrsf	86	72	63	66	69	67	65	-3.50	0.06	-3.56	2.33	1.59	1.18	1.70
Sawtrp	86	-	85	96	-	91	91	-3.50	-	-3.56	2.33	-	1.18	1.70
Secesr	-	61	63	65	70	71	71	-	0.06	-3.56	2.33	1.59	1.18	1.70

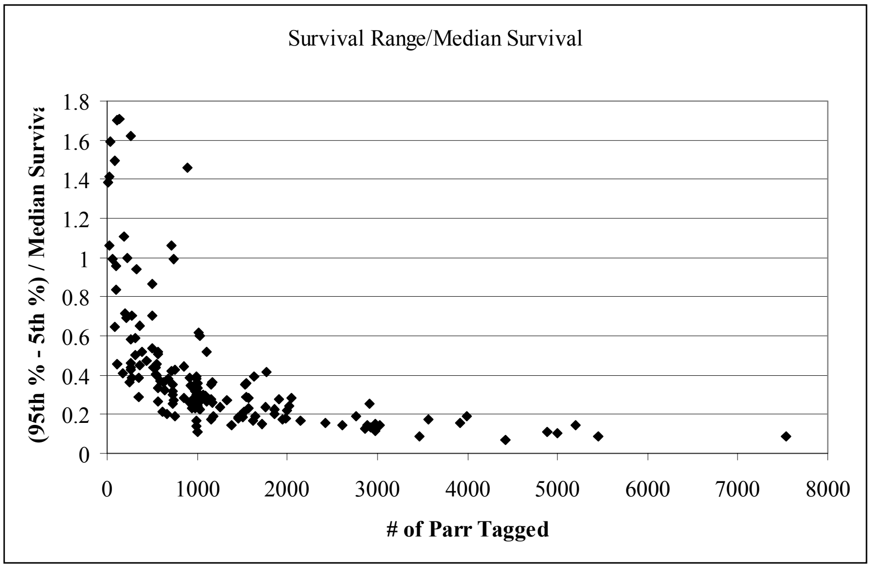

Figure 3.6-1: Survival Range/ Median Survival, 32 Tagging Sites, 1992-1998.

Table 3.6-5: 5% and 95% naïve bootstrap confidence limits on survival from tagging to LGR, 1992-1998.

	1992		**1993**		**1994**		**1995**		**1996**		**1997**		**1998**	
	5th %	**95th %**	**5th %**	**95th %**	**5th %**	**95th %**	**5th %**	**95th %**	**5th %**	**95th %**	**5th %**	**95th %**	**5th %**	**95th %**
Cathec	0.16	0.21	0.19	0.27	0.19	0.23	0.27	0.35	0.20	0.29	0.19	0.25	0.17	0.22
Cfctrp	0.28	0.37	0.27	0.34	0.18	0.21	0.24	0.38	0.20	0.31	0.49	0.57	0.30	0.35
Crotrp	0.21	0.76	0.22	0.32	0.11	0.16	0.03	0.17	-	-	0.19	0.36	0.16	0.32
Grandr	0.25	0.37	0.17	0.23	0.16	0.19	-	-	0.06	0.28	0.23	0.29	0.18	0.22
Imnahr	0.12	0.17	0.20	0.24	0.14	0.18	0.26	0.30	0.26	0.31	0.46	0.49	0.28	0.32
Lemhir	0.21	0.27	0.22	0.28	0.31	0.36	0.34	0.50	0.39	0.60	0.47	0.57	0.36	0.39
Loloc	0.26	0.34	0.25	0.30	0.20	0.24	0.01	0.09	-	-	0.40	0.49	0.16	0.20
Lookgc	-	-	0.21	0.25	0.13	0.15	0.21	0.27	0.09	0.44	0.27	0.32	0.24	0.28
Lostir	0.22	0.29	0.20	0.30	0.19	0.25	0.19	0.26	0.24	0.31	0.36	0.43	0.27	0.35
Marshc	0.11	0.16	0.28	0.31	0.20	0.22	0.31	0.45	-	-	0.54	0.60	0.29	0.32
Minamr	0.17	0.24	0.26	0.34	0.13	0.18	0.17	0.24	0.18	0.27	0.20	0.26	0.16	0.20
Pahsir	0.13	0.17	0.20	0.29	0.24	0.27	0.26	0.40	0.20	0.46	0.30	0.43	0.32	0.38
Red/Amer	0.12	0.18	0.26	0.33	0.12	0.14	0.23	0.33	0.14	0.44	0.30	0.35	0.13	0.17
Salrsf	0.26	0.36	0.17	0.20	0.10	0.12	0.13	0.19	0.13	0.17	0.24	0.28	0.14	0.16
Sawtrp	0.07	0.17	0.06	0.17	0.15	0.20	0.27	0.41	-	-	0.27	0.43	0.25	0.34
Secesr	-	-	0.10	0.14	0.11	0.15	0.10	0.16	0.18	0.28	0.29	0.35	0.22	0.26
Annual Average	*0.18*	*0.29*	*0.20*	*0.27*	*0.17*	*0.20*	*0.20*	*0.30*	*0.19*	*0.35*	*0.33*	*0.40*	*0.23*	*0.28*

General considerations for an adaptive management scheme include:

- Probably start small with modest number of sites treated (2-3?), to check for adverse effects.
- Perhaps add 2-3 sites/year, and treat every year after supplementation begins.
- Monitor parr-smolt survival, increasing size of tagged samples to increase precision of survival estimates to 2,000 per site and year, where feasible.
- Add additional treated (nutrient-enhanced) sites over time, assuming no apparent adverse effects.
- Nutrient-equivalent of 1950's spawner numbers (perhaps 1,000 carcasses/site) might provide an upper bound on supplementation inputs.
- Potential threshold effects: Bilby et al 1998 used 0.5-0.7 kg/m2 of carcasses. Don't know what this translates into for N, P, but would likely translate to substantial numbers of carcasses.
- Parr-smolt survival could be assessed starting 2nd year of study, recruitment would obviously take longer.
- Smolt-to-adult return rates (SAR's) contrasts between treated and untreated sites are not feasible. One would need to tag more parr than exist in the study areas (on the order of 1 million +).

3.6.2 Monitoring Approach

Variables to Monitor

The power analysis (see 3.6.2.2) focuses exclusively on parr-smolt survival. Other aspects that one might want to monitor include:

- Size of parr at tagging. The larger the parr size, the more likely it is that parr will survive to LGR (see next section), and perhaps survive to adults. Bilby (1998) shows a positive relationship between spawner abundance and parr size for coho and steelhead.
- Number of spawners returning. If supplementation works (i.e., increases survival to adult) this should be higher for treated sites. Increased spawner density may confound the treatment effects: if spawners increase substantially, the need (if any) for additional nutrients may decrease.
- Recruits/spawner. Same rationale as above. *Only* this variable directly addresses extra mortality, *if and only if* one can measure and control for other variables (i.e., spawner abundance, in-river survival, proportion transported, harvest, maturation timing, and upstream survival) that effect the ratio. It is possible that one might be able to test for R/S differences between treated and non-treated sites, but we do not analyze this here.
- Nutrient levels in streams. Supplementation should obviously increase these numbers relative to control sites. Since many spawning/rearing areas are in remote, high-elevation sites monitoring will probably be limited to a few occasions per year, when access roads are snow-free.
- Juvenile densities in rearing areas. These would be expected to increase in treated areas. In addition, increased numbers of juveniles may require more nutrients. Past monitoring of parr density has not produced consistent data that are readily amenable to quantitative modeling (William Thompson, USDA Forest Service, in prep).

- Marine-derived N and P in parr and other stream biota (see Bilby 1999).

Duration and intensity of monitoring

This section discusses the methods and results of a simple power analysis on parr-smolt survival. We first discuss how the data were derived from PIT-tag release-recapture information, bootstrapped to estimate moments of the survival distributions. Next, we demonstrate how this was combined with regional climate data, and used to estimate a simple "base case" model to explain how survival varies among sites and years. Finally, we show how this was used to simulate the power of future experiments to detect changes in survival as a result of (assumed) effects of nutrient supplementation, under a range of experimental designs (# of sites treated, # of years of monitoring, etc).

Creation of data for "base case" model:

1. Extract all wild spring/summer chinook tagged in June-December in the Snake above LGR (approximately 300K tagging records).
2. Extract from (1) records for 16 site in Table 3.6.2, 1992-98, inclusive. Eliminate records with questionable tagging locations or times. Result: approximately 147K records.
3. Bootstrap from (2) 5000 times (with replacement), to obtain 5k survival estimates for each site and year.
4. Use (3) to estimate mean, median, CV, 5th and 95th percentiles of survival data for each site and year.

Base-Case model:

1. Dependent variable is median survival for each site and year (median ≅ mean). Weight for each observation is 1/CV^2, following Smith (1999).
2. Possible independent variables include mean length at tagging, distance from tagging site to LGR, month of tagging, year effect (dummy variables), site (dummy variables), climate region (dummies), and Palmer Drought Severity Index (PDSI), for various periods before and after tagging.
3. "Best" model, or at least a reasonably good one, includes climate region, year of tagging, length at tagging (for each site and year), and the July-December PDSI, in year of tagging.
4. Base case model equation is Median Survival = intercept + **Region** + **Year of tagging** + Length + PDSI + error, with **Region** and **Year** being dummies.

Results for the base-case are shown in Table 3.6.6. The model explains about 78% of the variation in median survival over the 16 sites and seven years of data, using 13 independent variables for the 105 observations (one per site and year, with a few missing due to lack of tagging data). Cook's distance diagnostics reveal only one problematic observation – Lolo Creek for 1995. We suspect this is because the survival for that site and year is anomalously low – about 4% -- and not explained well by the model. However, it has little effect on the estimated parameters because it's CV is quite high. The reported results all include this observation. The Lemhi and Pahsimeroi parr are rather larger than those tagged at other sites, but these observations do not appear to be influential.

Table 3.6-6: Regression results, base case, weighted by 1/(Survival Coefficient of Variation). Dependent variable is median survival from tagging to LGR.

Base-Case Regression Results						
Parameter		**DF**	**Estimate**	**Std Err**	**Chi Square**	**Pr>Chi**
Intercept		1	-0.3331	0.0726	21.0555	0.0001
Climate Region:	Blues	1	-0.0652	0.0247	6.975	0.0083
	Cent_Mts	1	0.0491	0.0194	6.4252	0.0113
	NE_OR	1	-0.004	0.0217	0.0332	0.8554
	NE_Valleys	1	-0.1371	0.0284	23.341	0.0001
	N_Cent_Canyons	0				
Year of Tagging:	92	1	0.0439	0.0446	0.9687	0.325
	93	1	0.0066	0.0226	0.085	0.7707
	94	1	0.0473	0.0362	1.7045	0.1917
	95	1	-0.0506	0.025	4.0889	0.0432
	96	1	-0.061	0.0295	4.2714	0.0388
	97	1	0.0986	0.0156	40.0862	0.0001
	98	0				
Mean Length at Tagging		1	0.0069	0.0008	70.6881	0.0001
PDSI, July-December		1	0.0201	0.0065	9.46	0.0021
R-Square:	0.782					

Bootstrapping power tests: recall that we want to test the power of detecting a change in survival across a range of years post-treatment, number of sites treated, mean size of the treatment effect, and variation in the size of the effect (e.g., fixed size or drawn from a distribution). Therefore, we did the following:

1. Draw a base-case set of results at random from the 5k sets created in “base-case” data, step 3. Call this set “I”. It will have 7 years of data for each of the 16 sites, again with a few missing.
2. Draw a “post-treatment” set of results at random from the 5k sets. Call this set “J”, with I <> J. This set will have from 1-7 years of data for 16 sites. The number of treated sites may vary from 1-15, with control sites numbers equal to 16 minus the number of treated sites. Treatment and control sites are assigned at random from among the 16 base-case sites.
3. Add a treatment effect to each year of simulated survival data for each treated site selected in (2), above. This effect may be either fixed or drawn from a normal distribution. Note that other than survival at treated sites, the expected value for all variables in the post-treatment set “J” is the same as for the base-case set “I”. However, both survival and length at tagging will differ between the two, since the are drawn from two different outcomes of the 5k bootstrap games created previously.
4. Estimate a model identical to the base-case model previously described, but with a “treatment” dummy variable for the treated sites. If this treatment effect is significant at 0.05, the game is assigned a “1”, otherwise it is assigned a “0”.
5. Power is measured as the proportion of tests that have a “1”, for the # of power-test games are performed.

The above simulations were repeated from 100-1000 times. The power results appear to converge reasonably well after 100 or so iterations, but there are a few anomalies that don't affect the conclusions.

Note that several assumptions are implicit in this procedure. First, treatment (nutrient or carcass supplementation) is assumed to be in effect for each treatment site for each year post-treatment. Second, the independent variables other than length at tagging (region, the tagging year effects, and PDSI) are identical pre- and post-treatment. Length changes only because it was estimated separately for each of the 5k base-case games. This amounts to assuming that climate, at least as measured by the PDSI, can be represented post-experiment by the pre-experiment years of data. Although we have about 90 years of PDSI data available, we have not yet tested this assumption.

Results are shown in Tables 3.6.7 – 3.6.9. The results can be interpreted as follows, using the 1st row of 3.6.7 as an example. For an effect size of 0.025, 1 year of the experiment, and 1 treated site, 8% of the power tests were significant at 0.05 or better. As the effect size increase through 5%, 7.5%, and 10%, again with 1 treated site and 1 year of post-treatment data, the proportion detected correctly (at 0.05 or better) increases from 8% to 12%, 22%, and 32%. Looking at the 4th-to-last row (7 years post-treatment, 7 treated sites), the power increases from 48% to 89%, 99%, and 100% for effect sizes of 2.5%, 5%, 7.5%, and 10%, respectively.

Table 3.6.7 shows results with effect sizes "fixed," or assuming no variation in effect size. As one would expect, power increases with effect size (i.e., difference between survival with and without treatment), and with the number of years post-treatment. In addition, within a given number of years post-treatment and effect size, power usually increases with the number of sites treated, up to about 7 or 9, and then decreases slowly as the proportion of treated sites increases to more than half of the 16 sites. For some reason (we're not sure why) power is higher for 15 treated sites (i.e., only one control site) than when treating only a single site, with 15 controls.

Tables 3.6.8 and 3.6.9 display results for the most powerful type of tests — 7 treatment sites (and 9 controls), for 7 years post-treatment, with effect sizes drawn from normal distributions of different means and variances, as shown. In some respects, the results are more or less what one would expect: as variance in effect size increases, power decreases, all else held equal. However, for effect sizes that are reasonably powerful at low variance (5% and above), the variance can increase markedly without decreasing power by too much. This trend is continued in Table 3.6.9: the mean effect can be much smaller than it's standard deviation (see last few rows) without decreasing power dramatically. We believe a partial explanation is the normal distribution assumed for the effect size: for every anomalously small value of the effect, an anomalously large one will also be drawn, and the two balance one another.

Table 3.6-7: Power of ability to detect additive survival increase, assuming no variation in treatment effect.

Years Post-treatment	# of sites treated (of 16)	True effect = 0.025, Power (Proportion detected "correctly") @ 0.05	True effect = 0.050, Power (Proportion detected "correctly") @ 0.05	True effect = 0.075, Power (Proportion detected "correctly") @ 0.05	True effect = 0.10, Power (Proportion detected "correctly") @ 0.05
1	1	0.08	0.12	0.22	0.32
1	3	0.1	0.38	0.52	0.63
1	5	0.2	0.32	0.56	0.71
1	7	0.14	0.48	0.72	0.82
1	9	0.24	0.51	0.66	0.81
1	11	0.18	0.45	0.69	0.85
1	13	0.14	0.41	0.5	0.6
1	15	0.14	0.32	0.37	0.56
3	1	0.06	0.26	0.43	0.54
3	3	0.3	0.54	0.73	0.87
3	5	0.29	0.68	0.93	0.98
3	7	0.33	0.73	0.93	1
3	9	0.33	0.71	0.94	0.99
3	11	0.37	0.68	0.93	0.97
3	13	0.38	0.69	0.86	0.91
3	15	0.26	0.5	0.7	0.82
5	1	0.12	0.27	0.41	0.64
5	3	0.34	0.66	0.75	0.91
5	5	0.38	0.68	0.95	0.98
5	7	0.5	0.78	0.96	1
5	9	0.39	0.79	0.97	1
5	11	0.4	0.81	0.95	1
5	13	0.41	0.7	0.92	0.97
5	15	0.38	0.55	0.82	0.94
7	1	0.18	0.28	0.61	0.52
7	3	0.23	0.55	0.83	0.97
7	5	0.43	0.81	0.95	0.99
7	7	0.48	0.86	0.99	1
7	9	0.46	0.81	0.99	1
7	11	0.52	0.78	0.98	1
7	13	0.46	0.73	0.92	1
7	15	0.35	0.6	0.82	0.91

Table 3.6-8: Power of ability to detect additive survival increase, with variation as noted in treatment effect. All run with seven treatment sites and seven years post-treatment.

Variance	Std. Dev.	Power, Effect size = 0.01	Power, Effect size = 0.03	Power, Effect size = 0.05	Power, Effect size = 0.07	Power, Effect size = 0.09	Power, Effect size = 0.11
0.001	0.03	0.3	0.61	0.84	0.98	0.98	1
0.003	0.05	0.23	0.61	0.74	0.97	1	1
0.005	0.07	0.19	0.54	0.76	0.96	1	1
0.007	0.08	0.22	0.53	0.72	0.86	0.99	0.99
0.009	0.09	0.27	0.47	0.71	0.88	0.96	0.99
0.011	0.10	0.23	0.49	0.76	0.89	0.97	0.99
0.013	0.11	0.22	0.43	0.71	0.87	0.94	0.97
0.015	0.12	0.32	0.43	0.58	0.86	0.93	0.98
0.017	0.13	0.29	0.47	0.6	0.8	0.94	0.98
0.019	0.14	0.23	0.48	0.72	0.81	0.91	0.95
0.021	0.14	0.24	0.37	0.65	0.7	0.88	0.97

Table 3.6-9: Power of ability to detect additive survival increase, with (more) variation as noted in treatment effect. All run with 7 treatment sites and 7 years post-treatment.

Variance in Effect Size	Std. Dev. of Effect Size	Power, Effect Size = 0.05	Power, Effect Size = 0.07	Power, Effect Size = 0.09	Power, Effect Size = 0.11
0.023	0.15	0.58	0.79	0.91	0.96
0.025	0.16	0.60	0.74	0.90	0.93
0.027	0.16	0.60	0.74	0.86	0.93
0.029	0.17	0.58	0.76	0.87	0.95
0.031	0.18	0.57	0.73	0.84	0.92
0.033	0.18	0.57	0.71	0.84	0.94
0.035	0.19	0.55	0.68	0.80	0.91
0.037	0.19	0.54	0.71	0.79	0.90
0.039	0.20	0.57	0.69	0.81	0.91
0.041	0.20	0.53	0.64	0.79	0.90
0.043	0.21	0.53	0.67	0.80	0.89
0.045	0.21	0.51	0.70	0.78	0.88
0.047	0.22	0.52	0.65	0.79	0.88
0.049	0.22	0.50	0.66	0.78	0.90
0.051	0.23	0.49	0.65	0.76	0.86
0.053	0.23	0.49	0.65	0.79	0.84

3.6.3 Benefits, Risks, Costs, and Trade-offs

Benefits and Amount of Learning Possible

We need to work more on metrics here. Without some way of comparing trade-offs in extinction risks, experiment/monitoring costs, and other factors (sensu Dan Goodman's presentation at the NMFS workshop), it's going to be difficult to communicate this well to audiences outside PATH.

Risks to Stocks

- Disease spread is possible if carcasses are used.
- "Surprises" (both pleasant and unpleasant ones) obviously possible.

Costs

Obvious ones are:

- Increase in tagging effort (cost of tags and field researcher time).
- Fertilizer purchase and application.
- Time needed for carcass outplanting (assume cost of carcasses = 0).
- Spawner #'s and age may need to be monitored in areas where this is not done at present.

Trade-offs

3.6.4 Inferences

Anything beyond the obvious [i.e., smolt or R/S survival as f(fertilization)]?

Table 3.6-10: Observations and inferences for nutrient-driven stock viability hypothesis.

Variable	**Observation and Inference**	
	Observations Consistent with Nutrient-Driven Stock Viability Hypothesis	**Observations Not Consistent with Nutrient-Driven Stock Viability Hypothesis**
Parr-smolt survival	Increase (in fertilized streams), relative to controls	Decrease or no change (in fertilized streams), relative to controls
Parr Size	Increase (in fertilized streams), relative to controls. Assumes that fertilization effects egg-parr growth rates.	Decrease or no change (in fertilized streams), relative to controls
V_n	N/A, unless "enhanced" smolts perform differently	N/A, unless "enhanced" smolts perform differently
Spawner #'s and ages for S/S survival	Increase (in fertilized streams), relative to controls	Decrease or no change (in fertilized streams), relative to controls
R/S	Increase (in fertilized streams), relative to controls	Decrease or no change (in fertilized streams), relative to controls

3.6.5 Confounding Factors

Good design should be able to avoid most confounding, since real controls appear to be possible. One possible confounding factor is smolt or parr density and its effects on survival.

3.6.6 Practical Constraints

- The number of extra carcasses available may be a limitation.
- Public support would be needed, especially for actions on privately owned land.

3.7 Manipulate Production of Snake River Hatchery Steelhead

3.7.1 Description of Experimental Action/Research & Monitoring

Rationale

Study Objective: To determine if: 1) there is support for the stock viability extra mortality hypothesis (i.e., that something unrelated or additional to hydrosystem development has accounted for the total "extra mortality" [including D] estimated since the mid-1970s); and 2) reducing or eliminating exposure of wild Snake River spring/summer chinook migrants to hatchery steelhead can reduce total "extra mortality" of spring/summer chinook in the future, without breaching four Snake River dams. By simultaneously monitoring variables used to estimate D (Section 3.1), and/or by simultaneously conducting transportation experiments (Sections 3.2-3.3), relative impacts of hatchery steelhead production on transported vs. non-transported spring/summer chinook can be estimated.

Description of Hypothesis: Rationale for the stock viability extra mortality hypothesis and, specifically, the assumption that hatchery steelhead production is a causal factor, has been reviewed in the PATH August 1998 Weight of Evidence report and supporting documents. Briefly, Snake River hatchery smolt production increased greatly following the construction of the Lower Snake River dams. Steelhead production in particular increased from approximately 4 million smolts released per year to approximately 10 million smolts per year during the 1980's. The increase in hatchery production in the Snake basin coincides with increases in 'extra mortality' (including D) estimated for Snake River spring/summer chinook (e.g., Williams et al. PATH WOE Submission #1, 1998). Possible mechanisms for a negative effect of hatchery fish on co-mingled wild spring/summer chinook juveniles include: 1) delayed mortality resulting from stress of exposure during the outmigration from the upper Snake to below Bonneville Dam; 2) delayed mortality resulting from stress induced by interactions during periods of delay at hydropower projects or in the barge/collection systems; and 3) negative interactions in the lower river/estuary exacerbated by the relatively poor condition of wild Snake River spring/summer chinook migrants.

It is possible that any negative effects of hatchery production on wild Snake River spring/summer migrants are a result of a combination with hydropower effects. In that case, changes to the hydropower system may relieve mortality due to hatchery interactions. For example, effect (2) might be exacerbated by the lack of effective separation of hatchery steelhead from yearling chinook prior to holding in raceways and loading on barges. No separation occurs at Lower Granite Dam and separation efficiency at other collection projects ranges from only 36-71%. Future improvements in separation efficiency might eliminate at least part of the extra mortality, without reducing numbers or size of hatchery steelhead. However, it is also possible that hypothesized negative impacts of hatchery production may not be relieved by changes to the hydropower system. In that case, changes in the hatchery program would be necessary to relieve mortality effects on wild Snake River spring/summer chinook migrants.

Arguments against the stock viability hypothesis and, specifically, against the possibility of hatchery steelhead production as a causal factor, are summarized in the PATH August 1998 Weight of Evidence report. An experimental approach to evaluating the effect of hatchery production on "extra mortality" would attempt to resolve the differing interpretations of currently available information.

Experimental Action: Manipulate Snake River hatchery steelhead production to reduce exposure of wild Snake River spring/summer chinook juveniles to relative levels at or below those experienced in the 1970's. Hatchery steelhead exposure with wild Snake River spring/summer chinook juveniles could be reduced in several ways including reducing the number of steelhead smolts released, reducing the size of steelhead smolts at release (reducing steehead biomass), or delaying steelhead smolt releases until late in the migration season. It would be desirable to alternate or vary relative exposure across a series of brood years, taking advantage of the contrast to evaluate the relative effect of reduced exposure.

The exact experimental design would need to be developed as part of subsequent PATH experimental management tasks. However, two alternatives have been explored to provide examples of the efficacy of possible approaches. Each starts with a quantification of the hypothesized effect of hatchery steelhead production on total extra mortality (including D). A simple linear relationship between (m-M) and SH hatchery releases (as derived from WOE Submission 1, Figure 5) yields the functional relationship shown in **Figure 3.7-1**.

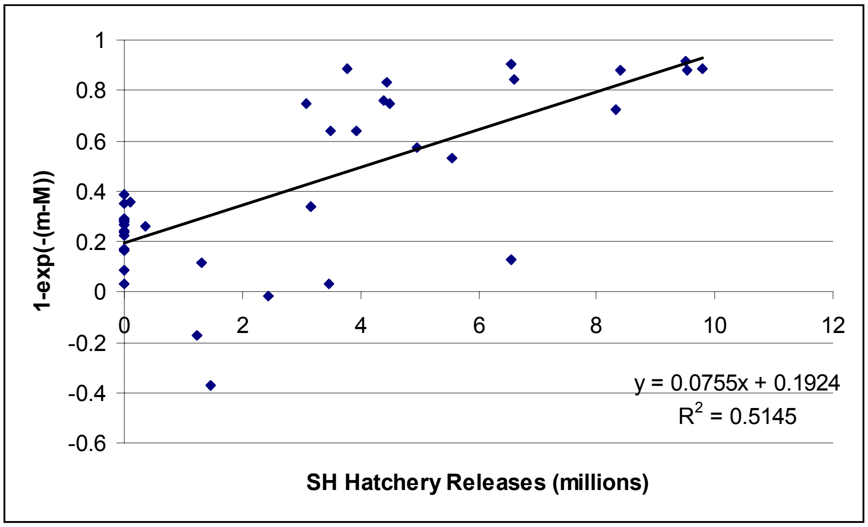

Figure 3.7-1: Regression of Snake River spring/summer chinook total extra mortality (including D), as determined from the PATH Delta model, and steelhead hatchery releases affecting 1952-1992 brood years.

Spatial and Temporal Components

Spatial contrasts are not possible with this approach because hatchery interactions are hypothesized to occur throughout the mainstem Snake and Columbia River. Temporal contrast would be generated by curtailing hatchery production or shifting release levels forward in time during treatment years to reduce exposure of wild migrating spring/summer chinook to hatchery steelhead. Treatments could either be in alternating years or in alternating brood cycles. The objective would be to reduce exposure in treatment years to no higher than the levels experienced in the late 1970s.

Two hypothetical experiments illustrate possibilities for generating temporal contrast in treatments (Table 3.7-1). In these examples, the treatments are held constant for 5-year intervals approximating brood cycles. Hypothetical Experiment 1 would attempt to generate extreme contrast by increasing hatchery releases well above current levels to 10 million smolts in one treatment and by reducing hatchery production to 500,000 smolts in the alternating treatment. Note that this example is provided only to show the effects of a somewhat extreme degree of contrast among treatments. Practical implications are discussed below. Hypothetical Experiment 2 would compare hatchery releases near current levels (8 million smolts) to a level more similar to that in the 1970s (4 million smolts).

Table 3.7-1: Hypothetical examples of two possible experiments to evaluate effects of hatchery steelhead production on Snake River spring/summer chinook salmon survival. Experiment 1 represents a high-contrast, five-replicate experiment, while Experiment 2 represents lower contrast among treatments and only two replications.

Experiment	Minimum smolt releases	Maximum smolt releases	Interval	Duration	Start of experiment
1	500,000	10 million	5 years	50 years	Year 10
2	4 million	8 million	5 years	20 years	Year 10

3.7.2 Monitoring Approach

Smolt-to-adult returns (SAR) and returns per spawner (R/S, or the difference between R/S and predicted R/S = RES) of the wild spring/summer chinook index stocks would be the primary response variables. Survival of fish from the alternating treatment periods would be compared to determine if there is an effect of hatchery releases. Lower river index stocks would need to be monitored to account for common year effects that could fortuitously coincide with different treatment periods. In-river survival (Vn) and ratios of transported and non-transported SARs would need to be monitored concurrently (Sections 1.1-1.3) to draw inferences about the relative effects of changes in hatchery releases on D and extra mortality of in-river migrants. See Table 3.7-2 for a summary. If possible, data from PIT tagged groups would be used in the analyses.

Duration and Intensity of Monitoring

The expected duration of the experiment would depend largely upon the details of implementation of the reductions in exposure to hatchery steelhead. For the two hypothetical alternatives described in Table 3.7-1, an analysis by C. Peters (July 26, 1999 report) suggests that treatment effects would be discernable with the high-contrast, long- duration (50+ years), Experiment 1 (Figure 3.7-2). However, treatment effects are not likely to be seen with the lower-contrast, shorter duration (20 years) Experiment 2 (Figure 3.7-3). Future analyses would be necessary to explore additional experimental options that are intermediate to the two examples presented in this report.

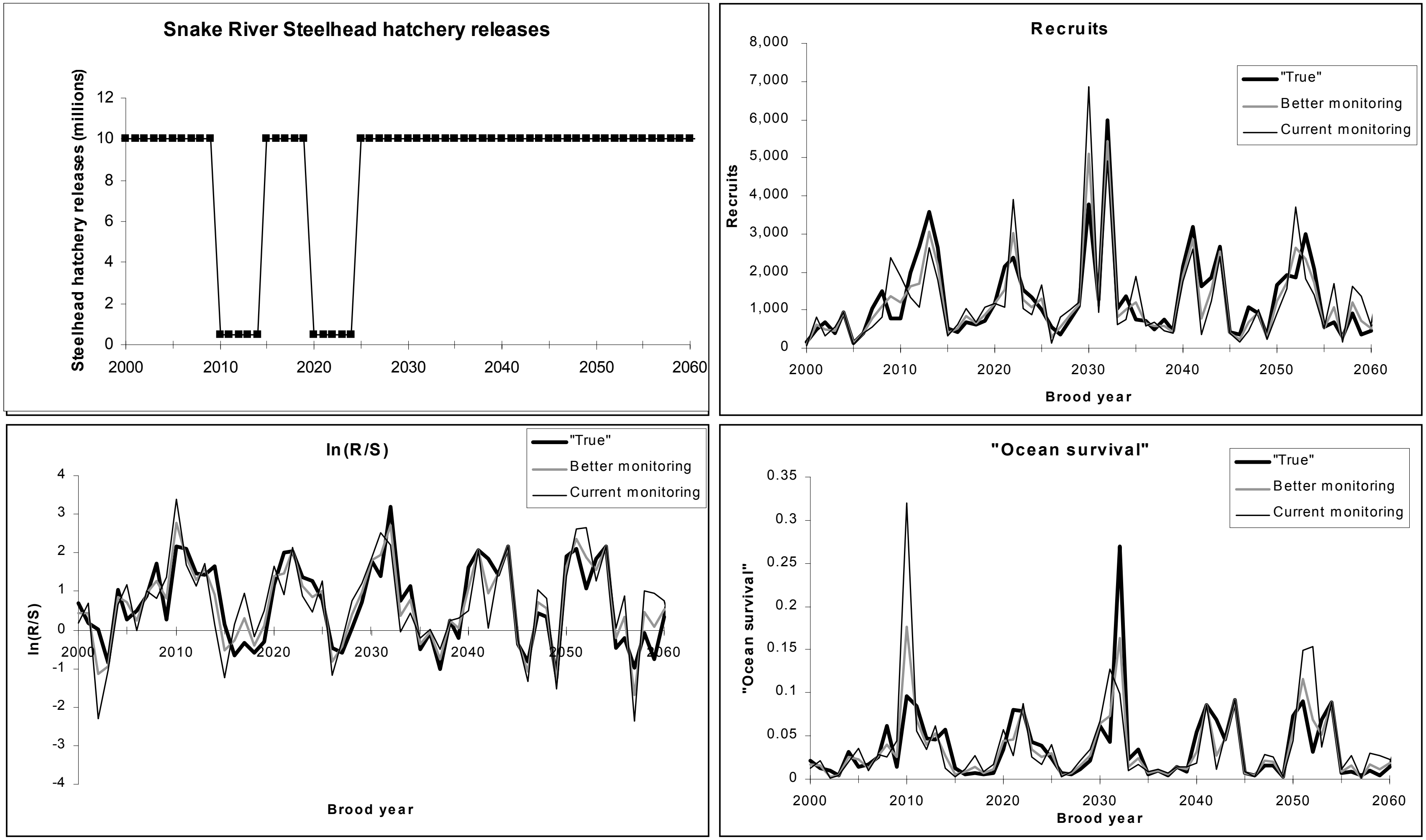

Figure 3.7-2: Simulation of expected results for hatchery Experiment 1 from Peters (1999). “Better monitoring” refers to a sensitivity to reducing random error in simulations to 50% of base value.

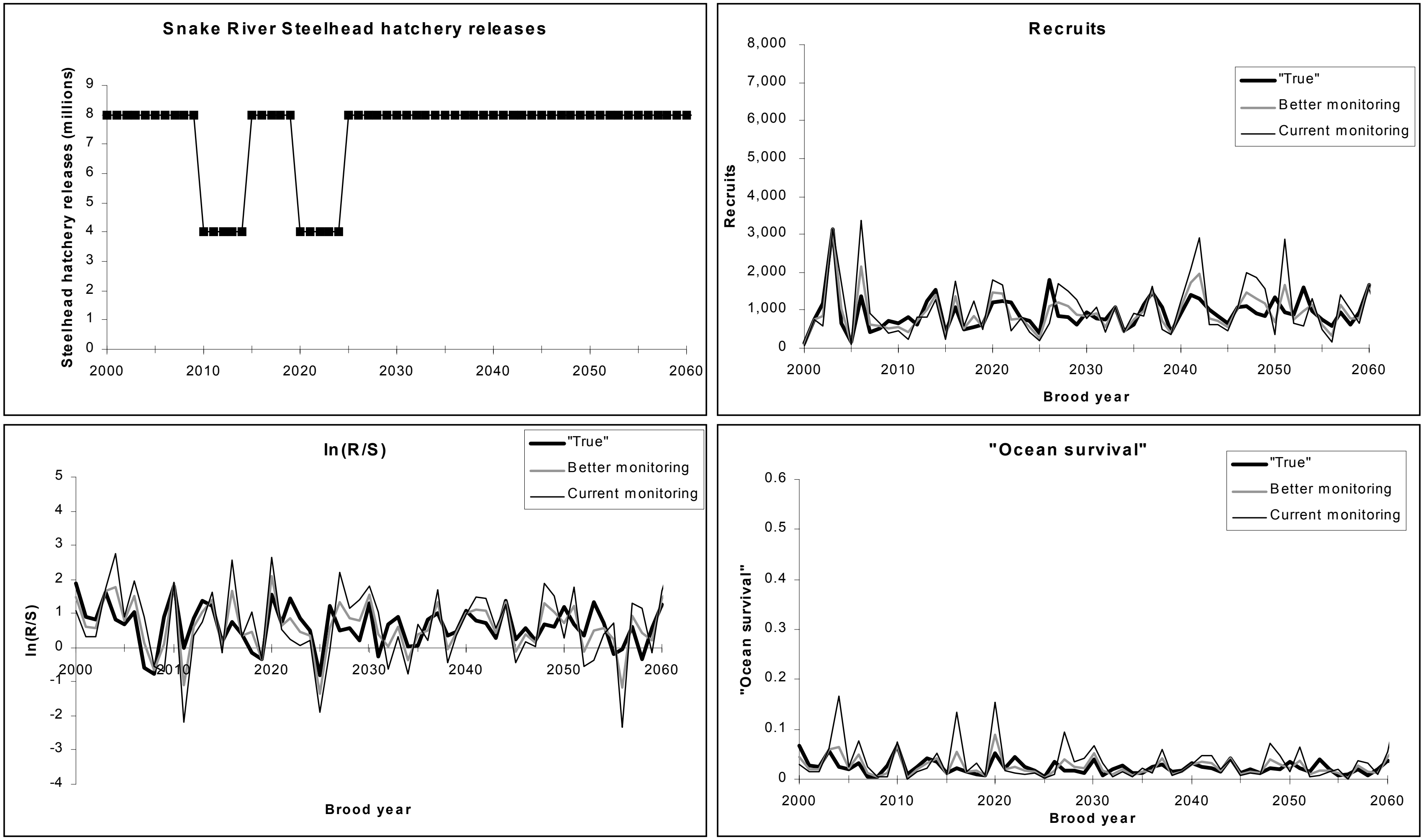

Figure 3.7-3: Simulation of expected results for hatchery Experiment 2 from Peters (1999). “Better monitoring” refers to a sensitivity to reducing random error in simulations to 50% of base value.

3.7.3 Benefits, Risks, Costs and Trade-offs

Benefits and Amount of Learning Possible

As has been noted in the review of the Weight of Evidence Report by the SRP, the effects of alternative causative factors on extra morality of spring/summer chinook are confounded in time. Evaluating the contribution of increased hatchery production to extra mortality of spring/summer chinook would clarify the long-term response of these populations to alternative management actions. The results of such a study could help determine which combinations of hydropower actions and hatchery management scenarios are most likely to result in achieving recovery goals for Snake River spring/summer chinook. Given the possibility that increased hatchery production has contributed to increased mortality of outmigrating spring/summer chinook, pairing experimental hatchery studies with whichever hydropower strategy is chosen would allow managers to adjust programs in the future to achieve rebuilding goals.

Risks to Stocks

The goal and cap for hatchery production of all stocks, as part of the Lower Snake Compensation Plan, is 20 million. The goal for steelhead is about 14 million. However, current programs are producing about 12+ million smolts per year because of limits imposed by ESA biological opinions and current fish culture practices. Because the production goal has rarely been met and because it is the number of fish released (not the production goal) that determines the potential for negative interaction, this paper focuses on potential hatchery reductions in terms of recent year releases (not production). Steelhead are produced at USFWS, ACOE, and Idaho Power facilities.

Steelhead releases in the Snake River in 1998 totaled 12.2 million. Approximately 3 million of these releases were used for conservation and/or restoration. ‘Conservation’ is defined here as programs based on native or local stocks, and ‘local’ means part of an ESU. Based on recent release numbers, this leaves a possible maximum reduction in hatchery steelhead releases of 9.22 million from all hatcheries combined without impacting conservation/restoration programs. Options for reducing exposure of spring/summer chinook to steelhead smolts should take into account the desire of maintaining hatchery program broodstocks to allow for rapid return to levels consistent with mitigation responsibilities. Estimating the degree to which reductions could occur before the otherwise successful steelhead hatcheries would become crippled is a tenuous proposition. There would also be some institutional resistance, and possibly legal barriers, to reducing the effectiveness of steelhead hatcheries.

A high-contrast experiment such as Hypothetical Experiment 1 would involve a special set of practical constraints. Also, the 20-fold difference in treatments (going from 500,000 to 10 million) would be difficult, if not impossible, to achieve, based on past experiences with the time required to build hatchery production capability.

Costs

Manipulating or reducing a major portion of the current hatchery production of steelhead in the Snake River basin would have substantial costs, both monetarily and in terms of risks to the future hatchery production program. To the extent it is feasible to reduce interactions by delaying releases at major facilities, additional manpower and feeding costs would be incurred. Other production programs at those hatcheries might be negatively affected by the need to allocate rearing space to steelhead for longer period of time. Post-release survival of steelhead smolts may be impacted by delayed releases. If it were necessary to reduce programs for some period of time to implement the experiment, additional costs of mothballing facilities and programs would be incurred. As described above, if high-contrast experiments required production above current levels, this would also result in significant additional costs.

If we discovered that reductions in hatchery fish were insufficient for recovery or detrimental to conservation efforts and the hatcheries were turned back on, there would be biological limitations and considerable costs. Historically, the length of time it took for the LWSCP hatcheries to meet their production goals varied dramatically across the various hatcheries and depended on the survival rate and return of adults, water temperature limitations, disease problems etc. For example, the first LSRCP facility (Grande Ronde R. basin) was completed in 1978, but the hatchery did not meet its production goal of 1.35 million until 1986, 8 years later. In addition, if hatcheries were turned back on under the condition that local stocks be used for broodstock, it is unlikely that the composition of returning adults would be appropriate to support the broodstock needs of the supplementation program for many years. Costs would include re-hiring or transferring staff back, taking the hatcheries out of moth-ball or maintenance mode, and running hatcheries at minimal production until broodstock could be built back up. Turning hatcheries back on could take from 5 to 10 years, depending on broodstock requirements and those factors listed above.

Tradeoffs

Under a scenario of status quo management of the hydrosystem, where transportation is maximized, reduced hatchery releases may have the potential to increase spring chinook returns. It is extremely important to note, however, that this option would require consensus from all those groups involved in Columbia River Fishery Management Plan renegotiations, may require congressional approval if the Lower Snake Compensation Plan Act and USFWS treaty trust responsibilities are violated, and would likely result in a substantial reduction in state and tribal fisheries. Thus, this scenario has complex management implications that require consideration of fishery regulations and treaty rights and how these may be affected by reductions in hatchery steelhead.

There may be alternatives to conducting the proposed experimental management action. Although limited, some data are available that would allow comparisons of spring chinook survival (SARs) during periods when densities of hatchery fish are high and when they are low. Data presented to date indicate SARs increase during periods of low density but also indicate SARs are still low during these periods (well below two percent). It is possible that additional analyses of existing information would remove the uncertainty regarding the influence of hatchery production on total extra mortality (including D). Some PATH members believe that additional analyses of available data should be conducted to assess the potential benefits of reduced hatchery production to spring chinook prior to implementing reductions that could harm affected hatchery stocks. Others believe that because of the confounding effects described in previous documents (e.g., August 1998 PATH Weight of Evidence report), it will not be possible to resolve this question without some form of management experiment. SRP comments also appear to support the second opinion.

The stock viability hypothesis, with hatcheries as a causal factor, has only been proposed to explain patterns of extra mortality in Snake River spring/summer chinook salmon. There would be little opportunity for improving fall chinook survival as a result of this experiment because there is little overlap in time between outmigration of falls and spring chinook and steelhead. The potential for improving other stocks such as sockeye and coho has not been assessed. *Note: this is also true of most of the other proposed experimental management actions*.

3.7.4 Inferences

Table 3.7-2: Example inference table for Hatchery-Caused Stock Viability Extra Mortality Hypothesis.

Variable	Observation and Inference	
	Observations consistent with "SV-Hatchery"	**Observations not consistent with "SV-Hatchery"**
$V_{n,}$	Must be monitored to concurrently estimate D, but not directly relevant to testing extra mortality hypothesis. Same with transport and non-transport SARs.	
SAR or RRS	Higher in reduced hatchery production years than in higher production years.	No difference among treatments, or lower in higher production years
λ_n	If possible to infer from estimate of RES and D; should go down in reduced hatchery production years	No change among treatments or goes up in reduced hatchery production years

3.7.5 Confounding Factors

As described previously, it is likely that hydro and hatchery factors that may be responsible for extra mortality are confounded. If changes to the hydrosystem are being made between treatments, these may confound results, especially if there are few replicated treatment blocks.

3.7.6 Practical Constraints

These were described in detail in Section 3.7.3.

3.8A. Reduce the Effects of American Shad on anadromous salmonids in the Lower Columbia River

3.8A.1 Description of Experimental Action / Research & Monitoring

Rationale

Study Objective: Estimate and reduce the effect of American shad on survival of juvenile salmonids in the lower Columbia River.

Description of Hypothesis: Anadromous American shad *Alosa sapidissima* were introduced into the Sacramento River in California in 1871, migrated north along the Pacific coast, and became well established in the Columbia River by 1885 (Smith 1896). The number of adult shad passing Bonneville Dam on the lower Columbia River has increased dramatically in the last 60 years, from an average of 16,700 adults between 1938-1957 to over 2,000,000 adults between 1988-1992 (Chapman et al. 1991; Quinn and Adams 1996). The number of adult American shad entering the lower Columbia River (Bonneville Dam) has recently been 2-3 times the total number of adult Pacific salmon. The dramatic increase in success of adult shad coincided quite closely with construction of dams on the lower Columbia and Snake rivers (Figure 3.8A-1).

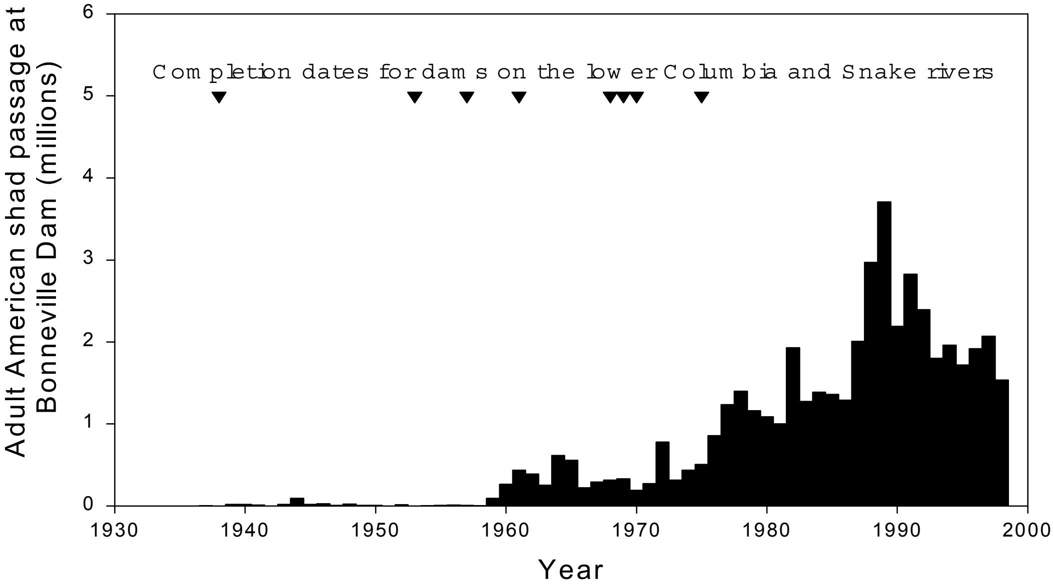

Figure 3.8A-1: Passage of adult American Shad past Bonneville Dam and construction dates for dams on the lower Columbia and Snake rivers.

Adult shad migrate into the Columbia River during May through July and adults spawn soon after migration. Incubation of eggs is 3–8 days and juvenile shad rear in the mainstem Columbia and Snake rivers during August through late fall, when they migrate to the ocean. American shad may affect juvenile and adult salmonids through several mechanisms:

Mechanism/Hypothesis #1 – Direct competition

The high abundance of juvenile shad and possible diet overlap with juvenile fall chinook salmon indicate a potential competitive interaction. Adult American shad ascend the Columbia River to spawn from May through July, with most spawning above Bonneville Dam occurring between John Day Dam and the confluence of the Snake and Columbia rivers (Quinn and Adams 1996). In that reach, the presence of high numbers of larval and juvenile American shad coincides with the early August median passage dates at McNary Dam of emigrating wild, juvenile fall chinook salmon from the Snake River based on PIT tag detections. Furthermore, this spatial and temporal overlap is not limited to the reservoirs. It is estimated that at least 600 million juvenile shad enter the Columbia River estuary annually to feed and grow. This rearing also overlaps with the rearing of juvenile fall chinook salmon in the estuary (Chapman et al. 1991).

It is well established that planktivorous fishes, such as American shad, can alter the abundance and size structure of zooplankton resources. Larval and juvenile American shad are effective planktivores that feed predominantly on crustacean zooplankton (Crecco and Blake 1983). Preliminary studies suggest that juvenile American shad in the Columbia River estuary feed on amphipods, calanoid copepods, cladocerans and insects (Dirkin et al. 1979). Rondorf et al. (1990) found that zooplankton (mostly

Daphnia spp.) were a primary component of subyearling chinook salmon diets in reservoir habitats. If rearing American shad greatly reduce or alter zooplankton abundance and community structure, then reservoir food webs may be inadequate to support emigrating juvenile fall chinook salmon. Furthermore, there may be significant overlap with the diet of fall chinook salmon through fall and winter in the estuary (McCabe et al. 1983).

Older juvenile shad (1+ and 2+ year olds) are found in the Columbia River estuary during spring and summer coincident with juvenile salmonids and their diets are similar (Hamman 198?, McCabe et al. 1983). However, the number of older juvenile shad found in the estuary is small compared to other nonsalmonid species.

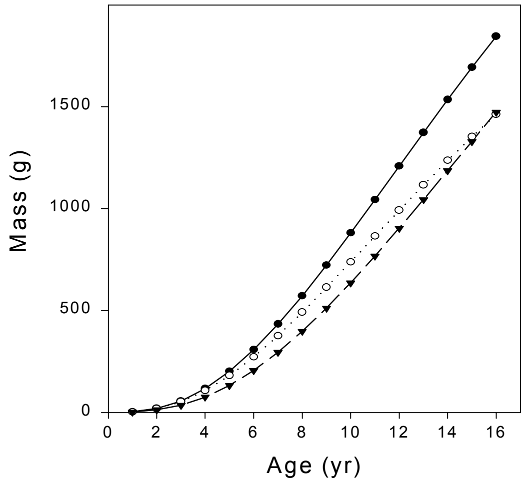

Figure 3.8A-2: Mass-at-age of female northern pikeminnow collected below Bonneville Dam (•), between Bonneville and McNary dam (O), and in the lower Snake River reservoirs (▼). Mass was predicted from von Bertalanffy growth models fit by Parker et al. (1995).

Mechanism/Hypothesis #2 – Juvenile American shad supplement predator diet

Juvenile American shad are very abundant during late summer and fall throughout the lower Columbia River, and juvenile shad are thus a potential source of food for resident predators on juvenile salmon such as northern pikeminnow. Juvenile shad could be supplementing the diet of salmonid predators, producing faster rates of growth, larger predators at a given age, and larger predators are known to consume more salmonids (Vigg et al. 1991; Poe et al. 1991). Predators, particularly northern pikeminnow, consume significant numbers of juvenile salmon (Rieman et al. 1991) and predation is the primary mechanism of mortality in passage models used in PATH studies (Marmorek et al.)

Length-at-age and weight-at-age plots suggest that female northern pikeminnow are considerably larger at a given age in the lower Columbia River compared to reaches further upriver (Figure 3.8A-2). For example, age 10 fish from below Bonneville dam were 421 mm and 881 g, whereas age 10 fish from the lower Snake River reservoirs were 374 mm and 636 g (Figure 3.8A-2). Female northern pikeminnow lower Columbia River were predicted to be from 52% (age 5) to 25% (age 16) larger in mass than similar aged fish from the lower Snake River reservoirs. The size of female fish from the lower Columbia River reservoirs was intermediate between the predicted sizes below Bonneville Dam and above Ice Harbor Dam (Figure 3.8A-2). It would be necessary to increase the growth rate parameter K in the von

Bertalanffy equation by about 18% for northern pikeminnow in the lower Snake River to show the same mass-at-age as the lower Columbia River fish. Male northern pikeminnow were smaller in the lower Snake River reservoirs than fish in the lower Columbia River, although there was little difference in size-at-age for fish collected throughout the Columbia River (lower river and reservoirs).

Relatively little is known about the seasonal growth pattern of northern pikeminnow. Tagged individuals that were recaptured within the same reservoir and year showed zero or slightly negative growth rates during spring and summer (Table 3.8A-1; Petersen and Ward in press). Based on these observations and using a bioenergetics model, Petersen and Ward (in press) concluded that most growth by northern pikeminnow in the Columbia River probably occurs in late summer and fall. They predicted that the specific growth rate for northern pikeminnow in John Day Reservoir increased in early summer to a peak in early July, declined in August because of relatively high water temperatures, and then increased again to a second peak in early October as temperatures cooled.

Table 3.8A-1: Specific growth rates of northern pikeminnow in John Day Reservoir (1983-86) based on mark and recapture data of individual fish. From Petersen and Ward (in press).

Period	**Growth rate (*SE*) (% • d^{-1})**	***N***	**Probability that growth rate = 0**
April-May	-0.14 (0.12)	27	0.25
June-July	-0.77 (0.19)	20	<0.001
August	-0.49 (0.12)	29	<0.001

The diet of northern pikeminnow during fall often includes a high proportion of juvenile American shad. Based on a four-year study in John Day Reservoir, Poe et al. (1991) found that northern pikeminnow "switched to nonsalmonid fishes — primarily prickly sculpin and American shad" during August. Petersen et al. (1994) sampled northern pikeminnow below Bonneville Dam in late August and early September of 1990-91. During four nights of sampling in 1991, juvenile American shad were about 5% of the diet of predators (*n* = 127 to 350 per night), however, during two nights sampled in 1990, juvenile shad were 78% of the diet (*n* = 253 and 398). The high percentage of shad in the diet during 1990 coincided with high passage indices of shad at Bonneville Dam, while the low diet percentage observed during 1991 occurred when shad passage was relatively low (Petersen et al. 1994).

Bioenergetic modeling (J. H. Petersen, USGS, unpublished results) suggests that 20–40% of juvenile shad in the diet of northern pikeminnow can produce significantly faster growth during fall months. Juvenile shad have a relatively high energy density compared to crayfish or other fishes, which they likely replace in the predator's diet. The larger size-at-age of northern pikeminnow in the lower Columbia River could thus be explained by invasion and proliferation of American shad in the system.

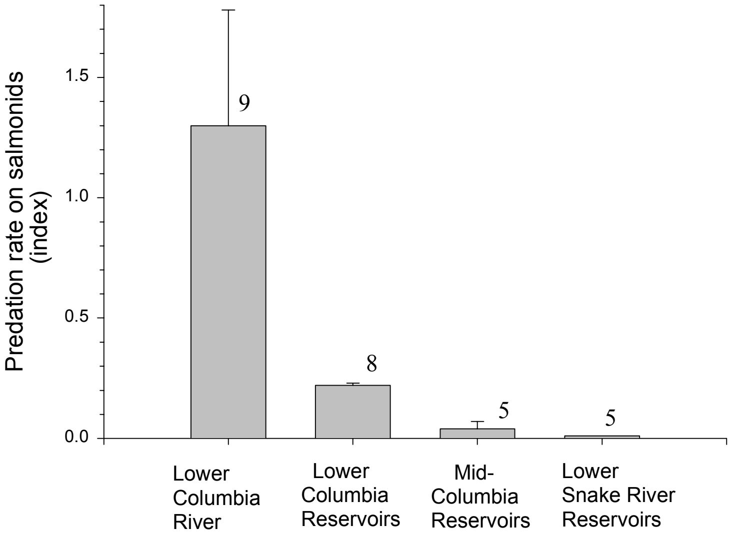

Figure 3.8A-3: Average index (± 1 SE) of predation on juvenile salmon by northern pikeminnow during summer for different reaches of the lower Snake and Columbia rivers. The index is approximately equal to the rate of predation (prey per predator per day). Averages do not include samples collected in dam forebays or tailraces. Numbers above the bars are sample sizes. Data are from Ward et al. (1995) and USGS (unpublished).

Predation rates on juvenile salmonids by northern pikeminnow is several times higher in the reach below Bonneville Dam than in the lower Columbia River reservoirs or in the lower Snake River reservoirs (Figure 3.8A-3). These higher rates of predation may cause higher mortality to juvenile salmonids in the lower Columbia River, which would be consistent with PATH conclusions concerning the “extra mortality” hypothesis.

Mechanism/Hypothesis #3 – Interference with adult salmon at fish ladders

There is some concern about the effects of adult shad on adult salmonids due to their interaction in fish ladders at mainstem dams where large numbers of migrating shad accumulate (*citation*). The high numbers of returning adult American shad in fish ladders may cause avoidance or delay in the return of adult salmon. The adult shad migration peaks from mid June to late July, and coincides with adult return migrations of sockeye and summer chinook salmon. One solution to adult passage problems has been to modify passage configuration so that adult shad can readily pass and not accumulate in the ladders. Ironically, such passage improvements have extended the range of American shad to Priest Rapids Dam on the Columbia River, and above Lower Granite Dam on the Snake River (Monk et al. 1989). We speculate that the consequences of opening access to such large reaches of spawning and rearing habitat suitable to American shad may not be fully realized at this time.

Experimental Action:

Reduce the effects of introduced American shad on juvenile and adult salmonids.

Block or limit the upstream passage of adult American shad at Bonneville Dam or dams further upriver, to reduce the abundance of juvenile shad in the system. Modifications should be relatively simple, and could include elimination of overfall weirs in sections of adult fish ladders (similar to their original configuration prior to modification of regulating sections to allow passage of shad; Monk et al. 1989), or the use of specially designed overfall weirs to attract shad into terminal capture areas. At least one year of research would be required to determine how best to modify the fish ladders to exclude adult shad. Harvest offers another alternative to reduce the number of adult American shad, but it has been generally under-utilized. For example, between 1977-97 the commercial harvest rate has ranged as high as 8%, but was only 1% in 1997.

Spatial and Temporal Components

The adult fish ladder modification could be reversible so that adult shad could pass the dams in alternate years. However, since we believe the interactions between shad and northern pikeminnow are not acute, but rather have developed over many years, a long-term reduction in adult shad above Bonneville Dam would most likely provide a stronger signal.

3.8A.2 Monitoring Approach

Variables to Monitor

The effects of American shad on juvenile salmonid survival may be indirect, so it may be necessary to monitor auxiliary variables to confirm the effects of the experiment. Variables monitored may also depend somewhat on the assumed mechanisms by which shad affect salmonids: via predator growth rates, etc.

- Measure the growth rate and size-at-age for predators within the lower Columbia River where American shad are being blocked. The ongoing Northern Pikeminnow Management Program will collect much of these data. We would predict that growth rate and size-at-age would decline with distance upriver, since there would be a decline in the availability of shad juveniles.
- Changes in diet of resident predators such as northern pikeminnow.

Duration and Intensity of Monitoring

Studies would need to be conducted for several years to detect a change since the mechanism may operate through the growth rate and size structure of the predator population.

Sample size analyses (e.g.)

- Number of fish needed to detect a change in the von Bertalanffy parameters
- Number of fish needed to detect diet change in predators
- Sampling needed to detect changes in the density of juvenile shad in reservoirs

3.8A.3 Benefits, Risks, Costs, and Trade-offs

Benefits and Amount of Learning Possible

Since American shad are an introduced species, reducing their abundance in a portion of their current range would seem prudent. At present, they provide limited benefit for commercial and recreational fishermen above Bonneville Dam. Furthermore, any modification that would reduce their numbers would be reversible.

Risks to Stocks

Modifications to fish ladders could cause some interference with migrating adult salmonids, however, additional research on adult shad passage prior to ladder modification and advice from Atlantic coast researchers experienced with shad passage would reduce potential interference.

Costs

Major costs would be modification of fish ladders at lower Columbia River dams.

Trade-offs

3.8A.4 Inferences

What could you learn? Identification of a direct or indirect mechanism that might regulate growth or survival of juvenile salmon in the lower reservoirs. Applications for regulating the upriver migration of adult shad.

3.8A.5 Confounding Factors

- Climate conditions.
- Hatchery effects. Increased numbers of hatchery fish in the lower river following ladder modifications could supplement the diet of downriver predators.

3.8A.6 Practical Constraints

None that are unique to this experiment.

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3.8B Reduce the Abundance and Effects of Non-native Predators in the Lower Snake River

3.8B.1 Description of Experimental Action / Research & Monitoring

Rationale

Study Objective: Estimate and reduce the effect of non-native predators (channel catfish, smallmouth bass, etc.) in the lower Snake River.

Description of Hypothesis: Predator-prey dynamics are important in the Columbia River system since dam passage and predation are assumed to be the primary causes of mortality for juvenile salmonids (Raymond 1979; Rieman et al. 1991). Impoundment has presumably created habitat conditions that have enabled native and introduced predator populations to increase (Poe et al. 1994*),* leading to increased predation on smolts compared to historical river conditions. Northern pikeminnow *Ptychocheilus oregonensis*, smallmouth bass *Micropterus dolomieu*, and walleye *Stizostedion vitreum* consumed 7-14% of juvenile salmonids that migrated through John Day Reservoir on the lower Columbia River during a year, with northern pikeminnow accounting for ≈78% of the total loss (Rieman et al. 1991). Predation rates on juvenile salmon by northern pikeminnow were especially high near dams on the Columbia and Snake rivers (Vigg et al. 1991; Ward et al. 1995), although the total loss of salmonids may be quite high in mid-reservoir areas because of the long residence time of the smolts and the large predator populations (Beamesderfer et al. 1990; Rieman et al. 1991; Petersen 1994).

Along with northern pikeminnow, large populations of non-native predators occur in the lower Snake River and these fish may be consuming a high proportion of migrating salmonids (Bennett et al. 1983; Ward et al. 1995; Petersen et al. 1999). Smallmouth bass density ranges from about 160 fish per river kilometer (fish/rkm) in Lower Monumental reservoir (D. Ward, Oregon Department of Fish and Wildlife, pers. comm.) to >800 fish/rkm in the forebay of Lower Granite Reservoir (Bennett and Naughton 1998). Petersen et al. (1999) estimated that predation by northern pikeminnow and smallmouth bass in the reservoirs of the lower Snake River caused about 1% mortality on spring migrants and about 59% mortality on fall chinook salmon migrants. These mortality estimates were similar to NMFS PIT-tag estimates, after adding expected dam mortality in the reach. Smallmouth bass were predicted to cause about 54% and 82% of predation-related mortality during spring (April-May) and summer (June-August), respectively (Petersen et al. 1999).

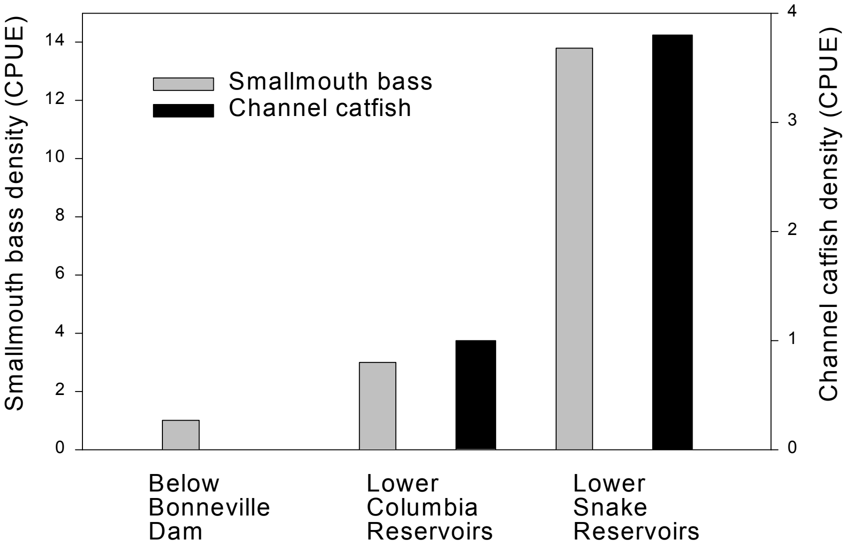

Figure 3.8B-1: Relative density of smallmouth bass and channel catfish in different river reaches. Smallmouth bass density is the number of fish captured during a 15-min electrofishing run. Channel catfish density is the number of fish collected in a bottom gillnet fished for 1-h. Data are from Zimmerman and Parker (1995).

Aside from smallmouth bass, several other non-native predators have become abundant in the impounded lower Snake River (Bennett report; Zimmerman and Parker 1995). Population or density estimates are not available for many of these species, but their abundance may be quite high. Zimmerman and Parker (1995) estimated relative density and abundance of smallmouth bass, channel catfish *Ictalurus punctatus*, and walleye throughout the lower Columbia and Snake rivers. The density of smallmouth bass and channel catfish increased by about one order of magnitude between the reach below Bonneville Dam compared to the lower Snake River reservoirs (Figure 3.8B-1).

The abundance of non-native species may be increasing because of suitable lentic habitat created by impoundment. For example, channel catfish, smallmouth bass, and yellow perch *Perca flavescens* counts at Little Goose Dam appear to have increased during the last decade (Figure 3.8B-2). The relative abundance of some native species, on the other hand, have remained relatively constant or declined slightly, with the exception of peamouth *Mylocheilus caurinus* (Figure 3.8B-2). The numbers of northern pikeminnow counted at Little Goose Dam has declined from 1,110 in 1990 to 31 in 1997.

Channel catfish may be particularly important in predator-prey dynamics of the lower Snake River because of their relatively high abundance and their potential predation on juvenile salmonids (Bennett et al. 1983; Poe et al. 1991; Vigg et al. 1991; McMichael et al. 1999). Bennett et al. (1983) studied feeding and growth of channel catfish in the lower Snake River. Channel catfish were as large as 810 mm and had a maximum age of 16 years. Of 83 channel catfish collected in Little Goose Reservoir, the percentage salmonids in the diet ranged from 73% (shoal area) to 86% (tailwater), measured as a volumetric occurrence (Bennett et al. 1983). Channel catfish are suspected to be important predators on juvenile salmonids in other parts of the Columbia River system. In John Day Reservoir, juvenile salmonids were 60% of the diet of channel catfish in McNary Dam tailrace (Poe et al. 1991) and predation rates on salmonids were similar to rates measured for northern pikeminnow (Vigg et al. 1991). Total loss due to channel catfish was not estimated because of the difficulty in making population estimates (Rieman et al. 1991). In the Yakima River, McMichael et al. (1999) also had difficulty estimating the size of the channel catfish population, but their crude estimates suggested very high densities for 1997 and 1998. Juvenile salmonids were 66% of the diet of channel catfish (N = 121) in the lower Yakima River during 1998 (McMichael et al. 1999).

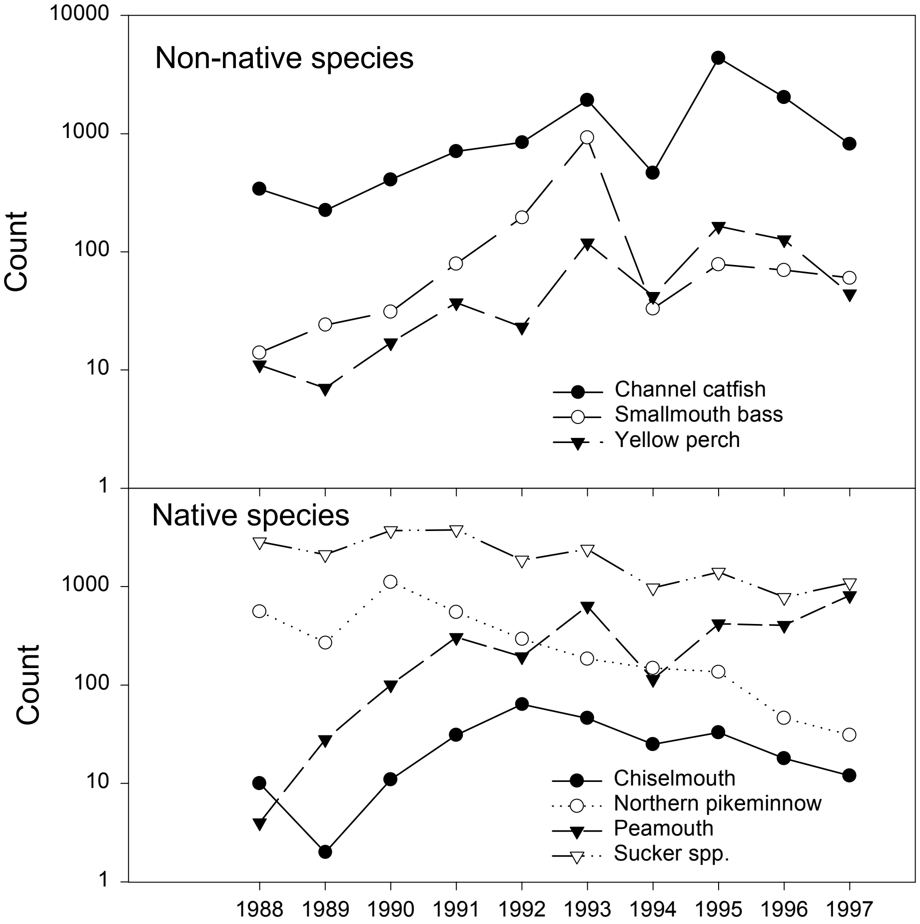

Figure 3.8B-2: Total counts of adult fish passing the collection system at Little Goose Dam, Snake River, during 1988-97. The top panel has selected non-native species and the bottom panel has selected native species. No adjustments to counts have been made for annual variations in spill or facility modifications, such as addition of extended length screens. Data are from Rex Baxter, ACOE.

Experimental Action: Reduce the number of predatory fishes in the lower Snake River, and thus increase survival through this reach for anadromous juvenile salmonids. Means of reducing predators might include intensive fishing (commercial and/or sport) and collection at dams in the lower Snake River. Adult fish are often observed at the separator in collection facilities (R. Baxter, ACOE, personal comm.), and these might be easily removed.

Spatial and Temporal Components

Exploitation of predators would occur in forebay, tailrace, and reservoir portions of the four lower Snake River impoundments. The objective would be to reduce the total number of predators and also to remove the largest individuals in the population, which likely cause a disproportionate amount of mortality on salmonids (Beamesderfer et al. 1990; Poe et al. 1991).

3.8B.2 Monitoring Approach

Variables to Monitor

- Estimate survival of PIT-tagged juvenile salmonids through the lower Snake River reservoirs. Groups of tagged fish could be released at the upper end of Lower Granite reservoir and survival would be measured to Ice Harbor Dam using existing PIT-tag detector systems. Survival estimates would be similar to those being conducted by NMFS. Survival rates should increase through time (years) if non-native predators are causing significant mortality.
- Consider variables being monitored by the Northern Pikeminnow Management Program – size structure of populations, density, predation rates, exploitation rates, diets, …

Duration and Intensity of Monitoring

Survival studies would need to be conducted for several years to detect changes in survival rate (see Section 3.2) or predator populations.

Sample size analyses
Sample sizes necessary for PIT tag survival studies (see Section 3.2, for example).

3.8B.3 Benefits, Risks, Costs, and Trade-offs

Benefits and Amount of Learning Possible

Risks to Stocks

Several commercial fishing techniques that might be used, such as gill netting or trawling, could capture adult salmonids of endangered or threatened species. Careful management of techniques or fishing seasons would be necessary. By-catch of other species such as white sturgeon might also be a problem.

Compensatory responses by un-exploited fish populations, especially species that could be potential predators on salmonids, might reduce the effectiveness of this action or even increase predation mortality. Compensatory responses could occur through feeding, growth, or reproductive rates (Jude et al. 1987; Saila et al. 1987; Meronek et al. 1996*)*. The Northern Pikeminnow Management Program, for example, has conducted extensive field studies to determine if compensation has occurred as a result of the northern pikeminnow bounty fishery (Beamesderfer et al. 1996). A strong response by yellow perch to removal of smallmouth bass, for example, might lead to an increase in the yellow perch population size or the frequency with which yellow perch consume juvenile salmonids. Removal, if applied as a long-term solution, would need to be continued perhaps indefinitely, otherwise populations would recover.

Costs

Major costs would include hiring commercial fishermen, supplementing fishing gear needed to catch predator species, and perhaps disposing of catch if there was no market.

Trade-offs

Implementation and potential effects might require several years, thus putting imperiled stocks at additional risk. This trade-off would, however, be similar to most other management experiments that would require several years to implement and evaluate (dam breaching, for example).

Reduction in populations of some non-native species, such as smallmouth bass, would decrease this sport fishery in the lower Snake River.

3.8B.4 Inferences

What could you learn? -- Are predators the major source of mortality on juvenile salmonids in the lower Snake River? Can populations of predators be effectively reduced and managed? And, does reduction cause an increase in survival of juvenile salmonids? If carried on for a long enough period, does reduction lead to an increase in adult returns?

3.8B.5 Confounding Factors

- Climate conditions.
- Other management actions specific to the lower Snake River, such as dam breaching.

3.8B.6 Practical Constraints

States regulate commercial and sport fishing, and special consideration would be needed from these agencies.

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3.9A Explore Possible Mechanisms for Delayed Mortality

Sections 3.9A and 3.9B describes different *research* approaches to extra mortality hypotheses. While these are not *experimental management* approaches, they could be combined with other experimental management actions (e.g., changes to the hydrosystem or hatchery operations).

Study Objective: Explore possible mechanisms for delayed mortality.

Description of Hypothesis: Delayed mortality below Bonneville Dam for transported and non transported smolts are important factors in the PATH decision analysis. However, little empirical data exists on delayed mortality below Bonneville Dam. Determining whether migrational experience within the hydropower system affects delayed mortality or whether there are inherent differences within stocks would provide important information.

Experimental Action: Collect PIT-tagged fish that have had different migration histories at a downstream dam (Bonneville or John Day) using the sort-by-code system and rear in saltwater for an extended period to explore possible mechanisms for delayed mortality.

For this experiment, PIT-tagged spring/summer chinook salmon will be collected at a downstream dam (Bonneville or John Day) using the sort-by-code system and reared for an extended period in a state-of-the-art salt water rearing facility. Target fish will be spring/summer chinook salmon that have had different passage histories including bypassed varying number of times, never detected in the system prior to capture, and transported by barge (sampled at release below Bonneville Dam). PIT-tagged spring/summer chinook salmon originating from different basins (i.e., Snake, John Day, and Yakima Rivers) would also be collected. Mortalities will be necropsied to determine cause of death and their PIT tag codes read to determine fish origin and migration history. Fish will also be sampled for physiological profiles to determine level of smoltification.

3.9A.1 Spatial and Temporal Components

Smolts from throughout the Columbia River Basin upstream from Bonneville Dam representing different stocks with a variety of migrational experiences will be collected and reared in saltwater for this study. The length of the saltwater rearing period will depend on survival rates; higher survival among groups will require longer rearing. The study should be repeated for several years.

3.9A.2 Monitoring Approach

Variables to Monitor

The sort-by-code system look-up table would be programmed to collect fish from different stocks and migrational experiences with the proportion collected from each group dependent on their availability (bypassed fish would be more common than never detected). From each group, a physiological (length/weight, stress indicators, lipid reserves, gill ATPase, etc.) and disease (BKD) profile would be developed prior to entering saltwater. Mortalities would be necropsied to determine cause of death. Survival rates would be compared among groups.

3.9A.3 Benefits, Risks, Costs, and Trade-offs

Uncertainty exists about the extent and cause of delayed mortality for both transported and non transported spring/summer chinook salmon. Reducing this uncertainty will be beneficial to the region in making informed decisions on operation of the hydropower system. If this experiment is conducted concurrently with other experiments with the hydropower system in place, there will be little additional risk to stocks since sample sizes needed will be relatively small.

3.9A.4 Inferences

The results of this study will show whether passage history or fish origin result in differential delayed mortality during simulated saltwater entry. If there are inherent differences in stock viability or differences in viability caused by migration experience, then survival should vary among groups. If they are the same, then differences in delayed mortality are more likely related to estuarine or early ocean entry timing.

3.9A.5 Confounding Factors

Confounding factors would include mortality caused by the rearing system unrelated to fish origin or migrational experience (i.e., disease outbreak). This factor will be controlled through the use of an ultraviolet filtering system and biological filters.

3.9A.6 Practical Constraints

Collecting sufficient numbers of PIT-tagged smolts from some groups may not be possible. However, the main groups of interest (transported and non transported) should not be difficult. The number of PIT-tagged fish released for particular groups could be increased for groups with expected low recapture rates.

3.9B Characterizing Delayed Mortality

3.9B.1 The Hypotheses

Underlying the hydro hypothesis for extra mortality and delayed mortality are the assumptions of mechanisms in which hydrosystem passage experience affects the following life stage survivals. The general mechanism for these hypotheses is an event occurs in one life stage that results in increased mortality in another life stage. Assessing coupling between experience in one stage and mortality in another is problematic. Here an approach that may identify this coupling is proposed.

A hypothesis is that stress in passage affects the survival of the fish in the estuary and ocean. Furthermore, the stress depends on the particular passage route and although the effects are not directly evident in terms of survival it may be reflected in changes in the fish predator avoidance and escape behaviors. Therefore, the passage route stress and the relative effect on life cycle survival may be characterized in terms of alterations of behavior after a fish experiences a particular passage route event.

Conceptually then stress in one life stage affects the survival in the following life stage and mechanistic linkage is through an alteration of behavior in one life stage or time that alters behavior in the next life or time.

Behavior

The approach to linking stress and mortality between life stages is through changes in the predator response of fish. This trans-lifestage effects hypothesis is based on the following observations:

1. Rate of predation is dependent on predator avoidance and escape behaviors;
2. Predator response behaviors are affected by fish physiological and motivational states; and
3. Changes in fish physiological and motivational states, as a result of an event, are transitory such that the fish eventually returns to a pre-event state.

Antipredator behaviors show a large range of traits but these can be categorized into two basic types: whether they act before (avoidance behavior) or after a predator encounter takes place (escape behavior) (Shi 1989). Avoidance behaviors minimize the encounter with predators and escape behavior is a response to an attack. These behaviors occur in a linked series.

Avoidance behaviors are particularly varied and depend on species and environmental conditions, and the state of the prey fish. The two general strategies are to avoid proximity to predators and to reduce detection. The approach is to seek low visibility habitats and to reduce activity in space and time (Smith 1997). Fish under stress typically employ avoidance behaviors.

Escape behaviors involve high-speed actions in a linked manner. The first response to a direct attack is called a C-startle response in which the fish forms a C shape within several milliseconds. This is followed by a burst speed escape in a direction set by the startle response. The following direction and distance traveled depends on a number of factors and may be varied to distract and confuse the predator. Escape actions are typically followed by avoidance behaviors (Shi 1989, Godin 1997).

The exact series of linked behaviors depends on the fish's learned response, their physiological condition, and their genetic predisposition to predators. Furthermore, the responses are temporally varying. Fish exhibit different levels of avoidance response depending on the length of time since the last predator encounter. A low avoidance response may result when fish have infrequent encounters with predators. In these cases fish may exhibit forging behavior in stead of anitpredator behavior. In environments with frequent predator encounters fish exhibit stronger and longer avoidance responses (Godin 1997).

The switch between the two behaviors is temporally asymmetric. A fish with a low avoidance behavior encountering a predator is likely to switch quickly into a high avoidance behavior. The return to a low avoidance behavior may occur gradually over a number of hours.

Genetic factors also affect avoidance behavior. Fish from a predator-sympatric population that are reared without experience with predators have a genotype selected by predation, but lack individual experience (Smith 1997).

Physiological condition also affects the ability of a fish to respond to predators. Such factors as low food ration, parasitism and gravidity can negatively affect physical conditions on swimming performance of fleeing prey fish (Mesa et al 1994).

Specific Hypothesis

The hypothesis to be explored is that different in passage conditions will be reflected in the measures of predator avoidance behavior of smolts and that these differences are significant to determining the delayed mortality of smolts in the life stages after the hydrosystem.

Management Objective

If a coupling between measurable behavioral responses and survival can be established the behavioral measures can be used to assess the contributions of different passage experiences and fish condition on survival. The behavioral measures can then be used to identify problem areas in passage routes and the measures can be used to infer effectiveness of passage improvements.

3.9B.2 Theoretical Basis for Relating Behavior to Survival

Relating survival to behavioral measures begins with a survival equation of the form:

$$S_d = \exp(-\,c\,T) \qquad \text{[Eq. 3.9B-1]}$$

where S_d is the delayed mortality for a particular passage route, c is a rate of mortality and it is further defined $c = a\ b$ where a is a prey's rate of encounter with predators, b is the probability of death with an encounter, and T is the exposure time to predators (Lima and Dill 1990).

Avoidance behavior determines the a parameter, and escape behavior determines the b parameter. The overall rate coefficient is expressed

$$\log(S)/T = -c \qquad \text{[Eq. 3.9B-2]}$$

Now two useful relationships can be written comparing delayed moralities (i.e., survivals) from two passage routes. Assuming that predator exposure time is the same for two passage routes, so $T_1 = T_2$, then two expressions can be written for ratio of survivals of passage routes 1 and 2. These are

$$\log(S1) / \log(S2) = c1 / c2 \qquad \text{[Eq. 3.9B-3]}$$

and

$$\log(S1 /S2) = -T (c1 + c2) \qquad \text{[Eq. 3.9B-4]}$$

The next step is to find behavioral surrogates for c such that [Eq. 3.9B-3] holds, i.e., a linear 1 to 1 relationship exists for the behavioral surrogate measure and the ratio of the log of survivals.

The most probable assumption for c is that the probability of death with a predator encounter is inversely proportional to the reaction time t of a prey to a controlled stimulus. This is expressed

$$c = g / t \qquad \text{[Eq. 3.9B-5]}$$

where g is a proportionality constant relating the predation rate c to a standardized reaction time measure t. In principle, g can be estimated from survival and behavioral data according to

$$g = -\log(S)\ t/T \qquad \text{[Eq. 3.9B-6]}$$

To test the hypothesis we require from [Eq. 3.9B-3] a relationship

$$\log(S1) / \log(S2) = t2 / t1 \qquad \text{[Eq. 3.9B-7]}$$

Applications

The ratio of two survivals from two passage routes can be expressed

$$\log(S1 /S2) = g\ (1/t2 - 1/t1) \qquad \text{[Eq. 3.9B-8]}$$

To assess the transportation effectiveness the relationship is

$$\log(D) = g\ (1/t2 - 1/t1) \qquad \text{[Eq. 3.9B-9]}$$

where t_2 is a behavioral response time of nontransported fish measured in fish collected in Bonneville Dam tailrace and t_1 is a behavioral response time of transported fish measured after they have been released from the transport barges.

To assess the delayed mortality in bypass, turbine and spill passage routes the reaction times are measured on fish collected in the tailrace after passing specific dam passage routes and S are the SAR measures for the specific passage routes.

Measures of Behavior

To characterize the change in the response time to predators we need to determine if the passage events alter the predator avoidance or predator escape behaviors. Both measures can be evaluated with controlled behavioral experiments. In principal though, they may be interactive, such that a fish that has a reduced escape behavior may increase its avoidance behavior. These issues will have to be resolved with experimental studies.

The two types of behavior can, in principle, be evaluated in a single experimental regime though. This involves subjecting individual fish, or small groups of fish, to a controlled stimulus. The escape behavior is characterized by the time required to escape the stimulus, t_e and the avoidance behavior is characterized by the time the fish spends hiding after experiencing the stimulus t_a.

Experiments to characterize these times can be developed to operate on fish with minimum fish handling. If the experimental procedure and relationship to survival can be established the experimental apparatus could be incorporated into PIT tag detection systems at dams or in a mobile (trawl) PIT tag detector.

3.9B.3 Benefits, Risks, and Trade-offs

The benefit of the approach is that it can potentially provide real-time information on the condition of fish and the resulting impact of their condition on delayed mortality. This information can help guide the efforts to minimize hydrosystem impacts and success of hatchery management without having to wait years to obtain results from adult returns.

The method, without complete evaluation against survival data, also has value since the ability to respond to a stimulus is likely a desirable trait in fish. Furthermore, the behavioral studies can complement the physiological studies and provide a better interpretation of the existing body of that information.

The trade-off is that the technique will take several years to develop and it is not guaranteed that meaningful results on fish survival can be obtained from measures of fish behavior.

Initiating behavioral studies to complement the ongoing studies on endangered species is likely to be productive because ultimately the linkage between the fishes' environment and their survival works through a linkage of fish behavior.

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3.10 Detecting Regime Shifts in the Climate Record and Experimental Management

3.10.1 Introduction

Numerous studies have linked shifts in some key climate indicators over the last century to salmon production in the NE Pacific Ocean. There are persistent trends in the dynamics of fish populations and climate/ocean conditions called "regimes," which are defined as a multiyear period of recruitment patterns in fish populations or as stable means in physical data series (Beamish *et al.* 1999). Key here is that the climate regime shifts are a low-frequency time scale phenomenon. In fact, linkages between climate indicators and salmon production occur at the interdecadal (regime) scale, but generally not at the interannual time scale (Francis and Hare 1994).

One of the challenges that face experimental management is the ability to separate the changes in salmon production that are due to management and those that are due to climate regime shifts. In experimental management designs it is difficult to control for climate effects because reference populations cannot be guaranteed to behave like treatment populations. This is because of the large variability in climate response among populations. Impacts of climate are observed over an aggregate of stocks and not necessarily in specific individual stocks (Beamish *et al.* 1999). Furthermore, the trends in salmon abundance are not the same for all ocean areas, rather, the impacts are regional, and in some cases, trends are inversely related (Hare *et al.* 1999). This limits our ability to design adaptive management experiments that sort out clearly the effects of climate from experimental actions.

The purpose of this paper is to determine how likely we will be able to detect a regime shift after it occurs and highlight the difficulties of confounded treatment effects with climate regime effects. One of the first problems to solve in dealing with the potential confounding of the effects of experimental management and climate regime shifts is to determine whether a regime shift has occurred during experimentation. Ideally, we will design experiments that are able to determine a treatment effect even when a climate/ocean regime shift occurs during the experiment. Since regime shifts are events that occur in the low-frequency time domain, with a period of approximately 60 years, it will be possible to tease out effects of experimental management from those of climate regime shifts if experiments are designed so that population responses are in the higher frequency time domain. The worst kind of experiment would be one that was irreversible: one which would produce a one-way trip in salmon production (a lower-frequency time domain phenomenon) and could easily be confounded with a climate regime shift (See Appendix C). Any manipulation of the ecosystem that results in low frequency responses that appear as either rapid shifts known as “sledgehammer blows” (Schindler 1987) or more gradual changes in trend should be avoided. For example, the 1976-77 regime shift coincided with the completion of the last run-of-the-river dam built on the Snake River (Lower Granite Dam), and one of the great scientific difficulties we face now is determining how much of the Snake river salmon declines is due to climate or the effects of dams.

We examine various climate indices which have been used to detect climate regime shifts, and use them to determine how long we must wait after a regime shift has occurred to detect it. This, of course, will depend on the size of the climate shift and the index's fit to the regime shifts or "interventions." Some indices will show a greater response to the climate regime shifts and will have a higher R^2 (and consequently, higher signal-to-noise ratio). Others may not be sensitive to these lower frequency events and will be useless as a regime-shift detector. We will evaluate the indices used by Mantua et al. (1997), determine their fit to the regime shifts and we will calculate statistical power for each of the indices. In our application, the statistical power is the probability that a given statistical test will detect a significant regime shift. The power will be a function of different lengths of time after a regime shift has taken place (1-30 years), and different response sizes (0-2 standard deviations from index mean). Each of the time series are fit using intervention models, which are now a common set of time series model (Box et al. 1994).

3.10.2 Data

The data used for this study are five different climate/ocean indices (Table 3.10-4). Each has been used extensively to gauge variability in climate. For each index, we calculate the "cold season" averages November -March average (Mantua et al. 1997), and we standardize each one by subtracting its mean and dividing by the its standard deviation.

Pacific Interdecadal Oscillation (PDO)

North Pacific sea surface temperature is the basis for the Pacific Interdecadal Oscillation (PDO) of Mantua et al. (1997). Data were obtained from U.K. Met. Office Historical SST Dataset for the period of record 1900-1993, and from the Reynold's Optimally Interpolated SST for 1994-May 1996 It is the leading principal component of North Pacific sea surface temperature (poleward of 20°N Latitude) for the 1900 through April 1999 period of record. The leading eigenvector of North Pacific SST variability, from 1900-1999 (April). (ftp://ftp.atmos.washington.edu/mantua/pnw_impacts/INDICES/PDO.latest).

Southern Oscillation Index (SOI)

The Southern Oscillation Index is the difference in the sea level pressures between Darwin, Australia and Tahiti. The index is formed by subtracting the standardized Darwin series from the standardized Tahiti series (http://nic.fb4.noaa.gov/data/cddb/). The data were used over the time period 1900-1999 (May). To make the sign of the series consistent with the others, we used the negative of the series (-SOI).

Sea Level Pressure for Northern Sector (NPPI)

This index is based on the sea level pressure anomalies poleward of 20°N Latitude and between 110°E and 110°W Longitude (Hare 1996). It is the leading principal component for sea level pressure in the North Pacific sector (1900-1996) (data provided by Dr. Nate Mantua, University of Washington, Department of Atmospheric Sciences JISAO: the Joint Institute for the Study of the Atmosphere and Oceans, Box 354235 Seattle, WA 98195-4235).

North Pacific Index (NP)

The North Pacific Index is related to the NPPI above. It is the area-weighted sea level pressure over the region 30°N-65°N Latitude, 160°E-140°W Longitude from 1899 to 1999 (March). (Trenberth and Hurrell 1994). To make the sign of the series consistent with the other, we formed the negative of the series (-NP). (http://www.cgd.ucar.edu:80/cas/climind/np.html)

Cold Tongue Index (CTI)

The average sea surface temperature anomaly from 6°N to 6°S Latitude, 180°W to 90°W Longitude, and index commonly used to monitor the oceanic aspects of El Nino/Southern Oscillation (Mantua et al. 1997). (http://tao.atmos.washington.edu/pacs/additional_analyses/sstanom6n6s18090w.html#calculation_details)

Note that these climate indicators fall into two categories the SOI-type (CTI and -SOI) and the PDO-type (PDO, NPPI, and -NP), as demonstrated by the climate indicator correlation matrix (Table 3.10-1). The indicators cannot be considered independent, but are all linked, to some degree by underlying physical processes.

Table 3.10-1: Correlation among climate indicators.

	PDO	CTI	-SOI	NPPI	-NP
PDO	1.00	0.40	0.34	0.66	0.51
CTI	0.40	1.00	0.72	0.35	0.39
-SOI	0.34	0.72	1.00	0.41	0.47
NPPI	0.66	0.35	0.41	1.00	0.65
-NP	0.51	0.39	0.47	0.65	1.00

3.10.3 Methods

Intervention model

A discussion of intervention modeling, a time-series technique developed by Box and Taio (1975), permitted the detection of major changes in a time series when the timing of the change is known beforehand. The timings of the regime shifts during this century are taken as 1925, 1947, and 1977, in accordance with Francis and Hare (1994) and Hare (1996). Wei (1990) provides a review of intervention modeling, and Noakes (1986) discusses their application to fisheries problems. At first it may seem possible to employ a standard two-sample t-test to test for differences in population means. Such a test is only appropriate, however, when the error term is "white" noise (independent and identically distributed gaussian deviates). The intervention models used were similar to those used by Mantua et al. (1997), for detecting climate regime shifts. They are of the following form:

$$Y(t) = regime(t) + N(t) \qquad \text{[Eq. 3.10-1]}$$
$$N(t+1) = \phi N(t) + a(t)$$

where Y(t) is the climate index of interest, N(t) is the noise process, and a(t) is a white noise process.

The function *regime(t)* takes on four different values, depending on the regime. It represents, at all times, the mean level of the climate variable for a given climate regime period.

$$\begin{aligned} regime(t) &= \beta_0 && \text{if} \quad t < 1925 && \text{[Eq. 3.10-2]} \\ &= \beta_0+\beta_1 && \text{if} \quad 1925 \le t < 1947 \\ &= \beta_0+\beta_1+\beta_2 && \text{if} \quad 1947 \le t < 1977 \end{aligned}$$

$$= \beta_0+\beta_1+\beta_2+\beta_3 \qquad \text{if} \qquad 1977 \le t$$

In this case the noise process is allowed to be red (i.e., $0<\phi<1$) or blue ($-1<\phi<0$), depending on whether the lag-1 autocorrelation is either positive or negative, respectively. The lag-1 autocorrelation is approximately ϕ. Positive autocorrelation is common in environmental series. The parameter β_0, represents the mean level of the process prior to 1925. The parameters β_1,β_2, and β_3 represent the shifts from one regime level to the next. A downward shift is indicated by a negative sign and an upward shift, by a positive sign.

The models were fit using maximum likelihood estimates of the parameters, and the t-values, standard errors, and fit statistics, R^2 , were calculated for each time series. In each case, we did not remove nonsignificant regime shift parameters from the model, because we were interested in the magnitude of all regime shifts from the series for calculating power. (A regime shift may be statistically nonsignificant, but biologically important, so the magnitude of each regime shift was estimated). Since the duration of the regimes were rather long (greater than 20 years), eliminating nonsignificant parameters would make little difference in the precision of the remaining parameter estimates, which varied inversely to the square root of the climate regime durations.

Power Analysis

The main goal of the power analysis was to determine how long we must wait until after a regime shift to know it occurred. This is a problem of power analysis, where we are interested in what the post-treatment sample size necessary to detect a regime shift with sufficiently high probability (0.8 is a common value used in ecological literature). In all cases we employ a hypothesis test of H0: no regime shift against the alternative that a regime shift occurred. We set the probability of detecting a regime shift given the null hypothesis is true, the significance level, at $\alpha=0.05$. In general, using a nonlinear time series model, it is necessary to estimate the time series model, project the time series in the future using different levels of regime shifts and different timings, applies the hypothesis test to determine whether to reject the null hypothesis (that no shift occurred), and running this sequence many times, arrive at the probability of detecting a regime shift (power). In our case, fortunately, it will be possible to make some simplifying approximations to determine the power without having to resort to Monte Carlo simulations or bootstrapping. This occurs because, in all cases, the estimate of the noise autoregression coefficient, ϕ, is not significantly different from zero, and can therefore be dropped from the model. This means that the autoregressive component of our time series analysis can be eliminated and shifts in mean can be tested using standard two-sample t-tests.

The power of our test for detecting a regime shift is given by

$$power(\theta,n1,n2) = \Pr(rejecting\ H0\ |\theta,n_1,n_2), \qquad \text{[Eq. 3.10-3]}$$

where θ is the size of the shift, n_1 is the duration of the current climate regime, and n_2 is the number of years after the regime shift of size θ occurs. When the autoregressive parameter, ϕ, is equal to zero,

$$\Pr(rejecting\ H0\ |\theta,n_1,n_2) = \Pr(\left|\hat{\theta}/se(\hat{\theta})\right| > z_{\alpha/2}\ |\theta,\mathrm{n}_1,\mathrm{n}_2) \qquad \text{[Eq. 3.10-4]}$$

$$= \Pr(\hat{\theta}/se(\hat{\theta}) > z_{\alpha/2}\ |\theta,\mathrm{n}_1,\mathrm{n}_2) + \Pr(\hat{\theta}/se(\hat{\theta}) < -z_{\alpha/2}\ |\theta,\mathrm{n}_1,\mathrm{n}_2)$$

$$= \Pr((\hat{\theta}-\theta)/se(\hat{\theta}) > z_{\alpha/2} - \theta/se(\hat{\theta})\,|\theta,\mathrm{n}_1,\mathrm{n}_2) + \Pr((\hat{\theta}-\theta)/se(\hat{\theta}) < -z_{\alpha/2} - \theta/se(\hat{\theta})\,|\theta,\mathrm{n}_1,\mathrm{n}_2)$$

$$= \Pr(Z > z_{\alpha/2} - \theta / se(\hat{\theta})) + \Pr(Z < -z_{\alpha/2} - \theta / se(\hat{\theta}))$$
$$= F(-z_{\alpha/2} + \theta / se(\hat{\theta})) + F(-z_{\alpha/2} - \theta / se(\hat{\theta}))$$

where $\hat{\theta}$ is the estimated shift in the climate index, $z_{\alpha/2}$ is the (1-α/2)*100 percentile of the standard normal distribution, *Z* is a standard normal random variable, *F* is the cumulative distribution function of a standard normal deviate, and the $se(\hat{\theta})$ is the standard error of the estimate of the regime shift. In this case, the standard error of the estimate is

$$se(\hat{\theta}) = \{\sigma^2 (1/n_1 + 1/n_2)\}^{1/2} \qquad \text{[Eq. 3.10-5]}$$

The reason we can use a standard normal distributions here instead of a t-distributions, is that we have a climate record that spans approximately a century. Recall that a t-distribution with a large number of degrees of freedom (greater than 20) is approximately equal to a normal distribution.

The power to detect a shift will increase with n_2. Since the last shift occurred in 1977, n_1>20 years. We will be interested in this power calculation for various values of θ. We know that the 1947 and 1977 regime shifts had important consequences on the NE Pacific ecosystems, so we will use the estimates of the magnitude of these shifts as a guide for choosing θ. For each index we will calculate the probability of detecting a shift of the magnitudes estimated for 1925, 1947 and 1977, namely, $\hat{\beta}_1, \hat{\beta}_2$, and $\hat{\beta}_3$. We use Eqs. 3.10-4 and 3.10-5 to calculate the number of years after the regime shift occurs (described by n_2) that is necessary to achieve a power of 0.8 for a given response level, θ. (Note that for some values of θ, it will be impossible to achieve a power of 0.8 by increasing n_2, since the standard error has a minimum value of $(\sigma^2/n_1)^{1/2}$.)

3.10.4 Results

Maximum Likelihood Estimates and Model Fits

The results of the maximum likelihood estimate are given in Table 3.10-2. The model fits to the data varied greatly from one indicator to the next, suggesting that some indicators may be better regime shift detectors that others. The best indicators are the PDO, -NP, and NPPI, with R2 values of 0.37, 0.26, and 0.22, respectively. The CTI and -SOI indices, have weak regime-shift signals, and have R^2 values of 0.08 and 0.07, respectively. The -NP and NPPI indices alone showed significant steps at each of the known climate regime shifts: 1925,1947, and 1977. Also notice that the estimate of the autoregressive parameter, ϕ, is in all cases non significant. This occurs even when the original time series (without shifts accounted for) contained significant autocorrelations, which was true for the PDO,-NP, and NPPI series. This commonly occurs when low frequency effects (such as regime shifts) are removed from a time series: the low frequencies are explained by the regime shifts rather than autocorrelations (which tend to produce most of their greatest variability in the low frequency domain). Hare and Francis (1995) found a similar loss of autocorrelation by introducing interventions in time series of salmon production.

Table 3.10-2: Results of maximum likelihood estimation of the intervention model.

Index	Coefficient	MLE	Std. Error	t-value
PDO	β_0	0.17	0.19	0.88
	1925 step	0.40	0.28	1.46
	1947 step	-1.46	0.26	-5.56
	1977 step	1.27	0.26	4.92
	ϕ	0.13	0.10	1.30
	R^2=	0.37	σ=	0.81
CTI	β_0	0.33	0.18	1.83
	1925 step	-0.27	0.26	-1.03
	1947 step	-0.44	0.25	-1.78
	1977 step	0.42	0.25	1.73
	ϕ	-0.10	0.10	-1.00
	R^2=	0.08	σ=	0.97
-SOI	β_0	-0.17	0.19	-0.88
	1925 step	0.12	0.28	0.43
	1947 step	-0.13	0.25	-0.52
	1977 step	0.57	0.24	2.36
	ϕ	-0.16	0.11	-1.42
	R^2=	0.07	σ=	1.00
NPPI	β_0	-0.18	0.22	-0.79
	1925 step	0.64	0.31	2.05
	1947 step	-1.03	0.29	-3.56
	1977 step	1.04	0.30	3.50
	ϕ	0.13	0.10	1.22
	R^2=	0.22	σ=	0.90
-NP	β_0	-0.66	0.18	-3.77
	1925 step	1.09	0.25	4.29
	1947 step	-0.71	0.24	-2.92
	1977 step	0.86	0.24	3.60
	ϕ	-0.01	0.10	-0.14
	R^2=	0.26	σ=	0.87

Note: Bolded t-values indicate significance at the 0.05 level.

Power Analysis

The results of the power analysis showed that the time necessary to detect regime shift varies greatly depending on what climate indicator is used (Table 3.10-3). Generally, the indicators that produce the shortest times to detection are those with the highest R^2 values (PDO, -NP, and NPPI). For these indices, the number of years needed to detect the shift vary from 3-12 years for a shift of the magnitude observed in 1947, and 4-8 years for a shift of the magnitude observed in 1977. Only one of these best indicators, -NP, was able to detect a shift of the magnitude seen in 1925 in a short time frame (5 years). The PDO was unable to detect a shift of that small magnitude in any time frame, and -NPPI would take at least 33 years (which is about the duration of a climate regime). The CTI and -SOI indices appear to be useless for detecting regime shifts with sufficiently high probability. This is expected, since the maximum likelihood estimates of the regime shifts for these models were generally not statistically significant and both had low R^2 values (less that 0.10).

Table 3.10-3: Power analysis results for levels of regime shifts equal to those measured during the 1925, 1947, and 1977 shifts.

	n_2 that achieves 0.80 power.			Maximum Power (n_2=infinity)		
Index	**1925-shift**	**1947-shift**	**1977-shift**	**1925-shift**	**1947-shift**	**1977-shift**
PDO	NA	3	4	0.772	1.000	1.000
CTI	NA	NA	NA	0.331	0.701	0.666
-SOI	NA	NA	115	0.100	0.110	0.878
NPPI	33	8	8	0.972	1.000	1.000
-NP	5	12	8	1.000	0.997	1.000

Note: NA denotes a case where it is impossible to achieve a power of 0.80.

3.10.5 Conclusions and Discussion

The best regime shift indicators in the set we examined were the PDO, -NP, and NPPI, which had the highest R^2 values for the intervention models employed. The PDO, the climate indicator with the highest R^2 value, detected both the 1947-shift and 1997 shifts, but missed the 1925 regime shift. Thus, it seems that it is not wise to rely on a single indicator of climate, but several different ones to see a more accurate portrait of historical low-frequency variability. Using the three best indicators, the shortest periods of time to detection of regime shifts of the sizes seen in 1925,1947, and 1977 (with sufficiently high probability) are 5 years, 3 years, and 4 years after they occur respectively. These are lower bounds because they are based on the indicators that give the maximum response (signal-to-noise ratio) at those specific shifts. Among our three best indicators, if we see climate regime shifts of the same strength observed in 1947 and 1977 (relatively large shifts), the time to wait for detection averages 8 years and 7 years, respectively. Thus it appears that we must wait approximately 7-8 years to determine whether a regime shift occurred during the experiment and could lead to erroneous conclusions about the post-pre treatment effects.

For some experiments, it may possible to use reference populations to "control" for the effect of climate, but unfortunately, we do not know for certain whether our reference populations respond to climate in the same way our treatment group does. In fact, there is great spatial variability in population responses to climate regime, with some regions showing an inverse response to others (i.e., the California current vs. the Alaskan current ecosystems, Hare et al. 1999). In many cases, it will be impossible to have such reference populations, since the treatments will be "whole-system" treatments that affect all the populations of interest (such as transportation or dam removal). Therefore it will be necessary to regularly apply the reference treatment (such as no-transportation) in order to measure changes over time in mortality not due to treatment (such as climate regime effects).

What effect will regime shifts have on our interpretation of experimental results that are based on temporal reference comparisons? First, as our power analysis shows, there will be a substantial waiting time to determine whether an experimental treatment is confounded with climate regime effects. Due to the noise in our indicators, a wait of 7-8 years after a regime shift event may be needed to detect it with high probability. Second, there is a problem with implementing experiments with "irreversible" treatments which have responses in the low-frequency domain, making the effect appear to be like a regime shift. If a climate regime shift occurs near to the time such an experiment is implemented, it will likely be impossible to understand what is truly a treatment effect and what is a climate effect. The best way to implement an experiment in the face of a possible regime shift, is to make sure the treatment responses are in the high frequency domain (e.g., applying the treatment every other year) and are not the product of "once on, once off" experiments (See Appendix C).

Table 3.10-4: Climate indices used in the intervention analysis.

Year	PDO	CTI	-SOI	NPPI	-NP
1900	0.77501	1.51313	0.87075	1.30383	1.57829
1901	0.41023	0.25769	0.24403	-0.0879	-0.2979
1902	0.47758	0.30045	-0.214	0.9279	-0.0705
1903	-0.078	2.04158	0.82254	-1.5196	-1.833
1904	-0.0528	-0.3624	-1.4433	-0.3119	-1.5269
1905	0.89567	1.40011	1.20822	1.75174	0.36685
1906	0.7722	1.31458	1.25642	0.67195	-0.4247
1907	0.1605	-0.5243	-1.9254	-0.8558	-2.1085
1908	1.06683	0.34932	NA	0.04008	-1.0939
1909	0.8171	-1.0527	0.24403	-1.5916	-0.556
1910	-0.3698	-1.1749	-0.9612	-0.4718	-1.1201
1911	-0.0584	-0.7259	-1.0335	-0.8957	-1.5137
1912	-0.7739	1.67197	1.1359	0.84792	-0.0486
1913	0.5814	-0.2311	0.48508	NA	-0.0399
1914	0.4888	0.91137	0.55739	-0.3918	0.10881
1915	0.19417	0.98163	NA	1.18385	-0.4991
1916	-0.2071	-0.4052	0.05119	-0.6718	-1.54
1917	-1.1808	-0.5029	-1.154	-1.1197	-1.2426
1918	-0.7542	0.60897	-2.1182	-2.0075	-0.4335
1919	0.41584	1.65975	1.1118	-0.4158	0.33186
1920	-0.3137	0.41347	0.6056	-0.3678	-1.1639
1921	-0.3362	0.62118	-0.8407	-0.7038	-0.2192
1922	1.0051	-0.1028	-0.8889	0.35202	-0.7396
1923	0.17734	-0.3532	-0.7925	0.62396	-0.674
1924	0.65155	0.81973	0.36455	0.55997	0.38434
1925	0.05668	-0.845	-1.0094	-0.2479	0.43245
1926	0.95179	1.12825	1.6662	1.15186	1.82758
1927	1.37549	-0.0447	0.19582	0.87991	0.59426
1928	0.50283	0.12939	-0.0693	-0.2479	0.34935
1929	0.74414	-0.341	-1.4674	1.13586	0.72109
1930	0.32605	0.21798	-1.0335	0.10406	-0.7047
1931	0.7217	1.53146	0.24403	1.02388	1.35524
1932	0.17734	-0.2494	NA	-0.1919	-0.8009
1933	-0.1425	0.07746	0.29224	-0.4478	-0.5822
1934	0.09596	-0.3991	-0.5996	-0.0799	1.16281
1935	1.44283	-0.396	-0.2863	1.19985	0.17879
1936	1.54104	0.1019	0.0994	1.08787	0.79107
1937	0.97704	-0.2463	0.21992	0.81593	-1.575
1938	0.26152	-0.4632	-0.4068	0.24004	0.16567
1939	0.94056	-0.7228	-1.1781	0.34402	-0.0311
1940	1.24641	1.96521	0.65381	1.05588	2.4661
1941	1.93388	1.26876	1.61799	1.9997	1.73574
1942	0.88444	1.6231	1.06359	1.12786	0.53304
1943	-0.3418	-1.3215	-0.913	-1.6716	-1.3169
1944	0.35411	-0.7076	0.34045	0.77593	0.66424
1945	0.07632	-0.9336	-0.2863	1.11187	1.41647

Year	PDO	CTI	-SOI	NPPI	-NP
1946	-1.0853	-0.2423	-0.1416	-0.6718	-0.0049
1947	-0.9591	0.09172	0.55739	-1.3916	-0.5909
1948	0.20259	-0.1608	-0.1416	-0.1039	-0.884
1949	-2.2891	-0.5793	0.31634	-0.7438	-0.7003
1950	-1.8598	-1.6667	-0.6961	0.02408	-0.4685
1951	-1.6213	-1.0894	-1.5397	-1.2637	-1.1289
1952	-1.2313	0.62729	0.94306	-0.2319	-0.7746
1953	-0.3698	-0.2188	0.53328	-0.1039	0.75171
1954	-0.7991	-0.3991	0.19582	-0.4238	-0.2148
1955	-0.2969	-1.0039	-0.7443	-0.1119	-0.8271
1956	-3.0888	-1.8408	-1.3469	-1.5436	-1.3038
1957	-1.6409	-0.958	-0.3827	-0.4238	-0.4335
1958	0.01179	1.23516	1.1118	1.13586	0.65112
1959	0.27555	-0.3563	1.03948	-0.1919	-0.4291
1960	0.57017	-0.3655	-0.455	0.49599	0.02572
1961	0.44671	-0.4418	-0.4791	0.36001	1.07097
1962	-1.9468	-0.6312	-0.9853	-0.3279	-0.5953
1963	-0.4989	-0.958	-0.5273	1.08787	0.80856
1964	-0.931	0.45929	0.72612	-0.0639	0.10444
1965	-1.3126	-0.7717	0.14761	-0.0079	-0.2498
1966	-0.3754	1.35429	0.91896	-1.6876	-0.8533
1967	-0.5073	-0.6954	-0.7684	-0.5118	-0.7178
1968	-0.4933	-1.0069	-0.1175	-0.1519	0.13068
1969	-1.0882	0.48984	0.77433	-0.6638	-0.6609
1970	0.73292	0.97552	0.6056	0.97589	1.56517
1971	-1.5062	-1.4895	-1.5397	-2.2155	-0.9452
1972	-1.9356	-0.5701	-0.5032	-1.9755	-1.1026
1973	-0.2211	1.87052	1.01538	-1.3916	-0.0836
1974	-1.5147	-1.3765	-2.4075	-1.0557	0.11318
1975	-0.3362	-0.7351	0.02709	-2.0555	-0.3067
1976	-1.8626	-1.3154	-1.7084	-1.9835	-0.3373
1977	1.47931	0.95719	-0.214	1.31983	1.47333
1978	0.11841	0.43485	1.56978	0.16005	1.23716
1979	-0.6645	-0.1364	-0.0211	1.56778	-0.5385
1980	0.36814	0.07136	0.21992	0.46399	0.71235
1981	0.66277	-0.2647	0.19582	1.55978	2.13372
1982	0.57579	-0.0508	-0.5273	0.31202	-0.5603
1983	0.49161	2.49366	3.47405	1.89572	2.56232
1984	1.53262	-0.9122	-0.1899	1.09587	1.329
1985	1.06122	-1.3032	-0.1658	1.27984	-0.3023
1986	0.67399	-0.7473	0.12351	0.416	1.98065
1987	2.02367	1.0641	1.37695	1.52779	1.46458
1988	1.38952	0.62424	0.36455	0.17605	0.51992
1989	-0.6673	-1.9538	-1.5397	-0.8558	-1.0589
1990	-0.454	-0.3777	0.79844	-0.4318	-0.6915
1991	-1.9889	-0.0539	0.0753	-0.6958	-0.8402
1992	0.2559	1.37262	1.81083	-0.0719	1.25466
1993	0.4888	0.04692	0.94306	-0.2639	0.24876
1994	1.16504	-0.0203	0.05119	0.61596	-0.4554

Year	PDO	CTI	-SOI	NPPI	-NP
1995	-1.0489	0.83195	0.72612	-0.7358	0.33186
1996	0.35411	-1.1444	-0.1175	-0.1359	0.67736
1997	0.17173	-0.7962	-0.7443	NA	0.28375
1998	1.18468	2.49671	2.00367	NA	1.67888
1999	-0.5326	-1.6667	-1.371	NA	-0.0574

3.10.6 References

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4.0 Overall Strategy and Design

The purpose of Section 4 is to catalogue interactions between the candidate experimental designs from Section 3, develop strategic alternatives that address these interactions, and to begin to identify logistic, legal, and economic components that need to be addressed in regional consultations. In Section 4.1 we catalogue the interactions between the different experimental designs. The experiments were developed in isolation of one another and cannot necessarily be implemented in their current form without confounding results or prohibiting other experiments. In Section 4.2 we explore strategic alternatives that may overcome the problems outlined in Section 4.1. These alternatives may actually increase the amount of learning possible. Section 7 (SRP Comments) is particularly relevant to the design of these alternatives. In Section 4.3, we begin to address the requirements of regional coordination. The implementation of the strategic alternatives would require coordination between the various agencies in the region responsible for management of anadromous fish in the Columbia River Basin.

4.1 Interactions Between Actions

In Section 3, we presented examples of experimental actions designed to address various key uncertainties. These actions were considered in isolation; however, as Table 4.1-1 shows, the experiments in their current form create problems that are both inferential and logistical in nature. The “four-dam breaching” experiment provides an example of an inferential problem. Breaching four dams changes conditions at the same time. Thus it will be hard to tell if future changes in survival are related to cessation of transportation, reduced transportation/hatchery interactions, and/or more natural flows above McNary pool. In addition, the sites for gathering information on smolt migration and adult returns (the dams) no longer exist. An example of a logistical problem is presented by the “hatchery” and “carcass” interaction. If hatchery releases are decreased, there may not be enough carcasses from returning adults in future years to sustain carcass experiments.

Table 4.1-1: Interactions between selected experimental management actions presented in Section 3.

	3.1 Current Operations, Measure D	**3.2 Modify Transportation, Measure D**	**3.4 2-Dam Breach**	**3.5 4-Dam Breach**	**3.6 Carcass Introduction**
3.2 Modify Transp., Measure D	Mutually exclusive (There may be some way to transport some groups under conditions that approximate modifications to transportation)				
3.4 2-Dam Breach	Depends on which dams breached. If LGR and LGO can't measure SAR comparable to historical data. If IHR and LMO can't transport fish by barge.				
3.5 4-Dam Breach	Mutually exclusive (except for MCN) – can't calculate comparable SAR without LGR	Mutually exclusive (except for MCN)	Mutually exclusive – unless one starts with 2-dam breach and then moves to 4-dam breach.		
3.6 Carcass Introduction	- Need a separate D for control and fertilized stock -Reduced precision of D estimates due to fewer numbers of wild fish from unfertilized streams?		Depends on which dams are breached and which variables are used. If LGR stays then Carcass experiments wouldn't affect ability to measure survival rates (i.e., parr, smolt, SAR, R/S). Breaching SAR would affect the measurement of SAR, R/S and Vn consistent with historical data	No smolt counts at upper dams. Could do smolt counts at lower dams, or use egg-to-parr, or parr density as performance measures (although these data are very noisy and are not good substitutes for smolt counts).	
3.7 Hatchery Operations	Possible interaction between the effectiveness of transportation and number of fish released from hatcheries. Need multiple iterations of hatchery manipulation to create contrast Might reduce the number of fish available for experiments.		Possible confounding as conditions under which fish travel together change. Need multiple iterations of hatchery manipulation.		Reducing hatchery releases may affect number of available carcasses for fertilization experiments.

4.2 Strategic Implementation of Actions

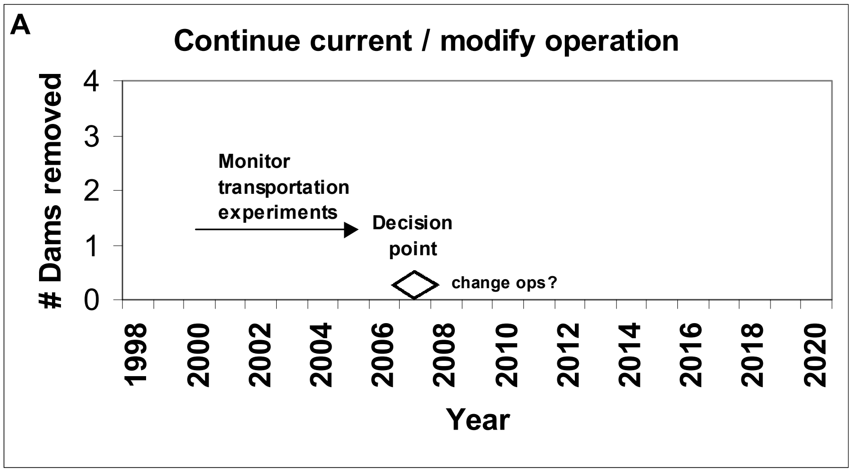
The experiments described in Section 3 are designed to explicitly address a specific uncertainty. However, we will accelerate learning if we can simultaneously test as many uncertainties as possible. Section 4.1 shows that the individual experiments cannot be implemented at exactly the same time. Some types of experiments are mutually exclusive. For example, a four-dam breach would make transportation experiments impossible using current methods.

The purpose of this section is to consider what sequencing of experimental actions would allow us to learn as much as possible about the likelihood of key uncertainties. This includes possible combinations of actions.

Here we outline four strategic alternatives (Table 4.2-1 and Figure 4.2-1) that incorporate components of the experiments presented in Section 3. Many other alternatives could be formulated. These alternatives range from a *status quo,* or baseline, "incremental" approach that used current system operations to the more "risk averse" designs that begin with the action found to provide the greatest benefits to stocks in the PATH analyses. The "incremental" and "risk averse" approach were described in the FY98 Final Report. These alternatives provide an example of the range of creative options that should be quantitatively evaluated (Walters and Green 1997).

Table 4.2-1: Strategic alternatives for implementing experimental actions. H = Hatchery Manipulation; T = Transportation Manipulation; F = Fertilization

Strategic Alternative	Relative Biological Risk to Listed Stocks	Rate of Learning	Relative Cost
A. Current system operation / modify (monitor for D)	High (Indefinite time to breach)	Low (passive)	low
B. H+T+ F + Staggered 2 Dam	Medium to High (Delay to breach)	High (active)	medium
C. H+T+F + 4 Dam + John Day	Low to medium (Delay to breach)	Medium (active)	high
D. 4 Dam + H+ T+ F + John Day	Low (Immediate breach)	Low (less active)	highest



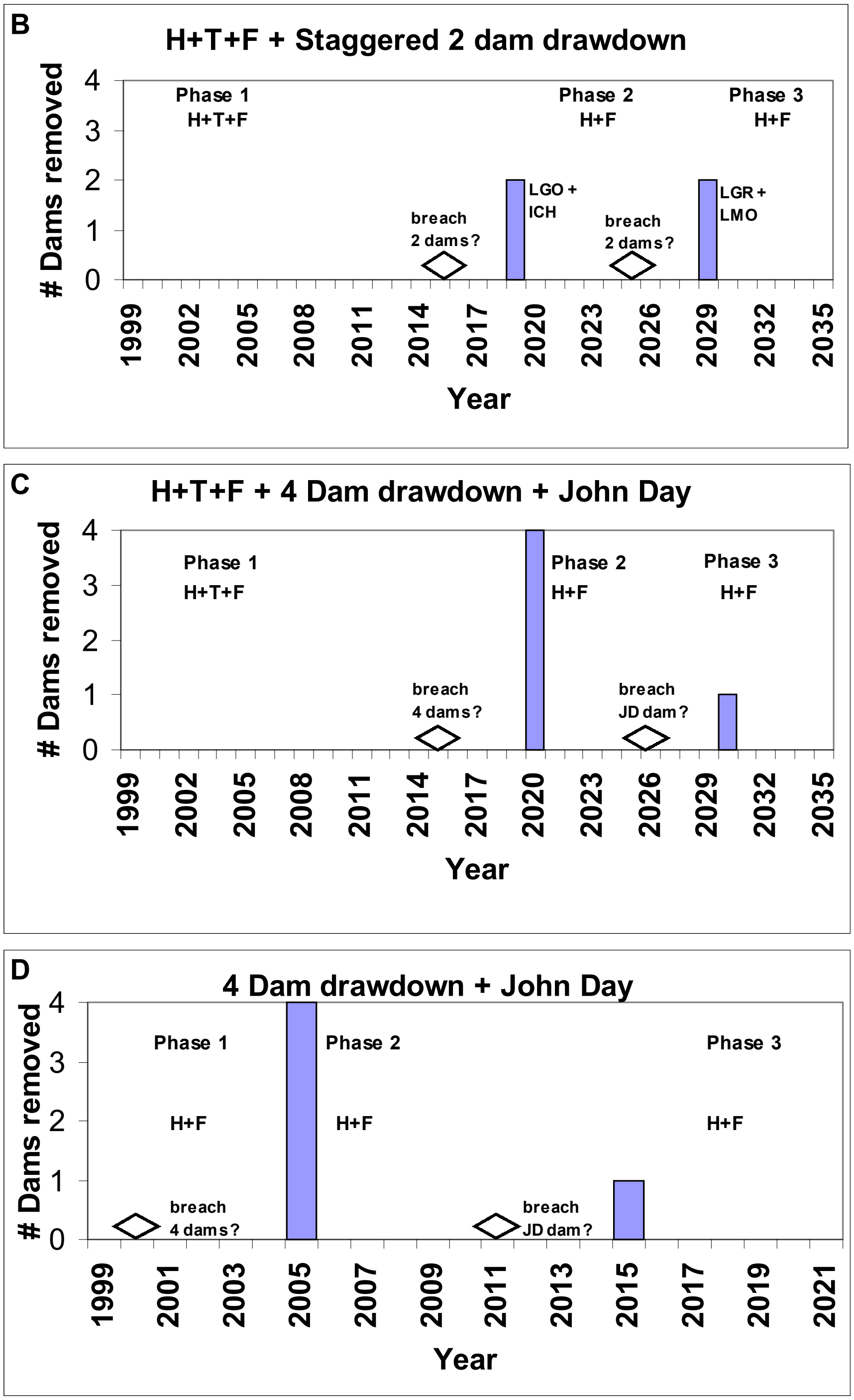


Figure 4.2-1: Four example strategic alternatives. Diamonds represent year in which decision is made. We assume a three-year period for Congressional approval subsequent to the decision, and assume it would take one year to breach two dams, and two years to breach four dams.

4.2.1 Strategic Alternative #1 – Continue Current or Modify Operation (Monitor for D) (Figure 4.2-1A)

Action:

This alternative is the experiment presented in Section 3.1 and 3.2. It continues the status quo and is therefore the most passive of the four strategic alternatives. Its basic components are:

- Continue transport evaluation studies in the Snake River using PIT tags for both yearling chinook salmon and steelhead.
- Optimize conditions for in-river migrants by maximizing spill at downstream projects during migration.
- Optimize conditions for in-river migrants by maximizing spill at downstream projects during migration.

Experimental period:

- Continue current monitoring and transportation system for another five years (2000-2004), complete adult evaluation by 2007.

Variables to monitor:

- PIT tagged fish from all major tributaries of the entire Columbia River Basin above BON
- Estimate NMFS D value using:
- estimation of survival through reaches of the Lower Snake River and Lower Columbia River;
- estimation of the number of PIT tagged fish that experience each passage history during juvenile migration;
- construction of experimental "treatment" and "control" groups of PIT tagged fish representative of the run at large; and
- calculation of SAR: LGR adult returns (Bonneville smolts problem if Lower Granite gone in the future).
- Collect additional information on migration timing from PIT tagged fish.

Decision point:

- 2007, modify system operations based on results.

Uncertainties addressed:

- Effectiveness of transportation (D)

Concerns:

- Experimental management options that propose continuation of status quo hydropower operations, while studying components of extra mortality, need to explicitly recognize the risk associate with the continued declining trend in R/S shown by some stocks.

4.2.2 Strategic Alternative #2: Hatchery + Transportation + Fertilization + 2 Dam + 2 Dam (Figure 4.2-1B)

Action:

This alternative is a hybrid of several actions presented in Section 3.

- Transportation is "pulsed", varying between "intense" and "reduced".
- Hatchery releases are pulsed, varying between "intense" and "reduced" (although this option would depend on the feasibility of maintaining viable hatchery operations when brood stock varies considerably over time). An alternative is to change the timing of hatchery releases (Figure 4.2-2.
- Fertilization (carcasses) is applied to the spawning areas of natal stream.
- Dam breaching following periods of transportation/hatchery/fertilization experiments.

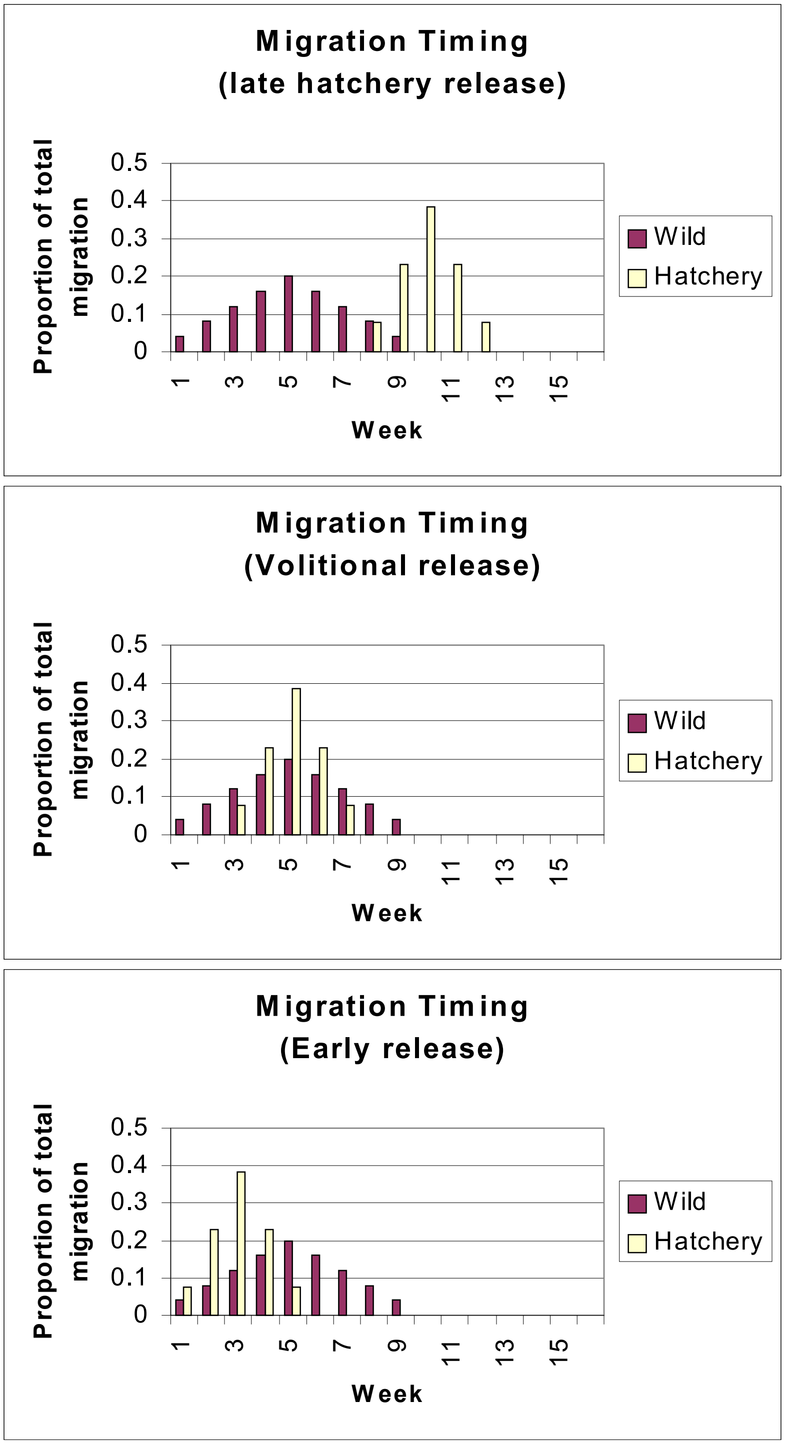


Figure 4.2-2: Example of alterations in the timing of the release of hatchery fish to change overlap in migration timing with wild fish, and thus the exposure of wild fish to interaction with hatchery fish. Week 1 is the first week of March. This is one potential way to implement transportation/hatchery interaction experiments. In the top panel, the release of hatchery fish is delayed. This allows the earlier migrating wild smolts to avoid interaction with hatchery fish over most of the migration period. This would provide cohorts of transported and in-river fish that have not experienced potential contact with hatchery fish. In experimental terms, this would provide treatments analogous to the Transportation “Intensive, Hatchery “Reduced” treatment shown in Table 4.2-3.

The timeline laid out below and in Figure 4.2-1B is an example only. The optimal length for each phase must be determined through detailed valuation studies that consider statistical power and acceptable risk.

For Phase 1, we assume there are four treatments (4 combinations of transportation intensive/reduced X hatchery releases intensive/reduced). Williams et al. (Section 3.1) recommend a minimum of three replicates per treatment for good experimental design. If each treatment is applied in consecutive years, this would require 12 years to complete three replicates of each treatment, plus an addition three years for adult returns to be completed, for a total of 15 years. If the first year of such an experiment was the year 2000, then Phase 1 of the experiment would not be complete until the year 2015.

Possible approaches to accelerate this schedule are discussed in Section 3.2. That section discusses experiments that would address transportation uncertainties under modified transportation conditions. For example, delaying hatchery releases would allow transportation of earlier migrating wild fish under conditions of reduced hatchery interaction. Methods such as this may allow more treatment combinations and replicates of those combinations per year. This could allow the experimental exploration of transportation/hatchery interactions to occur in a shorter time period than described in this example.

Experimental period:

Phase 1 (2000 – 2015) Establish “before” treatments

- Fertilization – treatment and control streams (7 and 7 as in Section 3.5 “Carcass”) Fertilization experiments could be implemented in a “staircase” design (e.g., Figure 4.2-3).
- Hatchery “pulsing” – reduce levels of hatchery releases (no lower than the level indicated in Section 3.7 “Hatchery”).
- Transportation “pulsing” prior to removal of first two dams – reduce the number of fish transported.
- The hatchery and transportation “pulsing” implemented in a pattern as outlined in Table 4.2-2.

Decision point: 2015 - Decision to drawdown LGO and ICH dams.

Pre-removal and removal: 2016 – 2019

Phase 2 (2020 – 2025)

- Continue fertilization experiments.
- Continue hatchery “pulsing” in the Snake River.
- Monitor conditions in unimpounded river reaches.

Decision point: 2025 - Decision to drawdown LGR and LMO.

Pre-removal and removal: 2026 – 2029

Phase 3 (2030 – Future) Monitor conditions in natural Lower Snake River.

- Continue fertilization.
- Continue hatchery “pulsing”.

Variables to monitor:

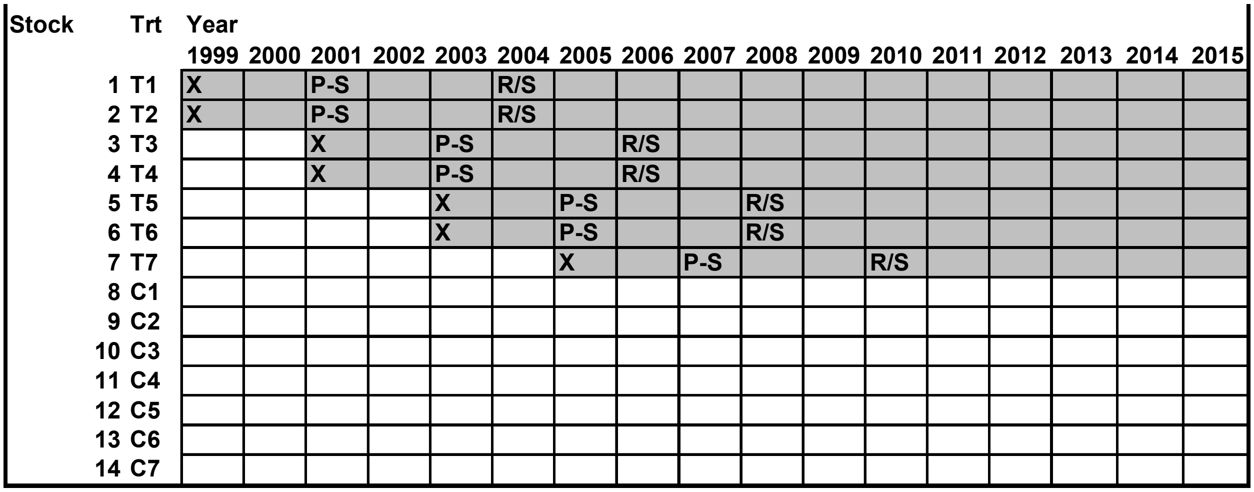
- Parr/Smolt survival to LGR (until LGR removed), perhaps monitor at BON as well to maintain a base measure prior to removal of all four dams.
- Spawners and Recruits, Relative Recruitment Success
- Vn (how will this be done once the dams are gone?).

Decision point(s):

- 2015 Decision to draw down first two Snake River dams.
- 2025 Decision to draw down second two Snake River dams.

Uncertainties addressed:

- Length of the transition period to equilibrium conditions.
- Juvenile survival rate after drawdown.
- Extra mortality:
    - Hydro;
    - Stock Viability; and
    - Hatchery.



Stock	Trt	Year																
		1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
1	T1	X		P-S			R/S											
2	T2	X		P-S			R/S											
3	T3			X		P-S			R/S									
4	T4			X		P-S			R/S									
5	T5					X		P-S			R/S							
6	T6					X		P-S			R/S							
7	T7							X		P-S			R/S					
8	C1																	
9	C2																	
10	C3																	
11	C4																	
12	C5																	
13	C6																	
14	C7																	

X = Year treatment initiated

¦ = Decision point

T = Treated stock **P-S** = Year of first parr-to-smolt survival estimate for treatment

C = Control stock **R/S** = Year of first Recruits per spawner estimate for treatment

Figure 4.2-3: Example of staircase design for part of strategic Alternative #2 [i.e., carcass placement (fertilization) and two-dam breach experiment: 14 stocks are selected, 7 are treated, 7 are controls]. Treatment is initiated at two-year intervals to control for confounding between changing environmental conditions and ephemeral management effects (e.g., an initial large effect followed by return to initial conditions, or worse). This experiment spans two phases: 1) present to decision about four-dam or two-dam breach; and 2) post-breach to decision to remove remaining two dams if the two-dam breach option is selected.

4.2.3 Strategic Alternative #3 – Hatchery + Transportation + Fertilization + 4 Dam + John Day (Figure 4.2-1C)

Action:

This alternative combines the "four-dam breach" design presented in Section 3.4B with experiments that address transportation uncertainties, SV-hatchery, SV-BKD, and SV-Nutrients extra mortality hypotheses.

Fertilization, transportation and hatchery treatments are established before the dams are breached.

Experimental period:

Length of pre-decision monitoring period depends on power analyses. This examples makes the same assumptions as Alternative B (4 treatments, three replicates of each treatment, total of 15 years for 1st phase).

Phase 1 (2000-2015)

- Implement and monitor fertilization experiments.
- Experiments to explore transportation, hatchery and transportation/hatchery interactions through "pulsing" of transportation and hatchery actions.

Decision point: 2015 – Decision to drawdown four Snake River dams.

Pre-removal and removal: 2016-2020

Phase 2 (2021- 2026)

- Continue fertilization experiments.
- Continue hatchery "pulsing."
- Monitor conditions in unimpounded river reaches.

Decision point: 2026 – Decision to drawdown John Day Dam.

Pre-removal and removal: 2027-2030

Phase 3 (2031 - future)

- Monitor conditions in unimpounded river reaches.

Variables to monitor:

- PIT tagged parr.
- Spawner surveys.
- Recruits to wherever.
- R/S, SAR, survival to LGR, Relative Recruitment Success

Decision point(s):

There are two decision points:

- Whether to breach the four Snake River dams or modify management system.
- Whether to breach the John Day Dam.

Uncertainties Addressed:

Concerns:

Breaching removes points where smolts are measured (e.g., PIT tagged smolts at LGR). If new ways of determining SAR are established, they may not be compatible with earlier methods. It will be necessary, therefore, to develop new methods prior to dam removal (constant point that there are just not enough fish to go around when it comes to tagging parr and detecting smolts).

4.2.4 Strategic Alternative #4 - 4 Dam + Hatchery + Fertilization + John Day (Figure 4.2-1D)

Action:

- Breach Snake River dams, stop transportation.
- Hatchery production pulsed or kept constant.
- Evaluate stocks responses.

Experimental period:

Phase 1 (1999-2003)

- Implement and monitor hatchery and fertilization experiments.

Decision point: 1999 - Decision to drawdown four Snake River dams.

Pre-removal and removal:

Phase 2 (2004- 2009)

- Monitor conditions in unimpounded river reaches.

Decision point: 2010 - Decision to drawdown John Day Dam.

Pre-removal and removal: 2010-2013

Phase 3 (2014 - future)

- Monitor conditions in unimpounded river reaches.

Variables to monitor:

- Index stocks in the Snake River, Mid-Columbia and Lower Columbia regions.

Decision point(s):

There are two decision points:

- Decision to remove the four dams (1999).
- Decision to remove John Day Dam (2010).

Uncertainties Addressed:

- Hydro I and II
- Stock Viability

Concerns:

If this action is implemented first, precludes other experiments because it changes conditions throughout the hydrosystem for migrating Snake River fish.

4.3 Regional Coordination

The selection of experimental management options is an iterative process and will require input from many people. First, the preliminary experiments and strategic alternative outlined in Sections 3 and 4.2 need to be reviewed and filtered by the Regional Forum Implementation Team and other decision-making groups such as the Northwest Power Planning Council. Second, the Drawdown Regional Economic Workgroup (DREW) may wish to explore the economic costs and benefits of particular experimental management options. Third, PATH cannot deal with the logistic complexities associated with changes that may be required in specific management sectors of the Columbia Basin ecosystem. When PATH has developed a list of potential experimental actions, we must consult with managers in these areas to refine and plan these actions. For example, if PATH decides that changing hatcheries or harvest policies is an acceptable experimental management option (from a conservation and learning perspective), then hatchery operators and harvest managers must be involved in the detailed planning of how to do this. This consultation will also be important to recognize and reduce the possibility of confounding between PATH experimental management options and other management activities taking place in the Columbia River basin.

Examples of Legal Issues with Harvest and Hatchery Actions

- Some hatchery fish are for conservation purposes, can't reduce these numbers.
- Steelhead programs are successful, don't want to impact those.
- Institutional resistance, legal implications.
- Need consensus from all groups involved in Columbia River Fishery Management Plan renegotiations.
- May require congressional approval if the Lower Snake Compensation Plan Act and USFWS treaty trust responsibilities are violated.
- Would likely result in a substantial reduction in State and Tribal fisheries.
- Complex management implications that require consideration of fishery regulations and treaty rights.

Implementing experimental actions will require a high level of commitment and trust between agencies and groups. It is important to begin consultation at a very preliminary stage to allow adequate input by affected groups to the design of experimental programs. Such participation is the key ingredient for successful adaptive management (Walters 1997).

Maintaining Experimental Conditions

Confounding can be introduced through continued changes to the Columbia system that are outside of the sphere of experimental control. For example, if the "four-dam breach" was implemented, using the Mid-Columbia stocks as "controls", then one would have to be concerned about activities taking place in that region to improve conditions for the stocks.

Available Resources

Are there enough resources for different experimental components? For example, are there enough fish to provide experimental tagging subjects for transportation, hatchery and fertilization experiments? Will there be enough carcasses for fertilization experiments if hatchery production is "pulsed"?

Success of Implemented Experiments

By “success” we mean that experimental conditions and infrastructure (e.g., monitoring, treatments, etc) can be maintained over the spatial and temporal extent of the experiment to reduce confounding, maintain a balanced design and therefore maintain expected levels of inferential power determined in preliminary analysis. Success may be facilitated by designing experiments that allow participating agencies to maintain control over experimental conditions.

Data

Experimental management actions have profound implications for the design of data collection programs throughout the Basin. Once the candidate experimental management actions have been reduced to a priority set of feasible actions and quantitatively evaluated, then we can begin to consider these implications in detail.

5.0 Evaluation of Experimental Actions

Sections 3 and 4 describe potential experimental management actions and some of the overall considerations in their implementation. The key question is, which experimental actions are the most appropriate? In this section, we present examples of some of the analytical methods and tools that might be used to answer this question. We also include an example presentation of comparisons between actions. The ideas presented here are preliminary. The exact issues, methods, and tools will depend on the refined list of actions that is generated by regional discussions of this report.

5.1 Evaluation Measures

Because of the inherent trade-offs in objectives, PATH needs to assess the quantitative performance of each experimental management design with respect to both conservation objectives (e.g., probability of survival and recovery), and learning objectives (e.g., ability to detect effects). Not all of the possible experimental designs will be equally informative or even feasible. Therefore, decision-makers need some way to compare different experimental designs and identify the one that best satisfies trade-offs among objectives. Cost evaluations (presumably by DREW, the Drawdown Regional Economic Workgroup) will need to be completed concurrently with PATH's evaluations. This section briefly reviews several measures that can be used to evaluate the expected performance of a given management experiment.

5.1.1 Power

A priori statistical power analysis can be used to determine the probability of detecting a biologically important effect of some explanatory variable (i.e., the one that will be experimentally manipulated); (see Appendix B for details). This type of analysis can be used to tell managers how long it might take to achieve a given statistical power, or the maximum power that can be achieved under a specified design (e.g., Figure 5.1-1). This can be helpful in revealing trade-offs between learning and conservation, as shown in Figure 5.1-2. In this hypothetical example, the experiment would have to run for about twenty years to achieve high power (i.e., 0.8 or greater). However, the probability of meeting the recovery standard declines over the same time period from 0.6 to 0.3. After five years, the probability of meeting the recovery standard is at 0.5, and power is also about 0.5. That means the chances of correctly detecting a true effect (power) and not detecting a true effect (Type II error) are equal, and both 0.5. Managers could use a figure such as this to visually assess trade-offs between learning and conservation.

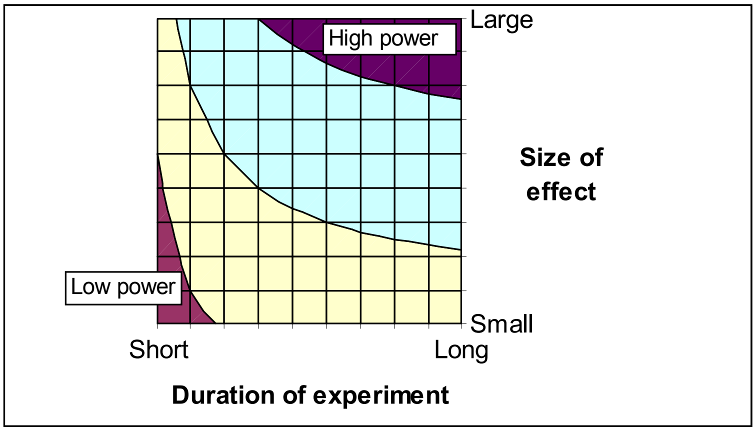


Figure 5.1-1: Example nomogram of the relationship between effect size and statistical power.

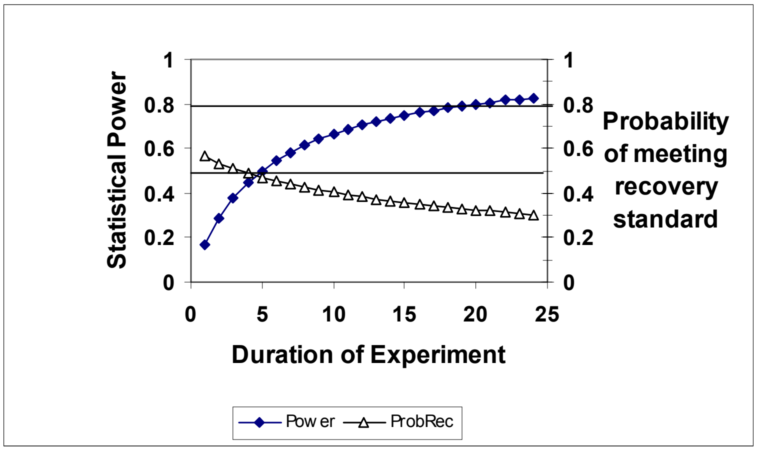


Figure 5.1-2: Hypothetical example of possible trade-offs between experimental power and conservation objectives (probability of meeting recovery standard after 48 years). Whether the lines correlate or diverge (as shown) will depend on whether the chosen action (treatment) augments a mechanism that turns out to be truly beneficial for survival.

This simple example illustrates some of the concepts behind the evaluation of management experiments using power analysis. In many cases, the assumptions behind classic statistical tests will not be met and other methods must be used to estimate power. These methods include the use of simulation modeling as discussed in Chapter 6 of the FY98 report (Marmorek et al. 1998, and see Appendix B in this document).

It is important to consider other objectives and valuation measures before ruling out low power experiments. A low power experiment (e.g., "low" because it may take a long time to obtain high power) *if well designed* (e.g., still uses good controls and interspersion), may still meaningfully reduce the probabilities of improbable hypotheses. Depending on the significance of the hypothesis in question, this information *may* be sufficient to improve conservation performance.

5.1.2 Net Benefits / Risks

Walters and Green (1997) addressed the importance of assessing the net benefits and risks of adaptive management experiments over two time periods: 1) the short-run experimental period; and 2) the post-experimental period. Thus the value of the experiment is the knowledge gained and is represented solely in terms of the effect of that new knowledge on expected management performance over the long term (Walters and Green 1997). This method uses the decision analysis approach of calculating the expected value of a range of outcomes. This "best" design is the one that maximizes the expected value. In the words of Walters and Green (1997), "valuation of experimental design choices has to be based not on any ill-defined or absolute standard of "best practice", but rather on comparison of alternatives to the status quo" (p. 991). Thus, traditional design concerns, such as statistical power, only apply in so far as they increase the probability of selecting the "optimal" design.

There are other methods that have been proposed for explicitly considering biological uncertainty and economic value in the design of experiments. These methods focus more on the classical statistical concerns about Type I and Type II errors. They include the consensual setting of acceptable error rates, for example, the use of *a priori* statistical power analysis (Section 5.1.1), and decision analysis (Section 5.2.2). Examples include: Mapstone 1995 ("scaleable decision rule"), Peterman 1990 (*a priori* statistical power analysis), and Antcliffe 1993 (decision analysis for selecting environmental monitoring designs). These methods explicitly recognize the probability of making the wrong decision under different designs and the related costs. In many ways these examples are simpler to implement than the method of Walters and Green (1997).

Economic

If the management objective is to select the experimental design that maximizes long-term performance, then the evaluation framework should combine biological and economic information. This is a challenging problem. Many of the costs and benefits cannot be estimated with certainty. There are both monetary and non-monetary values to consider, and economic evaluations are not part of PATH. Furthermore, DREW has not been asked to evaluate AM experiments.

Walters and Green (1997) address this challenge in their paper on the valuation of experimental management options. Walters and Green present a value function that breaks the value (benefit - cost) of a management experiment into three components:

$E(V)_{\text{experiment}}$ = {value from experimental units during the experiment} +
{value from nonexperimental units during the experiment} +
{value obtained from applying experimental results to all management units after the experiment}.

That is, the value function is composed of the value of doing the experiment and the value of applying the decision the experiment supports into the future. If economic valuation is required, it may be possible to adapt the Walters and Green (1997) accounting framework for PATH experimental-management analyses.

A more general equation for calculating the net benefits/risks of management experiment is:

$\text{Net Benefit}_{\text{experiment}}$ = {expected value of managing according to the best action with additional information from experimental manipulation} -
{expected value of managing according to the best action without additional information}.

This evaluation method requires simulating: 1) the variation in biophysical outcomes [e.g., SAR, (R/S), RRS] associated with the experimental management plan; 2) observation or measurement error in the monitoring program; 3) estimation of the probabilities assigned to alternative hypotheses at the end of the experiment; and 4) the decision rule that will be followed at the end of the experiment (Walters and Green 1997). These simulations are often numerically intensive.

Conservation Measures

Though their examples focused on monetary value, the valuation model proposed by Walters and Green (1997) applies equally to conservation objectives (i.e., probability of survival and stock recovery). Experiments that may be biologically risky in the short run (experimental period) may be highly valuable once the experiment is completed because key uncertainties may have been sufficiently reduced to identify a different management policy that considerably outperforms the routine practice (Walters and Green 1997). Thus, assessment of the conservation performance of a management experiment should involve an evaluation of the trade-off between short-run and long-run conservation performance as well as application of the "$\text{Net Benefit}_{\text{experiment}}$" equation above.

5.2 Analytical Approaches and Tools

The evaluation measures described above will require a set of analytical tools to project future outcomes of experimental actions, and allow a comparison of these outcomes between actions. An experimental management action is evaluated by assuming some underlying hypothesis that relates system response to that action, then simulating the effects of that action. The simulated data include both sampling error and natural variability. The data are analyzed to see if the hypothesized response can be detected and how long it will take to detect. This approach is known as “gaming”; a good description is found in Walters (1994).

The precise structure and level of detail for these tools will depend on which experimental actions are chosen, the measures to be generated, and other details of PATH’s experimental management approach that have not yet been finalized. Therefore, in this section of the report we discuss some of the issues that must be addressed when developing these tools, along with some examples of what these tools and outputs might look like. We anticipate that further development of these tools will occur as actions, objectives, and evaluation measures are defined in greater detail.

5.2.1 Simple Life Cycle Model

In their September 1998 Report, the SRP recommended that PATH develop a simpler set of tools for modeling life-cycle responses of fish populations to experimental actions. These simpler models would be flexible to allow the evaluation of responses to a broad range of potential actions, yet simple enough that the evaluations could be done rapidly. The SRP emphasized that using simpler models does not mean throwing the existing detailed models away. The simpler models must be checked for consistency against the detailed passage models. For example, the detailed models could generate data based on a set of assumptions under a specific hydrosystem management action. These data would capture the variability in key indicators expected under those conditions. Simple relationships between relevant management variables and key indicators could be developed using this data. These relationships could be included in simpler models that would be able to generate data in response to simulated management experiments.

Key features of these simpler tools are:

- they must be able to capture the key dynamics and uncertainty that is observed in the more detailed and complicated models and allow rapid evaluation of alternative experimental designs;
- they need only predict anomalies in measurable responses to the experimental actions under different hypotheses about these responses to the experimental treatment (i.e., they need not predict life stage-specific survival rates if overall life-cycle survival (recruits/spawner) is the response that is being measured);
- they should remove the effects of other factors on the measured response (e.g., effects of density-dependent spawner effects, common year effects, and the main in-river survival effects of lower dams on recruits/spawner); and
- they will require some form of passage model to remove the main in-river survival effects of lower dams. A simplified version of FLUSH or CRiSP has already been proposed by Dr. Jim Anderson; and other simpler models have been explored by Dr. Rick Deriso (Chapter 5 of PATH FY96 Retrospective Report (Marmorek 1996)).

Figure 5.2-1 shows an example of the output that a life-cycle model might generate for evaluating experimental actions. The figure shows the projected number of spawners for the Johnson Creek index stock of spring/summer chinook as a result of implementing an experimental pulsing of steelhead hatchery releases, given a hypothetical relationship between hatchery releases and extra mortality. The number of spawners varies from year to year because of natural variability in productivity and survival, and because of random effects that can increase or decrease survival rates in any year. Two cases are shown in the graph. The “True” line (thick black line) shows the projected number of spawners with just the natural variability and random effects included. That is, it provides an indicator of what the escapement would be IF the number of spawners could be counted with complete accuracy and precision. The other case (the “current monitoring” line on the graph) includes both natural variability AND some variability introduced by measurement error. In other words, this is the number of spawners that would actually be measured given the precision of monitoring programs.

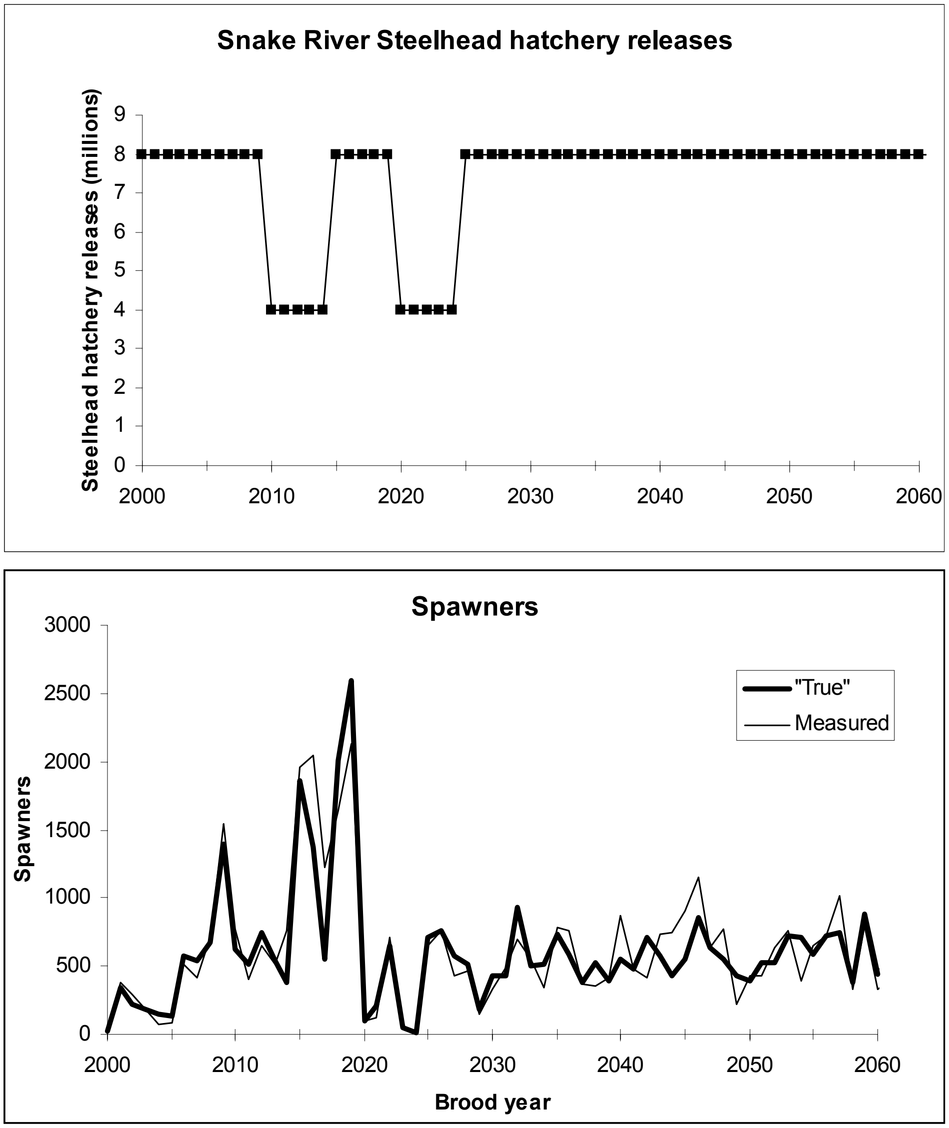


Figure 5.2-1: Example output of a simple evaluation model – projected number of spawners for Johnson Creek summer chinook.

The number of spawners provides a basic measure of stock response to the experimental action, and can also be used as the basis for calculating various conservation measures for this action. For example, in this particular case the survival escapement threshold of 150 spawners is exceeded in 98 out of 100 years. Further, the comparison of the “measured” response to the ‘true” response provides a basis for estimating statistical power of a particular action and monitoring program, as discussed in Section 2. Projected numbers of spawners may not be the best measure with which to estimate statistical power, because the response to the experimental action may be confounded by changes in in-river harvest, adult upstream survival rates, and other things that affect escapement. An alternative measure would be the ln(recruits/spawner), or the Relative Recruitment Success, which are indices of overall survival rate (Figure 5.2-2). Statistical power of detecting the effect of the experimental hatchery releases is likely to be higher with overall measures of survival because they do not include the effects of harvest and upstream survival rates. Here, the measurement error to be included in the model would have to consider the precision in the estimates of recruits, as well as in the spawner counts. Other response variables could also be modeled, depending on what objective is being assessed.

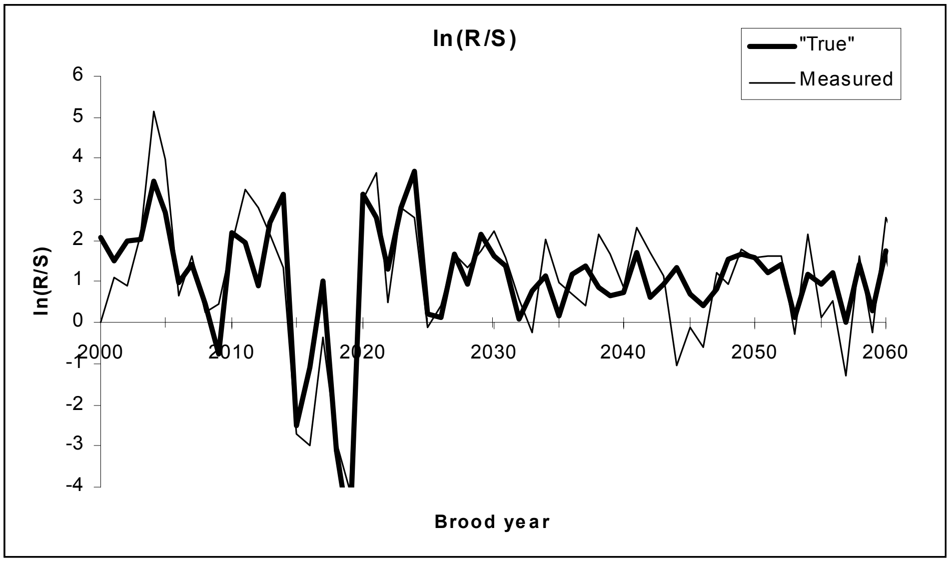


Figure 5.2-2: Example output of a simple evaluation model – projected ln(R/S) for Johnson Creek summer chinook.

The above examples illustrate some general points regarding tools used for evaluating experimental management actions:

1. Models can be relatively simple while still embodying hypotheses that relate experimental actions to various response variables.
2. They include sources of variability in projected responses, including natural variability and random effects, AND variability due to measurement error.
3. A wide range of outputs can be generated, and the exact outputs that are used will depend on which objective is being considered.

5.2.2 Decision Analysis

Decision analysis is a useful tool in adaptive management for selecting an appropriate experimental action and monitoring program. Before implementing an experimental management approach, decision makers must be able to evaluate alternative adaptive management plans and select one based on the plans' relative abilities to generate timely and cost-effective information, the biological consequences of alternative experiments, and the trade-offs between learning and conservation objectives.

A key question is, which experimental design is the most appropriate? There are many possible experimental actions, and variations on those actions, that could be implemented. However, not all of these possibilities are going to be equally informative, let alone feasible. If a sub-optimal action is chosen, the information may be overly costly or may not reduce uncertainties, or the actions may have detrimental effects on protected species. Decision-makers, therefore, need some way to compare different experimental designs and identify the one that best meets all objectives (both learning and conservation) of adaptive management.

The main components of a decision analysis to assess alternative experimental actions for Snake River spring/summer and fall chinook salmon are:

1. alternative actions to be assessed, and alternative combinations of those actions;
2. management objectives by which actions are evaluated;
3. performance measures used to compare actions;
4. uncertainties; and
5. models and tools to conduct the assessment.

These individual components have been discussed throughout this report. Decision analysis provides a structure for integrating these components into an overall analytical framework for assessing alternative designs and assessing potential trade-offs between conflicting objectives (e.g., Sainsbury 1991). The precise nature of this structure will be developed as the individual components are further defined.

5.3 Synthesis

The tools discussed in the previous section provide a way to project various outcomes of alternative experimental management actions or combinations of actions. Comparison of these outcomes across actions will allow decision-makers to assess the relative strengths and weaknesses of alternative actions, and select an action or combination of actions that best meets their multiple objectives. Various regional groups will have different primary objectives for experimental management actions, and these will need to be explicitly defined.

There are many ways of synthesizing the results of these assessments. Table 5.3-1 shows one possible way of doing this, as a starting point for discussion.

Table 5.3-1: Synthesis of outcomes for alternative experimental management actions.

Action	Power	Time to Detect	Pr(Surv)	Pr(Rec)	Logistical Feasibility	Cost
Continue transport, measure D					(1 to 5 scale)	
Modify transport, measure D						
2-dam breach						
4-dam breach						
Carcass introduction						
Hatchery operations						
Combination 2*						
Combination 3*						
Combination 4*						

* See Section 4 for an explanation of these combinations.

6.0 Next Steps

PATH Experimental Management work involves seven basic objectives (Table 6-1). Completion of this report represents substantial progress on objectives ExpM2 and ExpM3. The next critical step is to have this report reviewed by the Implementation Team, the Northwest Power Planning Council, other regional policy groups, and managers in the habitat, hatchery and harvest domains. Input from these groups is critical to ensure that a full range of feasible experimental management actions is evaluated by PATH.

We have also attempted in this report to identify possible issues and approaches for objectives 4 and 5 (tools and structures for evaluating experimental management actions). Discussion of these objectives is still preliminary, but is intended to stimulate thinking about future PATH experimental management work.

Table 6-1: Experimental management (ExpM) tasks of PATH.

Task	Task Description
ExpM1	Clarify the experimental management approach recommended by the Scientific Review Panel (SRP) (see Chapter 6 of FY98 final report (Marmorek et al. 1998)).
ExpM2	Describe the experimental management actions as variations to A1 (current management), A2 (maximize transportation), A3 (natural river drawdown of 4 lower Snake R. dams), etc.
ExpM3	More detailed description of experimental management options with review, input from SRP, Implementation Team, Northwest Power Planning Council, and regional managers.
ExpM4	Develop tools (modifying models, developing simpler models, compare simpler models to existing ones) for quickly evaluating experimental management options.
ExpM5	Evaluate experimental management actions in terms of risks to stocks versus amount of learning possible.
ExpM6	Evaluate proposed management actions with/without experimental management across populations (e.g., spring/summer and fall chinook).
ExpM7	Using results from ExpM evaluation, develop a research, monitoring, and evaluation plan to support the 1999 decision.

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Appendix A: Glossary of Terms, Acronyms, Variables and Parameters

α: extra mortality in a given year for a given sub-region (i.e., Snake River, Lower Columbia River).

δ: year – effect parameter for a given year (common year effects affecting both upstream and downstream stocks).

ε: normally distributed mixed process error and recruitment measurement, which depends on year and sub-basin.

λ: post-Bonneville survival factors for transported (λ_t) and non-transported smolts (λ_n).

μ: incremental total mortality between the Snake River Basin and the John Day project in a specific year.

ω: system survival (e^{-M} + [DP + 1 – P]).

a: Ricker *a* parameter.

b: Ricker b parameter.

A1, A2, A2', A3, B1: Management Actions (see Table 1.1-2).

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

AIC (Akaike Information Criterion): *-2 ln (Likelihood) + 2p*, where p = #parameters.

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

BIC (Bayesian Information Criterion): *-2 ln (Likelihood) + p*ln(k)*, where p = # parameters and k= # observations.

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

BOD (Bonneville Dam Observation Program: An accounting of the fall chinook at Bonneville Dam by bright (upriver late maturing stocks) and tule (lower river early maturing stocks) designation.

BON (Bonneville Dam)

BPA (Bonneville Power Administration)

BRWG (Biological Requirements Working Group)

BSM (Bayesian Simulation Model)

Brights: late maturing fall chinook typically from above The Dalles Dam. Bright in color and not yet ready to spawn when they enter the mouth of the Columbia River.

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

CARTs (Categorical Regression Trees)

cp: complexity parameter.

CPUE (Catch Per Unit Effort)

CRFMP (Columbia River Fish Management Plan) an agreement between sovereigns that allocates fishing effort in accordance with a harvest schedule designed to rebuild stocks and meet treaty obligations with Native Americans.

CRiSP (Columbia River Salmon Passage Model)

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

D: ratio of post-Bonneville survival of transported fish to post-Bonneville survival of in-river fish.

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

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

DES: the naturally spawning bright fall chinook index stock from the Deschutes River. A secondary component of the **URB** harvest management unit.

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

E: climate index variable (**PAPA** drift). Represents the latitude of a drifting object after three months drift starting at station **PAPA**.

EJUV: Equilibrated Juvenile survival rates following drawdown.

EMCLIM: Extra Mortality / future Climate.

ESA (Endangered Species Act)

ESBS (Extended Length Submersible Bar Screens)

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

Extra Mortality: extra mortality is any mortality occurring outside of the juvenile migration corridor that is not accounted for by either: 1) productivity parameters in spawner-recruit relationships; 2) estimates of direct mortality within the migration corridor (from passage models); or 3) for the Delta model only, common year effects affecting both Snake River and Lower Columbia River stocks.

F: average flow (in thousand cubic feet per second) at Astoria during April-June.

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

FCRPS (Federal Columbia River Power System): the major hydropower dams of the lower Snake and lower Columbia rivers.

FLUSH (Fish Leaving Under Several Hypotheses): a passage model developed by the State and Tribal fish agencies.

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

GBT (Gas Bubble Trauma): non-lethal or lethal effects of the growth of air bubbles in the cardiovascular systems of fish.

HAB: habitat effects.

HYSER: a U.S. Army Corps hydro-regulation model to predict monthly flows associated with a particular method of operating the hydro-system.

HYURB: the naturally spawning **Upriver Bright** fall chinook index stock from the Hanford Reach and Yakima River area (McNary Pool). The major component of the **URB** harvest management unit.

IHR/IHB (Ice Harbor Dam)

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

ISAB (Independent Scientific Advisory Board): scientific body that provides independent advice and reviews to NMFS and the NPPC.

I.T. (Implementation Team): an inter-agency policy group to whom **PATH** reports.

JDA/JDD (John Day Dam)

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

KCFS: a unit of measure for flowing water, expressed in thousands of cubic feet per second.

LGO/LGS (Little Goose Dam)

LGR (Lower Granite Dam)

LMO/LMN (Lower Monumental Dam)

LRW (Lower River Wild): a Columbia River fall chinook harvest management unit that is composed of bright stocks below Bonneville Dam, including the North Fork Lewis River stock.

m: total direct passage mortality rate, including both passage and extra mortality.

Δm: extra mortality rate, expressed as an instantaneous rate, which depends on year and region, and is calculated as the differences between total mortality (**m**) and passage mortality (**M**).

M: direct instantaneous passage mortality rate of juvenile fish (both transported and non-transported) from **LGR** pool to below **BON**.

MCN (McNary Dam)

MLE (Maximum Likelihood Estimate)

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

Natural Spawner: Adult salmon that spawn in-river as opposed to returning to artificial spawning channels and hatcheries. Their origin may be natural or hatchery.

NFL: the naturally spawning bright fall chinook index stock from the North Fork of the Lewis River. The major component of the **LRW** harvest management unit.

NMFS (National Marine Fisheries Service)

NPPC (Northwest Power Planning Council)

OSCURS: an ocean circulation model.

p: depensation parameter.

P or Pbt: the proportion of juvenile fish below **BON** that were transported.

PAPA: an index of ocean currents

PATH (Plan for Analyzing and Testing Hypotheses)

PDO (Pacific Decadal Oscillation)

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

PMOD: Passage Model.

PRD (Priest Rapids Dam)

PREM: Predator Removal effectiveness.

PRER: length of pre-removal period.

PROSP: prospective model for the distribution of extra mortality (Alpha or Delta).

Productivity: natural log of the ratio of recruits to spawners for a specified time period (in the absence of density dependent mortality). Measured here as the intercept or "*a*" value from the Ricker spawner/recruit function.

PSC-CTC (Pacific Salmon Commission Chinook Technical Committee): deals with ocean salmon harvest management issues.

R: "observed" recruitments (returning progeny) originating from a given set of natural spawners (parents). The measurement may be taken at different points, such as the spawning ground, or the mouth of the Columbia River (including or not including ocean harvest impacts). In this document, recruits include all mature (jack and adult) returns of natural origin.

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

Rkm (River kilometer): a measurement of river length in kilometers typically taken from the mouth of the river or tributary to the designated landmark following the course of the river.

R/S: recruits per spawner is the number of mature fish returning to the point of recruitment (R) divided by the number of spawners in the parent generation (S).

s: FLUSH variable for survival to below **BON** of control (non-transported) fish.

S: "observed" spawners (parents). In this document, jacks are not considered to contribute to spawning, so only adult spawners are counted as parents. All adults on the spawning ground, regardless of origin, are considered to be parents for the natural-origin recruits.

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

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

SRB (Snake River Brights): a Columbia River fall chinook harvest management sub-unit that is part of the **URB** unit, now tracked separately due to **ESA** listing of Snake River fall chinook. The naturally spawning bright fall chinook index stock from the lower Snake River.

SRI (Survival Rate Index): the residuals from a fit of stock recruitment function to a given period of brood years. The natural log of the ratio of observed **R/S** and predicted **R/S** from a fit of observed recruitment data to the Ricker spawner/recruit function.

SRP (Scientific Review Panel)

STEP: formulated to model the effect of a 1975 (brood year) climate regime shift, which has different effects in different subregions.

STS (Standard Length Submersible Travel Screens)

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

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

T:C or T/C or TCR: the Transport : Control ratio is the ratio of transported fish survival to in-river fish survival from juveniles at the collection point to adults at the same point.

TAC (*U.S. v Oregon* Technical Advisory Committee): advises on Columbia River harvest management issues for various species including salmonids.

TDA/TDD (The Dalles Dam)

TJUV: Transition period: Juvenile survival.

TRANS: transportation model.

TURB: historical turbine / bypass survival assumptions.

Tules: early maturing fall chinook of the lower Columbia River (not found naturally above The Dalles Dam). Dark in color and ready to spawn when they enter the mouth of the Columbia River.

URB (Upriver Brights): a Columbia River fall chinook harvest management unit that includes the Hanford Reach-Yakima River stock, the Deschutes River stock, and the Snake River bright stock.

Vn: direct passage survival of in-river juvenile fish, measured from the head of **LGR** pool to the tailrace of **BON**, including reservoir and dam survival at each project.

Wild Spawner: the natural spawner whose parents were of natural origin.

WOE (Weight of Evidence)

WTT (Water Transit Time)

Appendix B: Guidelines for *A Priori* Power Analyses

Statistical power is defined as the probability of correctly detecting some specified effect size, given a particular situation. For example, it is the probability of correctly rejecting the null hypothesis of no effect in an experiment testing the effect of stream fertilization on the **proportion** of eggs surviving to the smolt stage. Power is a function of four factors: Alpha (which is usually 0.05), the true effect size, sample size, and sample variance (Peterman 1990a, 1990b). Power increases with increasing alpha, effect size, and sample size, but decreases as sample variance increases because the greater level of "noise" tends to mask the true effect. In deliberately designed manipulative experiments on Columbia River chinook, researchers can, in theory, change all four factors to increase power. For instance, all else being equal, a larger effect size (e.g., more fertilizer, or greater reduction in numbers of hatchery steelhead smolts released) will make it easier to detect the "signal," or response to the manipulation, amidst the background variation. Similarly, larger sample size (e.g., more tagged groups of fish involved in a modified transportation regime) will increase the precision in estimates of changes in survival rates. Researchers can reduce residual (unexplained) variance by choosing a more rigorous design than simply a before/after setup (e.g., an enhanced BACI design with several stocks in different spatial locations sampled at several times both before and after the experiment starts). Such a design will improve the estimate of the effect of the experimental manipulation by permitting a separate estimate of the portion of total variance in survival rates that is attributable to spatial differences among stocks within years, to differences among years within stocks, etc. Finally, increasing alpha from 0.05 is difficult to justify because it is so ingrained. But in any case, in most situations, increasing it to 0.1 makes very little difference.

Power analysis prior to carrying out an experiment is an iterative process (Figure B-1). It frequently happens that an initial plan for an experiment will have low estimated power to detect a biologically important effect. The plan is then revised in some way to increase power to detect that effect to some acceptable level (e.g., by choosing a different response variable, or more years or stocks).

This raises a key question. What is the biologically important effect size that you want to detect in an experiment? This should ideally be resolved before the PATH power analyses are done. One approach is to express every response variable in terms of one or all of the NMFS jeopardy standards by translating the effect size into the probability of reaching the target abundance by the specified year. For instance, someone could calculate the increase required in in-river survival rate of non-transported Minam spring-summer chinook to just achieve the 48-year recovery standard. That value would be an example of a "biologically important effect size." If in-river survival rate was not to be estimated directly, then the measure that is being estimated and that is linked to that survival rate would need to be specified as the important effect size. At the very least, if such a biologically important effect size cannot be agreed upon, then analysts should show results for estimates of power for several different example effect sizes over some plausible range, using a response variable that is as directly relevant as possible. One possibility is to express the range of effect sizes in terms of a % increase in the response variable (e.g., explore 10%, 20%, 50%, 70% increases).

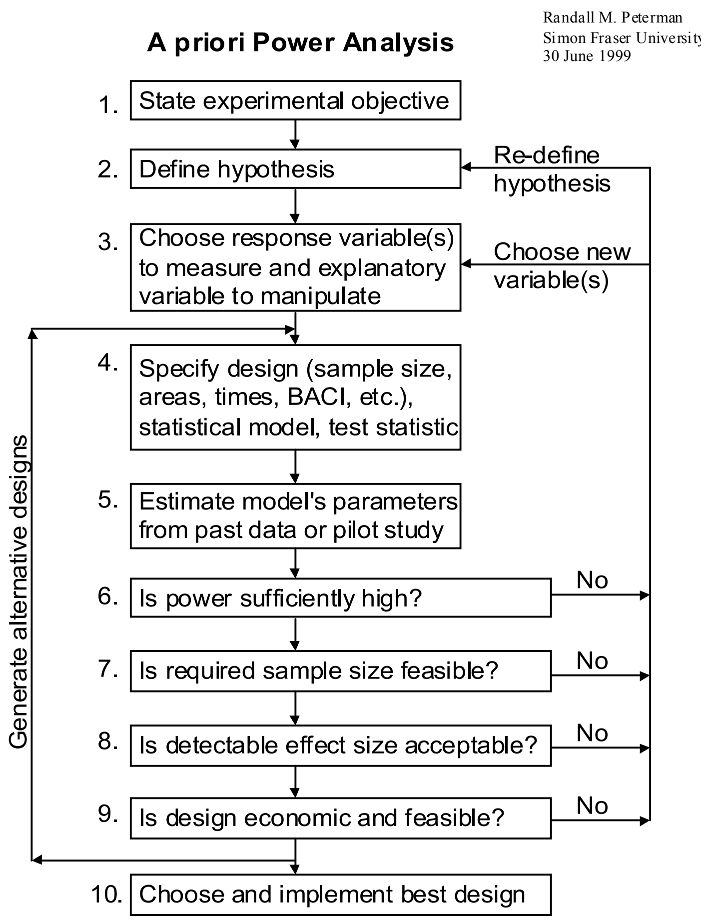

Figure B-1: Iterative process for completing power analyses.

Steps in Power Analysis

This section works through the flow chart (Figure B-1) to illustrate the steps in a power analysis, using the example of a reduction in hatchery releases of steelhead.

1. There must be a clearly stated experimental objective, such as, "reduce the possible effects of hatchery steelhead on extra mortality of Snake River spring/summer chinook."
2. A specific null hypothesis might be that "reduced number of hatchery steelhead smolts does not increase in-river survival rate of wild Snake River spring/summer chinook" (and/or residuals from S-R curve).
3. Next, you could choose a response variable such as survival rate over some particular reaches as estimated by PIT tags (and/or residuals from curve of ln(R/S) vs. S). The explanatory variable to manipulate could be steelhead smolts from a particular set of hatcheries.
4. A specific design might be to estimate survival rates of three lower Columbia stocks to compare with survival rates over the same reaches for five Snake River stocks, using past data from years 19XX-19YY as "before" and 20XX-20YY as "after" estimates. You would then need to write out the statistical model for factors affecting survival rates. A similar model could be written up for S-R residuals. If the design included different numbers of hatchery smolts in different blocks of years (e.g., 10 million for 5 years, 20 million for

another 5, and 40 million for another 5) then the statistical model would permit estimating the "slope" coefficients for the effect of different levels of abundance of hatchery steelhead. This would provide more information than simply comparing mean estimates of survival rates of chinook for high and low steelhead abundances. The test statistic and method of inference for testing the null hypothesis (e.g., t-test, F-test) would depend on the particular statistical model. Of course, violation of assumptions of the particular test would affect power estimates. Also note that here is where you decide whether to use a one-tailed or two-tailed test. Be careful not to assume that a one-tailed test is appropriate when the direction of effect might be uncertain due to some unknown process. For instance, although you might expect that reducing hatchery smolts will increase survival rates of chinook, it is possible that the opposite might occur due to a reduction in alternative prey for predators. If this is a possibility, then use a two-tailed test when calculating power.

5. Given the above statistical model, calculating power requires an estimate of the model's parameters, including variance terms (across space, time, groups of fish, etc.) and a plausible range of effect sizes to investigate. These can often be obtained from past data or a pilot study.
6. Power can be calculated at least four ways. First, Thomas and Krebs (1997) review power analysis software packages that use standard, well-worked-out analytical methods that are associated with standard parametric tests such as t-tests. There are also some packages available on the web. Second, you might do some simple calculations on a spreadsheet and then use a look-up table for the test statistic to determine power. However, if you do this, be very careful to avoid the problem mentioned by Vaughan and van Winkle (1982), who incorrectly used the central, rather than the non-central, t-distribution. Third, power can be estimated by bootstrapping. However, strong warnings are in order here. First, clear justification must be given for which years' and stocks' data are used as the "pool" of observations from which estimates will be drawn. A clear statistical model would help identify the assumptions more explicitly and thereby provide better justification. Second, as many of you know, there are several ways to do bootstrapping. As a reminder, see the brief description in Table 1 (being sent by mail) from Smith, S.J., J.J. Hunt, and D. Rivard (eds.). 1993. Risk Evaluation and Biological Reference Points for Fisheries Management, Canadian Spec. Public. Fish. and Aquatic Sci., 120. The so-called "unconditional-nonparametric" or "naive" bootstrap is where the raw observed data are resampled. However, given the typical, short fisheries data sets, using this type of bootstrapping (and some of the others) will miss sampling the tails of the distribution and hence will underestimate variances and overestimate power. This is a crucial point. If you know the probability distribution of a response variable or its components of variation, you may get better estimates of power using some standard analytical method rather than bootstrapping. Fourth, some mechanistically based stochastic model can simulate a series of outcomes, each of which can be used in the formal hypothesis test. This will produce a proportion of simulated cases in which the null hypothesis was correctly rejected (if in fact, the null was assumed false in the simulation). Bootstrapping is just a special case of this broader class of Monte Carlo simulation, and all the warnings above about bootstrapping apply to Monte Carlo simulation as well.
7. After calculating power for one of your assumed effect sizes in a "what if" type of analysis, you then evaluate the experimental design identified in step 4 above. First, is power adequate for that design? In other words, is the probability of detecting the assumed effect size sufficiently high? Definition of "sufficiently high" varies among people, situations, and risks, but 0.8 is a common goal in environmental management situations. If power is not greater than that goal for any reasonably plausible range of true effect sizes in nature, then you must go back and re-define the hypothesis, choose a different response variable to measure that

might have greater precision, or change the temporal or spatial aspects of the basic design (Figure B-1).

8. If power is adequate, then you must ask whether the sample size used to calculate that power is feasible. If not, then you again must go back to one of the early steps (Figure B-1).
9. Even though you had to assume an effect size in step 6 to calculate power, after you reach this point where power is adequate and the sample size is indeed feasible, you must re-confirm that the level of detectable effect size is acceptable, i.e., that the effect size necessary to generate sufficiently high power is biologically acceptable.
10. As well, you then need to check that the overall design is both economically justified and logistically feasible (i.e., the design's number of stocks, fish groups, and years of sampling are going to be tractable). If it is not, return to an earlier point in Figure B-1 and revise some step.

Steps 1 through 9 should be followed for one design. However, many alternative designs are possible and each of these should be explored. For instance, different designs could mean various numbers of years in either the before or after period, different numbers of fish stocks in the "treated" groups, "control" group, etc. Only after a range of designs are evaluated can managers then choose the best design to implement. Walters and Green (1997) illustrate a quantitative approach to choosing among various designs.

If followed, these general guidelines will help provide reasonable estimates of power for the various PATH experiments being contemplated, thus indicating what might be learned from proposed experiments. As well, they will provide decision-makers with better information for choosing among PATH experiments and will explicitly identify which experiments are less likely to be confounded with unplanned changes in non-experimental parts of the system. Some of the experiments may have low power under a wide range of plausible effect sizes and experimental designs, in which case they should not be pursued. Others may have reasonable power, but they need to meet the other "tests" of suitability described above.

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Appendix C: Optimal Treatment Schedule in a Before-After Experimental Design When There is a Possibility of a Low-frequency Climate Variability Effect

One of the difficulties we encounter while conducting an experiment is a possible intervention such as a regime shift. In a Before-After design, where no Control sites are used, there is a unfortunate possibility that the estimates of climate and the treatment effects will be highly correlated. How do we vary the treatment over the duration of the experiment so that the precision of the estimate of the treatment effect is maximal (minimal standard error), and the correlation between the treatment effect estimate and the climate effect estimate is minimal? This is the fundamental question this paper addresses. Throughout, we are interested in how to gain precision by varying the schedule of treatments for a given length of the experiment, *N*. The models used in this analysis lend themselves to analytical results, so that the standard errors and relevant correlations will be explicitly given in equation form.

We found that with the possibility of a low-frequency climate regime shift, the optimal way to schedule an experiment (when the timing of the shift is not known in advance) is to apply the treatment every other year. This ensures the lowest possible correlation among the estimates of the effects of a regime shift and the experimental treatment. In this paper, we consider experiments that regularly alternate between treatment and no treatment over a fixed experiment duration, *N*. The treatment intervals and non-treatment intervals are of equal length, $P/2$, where *P* is the length (or period) of a treatment/non-treatment cycle and $2 \leq P \leq N$. With any choice of *P* it is possible that the standard error is minimal (or nearly so) if a regime shift occurs at a favorable time during an experiment. However, taking a conservative approach, we are interested in designs that guard against the worst possible timing of a regime shift, thus we calculate the maximum possible standard error associated with a given design by maximizing with respect to possible regime shift timings. We show that the maximum standard error of the treatment effect increases with P. Thus an optimal design is achieved by setting P=2, and the least favorable design, P=N. It turns out that these two cases also correspond to the best and worst designs for minimizing the correlations between the estimates of the regime shift and treatment effects.

Experimental Design

To explore how to design an experiment in the face of a possible regime shift, we formulated a model that included both regime shift and experimental effects. The model was put in regression form:

$$y_t = b_1(t) + b_2(t) + a(t) + \varepsilon_t \qquad \text{[Eq. C-1).}$$

where $b_1(t)=\beta_1$ when $t<=n_1$ (0 otherwise), and $b_2(t)=\beta_2$ when $t>n_1$ (0 otherwise), and $a(t)=\alpha$ (during treatment) and 0 otherwise. In this form, β_1 and β_2 represent the average response during the first and second climate epochs, respectively, and α represents the treatment effect (i.e., the change in response due to treatment). The number of years a treatment is applied during the first climate epoch ($t \leq n_1$) is m_2. The number of years of treatment during the second climate epoch ($t>n_1$) is m_4. The first climate epoch lasts n_1 years, and the second, n_2 years. The first climate epoch lasts n_1 years, and the second, n_2 years. Equation A.1 can be written in standard form y=Xb+e:

$$y = X\beta + \varepsilon = \begin{bmatrix} 1_{n_1} & 0 & E_{m_2} \\ 0 & 1_{n_2} & E_{m_4} \end{bmatrix} \begin{bmatrix} \beta_1 \\ \beta_2 \\ \alpha \end{bmatrix} + \varepsilon , \qquad \text{[Eq. C-2]}$$

where 1_{n1} and 1_{n2} are vectors of all 1s, and are of length n_1 and n_2, respectively. E_{m2} is a vector of length n_1 having m_2 1 entries and n_1-m_2 zero entries. E_{m4} is a vector of length n_2 having m_4 1 entries and n_2-m_4 zero entries.

We seek the optimal number of years of treatment to apply during each climate epoch, and how frequently it should be applied given that we do not know exactly when a climate regime shift will occur. The optimal design will be one in which the standard error of the estimate of the treatment effect and its correlation with the regime shift estimate are minimal.

To examine the standard errors and correlations in detail, we construct the covariance matrix for the parameter estimates:

$$\sigma^2 (X^T X)^{-1} = \sigma^2 \frac{\begin{bmatrix} n_2(m_2+m_4)-m_4{}^2 & m_2 m_4 & -m_2 m_4 \\ m_2 m_4 & n_1(m_2+m_4)-m_2{}^2 & -n_1 m_4 \\ -m_2 n_2 & -n_1 m_4 & n_1 n_2 \end{bmatrix}}{n_1 n_2 (m_2+m_4) - n_1 m_4{}^2 - n_2 m_2{}^2}, \qquad \text{[Eq. C-3)}$$

where σ^2 is the variance of the Gaussian noise variable ε_t. For convenience, we drop the σ^2 term in our variance and covariance calculations because it simply appears as a multiplier. (This is equivalent to assuming the σ^2 =1).

The first problem is to determine how to choose m_2 and m_4 (the number of treatments during the first and second climate epochs, respectively) so that the standard error of the estimate of the treatment effect is as small as possible. From [Eq. C-3], we know that the variance of the estimate of the treatment effect is given by

$$\begin{aligned} \mathrm{var}(\hat{\alpha}) &= \frac{n_1 n_2}{n_1 n_2 (m_2+m_4) - n_1 m_4{}^2 - n_2 m_2{}^2}\,. \qquad \text{[Eq. C-4]} \\ &= \frac{n_1 n_2}{-n_1(m_4-n_2/2)^2 - n_2(m_2-n_1/2)^2 + n_1 n_2{}^2/4 + n_2 n_1{}^2/4} \end{aligned}$$

Notice that we minimize $\mathrm{var}(\hat{\alpha})$ when the denominator is at its maximum value. The denominator is a quadratic form in m_2 and m_4 which achieves its maximum value when the two squared terms are equal to zero (or as close as possible to zero). This occurs when m_2=n_1/2 (when n1 is even) or m_2= (n_1±1)/2 (when n1 is odd) and similarly, m_4=n_2/2 (when n2 is even) or m_4= (n_2±1)/2 (when n2 is odd). Thus, if we wish to minimize the standard error of the estimate of the treatment effect, we choose m_2 and m_4 such that they are exactly half of the duration of the first and second climate epochs, respectively. Any other choice for m_2 and m_4 results in a loss of precision.

The minimal standard error is

$$\mathrm{var}(\hat{\alpha}) = \frac{4}{n_1 + n_2 - \frac{1}{n_1}O(n_1) - \frac{1}{n_2}O(n_2)}, \qquad \text{[Eq. C-5]}$$

where $O(n)$=1 if n is odd 0 when n is even.

Notice that the worst cases for m_2 and m_4 occur when the denominator is minimal. This is achieved at four different points: (m_2=0 or n_1) and (m_1 =0 or n_2), each of which yields infinite standard error. The infinite standard error occurs for three possible reasons: (1) because the regime shift occurs simultaneously with onset or halt of treatment (m_2=0 and m_4=n_2 or m_2=m_1 and m_4=0), (2) there is no treatment effect (m_4=m_2=0),or (3) there is no non-treatment effect (m_2=n_1 and m_4=n_2). Dealing with (2) or (3) is trivial, but (1) may not be. Given that the timing of the regime shift is unknown, the only way to guarantee a minimal standard error is to vary the treatment on-off every other year. This yields the optimal number of treatment years: m_2=n_1/2 if n_1 is even and m_2=(n_1±1)/2 if n_1 is odd, and similarly, m_4=n_2/2 if n_2 is even and m_4=(n_2±1)/2 if n_2 is odd.

Correlation

What happens to the correlation between the estimated regime shift effect, $\hat{\beta}_2 - \hat{\beta}_1$, and the treatment effect, $\hat{\alpha}$ a, when m_2 and m_4 are at their optimal values m_2=n_1/2 and m_4=n_2/2 (assuming n_1 and n_2 are both even)? It is zero. To see this, notice that

$$\operatorname{cov}(\hat{\alpha}, \hat{\beta}_2 - \hat{\beta}_1) = \operatorname{cov}(\hat{\alpha}, \hat{\beta}_2) - \operatorname{cov}(\hat{\alpha}, \hat{\beta}_1) = \frac{-n_1 m_4 + m_2 n_2}{n_1 n_2 (m_2 + m_4) - n_1 {m_4}^2 - n_2 {m_2}^2} \qquad \text{[Eq. C-6]}$$

$$cor(\hat{\alpha}, \hat{\beta}_2 - \hat{\beta}_1) = \frac{\operatorname{cov}(\hat{\alpha}, \hat{\beta}_2 - \hat{\beta}_1)}{\{\operatorname{var}(\hat{\alpha}) \cdot \operatorname{var}(\hat{\beta}_2 - \hat{\beta}_1)\}^{1/2}} = \frac{m_2 n_2 - m_4 n_1}{\{n_1 n_2 (n_1 + n_2 - m_2 - m_4)(m_2 + m_4)\}^{1/2}}$$

which is zero whenever m_2n_2=n_1m_4. This occurs at the optimal choice of the number of treatment years (m_2=n_1/2 and m_4=n_2/2). If $n_1$or n_2 is odd, then the optimal choices for m_2 and m_4 will not necessarily yield a zero correlation in [Eq. C-6]; instead, we can say that the correlation is as close to zero as possible.

Thus, the correlation is zero (or minimal) at the optimal choice for the number of years of treatment, m_2, and m_4. Note that the converse is not true, for it is possible to have $\operatorname{cov}(\hat{\alpha}, \hat{\beta}2 - \hat{\beta}1) = 0$ while $\operatorname{var}(\hat{\alpha})$ is not minimal. However, if we insist that m_2+m_4=(n_1+n_2)/2, so that during the study period exactly half of the time the treatment is applied, then a correlation of zero implies maximal precision of the α estimate.

The worst possible correlation (ρ=-1 or 1) occurs if (m_2=0 and m_4=n_2) or (m_1 =n_2 and m_4=n_1). these are cases that make the standard error of the treatment estimate infinite. They are the cases were there is complete confounding of the treatment and regime shift effects.

Now we consider Before-After designs where the treatments/non treatment intervals cycle regularly with period=*P*. We have already shown that the estimate of the treatment effect has minimum standard error when the period of the experiment is as small as possible (*P*=2). Next we demonstrate that the maximum standard error (maximized with respect to possible regime shift timings) is an increasing function of *P*. This occurs because the maximum standard error is achieved (with respect to n1) whenever n_1=*P*/2 and m_2=*P*/2. This yields variance (square of the standard error).

$$\max \operatorname{var}(\hat{\alpha}) = \frac{4(N - P/2)}{N(N - P)}, \qquad \text{[Eq. C-7]}$$

where 2 ≤P ≤ N. Since the variance is an increasing function of *P* over this range, it will always be best, when faced with different choices of *P*, to choose the *P* that is smallest (ideally, *P*=2). We have assumed here, for convenience, that the period, *P*, divides the number of years of the experiment, N=$n1$+n_2.

The maximum correlation between the estimates of the treatment and regime shift effects is also an increasing function of *P*:

$$\max cor(\hat{\alpha}, \hat{\beta}_2 - \hat{\beta}_1) = \left[\frac{P/2}{N - P/2} \right]^{1/2} \qquad \text{[Eq.C-8]}$$

This correlation is smallest when *P*=2 (high frequency) and largest when *P*=*N* (low frequency). Thus the experimental design that yields the lowest maximum standard error of the treatment effect is the experiment that yields the lowest maximum correlation between the treatment and regime shift effects. Note that when *P*=*N*, then there is the possibility that the correlation is 1, which yields complete confounding between the treatment and regime shift.

Numerical Example

In this numerical example, we choose $N=n_1+n_2=20$, and examine the worst possible standard error that occurs (due to an unfortunate regime shift timing) for all possible treatments that vary according to a regular on-off treatment schedule of period length *P*. For example, an experiment of duration 20 years with a treatment schedule of period *P*=10 years has schedule XXXXXOOOOOXXXXXOOOOO, where the Xs correspond to years of treatment, and the Os to years of no treatment. For convenience, we assume that the period, *P*, divides evenly the duration of the experiment, *N*, and we assume that the first treatment begins during the first year and lasts *P*/2 years. The table of correlations and standard errors when *P*=10 is given in Table C-1 below. Notice that the maximum standard error occurs at $n_1=m_2=P/2=10/2=5$. The maximum correlation, using equation A.7, is sqrt[(10/2)/(20-(10/2))]=sqrt(5/15)=0.5477, and the maximum standard error, using equation A.6, is sqrt{4*(20-10/2)/[20*(20-10)]}=sqrt{4*15/(20*10)}= sqrt(3/10)=0.5477.

Table C-.2 demonstrates that as the period length, *P*, increases, so does the standard error. This minimum standard error occurs when *P*=2 (the treatment is applied every other year), and the maximum standard error occurs when the period of the treatment coincides with the duration of the experiment (*P*=N=20).

Table C-1: Standard errors and parameter estimate correlation when *N*=20. The cycle of treatment/non-treatment is *P*=10 years. σ^2=1 throughout.

n_1	n_2	m_2	m_4	Cor.	S.E.
0	20	0	10	0.0000	0.4472
1	19	1	9	0.2294	0.4595
2	18	2	8	0.3333	0.4743
3	17	3	7	0.4201	0.4928
4	16	4	6	0.5000	0.5164
5	**15**	**5**	**5**	**0.5774**	**0.5477**
6	14	5	5	0.4364	0.4971
7	13	5	5	0.3145	0.4711
8	12	5	5	0.2041	0.4568
9	11	5	5	0.1005	0.4495
10	10	5	5	0.0000	0.4472
11	9	6	4	0.1005	0.4495
12	8	7	3	0.2041	0.4568
13	7	8	2	0.3145	0.4711
14	6	9	1	0.4364	0.4971

n_1	n_2	m_2	m_4	Cor.	S.E.
15	**5**	**10**	**0**	**0.5774**	**0.5477**
16	4	10	0	0.5000	0.5164
17	3	10	0	0.4201	0.4928
18	2	10	0	0.3333	0.4743
19	1	10	0	0.2294	0.4595
20	0	10	0	0.0000	0.4472

Note: The correlation (Cor.) is defined as $cor(\hat{\alpha}, \hat{\beta}_2 - \hat{\beta}_1)$.

Table C-2: Maximum standard errors for different periods. The maximum is taken over all possible regime shift timings. σ^2=1 throughout.

Period	n1	n2	m2	m4	Cor.	S.E.
2	1	19	1	9	0.2294	0.4595
4	2	18	2	8	0.3333	0.4743
10	5	15	5	5	0.5774	0.5477
20	10	10	10	0	1.0000	∞

Note: The correlation (Cor.) is defined as $cor(\hat{\alpha}, \hat{\beta}_2 - \hat{\beta}_1)$.

Autocorrelation, Nonhomogeneous Noise, and Interaction

We further examined the standard error of the treatment estimate when (1) there was autocorrelation in the response variable, (2) the variability of the response was greater during one of the climate epochs, or (3) the level of treatment response can differ between climate epochs.

Autocorrelation

To understand how robust our conclusions are to the assumption of no serial correlation in the residuals, we modified the model in [Eq. C-1], to allow first order autocorrelation:

$$y_t = b_1(t) + b_2(t) + a(t) + N_t \qquad \text{[Eq.C-9]}$$
$$N_t = \phi N_{t-1} + \varepsilon_t$$

It turns out that for this model, the correlation between the regime shift and treatment effect estimates and the standard error of the treatment effect do not depend on β_1,β_2, or α. However, they do depend on ϕ, which is a measure of the degree of autocorrelation in the noise process. As before, we have assumed for convenience that the variance of ε_t is 1.

Table C-3: Maximum standard errors for different periods and different levels of autocorrelation in noise. The maximum is taken over all possible regime shift timings, n_1. σ^2=1 throughout.

	ϕ=0.0*		ϕ=0.2		ϕ=0.4		ϕ=0.6		ϕ=0.8	
Period	**Cor.**	**S.E.**	**Cor.**	**S.E.**	**Cor.**	**S.E.**	**Cor.**	**S.E.**	**Cor.**	**S.E.**
2	0.23	0.46	0.34	0.41	0.33	0.35	-0.32	0.30	-0.32	0.27
4	0.33	0.47	-0.38	0.50	-0.42	0.48	-0.40	0.44	-0.38	0.40
10	0.58	0.55	0.55	0.66	0.54	0.73	-0.55	0.78	-0.65	0.82
20	1.00	∞	1.00	∞	1.00	∞	1.00	∞	1.00	∞

Note: The correlation (Cor.) is defined as $cor(\hat{\alpha}, \hat{\beta}_2 - \hat{\beta}_1)$.

* ϕ is estimated in call cases except when ϕ=0.

We find that with the model that includes autocorrelation, improvements in maximum standard error are gained by reducing the period, *P*, of the treatment/no treatment cycle (Table C-3). Notice also, that the greater the autocorrelation, the greater the benefits of reducing the period, *P*, of the treatment/no treatment cycle.

Unequal Variance

Next we examine the case where the variance of the noise is unequal during different climate epochs. We will assume that the residual variance is 1 during the first epoch and $\sigma_2{}^2$ during the second epoch:

$$y_t = b_1(t) + b_2(t) + a(t) + N_t$$
$$N_t = \begin{cases} \varepsilon_t & if \;\; t \le n1 \\ \varepsilon_t \sigma_2{}^2 & if \;\; t > n1 \end{cases} \qquad \text{[Eq. C-10]}$$

In this case the maximum variance of the treatment effect estimate is given by

$$\max \operatorname{var}(\hat{\alpha}) = \frac{4(N - P/2)}{N(N-P)} \max(1, \sigma_2{}^2) \qquad \text{[Eq. C-11]}$$

Notice once again, that the maximum standard error is smallest when the period of the treatment/non treatment sequence is minimal (Table C-4).

Table C-4: Maximum standard errors for different periods and noise variance during second climate epoch. The maximum is taken over all possible regime shift timings, n_1. σ_1=1 throughout.

Period	**σ₂=1/4**		**σ₂=1/2**		**σ₂=2**		**σ₂=4**	
	Cor.	**S.E.**	**Cor.**	**S.E**	**Cor.**	**S.E.**	**Cor.**	**S.E.**
2	0.58	0.46	0.40	0.46	0.40	0.92	0.58	1.84
4	0.67	0.47	0.53	0.47	0.53	0.95	0.67	1.90
10	0.79	0.55	0.73	0.55	0.73	1.10	0.79	2.19
20	1	∞	1	∞	1	∞	1	∞

Note: The correlation (Cor.) is defined as $cor(\hat{\alpha}, \hat{\beta}_2 - \hat{\beta}_1)$.

Interaction

There is also the possibility that the treatment effect is not additive, so that the treatment effect changes depending on the climate epoch. In this case, we say that there is a treatment × "climate epoch" interaction, and it must be estimated as an additional parameter. In this case, the model is modified to:

$$y_t = b_1(t) + b_2(t) + a(t) + \varepsilon_t \qquad \text{[Eq. C-12]}$$

where $b_1(t)=\beta_1$ when $t<=n_1$ (0 otherwise), and $b_2(t)=\beta_2$ when $t>n_1$ (0 otherwise), and $a(t)=\alpha_1$ during treatment in the first climate epoch, $a(t) = \alpha_2$ during treatment in the second climate epoch, and $a(t)=0$ otherwise. In our original formulation, we assumed that $\alpha_1=\alpha_2$: here we allow them to differ at the expense of an additional parameter. Unfortunately, for this model it is necessary to have at least two climate epochs during the experiment, each of duration at least two years to be able to estimate both treatment effects, α_1 and α_2. If one of the climate epochs lasts less than two years, then it is impossible to estimate a treatment effect during that epoch since it is required to have at least one year of treatment and at least one year of no treatment. Therefore, for whatever treatment/no treatment schedule we devise, there will always be a regime shift timing that makes one of the estimated treatment effects (α_1 or α_2) have infinite standard error. (Contrast this with the case with no treatment × "climate epoch" interaction.).

Therefore, instead of choosing a design that guards against the worst possible regime shift timing, we are interested in a design (call it design*) that minimizes the standard error for each possible regime shift timing (i.e., stderr(design*,n1) <= stderr(design,n1) for all designs and all choices of regime shift timing, n1. We can show that the variances of the treatment estimates are given by:

$$\text{var}(\alpha_1) = \frac{n_1}{-(m_2 - n_1/2)^2 + n_1^{\,2}/4} \quad \text{and}$$

$$\text{var}(\alpha_2) = \frac{n_2}{-(m_4 - n_2/2)^2 + n_2^{\,2}/4} \qquad \text{[Eq. C-13]}$$

Notice that the choices of m_2 and m_4 that minimize these two variances are those that make the quadratic terms in the denominator as close to zero as possible. This occurs at $m_2^*=n_1/2$ and $m_4^*=n_2/2$ (if n_1 is even), and $m_2^*=(n_1\pm1)/2$ and $m_4^*=(n_2\pm1)/2$ if n_1 is odd. These choices for m_2 and m_4 can always be achieved by setting P=2 in the treatment schedule. Thus, once again, the best design is achieved by applying the treatment every other year. The numerical results for different regime shift timings and different treatment/"no treatment" periods when N=2 are given below (Table C-5). Note that the experiment with P=2 is superior to all others.

Table C-5: Standard errors for treatment effect estimates when N=20, at various regime shift timings (n_1) and treatment schedule periods (P) when $\sigma^2=1$.

		S.E. (α_1 estimate)				**S.E. (α_2 estimate)**			
n_1	n_2	***P*=2**	***P*=4**	***P*=10**	***P*=20**	***P*=2**	***P*=4**	***P*=10**	***P*=20**
0	20	∞	∞	∞	∞	0.45	0.45	0.45	0.45
1	19	∞	∞	∞	∞	0.46	0.46	0.46	0.46
2	18	1.41	∞	∞	∞	0.47	0.47	0.47	0.47
3	17	1.22	1.22	∞	∞	0.49	0.49	0.49	0.49
4	16	1.00	1.00	∞	∞	0.50	0.50	0.52	0.52
5	15	0.91	0.91	∞	∞	0.52	0.52	0.55	0.55
6	14	0.82	0.87	1.10	∞	0.53	0.54	0.56	0.59
7	13	0.76	0.76	0.84	∞	0.56	0.56	0.57	0.66
8	12	0.71	0.71	0.73	∞	0.58	0.58	0.59	0.77
9	11	0.67	0.67	0.67	∞	0.61	0.61	0.61	1.05
10	10	0.63	0.65	0.63	∞	0.63	0.65	0.63	∞
11	9	0.61	0.61	0.61	1.05	0.67	0.67	0.67	∞
12	8	0.58	0.58	0.59	0.77	0.71	0.71	0.73	∞
13	7	0.56	0.56	0.57	0.66	0.76	0.76	0.84	∞
14	6	0.53	0.54	0.56	0.59	0.82	0.87	1.10	∞
15	5	0.52	0.52	0.55	0.55	0.91	0.91	∞	∞
16	4	0.50	0.50	0.52	0.52	1.00	1.00	∞	∞
17	3	0.49	0.49	0.49	0.49	1.22	1.22	∞	∞
18	2	0.47	0.47	0.47	0.47	1.41	∞	∞	∞
19	1	0.46	0.46	0.46	0.46	∞	∞	∞	∞
20	0	0.45	0.45	0.45	0.45	∞	∞	∞	∞

Discussion

The results show that when there is a possibility of a confounding environmental or anthropogenic intervention (such as a regime shift) during a Before-After experiment, it is best to vary the experimental treatment in the high-frequency domain, preferably every other year, to increase the precision of the treatment effect estimate. Separating the frequencies of the treatment and climate effects is the key to minimizing the confounding between them and simultaneously maximizing the precision of the estimate of the treatment effect. If instead the treatment is varied in the low-frequency domain, it is possible that the regime shift effects and treatment effects will be completely confounded so that no treatment effect can be estimated. This dire situation occurs whenever an irreversible "experimental" action corresponds to the timing of a climate regime shift.

If there exist control sites (places far enough from the treatment to be unaffected by it), it may be possible to estimate the effect of treatment even if the environmental or anthropogenic intervention occur simultaneously and the treatment is applied only once (i.e., the treatment is applied in the low-frequency time domain), using a Before-After-Control-Impact (BACI) design. However, this relies on the assumption (perhaps unrealistic), that the Control and Impact sites respond the same to a climate/ocean regime shift in the absence of the treatment. Unfortunately, the BACI design confounds the effect of the treatment with other Time × Location interactions, which may, for example, be due to a climate/ocean regime shift. A design called Before-After-Control-Impact Paired Series (BACIPS) may help circumvent this limitation. It uses the difference between the Before and After site differences to provide an estimate of the magnitude of the treatment effect (Osenberg and Schmitt 1996). However, this design also includes assumptions which may be violated, limiting the usefulness of a Control site. Regardless of the design, given that a Control site cannot be expected to yield a reliable estimate of a low-frequency regime shift effect, it will always be optimal to vary the experimental treatment in the high frequency domain, applying the treatment every other year. All else being equal, we conclude (based on our analyses and numerical work) that this schedule will maximize the precision of the estimate of the treatment effect and reduce the confounding between the treatment and regime shift effects.

References:

Osenberg, C.W. and R.J. Schmitt. 1996. Detecting ecological impacts caused by human activities. In: R.J. Schmitt and C.W. Osenberg (eds.). Detecting Ecological Impacts: Concepts and Applications in Coastal Habitats. Academic Press, New York.

Appendix D:
Relating Monitoring Variables to PATH Life Cycle Models

This Appendix describes a framework for relating future monitoring data to PATH life cycle models, thereby helping to test hypotheses regarding the magnitude of responses to management actions. We have so far only developed this framework for the Delta model, but it is worth exploring similar ideas with the Alpha model. With the Delta model,

ln(R/S) = ln(generalized Ricker model) + ln(omega) + ln(Lambda_n) + delta + epsilon [Eq. D-1]

where:

omega = system survival;
Lambda_n = extra mortality;
delta = year effect common to both upstream and downstream stocks; and
epsilon = mixed process error and recruitment measurement error.

We are interested in the degree to which a management action shifts ln (R/S) away from the expected value under average historical conditions. Assume that by expected ln(R/S) we are simply referring to the following:

expected ln(R/S) = ln(generalized Ricker model) + epsilon [Eq. D-2]

In this case, the residuals (RRS) from the Ricker model become:

RRS = ln(omega) + ln(Lambda_n) + delta [Eq. D-3]

These residuals are useful, because they are measurable empirically, and also correspond to terms in the Delta model. Now consider the differences in performance between upstream and downstream stocks. We would like to see if an action changes the performance of upstream stocks relative to downstream stocks. Stocks in both subregions have residuals from their respective Ricker models, that is:

RRS_u = ln(omega_u) + ln(Lambda_n_u) + delta [Eq. D-4]

RRS_d = ln(omega_d) + ln(Lambda_n_d) + delta [Eq. D-5]

Subtracting [Eq. D-5] from [Eq. D-4] removes the common year effect (delta):

RRS_u - RRS_d = ln(omega_u) - ln(omega_d) + ln(Lambda_n_u) - ln(Lambda_n_d) [Eq. D-6]

If we assume that extra mortality (Lambda_n) only applies to upriver stocks, then [Eq. D-6] simplifies to:

RRS_u - RRS_d = ln(omega_u) - ln(V_d) + ln(Lambda_n) [Eq. D-7]

Though it would be nice to know whether an improvement due to an action occurs in system survival or extra mortality, the most important thing to know is that (RRS_u - RRS_d) is positive (i.e., the status of Snake River stocks is improving relative to downriver stocks). As long as we can see an improvement in ln(omega_u) + ln(Lambda_n) then we can say there is improvement due to the action and that improvement is measurable.

Recall from the PATH Preliminary Decision Analysis (pg. A-93) that:

m = ln(omega_u) + ln(Lambda_n) [Eq. D-8]

where:

m = total mortality rate, including both passage and extra mortality.

Therefore, to assess the response of the system to a management action, we measure "m" and see how much it changes. That is

RRS_u - RRS_d = m - ln(V_d) [Eq. D-9]

Therefore, a comparison of [b]efore versus [a]fter conditions would attempt to measure the change in the upstream-downstream differences in residuals, that is:

{RRS_u - RRS_d}[a] - {RRS_u - RRS_d}[b] = m[a] – m[b] - ln(V_d[a]/V_d[b]) [Eq. D-10]

Therefore the only thing we need to factor out is changes in the in-river survival of down-river stocks (V_d[a]/V_d[b]). Then we can directly measure the net benefit of an action in terms of m_a - m_b.

As to the question of where these net benefits occurred (i.e., in improved system survival, or in improved post-Bonneville survival), this becomes more difficult to determine. Using equation [7] to expand m_a - m_b, one obtains:

m_a - m_b = ln(omega_u[a]) - ln(omega_u[b]) + ln(Lambda_n[a]) - ln(Lambda_n[b]) [Eq. D-11]

or, the after-before difference in system survival plus the after-before difference in post-Bonneville survival. The after-before difference in system survival in turn can be expanded to:

ln(omega_u[a]) - ln(omega_u[b]) = {-M[a] + ln (D[a]P[a] + 1 – P[a])} – {-M[b] + ln (D[b]P[b] + 1 – P[b]) [Eq. D-12]

which can be re-arranged to:

ln(omega_u[a]) - ln(omega_u[b]) = {M[b] - M[a]} + {ln (D[a]P[a] + 1 – P[a]) – ln (D[b]P[b] + 1 – P[b])} [Eq. D-13]

Thus, determining the change in system survival requires estimates of M, D and P, both before and after the experiment. Errors in estimating these quantities (particularly D) are likely to make it very difficult to get accurate estimates of the change in system survival. If an experimental treatment discontinues transportation (i.e., P[b] = 0), then the last term in [Eq. D-13] drops out, which simplifies matters somewhat, but one still needs to know M, D and P for the period before the experiment.

Smolt to Adult Returns

We can model SAR's in a similar manner. Considering first the SARs to Bonneville, recall that BSM computes:

ln(SAR_u) = constant_u + ln(omega) + ln(Lambda_n) + year-effect [Eq. D-14]

so that ln(SAR_u) looks a lot like ln(R/S_u). There is the constant_u term that is a scalar which we have measurements of from our previous work [in principle the constant_u = logarithm of ocean survival, excluding any common year-effect]. So let's assume that the constant is known, in which case define,

ln(scaled SAR) = ln(SAR) - constant_u [Eq. D-15]

Now tag fish up-river and down-river and measure:

ln(scaled SAR_u) - ln(scaled SAR_d) = m - ln(V_d) [Eq. D-16]

This looks just like the result obtained above in [Eq. D-9]; it assumes that upstream and downstream stocks have similar ocean mortality. So tagging should in principle be an alternative way to get at "m". The SAR data involve fewer unknown coefficients, since the egg to smolt survival is not part of [Eq. D-14]. Therefore there is one less source of variation. However, for actions like carcass fertilization one would need to consider this part of the life cycle.

Appendix E:
Effects of D values on Survival and Recovery Probabilities – Detailed Results for Spring/Summer Chinook

In the PATH modeling analysis, "D" is defined as the ratio of post-Bonneville survival rate of transported fish to that of non-transported fish. Because most Snake River chinook juveniles are transported, estimates of the relative post-Bonneville survival of transported and non-transported fish are important in determining the relative efficacy of hydropower actions relying on smolt transportation (i.e., alternative actions A1, A2, and A2').

D values for spring/summer chinook are estimated using transport:control ratios (TCRs) developed through transport experiments. To date, there have been numerous analyses of TCRs, which have resulted in a range of estimated D values, both retrospective (what D was in the past) and prospective (what D will be in the future).

Given the uncertainties surrounding D, a subgroup of PATH participants decided to conduct a sensitivity analysis of prospective and retrospective D values ranging from 0 to 1, including all possible retrospective/prospective combinations. This Appendix describes the procedure used to do this, and provides detailed results. This material supplements the summary tables and figures provided in Section 2.2.2.

Procedure

The PATH subgroup specified a range of D values from 0 to 1, in increments of 0.1. This equates to 121 combinations of retrospective and prospective D values. To minimize the number of runs required, we selected the following subset of assumptions to model. There are twelve assumption sets for each action (2 passage models X 2 life-cycle models X 3 extra mortality hypotheses). The subset was based largely on the CART tree analyses in the FY98 Report.

Assumption	Subset used in sensitivity analysis
Actions	A2, A3 (3-year pre-removal), A3 (8-year pre-removal)
Passage models	CRiSP and FLUSH
Passage and drawdown assumptions	One set per passage model and action (6 in total)
Life-cycle models	Alpha and Delta
Extra mortality / climate	BKD / Cyclical, Hydro / Cyclical, Regime Shift / Cyclical

Notes:

1) A2 and A3 were selected as representative of transport and drawdown actions, respectively. A1 results are similar to A2, and B1 results are similar to A3.
2) The single set of passage assumptions selected for each passage model and action was the one that produced an average 48-year recovery probability that was closest to the average over all passage assumptions. The set of assumptions were (see FY98 report for description of codes):

Action	Passage Model	FGE	TURB	PREM	EJUV	TJUV
A2	FLUSH	2	5	3	n/a	n/a
	CRiSP	1	5	1	n/a	n/a
A3(3)	FLUSH	2	4	3	2	b
	CRiSP	1	5	3	1	a
A3(8)	FLUSH	2	5	3	1	b
	CRiSP	2	4	1	2	b

3) Future climate scenarios were not were important in determining outcomes, so the choice of which climate scenario to use was arbitrary. We used the cyclical future climate scenario because this was the only one that was run in conjunction with all three extra mortality hypotheses. This avoided potential problems with assigning equal weights to extra mortality hypotheses.

List of Tables in this Appendix

Note: Results for the Hydro II hypothesis can be approximated by using the SV results for A2, and the Hydro I results for A3.

Table E.1: Effects of D values on 24-year Survival Probabilities

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
CRiSP	SV	A2	0	0.06	0.06	0.05	0.05	0.06	0.06	0.05	0.05	0.05	0.05	0.05	0.15	0.14	0.15	0.15	0.15	0.15	0.14	0.15	0.15	0.15	0.15
			0.1	0.35	0.37	0.36	0.35	0.37	0.37	0.35	0.36	0.35	0.36	0.36	0.42	0.41	0.41	0.42	0.41	0.41	0.41	0.41	0.42	0.42	0.41
			0.2	0.53	0.53	0.53	0.54	0.54	0.54	0.53	0.54	0.54	0.53	0.55	0.51	0.50	0.50	0.52	0.51	0.49	0.49	0.51	0.50	0.52	0.50
			0.3	0.63	0.60	0.60	0.59	0.59	0.60	0.59	0.63	0.64	0.62	0.61	0.55	0.54	0.55	0.53	0.54	0.55	0.54	0.53	0.54	0.54	0.55
			0.4	0.63	0.62	0.63	0.66	0.63	0.65	0.64	0.62	0.64	0.63	0.66	0.56	0.57	0.57	0.58	0.57	0.58	0.54	0.56	0.58	0.56	0.58
			0.5	0.67	0.66	0.65	0.65	0.63	0.65	0.64	0.66	0.64	0.67	0.63	0.61	0.59	0.58	0.58	0.58	0.58	0.58	0.59	0.58	0.57	0.59
			0.6	0.70	0.69	0.69	0.68	0.66	0.68	0.67	0.66	0.66	0.67	0.68	0.61	0.58	0.60	0.60	0.60	0.61	0.58	0.60	0.60	0.58	0.60
			0.7	0.67	0.70	0.70	0.69	0.67	0.70	0.69	0.67	0.68	0.69	0.67	0.62	0.59	0.61	0.62	0.60	0.62	0.60	0.62	0.61	0.60	0.60
			0.8	0.67	0.69	0.70	0.72	0.69	0.71	0.70	0.68	0.69	0.69	0.68	0.62	0.63	0.63	0.61	0.61	0.62	0.62	0.63	0.61	0.60	0.62
			0.9	0.69	0.69	0.70	0.69	0.71	0.69	0.69	0.68	0.71	0.71	0.69	0.64	0.63	0.62	0.63	0.61	0.62	0.63	0.62	0.62	0.62	0.63
			1	0.69	0.69	0.71	0.69	0.71	0.71	0.69	0.71	0.69	0.71	0.71	0.63	0.63	0.63	0.63	0.64	0.62	0.62	0.64	0.62	0.64	0.60
		A3(3)	0	0.53	0.54	0.55	0.53	0.54	0.55	0.55	0.57	0.55	0.55	0.55	0.64	0.64	0.63	0.63	0.64	0.63	0.63	0.63	0.63	0.62	0.63
			0.1	0.69	0.71	0.68	0.68	0.67	0.67	0.70	0.69	0.70	0.69	0.70	0.71	0.71	0.71	0.71	0.71	0.71	0.70	0.71	0.71	0.71	0.71
			0.2	0.72	0.75	0.72	0.74	0.73	0.74	0.74	0.75	0.73	0.74	0.73	0.73	0.72	0.72	0.72	0.72	0.72	0.71	0.72	0.72	0.72	0.73
			0.3	0.75	0.73	0.75	0.76	0.76	0.75	0.75	0.75	0.75	0.74	0.76	0.72	0.72	0.71	0.72	0.72	0.72	0.72	0.72	0.73	0.72	0.72
			0.4	0.75	0.74	0.75	0.75	0.74	0.78	0.75	0.74	0.76	0.76	0.75	0.72	0.71	0.72	0.72	0.71	0.71	0.71	0.70	0.70	0.72	0.72
			0.5	0.75	0.74	0.75	0.74	0.73	0.75	0.74	0.75	0.74	0.76	0.74	0.70	0.70	0.69	0.69	0.69	0.70	0.70	0.70	0.71	0.70	0.69
			0.6	0.74	0.76	0.75	0.76	0.75	0.75	0.72	0.76	0.73	0.73	0.75	0.70	0.68	0.67	0.69	0.69	0.68	0.69	0.70	0.69	0.69	0.68
			0.7	0.72	0.73	0.74	0.72	0.69	0.72	0.74	0.72	0.72	0.74	0.73	0.67	0.68	0.67	0.67	0.68	0.68	0.68	0.68	0.67	0.68	0.69
			0.8	0.73	0.72	0.74	0.73	0.72	0.75	0.71	0.73	0.72	0.73	0.74	0.67	0.65	0.65	0.67	0.66	0.67	0.66	0.66	0.66	0.66	0.64
			0.9	0.72	0.71	0.72	0.71	0.71	0.70	0.71	0.72	0.71	0.73	0.70	0.64	0.65	0.64	0.63	0.64	0.63	0.65	0.64	0.64	0.64	0.65
			1	0.72	0.73	0.72	0.71	0.71	0.73	0.71	0.72	0.72	0.71	0.70	0.63	0.63	0.60	0.62	0.63	0.61	0.63	0.64	0.63	0.62	0.62
		A3(8)	0	0.47	0.47	0.46	0.45	0.47	0.46	0.50	0.46	0.47	0.48	0.47	0.64	0.65	0.64	0.63	0.66	0.64	0.63	0.65	0.65	0.64	0.65
			0.1	0.60	0.63	0.59	0.60	0.62	0.61	0.63	0.61	0.62	0.61	0.61	0.66	0.66	0.66	0.65	0.66	0.66	0.65	0.66	0.66	0.66	0.66
			0.2	0.65	0.67	0.65	0.65	0.65	0.65	0.64	0.65	0.65	0.66	0.66	0.66	0.65	0.65	0.66	0.66	0.64	0.64	0.66	0.65	0.66	0.65
			0.3	0.65	0.66	0.65	0.66	0.65	0.68	0.66	0.67	0.65	0.67	0.67	0.64	0.64	0.64	0.62	0.63	0.64	0.63	0.62	0.64	0.63	0.64
			0.4	0.65	0.64	0.67	0.67	0.64	0.66	0.66	0.64	0.66	0.66	0.69	0.62	0.62	0.62	0.64	0.63	0.63	0.59	0.61	0.63	0.61	0.63
			0.5	0.64	0.65	0.67	0.64	0.63	0.64	0.64	0.63	0.63	0.67	0.64	0.63	0.62	0.61	0.61	0.60	0.61	0.61	0.62	0.60	0.60	0.62
			0.6	0.67	0.66	0.67	0.65	0.63	0.65	0.65	0.63	0.65	0.65	0.66	0.61	0.58	0.60	0.61	0.60	0.61	0.58	0.60	0.60	0.58	0.60
			0.7	0.62	0.65	0.66	0.65	0.62	0.67	0.64	0.63	0.65	0.65	0.63	0.60	0.57	0.60	0.60	0.58	0.60	0.58	0.60	0.60	0.58	0.58
			0.8	0.62	0.62	0.65	0.65	0.63	0.66	0.64	0.64	0.62	0.62	0.63	0.58	0.59	0.59	0.57	0.57	0.59	0.59	0.59	0.58	0.56	0.58
			0.9	0.62	0.62	0.62	0.62	0.63	0.63	0.63	0.61	0.66	0.64	0.63	0.58	0.57	0.57	0.57	0.55	0.57	0.57	0.56	0.57	0.57	0.58
			1	0.60	0.60	0.62	0.61	0.64	0.63	0.62	0.62	0.62	0.64	0.63	0.56	0.57	0.56	0.56	0.58	0.55	0.55	0.57	0.55	0.57	0.54
	Hydro	A2	0	0.06	0.52	0.75	0.84	0.88	0.90	0.91	0.92	0.92	0.93	0.93	0.13	0.70	0.84	0.89	0.92	0.92	0.93	0.93	0.93	0.93	0.93
			0.1	0.04	0.38	0.64	0.77	0.84	0.87	0.89	0.90	0.91	0.92	0.92	0.06	0.44	0.71	0.81	0.86	0.89	0.90	0.91	0.92	0.92	0.93

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.2	0.03	0.25	0.54	0.70	0.78	0.84	0.86	0.88	0.89	0.91	0.91	0.04	0.32	0.59	0.74	0.81	0.84	0.88	0.89	0.90	0.91	0.92
			0.3	0.02	0.22	0.49	0.62	0.73	0.80	0.83	0.87	0.88	0.89	0.90	0.03	0.24	0.49	0.64	0.74	0.82	0.85	0.87	0.89	0.90	0.91
			0.4	0.02	0.16	0.41	0.61	0.68	0.77	0.80	0.83	0.86	0.88	0.89	0.02	0.20	0.42	0.60	0.70	0.78	0.80	0.84	0.87	0.88	0.89
			0.5	0.01	0.13	0.36	0.51	0.62	0.72	0.77	0.81	0.83	0.86	0.86	0.02	0.15	0.38	0.54	0.64	0.73	0.78	0.83	0.85	0.86	0.88
			0.6	0.01	0.09	0.30	0.48	0.58	0.70	0.75	0.78	0.82	0.84	0.86	0.02	0.13	0.34	0.49	0.64	0.71	0.76	0.81	0.85	0.85	0.88
			0.7	0.01	0.08	0.25	0.44	0.55	0.65	0.73	0.76	0.80	0.83	0.84	0.02	0.12	0.31	0.47	0.58	0.70	0.73	0.80	0.83	0.84	0.86
			0.8	0.01	0.07	0.24	0.42	0.52	0.63	0.70	0.74	0.78	0.81	0.82	0.02	0.12	0.28	0.44	0.54	0.67	0.73	0.78	0.80	0.83	0.85
			0.9	0.01	0.05	0.18	0.36	0.50	0.57	0.66	0.71	0.77	0.81	0.81	0.01	0.09	0.26	0.41	0.52	0.61	0.71	0.75	0.79	0.82	0.85
			1	0.01	0.05	0.18	0.33	0.46	0.56	0.61	0.70	0.73	0.78	0.79	0.01	0.09	0.24	0.38	0.53	0.60	0.68	0.74	0.76	0.81	0.82
		A3(3)	0	0.52	0.75	0.84	0.87	0.90	0.91	0.92	0.92	0.92	0.93	0.93	0.62	0.81	0.87	0.90	0.91	0.92	0.93	0.93	0.94	0.94	0.94
			0.1	0.48	0.72	0.78	0.84	0.86	0.88	0.90	0.91	0.92	0.92	0.93	0.52	0.73	0.80	0.86	0.88	0.90	0.91	0.92	0.92	0.93	0.93
			0.2	0.44	0.66	0.73	0.81	0.84	0.87	0.88	0.90	0.90	0.91	0.92	0.47	0.68	0.77	0.82	0.85	0.88	0.89	0.90	0.91	0.92	0.92
			0.3	0.41	0.60	0.70	0.78	0.83	0.85	0.87	0.88	0.89	0.90	0.91	0.44	0.62	0.73	0.78	0.83	0.86	0.88	0.89	0.90	0.91	0.91
			0.4	0.38	0.55	0.67	0.75	0.79	0.85	0.85	0.86	0.88	0.89	0.90	0.42	0.59	0.71	0.77	0.82	0.84	0.86	0.88	0.89	0.90	0.91
			0.5	0.32	0.55	0.65	0.71	0.76	0.80	0.83	0.85	0.87	0.88	0.89	0.40	0.57	0.66	0.75	0.79	0.83	0.85	0.87	0.88	0.89	0.90
			0.6	0.31	0.54	0.63	0.72	0.76	0.79	0.81	0.85	0.84	0.86	0.88	0.38	0.55	0.65	0.73	0.78	0.81	0.83	0.86	0.87	0.89	0.89
			0.7	0.31	0.50	0.60	0.66	0.70	0.76	0.81	0.82	0.83	0.86	0.87	0.36	0.55	0.63	0.71	0.76	0.79	0.83	0.86	0.87	0.88	0.89
			0.8	0.27	0.49	0.59	0.66	0.72	0.77	0.77	0.82	0.83	0.85	0.86	0.35	0.52	0.62	0.71	0.75	0.81	0.82	0.83	0.86	0.87	0.87
			0.9	0.25	0.46	0.56	0.64	0.68	0.73	0.78	0.81	0.82	0.84	0.85	0.32	0.52	0.61	0.69	0.73	0.78	0.81	0.82	0.85	0.86	0.87
			1	0.25	0.44	0.56	0.63	0.67	0.75	0.75	0.79	0.80	0.82	0.84	0.33	0.51	0.60	0.66	0.73	0.76	0.80	0.83	0.84	0.86	0.87
		A3(8)	0	0.47	0.70	0.81	0.85	0.89	0.89	0.91	0.91	0.92	0.93	0.93	0.62	0.82	0.87	0.90	0.92	0.93	0.93	0.93	0.93	0.93	0.93
			0.1	0.40	0.63	0.73	0.80	0.86	0.87	0.89	0.89	0.91	0.91	0.92	0.47	0.67	0.78	0.83	0.87	0.89	0.90	0.91	0.92	0.92	0.93
			0.2	0.34	0.53	0.66	0.74	0.80	0.84	0.86	0.88	0.88	0.90	0.90	0.38	0.58	0.71	0.78	0.83	0.84	0.88	0.89	0.90	0.91	0.91
			0.3	0.29	0.49	0.62	0.71	0.75	0.82	0.83	0.86	0.86	0.88	0.89	0.32	0.51	0.64	0.70	0.77	0.82	0.85	0.87	0.88	0.89	0.90
			0.4	0.21	0.44	0.57	0.68	0.70	0.77	0.81	0.82	0.85	0.86	0.87	0.28	0.48	0.58	0.68	0.73	0.79	0.80	0.84	0.86	0.87	0.88
			0.5	0.20	0.39	0.53	0.60	0.66	0.73	0.77	0.78	0.81	0.85	0.84	0.26	0.43	0.55	0.63	0.69	0.74	0.78	0.82	0.84	0.85	0.87
			0.6	0.16	0.34	0.48	0.57	0.63	0.72	0.75	0.76	0.81	0.82	0.85	0.23	0.39	0.51	0.59	0.68	0.72	0.75	0.80	0.83	0.84	0.86
			0.7	0.14	0.32	0.44	0.55	0.61	0.68	0.71	0.74	0.79	0.81	0.81	0.21	0.38	0.49	0.58	0.64	0.72	0.73	0.79	0.81	0.82	0.84
			0.8	0.10	0.27	0.42	0.51	0.58	0.66	0.70	0.74	0.75	0.77	0.81	0.19	0.36	0.46	0.55	0.60	0.69	0.73	0.77	0.79	0.81	0.83
			0.9	0.09	0.21	0.38	0.46	0.56	0.61	0.66	0.69	0.75	0.78	0.79	0.18	0.33	0.45	0.52	0.59	0.64	0.71	0.73	0.78	0.80	0.83
			1	0.09	0.22	0.36	0.46	0.53	0.59	0.63	0.68	0.71	0.76	0.76	0.16	0.31	0.42	0.49	0.59	0.62	0.68	0.73	0.74	0.79	0.80
	Reg. Shift	A2	0	0.06	0.06	0.05	0.05	0.06	0.06	0.06	0.05	0.05	0.05	0.05	0.12	0.11	0.12	0.12	0.12	0.11	0.11	0.11	0.12	0.11	0.12
			0.1	0.35	0.38	0.36	0.36	0.37	0.37	0.37	0.36	0.36	0.37	0.36	0.38	0.38	0.38	0.38	0.37	0.38	0.38	0.38	0.38	0.38	0.38
			0.2	0.55	0.54	0.54	0.55	0.56	0.55	0.55	0.55	0.54	0.54	0.56	0.52	0.52	0.52	0.54	0.53	0.50	0.51	0.53	0.52	0.54	0.52
			0.3	0.65	0.63	0.63	0.61	0.62	0.62	0.62	0.65	0.66	0.63	0.63	0.59	0.59	0.60	0.56	0.57	0.60	0.58	0.57	0.59	0.58	0.59
			0.4	0.66	0.65	0.67	0.69	0.66	0.68	0.67	0.65	0.68	0.66	0.69	0.61	0.63	0.63	0.64	0.63	0.64	0.59	0.61	0.64	0.62	0.64
			0.5	0.70	0.70	0.69	0.69	0.67	0.69	0.69	0.69	0.68	0.70	0.67	0.67	0.65	0.65	0.65	0.64	0.64	0.64	0.65	0.64	0.64	0.66

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.6	0.73	0.73	0.72	0.72	0.70	0.72	0.71	0.70	0.70	0.71	0.71	0.68	0.65	0.67	0.67	0.67	0.67	0.65	0.67	0.67	0.65	0.67
			0.7	0.71	0.74	0.74	0.73	0.71	0.73	0.73	0.72	0.73	0.73	0.71	0.69	0.66	0.69	0.69	0.68	0.69	0.67	0.69	0.69	0.67	0.67
			0.8	0.71	0.73	0.75	0.75	0.73	0.75	0.74	0.73	0.73	0.73	0.72	0.70	0.70	0.70	0.68	0.68	0.70	0.69	0.70	0.69	0.68	0.69
			0.9	0.73	0.73	0.74	0.73	0.75	0.73	0.73	0.73	0.75	0.75	0.73	0.71	0.70	0.70	0.70	0.68	0.70	0.70	0.69	0.69	0.70	0.71
			1	0.73	0.74	0.75	0.73	0.75	0.75	0.74	0.75	0.73	0.75	0.75	0.71	0.71	0.70	0.70	0.72	0.69	0.70	0.71	0.69	0.71	0.68
		A3(3)	0	0.52	0.54	0.56	0.53	0.54	0.55	0.55	0.57	0.55	0.55	0.55	0.64	0.64	0.62	0.63	0.63	0.63	0.63	0.62	0.63	0.61	0.63
			0.1	0.69	0.72	0.68	0.68	0.67	0.67	0.71	0.70	0.70	0.68	0.70	0.71	0.71	0.71	0.71	0.71	0.71	0.70	0.71	0.72	0.71	0.71
			0.2	0.73	0.75	0.73	0.74	0.73	0.74	0.74	0.75	0.73	0.75	0.74	0.73	0.73	0.73	0.73	0.72	0.72	0.72	0.73	0.72	0.73	0.74
			0.3	0.75	0.74	0.75	0.76	0.77	0.75	0.76	0.75	0.76	0.75	0.76	0.73	0.73	0.72	0.73	0.73	0.73	0.73	0.73	0.74	0.73	0.73
			0.4	0.76	0.75	0.76	0.76	0.75	0.79	0.76	0.75	0.76	0.77	0.77	0.74	0.73	0.74	0.73	0.73	0.73	0.72	0.72	0.72	0.73	0.74
			0.5	0.77	0.75	0.76	0.75	0.75	0.76	0.76	0.76	0.76	0.77	0.75	0.73	0.73	0.72	0.72	0.72	0.73	0.73	0.73	0.74	0.73	0.72
			0.6	0.77	0.78	0.77	0.77	0.76	0.77	0.74	0.78	0.75	0.75	0.77	0.74	0.72	0.72	0.73	0.73	0.72	0.73	0.73	0.73	0.73	0.72
			0.7	0.75	0.75	0.76	0.75	0.72	0.74	0.77	0.75	0.74	0.77	0.76	0.73	0.73	0.72	0.73	0.73	0.72	0.73	0.73	0.73	0.73	0.74
			0.8	0.76	0.76	0.77	0.76	0.76	0.77	0.74	0.76	0.75	0.76	0.77	0.73	0.71	0.72	0.73	0.72	0.73	0.72	0.72	0.72	0.72	0.70
			0.9	0.76	0.75	0.76	0.75	0.75	0.75	0.75	0.76	0.76	0.77	0.74	0.71	0.72	0.71	0.71	0.72	0.71	0.72	0.71	0.71	0.72	0.72
			1	0.76	0.78	0.76	0.76	0.75	0.78	0.75	0.76	0.76	0.75	0.75	0.71	0.72	0.68	0.70	0.72	0.70	0.72	0.73	0.71	0.71	0.71
		A3(8)	0	0.47	0.47	0.47	0.46	0.48	0.46	0.50	0.46	0.47	0.48	0.48	0.62	0.63	0.62	0.61	0.64	0.62	0.61	0.63	0.63	0.62	0.63
			0.1	0.60	0.63	0.59	0.60	0.62	0.61	0.64	0.61	0.61	0.62	0.61	0.66	0.66	0.66	0.64	0.66	0.66	0.65	0.65	0.66	0.65	0.66
			0.2	0.65	0.67	0.65	0.66	0.66	0.65	0.65	0.66	0.65	0.67	0.67	0.67	0.66	0.66	0.67	0.67	0.65	0.66	0.67	0.66	0.68	0.66
			0.3	0.67	0.67	0.67	0.68	0.66	0.69	0.68	0.69	0.67	0.68	0.69	0.67	0.66	0.67	0.64	0.65	0.67	0.66	0.65	0.66	0.66	0.67
			0.4	0.67	0.67	0.70	0.70	0.66	0.69	0.68	0.67	0.69	0.68	0.71	0.65	0.66	0.66	0.67	0.66	0.67	0.63	0.65	0.67	0.65	0.67
			0.5	0.68	0.69	0.70	0.67	0.67	0.67	0.68	0.67	0.66	0.70	0.67	0.68	0.67	0.66	0.66	0.65	0.66	0.66	0.67	0.66	0.65	0.67
			0.6	0.71	0.71	0.71	0.70	0.67	0.70	0.69	0.67	0.70	0.69	0.70	0.68	0.64	0.66	0.67	0.67	0.67	0.65	0.67	0.67	0.65	0.67
			0.7	0.68	0.70	0.71	0.70	0.68	0.71	0.69	0.68	0.70	0.70	0.68	0.67	0.65	0.67	0.68	0.66	0.67	0.65	0.68	0.67	0.66	0.66
			0.8	0.68	0.68	0.71	0.71	0.69	0.71	0.70	0.70	0.68	0.68	0.69	0.67	0.68	0.68	0.66	0.66	0.68	0.67	0.68	0.66	0.66	0.67
			0.9	0.68	0.68	0.70	0.67	0.70	0.69	0.69	0.68	0.71	0.71	0.70	0.68	0.67	0.67	0.67	0.65	0.66	0.67	0.66	0.66	0.67	0.68
			1	0.67	0.68	0.70	0.68	0.71	0.70	0.69	0.69	0.69	0.71	0.69	0.67	0.67	0.67	0.67	0.68	0.66	0.66	0.68	0.65	0.67	0.65
FLUSH	SV	A2	0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
			0.1	0.15	0.15	0.14	0.15	0.15	0.14	0.15	0.15	0.15	0.15	0.15	0.38	0.38	0.38	0.37	0.38	0.39	0.38	0.38	0.37	0.37	0.38
			0.2	0.37	0.38	0.37	0.37	0.36	0.38	0.36	0.37	0.38	0.38	0.37	0.47	0.46	0.45	0.48	0.45	0.45	0.46	0.46	0.45	0.46	0.47
			0.3	0.51	0.50	0.50	0.52	0.52	0.51	0.52	0.51	0.51	0.50	0.49	0.50	0.50	0.49	0.50	0.51	0.49	0.50	0.50	0.50	0.51	0.51
			0.4	0.57	0.57	0.57	0.58	0.57	0.57	0.58	0.57	0.57	0.58	0.57	0.52	0.52	0.54	0.54	0.51	0.52	0.54	0.52	0.54	0.52	0.53
			0.5	0.57	0.60	0.58	0.59	0.59	0.58	0.58	0.59	0.60	0.59	0.59	0.53	0.54	0.52	0.53	0.54	0.53	0.52	0.54	0.53	0.54	0.53
			0.6	0.58	0.57	0.58	0.59	0.58	0.59	0.58	0.59	0.59	0.58	0.57	0.54	0.54	0.56	0.55	0.56	0.52	0.53	0.55	0.53	0.55	0.57
			0.7	0.60	0.60	0.57	0.57	0.58	0.58	0.58	0.58	0.62	0.59	0.60	0.56	0.54	0.55	0.54	0.55	0.54	0.56	0.54	0.54	0.56	0.56
			0.8	0.62	0.61	0.57	0.58	0.59	0.59	0.58	0.57	0.59	0.59	0.58	0.54	0.56	0.56	0.56	0.57	0.54	0.56	0.56	0.54	0.56	0.57
			0.9	0.60	0.59	0.59	0.59	0.62	0.60	0.60	0.59	0.58	0.59	0.59	0.54	0.57	0.57	0.55	0.57	0.55	0.57	0.57	0.55	0.55	0.57

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			1	0.60	0.60	0.61	0.59	0.61	0.60	0.60	0.59	0.60	0.58	0.58	0.58	0.58	0.55	0.56	0.57	0.57	0.58	0.56	0.55	0.57	0.58
		A3(3)	0	0.54	0.56	0.55	0.56	0.56	0.54	0.56	0.53	0.54	0.56	0.54	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75
			0.1	0.61	0.63	0.60	0.61	0.61	0.63	0.63	0.61	0.62	0.61	0.61	0.74	0.74	0.74	0.73	0.74	0.74	0.73	0.74	0.74	0.74	0.74
			0.2	0.63	0.64	0.65	0.65	0.65	0.64	0.64	0.65	0.65	0.65	0.65	0.72	0.71	0.72	0.72	0.72	0.70	0.71	0.72	0.71	0.72	0.71
			0.3	0.66	0.66	0.67	0.65	0.66	0.67	0.66	0.65	0.67	0.66	0.66	0.69	0.68	0.69	0.67	0.68	0.69	0.68	0.67	0.68	0.68	0.69
			0.4	0.65	0.65	0.64	0.63	0.65	0.65	0.65	0.64	0.63	0.64	0.65	0.64	0.65	0.65	0.66	0.66	0.66	0.62	0.64	0.66	0.64	0.66
			0.5	0.61	0.61	0.61	0.59	0.59	0.61	0.60	0.61	0.61	0.62	0.62	0.64	0.63	0.62	0.62	0.61	0.62	0.62	0.62	0.62	0.61	0.63
			0.6	0.58	0.57	0.58	0.56	0.58	0.57	0.57	0.55	0.57	0.58	0.57	0.60	0.57	0.59	0.60	0.60	0.60	0.57	0.59	0.59	0.57	0.59
			0.7	0.54	0.55	0.56	0.55	0.55	0.55	0.55	0.56	0.55	0.53	0.56	0.58	0.55	0.57	0.57	0.56	0.57	0.56	0.58	0.57	0.55	0.56
			0.8	0.53	0.52	0.52	0.52	0.51	0.53	0.52	0.52	0.50	0.54	0.52	0.54	0.55	0.55	0.53	0.53	0.54	0.54	0.55	0.53	0.52	0.54
			0.9	0.49	0.51	0.51	0.49	0.48	0.49	0.48	0.48	0.50	0.49	0.50	0.53	0.52	0.51	0.51	0.49	0.51	0.52	0.51	0.51	0.51	0.52
			1	0.47	0.47	0.45	0.47	0.48	0.47	0.48	0.49	0.47	0.47	0.48	0.49	0.49	0.49	0.49	0.50	0.48	0.48	0.50	0.48	0.49	0.47
		A3(8)	0	0.32	0.34	0.34	0.33	0.34	0.34	0.34	0.36	0.36	0.34	0.35	0.72	0.73	0.72	0.72	0.72	0.72	0.72	0.71	0.72	0.70	0.72
			0.1	0.47	0.51	0.46	0.48	0.48	0.46	0.47	0.47	0.49	0.50	0.47	0.65	0.65	0.64	0.65	0.65	0.65	0.63	0.65	0.66	0.65	0.66
			0.2	0.55	0.57	0.55	0.55	0.56	0.54	0.54	0.55	0.54	0.54	0.55	0.63	0.62	0.62	0.61	0.61	0.61	0.60	0.62	0.62	0.62	0.63
			0.3	0.59	0.59	0.57	0.58	0.60	0.59	0.58	0.59	0.58	0.59	0.58	0.59	0.58	0.58	0.58	0.59	0.58	0.59	0.59	0.61	0.59	0.58
			0.4	0.61	0.60	0.61	0.60	0.61	0.64	0.60	0.60	0.60	0.62	0.61	0.57	0.56	0.57	0.57	0.56	0.57	0.55	0.55	0.54	0.57	0.57
			0.5	0.61	0.59	0.58	0.60	0.61	0.60	0.61	0.60	0.60	0.60	0.58	0.54	0.54	0.53	0.53	0.53	0.54	0.53	0.54	0.55	0.53	0.52
			0.6	0.56	0.56	0.56	0.56	0.57	0.58	0.56	0.57	0.57	0.57	0.59	0.54	0.51	0.50	0.53	0.53	0.51	0.52	0.53	0.52	0.52	0.51
			0.7	0.53	0.52	0.55	0.54	0.54	0.54	0.56	0.54	0.54	0.54	0.54	0.50	0.51	0.49	0.50	0.51	0.50	0.51	0.51	0.50	0.50	0.52
			0.8	0.52	0.51	0.51	0.52	0.51	0.53	0.53	0.53	0.50	0.53	0.52	0.50	0.47	0.48	0.49	0.48	0.50	0.48	0.49	0.48	0.49	0.47
			0.9	0.52	0.51	0.50	0.51	0.51	0.51	0.50	0.50	0.49	0.50	0.50	0.47	0.48	0.46	0.45	0.47	0.46	0.47	0.46	0.47	0.48	0.46
			1	0.49	0.51	0.50	0.49	0.49	0.49	0.49	0.51	0.50	0.50	0.48	0.44	0.47	0.45	0.44	0.46	0.46	0.44	0.46	0.47	0.46	0.46
	Hydro	A2	0	0.00	0.39	0.71	0.81	0.84	0.86	0.86	0.86	0.85	0.86	0.85	0.01	0.86	0.91	0.92	0.91	0.91	0.90	0.90	0.87	0.87	0.86
			0.1	0.00	0.16	0.49	0.71	0.81	0.84	0.87	0.89	0.90	0.91	0.91	0.00	0.38	0.71	0.84	0.88	0.90	0.91	0.92	0.92	0.93	0.93
			0.2	0.00	0.07	0.37	0.60	0.71	0.81	0.81	0.86	0.88	0.89	0.90	0.00	0.17	0.50	0.73	0.80	0.85	0.88	0.90	0.90	0.91	0.92
			0.3	0.00	0.05	0.31	0.52	0.64	0.75	0.78	0.82	0.85	0.87	0.88	0.00	0.08	0.38	0.58	0.73	0.79	0.84	0.87	0.88	0.89	0.90
			0.4	0.00	0.03	0.21	0.46	0.58	0.65	0.75	0.78	0.82	0.84	0.86	0.00	0.05	0.27	0.48	0.61	0.73	0.80	0.83	0.87	0.87	0.88
			0.5	0.00	0.02	0.16	0.37	0.51	0.60	0.69	0.74	0.79	0.81	0.83	0.00	0.03	0.20	0.39	0.57	0.68	0.75	0.79	0.83	0.85	0.86
			0.6	0.00	0.01	0.11	0.28	0.43	0.54	0.61	0.69	0.74	0.77	0.80	0.00	0.02	0.16	0.35	0.49	0.59	0.69	0.76	0.79	0.83	0.85
			0.7	0.00	0.01	0.06	0.18	0.37	0.48	0.56	0.61	0.72	0.72	0.78	0.00	0.02	0.12	0.30	0.45	0.55	0.66	0.71	0.75	0.81	0.84
			0.8	0.00	0.01	0.05	0.15	0.28	0.42	0.50	0.57	0.63	0.70	0.73	0.00	0.02	0.10	0.26	0.39	0.50	0.60	0.70	0.73	0.79	0.81
			0.9	0.00	0.01	0.04	0.11	0.26	0.38	0.49	0.56	0.59	0.65	0.69	0.00	0.01	0.08	0.23	0.37	0.47	0.59	0.66	0.70	0.75	0.80
			1	0.00	0.00	0.03	0.11	0.19	0.32	0.44	0.49	0.57	0.62	0.67	0.00	0.01	0.07	0.18	0.34	0.44	0.56	0.61	0.69	0.74	0.78
		A3(3)	0	0.54	0.73	0.81	0.85	0.88	0.89	0.89	0.89	0.89	0.89	0.88	0.74	0.87	0.90	0.92	0.92	0.93	0.93	0.93	0.92	0.92	0.92
			0.1	0.44	0.63	0.69	0.78	0.83	0.86	0.88	0.88	0.90	0.90	0.91	0.57	0.74	0.82	0.86	0.89	0.91	0.91	0.92	0.93	0.93	0.93
			0.2	0.34	0.54	0.65	0.73	0.76	0.80	0.83	0.86	0.86	0.88	0.88	0.48	0.65	0.75	0.81	0.84	0.86	0.88	0.90	0.91	0.91	0.92

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.3	0.29	0.52	0.62	0.67	0.71	0.77	0.80	0.82	0.83	0.85	0.87	0.41	0.58	0.69	0.73	0.79	0.83	0.85	0.87	0.88	0.89	0.90
			0.4	0.21	0.43	0.55	0.60	0.67	0.71	0.76	0.77	0.81	0.82	0.84	0.36	0.54	0.62	0.70	0.75	0.79	0.80	0.84	0.86	0.87	0.88
			0.5	0.17	0.39	0.51	0.56	0.62	0.66	0.69	0.72	0.74	0.81	0.79	0.34	0.49	0.59	0.65	0.70	0.74	0.78	0.82	0.83	0.84	0.87
			0.6	0.15	0.29	0.44	0.53	0.59	0.64	0.66	0.68	0.73	0.74	0.79	0.30	0.46	0.55	0.61	0.69	0.72	0.74	0.78	0.82	0.82	0.85
			0.7	0.11	0.27	0.37	0.49	0.55	0.59	0.62	0.65	0.69	0.73	0.74	0.28	0.44	0.53	0.59	0.64	0.71	0.72	0.78	0.80	0.80	0.82
			0.8	0.08	0.24	0.36	0.47	0.52	0.57	0.60	0.63	0.66	0.67	0.72	0.26	0.42	0.50	0.57	0.61	0.68	0.71	0.75	0.76	0.79	0.81
			0.9	0.07	0.18	0.31	0.38	0.50	0.53	0.57	0.60	0.65	0.68	0.68	0.23	0.39	0.49	0.54	0.60	0.63	0.69	0.71	0.75	0.77	0.81
			1	0.07	0.18	0.32	0.41	0.43	0.52	0.54	0.59	0.61	0.65	0.65	0.22	0.37	0.46	0.51	0.59	0.62	0.66	0.70	0.71	0.76	0.77
		A3(8)	0	0.32	0.67	0.78	0.83	0.85	0.86	0.86	0.86	0.85	0.86	0.85	0.70	0.89	0.92	0.92	0.92	0.91	0.91	0.90	0.88	0.88	0.87
			0.1	0.18	0.50	0.65	0.76	0.82	0.84	0.87	0.88	0.90	0.90	0.90	0.36	0.66	0.79	0.86	0.88	0.90	0.91	0.92	0.92	0.93	0.93
			0.2	0.11	0.38	0.55	0.67	0.76	0.78	0.82	0.85	0.86	0.88	0.88	0.23	0.51	0.67	0.76	0.82	0.86	0.87	0.89	0.90	0.91	0.91
			0.3	0.05	0.30	0.46	0.58	0.69	0.72	0.78	0.81	0.83	0.84	0.86	0.16	0.40	0.56	0.66	0.76	0.81	0.84	0.86	0.88	0.89	0.89
			0.4	0.04	0.23	0.40	0.53	0.62	0.72	0.72	0.76	0.78	0.83	0.84	0.11	0.34	0.49	0.60	0.70	0.75	0.78	0.82	0.84	0.86	0.88
			0.5	0.02	0.15	0.34	0.48	0.57	0.62	0.66	0.72	0.76	0.78	0.81	0.08	0.28	0.43	0.55	0.62	0.71	0.76	0.79	0.82	0.83	0.85
			0.6	0.01	0.10	0.23	0.39	0.50	0.57	0.61	0.69	0.68	0.74	0.79	0.07	0.23	0.39	0.50	0.59	0.65	0.70	0.76	0.79	0.82	0.83
			0.7	0.01	0.09	0.20	0.30	0.43	0.52	0.57	0.63	0.65	0.71	0.72	0.05	0.21	0.36	0.46	0.54	0.60	0.68	0.74	0.77	0.80	0.82
			0.8	0.01	0.06	0.16	0.28	0.40	0.47	0.53	0.58	0.61	0.66	0.69	0.04	0.15	0.33	0.43	0.52	0.62	0.64	0.69	0.74	0.78	0.78
			0.9	0.01	0.04	0.13	0.23	0.32	0.42	0.49	0.53	0.57	0.62	0.65	0.04	0.17	0.30	0.41	0.49	0.55	0.63	0.64	0.71	0.75	0.76
			1	0.01	0.04	0.10	0.20	0.31	0.39	0.45	0.52	0.55	0.58	0.61	0.03	0.14	0.26	0.38	0.46	0.53	0.58	0.63	0.70	0.73	0.76
	Reg. Shift	A2	0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01
			0.1	0.15	0.15	0.14	0.15	0.15	0.14	0.15	0.14	0.15	0.15	0.15	0.29	0.29	0.28	0.29	0.29	0.30	0.28	0.29	0.28	0.28	0.29
			0.2	0.37	0.38	0.37	0.37	0.37	0.38	0.36	0.37	0.38	0.38	0.37	0.45	0.43	0.43	0.46	0.42	0.43	0.44	0.44	0.43	0.43	0.44
			0.3	0.52	0.51	0.51	0.52	0.52	0.52	0.52	0.51	0.51	0.51	0.50	0.53	0.53	0.51	0.53	0.53	0.51	0.52	0.53	0.52	0.53	0.53
			0.4	0.58	0.58	0.58	0.59	0.58	0.58	0.59	0.58	0.58	0.59	0.58	0.58	0.58	0.59	0.59	0.56	0.57	0.59	0.58	0.60	0.58	0.57
			0.5	0.61	0.62	0.61	0.61	0.62	0.61	0.61	0.62	0.61	0.61	0.61	0.60	0.61	0.57	0.59	0.61	0.60	0.59	0.60	0.60	0.61	0.59
			0.6	0.62	0.62	0.60	0.63	0.62	0.62	0.62	0.63	0.62	0.62	0.62	0.62	0.62	0.63	0.62	0.63	0.59	0.61	0.63	0.61	0.63	0.64
			0.7	0.65	0.64	0.62	0.63	0.63	0.63	0.63	0.62	0.67	0.64	0.65	0.64	0.63	0.63	0.62	0.63	0.62	0.64	0.63	0.62	0.64	0.65
			0.8	0.68	0.66	0.64	0.64	0.66	0.64	0.63	0.63	0.64	0.64	0.64	0.62	0.64	0.65	0.65	0.65	0.62	0.65	0.65	0.62	0.65	0.65
			0.9	0.66	0.65	0.65	0.65	0.67	0.66	0.65	0.66	0.64	0.65	0.65	0.63	0.66	0.66	0.64	0.66	0.64	0.66	0.66	0.64	0.64	0.66
			1	0.66	0.66	0.67	0.66	0.68	0.66	0.66	0.65	0.66	0.65	0.65	0.67	0.67	0.65	0.65	0.67	0.66	0.67	0.65	0.65	0.66	0.67
		A3(3)	0	0.54	0.56	0.55	0.56	0.56	0.55	0.56	0.54	0.54	0.55	0.55	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75
			0.1	0.61	0.63	0.60	0.61	0.61	0.62	0.63	0.61	0.62	0.61	0.61	0.74	0.74	0.73	0.73	0.74	0.74	0.73	0.73	0.74	0.73	0.74
			0.2	0.63	0.64	0.65	0.65	0.65	0.64	0.65	0.66	0.65	0.65	0.65	0.72	0.71	0.72	0.72	0.72	0.70	0.71	0.72	0.72	0.73	0.71
			0.3	0.66	0.67	0.67	0.66	0.66	0.67	0.66	0.66	0.67	0.66	0.67	0.70	0.69	0.70	0.68	0.69	0.70	0.69	0.69	0.70	0.69	0.70
			0.4	0.66	0.66	0.65	0.65	0.66	0.66	0.66	0.65	0.65	0.66	0.66	0.67	0.68	0.68	0.69	0.68	0.69	0.65	0.67	0.69	0.67	0.68
			0.5	0.64	0.64	0.64	0.62	0.63	0.64	0.63	0.64	0.63	0.65	0.64	0.68	0.67	0.66	0.66	0.65	0.66	0.66	0.67	0.66	0.65	0.67
			0.6	0.63	0.63	0.63	0.61	0.63	0.62	0.62	0.60	0.61	0.62	0.62	0.66	0.63	0.65	0.66	0.66	0.66	0.63	0.65	0.65	0.63	0.65

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.7	0.60	0.61	0.61	0.61	0.62	0.61	0.61	0.62	0.62	0.60	0.62	0.65	0.62	0.65	0.65	0.63	0.65	0.63	0.65	0.65	0.63	0.63
			0.8	0.61	0.60	0.60	0.60	0.60	0.60	0.59	0.60	0.58	0.61	0.60	0.64	0.64	0.64	0.62	0.62	0.64	0.63	0.64	0.62	0.62	0.63
			0.9	0.59	0.60	0.60	0.59	0.59	0.58	0.58	0.59	0.60	0.59	0.59	0.64	0.63	0.62	0.62	0.60	0.62	0.62	0.61	0.61	0.62	0.63
			1	0.58	0.58	0.57	0.58	0.59	0.58	0.58	0.59	0.59	0.58	0.59	0.62	0.62	0.61	0.61	0.63	0.60	0.60	0.62	0.60	0.62	0.59
		A3(8)	0	0.32	0.34	0.34	0.33	0.34	0.34	0.34	0.36	0.35	0.35	0.35	0.70	0.71	0.68	0.69	0.70	0.69	0.69	0.68	0.69	0.67	0.69
			0.1	0.47	0.51	0.46	0.47	0.48	0.46	0.48	0.47	0.49	0.50	0.47	0.62	0.63	0.61	0.63	0.62	0.62	0.60	0.62	0.64	0.63	0.63
			0.2	0.55	0.57	0.55	0.55	0.56	0.54	0.54	0.55	0.54	0.55	0.55	0.63	0.62	0.62	0.61	0.61	0.61	0.60	0.61	0.61	0.61	0.63
			0.3	0.60	0.59	0.57	0.59	0.60	0.60	0.58	0.59	0.58	0.59	0.58	0.61	0.60	0.60	0.60	0.61	0.60	0.61	0.61	0.63	0.60	0.60
			0.4	0.63	0.61	0.62	0.61	0.62	0.64	0.61	0.61	0.61	0.63	0.61	0.62	0.60	0.61	0.61	0.60	0.61	0.59	0.59	0.59	0.61	0.61
			0.5	0.63	0.62	0.61	0.62	0.62	0.62	0.62	0.62	0.62	0.62	0.61	0.60	0.60	0.59	0.60	0.59	0.61	0.60	0.60	0.61	0.59	0.58
			0.6	0.60	0.61	0.60	0.60	0.61	0.62	0.60	0.62	0.61	0.61	0.63	0.62	0.59	0.58	0.60	0.61	0.59	0.60	0.61	0.61	0.60	0.59
			0.7	0.60	0.59	0.61	0.60	0.60	0.60	0.61	0.61	0.60	0.61	0.60	0.60	0.61	0.59	0.60	0.60	0.59	0.61	0.61	0.60	0.60	0.62
			0.8	0.60	0.59	0.60	0.61	0.61	0.60	0.60	0.61	0.58	0.60	0.60	0.61	0.58	0.59	0.61	0.60	0.62	0.60	0.60	0.60	0.60	0.57
			0.9	0.60	0.60	0.60	0.60	0.59	0.60	0.59	0.60	0.60	0.60	0.60	0.58	0.60	0.59	0.58	0.60	0.59	0.60	0.58	0.60	0.60	0.59
			1	0.60	0.62	0.60	0.61	0.60	0.60	0.59	0.61	0.60	0.60	0.59	0.57	0.62	0.58	0.58	0.60	0.60	0.58	0.59	0.60	0.60	0.60

Table E.2: Effects of D values on 100-year Survival Probabilities

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
CRiSP	SV	A2	0	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.09	0.09	0.09	0.08	0.09	0.08	0.08	0.09	0.09	0.09	0.08
			0.1	0.43	0.46	0.42	0.41	0.44	0.42	0.44	0.42	0.44	0.45	0.41	0.52	0.52	0.51	0.52	0.51	0.52	0.52	0.52	0.52	0.53	0.52
			0.2	0.69	0.67	0.68	0.68	0.68	0.69	0.68	0.69	0.69	0.69	0.70	0.60	0.60	0.60	0.60	0.60	0.60	0.60	0.59	0.60	0.59	0.60
			0.3	0.77	0.75	0.75	0.77	0.76	0.76	0.75	0.76	0.77	0.78	0.77	0.67	0.64	0.65	0.67	0.67	0.65	0.65	0.65	0.65	0.64	0.65
			0.4	0.78	0.78	0.77	0.79	0.78	0.79	0.79	0.78	0.78	0.77	0.79	0.69	0.67	0.67	0.68	0.68	0.69	0.70	0.68	0.68	0.68	0.68
			0.5	0.80	0.81	0.80	0.80	0.79	0.80	0.80	0.79	0.80	0.80	0.79	0.69	0.70	0.69	0.69	0.71	0.70	0.71	0.70	0.69	0.70	0.69
			0.6	0.81	0.80	0.82	0.81	0.82	0.81	0.81	0.80	0.81	0.81	0.81	0.70	0.72	0.70	0.71	0.69	0.72	0.70	0.71	0.70	0.71	0.70
			0.7	0.81	0.81	0.82	0.82	0.80	0.83	0.82	0.81	0.82	0.82	0.81	0.71	0.73	0.71	0.71	0.71	0.70	0.72	0.70	0.71	0.72	0.72
			0.8	0.80	0.83	0.84	0.82	0.84	0.83	0.82	0.82	0.81	0.83	0.82	0.71	0.71	0.71	0.71	0.73	0.71	0.72	0.71	0.72	0.71	0.71
			0.9	0.81	0.81	0.82	0.82	0.83	0.82	0.84	0.82	0.83	0.84	0.82	0.73	0.72	0.72	0.72	0.73	0.73	0.72	0.73	0.73	0.72	0.71
			1	0.83	0.83	0.83	0.84	0.82	0.84	0.81	0.82	0.83	0.84	0.81	0.72	0.73	0.73	0.74	0.72	0.73	0.73	0.74	0.74	0.73	0.73
		A3(3)	0	0.85	0.85	0.85	0.85	0.85	0.85	0.84	0.85	0.84	0.85	0.85	0.86	0.87	0.88	0.87	0.86	0.87	0.87	0.88	0.87	0.87	0.87
			0.1	0.90	0.90	0.91	0.90	0.91	0.90	0.90	0.91	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.91	0.90	0.90	0.90	0.90	0.90	0.90
			0.2	0.92	0.91	0.92	0.92	0.92	0.92	0.92	0.92	0.92	0.92	0.92	0.90	0.90	0.89	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.89
			0.3	0.92	0.91	0.91	0.92	0.92	0.92	0.91	0.91	0.92	0.91	0.91	0.88	0.88	0.88	0.88	0.88	0.88	0.88	0.89	0.88	0.89	0.89

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.4	0.91	0.90	0.90	0.91	0.90	0.90	0.91	0.90	0.91	0.91	0.91	0.87	0.87	0.87	0.87	0.87	0.87	0.88	0.87	0.87	0.86	0.86
			0.5	0.89	0.89	0.90	0.89	0.88	0.89	0.89	0.88	0.89	0.89	0.89	0.85	0.84	0.85	0.85	0.84	0.84	0.84	0.85	0.84	0.84	0.85
			0.6	0.87	0.88	0.88	0.88	0.88	0.88	0.88	0.89	0.86	0.88	0.88	0.80	0.82	0.81	0.81	0.81	0.81	0.82	0.81	0.81	0.81	0.81
			0.7	0.86	0.87	0.86	0.85	0.84	0.86	0.86	0.86	0.85	0.86	0.86	0.78	0.78	0.78	0.78	0.78	0.79	0.78	0.77	0.78	0.78	0.78
			0.8	0.84	0.85	0.85	0.85	0.85	0.84	0.85	0.86	0.84	0.84	0.84	0.75	0.76	0.74	0.73	0.74	0.73	0.74	0.76	0.74	0.74	0.76
			0.9	0.83	0.82	0.84	0.83	0.82	0.83	0.83	0.83	0.83	0.84	0.83	0.72	0.70	0.70	0.70	0.71	0.70	0.70	0.73	0.70	0.70	0.70
			1	0.81	0.80	0.82	0.81	0.82	0.81	0.79	0.81	0.81	0.80	0.80	0.67	0.68	0.69	0.67	0.67	0.67	0.67	0.66	0.67	0.66	0.66
		A3(8)	0	0.83	0.84	0.84	0.84	0.84	0.84	0.84	0.84	0.83	0.84	0.84	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.86	0.87
			0.1	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.88	0.88	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.90	0.89
			0.2	0.90	0.91	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.91	0.90	0.88	0.89	0.88	0.88	0.89	0.89	0.88	0.88	0.88	0.88	0.89
			0.3	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.89	0.90	0.91	0.90	0.88	0.87	0.87	0.88	0.88	0.87	0.87	0.87	0.87	0.87	0.87
			0.4	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.87	0.85	0.85	0.85	0.86	0.86	0.87	0.85	0.85	0.85	0.85
			0.5	0.88	0.88	0.88	0.88	0.87	0.87	0.87	0.87	0.88	0.88	0.88	0.82	0.83	0.82	0.82	0.84	0.83	0.83	0.83	0.83	0.83	0.83
			0.6	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.85	0.87	0.86	0.87	0.79	0.82	0.80	0.80	0.79	0.81	0.80	0.80	0.80	0.80	0.80
			0.7	0.84	0.84	0.85	0.85	0.83	0.86	0.85	0.84	0.84	0.85	0.84	0.77	0.78	0.76	0.76	0.77	0.76	0.78	0.76	0.76	0.78	0.77
			0.8	0.82	0.82	0.84	0.83	0.83	0.83	0.83	0.81	0.82	0.83	0.83	0.73	0.72	0.73	0.73	0.74	0.73	0.73	0.73	0.74	0.72	0.73
			0.9	0.80	0.79	0.81	0.79	0.80	0.80	0.81	0.81	0.82	0.80	0.80	0.71	0.70	0.70	0.70	0.71	0.71	0.70	0.71	0.71	0.70	0.69
			1	0.78	0.78	0.78	0.79	0.79	0.78	0.77	0.79	0.79	0.79	0.78	0.66	0.67	0.67	0.68	0.66	0.66	0.67	0.67	0.68	0.67	0.67
	Hydro	A2	0	0.02	0.63	0.87	0.92	0.93	0.94	0.93	0.93	0.93	0.92	0.92	0.06	0.82	0.93	0.96	0.95	0.96	0.95	0.95	0.94	0.93	0.94
			0.1	0.01	0.47	0.80	0.89	0.92	0.94	0.94	0.95	0.94	0.94	0.94	0.02	0.55	0.83	0.91	0.94	0.95	0.96	0.96	0.96	0.96	0.95
			0.2	0.01	0.28	0.70	0.83	0.88	0.91	0.93	0.94	0.94	0.95	0.95	0.01	0.36	0.69	0.84	0.90	0.94	0.94	0.94	0.95	0.95	0.95
			0.3	0.01	0.18	0.59	0.81	0.85	0.89	0.91	0.93	0.93	0.94	0.94	0.01	0.24	0.59	0.79	0.87	0.90	0.93	0.94	0.94	0.95	0.95
			0.4	0.00	0.11	0.48	0.73	0.82	0.87	0.88	0.90	0.92	0.93	0.93	0.01	0.16	0.52	0.71	0.83	0.89	0.91	0.93	0.94	0.95	0.95
			0.5	0.00	0.08	0.39	0.64	0.79	0.85	0.88	0.88	0.91	0.92	0.92	0.01	0.12	0.45	0.65	0.79	0.86	0.89	0.92	0.93	0.93	0.95
			0.6	0.00	0.05	0.32	0.57	0.75	0.82	0.86	0.87	0.90	0.90	0.92	0.01	0.08	0.38	0.61	0.74	0.83	0.87	0.90	0.92	0.93	0.94
			0.7	0.00	0.04	0.24	0.53	0.69	0.80	0.84	0.86	0.89	0.90	0.90	0.00	0.07	0.34	0.57	0.72	0.79	0.85	0.89	0.91	0.93	0.94
			0.8	0.00	0.03	0.20	0.51	0.65	0.77	0.82	0.85	0.86	0.89	0.90	0.00	0.09	0.31	0.55	0.65	0.78	0.84	0.88	0.90	0.91	0.93
			0.9	0.00	0.02	0.14	0.39	0.61	0.73	0.81	0.83	0.86	0.89	0.88	0.00	0.06	0.26	0.50	0.64	0.74	0.82	0.87	0.89	0.91	0.92
			1	0.00	0.02	0.13	0.36	0.56	0.71	0.75	0.81	0.84	0.87	0.86	0.00	0.06	0.22	0.45	0.62	0.72	0.80	0.85	0.88	0.90	0.92
		A3(3)	0	0.84	0.89	0.91	0.92	0.92	0.93	0.92	0.92	0.93	0.93	0.92	0.88	0.93	0.95	0.95	0.95	0.95	0.95	0.96	0.95	0.95	0.95
			0.1	0.85	0.90	0.93	0.93	0.94	0.94	0.94	0.95	0.94	0.94	0.94	0.87	0.92	0.94	0.95	0.96	0.96	0.96	0.96	0.96	0.96	0.96
			0.2	0.84	0.89	0.92	0.93	0.94	0.94	0.94	0.95	0.95	0.95	0.95	0.85	0.91	0.93	0.94	0.95	0.95	0.96	0.96	0.96	0.96	0.95
			0.3	0.82	0.89	0.91	0.92	0.93	0.93	0.93	0.93	0.94	0.94	0.94	0.83	0.89	0.92	0.93	0.94	0.94	0.95	0.95	0.95	0.96	0.95
			0.4	0.81	0.88	0.90	0.92	0.92	0.93	0.93	0.93	0.93	0.93	0.94	0.83	0.88	0.91	0.93	0.94	0.95	0.95	0.95	0.95	0.95	0.95
			0.5	0.79	0.87	0.89	0.90	0.91	0.92	0.92	0.92	0.93	0.93	0.93	0.81	0.88	0.91	0.92	0.93	0.93	0.94	0.95	0.94	0.95	0.95
			0.6	0.78	0.86	0.88	0.90	0.90	0.91	0.92	0.93	0.92	0.92	0.92	0.81	0.87	0.90	0.91	0.92	0.93	0.94	0.94	0.94	0.94	0.95
			0.7	0.74	0.85	0.88	0.88	0.89	0.91	0.91	0.91	0.91	0.91	0.92	0.79	0.86	0.89	0.91	0.92	0.93	0.93	0.94	0.94	0.94	0.94

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.8	0.75	0.84	0.87	0.89	0.90	0.90	0.90	0.91	0.91	0.91	0.91	0.78	0.85	0.88	0.90	0.91	0.92	0.93	0.94	0.94	0.93	0.95
			0.9	0.72	0.82	0.86	0.88	0.88	0.89	0.90	0.90	0.91	0.92	0.91	0.75	0.85	0.88	0.90	0.91	0.92	0.93	0.94	0.93	0.94	0.93
			1	0.73	0.82	0.85	0.87	0.88	0.89	0.88	0.90	0.89	0.89	0.91	0.75	0.84	0.86	0.89	0.91	0.92	0.92	0.92	0.93	0.93	0.93
		A3(8)	0	0.83	0.89	0.92	0.93	0.93	0.93	0.93	0.93	0.92	0.93	0.93	0.89	0.93	0.94	0.95	0.95	0.95	0.95	0.95	0.95	0.94	0.95
			0.1	0.83	0.89	0.92	0.94	0.95	0.94	0.94	0.94	0.93	0.93	0.95	0.85	0.90	0.93	0.94	0.95	0.95	0.96	0.96	0.96	0.96	0.96
			0.2	0.80	0.87	0.91	0.93	0.93	0.94	0.94	0.94	0.95	0.95	0.95	0.83	0.89	0.92	0.93	0.94	0.95	0.95	0.95	0.95	0.95	0.95
			0.3	0.78	0.85	0.89	0.91	0.92	0.93	0.93	0.93	0.94	0.94	0.94	0.80	0.87	0.90	0.92	0.93	0.94	0.94	0.95	0.95	0.95	0.95
			0.4	0.75	0.82	0.87	0.90	0.92	0.92	0.93	0.93	0.93	0.93	0.93	0.76	0.85	0.88	0.90	0.92	0.93	0.94	0.94	0.95	0.95	0.95
			0.5	0.73	0.82	0.86	0.88	0.90	0.91	0.92	0.92	0.92	0.93	0.92	0.76	0.83	0.87	0.89	0.91	0.92	0.93	0.94	0.94	0.94	0.95
			0.6	0.72	0.81	0.84	0.87	0.89	0.91	0.91	0.91	0.92	0.92	0.93	0.75	0.80	0.86	0.88	0.90	0.92	0.92	0.93	0.94	0.94	0.94
			0.7	0.67	0.79	0.83	0.86	0.88	0.90	0.90	0.91	0.91	0.92	0.91	0.72	0.79	0.84	0.87	0.90	0.90	0.91	0.92	0.93	0.94	0.94
			0.8	0.66	0.76	0.82	0.85	0.87	0.89	0.90	0.90	0.90	0.91	0.91	0.71	0.80	0.84	0.87	0.88	0.90	0.91	0.92	0.93	0.93	0.93
			0.9	0.63	0.73	0.80	0.82	0.87	0.88	0.89	0.90	0.90	0.91	0.90	0.69	0.79	0.82	0.85	0.87	0.89	0.90	0.92	0.92	0.92	0.93
			1	0.63	0.72	0.79	0.82	0.85	0.87	0.87	0.89	0.89	0.90	0.89	0.67	0.77	0.81	0.84	0.87	0.89	0.90	0.91	0.91	0.92	0.93
	Reg. Shift	A2	0	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
			0.1	0.44	0.47	0.44	0.43	0.46	0.43	0.46	0.43	0.46	0.47	0.43	0.43	0.42	0.43	0.40	0.43	0.43	0.41	0.41	0.43	0.41	0.42
			0.2	0.72	0.69	0.71	0.71	0.70	0.72	0.71	0.72	0.71	0.72	0.72	0.64	0.64	0.64	0.65	0.64	0.63	0.64	0.65	0.65	0.65	0.65
			0.3	0.82	0.82	0.81	0.82	0.80	0.82	0.80	0.81	0.81	0.81	0.81	0.76	0.76	0.76	0.76	0.76	0.76	0.76	0.76	0.75	0.76	0.76
			0.4	0.83	0.83	0.83	0.83	0.83	0.84	0.83	0.83	0.85	0.85	0.84	0.81	0.80	0.81	0.80	0.81	0.81	0.81	0.81	0.81	0.81	0.81
			0.5	0.85	0.86	0.85	0.86	0.86	0.85	0.86	0.83	0.86	0.85	0.84	0.83	0.84	0.83	0.83	0.84	0.83	0.84	0.83	0.83	0.83	0.83
			0.6	0.86	0.86	0.86	0.86	0.87	0.87	0.87	0.86	0.87	0.86	0.87	0.84	0.85	0.84	0.84	0.84	0.85	0.84	0.85	0.84	0.85	0.84
			0.7	0.87	0.88	0.87	0.87	0.86	0.87	0.87	0.87	0.88	0.88	0.86	0.85	0.86	0.85	0.85	0.85	0.84	0.85	0.85	0.85	0.86	0.85
			0.8	0.86	0.87	0.89	0.88	0.88	0.87	0.87	0.87	0.86	0.87	0.87	0.85	0.85	0.85	0.85	0.86	0.85	0.85	0.85	0.86	0.85	0.85
			0.9	0.87	0.86	0.87	0.87	0.89	0.88	0.88	0.87	0.88	0.89	0.87	0.86	0.86	0.86	0.86	0.87	0.86	0.85	0.86	0.86	0.86	0.86
			1	0.88	0.87	0.88	0.88	0.87	0.88	0.87	0.87	0.87	0.88	0.86	0.86	0.86	0.86	0.87	0.86	0.86	0.86	0.87	0.87	0.87	0.86
		A3(3)	0	0.85	0.85	0.85	0.85	0.85	0.85	0.84	0.85	0.84	0.85	0.85	0.87	0.88	0.88	0.88	0.87	0.88	0.88	0.88	0.88	0.88	0.88
			0.1	0.90	0.90	0.91	0.90	0.91	0.90	0.90	0.90	0.90	0.90	0.90	0.91	0.90	0.91	0.91	0.92	0.91	0.91	0.91	0.91	0.91	0.91
			0.2	0.92	0.91	0.92	0.92	0.92	0.92	0.92	0.92	0.92	0.92	0.92	0.91	0.91	0.91	0.91	0.91	0.91	0.91	0.91	0.91	0.91	0.91
			0.3	0.92	0.92	0.92	0.92	0.92	0.92	0.91	0.92	0.92	0.92	0.92	0.91	0.91	0.90	0.91	0.91	0.90	0.91	0.91	0.90	0.91	0.91
			0.4	0.91	0.91	0.91	0.92	0.91	0.91	0.92	0.91	0.92	0.92	0.92	0.90	0.91	0.91	0.90	0.90	0.91	0.91	0.91	0.91	0.90	0.90
			0.5	0.91	0.91	0.91	0.90	0.90	0.91	0.90	0.90	0.91	0.91	0.91	0.90	0.90	0.90	0.90	0.90	0.89	0.90	0.90	0.89	0.90	0.90
			0.6	0.90	0.90	0.90	0.91	0.90	0.90	0.90	0.91	0.89	0.90	0.89	0.88	0.89	0.89	0.89	0.88	0.88	0.89	0.89	0.88	0.89	0.89
			0.7	0.89	0.90	0.89	0.89	0.88	0.89	0.89	0.89	0.89	0.89	0.89	0.88	0.88	0.88	0.88	0.88	0.88	0.88	0.87	0.88	0.88	0.87
			0.8	0.89	0.89	0.89	0.89	0.89	0.89	0.88	0.90	0.88	0.88	0.89	0.87	0.87	0.86	0.86	0.86	0.86	0.87	0.87	0.86	0.86	0.87
			0.9	0.87	0.87	0.88	0.88	0.86	0.88	0.89	0.88	0.88	0.89	0.88	0.86	0.85	0.85	0.85	0.85	0.85	0.85	0.87	0.85	0.85	0.85
			1	0.87	0.86	0.87	0.87	0.87	0.87	0.85	0.87	0.87	0.86	0.87	0.84	0.85	0.85	0.85	0.84	0.84	0.84	0.84	0.85	0.84	0.84
		A3(8)	0	0.83	0.84	0.84	0.84	0.84	0.84	0.84	0.84	0.83	0.84	0.84	0.88	0.87	0.87	0.88	0.87	0.88	0.87	0.87	0.87	0.87	0.88

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.1	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.88	0.88	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.90	0.90	0.89	0.90	0.89
			0.2	0.90	0.91	0.91	0.91	0.90	0.91	0.90	0.90	0.91	0.91	0.91	0.89	0.90	0.90	0.89	0.90	0.90	0.89	0.89	0.89	0.89	0.90
			0.3	0.90	0.90	0.91	0.90	0.91	0.90	0.90	0.90	0.91	0.91	0.91	0.90	0.89	0.89	0.90	0.90	0.89	0.89	0.89	0.89	0.89	0.89
			0.4	0.90	0.90	0.90	0.90	0.91	0.91	0.90	0.90	0.90	0.90	0.91	0.90	0.89	0.89	0.88	0.89	0.90	0.90	0.89	0.89	0.89	0.89
			0.5	0.89	0.90	0.89	0.90	0.89	0.90	0.90	0.89	0.90	0.90	0.90	0.88	0.89	0.88	0.88	0.89	0.89	0.89	0.88	0.88	0.88	0.89
			0.6	0.89	0.89	0.89	0.89	0.89	0.89	0.90	0.89	0.89	0.89	0.89	0.87	0.89	0.87	0.88	0.87	0.89	0.87	0.88	0.88	0.88	0.87
			0.7	0.88	0.88	0.89	0.89	0.88	0.89	0.88	0.88	0.88	0.88	0.88	0.87	0.87	0.86	0.87	0.87	0.86	0.87	0.86	0.87	0.87	0.87
			0.8	0.87	0.87	0.88	0.88	0.89	0.88	0.88	0.87	0.87	0.88	0.88	0.86	0.85	0.86	0.85	0.86	0.86	0.86	0.86	0.86	0.85	0.86
			0.9	0.86	0.86	0.87	0.86	0.87	0.87	0.87	0.87	0.88	0.87	0.87	0.85	0.85	0.85	0.85	0.85	0.85	0.84	0.85	0.85	0.85	0.84
			1	0.86	0.86	0.86	0.87	0.86	0.86	0.85	0.86	0.86	0.87	0.85	0.84	0.84	0.84	0.85	0.83	0.84	0.84	0.84	0.84	0.84	0.84
FLUSH	SV	A2	0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
			0.1	0.11	0.11	0.10	0.10	0.11	0.09	0.11	0.10	0.12	0.11	0.10	0.46	0.47	0.46	0.47	0.46	0.46	0.43	0.46	0.47	0.46	0.47
			0.2	0.45	0.39	0.44	0.44	0.42	0.44	0.41	0.46	0.45	0.44	0.43	0.57	0.57	0.56	0.57	0.58	0.57	0.57	0.57	0.57	0.57	0.57
			0.3	0.63	0.61	0.62	0.62	0.62	0.63	0.61	0.64	0.63	0.63	0.61	0.61	0.61	0.61	0.60	0.60	0.60	0.61	0.60	0.61	0.61	0.60
			0.4	0.70	0.70	0.70	0.71	0.70	0.70	0.71	0.71	0.73	0.70	0.71	0.62	0.62	0.63	0.63	0.63	0.63	0.62	0.63	0.62	0.63	0.65
			0.5	0.72	0.73	0.74	0.73	0.74	0.72	0.73	0.72	0.74	0.73	0.72	0.64	0.64	0.66	0.66	0.64	0.64	0.64	0.64	0.64	0.64	0.66
			0.6	0.70	0.71	0.69	0.72	0.73	0.74	0.71	0.74	0.73	0.74	0.72	0.65	0.65	0.65	0.66	0.67	0.68	0.66	0.65	0.65	0.65	0.65
			0.7	0.74	0.74	0.70	0.71	0.73	0.73	0.72	0.72	0.74	0.73	0.73	0.67	0.66	0.65	0.68	0.67	0.67	0.66	0.66	0.67	0.66	0.65
			0.8	0.74	0.74	0.74	0.73	0.73	0.73	0.73	0.74	0.74	0.73	0.71	0.68	0.67	0.67	0.66	0.68	0.67	0.67	0.66	0.67	0.66	0.67
			0.9	0.74	0.74	0.73	0.72	0.75	0.74	0.72	0.71	0.73	0.74	0.73	0.68	0.67	0.66	0.67	0.66	0.68	0.66	0.66	0.68	0.68	0.67
			1	0.74	0.75	0.75	0.75	0.74	0.74	0.73	0.72	0.75	0.73	0.73	0.66	0.67	0.67	0.68	0.67	0.67	0.67	0.68	0.66	0.67	0.68
		A3(3)	0	0.80	0.80	0.80	0.80	0.80	0.80	0.80	0.80	0.80	0.80	0.80	0.85	0.84	0.84	0.86	0.84	0.85	0.85	0.85	0.85	0.84	0.85
			0.1	0.88	0.87	0.87	0.88	0.88	0.87	0.87	0.87	0.87	0.87	0.88	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.91	0.90
			0.2	0.89	0.90	0.90	0.90	0.90	0.89	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.91	0.90	0.89	0.90	0.89	0.90
			0.3	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.89	0.88	0.88	0.89	0.89	0.88	0.88	0.88	0.88	0.88	0.88
			0.4	0.88	0.87	0.87	0.87	0.88	0.88	0.88	0.88	0.88	0.88	0.88	0.87	0.86	0.86	0.86	0.86	0.87	0.87	0.86	0.86	0.86	0.86
			0.5	0.84	0.86	0.85	0.84	0.84	0.84	0.84	0.84	0.84	0.85	0.84	0.83	0.84	0.83	0.83	0.85	0.84	0.84	0.83	0.83	0.84	0.84
			0.6	0.81	0.81	0.82	0.82	0.80	0.81	0.81	0.79	0.82	0.79	0.81	0.79	0.81	0.80	0.80	0.79	0.81	0.80	0.80	0.80	0.80	0.79
			0.7	0.75	0.77	0.76	0.76	0.75	0.78	0.77	0.75	0.76	0.77	0.77	0.76	0.77	0.75	0.75	0.76	0.75	0.76	0.75	0.75	0.77	0.76
			0.8	0.70	0.70	0.73	0.72	0.72	0.73	0.71	0.71	0.70	0.72	0.71	0.71	0.70	0.71	0.71	0.72	0.71	0.71	0.71	0.72	0.70	0.71
			0.9	0.65	0.65	0.66	0.65	0.66	0.66	0.66	0.62	0.69	0.67	0.65	0.67	0.66	0.67	0.67	0.68	0.67	0.66	0.67	0.67	0.66	0.66
			1	0.59	0.59	0.63	0.60	0.61	0.62	0.60	0.62	0.61	0.61	0.61	0.61	0.62	0.63	0.63	0.61	0.62	0.62	0.62	0.63	0.62	0.62
		A3(8)	0	0.73	0.73	0.73	0.73	0.72	0.73	0.73	0.73	0.72	0.73	0.73	0.81	0.82	0.83	0.82	0.81	0.81	0.82	0.83	0.82	0.82	0.82
			0.1	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.89	0.88	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.89	0.88
			0.2	0.87	0.86	0.87	0.86	0.87	0.86	0.86	0.86	0.86	0.87	0.86	0.88	0.88	0.87	0.88	0.88	0.88	0.88	0.88	0.88	0.88	0.87
			0.3	0.86	0.86	0.87	0.87	0.88	0.87	0.87	0.87	0.86	0.86	0.87	0.85	0.85	0.84	0.85	0.85	0.84	0.85	0.86	0.85	0.85	0.85
			0.4	0.87	0.86	0.86	0.86	0.87	0.87	0.86	0.86	0.85	0.87	0.86	0.82	0.83	0.83	0.82	0.82	0.83	0.84	0.82	0.82	0.82	0.82

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.5	0.84	0.84	0.84	0.83	0.84	0.84	0.83	0.82	0.84	0.84	0.83	0.79	0.78	0.79	0.79	0.78	0.78	0.78	0.79	0.78	0.78	0.79
			0.6	0.78	0.78	0.79	0.80	0.80	0.81	0.80	0.81	0.78	0.81	0.81	0.73	0.75	0.74	0.74	0.73	0.73	0.74	0.74	0.74	0.74	0.74
			0.7	0.75	0.76	0.76	0.74	0.72	0.76	0.76	0.75	0.75	0.75	0.74	0.68	0.69	0.69	0.69	0.69	0.69	0.69	0.68	0.69	0.68	0.68
			0.8	0.71	0.71	0.70	0.70	0.70	0.71	0.70	0.71	0.68	0.69	0.71	0.63	0.64	0.63	0.62	0.63	0.62	0.63	0.65	0.63	0.63	0.65
			0.9	0.66	0.64	0.67	0.66	0.63	0.67	0.65	0.65	0.63	0.66	0.64	0.60	0.58	0.58	0.58	0.58	0.58	0.58	0.59	0.59	0.58	0.59
			1	0.59	0.61	0.62	0.60	0.61	0.61	0.59	0.61	0.61	0.61	0.58	0.54	0.54	0.55	0.54	0.54	0.54	0.54	0.54	0.54	0.54	0.54
	Hydro	A2	0	0.00	0.44	0.74	0.81	0.83	0.85	0.85	0.85	0.85	0.84	0.86	0.00	0.91	0.94	0.92	0.90	0.89	0.88	0.87	0.85	0.83	0.82
			0.1	0.00	0.11	0.62	0.83	0.89	0.91	0.91	0.92	0.91	0.92	0.92	0.00	0.48	0.85	0.92	0.95	0.95	0.95	0.95	0.95	0.95	0.94
			0.2	0.00	0.03	0.44	0.74	0.84	0.90	0.91	0.92	0.93	0.93	0.94	0.00	0.11	0.62	0.83	0.90	0.92	0.94	0.94	0.94	0.95	0.95
			0.3	0.00	0.02	0.28	0.62	0.80	0.84	0.88	0.91	0.92	0.93	0.94	0.00	0.04	0.44	0.70	0.84	0.89	0.92	0.93	0.94	0.95	0.95
			0.4	0.00	0.01	0.17	0.52	0.71	0.82	0.85	0.88	0.91	0.91	0.93	0.00	0.02	0.28	0.60	0.76	0.84	0.88	0.91	0.92	0.94	0.94
			0.5	0.00	0.01	0.11	0.40	0.63	0.74	0.82	0.85	0.88	0.89	0.91	0.00	0.01	0.15	0.50	0.68	0.79	0.86	0.89	0.91	0.92	0.94
			0.6	0.00	0.00	0.05	0.24	0.51	0.67	0.75	0.83	0.86	0.88	0.89	0.00	0.01	0.12	0.40	0.61	0.76	0.82	0.87	0.90	0.91	0.92
			0.7	0.00	0.00	0.02	0.14	0.41	0.57	0.67	0.77	0.83	0.84	0.87	0.00	0.01	0.08	0.28	0.55	0.69	0.78	0.84	0.87	0.90	0.91
			0.8	0.00	0.00	0.02	0.10	0.30	0.51	0.63	0.73	0.79	0.83	0.84	0.00	0.01	0.06	0.26	0.49	0.63	0.74	0.82	0.86	0.88	0.90
			0.9	0.00	0.00	0.01	0.06	0.24	0.42	0.57	0.66	0.74	0.81	0.82	0.00	0.00	0.05	0.20	0.45	0.59	0.70	0.78	0.84	0.87	0.89
			1	0.00	0.00	0.01	0.05	0.18	0.36	0.52	0.62	0.72	0.76	0.80	0.00	0.00	0.04	0.15	0.39	0.56	0.66	0.75	0.81	0.85	0.88
		A3(3)	0	0.80	0.84	0.86	0.87	0.87	0.87	0.87	0.88	0.87	0.87	0.87	0.89	0.91	0.92	0.93	0.92	0.93	0.92	0.92	0.92	0.91	0.92
			0.1	0.83	0.87	0.90	0.91	0.93	0.92	0.92	0.93	0.92	0.92	0.93	0.88	0.91	0.93	0.94	0.95	0.95	0.96	0.95	0.96	0.96	0.96
			0.2	0.80	0.87	0.90	0.92	0.93	0.93	0.94	0.94	0.94	0.94	0.94	0.86	0.90	0.92	0.93	0.94	0.95	0.95	0.95	0.95	0.95	0.95
			0.3	0.78	0.84	0.88	0.90	0.92	0.93	0.93	0.93	0.93	0.93	0.93	0.83	0.88	0.90	0.93	0.93	0.94	0.95	0.95	0.95	0.95	0.95
			0.4	0.74	0.81	0.85	0.88	0.90	0.91	0.91	0.91	0.92	0.92	0.92	0.80	0.87	0.89	0.91	0.93	0.93	0.94	0.94	0.95	0.95	0.95
			0.5	0.72	0.80	0.83	0.86	0.88	0.89	0.90	0.90	0.90	0.92	0.90	0.79	0.85	0.88	0.90	0.92	0.92	0.93	0.94	0.94	0.94	0.95
			0.6	0.71	0.78	0.82	0.85	0.85	0.88	0.89	0.89	0.91	0.90	0.91	0.78	0.83	0.87	0.89	0.90	0.92	0.92	0.93	0.94	0.94	0.94
			0.7	0.64	0.78	0.78	0.82	0.83	0.86	0.87	0.87	0.89	0.90	0.89	0.75	0.81	0.85	0.88	0.90	0.90	0.91	0.92	0.93	0.93	0.94
			0.8	0.63	0.72	0.79	0.81	0.83	0.84	0.85	0.87	0.87	0.88	0.89	0.74	0.81	0.85	0.87	0.88	0.90	0.91	0.92	0.92	0.92	0.93
			0.9	0.60	0.69	0.75	0.77	0.83	0.82	0.84	0.85	0.87	0.89	0.87	0.73	0.80	0.83	0.86	0.87	0.89	0.90	0.91	0.92	0.92	0.92
			1	0.60	0.68	0.76	0.77	0.79	0.82	0.80	0.84	0.85	0.88	0.86	0.71	0.79	0.82	0.85	0.87	0.88	0.89	0.90	0.90	0.91	0.92
		A3(8)	0	0.73	0.80	0.83	0.83	0.85	0.85	0.86	0.85	0.86	0.85	0.86	0.86	0.90	0.91	0.91	0.90	0.90	0.89	0.90	0.89	0.89	0.88
			0.1	0.74	0.83	0.88	0.90	0.91	0.91	0.91	0.92	0.91	0.92	0.92	0.81	0.90	0.94	0.95	0.96	0.95	0.95	0.95	0.95	0.96	0.95
			0.2	0.67	0.81	0.87	0.89	0.91	0.91	0.92	0.92	0.93	0.93	0.93	0.75	0.86	0.91	0.93	0.94	0.95	0.95	0.95	0.95	0.95	0.95
			0.3	0.59	0.74	0.83	0.87	0.90	0.91	0.91	0.92	0.92	0.92	0.92	0.69	0.83	0.88	0.91	0.92	0.93	0.94	0.94	0.94	0.95	0.95
			0.4	0.55	0.72	0.78	0.84	0.88	0.89	0.89	0.90	0.89	0.91	0.90	0.64	0.79	0.86	0.89	0.91	0.92	0.94	0.94	0.94	0.94	0.94
			0.5	0.48	0.67	0.76	0.80	0.83	0.86	0.87	0.87	0.89	0.88	0.89	0.60	0.77	0.83	0.87	0.89	0.90	0.92	0.93	0.92	0.93	0.94
			0.6	0.41	0.64	0.72	0.78	0.81	0.84	0.86	0.88	0.85	0.88	0.90	0.56	0.74	0.80	0.85	0.88	0.89	0.91	0.91	0.91	0.92	0.93
			0.7	0.36	0.62	0.70	0.72	0.76	0.81	0.82	0.85	0.85	0.87	0.86	0.52	0.74	0.79	0.83	0.86	0.88	0.89	0.91	0.92	0.92	0.92
			0.8	0.33	0.58	0.64	0.71	0.77	0.78	0.80	0.83	0.84	0.84	0.86	0.49	0.69	0.78	0.81	0.85	0.87	0.88	0.90	0.91	0.90	0.92

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.9	0.29	0.57	0.63	0.70	0.69	0.75	0.78	0.80	0.83	0.85	0.85	0.44	0.70	0.76	0.81	0.83	0.85	0.87	0.89	0.90	0.90	0.90
			1	0.26	0.54	0.61	0.66	0.69	0.73	0.74	0.77	0.81	0.81	0.84	0.42	0.69	0.73	0.78	0.82	0.85	0.86	0.88	0.89	0.89	0.90
	Reg. Shift	A2	0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
			0.1	0.11	0.11	0.11	0.10	0.11	0.09	0.12	0.11	0.12	0.11	0.10	0.17	0.19	0.15	0.18	0.17	0.16	0.15	0.18	0.20	0.17	0.18
			0.2	0.46	0.41	0.45	0.44	0.43	0.45	0.41	0.46	0.46	0.45	0.44	0.52	0.53	0.52	0.54	0.52	0.53	0.53	0.53	0.53	0.53	0.53
			0.3	0.65	0.63	0.63	0.63	0.64	0.64	0.62	0.65	0.64	0.65	0.62	0.67	0.67	0.67	0.66	0.66	0.66	0.67	0.67	0.66	0.67	0.67
			0.4	0.72	0.72	0.72	0.73	0.72	0.73	0.73	0.73	0.76	0.73	0.74	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75	0.75
			0.5	0.79	0.78	0.79	0.78	0.80	0.77	0.79	0.77	0.78	0.78	0.77	0.79	0.79	0.79	0.79	0.79	0.79	0.79	0.79	0.79	0.79	0.79
			0.6	0.79	0.80	0.75	0.80	0.81	0.81	0.80	0.81	0.81	0.82	0.81	0.81	0.81	0.81	0.81	0.81	0.82	0.81	0.81	0.81	0.81	0.81
			0.7	0.83	0.83	0.80	0.82	0.82	0.82	0.80	0.83	0.83	0.82	0.82	0.83	0.82	0.82	0.83	0.83	0.83	0.82	0.82	0.82	0.82	0.82
			0.8	0.82	0.83	0.84	0.82	0.83	0.83	0.83	0.84	0.83	0.84	0.82	0.84	0.83	0.83	0.82	0.84	0.83	0.83	0.83	0.83	0.83	0.83
			0.9	0.84	0.83	0.84	0.82	0.83	0.84	0.83	0.82	0.84	0.84	0.83	0.84	0.83	0.83	0.84	0.83	0.84	0.83	0.83	0.84	0.84	0.83
			1	0.84	0.85	0.85	0.85	0.84	0.84	0.84	0.83	0.84	0.84	0.83	0.83	0.83	0.83	0.84	0.83	0.84	0.83	0.84	0.83	0.83	0.84
		A3(3)	0	0.80	0.80	0.80	0.80	0.80	0.80	0.80	0.81	0.80	0.80	0.80	0.87	0.86	0.86	0.88	0.86	0.87	0.87	0.87	0.87	0.86	0.87
			0.1	0.88	0.87	0.87	0.88	0.88	0.87	0.87	0.87	0.87	0.87	0.88	0.90	0.90	0.90	0.90	0.90	0.90	0.91	0.90	0.90	0.91	0.90
			0.2	0.89	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.91	0.90	0.90	0.90	0.91	0.90	0.90	0.90	0.90	0.90
			0.3	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.90	0.89	0.91	0.90	0.90	0.90	0.90	0.90	0.90	0.90
			0.4	0.89	0.89	0.89	0.89	0.89	0.89	0.90	0.89	0.89	0.89	0.89	0.90	0.89	0.89	0.88	0.89	0.90	0.90	0.89	0.89	0.89	0.89
			0.5	0.88	0.88	0.88	0.87	0.88	0.87	0.88	0.88	0.88	0.88	0.88	0.88	0.89	0.88	0.88	0.89	0.89	0.89	0.88	0.88	0.88	0.88
			0.6	0.87	0.86	0.86	0.87	0.86	0.87	0.87	0.86	0.87	0.86	0.86	0.86	0.88	0.87	0.87	0.86	0.88	0.87	0.87	0.87	0.87	0.87
			0.7	0.85	0.85	0.85	0.85	0.84	0.86	0.86	0.84	0.85	0.85	0.85	0.86	0.86	0.85	0.86	0.86	0.85	0.86	0.85	0.85	0.86	0.86
			0.8	0.83	0.84	0.84	0.84	0.84	0.84	0.83	0.84	0.83	0.83	0.84	0.84	0.83	0.84	0.84	0.85	0.84	0.84	0.84	0.84	0.83	0.84
			0.9	0.81	0.82	0.82	0.82	0.82	0.82	0.82	0.82	0.83	0.82	0.82	0.83	0.82	0.83	0.83	0.83	0.83	0.82	0.83	0.83	0.83	0.82
			1	0.80	0.80	0.80	0.80	0.80	0.80	0.79	0.81	0.81	0.80	0.80	0.81	0.81	0.81	0.82	0.81	0.81	0.81	0.82	0.82	0.81	0.81
		A3(8)	0	0.73	0.73	0.73	0.73	0.73	0.73	0.73	0.73	0.73	0.73	0.73	0.84	0.85	0.86	0.85	0.84	0.85	0.85	0.86	0.85	0.85	0.85
			0.1	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.89	0.88	0.88	0.89	0.89	0.88	0.88	0.88	0.88	0.89	0.88
			0.2	0.87	0.86	0.87	0.87	0.87	0.86	0.86	0.87	0.86	0.87	0.86	0.88	0.88	0.88	0.88	0.89	0.89	0.89	0.88	0.88	0.88	0.88
			0.3	0.87	0.86	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.88	0.87	0.87	0.88	0.88	0.87	0.88	0.88	0.87	0.88	0.88
			0.4	0.88	0.87	0.86	0.87	0.88	0.87	0.87	0.87	0.86	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.87	0.88	0.87	0.87	0.87	0.87
			0.5	0.85	0.86	0.86	0.86	0.86	0.86	0.85	0.85	0.86	0.86	0.86	0.86	0.86	0.86	0.86	0.86	0.85	0.86	0.86	0.86	0.86	0.86
			0.6	0.84	0.84	0.84	0.85	0.85	0.85	0.86	0.86	0.84	0.86	0.86	0.84	0.85	0.84	0.84	0.84	0.84	0.85	0.85	0.84	0.84	0.85
			0.7	0.84	0.85	0.84	0.82	0.83	0.84	0.83	0.83	0.84	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83	0.83
			0.8	0.82	0.83	0.82	0.83	0.83	0.82	0.82	0.83	0.82	0.81	0.83	0.81	0.82	0.81	0.81	0.81	0.81	0.81	0.82	0.81	0.81	0.82
			0.9	0.81	0.80	0.82	0.81	0.79	0.82	0.81	0.81	0.81	0.82	0.82	0.80	0.80	0.80	0.79	0.79	0.79	0.79	0.81	0.80	0.79	0.80
			1	0.80	0.78	0.80	0.80	0.80	0.80	0.78	0.79	0.80	0.79	0.79	0.78	0.77	0.78	0.78	0.78	0.78	0.78	0.78	0.78	0.78	0.78

Table E.3: Effects of D values on 48-year Recovery Probabilities

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
CRiSP	SV	A2	0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
			0.1	0.07	0.07	0.07	0.07	0.06	0.06	0.06	0.07	0.07	0.07	0.07	0.15	0.14	0.13	0.13	0.14	0.14	0.14	0.14	0.14	0.15	0.14
			0.2	0.28	0.28	0.28	0.27	0.28	0.28	0.28	0.28	0.27	0.27	0.28	0.24	0.24	0.23	0.23	0.24	0.24	0.24	0.22	0.23	0.22	0.23
			0.3	0.41	0.37	0.36	0.40	0.40	0.37	0.40	0.41	0.40	0.42	0.41	0.30	0.29	0.28	0.30	0.31	0.29	0.28	0.29	0.28	0.29	0.28
			0.4	0.41	0.43	0.39	0.46	0.54	0.47	0.47	0.50	0.43	0.43	0.47	0.34	0.31	0.32	0.31	0.32	0.33	0.31	0.32	0.32	0.31	0.31
			0.5	0.47	0.49	0.42	0.49	0.43	0.46	0.46	0.56	0.48	0.46	0.43	0.32	0.35	0.34	0.33	0.36	0.35	0.35	0.34	0.33	0.36	0.34
			0.6	0.48	0.46	0.49	0.47	0.45	0.45	0.51	0.53	0.49	0.46	0.46	0.36	0.35	0.37	0.37	0.34	0.40	0.36	0.37	0.36	0.37	0.37
			0.7	0.47	0.48	0.53	0.45	0.45	0.53	0.52	0.44	0.44	0.50	0.48	0.39	0.38	0.36	0.36	0.38	0.35	0.39	0.36	0.36	0.39	0.38
			0.8	0.40	0.49	0.52	0.43	0.51	0.50	0.51	0.45	0.48	0.47	0.46	0.39	0.37	0.39	0.38	0.38	0.38	0.40	0.38	0.40	0.36	0.39
			0.9	0.44	0.45	0.45	0.54	0.51	0.52	0.50	0.46	0.61	0.57	0.57	0.41	0.41	0.40	0.40	0.39	0.41	0.39	0.40	0.41	0.39	0.39
			1	0.48	0.51	0.53	0.56	0.49	0.53	0.59	0.48	0.47	0.59	0.46	0.42	0.40	0.41	0.44	0.41	0.41	0.39	0.42	0.40	0.42	0.40
		A3(3)	0	0.95	0.94	0.95	0.95	0.94	0.95	0.94	0.95	0.94	0.95	0.94	0.95	0.96	0.94	0.96	0.95	0.95	0.96	0.95	0.95	0.95	0.94
			0.1	0.94	0.94	0.93	0.94	0.92	0.92	0.93	0.93	0.93	0.93	0.93	0.96	0.97	0.96	0.96	0.96	0.96	0.95	0.95	0.97	0.96	0.96
			0.2	0.91	0.91	0.93	0.90	0.91	0.90	0.89	0.91	0.90	0.91	0.89	0.95	0.94	0.93	0.93	0.93	0.92	0.93	0.93	0.92	0.93	0.93
			0.3	0.83	0.85	0.88	0.89	0.83	0.86	0.87	0.80	0.86	0.84	0.89	0.88	0.87	0.88	0.87	0.89	0.86	0.88	0.86	0.86	0.90	0.85
			0.4	0.77	0.86	0.77	0.81	0.83	0.81	0.81	0.77	0.86	0.78	0.78	0.83	0.82	0.84	0.82	0.83	0.83	0.81	0.83	0.79	0.81	0.82
			0.5	0.69	0.71	0.79	0.71	0.69	0.75	0.69	0.66	0.71	0.74	0.71	0.71	0.75	0.73	0.71	0.70	0.74	0.72	0.72	0.72	0.75	0.69
			0.6	0.65	0.63	0.69	0.69	0.66	0.66	0.67	0.70	0.64	0.64	0.69	0.63	0.67	0.64	0.64	0.63	0.65	0.66	0.66	0.65	0.65	0.60
			0.7	0.59	0.65	0.58	0.60	0.50	0.61	0.62	0.56	0.64	0.68	0.55	0.53	0.54	0.53	0.56	0.53	0.55	0.56	0.55	0.55	0.53	0.55
			0.8	0.53	0.54	0.61	0.50	0.54	0.54	0.58	0.54	0.58	0.48	0.59	0.48	0.46	0.45	0.45	0.47	0.45	0.44	0.46	0.45	0.45	0.46
			0.9	0.56	0.51	0.54	0.51	0.47	0.55	0.54	0.50	0.52	0.56	0.51	0.42	0.38	0.36	0.39	0.38	0.39	0.37	0.41	0.37	0.39	0.38
			1	0.45	0.48	0.48	0.47	0.52	0.49	0.52	0.41	0.45	0.46	0.45	0.32	0.34	0.31	0.32	0.33	0.31	0.31	0.30	0.33	0.31	0.31
		A3(8)	0	0.95	0.95	0.95	0.94	0.95	0.94	0.95	0.95	0.94	0.95	0.93	0.94	0.95	0.94	0.95	0.95	0.95	0.94	0.95	0.94	0.95	0.94
			0.1	0.94	0.92	0.93	0.92	0.94	0.93	0.93	0.95	0.94	0.95	0.93	0.97	0.96	0.96	0.95	0.96	0.96	0.97	0.96	0.96	0.96	0.97
			0.2	0.91	0.91	0.91	0.90	0.88	0.90	0.89	0.89	0.93	0.91	0.89	0.95	0.95	0.95	0.94	0.95	0.93	0.94	0.94	0.94	0.95	0.95
			0.3	0.83	0.83	0.81	0.83	0.82	0.84	0.83	0.84	0.81	0.86	0.85	0.87	0.90	0.89	0.86	0.88	0.90	0.90	0.90	0.90	0.90	0.91
			0.4	0.74	0.76	0.74	0.82	0.79	0.81	0.76	0.79	0.79	0.80	0.83	0.82	0.82	0.82	0.83	0.82	0.81	0.74	0.82	0.85	0.83	0.84
			0.5	0.74	0.75	0.71	0.74	0.68	0.68	0.71	0.72	0.72	0.73	0.70	0.76	0.76	0.74	0.76	0.74	0.72	0.71	0.73	0.76	0.75	0.76
			0.6	0.72	0.67	0.66	0.68	0.63	0.69	0.66	0.67	0.68	0.69	0.66	0.64	0.63	0.67	0.67	0.66	0.71	0.65	0.68	0.65	0.66	0.65
			0.7	0.59	0.58	0.62	0.56	0.56	0.65	0.63	0.55	0.56	0.61	0.59	0.57	0.56	0.55	0.56	0.58	0.54	0.59	0.55	0.55	0.57	0.57
			0.8	0.48	0.50	0.57	0.53	0.59	0.55	0.56	0.54	0.57	0.53	0.53	0.48	0.45	0.48	0.48	0.50	0.47	0.48	0.48	0.50	0.45	0.48
			0.9	0.46	0.46	0.48	0.60	0.50	0.52	0.50	0.49	0.56	0.54	0.52	0.42	0.40	0.40	0.42	0.41	0.42	0.39	0.41	0.41	0.40	0.41
			1	0.43	0.47	0.50	0.48	0.45	0.47	0.47	0.45	0.44	0.50	0.41	0.33	0.34	0.35	0.36	0.33	0.33	0.34	0.36	0.35	0.35	0.34

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
	Hydro	A2	0	0.00	0.19	0.64	0.80	0.84	0.90	0.94	0.93	0.95	0.95	0.94	0.00	0.52	0.85	0.92	0.96	0.97	0.97	0.97	0.96	0.96	0.95
			0.1	0.00	0.06	0.42	0.67	0.75	0.81	0.87	0.89	0.92	0.94	0.95	0.00	0.15	0.56	0.73	0.91	0.93	0.96	0.96	0.97	0.97	0.97
			0.2	0.00	0.03	0.29	0.51	0.61	0.74	0.82	0.84	0.90	0.92	0.92	0.00	0.05	0.33	0.62	0.79	0.84	0.92	0.94	0.95	0.96	0.97
			0.3	0.00	0.01	0.16	0.46	0.55	0.64	0.70	0.80	0.82	0.87	0.90	0.00	0.03	0.22	0.48	0.64	0.79	0.84	0.91	0.93	0.95	0.96
			0.4	0.00	0.00	0.10	0.38	0.57	0.62	0.69	0.76	0.80	0.83	0.86	0.00	0.02	0.16	0.38	0.57	0.68	0.68	0.84	0.91	0.91	0.94
			0.5	0.00	0.00	0.07	0.22	0.42	0.56	0.63	0.74	0.76	0.78	0.79	0.00	0.01	0.12	0.32	0.48	0.62	0.68	0.80	0.87	0.88	0.93
			0.6	0.00	0.00	0.04	0.16	0.36	0.48	0.61	0.71	0.72	0.74	0.78	0.00	0.01	0.09	0.26	0.43	0.58	0.67	0.78	0.86	0.84	0.91
			0.7	0.00	0.00	0.03	0.13	0.29	0.49	0.58	0.55	0.62	0.71	0.75	0.00	0.01	0.07	0.22	0.40	0.51	0.64	0.74	0.82	0.82	0.85
			0.8	0.00	0.00	0.02	0.10	0.25	0.42	0.52	0.53	0.59	0.62	0.68	0.00	0.01	0.07	0.20	0.32	0.49	0.62	0.73	0.76	0.82	0.87
			0.9	0.00	0.00	0.01	0.09	0.20	0.39	0.45	0.48	0.69	0.70	0.73	0.00	0.01	0.05	0.16	0.30	0.44	0.59	0.67	0.73	0.78	0.87
			1	0.00	0.00	0.01	0.07	0.16	0.33	0.51	0.47	0.51	0.67	0.59	0.00	0.01	0.04	0.14	0.30	0.39	0.53	0.61	0.68	0.79	0.79
		A3(3)	0	0.94	0.95	0.95	0.95	0.95	0.95	0.94	0.95	0.94	0.95	0.95	0.98	0.98	0.97	0.98	0.98	0.98	0.98	0.98	0.97	0.97	0.97
			0.1	0.93	0.93	0.94	0.94	0.92	0.93	0.94	0.94	0.94	0.94	0.94	0.97	0.98	0.97	0.98	0.98	0.97	0.98	0.98	0.98	0.98	0.98
			0.2	0.90	0.92	0.94	0.91	0.91	0.91	0.90	0.92	0.91	0.91	0.90	0.97	0.97	0.97	0.96	0.97	0.97	0.97	0.96	0.96	0.96	0.97
			0.3	0.87	0.87	0.91	0.91	0.87	0.88	0.90	0.86	0.90	0.88	0.91	0.96	0.95	0.96	0.95	0.97	0.95	0.96	0.93	0.95	0.96	0.95
			0.4	0.87	0.91	0.84	0.86	0.89	0.88	0.87	0.85	0.90	0.87	0.86	0.95	0.94	0.95	0.95	0.95	0.95	0.95	0.95	0.94	0.95	0.96
			0.5	0.81	0.83	0.89	0.83	0.80	0.84	0.82	0.79	0.83	0.84	0.82	0.92	0.94	0.94	0.94	0.93	0.95	0.94	0.94	0.94	0.94	0.92
			0.6	0.79	0.81	0.83	0.84	0.82	0.80	0.83	0.83	0.80	0.80	0.83	0.91	0.94	0.93	0.94	0.94	0.93	0.93	0.93	0.92	0.93	0.91
			0.7	0.77	0.82	0.78	0.79	0.71	0.80	0.80	0.78	0.80	0.85	0.74	0.90	0.92	0.92	0.92	0.92	0.92	0.92	0.94	0.94	0.93	0.93
			0.8	0.76	0.79	0.82	0.75	0.77	0.78	0.76	0.76	0.80	0.71	0.79	0.87	0.90	0.92	0.91	0.92	0.92	0.91	0.88	0.93	0.90	0.89
			0.9	0.74	0.77	0.78	0.78	0.73	0.78	0.78	0.76	0.77	0.79	0.75	0.83	0.91	0.91	0.91	0.90	0.91	0.90	0.89	0.91	0.91	0.91
			1	0.74	0.79	0.74	0.73	0.78	0.75	0.74	0.67	0.72	0.74	0.73	0.83	0.89	0.83	0.89	0.90	0.89	0.90	0.91	0.90	0.89	0.90
		A3(8)	0	0.94	0.95	0.96	0.95	0.94	0.95	0.95	0.95	0.94	0.95	0.95	0.98	0.97	0.97	0.98	0.97	0.98	0.97	0.98	0.98	0.98	0.98
			0.1	0.94	0.93	0.95	0.93	0.94	0.95	0.93	0.95	0.94	0.95	0.94	0.98	0.98	0.98	0.97	0.98	0.98	0.98	0.98	0.98	0.98	0.98
			0.2	0.92	0.93	0.92	0.91	0.90	0.92	0.91	0.91	0.94	0.92	0.91	0.98	0.98	0.98	0.98	0.98	0.97	0.98	0.97	0.98	0.97	0.97
			0.3	0.88	0.90	0.87	0.89	0.87	0.90	0.87	0.89	0.87	0.89	0.90	0.95	0.97	0.96	0.95	0.95	0.97	0.96	0.97	0.96	0.96	0.96
			0.4	0.85	0.85	0.86	0.90	0.88	0.88	0.85	0.88	0.87	0.87	0.88	0.91	0.96	0.96	0.95	0.95	0.94	0.90	0.94	0.96	0.95	0.96
			0.5	0.84	0.86	0.85	0.86	0.82	0.83	0.84	0.84	0.85	0.86	0.82	0.91	0.94	0.94	0.95	0.93	0.92	0.91	0.94	0.95	0.93	0.96
			0.6	0.81	0.84	0.83	0.85	0.81	0.85	0.82	0.84	0.84	0.84	0.83	0.89	0.91	0.93	0.94	0.95	0.93	0.92	0.94	0.94	0.92	0.95
			0.7	0.79	0.81	0.80	0.79	0.78	0.83	0.84	0.78	0.79	0.81	0.79	0.84	0.90	0.93	0.93	0.93	0.93	0.91	0.93	0.93	0.91	0.91
			0.8	0.74	0.78	0.81	0.79	0.83	0.79	0.80	0.78	0.80	0.75	0.76	0.83	0.90	0.92	0.93	0.90	0.93	0.93	0.93	0.91	0.91	0.92
			0.9	0.74	0.76	0.75	0.82	0.80	0.81	0.78	0.76	0.80	0.81	0.79	0.81	0.89	0.90	0.91	0.88	0.91	0.91	0.90	0.91	0.90	0.93
			1	0.73	0.78	0.80	0.78	0.76	0.78	0.75	0.74	0.72	0.78	0.69	0.79	0.86	0.89	0.90	0.91	0.90	0.89	0.89	0.88	0.91	0.89
	Reg. Shift	A2	0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
			0.1	0.07	0.07	0.07	0.08	0.07	0.07	0.08	0.07	0.08	0.07	0.08	0.04	0.04	0.04	0.04	0.04	0.04	0.04	0.03	0.04	0.04	0.04
			0.2	0.32	0.32	0.32	0.31	0.31	0.31	0.32	0.33	0.31	0.34	0.33	0.30	0.30	0.30	0.30	0.30	0.28	0.30	0.30	0.30	0.28	0.29
			0.3	0.49	0.54	0.45	0.48	0.46	0.48	0.48	0.52	0.46	0.48	0.55	0.48	0.48	0.49	0.48	0.49	0.49	0.48	0.49	0.48	0.51	0.49

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.4	0.50	0.53	0.52	0.64	0.64	0.58	0.59	0.60	0.63	0.61	0.61	0.60	0.61	0.62	0.63	0.62	0.56	0.51	0.59	0.62	0.60	0.63
			0.5	0.64	0.64	0.59	0.69	0.61	0.64	0.63	0.69	0.66	0.61	0.55	0.71	0.69	0.69	0.73	0.68	0.66	0.62	0.66	0.69	0.69	0.71
			0.6	0.68	0.65	0.68	0.69	0.63	0.65	0.69	0.75	0.71	0.66	0.67	0.77	0.67	0.75	0.73	0.79	0.75	0.72	0.78	0.78	0.70	0.77
			0.7	0.67	0.71	0.71	0.63	0.64	0.73	0.75	0.64	0.65	0.72	0.70	0.83	0.74	0.80	0.79	0.79	0.82	0.76	0.81	0.80	0.76	0.75
			0.8	0.61	0.71	0.74	0.64	0.75	0.72	0.75	0.70	0.72	0.67	0.69	0.82	0.82	0.83	0.82	0.77	0.83	0.84	0.83	0.81	0.81	0.82
			0.9	0.69	0.72	0.66	0.79	0.74	0.77	0.74	0.71	0.83	0.79	0.80	0.84	0.84	0.83	0.84	0.75	0.82	0.84	0.83	0.81	0.81	0.85
			1	0.72	0.75	0.76	0.79	0.75	0.77	0.83	0.74	0.72	0.79	0.72	0.87	0.88	0.82	0.84	0.86	0.84	0.80	0.84	0.82	0.86	0.81
		A3(3)	0	0.95	0.95	0.95	0.95	0.95	0.94	0.94	0.94	0.95	0.96	0.94	0.99	0.98	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.98	0.98
			0.1	0.93	0.94	0.93	0.94	0.91	0.93	0.93	0.93	0.93	0.94	0.93	0.97	0.98	0.97	0.98	0.98	0.97	0.97	0.97	0.98	0.98	0.97
			0.2	0.90	0.91	0.93	0.90	0.91	0.91	0.90	0.91	0.91	0.91	0.90	0.97	0.96	0.96	0.96	0.96	0.96	0.96	0.97	0.95	0.96	0.96
			0.3	0.85	0.86	0.90	0.90	0.86	0.88	0.89	0.85	0.89	0.87	0.90	0.95	0.94	0.95	0.93	0.95	0.94	0.95	0.93	0.93	0.96	0.93
			0.4	0.83	0.91	0.83	0.86	0.88	0.87	0.86	0.83	0.90	0.84	0.84	0.95	0.92	0.94	0.94	0.94	0.94	0.94	0.94	0.91	0.93	0.94
			0.5	0.80	0.80	0.87	0.82	0.79	0.83	0.80	0.77	0.81	0.83	0.81	0.89	0.93	0.92	0.91	0.90	0.93	0.91	0.91	0.93	0.92	0.90
			0.6	0.79	0.76	0.80	0.82	0.80	0.79	0.80	0.82	0.79	0.78	0.81	0.88	0.91	0.90	0.91	0.90	0.91	0.91	0.91	0.90	0.91	0.86
			0.7	0.74	0.80	0.75	0.77	0.70	0.77	0.79	0.74	0.80	0.82	0.70	0.90	0.88	0.87	0.88	0.89	0.88	0.89	0.90	0.89	0.90	0.91
			0.8	0.73	0.75	0.79	0.68	0.73	0.76	0.76	0.72	0.80	0.68	0.77	0.89	0.85	0.86	0.87	0.87	0.88	0.85	0.80	0.87	0.84	0.82
			0.9	0.76	0.74	0.77	0.76	0.70	0.78	0.76	0.72	0.74	0.76	0.73	0.79	0.84	0.83	0.85	0.84	0.86	0.85	0.83	0.82	0.84	0.83
			1	0.71	0.79	0.71	0.70	0.77	0.73	0.71	0.62	0.70	0.73	0.68	0.81	0.75	0.69	0.78	0.83	0.80	0.82	0.83	0.83	0.81	0.85
		A3(8)	0	0.94	0.95	0.95	0.95	0.95	0.95	0.94	0.95	0.94	0.95	0.94	0.98	0.98	0.98	0.98	0.97	0.98	0.98	0.98	0.98	0.98	0.98
			0.1	0.94	0.92	0.94	0.93	0.94	0.94	0.92	0.95	0.94	0.95	0.93	0.97	0.97	0.97	0.96	0.97	0.97	0.97	0.97	0.97	0.97	0.97
			0.2	0.91	0.92	0.91	0.91	0.89	0.90	0.90	0.89	0.94	0.92	0.91	0.96	0.96	0.96	0.97	0.97	0.95	0.96	0.95	0.96	0.96	0.96
			0.3	0.86	0.87	0.86	0.87	0.85	0.88	0.86	0.87	0.84	0.88	0.88	0.92	0.95	0.94	0.93	0.93	0.95	0.94	0.95	0.95	0.96	0.95
			0.4	0.82	0.82	0.82	0.87	0.86	0.86	0.84	0.87	0.84	0.84	0.88	0.93	0.93	0.94	0.93	0.93	0.90	0.86	0.91	0.94	0.92	0.94
			0.5	0.83	0.83	0.81	0.83	0.80	0.80	0.81	0.83	0.82	0.83	0.81	0.93	0.92	0.91	0.94	0.90	0.89	0.87	0.90	0.92	0.92	0.93
			0.6	0.83	0.80	0.80	0.82	0.76	0.81	0.79	0.82	0.83	0.81	0.80	0.92	0.86	0.89	0.89	0.92	0.92	0.88	0.92	0.92	0.87	0.92
			0.7	0.75	0.76	0.78	0.71	0.72	0.80	0.81	0.73	0.73	0.79	0.79	0.92	0.85	0.90	0.89	0.89	0.90	0.87	0.89	0.89	0.86	0.86
			0.8	0.68	0.70	0.77	0.73	0.80	0.74	0.77	0.74	0.76	0.72	0.73	0.88	0.87	0.88	0.88	0.84	0.89	0.89	0.88	0.87	0.86	0.87
			0.9	0.70	0.71	0.68	0.81	0.75	0.75	0.73	0.71	0.80	0.77	0.75	0.86	0.86	0.85	0.86	0.77	0.84	0.86	0.85	0.83	0.83	0.86
			1	0.66	0.73	0.76	0.74	0.71	0.76	0.72	0.68	0.68	0.73	0.64	0.86	0.87	0.81	0.83	0.85	0.83	0.79	0.82	0.81	0.85	0.80
FLUSH	SV	A2	0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
			0.1	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.10	0.10	0.11	0.10	0.11	0.11	0.11	0.10	0.09	0.09	0.10
			0.2	0.07	0.07	0.08	0.07	0.08	0.08	0.07	0.08	0.07	0.08	0.08	0.18	0.20	0.19	0.19	0.21	0.19	0.20	0.18	0.19	0.20	0.20
			0.3	0.22	0.23	0.22	0.23	0.23	0.22	0.24	0.22	0.22	0.21	0.21	0.24	0.23	0.22	0.23	0.23	0.23	0.24	0.22	0.25	0.25	0.24
			0.4	0.41	0.36	0.36	0.34	0.37	0.37	0.34	0.38	0.40	0.34	0.36	0.27	0.26	0.27	0.27	0.27	0.27	0.26	0.26	0.25	0.26	0.28
			0.5	0.38	0.35	0.37	0.38	0.36	0.35	0.36	0.36	0.39	0.39	0.34	0.29	0.27	0.30	0.30	0.28	0.27	0.28	0.27	0.28	0.28	0.31
			0.6	0.34	0.34	0.49	0.43	0.42	0.42	0.43	0.40	0.39	0.46	0.38	0.28	0.29	0.28	0.29	0.31	0.29	0.29	0.29	0.29	0.29	0.28
			0.7	0.40	0.36	0.40	0.35	0.38	0.38	0.47	0.38	0.39	0.43	0.39	0.31	0.30	0.29	0.31	0.30	0.30	0.30	0.30	0.31	0.30	0.30

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.8	0.37	0.37	0.39	0.34	0.37	0.42	0.45	0.40	0.41	0.35	0.42	0.31	0.31	0.30	0.30	0.34	0.31	0.31	0.31	0.31	0.31	0.31
			0.9	0.36	0.37	0.36	0.36	0.42	0.41	0.34	0.30	0.40	0.37	0.35	0.31	0.29	0.28	0.31	0.29	0.32	0.30	0.30	0.33	0.32	0.30
			1	0.38	0.38	0.36	0.40	0.39	0.42	0.36	0.42	0.39	0.37	0.36	0.30	0.33	0.32	0.33	0.31	0.32	0.32	0.31	0.29	0.32	0.33
		A3(3)	0	0.85	0.88	0.86	0.87	0.85	0.86	0.89	0.86	0.87	0.87	0.85	0.87	0.87	0.86	0.87	0.88	0.88	0.87	0.88	0.87	0.87	0.87
			0.1	0.91	0.92	0.91	0.91	0.93	0.92	0.92	0.92	0.93	0.93	0.91	0.96	0.96	0.96	0.94	0.96	0.96	0.96	0.96	0.95	0.96	0.97
			0.2	0.89	0.85	0.86	0.84	0.86	0.85	0.87	0.86	0.87	0.87	0.85	0.96	0.96	0.95	0.95	0.95	0.94	0.95	0.95	0.95	0.96	0.95
			0.3	0.78	0.79	0.77	0.80	0.78	0.81	0.79	0.79	0.78	0.79	0.79	0.87	0.91	0.89	0.87	0.88	0.90	0.91	0.90	0.91	0.91	0.91
			0.4	0.69	0.71	0.66	0.79	0.75	0.74	0.70	0.71	0.77	0.70	0.77	0.83	0.82	0.82	0.83	0.83	0.81	0.73	0.82	0.85	0.83	0.84
			0.5	0.63	0.66	0.61	0.66	0.61	0.58	0.62	0.65	0.60	0.64	0.62	0.74	0.75	0.73	0.74	0.73	0.71	0.71	0.72	0.75	0.73	0.75
			0.6	0.56	0.55	0.56	0.59	0.49	0.57	0.56	0.59	0.58	0.54	0.53	0.61	0.61	0.64	0.64	0.63	0.68	0.63	0.65	0.62	0.63	0.61
			0.7	0.52	0.48	0.47	0.45	0.47	0.53	0.51	0.43	0.45	0.50	0.47	0.52	0.53	0.50	0.51	0.53	0.48	0.55	0.50	0.50	0.54	0.53
			0.8	0.37	0.41	0.41	0.37	0.42	0.42	0.41	0.40	0.41	0.41	0.40	0.41	0.41	0.43	0.43	0.45	0.42	0.43	0.43	0.43	0.40	0.44
			0.9	0.32	0.33	0.35	0.37	0.29	0.33	0.34	0.31	0.36	0.31	0.32	0.37	0.34	0.35	0.35	0.35	0.37	0.35	0.36	0.36	0.34	0.36
			1	0.23	0.23	0.24	0.24	0.25	0.24	0.27	0.27	0.25	0.24	0.27	0.28	0.28	0.29	0.30	0.28	0.28	0.28	0.30	0.30	0.29	0.28
		A3(8)	0	0.81	0.82	0.83	0.81	0.82	0.83	0.83	0.81	0.82	0.80	0.84	0.78	0.80	0.78	0.82	0.79	0.79	0.79	0.80	0.79	0.78	0.78
			0.1	0.91	0.90	0.91	0.92	0.90	0.90	0.89	0.90	0.91	0.92	0.90	0.96	0.96	0.95	0.96	0.96	0.95	0.95	0.95	0.96	0.96	0.96
			0.2	0.86	0.85	0.85	0.84	0.85	0.83	0.81	0.84	0.82	0.83	0.84	0.95	0.94	0.94	0.93	0.93	0.92	0.94	0.93	0.92	0.93	0.93
			0.3	0.74	0.75	0.81	0.78	0.75	0.78	0.79	0.75	0.81	0.75	0.78	0.86	0.86	0.88	0.86	0.88	0.85	0.87	0.85	0.85	0.88	0.84
			0.4	0.70	0.78	0.67	0.72	0.77	0.77	0.72	0.70	0.77	0.72	0.72	0.78	0.79	0.80	0.77	0.78	0.79	0.78	0.79	0.76	0.77	0.77
			0.5	0.62	0.60	0.67	0.59	0.61	0.64	0.62	0.58	0.65	0.62	0.63	0.65	0.67	0.66	0.64	0.63	0.64	0.64	0.65	0.65	0.67	0.63
			0.6	0.55	0.48	0.56	0.56	0.57	0.55	0.56	0.57	0.56	0.55	0.60	0.50	0.56	0.52	0.52	0.51	0.50	0.53	0.53	0.53	0.51	0.51
			0.7	0.52	0.50	0.50	0.48	0.44	0.48	0.51	0.44	0.50	0.52	0.44	0.41	0.40	0.39	0.41	0.39	0.41	0.41	0.41	0.41	0.41	0.41
			0.8	0.40	0.43	0.42	0.34	0.37	0.41	0.42	0.41	0.36	0.36	0.42	0.33	0.33	0.33	0.32	0.32	0.32	0.32	0.33	0.33	0.31	0.35
			0.9	0.34	0.32	0.33	0.33	0.36	0.35	0.34	0.32	0.26	0.30	0.29	0.27	0.25	0.24	0.25	0.24	0.25	0.24	0.27	0.26	0.24	0.25
			1	0.21	0.26	0.27	0.23	0.25	0.24	0.25	0.29	0.25	0.28	0.21	0.19	0.18	0.21	0.19	0.19	0.18	0.18	0.20	0.20	0.19	0.18
	Hydro	A2	0	0.00	0.07	0.41	0.63	0.74	0.81	0.83	0.80	0.81	0.80	0.82	0.00	0.86	0.92	0.93	0.90	0.89	0.86	0.83	0.81	0.77	0.74
			0.1	0.00	0.00	0.16	0.50	0.63	0.70	0.80	0.85	0.91	0.93	0.93	0.00	0.09	0.56	0.85	0.92	0.94	0.95	0.96	0.96	0.96	0.96
			0.2	0.00	0.00	0.07	0.32	0.47	0.73	0.69	0.78	0.81	0.85	0.90	0.00	0.01	0.24	0.55	0.78	0.86	0.93	0.95	0.96	0.96	0.96
			0.3	0.00	0.00	0.04	0.21	0.42	0.49	0.57	0.74	0.71	0.81	0.86	0.00	0.00	0.09	0.35	0.58	0.73	0.83	0.88	0.90	0.95	0.95
			0.4	0.00	0.00	0.02	0.15	0.38	0.48	0.50	0.62	0.73	0.70	0.78	0.00	0.00	0.03	0.21	0.42	0.62	0.74	0.83	0.89	0.91	0.89
			0.5	0.00	0.00	0.01	0.11	0.26	0.37	0.45	0.51	0.62	0.66	0.68	0.00	0.00	0.01	0.12	0.32	0.49	0.66	0.75	0.83	0.86	0.87
			0.6	0.00	0.00	0.00	0.04	0.16	0.36	0.44	0.51	0.55	0.68	0.65	0.00	0.00	0.00	0.07	0.24	0.39	0.54	0.68	0.74	0.82	0.87
			0.7	0.00	0.00	0.00	0.01	0.08	0.21	0.42	0.44	0.51	0.56	0.60	0.00	0.00	0.00	0.05	0.18	0.32	0.47	0.61	0.68	0.78	0.83
			0.8	0.00	0.00	0.00	0.01	0.04	0.15	0.26	0.39	0.48	0.45	0.58	0.00	0.00	0.00	0.03	0.12	0.28	0.42	0.54	0.61	0.74	0.79
			0.9	0.00	0.00	0.00	0.00	0.04	0.10	0.19	0.29	0.41	0.44	0.44	0.00	0.00	0.00	0.02	0.09	0.22	0.36	0.46	0.57	0.65	0.74
			1	0.00	0.00	0.00	0.00	0.01	0.08	0.16	0.23	0.35	0.40	0.41	0.00	0.00	0.00	0.01	0.07	0.18	0.33	0.41	0.55	0.63	0.71
		A3(3)	0	0.85	0.87	0.86	0.86	0.85	0.87	0.88	0.86	0.86	0.86	0.85	0.94	0.95	0.95	0.95	0.94	0.96	0.95	0.95	0.95	0.95	0.95

Pass Model	Extra Mort.	Action	Retro D	Alpha Model											Delta Model										
				Prospective D											Prospective D										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.1	0.91	0.93	0.92	0.92	0.94	0.93	0.91	0.92	0.93	0.93	0.92	0.98	0.98	0.98	0.97	0.98	0.98	0.98	0.98	0.98	0.98	0.98
			0.2	0.89	0.87	0.86	0.86	0.86	0.86	0.86	0.86	0.87	0.87	0.86	0.98	0.98	0.98	0.98	0.97	0.97	0.98	0.97	0.98	0.98	0.97
			0.3	0.82	0.83	0.79	0.83	0.80	0.83	0.80	0.80	0.79	0.80	0.81	0.95	0.97	0.96	0.94	0.95	0.97	0.96	0.97	0.96	0.96	0.97
			0.4	0.77	0.77	0.76	0.86	0.80	0.79	0.76	0.77	0.81	0.75	0.79	0.93	0.96	0.96	0.95	0.95	0.94	0.89	0.94	0.96	0.95	0.95
			0.5	0.76	0.78	0.75	0.77	0.74	0.71	0.73	0.74	0.73	0.75	0.72	0.93	0.94	0.93	0.95	0.93	0.92	0.91	0.93	0.95	0.93	0.95
			0.6	0.75	0.74	0.73	0.77	0.68	0.77	0.73	0.77	0.75	0.71	0.72	0.91	0.90	0.93	0.93	0.94	0.92	0.91	0.93	0.93	0.91	0.94
			0.7	0.71	0.74	0.68	0.69	0.66	0.74	0.73	0.64	0.68	0.71	0.67	0.86	0.91	0.92	0.92	0.92	0.92	0.90	0.92	0.93	0.90	0.90
			0.8	0.67	0.70	0.72	0.70	0.72	0.68	0.68	0.67	0.70	0.63	0.65	0.85	0.91	0.91	0.92	0.89	0.92	0.91	0.92	0.90	0.90	0.91
			0.9	0.66	0.67	0.66	0.70	0.73	0.71	0.70	0.65	0.75	0.70	0.64	0.83	0.89	0.90	0.90	0.87	0.89	0.90	0.89	0.89	0.89	0.92
			1	0.64	0.69	0.73	0.71	0.63	0.68	0.68	0.63	0.62	0.72	0.59	0.80	0.87	0.88	0.89	0.90	0.89	0.88	0.87	0.86	0.90	0.88
		A3(8)	0	0.81	0.83	0.83	0.81	0.83	0.84	0.85	0.82	0.83	0.81	0.84	0.90	0.91	0.90	0.91	0.90	0.91	0.91	0.91	0.90	0.90	0.89
			0.1	0.89	0.91	0.91	0.92	0.90	0.90	0.90	0.90	0.92	0.93	0.90	0.97	0.98	0.97	0.98	0.98	0.97	0.98	0.97	0.98	0.97	0.97
			0.2	0.81	0.86	0.86	0.85	0.85	0.83	0.81	0.85	0.82	0.82	0.84	0.93	0.97	0.96	0.96	0.97	0.97	0.97	0.96	0.96	0.96	0.97
			0.3	0.70	0.77	0.82	0.79	0.76	0.78	0.80	0.76	0.81	0.76	0.78	0.84	0.95	0.96	0.94	0.96	0.94	0.96	0.92	0.94	0.96	0.94
			0.4	0.65	0.75	0.70	0.75	0.80	0.79	0.73	0.71	0.78	0.76	0.74	0.78	0.92	0.94	0.94	0.95	0.94	0.94	0.94	0.93	0.94	0.94
			0.5	0.47	0.70	0.75	0.67	0.64	0.67	0.66	0.62	0.72	0.65	0.68	0.74	0.90	0.93	0.93	0.91	0.93	0.93	0.93	0.92	0.92	0.90
			0.6	0.36	0.67	0.70	0.70	0.69	0.65	0.69	0.71	0.64	0.66	0.72	0.68	0.85	0.90	0.91	0.92	0.91	0.91	0.92	0.90	0.92	0.89
			0.7	0.36	0.68	0.67	0.65	0.62	0.65	0.64	0.64	0.65	0.71	0.58	0.63	0.85	0.88	0.90	0.90	0.89	0.90	0.91	0.92	0.90	0.91
			0.8	0.24	0.60	0.64	0.63	0.65	0.64	0.62	0.58	0.63	0.54	0.65	0.59	0.79	0.86	0.88	0.90	0.90	0.89	0.85	0.90	0.88	0.86
			0.9	0.19	0.50	0.63	0.65	0.62	0.65	0.63	0.63	0.60	0.65	0.60	0.54	0.82	0.84	0.87	0.87	0.88	0.88	0.86	0.88	0.87	0.87
			1	0.17	0.46	0.61	0.62	0.61	0.61	0.60	0.56	0.63	0.59	0.59	0.52	0.79	0.81	0.84	0.86	0.87	0.87	0.87	0.87	0.86	0.88
	Reg. Shift	A2	0	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
			0.1	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
			0.2	0.08	0.08	0.08	0.07	0.08	0.09	0.08	0.08	0.08	0.07	0.08	0.10	0.10	0.10	0.10	0.09	0.10	0.10	0.10	0.10	0.10	0.10
			0.3	0.24	0.24	0.24	0.22	0.24	0.23	0.26	0.24	0.24	0.24	0.22	0.33	0.32	0.32	0.32	0.32	0.31	0.31	0.33	0.31	0.33	0.33
			0.4	0.45	0.38	0.37	0.34	0.40	0.40	0.35	0.39	0.43	0.35	0.39	0.48	0.49	0.49	0.51	0.47	0.48	0.48	0.48	0.48	0.47	0.47
			0.5	0.48	0.41	0.44	0.44	0.45	0.45	0.44	0.41	0.45	0.46	0.41	0.60	0.58	0.55	0.57	0.59	0.57	0.59	0.59	0.61	0.59	0.57
			0.6	0.45	0.47	0.64	0.52	0.53	0.56	0.55	0.53	0.50	0.61	0.52	0.65	0.66	0.67	0.65	0.60	0.54	0.63	0.66	0.64	0.67	0.68
			0.7	0.59	0.52	0.55	0.52	0.53	0.52	0.62	0.57	0.57	0.55	0.56	0.71	0.70	0.75	0.69	0.67	0.64	0.67	0.71	0.70	0.72	0.72
			0.8	0.57	0.57	0.62	0.49	0.59	0.61	0.71	0.62	0.63	0.55	0.62	0.67	0.74	0.72	0.78	0.75	0.71	0.77	0.77	0.69	0.76	0.78
			0.9	0.62	0.57	0.54	0.54	0.65	0.64	0.53	0.48	0.63	0.60	0.55	0.73	0.78	0.77	0.77	0.80	0.76	0.79	0.78	0.74	0.73	0.76
			1	0.63	0.62	0.60	0.67	0.63	0.68	0.61	0.69	0.59	0.62	0.61	0.80	0.80	0.80	0.74	0.81	0.82	0.81	0.79	0.78	0.80	0.80
		A3(3)	0	0.86	0.86	0.86	0.87	0.85	0.85	0.89	0.87	0.87	0.87	0.86	0.98	0.98	0.99	0.99	0.98	0.99	0.99	0.99	0.99	0.98	0.99
			0.1	0.92	0.92	0.92	0.91	0.93	0.92	0.92	0.93	0.93	0.93	0.91	0.98	0.97	0.98	0.96	0.97	0.98	0.98	0.98	0.97	0.97	0.98
			0.2	0.88	0.86	0.86	0.86	0.86	0.85	0.87	0.86	0.88	0.88	0.87	0.97	0.96	0.96	0.97	0.97	0.96	0.97	0.96	0.96	0.96	0.96
			0.3	0.81	0.83	0.79	0.82	0.81	0.82	0.81	0.81	0.78	0.81	0.81	0.91	0.94	0.94	0.92	0.92	0.94	0.94	0.95	0.94	0.95	0.95
			0.4	0.75	0.75	0.75	0.85	0.79	0.78	0.76	0.77	0.81	0.74	0.81	0.91	0.92	0.93	0.92	0.92	0.89	0.84	0.90	0.93	0.90	0.93

Pass Model	**Extra Mort.**	**Action**	**Retro D**	**Alpha Model**											**Delta Model**										
				Prospective D											**Prospective D**										
				0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1	0	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
			0.5	0.74	0.76	0.72	0.75	0.74	0.69	0.72	0.75	0.72	0.74	0.72	0.91	0.90	0.89	0.92	0.88	0.87	0.85	0.89	0.91	0.90	0.92
			0.6	0.74	0.70	0.71	0.74	0.63	0.75	0.70	0.76	0.75	0.70	0.69	0.89	0.83	0.87	0.87	0.90	0.90	0.86	0.91	0.90	0.84	0.91
			0.7	0.69	0.69	0.65	0.62	0.61	0.72	0.70	0.61	0.63	0.69	0.65	0.89	0.82	0.87	0.87	0.86	0.89	0.84	0.87	0.86	0.84	0.83
			0.8	0.57	0.63	0.64	0.59	0.69	0.63	0.64	0.64	0.68	0.58	0.62	0.84	0.84	0.85	0.85	0.80	0.86	0.86	0.85	0.83	0.83	0.84
			0.9	0.60	0.58	0.58	0.67	0.67	0.67	0.67	0.62	0.74	0.66	0.62	0.82	0.82	0.81	0.82	0.73	0.80	0.82	0.81	0.80	0.78	0.83
			1	0.53	0.62	0.69	0.63	0.55	0.62	0.67	0.57	0.56	0.68	0.56	0.82	0.83	0.77	0.78	0.81	0.79	0.74	0.78	0.76	0.81	0.76
		A3(8)	0	0.82	0.82	0.82	0.81	0.83	0.82	0.83	0.81	0.82	0.79	0.84	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99
			0.1	0.91	0.91	0.91	0.92	0.89	0.91	0.90	0.91	0.92	0.92	0.90	0.97	0.98	0.97	0.98	0.98	0.97	0.97	0.97	0.98	0.98	0.97
			0.2	0.87	0.86	0.85	0.86	0.84	0.84	0.82	0.85	0.84	0.84	0.85	0.96	0.95	0.95	0.94	0.95	0.95	0.95	0.96	0.94	0.95	0.96
			0.3	0.74	0.76	0.82	0.78	0.76	0.78	0.79	0.77	0.82	0.76	0.79	0.93	0.93	0.93	0.91	0.94	0.92	0.94	0.91	0.91	0.94	0.90
			0.4	0.74	0.80	0.70	0.74	0.79	0.79	0.74	0.73	0.79	0.73	0.73	0.91	0.89	0.91	0.91	0.90	0.91	0.90	0.90	0.88	0.89	0.91
			0.5	0.64	0.65	0.75	0.64	0.63	0.68	0.66	0.62	0.72	0.67	0.68	0.84	0.89	0.88	0.86	0.86	0.89	0.87	0.87	0.89	0.87	0.84
			0.6	0.68	0.58	0.65	0.68	0.67	0.63	0.68	0.71	0.65	0.65	0.72	0.82	0.85	0.84	0.86	0.84	0.85	0.85	0.85	0.84	0.85	0.80
			0.7	0.66	0.68	0.65	0.63	0.58	0.63	0.62	0.61	0.65	0.71	0.55	0.83	0.80	0.79	0.81	0.81	0.81	0.82	0.83	0.82	0.83	0.84
			0.8	0.59	0.67	0.63	0.51	0.60	0.60	0.59	0.57	0.62	0.50	0.65	0.82	0.76	0.78	0.79	0.79	0.79	0.78	0.71	0.79	0.75	0.73
			0.9	0.60	0.53	0.63	0.59	0.57	0.65	0.61	0.61	0.58	0.61	0.56	0.71	0.75	0.73	0.75	0.75	0.77	0.75	0.73	0.75	0.76	0.76
			1	0.59	0.62	0.53	0.50	0.65	0.57	0.56	0.47	0.60	0.56	0.53	0.68	0.69	0.73	0.72	0.74	0.71	0.72	0.72	0.73	0.71	0.72

Appendix F: SRP Comments on Experimental Management

Comments from the SRP on Chapter 6 of the FY98 Report are included below. Some of these reviews follow the standard format for reviews while others do not.

F.1 Steve Carpenter

a) scientific soundness of the methodology

This is an excellent summary of adaptive management with some preliminary thoughts on possible experimental designs gleaned from the Sept 1998 workshop.

b) general suitability of the data for use in the analyses

The experimental designs discussed are well grounded in the uncertainties and alternative hypotheses developed for spring chinook.

c) validity of inferences and conclusions reached

On p. 222, Table 6.3-1, 2-pool drawdown, I did not understand why the next-to-last design was considered most risk-averse. Couldn't the first design, with experimentation on both hatcheries and transport, be equally risk-averse? Maybe risk aversion needs to be quantified through model runs.

d) suggestions for improvements and extensions to the analytical approaches used

p. 219 bottom: This paragraph really makes 2 points, one about passive versus active management and the other about various time series methods for interpreting unreplicated ecosystem experiments. These points should be addressed separately.

First, I have repeatedly stressed the importance of strong, sustained manipulations for ecosystem experiments. This is an active approach. Carpenter (1998) is a good summary of these arguments. For analyses that address treatment strength for specific proposed ecosystem experiments in midwestern lakes, see Carpenter (1989) and Carpenter et al. (1995). The general argument for strong, sustained manipulations is independent of the particular statistical approach chosen for any given experiment.

Second, I would not rule out the possibility of using time-series approaches for adaptive management experiments in the Snake/Columbia system. In some cases, substantial baseline time series already exist. The original paper by Box and Tiao (based on passive adaptive data) obtained insights from relatively short time series. With active approaches, one may have even more sensitivity (Carpenter 1993). Finally, clever choices of reference ('control') ecosystems can sometimes improve the ability to discriminate alternative models in ecosystem experiments, using relatively short time series (Carpenter et al. 1998). Other streams in the region, or other tributaries to the Columbia that enter downstream of the Snake, may provide informative reference ecosystems.

It is important to note that some responses to adaptive management in the Snake/Columbia will be "one-way trips" and Box-Jenkins type time series methods will not work. This is not an unfamiliar problem in fisheries analysis (Hilborn and Walters 1992). The "one-way trips" will pose a special set of challenges for comparing alternative models. In other cases, however, baseline data exist and stock responses may show a "two-way trip" (down, then up). It may prove easier to discriminate alternative models for these cases.

e) opportunities for integration of the different component analyses into an adaptive management approach

That's the whole point of this section.

We must be cautious not to promise too much too fast. For example, the top of p. 220 states that studies taking many decades are inconsistent with salmon recovery. **Delaying** many decades is inconsistent with salmon recovery, but certain experiments initiated promptly are consistent with salmon recovery. Even if we initiate recovery now it could take many years to learn. We should be acknowledge that clear evidence for and against various hypotheses could take a decade or more to develop.

f) relative priorities for future work on these analyses

The general plan laid out in the section seems appropriate.

F.2 Jeremy Collie

c) validity of inference and conclusions reached

p 216: Active adaptive management is better than passive adaptive management in the sense that it is always better to account for learning about uncertain parameters. If the best passive adaptive policy (action) is quite different than the status quo, learning rates about uncertain parameters may be sufficiently fast that a more experimental policy (action) is unnecessary (Collie and Walters 1991, 1993).

Table 6.2-2: From the previous PATH analyses, the key management indicators are known (Step 4).

Table 6.3-1: I think that this table of possible experimental manipulations is very useful. The 2-pool drawdown option has merit provided that it is started soon. If delayed it would not be sufficient to meet the survival standards. A 2-pool drawdown would be very informative about two of the key uncertainties in Table 6.2-3: the length of the transition period to equilibrium conditions and juvenile survival rate after drawdown. A 2-stage implementation of the 4-pool drawdown could also reduce temporal confounding of factors affecting survival by virtue of its "staircase design" (Walters et al. 1988, CJFAS 45:530-538).

It may be impractical and risky to turn hatchery production on and off for periods of time. Impractical because of the need to maintain brood stock and risky because the predator populations in reservoirs could inflict a depensatory predation rate on wild salmon smolts in years of low hatchery production. If depensatory predation is a risk, hatchery production may need to be reduced more gradually. The option of Intensive hatcheries and Intensive/reduced transportation may be more feasible for resolving the confounding between transportation and hatcheries.

The options in this table consider mainly temporal comparisons, such as intensive/reduced transportation and hatcheries. The spatial scale of experimentation and the possibilities for spatial contrasts should also be considered. The spatial scale is generally quite large and involves comparisons between Snake River stocks and other Columbia River stocks. However, it may be possible to establish up-river/down-river comparisons, e.g., by transporting smolts only from the up-river dams and allowing lower river stocks to migrate in the river.

Figure 6.4-1: I agree that the SRP did recognize the need for simpler life-cycle models for evaluating experimental options.

d) *suggestions for improvements and extensions to the analytical approaches used*

e) *opportunities for integration of the different component analyses into an adaptive management approach*

p 223: PATH could calculate the Expected Value of Perfect Information for the key uncertainties in Table 6.2-3. These calculations would suggest how much it is worth to resolve key uncertainties. PATH could also extend the prospective models to simulate the collection of new data and thereby the rate of learning about uncertain hypotheses. The methodology for this type of simulation is outlined in Walters (1986) book; example applications are (Collie and Walters 1991, 1993). The general question is "If a certain hypothesis is correct (e.g., equilibrated juvenile survival rate) how long would it take to detect it under the different actions. These types of simulations determine how much of the EVPI is realistically attainable.

f) *relative priorities for future work on these analyses*

I give a higher priority to the experimental management tasks than to further sensitivity analyses. I am afraid that additional sensitivity analyses of new factors may delay the implementation of management actions, and thereby make the survival standards more difficult to attain. Proceeding with the experimental management tasks will focus attention on key uncertainties in the life-cycle models, and also on the types of monitoring that will be required to measure the performance of management actions.

F.3 Saul Saila

I believe that the description and explanation of the methodology were very effectively presented. I do not consider it necessary or appropriate to follow the review guidelines for this section. There is no question but that the methodology is sound, and the available data and results are suitable for experimental management.

The only suggestion I can make at this point is that the incorporation of the precautionary principle should be explicitly made in the experimental management plan. Although the example provided in the following reference applies to a forest-wetland environment example, it may provide some useful ideas to incorporate into this Experimental Management Section. The reference is:

> **Rogers, M.F., J.A. Sinden, and T. DeLacy.** 1997. The precautionary principle for environmental management: A defensive expenditure application. J. Environ. Manage. 51: 343-360.

F.4 Carl Walters

I agree that your top priority now should be the development and evaluation of alternative experimental designs. You have sketched out some design alternatives, and the trick mentioned above [sampling hypotheses to reduce # of runs] can be used to develop an efficient screening procedure for design alternatives. Your main emphasis in design analysis now should be on careful modeling of future data gathering and how to represent how that data will be analyzed and interpreted. In particular, you should pay considerable attention to whether any experimental response measures besides net escapement change should be used as indicators of response. All my intuition is that experimental results should be judged only in terms of net abundance response, with auxiliary data collection (and expense) justified only in so far as it may help split more detailed alternatives.

Carl Walters' Addenda by email

-----Original Message-----
From: walters@fisheries.com [SMTP:walters@fisheries.com]
Sent: Wednesday, April 14, 1999 9:23 AM
To: dmarmorek@msmail.essa.com
Subject: Experimental management section, PATH 98

Had a chance to look more closely at Section 6 (experimental management), and have two additional (both nasty) comments to add to my review report:

1. Figure 6.2-2 (adaptive mgmt vs. basic research) is deeply, fundamentally misleading. In complex dynamic systems, "basic research" does not "maximize learning"; such research cannot in principle deal with the kind of deep confounding of effects that you demonstrate by excellent example in Fig. 6.3.1. Research can help in hypothesis generation, and in posterior explanation of experimental response patterns, but it is simply wrong to claim that it can in any way substitute for actually seeing the responses to be explained! The correct distinction is just active/passive, and here the real issue is whether passive approach "hides" (or fails to test, or fails to reveal) responses, i.e., whether it prevents learning.

2. The data imply strong trends in total mortality rate Zt that cannot confidently be attributed to (corrected for) known factors like passage survival changes. This means that a) for "irreversible" treatments (like dam removal) there is a high risk that post-pre estimates of treatment effect would be wrong, i.e., high risk that real response is due to some Z factor other than treatment, and there is no way to "control" for this risk; and b) for reversible treatments (transportation, hatchery, flow, etc.) confounding of treatment responses with other factors possibly causing Z change can only be avoided by interspersing (blocking) treatment and reference comparisons, i.e., by regularly operating the system under a reference treatment option in order to measure changes over time in Z due to factors other than the management treatment. This requirement for temporal reference comparisons greatly increases the time needed for effective experimentation, and seriously calls into question your representation of experimental options as incremental vs. reverse staircase. Actually, you cannot conduct a staircase experiment at all for whole-system treatments like dam removal, since such experiments are defined not by treatments within an experimental unit over time but rather by starting treatment on different experimental units at different times (i.e., you are wrong to represent years as experimental units in defining what you call a reverse staircase design).

-----Original Message-----
From: walters@fisheries.com [SMTP:walters@fisheries.com]
Sent: Sunday, April 18, 1999 10:10 AM
To: dmarmorek@msmail.essa.com
Subject: PATH experimental management advocacy

It might be possible to obtain consensus in PATH about priorities for experimental tests of restoration options, if you can get their minds off the A-B set that has caused so much controversy. To start the discussion, how about this sequence:

1. direct reduction in presumed reservoir mortality via subsidized commercial fishery: passage models say reservoir (not dam) mortalities are biggest problem; for $10M/yr (5% of dam removal extra power cost), could provide 50K contracts/licenses (gear, salary costs, etc.) to 200 knowledgeable commercial salmon fishers, to essentially remove predation risk from the Snake reservoirs for a few years (and make a few $ selling walleyes, catfish, etc.) by late winter/spring

fishing during times when few upstream migrating salmon would be at risk to big gill nets and such. Failure of this test would an almost sure sign that the much more expensive dam removal also would not do the job. Think of the wonderful side components, like allocating some of those contracts to the farmers who stand to lose most from dam removal, having spring walleye eating festivals, etc.

2. severe reduction in hatchery releases: people keep talking about competition/genetic pollution effects of hatchery fish; in fact the biggest risk they pose is as disease reservoirs/concentrators (BKD etc.). With most fisheries shut down anyway, the cost of this policy in terms of lost fishery production would be minimal in any case. Subject any hatcheries that are allowed to continue operation to severe disease prophylaxis (e.g., 100% BKD egg testing).

3. stop transportation, assuming the models do in fact predict increased overall passage survival due to elimination of D effects. This would resolve the D issue once and for all, whereas "maximizing transportation" as you've discussed before would still leave a cloud of uncertainty about just how effective this maximization had really been at even capturing higher proportions of total downstream migrants.

All my instincts are that tests like these should precede any irreversible dam removal trials.