EVALUATION AND APPLICATION OF STATISTICAL METHODS FOR ESTIMATING SMOLT SURVIVAL

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EXECUTIVE SUMMARY

Existing protocols and associated statistical theory for estimating survival of smolts during their outmigration were evaluated to determine their application to deriving estimates of reach survival. Our evaluation indicated that survival of smolts through a reach can be reliably estimated even though survival rates vary among individuals and passage routes of hydroelectric projects. Paired releases of treatment and control groups are appropriate for estimating survival of smolts past a point impact zone (i.e., turbines, bypass system), but less appropriate for estimating reach survival. Measures of collection efficiency do not provide adequate estimates of reach survival because of variable operating conditions at hydroelectric dams. A proposed design was developed for the Snake River, based on release-recapture methodology, with a set of assumptions easier to fulfill than those of survival protocols currently in use. The study would include a primary release of fish above Lower Granite Dam and a set of secondary releases in the bypass system at Lower Granite Dam to estimate any post-detection mortality. The secondary releases are independent of the primary release and may experience unique reach survival and detection probabilities. Collectively, these sets of Passive Integrated Transponder (PIT) tag releases should provide a reliable estimate of reach survival from a point of release above Lower Granite Dam through Lower Granite Dam. If the secondary releases indicate that detection/bypass mortality is insignificant, the experimental design requires that only a primary release group be used to estimate reach survival.

Yearling chinook salmon and steelhead appear to be the most suitable groups of fish for evaluation of survival. Sample size calculations based on the modified release-recapture model indicate reasonable levels of precision (i.e., 95% confidence intervals of less than $\pm$ 5%) can be obtained with moderate sizes of PIT-tag fish releases (n = 1500 for the primary release and n = 1500 for secondary releases) and that these levels of study performance are feasible with current PIT-tag facilities on the Snake River. Calculations were based on the operation of the decoder and slide-gate facilities at Lower Granite Dam and decoder facilities at Little Goose and McNary dams. Fish

transportation programs at Little Goose and McNary dams could operate during the survival study with only PIT-tagged fish being diverted back in the river at Lower Granite Dam.

Field testing of the proposed survival protocol is the next step in evaluating the estimation technique. If testing is successful, a larger scale study design should be devised to determine if relationships exist among smolt survival and parameters of interest (i.e., flow, smolt condition).

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1.0 INTRODUCTION

Implementation of a successful recovery plan for depressed stocks of salmon and steelhead in the Columbia and Snake river systems requires reliable methods for obtaining smolt survival estimates through key reaches of the system. Valid survival estimates are necessary to monitor the effectiveness of proposed management strategies. In the past, fisheries scientists and managers have relied on general system mortality estimates as a measure of smolt survival (Raymond 1979; Sims and Ossiander 1981; Sims et al. 1983). However, the methods used by Raymond et al. (1974, 1975), Raymond (1979), Sims and Ossiander (1981), and Sims et al. (1983) failed to address model assumptions and provided no assessment of bias or measures of precision.

More recently, Burnham et al. (1987) described release-recapture models that can be used to estimate survival. The primary objective of their descriptions is to estimate the relative survival between treatment and control releases of tagged fish. The approach is useful in estimating passage mortality (i.e., turbine, spill, or bypass mortality) at hydroelectric facilities. However, the paired release-recapture methods of Burnham et al. (1987) are generally inappropriate for estimating reach survival because the key assumption (random mixing of control and treatment fish downstream) to assure coincident passage through reaches and at recovery sites is not likely to be met. Random mixing of control and treatment releases becomes less probable as the distance between release and recovery points increases.

The single release-recapture likelihood presented in Burnham et al. (1987) is a special case of the Jolly-Seber (Jolly 1965; Seber 1965) model and can estimate reach survival without control and treatment releases mixing downstream. The estimation technique relies on a single release of Passive Integrated Transponder (PIT) tagged fish with captured/detected fish released at downstream recovery sites. The assumptions of the single release-recapture model are easier to fulfill than the paired-release methods, and the method is logistically easier to implement. The estimation method is dependent upon the use of PIT-tag detectors and slide-gate facilities at hydroelectric projects to return detected fish back into the river after they have gone through the bypass system.

We prqpose to evaluate in the field, methods of estimating reach survival based on a modification of the single release-recapture model. In analyzing the assumptions of the single release-recapture model of Burnham et al. (1987), we found the model to be sensitive to the presence of post-detection bypass mortality. Mortality of marked fish at the PIT-tag detector, or shortly thereafter in the slide-gate/diverter and before mixing with undetected fish, resulted in biased reach survival estimates. The modified single release-recapture method presented here can estimate any post-detector bypass mortality and provide reach survival estimates that are corrected for bias. The field evaluation of the release-recapture methods would include replicate trials to determine the reproducibility of the estimation technique and estimation of the natural variation in reach survival over time.

The report begins with the proposed reach survival study. Recommendations for the study include location, test species, sample sizes, and level of replication. We then review three alternative reach survival estimation schemes: 1) paired release-recapture methods, 2) single release-recapture method, and 3) smolt abundance estimator based on fish collection efficiency (CE). These evaluations ultimately lead to the recommended study design. The report concludes with recommendations on the most appropriate estimation schemes to use with alternative survival estimation objectives. More detailed information on model simulations and statistical theory used in our evaluations is provided in the appendices.

2.0 REACH SURVIVAL TO LOWER GRANITE DAM: PILOT STUDY (1993-1994)

To be successful, any experiment must correctly consider logistical constraints. Our intent is to outline these experimental considerations in sufficient detail that researchers and managers can evaluate their needs and consider constraints when designing specific experiments. In designing a survival study for the Snake/Columbia rivers, expectations of the objectives and outcomes of the initial study are quite varied and no single study is likely to satisfy the expectations of everyone. However, some basic goals and limitations were established for an initial survival study. The framework for the proposed study is as follows:

- The primary goal of the survival study is to assess the feasibility of estimating smolt survival through reaches of the Snake and/or Columbia rivers and their tributaries.
- The study design must be implementable, given the facilities, marking, and handling techniques available in Spring 1993.
- The specific river reaches over which survival is estimated are not as important as demonstrating the methodology and assessing the estimator method.
- Similarly, the inferences to specific fish stocks (e.g., hatchery vs. wild) are of secondary concern at this time.

It is anticipated that following an initial evaluation of survival estimation methodology, the focus of future studies will shift to the more relevant issues of estimating smolt survival through key reaches, comparing survival rates among stocks, and evaluating factors that influence smolt survival. The design of survival studies to assess effects on smolt survival will not be addressed in this report.

This section of the report provides a proposed study design that could be used to estimate survival of juvenile salmon migrating through a reach of the Snake River (Figure 2.1). This design is generic in that it could also be applied to other locations, including the Yakima River, if suitable recapture facilities are available. We also discuss test species selection and the required number of PIT-tagged fish released per study. Finally, we present

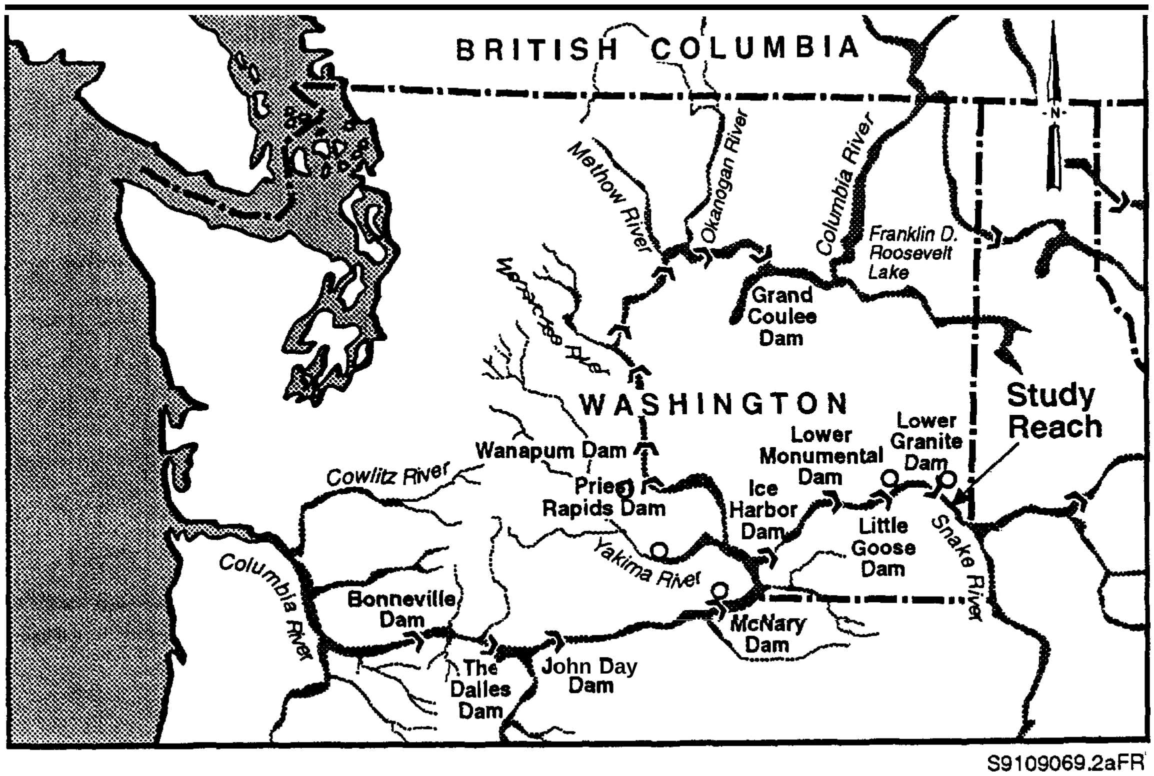

FIGURE 2.1. Map of the Columbia River Basin, Indicating Location of Hydroelectric Dams and Current PIT-Tag Facilities (0)

the number of replicate trials to evaluate the protocol and the precision and accuracy of the survival estimates under different test conditions. More detailed description of the study design and results of the model simulations can be found in Appendix A.

2.1 PROPOSED STUDY DESIGN

The proposed design consists of two components: 1) a primary release group of PIT-tagged fish released at the head of the reach of interest (R,) and 2) two secondary release groups of PIT-tagged fish released in the vicinity of the PIT-tag detectors/diverters (R_C and R,; see Figure 2.2). For our study, the primary release (R,) will occur above Lower Granite Dam, the reason for this is to estimate smolt survival between the point of release and the tailrace of Lower Granite Dam. It will be necessary for the slide-gate facilities at Lower Granite Dam to operate while these fish pass the dam. Reach

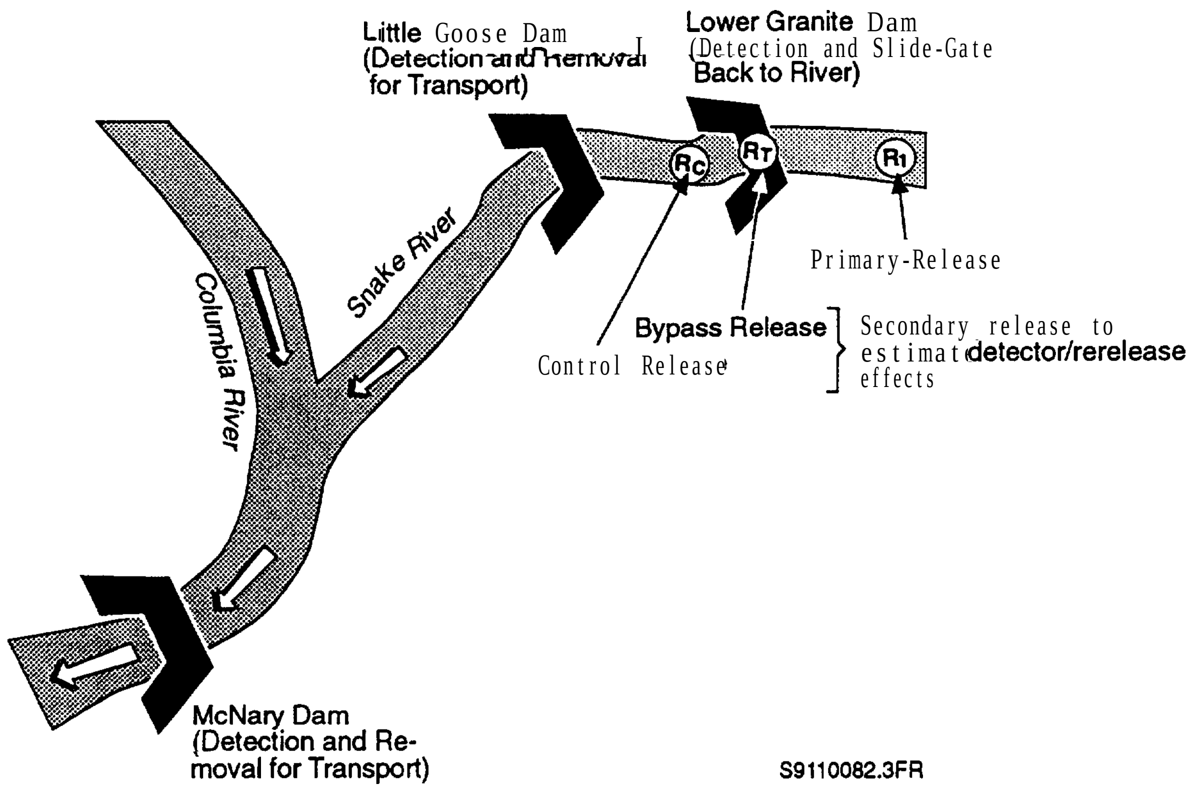

FIGURE 2.2. Conceptual Diagram of the Proposed Study Site in the Snake River and of Release/Recapture Locations

survival will not be estimable unless the PIT-tagged fish are rereleased back into the river. Fish from the primary release will also be detectable at the decoder facilities at Little Goose and McNary dams. Fish detected at Little Goose and McNary dams can enter transportation programs without compromising estimates of reach survival above Lower Granite Dam.

Concerns have been raised regarding the potential for post-detection mortality occurring as the result of the design and/or operation of fish bypass systems. For example, a key assumption in all rerelease-recapture protocols is that appreciable mortality is not associated with detection and rerelease. To address this issue, two secondary releases of PIT-tagged fish (i.e., R_C and R_T) are required to concurrently estimate any detection/bypass mortality at Lower Granite Dam. The objective of these releases is to determine if PIT-tagged fish that travel through the detector system incur mortality associated with detection/bypass. Since the PIT-tag detector is housed in the bypass, the evaluation must include any bypass-related effect below the detector device. The secondary release groups will be released above the zone of potential detector/slide-gate effects (R_T or treatment group) and in the tailrace below, the dam (R_C or control group). The control group will be dispersed across the downstream face of the dam and approximately aligned with the outfall. The secondary releases will be timed to coincide with the passage of the primary release group (R,). Mixing of the primary and secondary release groups is not necessary for valid estimation of reach survival. Rather, downstream mixing of the two secondary release groups among themselves is necessary for valid estimation of bypass mortality. If significant mortality occurs, this estimate can be used to adjust the estimate of reach survival.

Appropriate adjustment of the reach survival estimate is predicated upon primary and secondary release groups experiencing the same post-detection bypass mortality. If such mortality exists (e.g., as a result of mechanical damage in the detector or slide-gate, delayed stress-related effects, or predators concentrated near the bypass outfall), it is likely to be relatively invariant over short periods of time and over fish stocks. For this reason, fish stocks comprising the primary and secondary releases need not be the same. However, fish stocks used for the secondary releases should be the same race and general size range as fish in the primary release group. Conditions or assumptions of our proposed single-release procedure are as follows:

1. Test fish are representative of the population of interest.
2. Test conditions are representative of the conditions of interest.
3. Numbers of fish released (i.e., R,, R,, and R_T) are exactly known.
4. Marking is accurate, no handling mortality or tag loss occurs, and tags are read correctly.
5. Initial release is instantaneous and all detected fish are rereleased immediately upon detection. These are requirements for making inference to river conditions, not estimation per se.
6. Fate of each tagged fish is independent of those of other tagged fish.

7. If replicate studies are conducted, then the data from different releases are statistically independent.

8. All fish within the primary release group (R,) or within secondary release groups (R_C and R,) have the same capture probability at a particular detector site.

9. All fish within the primary release group (R,) or within secondary release groups (R, and R_T) have the same survival probabilities.

10. Secondary release groups (R_C and R_T) mix below Lower Granite Dam and move downriver together.

11. Fish detected at Lower Granite Dam may have a different mortality rate (τ) from that of undetected fish. This mortality rate is the same for both the primary (R,) and the secondary (R, and R_T) release groups.

Several of these assumptions (i.e., Assumptions 1, 2, 6, and 7) are implicit conditions of any research endeavor. However, other assumptions (i.e., Assumptions 3, 4, and 5) are logistical considerations of any well-designed tag-release investigation. The pivotal assumptions of homogeneous survival and capture probabilities (i.e., Assumptions 8 and 9) are typically of concern to fisheries scientists and will be discussed further in this report. Two additional assumptions have been added to the model (i.e., Assumptions 10 and 11) as part of the proposed study design. Assumptions 10 and 11 are necessary conditions to estimate post-detection bypass mortality and to adjust the reach survival estimates accordingly. The design of subsequent survival studies could be simplified to include only the single-release groups upstream from Lower Granite Dam, if detection/bypass-related mortality is found to be negligible.

The proposed study design for the PIT-tag study will yield the following key information:

1. An estimate of reach survival (S) from point of release of the primary group (R,) through Lower Granite Dam and associated variance.

2. An estimate of detector/bypass mortality at Lower Granite Dam and associated variance.

2.2 TEST SPECIES AND SOURCE

Yearling chinook salmon and steelhead appear to be the most appropriate species for evaluations of the reach survival estimates. Both species are guided through bypass systems at reasonable rates and both readily migrate. As one alternative, active migrants could be acquired from a collection system at a planned upstream release site, from a scoop or inclined plane trap deployed at some upstream recovery site, or from a **forebay** sampling device such as a Merwin trap. It could take several days to obtain the fish numbers needed for a single release at some trap sites. Thus, appropriate holding facilities would need to be established near the traps. Alternatively, hatchery stocks provide a ready source of tagged fish for replicate evaluations. Therefore we recommend using hatchery fish to initially evaluate survival estimation.

There is interest in the region that both life stages of chinook should be considered (i.e., yearling stream-type and subyearling or ocean-type) if a survival study is to emphasize or to focus on Endangered Species Act (ESA) issues. However, subyearling chinook would be a difficult test species because they do not migrate readily, they incur high mortality from predation, and they are guided at low rates at recovery sites. These attributes could affect the precision of survival estimates and would require higher release numbers than outlined in the next section of this report. In our opinion, there are currently too few wild/natural subyearling chinook migrants in the Snake River to consider using them for the proposed survival studies. Fish proposed for the R_C and R_T releases would be either run-of-the-river chinook yearlings collected at trap sites upriver of Lower Granite Dam or fish taken from the collection facility at the dam.

2.3 SAMPLE SIZE REQUIREMENTS

There are three sample sizes (R,, R,, and R_T) that must be specified in the design of this study. To simplify sample size calculations, we allocated 50% of the total sample size of PIT-tagged fish to the primary release R,, and 25% to each of the secondary releases (R_C and R_T) used in estimating post-detection bypass mortality. This allocation serves as a general guide, but will not necessarily be optimal for all sampling conditions. Too great an allocation to samples R_C and R_T will result in an unnecessary loss of precision of S. Alternatively, too few fish allocated to R_T and R_C may result in an imprecise evaluation of any post-detection bypass mortality that may exist. Precision of the reach survival estimate is defined as:

$$P\left(|\hat{S} - S| < \epsilon\right) = 1 - \alpha \qquad (2.1)$$

where the absolute error in estimation i.e., $|\hat{S} - S|$, is $< \epsilon$ $(1 - \alpha)$ 100% of the time. For example, a precision of $\pm$ 0.05, 95% of the time, is expressed as the probability, $P(|\hat{S} - S| < 0.05) = 0.95$. Here, precision is equivalent to the expected half-width of a 95% confidence interval being $\pm$ 0.05.

The anticipated precision of the reach survival estimate to Lower Granite Dam as a function of a range of spill and survival rates is given in Table 2.1 for release sizes 3000 and 5000. For example, with sample sizes of R_1 = 1500, R_C = R_T = 750, and reach survival of S = 0.6 under no spill conditions, the anticipated precision is $\pm$ 0.04, 95% of the time. Figure 2.3 provides precision of reach survival estimates for a wide range of release sizes (1000-15,000). Inspection of Figure 2.3 indicates that the anticipated precision improves little with release sizes greater than 3000 to 5000 PIT-tagged fish. Therefore, we recommend the release groups in the initial pilot study consist of a total sample size of 3000 fish, R_1 = 1500 and R_C = R_T = 750. More informed decisions concerning release sizes can be made after inspection of the performance of initial pilot studies.

We further recommend the pilot study consist of replicated releases to determine the reproducibility of the test results and estimate the natural variation in reach survival over time. The numbers of replicate trials depends on the average sampling error of the individual tests [i.e., $Var(\hat{S}_i|S_i)$] and the magnitude of the natural variation in survival (i.e., σ_s^2) between trials. The overall variation in the estimate of mean survival among k replicate trials is expressed as:

TABLE 2.1. Predicted Precision (ϵ)[a] for a Single Replicate of the Modified Single Release-Recapture Model in Estimating Reach Survival (S) and Post-Detection Mortality (τ) Where $\tau = 0.9$. Precision is estimated for two different release sizes under various spill levels and target values of survival (S) at $(1-\alpha)$ 100% = 95%. These predictions are based on 1000 simulations.

S,	Percent Spill	ϵ at R = 3000[c]		ϵ at R = 5000[b]	
		S	τ	S	τ
0.2	0	0.0275	0.0034	0.0186	0.0021
	20	0.0350	0.0048	0.0240	0.0029
	40	0.0459	0.0072	0.0353	0.0043
	60	0.0812	0.0118	0.0564	0.0071
0.6	0	0.0424	0.0034	0.0315	0.0021
	20	0.0599	0.0048	0.0459	0.0029
	40	0.0849	0.0072	0.0586	0.0043
	60	0.1384	0.0118	0.0966	0.0071
0.9	0	0.0499	0.0034	0.0358	0.0021
	20	0.0769	0.0048	0.0553	0.0029
	40	0.1047	0.0072	0.0819	0.0043
	60	0.1518	0.0118	0.1278	0.0071

(a) Precision denoted by ϵ corresponds to the half-width of a 95% confidence interval. Here, S denotes the smolt survival rate from the release site through Lower Granite Dam and S,, the survival rate from the release site to Lower Granite Dam.

(b) R = 5000, with $R_1 = 2500$, $R_C = R_T = 1250$.

(c) R = 3000, with $R_1 = 1500$, $R_C = R_T = 750$.

$$\mathrm{Var}(\overline{\hat{S}_i}) = \frac{\sigma_s^2 + \overline{\mathrm{Var}(\hat{S}_i|S_i)}}{k} \qquad (2.2)$$

where $\overline{\mathrm{Var}(\hat{S}_i|S_i)}$ is the average sampling error of a reach survival estimate. Unfortunately, prior data on the anticipated variation in survival over time is unavailable. Thus, one of the objectives of a replicated pilot study is to estimate σ_s^2 for future design considerations.

An alternative objective in designing the pilot study is that an estimate of average reach survival $(\bar{S})$ across replicate trials would have a specified precision. Defining precision for an estimate of average reach

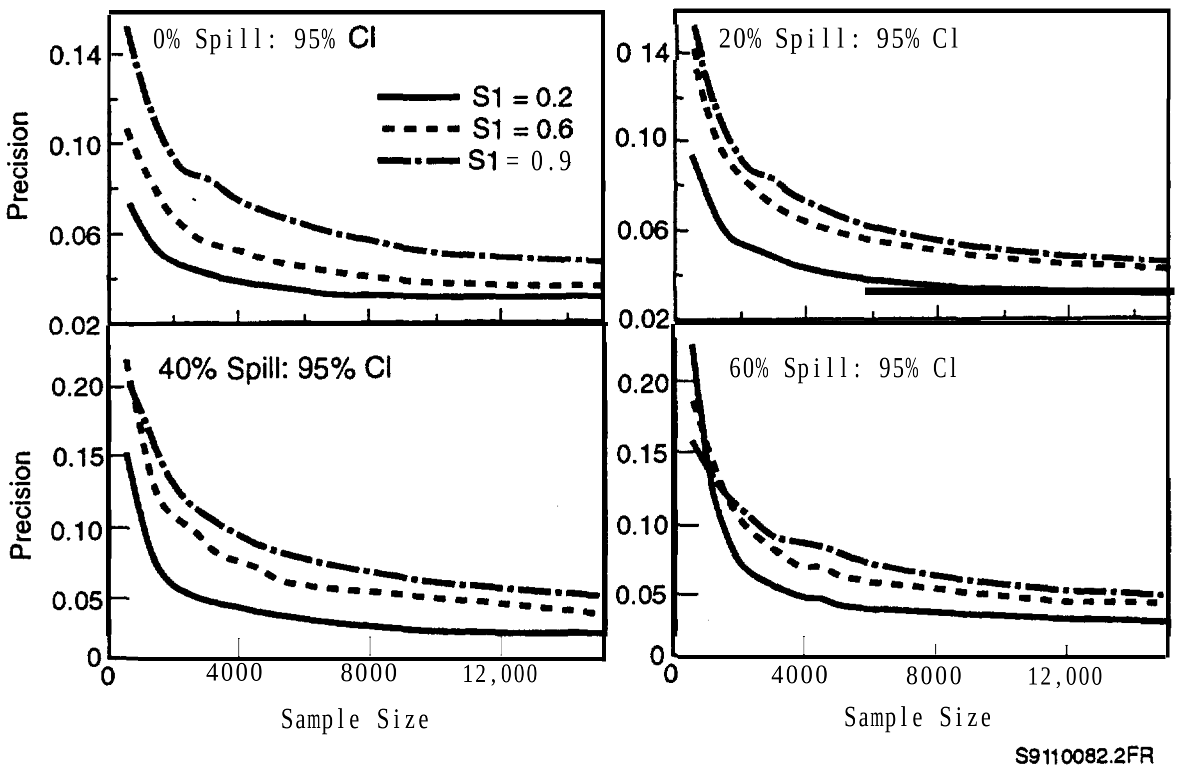

FIGURE 2.3. Relationship Between Sample Size and Anticipated Precision (ϵ) at (1 - a) = 0.90 for Estimating Survival (S) at Four Spill Levels (see also Appendix A). Sample size is for a single trial of the modified single release-recapture method upstream from Lower Granite Dam.

survival ($\bar{S}$) as a function of the absolute deviance from the true value (i.e., $|\bar{\hat{S}} - \bar{S}|$), sampling precision can be expressed as

$$P\left(\left|\bar{\hat{S}} - \bar{S}\right| < \epsilon\right) = 1 - \alpha \tag{2.3}$$

Assuming $\sigma_s = 0.05$ (i.e., $\sigma_s^2 = 0.0025$), Table 2.2 gives various combinations of total release size (R) and number of replicate trials (k) to have a precision of $\epsilon = 0.05$, $1 - \alpha = 0.95$, under various spill and reach survival conditions. The sample size calculations indicate that increasing the total release size (R) from 3000 to 5000 fish has little or no effect on the number of replicate trials required. Furthermore, the number of replicate trials

TABLE 2.2. Predicted Number of Replicate Survival Studies to Have a Precision of + 0.05 About the Mean Survival $(\bar{S})$, 95% of the Time Under Various Spill and Reach Survival Conditions. Here, S denotes the survival rate from the release site to Lower Granite barn, R is the number of fish per trial, and k is the number of replicate trials.

S_1	Percent Spill	R	k
0.20	0	3,000	5
	20	3,000	5
	40	3,000	5
	60	3,000	7
0.20	0	5,000	5
	20	5,000	5
	40	5,000	5
	60	5,000	6
0.60	0	3,000	5
	20	3,000	6
	40	3,000	7
	60	3,000	10
0.90	0	3,000	6
	20	3,000	7
	40	3,000	8
	60	3,000	11

required (ϵ = 0.05, 1 - α = 0.95) varies between k = 5 and k = 11 as spill ranges from 0 to 100%, and survival through the reach (S_1) ranges from 0.20 to 0.90. Inspection of Table 2.2 suggests five to seven replicate trials will be adequate for the pilot study under a wide range of conditions during spring outmigration.

3.0 PROPOSED ACTIVITIES UNDER THE PILOT STUDY

The following paragraphs describe the activities that would be undertaken during this study.

3.1 PIT-TAG EXPERIMENTAL GROUPS AT REPRESENTATIVE HATCHERIES

Initial test species should include spring and summer chinook salmon and steelhead. Candidate hatcheries for these species include Dworshak, McCall, Lookingglass, Kooskia, and Rapid River. The ability of specific hatcheries to maintain replicate groups or serially release segments from a pooled population (pseudoreplication) will in large part influence site selection.

For each species at each hatchery, a total of approximately 9000 experimental fish is required. PIT-tagging would occur during the fall/winter preceding release. The total number should be blocked into six lots (replicates or pseudoreplicates) of 1500 fish, employing one of two available strategies. Either each lot will be held in separate rearing raceways/containers/ponds until release, or all 9000 can be reared in a common environment and blocked into 1500-fish groups at release. One strategy would include variability associated with different rearing containers, while the alternative would not. Facility limitations would be a central consideration in this regard.

Experimental lots could be released in one of two ways, either all in one day or serially over the course of 6 days (pseudoreplication). The second procedure would lend itself to the situation where all fish were held in a common vessel until release. To facilitate comparison across hatcheries, it would be preferable that a common rearing and release strategy be employed at all sites. We recommend that separate raceways and separate releases be performed to obtain an empirical estimate of experimental error.

3.2 MEASURE EFFECTS ASSOCIATED WITH DETECTION AND RERELEASE AT HYDROELECTRIC FACILITIES EOUIPPED WITH SLIDE-GATES

A key assumption in the reach survival models is that the detection and rerelease process have no effect on the subsequent survival of these fish. However, fish passing through the detector and slide-gate that are housed in the bypass system may experience a source of mortality associated with this route that is exhibited after the fish are detected and recorded as live. This mortality could be acute or delayed mortality associated with the diversion process or facility-related stress, or it could be caused by predation that may target the outfall discharge and occur prior to mixing with tagged fish from other passage routes.

Migrant yearling chinook and steelhead collected at dam sites would be used as experimental animals. Fish would be PIT-tagged and split into two groups. One group would be released just upstream from the slide-gate, perhaps near the separator. The second group would be released in the tailrace and dispersed across the front of the dam in line with the outfall port.

Each release group would consist of 750 PIT-tagged migrants, all of the same species. For each species (yearling chinook and steelhead), a set of paired releases ($R_C + R_T = 1500$ total) would occur repeatedly every 2 to 4 days, targeting the time frame that experimental groups from upriver release sites would arrive at the dam (rerelease site). This period could last approximately 2 to 4 weeks, and require a total of 6000 to 27,000 fish of each species, depending on the frequency of releases and duration of passage of upstream experimental groups.

3.3 EXECUTE DATA ANALYSIS

Estimates of survival and capture probabilities would be calculated along with associated variance estimates. These estimates would be compared to estimates reported by other investigators. Tests of post-detection bypass mortality and associated point estimates would be calculated. Findings would be reported, and recommendations regarding strengths and limitations of these procedures presented.

4.0 EVALUATION OF OTHER SMOLT SURVIVAL PROTOCOLS

In February 1989 a workshop was held in Friday Harbor, Washington to review methods for measuring salmon and steelhead smolt survival and to outline research needs for the Columbia and Snake River mainstream passage projects (Anderson et al. 1989). This workshop was the third in a series that addressed measurement of juvenile salmonid survival in the Columbia River system. A recommendation of workshop participants was that a survival study be designed based on the protocol of Burnham et al. (1987). They felt an evaluation of this protocol would help determine if survival studies based on the computer program RELEASE were practical, or if certain logistic considerations and/or critical assumptions would limit utility of the model. The evaluation was suggested to assist fisheries managers and water managers in their monitoring of smolt survival in the Columbia River system. This section of the report summarizes recent activities related to addressing that concern, including a summary of the limitations and potential application of various experimental protocols for estimating smolt survival.

Historically, two general approaches have been used to estimate reach or system survival of smolts in the Columbia and Snake rivers: 1) paired releases of treatment and control groups (i.e., first capture history protocol of Burnham et al. 1987; Ricker 1945, 1958 and 2) estimated numbers of smolt passing a dam based on sampling efficiency curves (Raymond et al. 1974, 1975; Raymond 1979; Sims and Ossiander 1981; Sims et al. 1983). A third approach is the single release-recapture model of Burnham et al. (1987). Experimental considerations for these estimation techniques, including the potential effect of assumption violations on results, are briefly summarized in the following section.

4.1 PAIRED RELEASE-RECAPTURE CONSIDERATIONS

A major limiting factor in using the paired treatment-control approach of Burnham et al. (1987) to estimate reach survival is that sampling rates at a dam usually are not constant over time because of variations in spill volumes, as well as differences in fish guidance efficiency (FGE). Thus, treatment and control groups must pass the mark-recovery facility at the same time to ensure they are subjected to the same sampling effort. This becomes increasingly difficult if treatment and control groups are released far apart in time and/or space. This condition could violate the assumption that the groups are sampled at the same rates at the downstream recovery sites.

The assumptions of the paired release-recapture methods, as found in Burnham et al. (1987, pp. 51-52), are as follows:

"1. The test fish used are representative of the population of fish about which one seeks mortality information.

2. Test conditions are representative of the conditions of interest.

3. Treatment and control fish are biologically identical prior to release at the first dam. A strict interpretation of Assumption 3 is that initial handling, marking, and holding do not affect survival rate.

4. The numbers of fish released are exactly known.

5. Marking (tagging) is accurate; there are no mark (tag) losses and no misread marks (tags).

6. All releases and recaptures occur in brief time intervals, and recaptured fish are released immediately.

7. The fate of each individual fish, after any known release, is independent of the fate of any other fish.

8. With multiple lots (or other replication), the data are statistically independent over lots.

9. Statistical analyses of the data are based on the correct model.

10. Treatment and control fish move downstream together.

11. Captured fish that are rereleased have the same subsequent survival and capture rates as fish alive at the site which were not caught, i.e., capture and rerelease do not affect their subsequent survival or recapture.

12. All fish (in the study) of an identifiable class (e.g., treatment or control, or size, or replicate) have the same survival 'and capture probabilities; this is an assumption of parameter homogeneity."

Principal concerns of workshop participants were the ability of any experimental design to fulfill Assumptions 10 to 12 of Burnham et al. (1987). However, it was Assumption 10, coincidental passage (mixing) at the recovery sites, that was of greatest concern. Past studies in the Columbia River have not always been successful in satisfying this assumption when treatment and control groups are released at distant locations. McKenzie et al. (1984) presented results that indicated mixing was accomplished through reaches of the Columbia River. However, in a previous effort, the same study protocol resulted in inadequate mixing at the recovery site (McKenzie et al. 1983). Similarly, Giorgi and Stuenhrenberg (1988) failed to achieve mixing while attempting to estimate survival through Lower Granite Pool. However, those authors suggested using river-run fish rather than fish released directly from the hatchery to improve the probability of mixing, and they described a procedure to accomplish it. Because of the mixed results reported when employing this paired-release protocol, we have little confidence in it as a reliable methodology for estimating survival over expansive distances. For this reason, paired release-recapture methods for reach survival are not recommended. Instead, the paired release-recapture method should be used at either a localized point (i.e., turbine or bypass evaluation) or perhaps over a very short reach of the river.

4.2 SINGLE RELEASE-RECAPTURE CONSIDERATIONS

The single-release model is a special case of the paired release-recapture methods described in Burnham et al. (1987), which involves treatment and control releases. If the control release is ignored, the model becomes the single-release model reproduced in Appendix B. The single-release model is the most likely of the three methods discussed in this section to give valid reach survival estimates. Therefore, we conducted an extensive simulation study of the approach (Appendix A) to determine the robustness of the method to assumption violations.

The assumptions of the single release-recapture model are:

1. The test fish are representative of the population about which one seeks mortality information.

2. Test conditions are representative of the conditions of interest.

3. The number of fish released is exactly known.

4. Marking is accurate, i.e., there is no post-release marking mortality and no misread marks.

5. Initial release is instantaneous, and all detected fish are rereleased immediately upon detection. These are requirements for making inferences to river conditions, not an estimation per se.

6. The fate of each individual fish is independent of the fates of all other fish.

7. If replicate studies are conducted, then the data from different releases are statistically independent.

8. Detected fish that are rereleased have the same subsequent survival and capture rates as fish alive at the site that are not detected.

9. All fish in the study have the same survival and capture probabilities.

This list of assumptions is a subset of that for the paired release-recapture method protocol (Burnham et al. 1987) because only a single release is required. Either the complete capture-history or partial capture-history protocols of Burnham et al. (1987) can be used to estimate reach survival. Because of the smolt transportation program at Little Goose Dam, fish going through the collection facility are not returned to the river. The removal of the tagged fish before the last detector at McNary Dam results in some fish having only a partial recapture history. Thus, estimates of reach survival are limited to areas between the release site and Lower Granite Dam, where it is assumed the slide gate will be operating to rerelease tagged fish.

4.2.1 Robustness to Heterogeneous Survival Probabilities

Reliable use of the tag analysis to estimate reach survival is predicated upon fulfilling the model assumptions or upon the methods being robust to violations of those assumptions. In the formulation of the release-recapture model for the Snake River studies, a key assumption in survival estimation is that all PIT-tagged fish have equal survival probabilities in a river reach. This assumption of homogeneous survival probabilities can be violated in three general ways:

1. Survival probabilities for individual smolt may be heterogeneous because of inherent differences in the viability of PIT-tagged fish.

2. Survival probabilities for individual smolt may be heterogeneous because routes taken through the hydroelectric project (i.e., spill, bypass, turbines) subject fish to different risks.

3. Detected and undetected fish have differential rates of survival (i.e., fish going through the PIT-tag detector and slide-gate are exposed to additional sources of detector/rerelease mortality that may be expressed post-detection).

Valid use of the single release-recapture model for survival estimation depends on the robustness of the estimation procedures to the above forms of heterogeneity in smolt survival.

Monte Carlo simulation studies were used to evaluate the robustness of the single release-recapture model to the three forms of heterogeneous survival considered possible on the Snake River. The survival terms and values used in the simulations are described in Table 4.1. In all scenarios, hypothetical fish were allowed to experience differential mortality rates in the three passage routes (Figure 4.1) through the dam (i.e., turbine, spill, bypass) and in the heterogeneity of reach survival rates among tagged fish. In Scenario 1, each fish has a unique reach survival probability (Figure 4.2). For example, the survival probability from the point of release to Lower Granite Dam forebay, S_{j1}, varied from 0.83 to 0.95 among individual smolt. This scenario represents the first type of heterogeneity investigated by our simulations where unique survival rates depended upon a hypothetical condition of index of the fish prior to release. Fish with higher condition indices were assumed to have higher survival probabilities. In Scenario 2, fish going through the bypass system experienced a bypass mortality prior to detection. In Scenario 3, the bypassed fish experienced additional mortality after they were detected (and recorded as alive) and before remixing with fish that took different routes through the dam. In both Scenarios 2 and 3, all fish experience the same probability of surviving the downstream reaches. For Scenario 1 the estimator for survival from the point of release to the Lower Granite Dam is shown in Figure 4.2. This schematic of the survival process

TABLE 4.1. Survival Terms Used in the Simulations and Conditions for Which Sample Size Calculations Were Performed

Term	Value Used in Sample Description	Size Calculations
S_1	Survival from release to Lower Granite forebay	0.2, 0.6, 0.9
S_2	Survival from Lower Granite tailrace to Little Goose forebay	0.8
S_3	Survival from Little Goose forebay tailrace to McNary forebay	0.8
S_{spill}	Survival over a spillway	0.98
$S_{turbine}$	Survival through a turbine	0.85
S_{bypass}	Pre-detection survival in the bypass	0.98
7	Post-detection survival in the bypass	0.90
Spill passage rate	Probability a fish will go over the spillway	0, 0.2, 0.4, 0.6
FGE[a]	Fish guidance efficiency	0.5 (Lower Granite) 0.7 (Little Goose) 0.7 (McNary)

(a) FGE values were taken from Swan et al. (1990) and Gessel et al. (1989).

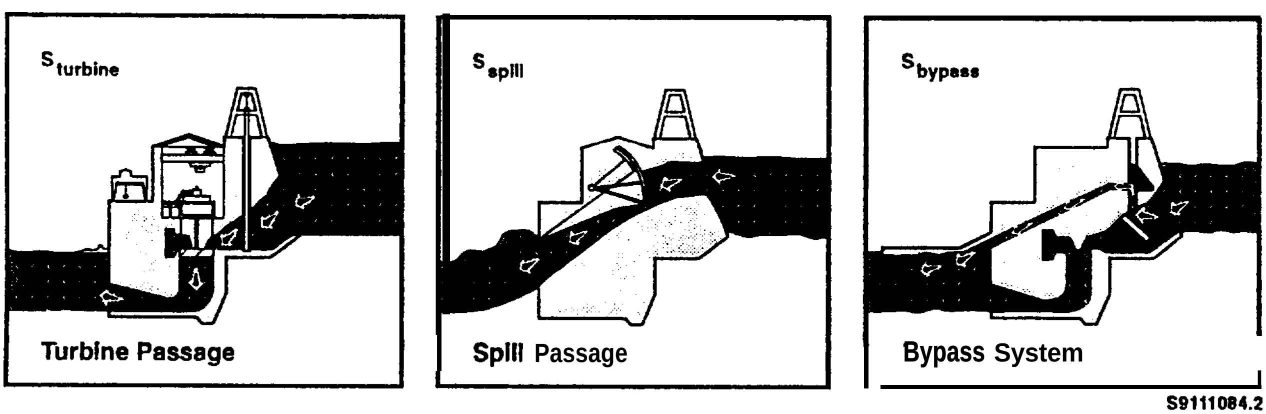

FIGURE 4.1. Potential Passage Routes for Juvenile Salmonids Through the Hydroelectric Dam Complex

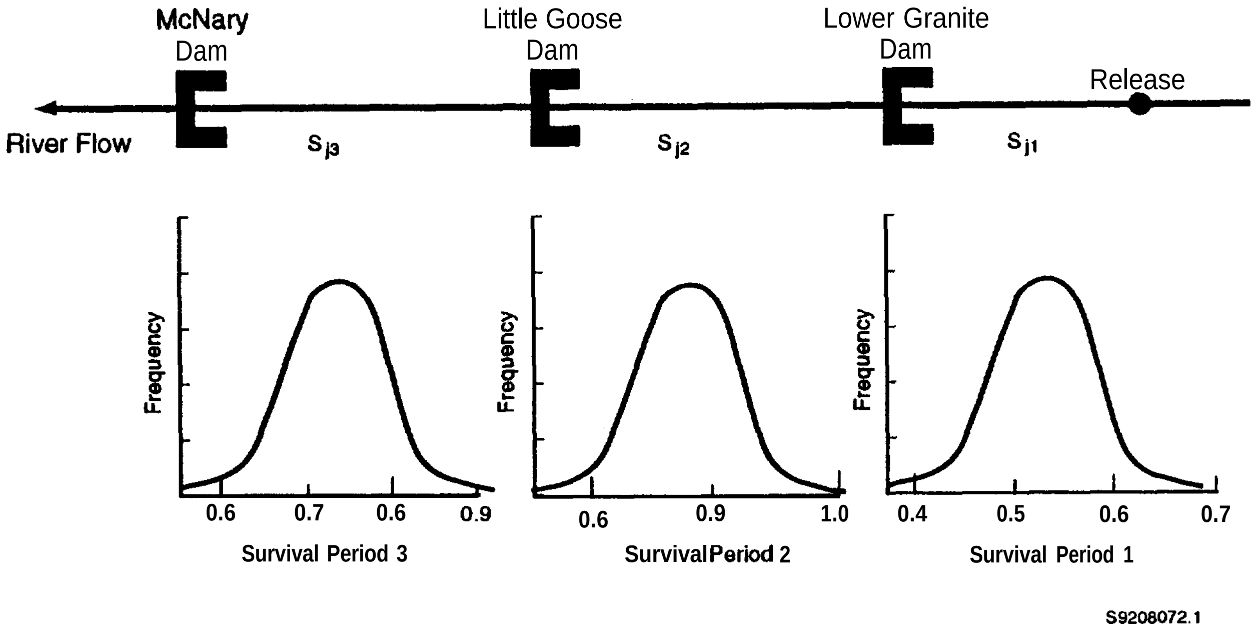

FIGURE 4.2. Schematic of the Survival Process for Smolt in the Snake/Columbia River System

for smolt in the Snake/Columbia River system indicates the release location above Lower Granite Dam and the distribution of survival probabilities for each reach. Passage through the dam is as described for Scenario 2 (see also Appendix A). The estimator for survival from the point of release to the Lower Granite Dam tailrace was evaluated under these three scenarios using the following criteria: 1) determine if the survival estimator was unbiased, 2) determine if the variance was unbiased, and 3) determine if the 95% confidence interval included the true parameter value 95% of the time. More detailed information on the rates of mortality used in the various passage scenarios is present in Appendix A.

Scenario 1. Heterogeneity in survival probabilities of the tagged fish, resulting from differential fitness, violates the assumption of equal survival of all fish. Results of simulation under Scenario 1, where survival probabilities are heterogeneous, indicated survival estimators had the following properties:

- Point estimates of survival were unbiased.

- Variance estimates are robust, being generally biased upward when affected.
- 95% confidence intervals included the true parameter value 95% of the time.

Thus, heterogeneity in the inherent survival potentials of fish had no effect on estimators of reach survival as long as the inter-fish variability is independent of dam passage routes.

Scenario 2. When the route that a fish travels through the dam is random, all tagged fish have the same expectation of survival at the beginning of each river reach (i.e., just below the dam beyond the tailrace), despite the differential mortality in the different routes past the dam. Hence, all fish under Scenario 2 conform to the assumption of equal and independent survival rates in each reach. Simulation results for Scenario 2 show the survival estimators for release-recapture models are robust to heterogeneous survival probabilities among passage rates in the dam as long as this mortality occurs prior to the detection. For Scenario 2, the estimators of reach survival had the following properties:

- Point estimators were unaffected by heterogeneous survival rates among the various passage rates at the decoder dam.
- Variance estimates were unbiased.
- 95% confidence intervals included the true parameter value 95% of the time.

The differential mortality rates induced through various routes at a dam in these simulations do not introduce bias because once a smolt survives passage at a dam, it has the same expectation of being seen at the next dam as any other smolt who survived the dam.

Scenario 3. Decoded fish going through the detector/bypass experience a source of mortality associated with this route that is exhibited at the time of decoding or shortly after detection. This effect can be acute or delayed mortality associated with handling or facility stress, or can be caused by predation that occurs at the bypass outfall and prior to mixing with tagged fish from other passage routes. Under these conditions, the model provided

- Point estimators of reach and dam survival that were positively biased
- Variance estimates that were only slightly biased
- 95% confidence interval that included the true parameter value much less than 95% of the'time.

A smolt that is detected at Lower Granite Dam does not have the same expectation of being seen at the next dam as every other smolt that survived passage under this scenario. This is because a detected smolt experiences an extra mortality source after its detection. Consequently, the release-recapture estimators of reach survival are not robust to differential bypass mortality that occurs during or after detection. This discrepancy in expectations causes the model to fail in accurately estimating the true survival.

Each of the three passage scenarios presents an underlying survival process influencing smolt survival. The simulation studies showed that differential mortality among the various passage routes does not affect the validity of the survival estimates unless there is appreciable mortality in the bypass route after the decoders and before the fish mix again in the downstream reach. Because of the failure of the single-release model in the presence of post-detection mortality sources, we propose that a modified single-release model be used unless it can be shown that post-detection mortality is insignificant (Appendix A).

4.2.2 Robustness to Heterogeneous Capture Probabilities

The likelihood model used in estimating reach survival makes no assumptions regarding changes in the rates of PIT-tag detection over time. The values of FGE can vary between dams and for a single dam over time. Having the FGE vary daily or even hourly does not affect estimation of reach survival (S). The only effect that time-varying FGE has on the estimation process is on the interpretation of the estimated capture probabilities. The capture probabilities estimated at PIT-tag detectors are a weighted average of the varying FGE values over the outmigration timing of a release. The FGE values are weighted by the rate of fish passage during the respective period of fluctuating FGE.

Pollock (1982) found survival models to be very robust to unequal capture probabilities among individuals. The only situation where heterogeneous capture rates can have a profound effect on survival estimates is when survival and recapture probabilities are correlated between individuals over reaches. Currently, no empirical data demonstrate a correlation between detection and smolt survival potential.

4.3 SMOLT ABUNDANCE ESTIMATOR

Another method of estimating reach survival is possible, if the number of smolt arriving at a collector site could be estimated ($\hat{N}_1$) from among a known release of $N_\emptyset$ fish upstream. In the case where smolt abundance at a downriver site can be estimated, reach survival would then be estimated as

$$\hat{S} = \frac{\hat{N}_1}{N_\emptyset} \tag{4.1}$$

The estimation of $\hat{N}_1$ is the central feature of the technique, and several options are available.

One approach for the estimates of downstream abundance is based on knowing the collection efficiency (CE) at a hydroelectric facility and sampling the outmigration to recapture some of the $N_\emptyset$ fish marked and released (n). In this case, reach survival is estimated as

$$\hat{S} = \frac{n/CE}{N_\emptyset} \tag{4.2}$$

Collection efficiency is determined by two independent factors: FGE and spill efficiency (SE) at a hydroelectric facility. Thus, it would be useful to have independent estimates of both FGE and SE. However, species-specific functional estimates of these two parameters do not exist. FGE estimates generated under standardized test conditions are available at some sites but may not reflect the overall functional FGE at the dam. For example, variations in turbine load will change intake velocities and can affect FGE. Thus, the level of precision for survival estimates determined from expansion of available FGE values may be inadequate. Although passage estimates can be derived for either specific (i.e., marked) stocks or the population at large, species-specific differences in migration behavior influence fish response to collection devices and must be accounted for in test design. The only species-specific estimate of SE known involved fitting yearling chinook salmon with radiotags at Lower Granite Dam (Giorgi et al. 1988; Wilson et al. 1991).

Giorgi and Sims (1988) developed calibration curves for passage of yearling chinook salmon and steelhead at McNary Dam, based on recoveries of freeze-branded fish. However, McCutcheon and Giorgi (1989) determined that more accurate and precise mark-recovery data could be obtained at McNary Dam if PIT-tags were used rather than freeze-brands. Stuehrenberg and Johnson (1990) reexamined CE at McNary Dam using PIT-tags rather than freeze-brands. A primary objective of their investigation was to determine if factors other than discharge volumes at the powerhouse affected the recovery proportions of PIT-tagged fish at McNary Dam. They noted that regular changes occurred in spill volume over many 24-h periods. This suggests that time of arrival and distribution of tagged fish at the dam would result in different recovery proportions. For example, the migration rate of smolts originating from the Snake River was slower than Columbia River smolts, even though both groups were released simultaneously at the same location. Also, when spill levels were high, fish released on the north shore were recovered in lower proportions than those released on the south shore (Stuenhrenberg and Johnson 1990). Thus, several operational features of hydropower facilities can contribute to variability in collection efficiency/flow calibration curves. Hydroacoustic assessment does not seem to offer additional hope for generating species-specific estimates of either FGE or SE.

The release and subsequent recapture of marked groups released upstream from a dam (m_j) appears to be the only available method for calibration of the sampling device (i.e., dam). Daily releases of tagged fish (M_j) in the forebay of the dam could be used to estimate the CE during the outmigration of the primary release (N,). These daily estimates of CE (m_j/M_j) could then be used to calibrate the daily recoveries of the ($N_,$) tagged fish and provide a survival estimate. However, this technique has some problems apart from limitations already discussed. Assumptions associated with this tagging method to estimate CE and subsequent reach survival include the following (see also Appendix C):

1. The M_j fish used in estimating daily recovery efficiency (CE) have the same capture probability as the initial release of $N_\emptyset$ PIT-tagged fish on day j.

2. The daily releases of M_j fish to estimate recovery efficiency and subsequent recoveries of m_j fish are independent.

3. The M_j fish have independent and equal probabilities of recovery.

4. There is no natural mortality or handling mortality among the M_j fish used to estimate recovery efficiency (CE) between the release site and point of recovery.

One shortcoming in the estimation of CE is the need to assume no mortality among the M_j fish released in the forebay (Assumption 4). Alternatively, an independent estimate of post-release survival for each of the marked groups could be estimated. However, if we could estimate the post-release mortality among the M_j, the same methodology could also be applied to estimate reach mortality among the $N_\emptyset$ fish. But such a methodology does not exist. Giorgi and Sims (1988) assumed a 10% mortality to each release group used in their analysis. However, other investigators (e.g., Raymond 1979) did not account for post-release survival when generating CE curves at other mainstream Columbia and Snake River dams.

Another shortcoming of the CE method is the selection of a release site for the M_j fish such that the fish will be detected at the same rate (Assumption 1) as the $N_\emptyset$ fish released upstream. For this to occur, the M_j and $N_\emptyset$ fish must be equally dispersed in the river and have the same migratory characteristics. Releasing the M_j fish far enough upstream to have the same dispersion as the $N_\emptyset$ fish, however, increases their travel time and risk of mortality, which again would violate Assumption 4.

A final consideration is the assumption of independence (Assumption 2) between the daily releases and subsequent estimates of CE. This assumption implies the M_j fish released daily are all recaptured within, for example, a 24-h period in order to properly calibrate that day's recovery of fish from the $N_Ø$ release. In order for all the M_j fish to have a potential of being recaptured within 24 h, daily releases must be extremely close to the dam and intake facilities. However, this requirement contradicts the computing requirement of equal mixing with the other $N_Ø$ fish (Assumption 1).

The apparent inability to satisfy the competing logistical demands of Assumptions 1-4 strongly suggests the CE method of reach survival estimation will be biased. For this reason, we do not recommend this approach as the primary method of estimating reach survival.

4.4 APPLICATION OF THE VARIOUS SURVIVAL ESTIMATES

We completed a list of research needs and suggestions for future survival studies from participants in the first scoping meeting. Together with this list of applications, we have identified the suitable survival estimation techniques to apply for each objective (Table 4.2). For example, participants in the scoping meeting felt that the Burnham et al. (1987) approach (treatment/control) could be used to accurately estimate turbine, spill, and bypass survival. It is possible to obtain these site-specific survival estimates now. In conjunction with system survival estimates, these estimates may provide meaningful insight on the dynamics of smolt survival through the system. However, the paired release-recapture model was felt not to be accurate or precise for estimating reach survival because of the failure to meet the assumptions of equal mixing.

The application of existing protocols to the various objectives of estimating smolt survival is summarized in Table 4.2. This comparison suggests that no single protocol will answer all of the questions being considered in the Columbia River system. However, there appears to be at least one suitable methodology that will provide an estimate of smolt survival for anticipated release and/or passage scenarios. In some situations, multiple methods may be used together as a check on relative precision and accuracy. This is important because each method has its own limitations and logistical constraints.

TABLE 4.2. Applicability of Different Survival Estimate Methods Under Different Scenarios in the Columbia River System

Protocol	Multi-Reach System	Reach/Pool	Dam Passage Route
Burnham paired release-recapture	no	maybe[a]	yes
Abundance estimate (CE)	unlikely[b]	unlikely[b]	no
Single-release recapture (Burnham et al. 1987)	maybe	maybe	no
Modified single release-recapture	yes	yes	no

(a) Until mixing is violated.
(b) Empirical assessments of bias and precision are required with this method.

The following examples are of situations in which survival studies could provide useful information:

- Evaluate the effects of different passage routes (i.e., turbines versus bypass versus spill) on juvenile fish survival.
- Determine effects of operational conditions, including turbine loading, on fish survival.
- Evaluate the design and operation of fish bypass systems.
- Evaluate the success of predator control measures.
- Examine differences between survival rates of guided versus naive fish.
- Compare survival rates between hatchery and wild fish populations.
- Compare survival of fish released under a range of physiological conditions (e.g., disease incidence, descaling, condition factor).
- Evaluate the effects of a range of ambient conditions (i.e., flow, temperature, water quality) on fish survival.
- Determine the survival of fish through key reaches of the Snake and Columbia rivers.

The modified single release-recapture technique presented in this document permits accurate and precise estimates of reach survival, but this is currently possible within only a limited number of reaches in the Columbia River Basin. The protocol requires the rerelease of tagged fish. Lower Granite Dam (Snake River) and the Prosser smolt trap (Yakima River) are the only two sites that currently have detection and rerelease capabilities in the Columbia River system. Consequently, use of this technique is restricted geographically to the reaches above these sites until new tag diverters are installed, and it is agreed that experimental tagged fish will be rereleased.

The Burnham et al. (1987) paired-release approach is limited to dam passage scenarios. Model assumptions will likely be violated during a reach or systems study because spatial and/or temporal separation of treatment and control groups preclude synchronous passage at recovery sites. Survival estimates that are derived from the abundance estimation technique estimates will necessarily be general in nature because the abundance estimates are a function of two poorly defined and ever-changing probabilities, i.e., FGE and SE. Further, precision of estimates may be unacceptable and sources of bias are a concern. The single release-recapture method of Burnham et al. (1987) or the modified version described in this report appear the most likely candidates for estimating reach survival.

5.0 CONCLUSIONS AND RECOMMENDATIONS

We reached several conclusions as the result of our analysis of techniques used to estimate smolt survival:

1. It is possible to obtain precise and reliable estimates of smolt survival using PIT-tags and release-recapture protocols.

2. Reach survival studies using PIT-tags in the Snake River are presently limited to the reach above Lower Granite Dam (currently the most downstream rerelease site). The length of the reach study could be extended downstream if fish are rereleased at Little Goose Dam and subsequent downstream locations. Reach survival studies could also be conducted in the reach above the Prosser smolt trap in the Yakima River.

3. Smolt survival using the single release-recapture methods can be estimated from release sites upstream from Lower Granite Dam to the tailrace of the dam.

4. Active migrants of both yearling chinook salmon and steelhead, including hatchery stocks, can be considered for use in the initial pilot studies.

5. Samples sizes needed for the pilot studies on reach survival appear manageable.

6. In general, reach survival estimates can be made in areas upstream from a PIT-tag rerelease site.

7. Installation of PIT-tag detection and diversion systems at downstream sites in the Snake and Columbia rivers will permit extending reach survival estimates.

8. Point impact effects, such as mortality from turbine passage, can be assessed using the appropriate paired release-recapture protocols presented by Burnham et al. (1987).

Specific recommendations for future studies include:

- A smolt survival study should be conducted in spring 1993. A basic design for this pilot study would involve: 1) using any PIT-tagged groups already scheduled for tagging at hatcheries, 2) rereleasing all PIT-tagged fish during the time upstream groups pass Lower Granite Dam, and 3) making additional PIT-tag releases above the tag detector and well below the bypass outfall at Lower Granite Dam. The pilot study should be replicated five to seven times within a several-week block of time.

- Additional PIT-tag facilities should be installed in the mainstream Columbia River to allow estimates of survival to be made outside of the current restricted area (i.e., above Lower Granite Dam). For example, installation of a PIT-tag diverter at McNary Dam and a detector at a lower dam (e.g., Bonneville) would allow for measures of system survival in the Snake and mid-Columbia Rivers to be conducted. A diverter located at McNary Dam is strategic in that it would increase opportunities for measuring survival of smolts migrating in the mid-Columbia River system.

- Studies should be initiated to determine the feasibility of manipulative and/or correlative investigations to identify causal factors influencing survival.

- New technology (e.g., electronic tags) should be investigated for the purpose of estimating smolt survival through the hydroelectric complex. Radiotags offer only limited capabilities for these situations, because tags can be implanted only in smolts >155 mm, and the tags have a relatively short life (e.g., 7 days; Stuehrenberg et al. 1990). Survival estimates could be improved if a smaller, longer-active tag was developed to allow detection of smolts at various serial locations along a reach of river. The feasibility of developing such a tool warrants consideration if we are to improve our capabilities to estimate smolt survival.

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APPENDIX A

EVALUATION OF RELEASE-RECAPTURE MODELS FOR SNAKE RIVER SURVIVAL STUDIES

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1.0 Introduction

Release-recapture methods are proposed as a possible means of estimating reach survival of outmigrating salmon and steelhead smolt during future Snake River survival studies. Reliable use of the tag analysis to estimate reach survival is predicated upon fulfilling model assumptions or the methods being robust to violations of those assumptions. The purpose of this report is to evaluate the utility of using release-recapture methods during Snake River survival studies. Specific objectives of this evaluation are the following:

1. Determine the robustness of release-recapture models to heterogeneous survival probabilities.
2. Recommend modifications of release-recapture methods that may enhance their utility in reach survival studies.
3. Determine necessary sample sizes for conducting release-recapture studies on the Snake River.

It is anticipated that results of this investigation will provide the guidance needed to design and implement reach survival studies using release-recapture methods.

In the formulation of the release-recapture model for the Snake River studies, a key assumption was identified that may influence the successful implementation of the tagging studies. It is:

All fish in the study have the same survival probabilities.

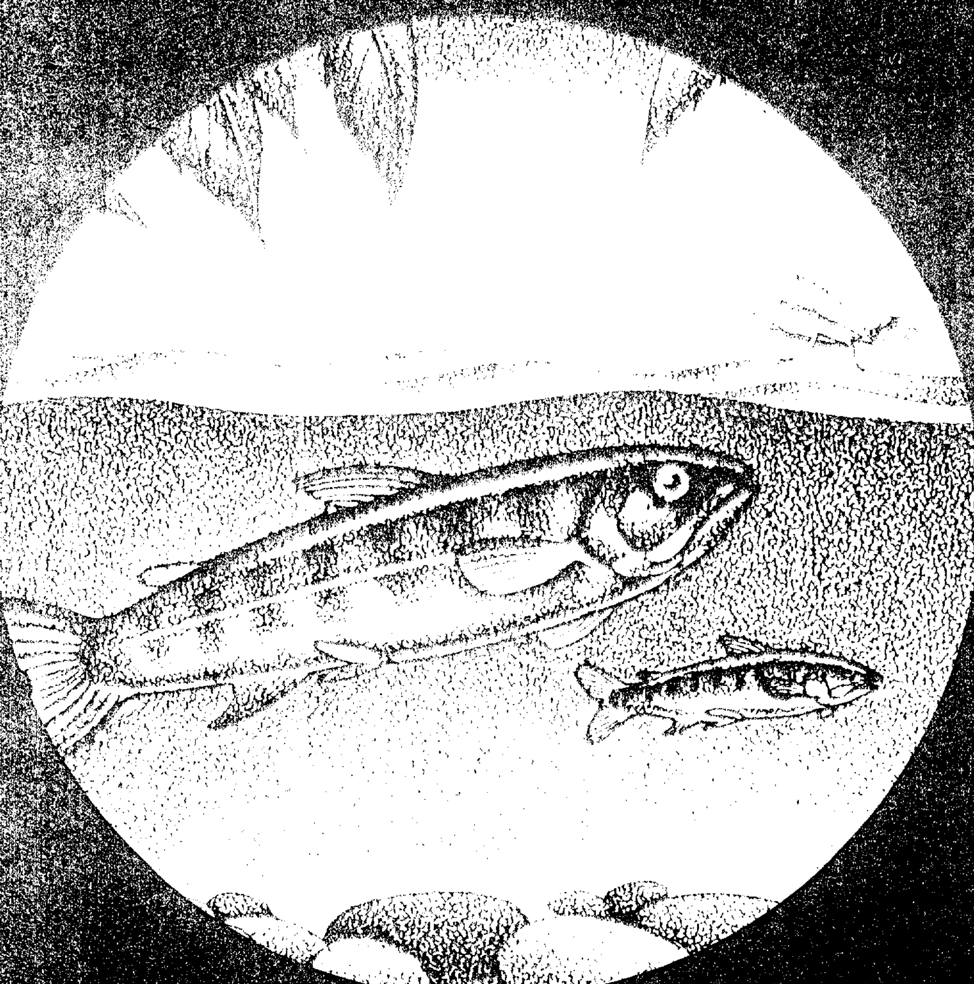
Violation of this assumption may be caused by three potential sources of heterogeneous survival probabilities; these are:

a. Survival probability for individual smolt may be heterogeneous because of inherent differences in viability of fish.

b. Survival probabilities for individual smolt may be heterogeneous because of the route taken through hydroelectric projects (i.e., spill, bypass, turbines).

Evaluation & Application of Statistical Methods for Estimating Smolt Survival

BPA 9



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condition index had a proportional hazards effect on survival. That is, if a smolt's condition index was X , then its survival probability in the reaches was calculated according to the following table:

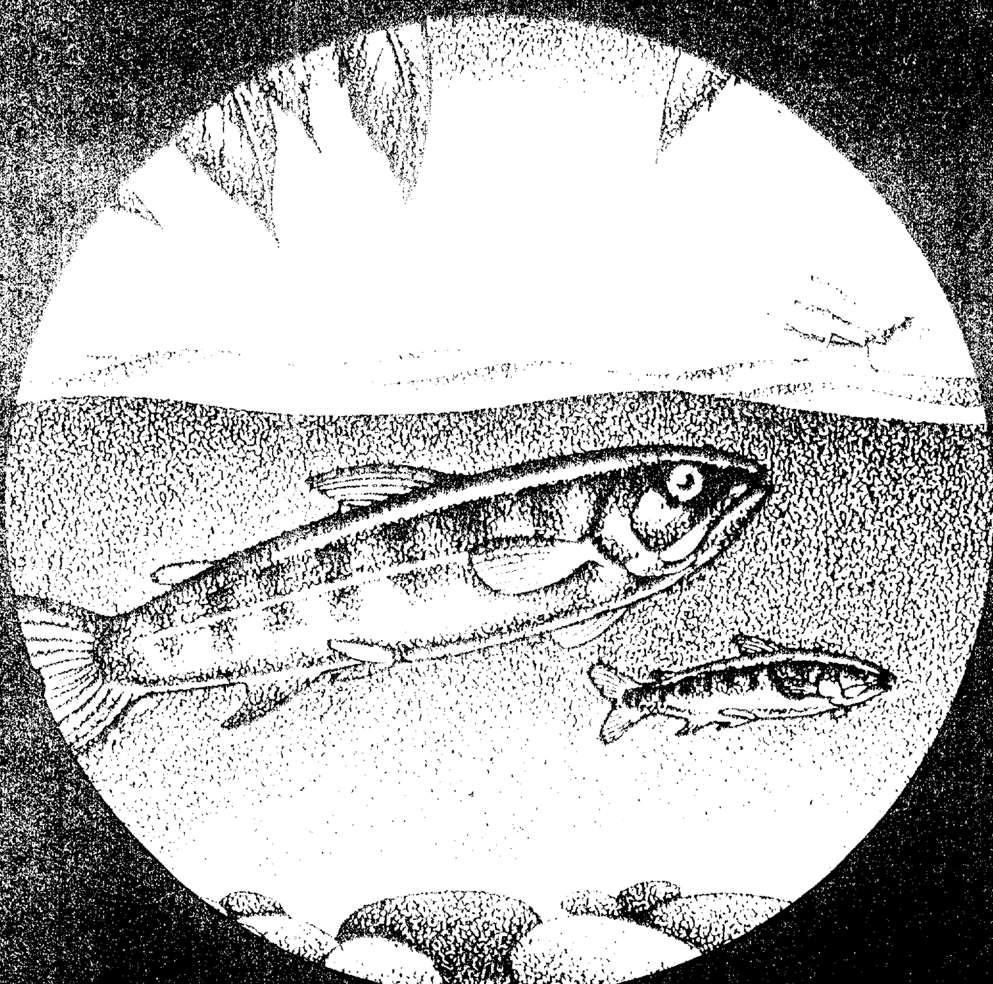
Reach	Hypothetical Reach Survival Probability
Lower Granite	$0.9^{e^{2X}}$
Little Goose	$0.8^{e^{2X}}$
McNary	$0.6^{e^{2X}}$

The distributions in reach survival of the PIT-tagged smolt (Figure Al) depended on the distributions of condition indices. This heterogeneity is a violation of assumption 12 (Burnham et al. 1987, p. 52) and is more extensive than the heterogeneity discussed in Burnham et al. (1987). In our simulations, these unique survival rates depend on the condition index of the fish before release. Fish with high condition indices are more likely to survive each reach than fish with low condition indices, so that survival rates are heterogeneous within a release.

In Scenario 2, the fish going through the bypass system experience a bypass mortality **before** they are detected (Figure A2). In Scenario 3, the bypassed fish experience a bypass mortality **after** they are detected (Figure A3). In both Scenarios 2 and 3, all fish have homogeneous survival probabilities in downstream reaches.

Evaluation & Application of Statistical Methods for Estimating Smolt Survival

BPA 9



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Scenario 2.

Underlying Survival Process Where Bypass Mortality is Pre-Detection

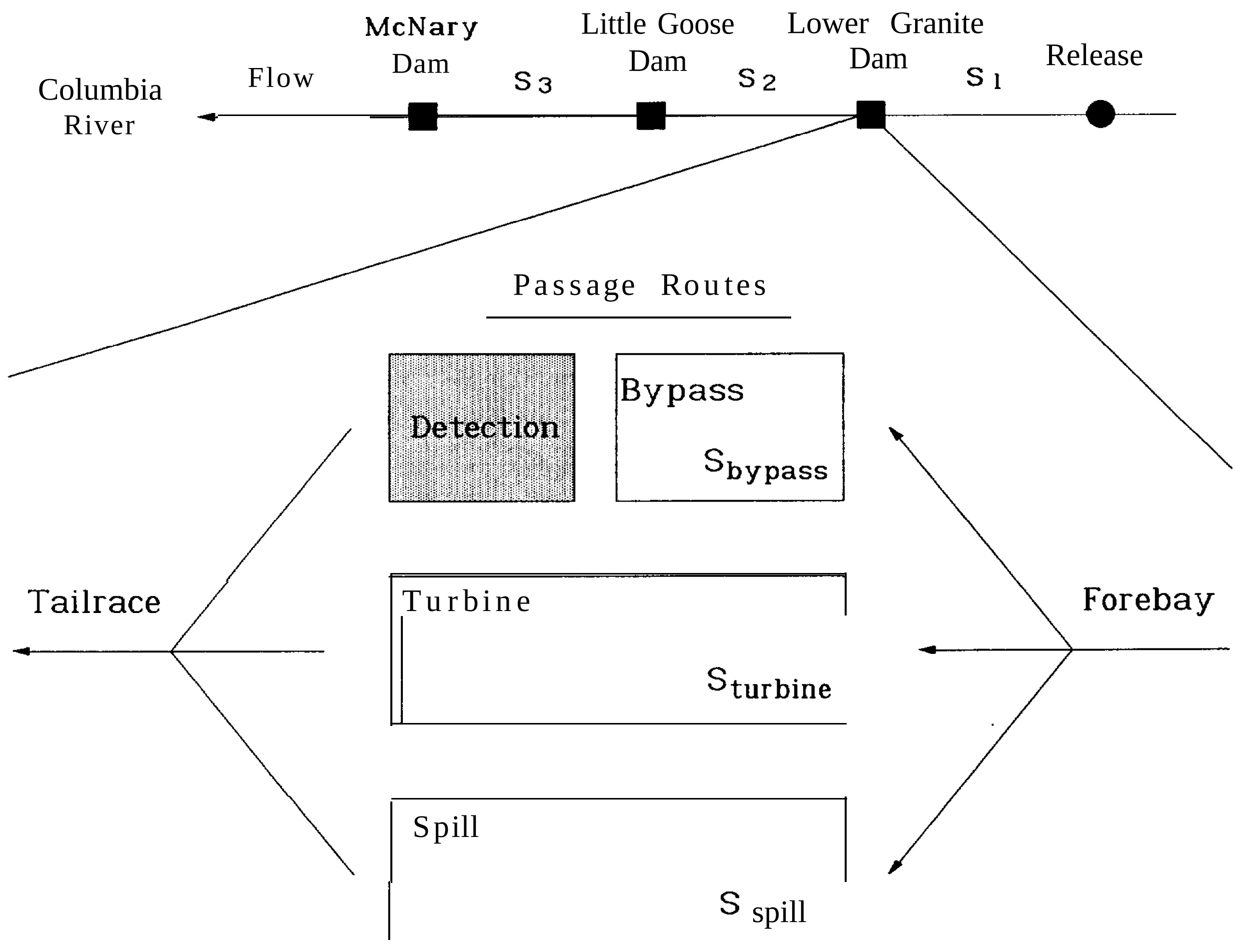


Figure A2. Schematic of the survival process for smolt in the Snake-Columbia river system. Fish are released above Lower Granite Dam. In each reach, a fish experiences a survival probability which reflects its condition: S_{j1}, S_{j2}, S_{j3} are the probabilities of surviving the reaches indicated for the jth fish $j = 1, \ldots, 1000$. At each dam, the fish take one of the three passage routes shown. A fish going through a particular route experiences a survival probability associated with the route as shown: S_{Spill}, S_{Turbin}, or S_{Bypass}. Detection in the bypass occurs only if the fish survived the bypass (i.e., mortality occurs before detection in the bypass route). Thus, not all fish that entered the bypass are detected.

Scenario 3.

Underlying Survival Process Where Bypass Mortality is Post-Detection

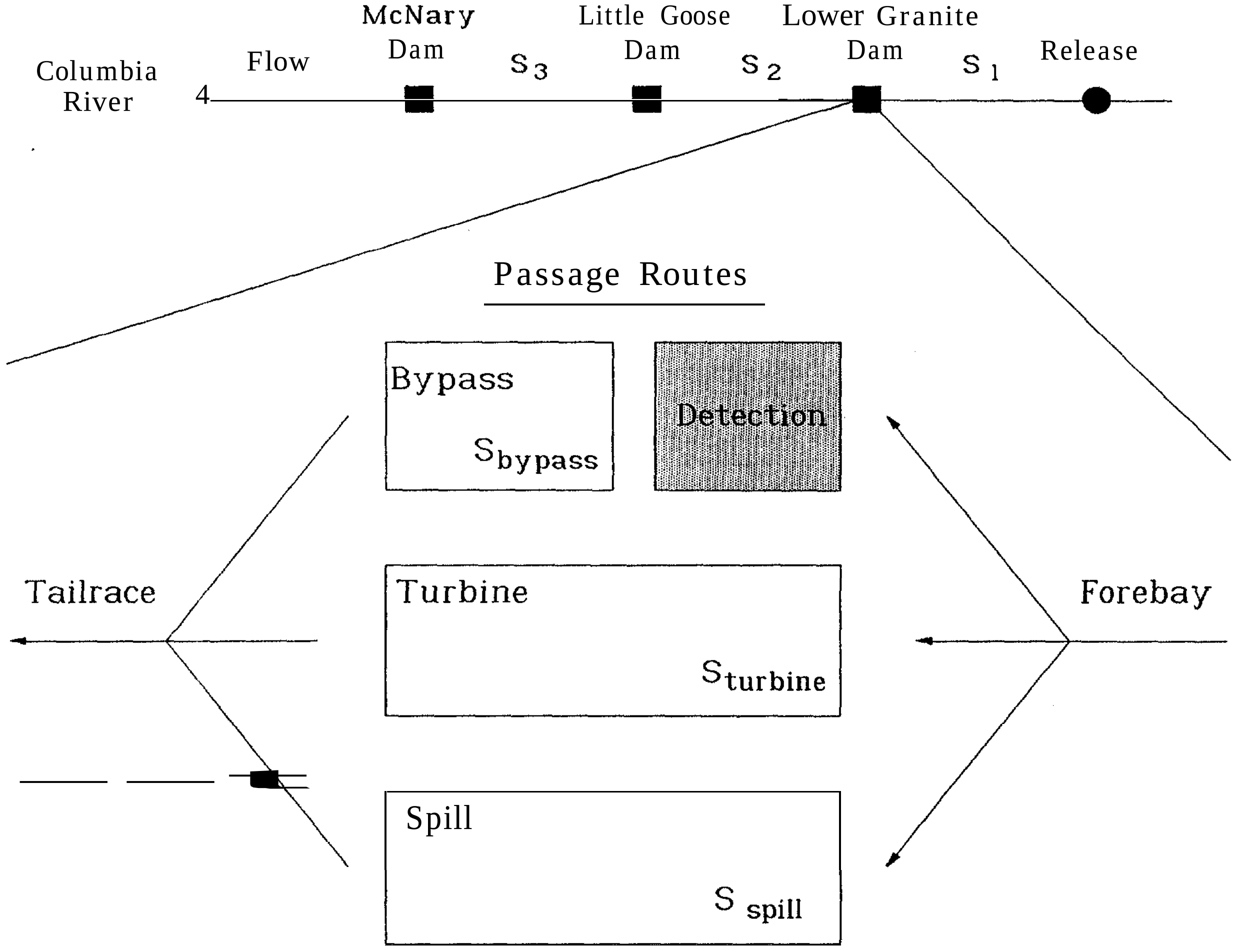


Figure A3. Schematic of the survival process for smolt in the Snake-Columbia river system. Fish are released above Lower Granite dam. In the reaches, each fish experiences a survival probability: S_1, S_2, S_3, as indicated. At each dam, the fish take one of the three passage routes shown. A fish going through a particular route experiences a survival probability associated with that route as shown: S_{Spill}, $S_{Turbine}$, S_{Bypass}. Detection in the bypass occurs before the mortality process, so that all fish entering the bypass are detected, but not all survive the bypass.

Table A 1. Parameter values used in each simulation with FGE = 0.532 at Lower Granite, FGE = 0.620 at Little Goose, FGE = 0.680 at **McNary,** and 1,000 releases per simulation.

Sim. #	Probability Through Spill[1]	Survival Probabilities		
		S_{Spill}[2]	S_{Bypass}[3]	$S_{Turbine}$[4]
1	0.0	1.00	1.00	1.00
2	0.0	1.00	0.89	0.89
3	0.0	1.00	0.85	0.85
4	0.2	0.89	0.89	0.89
5	0.4	0.89	0.89	0.89
6	0.6	0.89	0.89	0.89
7	0.2	0.85	0.85	0.85
8	0.4	0.85	0.85	0.85
9	0.6	0.85	0.85	0.85
10	0.2	0.98	0.89	0.89
11	0.4	0.98	0.89	0.89
12	0.6	0.98	0.89	0.89
13	0.2	0.98	0.85	0.85
14	0.4	0.98	0.85	0.85
15	0.6	0.98	0.85	0.85
16	0.0	1.00	0.98	0.89
17	0.0	1.00	0.98	0.85
18	0.2	0.98	0.98	0.89
19	0.2	0.98	0.98	0.85
20	0.4	0.98	0.98	0.89
21	0.4	0.98	0.98	0.85
22	0.6	0.98	0.98	0.89
23	0.6	0.98	0.98	0.85
24	0.2	0.98	0.92	0.89
25	0.4	0.98	0.92	0.89
26	0.6	0.98	0.92	0.85
27	0.2	0.98	0.89	0.92
28	0.4	0.98	0.89	0.92
29	0.6	0.98	0.89	0.92
30	0.0	1.00	0.90	0.30
31	0.2	0.5	0.80	0.30
32	0.4	0.5	0.80	0.30
33	0.6	0.5	0.80	0.30

[1] This column gives the spill passage rate for each simulation, i.e., probability a fish goes over the spillway.

[2] S_{Spill} is the survival rate for a smolt passing over the spillway.

[3] S_{Bypass} is the survival rate for a smolt passing through the bypass system.

[4] $S_{Turbine}$ is the survival rate for a smolt passing through a turbine.

The parameters in each simulation study are the route-specific survival rates, spill passage rates, and **FGE's**. We created different scenarios by changing the values of S_{Spill}, $S_{Turbine}$, S_{Bypass}, and the spill rate. Table Al lists the parameter values used for each simulation. For example, Simulation **#1** corresponds to the assumptions outlined in Appendix B. In this case, there is no differential mortality associated with any passage route ($S_{Spill} = S_{Turbine} = S_{Bypass} = 1.0$) and each fish experiences the same reach survival rates and the same capture rates. In Simulation **#25,** for example, 40% of the fish go over the spillway. Those that go over the spillway survive their passage 98% of the time. Those fish that go through the bypass survive 92% of the time, and those that go through the turbines survive **89%** of the time.

Each simulation is made up of 1,000 independent releases where each release consisted of 1,000 fish. Each of the 1,000 fish in each release experienced the same survival process described in the simulation.

2.2 Results

To evaluate the performance of the single release model, we calculated six statistics (Table A2). Table A3 shows results for simulations under Scenario 1, Table A4 for simulations under Scenario 2, and Table A5 for simulations under Scenario 3.

Each scenario presents violations of assumptions described above. The single release model was considered to be robust to the violations of a given scenario if

1. The point estimators of S_k were unbiased.
2. The variance estimators of $Var(\hat{S}_k)$ were unbiased.
3. The confidence interval coverage was nominal (i.e., a 95% confidence interval estimator included the true value, S , at least 95% of the time).

Table A2. Description of the summary statistics use to evaluate the performance of single release model.

Summary Statistics	Description
S_k	Probability that a fish alive just before reach k survives both reach ***k*** **and dam *k,*** **k-1.2.** **(S_1 =** the probability a fish survives Lower Granite reach, Dam 1 = Lower Granite;. **S_2 =** the probability a fish who survived past Lower Granite Dam survives Little Goose reach, Dam 2 = Little Goose)
$\hat{S}_{k,i}$	Estimate of Sk for the ith release, $k = 1, 2$, $i = 1, \ldots,$ **1,000**
$\bar{S}_k$	Average of the 1,000 estimates of S_k calculated for the single release model = $$\frac{1}{1000} \sum_{i=1}^{1000} \hat{S}_{k,i}$$
$V\hat{a}r(\hat{S}_{k,i})$	Estimate of the variance of $\hat{S}_{k,i}$
$\overline{V\hat{a}r}_k$	Average of the 1,000 estimates of the variance of $\hat{S}_k$ calculated for the single release model **=** $$\frac{1}{1000} \sum_{i=1}^{1000} V\hat{a}r(\hat{S}_{k,i})$$
$s^2_{\hat{S}_k}$	Empirical variance among the 1000 estimates $\hat{S}_{k,i}$, *i- 1, ...* **1000** $$s^2_{\hat{S}_k} = \frac{1}{999} \sum_{i=1}^{1000} (\hat{S}_{k,i} - \bar{S}_k)^2$$
$CV(\bar{S}_k)$	Coefficient of variation for $\bar{S}_k$ $$CV(\bar{S}_k) = \frac{\sqrt{\overline{V\hat{a}r}_k}}{\bar{S}_k}$$
Conf (S,)	Percent of the 1,000 releases where a 95% confidence interval included the true value Sk

T'able A3. Simulation results for Scenario 1 at Lower Granite (L.Gr.) and Little Goose (L.Go.). Simulations were performed with FGE = 0.532 at Lower Granite, FGE = 0.620 at Little Goose, and FGE = 0.680 at McNary. Results are based on 1,000 releases per simulation. Refer to Table A2 for column definitions.

Sim. #	Reach	S_k	$\bar{S}_k$	$\overline{\hat{Var}}_k$	$s^2_{\hat{S}_k}$	$CV(\bar{S}_k)$	95% Conf (S_k)
1	L.Gr.	0.8847	0.8850	0.0004886	0.0004470	0.0239	96.0
	L.Go.	0.8792	0.8820	0.0014288	0.00 13292	0.0413	95.6
2	L.Gr.	0.7874	0.7923	0.0006793	0.0006943	0.0333	94.9
	L.Go.	0.7824	0.7855	0.0018310	0.0018106	0.0542	95.3
3	L.Gr.	0.7520	0.7565	0.0007433	0.00076 12	0.0365	94.0
	L.Go.	0.7473	0.7512	0.0020070	0.0019482	0.0588	95.8
4	L.Gr.	0.7874	0.7928	0.0013226	0.0013236	0.0459	95.1
	L.Go.	0.7824	0.7872	0.0040623	0.0040623	0.0810	95.3
5	L.Gr.	0.7874	0.7915	0.0029302	0.0030444	0.0697	94.6
	L.Go.	0.7824	0.7947	0.0099536	0.0084390	0.1156	96.3
6	L.Gr.	0.7874	0.8020	0.0090385	0.0082866	0.1135	94.9
	L.Go.	0.7824	0.7883	0.0306642	0.0200607	0.1797	94.5
7	L.Gr.	0.7520	0.7588	0.0014293	0.0013974	0.0493	95.1
	L.Go.	0.7473	0.7512	0.0044072	0.0044368	0.0887	94.6
8	L.Gr.	0.7520	0.7581	0.0031250	0.0031744	0.0743	93.5
	L.Go.	0.7473	0.7586	0.0108216	0.0098945	0.1311	96.1
9	L.Gr.	0.7520	0.7647	0.0093840	0.0090656	0.1245	94.9
	L.Go.	0.7473	0.7600	0.0338 121	0.0226039	0.1978	94.1
10	L.Gr.	0.8034	0.8085	0.00 13423	0.0013974	0.0462	93.4
	L.Go.	0.7983	0.8020	0.0041642	0.00423 18	0.0811	94.3
11	L.Gr.	0.8193	0.8250	0.0031417	0.003657 1	0.0733	92.8
	L.Go.	0.8141	0.8234	0.0105102	0.0091587	0.1162	96.8
12	L.Gr.	0.8352	0.8478	0.0098526	0.0083678	0.1079	95.0
	L.Go.	0.8299	0.8279	0.0320388	0.0190676	0.1668	94.3
13	L.Gr.	0.7750	0.7816	0.0014780	0.0014527	0.0488	94.6
	L.Go.	0.7701	0.7756	00.004623	0.0045466	0.0869	95.9
14	L.Gr.	0.7980	0.8037	0.0034027	0.0035710	0.0744	95.1
	L.Go.	0.7930	0.8068	0.0117422	0.0099 108	0.1234	95.5
15	L.Gr.	0.8210	0.8384	0.0109913	0.0089508	0.1128	96.3
	L.Go.	0.8159	0.8132	0.0356385	0.0203456	0.1754	95.0
16	L.Gr.	0.8298	0.8317	0.0005 156	0.0005198	0.0274	94.2
	L.Go.	0.8363	0.8409	0.0014516	0.0015383	0.0466	94.5
17	L.Gr.	0.8132	0.8154	0.0005070	0.0004562	0.0262	96.7
	L.Go.	0.8250	0.8298	0.0014100	0.0014622	0.0461	94.8
18	L.Gr.	0.8372	0.8397	0.0010360	0.0009885	0.0374	94.8
	L.Go.	0.8413	0.8450	0.0033030	0.0030038	0.0649	96.1

Table A3. (Continued)

Sim. #	Reach	S_k	$\bar{S}_k$	Var,	$s^2_{\bar{S}_k}$	$CV(\bar{S}_k)$	95% Conf (S_k)
19	L.Gr.	0.8240	0.8266	0.0010148	0.0009034	0.0364	96.7
	L.Go.	0.8323	0.8362	0.0032703	0.0031488	0.0671	96.2
20	L.Gr.	0.8447	0.8482	0.0024677	0.0025568	0.0596	95.1
	L.Go.	0.8464	0.8502	0.0082885	0.0068024	0.0970	96.3
21	L.Gr.	0.8348	0.8366	0.0023926	0.0026619	0.0617	94.0
	L.Go.	0.8396	0.8506	0.00838 14	0.0078995	0.1045	96.1
22	L.Gr.	0.852 1	0.8616	0.0077860	0.006442 1	0.0932	95.6
	L.Go.	0.8514	0.8485	0.0256216	0.0154565	0.1465	94.0
23	L.Gr.	0.8455	0.8588	0.0077730	0.0061524	0.0913	96.1
	L.Go.	0.8469	0.8443	0.0254994	0.0153479	0.1467	95.0
24	L.Gr.	0.8146	0.8177	0.0012301	0.0012844	0.0438	94.2
	L.Go.	0.8126	0.8181	0.0038953	0.0040359	0.0777	95.2
25	L.Gr.	0.8277	0.8330	0.0028996	0.0032514	0.0685	92.6
	L.Go.	0.8249	0.8335	0.0097252	0.008 1578	0.1084	95.7
26	L.Gr.	0.8409	0.8515	0.0090629	0.0074060	0.1011	95.3
	L.Go.	0.8371	0.8387	0.0299075	0.0167441	0.1543	94.7
27	L.Gr.	0.7802	0.7865	0.0012914	0.0013438	0.0466	95.1
	L.Go.	0.7825	0.7863	0.0040654	0.0039275	0.0797	95.7
28	L.Gr.	0.8019	0.8079	0.0030267	0.0031954	0.0700	93.9
	L.Go.	0.8023	0.8084	0.0101840	0.0089697	0.1172	96.6
29	L.Gr.	0.8236	0.8354	0.0095605	0.0084084	0.1098	94.4
	L.Go.	0.8220	0.8259	0.0326190	0.0188086	0.1660	94.8
30	L.Gr.	0.5478	0.5507	0.0004058	0.0003443	0.0337	96.5
	L.Go.	0.6224	0.6236	0.0011099	0.0010853	0.0528	94.6
31	L.Gr.	0.489 1	0.4922	0.0007643	0.0007042	0.0539	96.0
	L.Go.	0.5380	0.5390	0.0029026	0.0024953	0.0927	96.2
32	L.Gr.	0.4774	0.479 1	0.0015371	0.0015869	0.083 I	94.6
	L.Go.	0.5134	0.5247	0.0075632	0.0079141	0.1695	94.3
33	L.Gr.	0.4657	0.4778	0.0046253	0.0045732	0.1415	95.3
	L.Go.	0.4888	0.5207	0.0298923	0.0237155	0.2958	93.5

Table A4. Simulation results for Scenario 2 at Lower Granite (L.Gr.) and Little Goose (L.Go.). Simulations were performed with FGE = 0.532 at Lower Granite, FGE = 0.620 at Little Goose, and FGE = 0.680 at McNary. Results are based on 1,000 releases per simulation. Refer to Table A2 for column definitions.

Sim. #	Reach	S_k	$\bar{S}_k$	$\overline{\widehat{Var}}_k$	$s^2_{\bar{S}_k}$	$CV(\bar{S}_k)$	95% Conf (S_k)
1	L.Gr.	0.600	0.5998	0.0005995	0.0005936	0.0406	94.7
	L.Go.	0.900	0.9059	0.0036720	0.0031197	0.0617	97.2
2	L.Gr.	0.534	0.5359	0.0005489	0.0005127	0.0423	96.1
	L.Go.	0.801	0.8032	0.0021611	0.0020801	0.0568	95.2
3	L.Gr.	0.510	0.5123	0.0005843	0.0005897	0.0474	95.4
	L.Go.	0.765	0.7664	0.0023975	0.0023115	0.0627	95.4
4	L.Gr.	0.534	0.5388	0.0009375	0.0009531	0.0573	95.3
	L.Go.	0.801	0.8022	0.0048161	0.0046461	0.0850	95.6
5	L.Gr.	0.534	0.5391	0.0019292	0.0018657	0.0801	96.5
	L.Go.	0.801	0.8040	0.0118617	0.0103896	0.1268	96.3
6	L.Gr.	0.534	0.5463	0.0058040	0.0054595	0.1353	95.8
	L.Go.	0.801	0.8036	0.0379021	0.0206777	0.1789	95.7
7	L.Gr.	0.5100	0.5157	0.0010071	0.0011393	0.0655	94.4
	L.Go.	0.7650	0.7675	0.0053005	0.0051651	0.0936	95.0
8	L.Gr.	0.510	0.5132	0.0020168	0.0017749	0.0821	96.5
	L.Go.	0.765	0.7742	0.0129992	0.0112116	0.1368	95.4
9	L.Gr.	0.510	0.5197	0.0059744	0.0054875	0.1425	95.2
	L.Go.	0.765	0.7806	0.0420265	0.0237523	0.1974	96.1
10	L.Gr.	0.5448	0.5499	0.0009614	0.0009919	0.0573	94.5
	L.Go.	0.8172	0.8174	0.0049463	0.0044009	0.0812	95.9
11	L.Gr.	0.5556	0.5599	0.0020266	0.0019429	0.0787	95.2
	L.Go.	0.8334	0.8372	0.0125799	0.0098750	0.1187	96.1
12	L.Gr.	0.5664	0.5798	0.0063376	0.0061841	0.1356	95.6
	L.Go.	0.8496	0.8422	0.0397666	0.0191486	0.1643	95.5
13	L.Gr.	0.5256	0.5321	0.0010490	0.0010919	0.0621	95.6
	L.Go.	0.7884	0.7897	0.0055503	0.0052185	0.0915	94.9
14	L.Gr.	0.5412	0.5450	0.0022080	0.0022300	0.0866	94.0
	L.Go.	0.8118	0.8207	0.0141819	0.0110163	0.1279	95.7
15	L.Gr.	0.5568	0.5715	0.0070774	0.0069308	0.1457	94.6
	L.Go.	0.8352	0.8306	0.0446804	0.0222136	0.1794	94.6
16	L.Gr.	0.5627	0.5628	0.0004584	0.0004673	0.0384	95.8
	L.Go.	0.8561	0.8602	0.0016599	0.0016751	0.0476	95.0
17	L.Gr.	0.5515	0.5525	0.0004525	0.0004583	0.0387	94.9
	L.Go.	0.8446	0.8469	0.0016270	0.0015995	0.0472	95.1
18	L.Gr.	0.5677	0.5710	0.0007804	0.0008322	0.0505	95.1
	L.Go.	0.8613	0.8619	0.0039036	0.0038868	0.0723	95.9

Table A4. (Continued)

Sim. #	Reach	S_k	$\bar{S}_k$	$\overline{Var}_k$	$s^2_{\bar{S}_k}$	$CV(\bar{S}_k)$	95% Conf (S_k)
19	L.Gr.	0.5588	0.5617	0.0007650	0.0008067	0.0506	95.3
	L.Go.	0.8520	0.8524	0.0038512	0.0037 187	0.0715	94.8
20	L.Gr.	0.5728	0.5749	0.0016265	0.0016795	0.0713	94.5
	L.Go.	0.8664	0.8670	0.00986 16	0.0077 119	0.1013	95.8
21	L.Gr.	0.5661	0.5688	0.0016005	0.0016160	0.0707	94.8
	L.Go.	0.8595	0.8621	0.0098809	0.008 1829	0.1049	94.7
22	L.Gr.	0.5779	0.5891	0.0050241	0.0049358	0.1193	96.1
	L.Go.	0.8716	0.8601	0.0314155	0.0152967	0.1438	95.2
23	L.Gr.	0.5734	0.5863	0.0050212	0.0053255	0.1245	95.9
	L.Go.	0.8670	0.8554	0.0311025	0.0147334	0.1419	95.9
24	L.Gr.	0.5525	0.5560	0.0008889	0.0008964	0.0538	95.2
	L.Go.	0.8319	0.8339	0.0045675	0.0043444	0.0790	95.2
25	L.Gr.	0.5613	0.5650	0.0018775	0.0017751	0.0746	95.0
	L.Go.	0.8444	0.8473	0.0115919	0.0089496	0.1117	95.8
26	L.Gr.	0.5702	0.5829	0.0058235	0.0058244	0.1309	95.3
	L.Go.	0.8569	0.8511	0.0367750	0.0172932	0.1545	95.9
27	L.Gr.	0.5515	0.5559	0.0009729	0.000968 1	0.5600	95.2
	L.Go.	0.8241	0.8252	0.0050063	0.0047827	0.0838	94.2
28	L.Gr.	0.5607	0.5657	0.0020645	0.0020248	0.0795	95.5
	L.Go.	0.8386	0.8418	0.0126717	0.0099037	0.1182	96.2
29	L.Gr.	0.5698	0.5833	0.006429 1	0.0066122	0.1394	95.0
	L.Go.	0.853 1	0.8456	0.0398466	0.0188964	0.1626	95.7
30	L.Gr.	0.3715	0.3733	0.0003324	0.0002912	0.0457	96.9
	L.Go.	0.6372	0.6376	0.0014419	0.0013558	0.0577	95.5
31	L.Gr.	0.3317	0.3350	0.0005526	0.0005795	0.0719	94.7
	L.Go.	0.5508	0.5533	0.0037470	0.0037412	0.1105	95.1
32	L.Gr.	0.3238	0.3248	0.0010289	0.0009263	0.0937	94.8
	L.Go.	0.5256	0.5400	0.0097005	0.0087612	0.1733	96.6
33	L.Gr.	0.5358	0.5412	0.0009422	0.0010050	0.0586	94.1
	L.Go.	0.8080	0.8077	0.004876 1	0.0047793	0.0856	94.8

Table A5. Simulation results for Scenario 3 at Lower Granite (L.Gr.) and Little Goose (L.Go.). Simulations were performed with FGE = 0.532 at Lower Granite, FGE = 0.620 at Little Goose, and FGE = 0.680 at McNary. Results are based on 1,000 releases per simulation. Refer to Table A2 for column definitions.

Sim. #	Reach	S_k	$\bar{S}_k$	Var_k	$s^2_{\bar{S}_k}$	$CV(\bar{S}_k)$	95% Conf (S_k)
1	L.Gr.	0.600	0.5998	0.0005995	0.0005936	0.0406	94.7
	L.Go.	0.900	0.9059	0.0036720	0.0031197	0.0617	97.2
2	L.Gr.	0.5340	0.5925	0.0005362	0.0006149	0.0419	29.5
	L.Go.	0.8010	0.8422	0.0020584	0.0021248	0.0547	86.9
3	L.Gr.	0.5100	0.5892	0.0005683	0.0005988	0.0415	5.4
	L.Go.	0.7650	0.8197	0.0022429	0.0022459	0.0578	81.3
4	L.Gr.	0.5340	0.5922	0.0009175	0.0009072	0.0509	52.7
	L.Go.	0.8010	0.8441	0.0046833	0.0042324	0.0771	92.7
5	L.Gr.	0.5340	0.5894	0.0019030	0.0019661	0.0752	82.1
	L.Go.	0.8010	0.8394	0.0112351	0.0087125	0.1112	98.4
6	L.Gr.	0.5340	0.5949	0.0057341	0.0055675	0.1254	95.3
	L.Go.	0.8010	0.8308	0.0351761	0.0189330	0.1656	96.6
7	L.Gr.	0.5100	0.5892	0.0009828	0.0010157	0.0541	25.9
	L.Go.	0.7650	0.8189	0.0049801	0.0051076	0.0873	92.1
8	L.Gr.	0.5100	0.5859	0.0020217	0.0018995	0.0744	64.0
	L.Go.	0.7650	0.8177	0.0119730	0.0095710	0.1196	98.3
9	L.Gr.	0.5100	0.5909	0.0060325	0.0052747	0.1229	93.6
	L.Go.	0.7650	0.8082	0.0371979	0.0202051	0.1759	97.5
10	L.Gr.	0.5448	0.6039	0.0009367	0.0010291	0.0531	51.8
	L.Go.	0.8172	0.8607	0.0048125	0.0043620	0.0767	93.8
11	L.Gr.	0.5556	0.6130	0.0020080	0.0020074	0.0731	81.3
	L.Go.	0.8334	0.8717	0.0119073	0.0082153	0.1040	98.5
12	L.Gr.	0.5664	0.6304	0.0061921	0.0059903	0.1228	95.0
	L.Go.	0.8496	0.8698	0.0367363	0.0161516	0.1461	97.5
13	L.Gr.	0.5256	0.6069	0.0010152	0.0010318	0.0529	25.2
	L.Go.	0.7884	0.8433	0.0051924	0.0047596	0.0818	91.5
14	L.Gr.	0.5412	0.6203	0.0021936	0.0022365	0.0762	65.4
	L.Go.	0.8118	0.8631	0.0128309	0.0092945	0.1117	98.4
15	L.Gr.	0.5568	0.6453	0.0069262	0.0066759	0.1266	91.8
	L.Go.	0.8352	0.8619	0.0390297	0.0169145	0.1509	97.8
16	L.Gr.	0.5627	0.5731	0.0004567	0.0004661	0.0377	94.1
	L.Go.	0.8561	0.8648	0.0016297	0.0016127	0.0464	94.3
17	L.Gr.	0.5515	0.5623	0.0004504	0.0004829	0.0391	92.2
	L.Go.	0.8446	0.8530	0.0016024	0.0016442	0.0475	94.6
18	L.Gr.	0.5678	0.5811	0.0007753	0.0007764	0.0479	93.9
	L.Go.	0.8613	0.8701	0.0038878	0.0035206	0.0682	97.3

Table AS. (Continued)

Sim. #	Reach	S_k	$\bar{S}_k$	$\overline{V\hat{a}r}_k$	$s^2_{\bar{S}_k}$	$CV(\bar{S}_k)$	95% Conf (S_k)
19	L.Gr.	0.5588	0.5723	0.0007633	0.0008124	0.0498	93.2
	L.Go.	0.8520	0.8594	0.0038275	0.0037226	0.0710	96.0
20	L.Gr.	0.5728	0.5846	0.0016210	0.00 15632	0.0676	95.8
	L.Go.	0.8664	0.8731	0.0097933	0.0073357	0.0981	96.5
21	L.Gr.	0.5661	0.5783	0.0015982	0.0016153	0.0695	95.2
	L.Go.	0.8595	0.8661	0.0097046	0.0072443	0.0983	96.3
22	L.Gr.	0.5779	0.5972	0.0049717	0.004888 1	0.1171	96.8
	L.Go.	0.8716	0.8649	0.0305633	0.0144657	0.1391	97.1
23	L.Gr.	0.5734	0.5934	0.0049236	0.0048242	0.1170	96.3
	L.Go.	0.8670	0.8623	0.0307007	0.0141956	0.1382	97.0
24	L.Gr.	0.5525	0.5956	0.0008774	0.0008799	0.0498	71.6
	L.Go.	0.8319	0.8621	0.0044421	0.0039393	0.0728	95.8
25	L.Gr.	0.5613	0.6027	0.0018658	0.0018895	0.072 1	89.0
	L.Go.	0.8444	0.8724	0.0 110998	0.0074817	0.0991	97.8
26	L.Gr.	0.5702	0.6173	0.005678 1	0.0051889	0.1167	96.6
	L.Go.	0.8569	0.8726	0.0349112	0.0147290	0.1391	97.5
27	L.Gr.	0.5515	0.6101	0.00094 16	0.0009546	0.0506	54.5
	L.Go.	0.8241	0.8701	0.0048704	0.0041614	0.0741	95.0
28	L.Gr.	0.5607	0.6188	0.002037 1	0.0020914	0.0739	81.1
	L.Go.	0.8386	0.8758	0.0118882	0.007674 1	0.1000	98.5
29	L.Gr.	0.5698	0.6318	0.0061639	0.0059192	0.1218	95.4
	L.Go.	0.853 1	0.8734	0.0366633	0.0152356	0.1413	97.7
30	L.Gr.	0.3715	0.4132	0.0003458	0.0003100	0.0426	36.4
	L.Go.	0.6372	0.6592	0.0013922	0.0014148	0.057 1	91.6
31	L.Gr.	0.3317	0.4116	0.0006029	0.0005600	0.0575	6.1
	L.Go.	0.5508	0.5905	0.003445 1	0.0031989	0.0958	95.0
32	L.Gr.	0.3238	0.3997	0.0011547	0.0011113	0.0834	34.9
	L.Go.	0.5256	0.5667	0.0082215	0.0080457	0.1583	96.4
33	L.Gr.	0.3158	0.3926	0.0032820	0.0032262	0.1447	84.1
	L.Go.	0.5004	0.5485	0.0302273	0.0247045	0.2866	95.6

The point estimators were regarded as unbiased if the difference between true survival and average estimate of survival ($|\overline{\hat{S}_k} - S_k|$) was small relative to the empirical variance ($s^2_{\hat{S}_k}$). The variance estimators of $Var(\hat{S}_k)$ were regarded as unbiased if the empirical variance estimate ($s^2_{\hat{S}_k}$) was approximately equal to the average variance ($\overline{\hat{Var}_k}$). Finally, the confidence coverage was considered to be nominal $[(1 - \alpha)100\% = 95\%]$ if it was approximately 95%. Based on 1,000 simulations, the 95% confidence limits should be within

$$\pm 2\sqrt{\frac{0.05(0.95)}{1,000}} \quad \text{or} \pm 1.4\%$$

of the nominal value to be considered unbiased.

The results of the simulation studies differ between the three scenarios investigated. Heterogeneity in survival probabilities of the tagged fish, the result of differential fitness, violates the assumption of equal survival of fish in the single release model. Results of simulation under Scenario I where survival probabilities are heterogeneous indicate survival estimation have the following properties (Table A3):

1. point estimates of survival are apparently unbiased;
2. variance estimates are robust, being biased upward when affected;
3. the **95%** confidence intervals have nominal coverage.

Hence, heterogeneity in the inherent survival potentials of fish has no effect on estimators of reach survival as long as the inter-fish variability is independent of dam passage routes.

Despite the differential mortality in the spill, bypass and turbine routes of the dam in Scenario 2, all tagged fish have the same expectation of survival at the beginning of each river reach (i.e., just below the dam beyond the tailrace). Hence, Scenario 2 conforms to the assumptions of the single release model (Appendix B), that all fish have equal and independent survival probabilities in each reach. Simulation results for Scenario 2 (Table A4) show the estimators for release-recapture models are robust to heterogeneous survival probabilities among passage routes in the dam as long as such mortality occurs prior to the decoding. For Scenario 2, the estimators of reach survival have the following properties (Table A4):

1. Point estimators are unaffected by heterogeneous survival rates among the various passage rates at the decoder dam.
2. The asymptotic variance estimates are unbiased.
3. The 95% confidence intervals have nominal coverage.

Under Scenario 3, decoded fish going through the bypass experience a source of mortality exhibited at the time of decoding or after detection. This mortality could be either acute or delayed mortality associated with the stress of the decoder and slide gate facilities. Alternatively, the mortality of decoded fish could be associated with predation at the outfall of the bypass facility, prior to mingling with tagged fish from other passage routes. Under such conditions of differential mortality of tagged fish during or after detection, the release-recapture models provide (Table A5):

1. positively biased point estimators of reach and dam survival;
2. variance estimates that are only slightly biased; and
3. the 95% confidence estimates have less than nominal coverage.

Consequently, the release-recapture estimators of reach survival are not robust to differential bypass mortality that occurs during or after detection. Alternative estimation and experimental design is necessary to estimate reach survival under Scenario 3.

2.3 Discussion

Scenario 1 presents a situation similar to that of Scenario 2, with the exception that the survival rates are heterogeneous among smolts (Figure Al). Survival estimates are unbiased, and confidence interval coverage is approximately nominal. Therefore, the estimators in the single release model are robust to violations of the homogeneous survival rate assumption.

In Scenario 2, where each of the passage routes is associated with a different mortality rate (Figure A2), the survival estimates are unbiased. The differential mortality rates induced in these simulations do not introduce bias because once a smolt survives passage at a dam, it has the same expectation of being seen at the next dam as any other smolt who survived the dam. The simulation results support this conclusion not only with estimated survival rates close to the simulated rates, but also with confidence intervals that have approximately nominal coverage (Table A4).

In Scenario 3, a smolt which is detected at dam A does not have the same expectation of being seen at the next dam as every other smolt that survived dam A. Smolt that are detected in the bypass experience an extra mortality term, i.e., bypass mortality which occurs after detection (Figure A3). The discrepancy in expectations causes the model to fail in accurately estimating true survival. This can be seen by the poor estimation performance as well as the less than nominal confidence coverage (Table A5).

Each of the scenarios presents an underlying survival process for smolt survival. Our simulation study has shown that differential mortality in the various passage routes does not affect the validity of the survival estimates unless there is mortality in the bypass route after the detectors and before the fish intermingled once again in the downstream reach. We have also shown that the estimators are not adversely affected when the survival rates vary among individual smolt.

Tagging studies at Bonneville Dam Powerhouse #2 have indicated the possibility of mortality associated with bypass passage (Ledgerwood et al. 1990). If this type of mortality exists at Lower Granite or Little Goose dams, and furthermore is exhibited after detection of the pit-tagged fish, then the single release model will result in biased reach survival estimates.

To test and adjust for any post detection bypass mortality at decoder dams, the existing single release model can be modified to include an additional pair of tag releases; one release above and one below the source of mortality in the bypass system. The paired release at the bypass unit can be used to quantify this additional source of mortality and adjust the bias of the survival estimates. A statistical description of a release-recapture model adjusted for post-detection bypass mortality is presented in the next section of this report.

3.0 Release-Recapture Model Adjusted for Bypass Mortality

Robustness studies of the single release model for reach survival revealed that survival estimates were sensitive to the presence of a source of bypass mortality during or following detection of smolt in the PIT-tag facilities. The existence of differential mortality of tagged fish in the bypass facilities of Lower Granite and Little Goose Dams is unknown at this time. However, the possibility of bypass mortality and subsequent estimation bias of reach survival cannot be ignored ***a priori*** in the design of survival studies.

The purpose of this section is to propose a modification of the single release-recapture model that accounts for the existence of post-detection bypass mortality, and consequently provides unbiased reach survival estimates. The modified survival model is not only robust to the presence of post-detection bypass mortality but also provides a means to test for its existence, as well as, estimate its magnitude.

3.1 Likelihood Model

A modification of the single release-recapture model will be illustrated for the case of a tagging study with a release above Lower Granite Dam and recoveries at Lower Granite, Little Goose, and McNary Dams. Two additional tag releases within the bypass facilities of Lower Granite Dam will be conducted to estimate post-detection bypass mortality at that facility (Figure A4).

A primary release (R_1) of PIT-tagged fish above Lower Granite Dam will be used to estimate reach survival from the point of release to Lower Granite Dam (S_1) . A paired release of two additional groups of PIT-tagged fish (i.e., R_C and R_T) will be used to estimate the bypass survival rate (τ) at Lower Granite Dam. The paired release consists of treatment fish (R_T) released just above the PIT-tag detector at Lower Granite Dam and control fish (R_C) released just below the bypass outfall. The detection probabilities at the PIT-tag decoder facilities at Lower Granite and Little Goose Dams may be distinct between facilities and the primary and secondary release groups. In practice, the releases of the secondary PIT-tag fish groups (i.e., R_T and R_C) would be timed to coincide with the passage of the primary release (i.e., R_1) to permit inferences to similar temporal survival processes at the bypass facilities. However, coincidence of the primary and secondary release groups is not necessary for numerical estimation procedures.

Parameters used in the likelihood model are defined as follows:

R_1 = number of PIT-tagged fish in the primary release above Lower Granite Dam,

R_2 = number of PIT-tagged fish detected and re-released at Lower Granite Dam from the primary' release R_1,

S_1 = survival probability from release site to the Lower Granite Dam **tailrace** for the primary release,

S_2 = survival probability from Lower Granite Dam to the Little Goose Dam **tailrace** for the primary release,

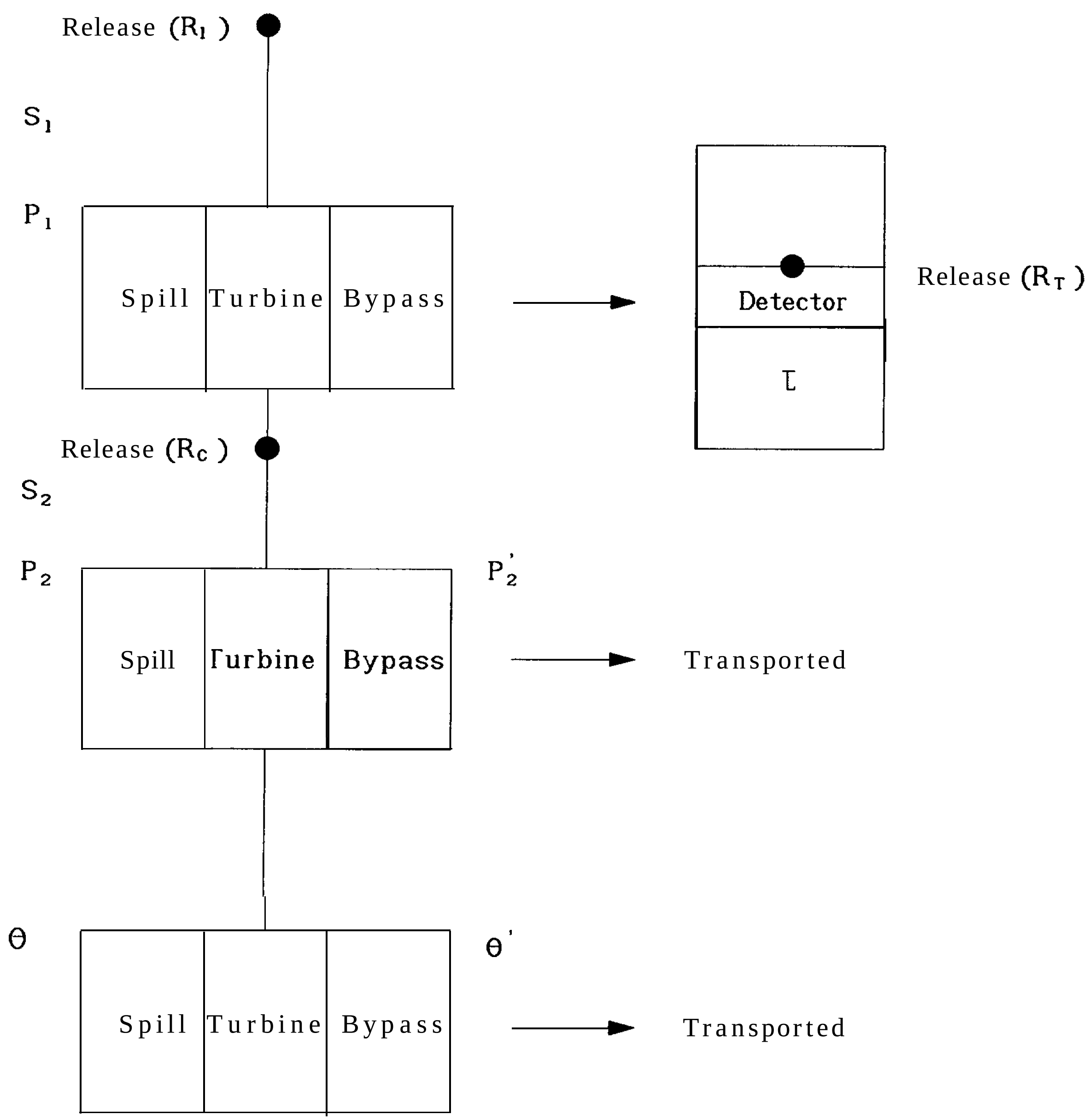


Figure A4. Release-recapture scenario for the modified tag-recapture model. Primary release of R_1 PIT-tagged fish occurs above Lower Granite Dam to estimate reach survivals S_1. Secondary releases above (R_T) and below (R_c) the bypass facilities and PIT-tag decoder at Lower Granite Dam are used to estimate the bypass survival (τ) of detected fish. The parameters for the primary (S_2, S_2, p_1, p_2, θ) and secondary (S_2', p_2', θ') release groups can be modeled with similar or dissimilar downriver survival and detection probabilities.

p_1 = probability of detection at decoder facilities at Lower Granite Dam for the primary release,

p_2 = probability of detection at decoder facilities at Little Goose Dam for the primary release,

θ = joint probability of survival between Little Goose and McNary Dams and detection at McNary Dam for the primary release,

R_T = number of PIT-tagged fish in secondary release above decoder facilities in bypass at Lower Granite Dam,

R_C = number of PIT-tagged fish in secondary release below decoder facilities in bypass at Lower Granite Dam,

τ = survival probability through decoder facilities and bypass at Lower Granite Dam,

S_2' = survival probability from Lower Granite to Little Goose Dam for the secondary release,

p_2' = probability of detection at decoder facilities at Little Goose Dam for secondary release,

θ' = joint probability of survival between Little Goose and McNary Dams and detection at McNary Dam for the secondary release.

The recapture data for the primary and secondary releases can be summarized in data matrices (Table A6).

Table A6. Summary of the numbers of PIT-tagged fish detected among the various release groups.

a. Recaptures from Primary Release

Release	Detections		
	Lower Granite	Little Goose	McNary
R_1	n_{11}	n_{12}	n_{13}
R_2		n_{22}	n_{23}

b. Recaptures from Secondary Releases

Release	Detections	
	Little Goose	McNary
R_T	n_{T2}	n_{T3}
R_C	n_{C2}	n_{C3}

The joint likelihood for the primary and secondary releases is a product of multinomial distributions where

$$L = \binom{R_1}{n_{11}, n_{12}, n_{13}}(S_1 p_1)^{n_{11}}(S_1 S_2(1-p_1)p_2)^{n_{12}}(S_1 S_2(1-p_1)(1-p_2)\theta)^{n_{13}}$$

$$(1-S_1 p_1 - S_1 S_2(1-p_1)(p_2+(1-p_1)\theta))^{R_1-n_{1.}}$$

$$\cdot\binom{R_2}{n_{22}, n_{23}}(\tau S_2 p_2)^{n_{22}}(\tau S_2(1-p_2)\theta)^{n_{23}}(1-\tau S_2(p_2+\theta-p_2\theta))^{R_2-n_{2.}}$$

$$\cdot\binom{R_T}{n_{T2}, n_{T3}}(\tau S_2' p_2')^{M_{T2}}(\tau S_2'(1-p_2')\theta)^{n_{T3}}(1-\tau S_2'(p_2'+\theta'-p_2'\theta'))^{R_T-n_{T.}}$$

$$\cdot\binom{R_C}{n_{C2}, n_{C3}}(S_2' p_2')^{n_{C2}}(S_2'(1-p_2')\theta')^{n_{C3}}(1-S_2'(p_2'+\theta'-p_2'\theta'))^{R_C-n_{C.}}$$

and where the dot notation refers to summation over the second subscript. The likelihood as written is over **parameterization.** Using the reparameterization

$$\alpha_1 = S_2 p_2$$

$$\beta_1 = S_2(1-p_2)\theta$$

$$\alpha_2 = S_2' p_2'$$

$$\beta_2 = S_2'(1-p_2')\theta'$$

the likelihood model can be rewritten as follows:

$$
\begin{aligned}
L = & \binom{R_1}{n_{11}, n_{12}, n_{13}} (S_1 p_1)^{n_{11}} (S_1(1-p_1)\alpha_1)^{n_{12}} (S_1(1-p_1)\beta_1)^{n_{12}} \\
& (1 - S_1 p_1 - S_1(1-p_1)(\alpha_1 + \beta_1))^{R_1 - n_{1.}} \\
& \cdot \binom{R_2}{n_{22}, n_{23}} (\tau\alpha_1)^{n_{22}} (\tau\beta_1)^{n_{23}} (1 - \tau(\alpha_1 + \beta_1))^{R_2 - n_{2.}} \\
& \cdot \binom{R_T}{n_{T2}, n_{T3}} (\tau\alpha_2)^{n_{T2}} (\tau\beta_2)^{n_{T3}} (1 - \tau(\alpha_2 + \beta_2))^{R_T - n_{T.}} \\
& \cdot \binom{R_C}{n_{C2}, n_{C3}} \alpha_2^{n_{C2}} \beta_2^{n_{C3}} (1 - (\alpha_2 + \beta_2))^{R_C - n_{C.}}
\end{aligned}
$$

The resulting likelihood consists of seven parameters and nine minimum sufficient statistics requiring an iterative numerical procedures for maximum likelihood estimation.

Assumptions of the modified release-recapture model are as follows:

1. Test fish are representative of the population of inference.
2. Test conditions are representative of the conditions of interest.
3. Numbers of fish released (i.e., R_1, R_C, and R_T) are exactly known.
4. Marking is accurate with regard to no handling mortality or tag loss and tags are read correctly.
5. Initial release is instantaneous and all detected fish are re-released immediately upon detection.
6. Fate of each tagged fish is independent.
7. If replicate studies are conducted, then the data from different releases are statistically independent.

8. All fish in a release group (R_1, R_C , and R_T) have the same capture probability at a particular detector site.

9. All fish in a release group (R_1, R_C , and R_T) have the same survival probability.

10. Secondary release groups (i.e., R_C and R_T) mix below Lower Granite Dam and move downriver together.

11. Fish detected at Lower Granite Dam have a differential mortality rate (τ) that undetected fish do not have. This mortality rate is the same for the primary (R_1) and the secondary (R_C and R_T) release groups.

A key assumption in estimating the bypass survival rate is that the PIT-tagged fish of the R_T and R_C releases have equal downstream survival and detection probabilities (i.e., move downriver together). A test of homogeneity for the R_C and R_T releases can be performed based on a $K \times 2$ contingency table of the form

		Release	
		R_T	R_C
Day	1		
	2		
	⋮	⋮	⋮
	K		
		n_{T2}	n_{C2}

The entries in the contingency table are the numbers of PIT-tagged fish from each release group detected at Little Goose Dam on a daily basis. A similar test of homogeneity can be based on daily tag detections at McNary Dam.

A simplification of the modified release-recapture likelihood is achieved by specifying $\alpha_1 = a_2$ and/or $\beta_1 = \beta_2$. Homogeneity of the primary (R) and secondary $(R_T$ and $R_C)$ release groups is not required for survival estimation, but can result in greater sampling precision if it exists. Likelihood ratio tests can be used to test for homogeneity of primary and secondary release groups with the hypotheses

$$H_o: \quad \alpha_1 = \alpha_2$$

$$H_a: \quad \alpha_1 \neq \alpha_2$$

and

$$H\ ,: \quad \beta_1 = \beta_2$$

$$H\ ,: \quad \beta_1 \neq \beta_2$$

or

$$H\ ,: \quad \alpha_1 = \alpha_2 \text{ and } \beta_1 = \beta_2$$

$$H_a: \quad \text{not above conditions.}$$

3.2 Results

Monte Carlo simulation studies were conducted to determine the robustness of the modified single release model to the presence of post-detection bypass mortality at Lower Granite Dam. The parameter values used in the simulations are those presented in Table A 1. All simulations were conducted under Scenario 3, found earlier to produce biased estimates of reach survival for the single release model. Thus, S_{Bypass} becomes the post-detection bypass survival rate in these simulations.

Performance of the single release model was evaluated using the summary statistics described in Table A2. However, in these later simulations, we allowed fish transportation at both Little Goose and McNary Dams. Consequently, only reach survival S_1 from the point of release through Lower Granite Dam is estimable (Table A7). Inspection of the average values for reach survival shows excellent agreement with parameter values simulated. The 95% confidence interval estimates for S_1 also were near nominal levels for all parametric values investigated (Table A7). Thus, it appears the modified single release model resolves the problem of biased reach survival estimates caused by any post-detection bypass mortality.

Some of the simulations produced a singular covariance matrix. This occurred when the estimate of τ was approximately 1.0. In these cases, $V\hat{a}r(\hat{\tau})$ = 0.0 and $C\hat{o}v(\hat{\tau}, \hat{\theta})$ = 0.0 where $\hat{\theta}$ is any other parameter estimate. Consequently, the estimated covariance matrix was singular and the variance estimates could not be calculated. Thus, the average variance values were calculated from only those simulations where variances could be calculated, introducing a variance bias. Therefore, in the simulations, the average variances are not expected to equal the empirical variances but are expected to be greater. The average variances will be greater than or equal to the empirical variances because the variances excluded in calculating average variances are the smallest ones. The closer an estimate is to 0.0 or 1.0, the smaller its variance. Estimates $\hat{\tau}$ and $\hat{S}$, are positively correlated so that bias in $\overline{Var(\tau)}$ caused by runs where $\hat{\tau}$ = 1 will bias $\overline{Var(S_1)}$. The empirical variances as well as the average estimates remain unbiased because estimation was possible in all the simulations.

Table A7. Simulation results for the modified single release model for Lower Granite reach. Each simulation consists of 1,000 runs. In each run, $R_1 = R_T = R_C = 1{,}000$. Refer to Table A2 for column definitions.

Sim. #	S_1	$\bar{S}_1$	$\overline{Var}_1$	$S^2_{\bar{S}_1}$	$CV(\bar{S}_1)$	95% Conf (S_1)
1	0.9000	0.8999	0.000985	0.001057	0.0361	94.37
2	0.8537	0.8541	0.000901	0.000865	0.0344	95.48
3	0.8368	0.8370	0.000908	0.000918	0.0362	95.20
4	0.8431	0.8414	0.001767	0.001839	0.0510	94.59
5	0.8326	0.8311	0.003868	0.004044	0.0765	94.14
6*	0.8221	0.8188	0.009869	0.008905	0.1152	93.05
7	0.8225	0.8196	0.001757	0.001744	0.0510	92.77
8	0.8081	0.8092	0.003908	0.004012	0.0783	94.67
9*	0.7937	0.7965	0.010113	0.009019	0.1192	94.95
10	0.8593	0.8586	0.001830	0.001750	0.0487	95.66
11	0.8650	0.8637	0.004056	0.004100	0.0741	96.30
12*	0.8707	0.8674	0.010659	0.008058	0.1035	93.22
13	0.8459	0.8445	0.001865	0.001876	0.0513	94.80
14	0.8549	0.8502	0.004182	0.003955	0.0740	93.90
15*	0.8639	0.8632	0.011383	0.008020	0.1038	94.56
16	0.8537	0.8514	0.000782	0.000697	0.0310	95.63
17	0.8368	0.8349	0.000761	0.000642	0.0303	95.91
18	0.8593	0.8561	0.001552	0.001478	0.0449	94.13
19	0.8459	0.8403	0.001509	0.001341	0.0436	93.76
20*	0.8650	0.8536	0.003515	0.002759	0.0615	95.63
21*	0.8549	0.8463	0.003445	0.002811	0.0627	93.89
22*	0.8707	0.8655	0.009520	0.006617	0.0940	94.42
23*	0.8639	0.8570	0.009350	0.006809	0.0963	92.55
24	0.8593	0.8590	0.001757	0.001746	0.0486	94.80
25	0.8650	0.8618	0.003921	0.003585	0.0695	94.11
26*	0.8707	0.8675	0.010238	0.007607	0.1005	94.73
27	0.8694	0.8685	0.001861	0.001713	0.0477	94.93
28	0.8726	0.8705	0.004122	0.003159	0.0646	97.08
29*	0.8757	0.8709	0.010643	0.007661	0.1005	93.60
30	0.6052	0.6060	0.000444	0.000424	0.0340	95.87
31	0.5741	0.5745	0.000816	0.000852	0.0508	94.60
32	0.5431	0.5417	0.001682	0.001682	0.0757	93.40
33	0.5121	0.5151	0.004748	0.004890	0.1358	93.69

* This simulation had 50 or more cases not included in average variance calculation due to singularity of the covariance matrix.

3.3 Discussion

The existence of post-detection bypass mortality at Lower Granite Dam precludes obtaining unbiased reach survival estimates using the single release model. To eliminate this potential source of bias, a modified single release model was developed that can be used to test for and adjust reach survival estimates for the existence of post-detection bypass mortality. Simulation studies (Table A7) indicate unbiased reach survival estimates are possible with the use of the modified model. Hence, the assumption violation which proved to have the greatest effect on bias can be managed by the modified single release model.

With the statistical feasibility of providing reliable reach survival estimates, a remaining consideration is the sample sizes of PIT-tagged fish needed for conducting a Snake River survival study. The last section of this report provides estimates of anticipated precision of reach survival estimates ($\hat{S}_1$) using the modified release-recapture model for various sample sizes (R_1, R_T, R_C).

4.0 Sample Size Calculations

Sample size calculations consist of determining numbers of PIT-tagged fish that must be allocated to releases R_1, R_C, and R_T for the modified release-recapture model for a prescribed level of sampling precision. For simplification of the sample size calculations, R_1 was arbitrarily set equal to $R_C + R_T$ and $R_C = R_T$. Hence, 50% of the total sample size (i.e., $R_. = R_1 + R_C + R_T$) was allocated to R_1 (i.e., $0.5R_. = R_1$) and 25% to each of the secondary releases (i.e., $0.25R_. = R_C = R_T$). This allocation of PIT-tagged fish will not be optimal for all sample sizes but serves as a general guide for design considerations. In general, when too much of the sample ($R_.$) is allocated to R_T and

R_C , there will be an unnecessary loss of precision in $\hat{S}_1$. Alternatively, when too few fish are allocated to R_T and R_C , the chances of collecting a data set where the covariance matrix is singular (i.e., where the sample variances cannot be estimated for $\hat{\tau}$) increases markedly.

Because estimation of reach survival (i.e., S_1) is the focus of the Snake River survival studies, sample size calculations were based on the precision of $\hat{S}_1$. Precision was defined as

$$P(|\hat{S}_1 - S_1| < \epsilon) = 1 - a \quad .$$

In other words, we desire the absolute difference between the true value and estimate of reach survival (i.e., $|\hat{S}_1 - S_1|$) to be less than ϵ, (1 − a) 100% of the time. This definition of precision is consistent with the half-width of a (1 − a) 100% confidence interval for S_1 , equaling

$$\epsilon = Z_{1-\frac{\alpha}{2}}\sqrt{Var(\hat{S}_1)} \quad .$$

Values of ϵ were calculated for both 90% and 95% confidence intervals as a function of total sample size $(R_. = R_1 + R_C + R_T)$.

Sample size calculations were performed under a single scenario (#19, Table Al) where

a. S_{Spill} = 0.98
b. S_{Bypass} = 0.98
c. $S_{Turbine}$ = 0.85
d. τ = 0.90
e. Percent Spill = 0, 20%, 40%, 60%
f. S_1 = 0.2, 0.6, 0.9
g. S_2 = 0.6

h. FGE = 0.532 at Lower Granite Dam

FGE = 0.680 at Little Goose Dam

FGE = 0.680 at McNary Dam

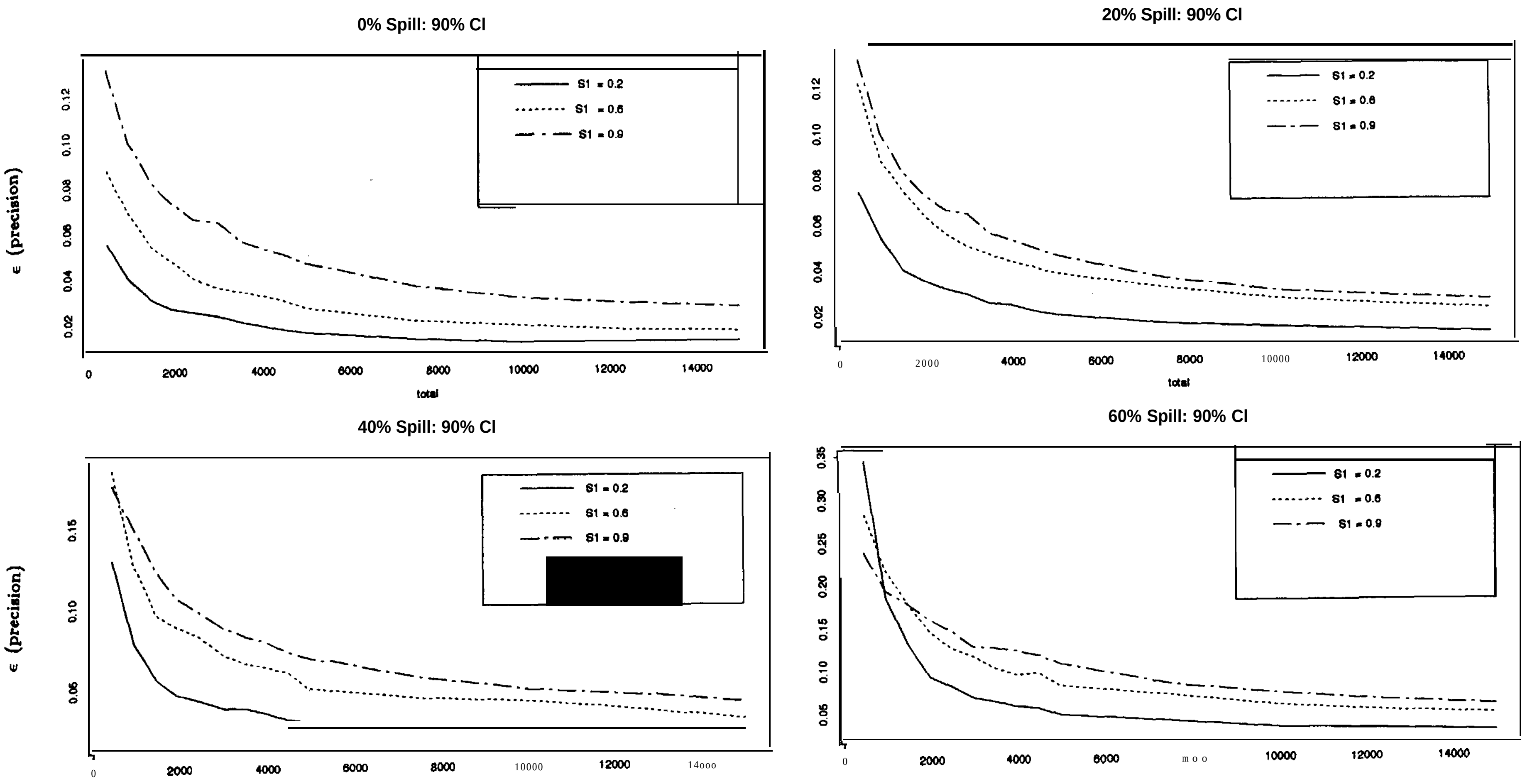
Precision curves as a function of $R_{.}$ are presented in Figures A5 and A6.

Figures A5 and A6 show that precision in estimating $\hat{S}_1$ decreases with increasing spill and with decreasing values of S_1. The curves level off at a total sample size of about 3,000 so that marked increases in sample size above 3,000 are not associated with marked decreases in precision. Figure A7 shows that the chances of collecting data sets for which the covariance matrix will be singular also does not decrease markedly after the total sample size reaches 3,000. Because the percentage of singularities is uncomfortably high for total sample sizes < 3,000, we advise experimenters to conduct such mark-recapture studies with at least 3,000 smolts. (1500 at the primary release and 750 in each of the secondary paired releases at Lower Granite Dam.)

5.0 Conclusions

The single release-recapture method of estimating reach survival is robust to most forms of heterogeneity in smolt survival. Survival rates of smolt can vary between individuals and differ between passage routes within hydroelectric projects, yet reach survival can be reliably estimated. A potential bias was identified with the use of release-recapture methods when smolt experience a different mortality associated with bypass passage when the mortality acts during or following detection.

A modified release-recapture model was developed and evaluated in this report. It can estimate bypass mortality and provide reliable estimates of reach survival. The modified study design



FigureA5. Plots of total sample size against $\epsilon = 1.645 * SE(\hat{S}_1)$ in estimating survival based on a 90% CI at 0, 20, 40, and 60% spill. Precision is defined by $P(|\hat{S}_1 - S_1| > \epsilon) = 0.90$.

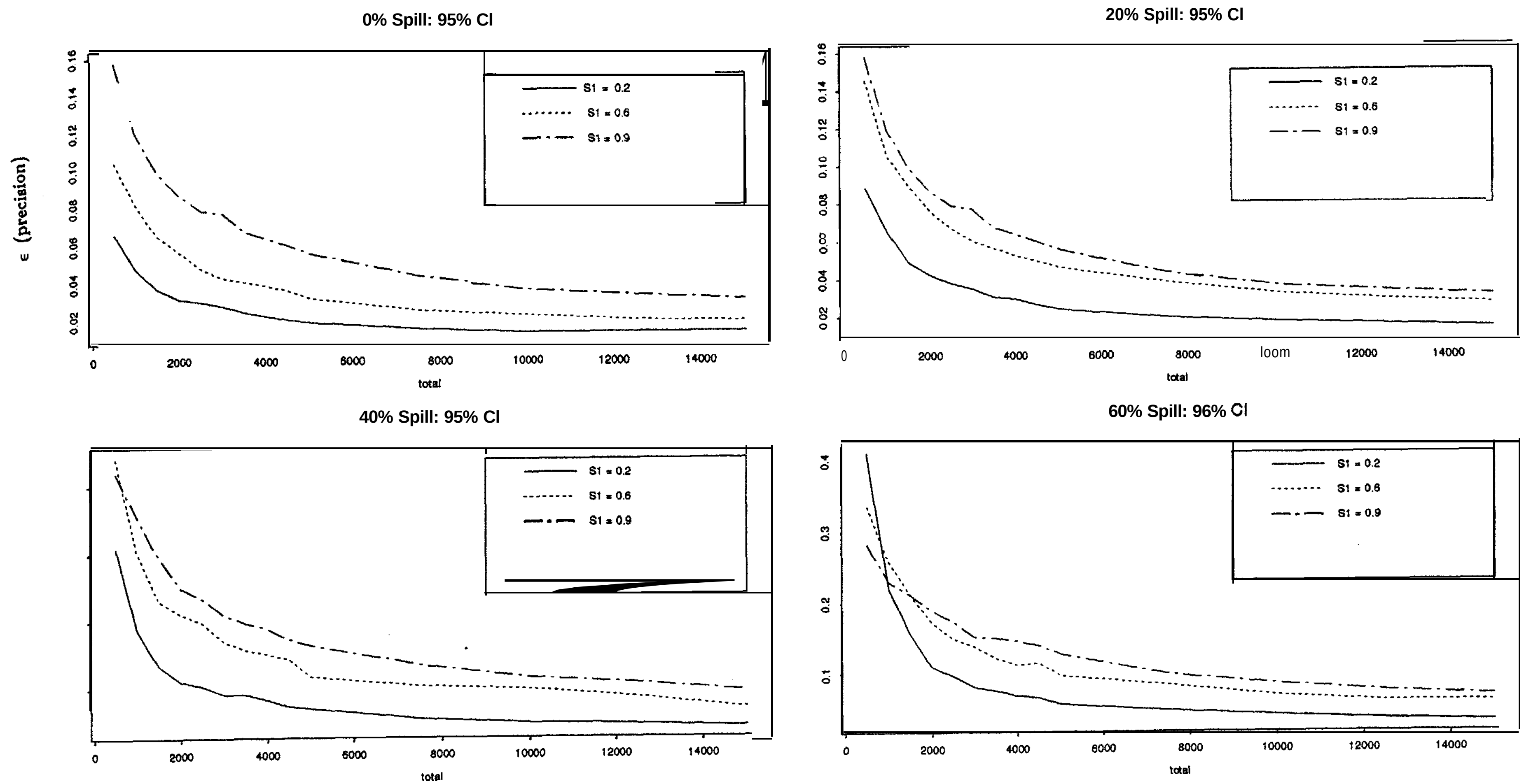


Figure **A6.** Plots of total sample size against $\epsilon = 1.96 * SE(S_1)$ in estimating survival based on a 90% CI at 0, 20, 40, and 60% spill. Precision isdefinedby $P(|\hat{S}_1 - S_1| > \epsilon) = 0.95.$

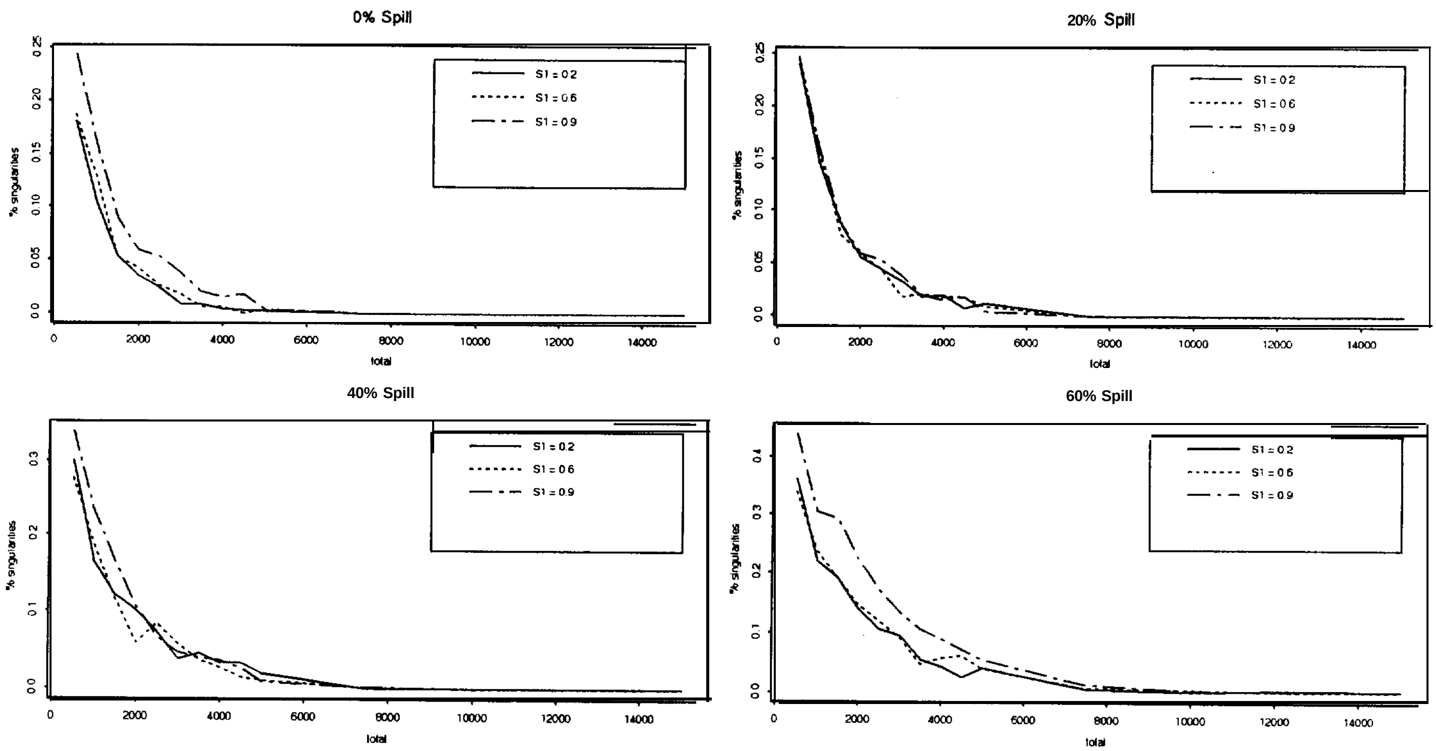


Figure A7. Plots of sample size against chances of collecting a data set where the variances could not be estimated due to singularity.

requires a set of secondary releases in the bypass system to estimate post-detection mortality, The set of secondary releases (i.e., R_C and R_T) is independent of the primary release (R_1) and may experience unique reach survival and detection probabilities.

Sample size calculations based on the modified release-recapture model indicate reasonable levels of precision with moderate sizes of PIT-tag fish releases. With a total sample size of $R. = 3{,}000$ fish, precision of

$$P(|0.6-\hat{S}_1| < 0.04) = 0.90$$

is anticipated under 0% spill conditions, and a precision of

$$P(|0.6-\hat{S}_1| < 0.12) = 0.90$$

under **60%** spill conditions. These levels of study performance are feasible with current PIT-tag facilities on the Snake River. Calculations were based on the operation of the decoder and slide-gate facilities at Lower Granite Dam and decoder facilities at Little Goose and McNary Dams. Fish transportation programs at Little Goose and McNary Dams could operate during the survival study with only PIT-tagged fish being diverted back in the river at Lower Granite Dam.

Results of this evaluation of release-recapture methods for survival studies indicate both the feasibility and reliability of PIT-tag estimators to estimate reach survival with existing facilities on the Snake River. Furthermore, sample size calculations suggest the likelihood of precise estimates of reach survival with moderate sample sizes of marked smolt.

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APPENDIX B

SINGLE RELEASE-RECAPTURE ESTIMATOR

Prepared by

John R. Skalski

A single release-recapture study for the Snake River could be conceptualized as a single release of R , PIT-tagged fish above the Lower Granite Dam with PIT-tag detections at Lower Granite, Little Goose, and McNary Dams. Fish detected at Lower Granite Dam are assumed to be released via operating slide gates and constituting a second **conceptual** release of R_2 PIT-tagged fish. Fish detected at Little Goose and McNary Dams are presumed to enter the transport program and are eliminated from subsequent detections downstream. The expected number of detections at PIT-tag facilities are summarized in Table **Bl** below.

Parameters used in the likelihood for the single release-recapture model are defined as follows:

R_1 = number of PIT-tagged fish released above Lower Granite Dam,

R_2 = number of PIT-tagged fish detected at Lower Granite Dam that are subsequently re-released,

S_1 = probability of survival of PIT-tagged fish from the initial release point through Lower Granite Dam,

S_2 = probability of survival of PIT-tagged fish from just below Lower Granite Dam to through Little Goose Dam,

S_3 = probability of survival of PIT-tagged fish from just below Little Goose Dam through McNary Dam,

p_1 = detection probability of a PIT-tagged fish at Lower Granite Dam,

p_2 = detection probability of a PIT-tagged fish at Little Goose Dam,

p_3 = detection probability of a PIT-tagged fish at McNary Dam.

As will be seen below, not all of these parameters are estimable with the available PIT-tag facilities on the Snake River.

Table B 1. Expected number of fish detections at PIT-tag facilities at Lower Granite, Little Goose, and McNary Dams.

Release Site	Numbers Released	Expected Numbers Detected at Dam		
		Lower Granite (2)	Little Goose (3)	McNary (4)
Upper Snake (1)	R_1	$R_1S_1p_1$	$R_1S_1(1-p_1)S_2p_2$	$R_1S_1(1-p_1)S_2(1-p_2)S_3p_3$
Lower Granite (2)	R_2		$R_2S_2p_2$	$R_2S_2(1-p_2)S_3p_3$

Date used in estimating reach survival are the numbers of detections at each dam from the initial release (R_1) above Lower Granite and the conceptual release of R_2 fish at Lower Granite from the fish detected at that dam. The five recapture counts illustrated in Table B2 constitute the basis of survival estimation.

Table B2. Number of detections at PIT-tag facilities at Lower Granite, Little Goose, and McNary Dam.

Release Site	Numbers Released	Numbers Detected at Dam			Totals
		Lower Granite (2)	Little Goose (3)	McNary (4)	
Upper Snake	R_1	m_{12}	m_{13}	m_{14}	r_1
Lower Granite	R_2		m_{23}	m_{24}	r_2

Parameter estimation will be accomplished using maximum likelihood estimate (MLE) techniques. The likelihood model for the proposed study can be written as follows:

$$
\begin{aligned}
L\left(\underset{\sim}{m}_1, \underset{\sim}{m}_2 \middle| R_1, R_2\right) = & \binom{R_1}{m_{12}, m_{13}, m_{14}} (S_1 p_1)^{m_{12}} (S_1(1-p_1)S_2 p_2)^{m_{13}} \\
& \cdot (S_1(1-p_1)S_2(1-p_2)S_3 p_3)^{m_{14}} \\
& \cdot (1 - S_1(p_1 + (1-p_1)S_2(p_2 + (1-p_2)S_3 p_3)))^{R_1 - r_1} \\
& \cdot \binom{R_2}{m_{23}, m_{24}} (S_2 p_2)^{m_{23}} (S_2(1-p_2)S_3 p_3)^{m_{24}} \\
& \cdot (1 - S_2(p_2 + (1-p_2)S_3 p_3))^{R_2 - r_2} \quad .
\end{aligned}
$$

However, all six parameters are not estimable in the likelihood as initially written. The likelihood model must be reparameterized with the following four composite parameters:

a. S_1,

b. p_1,

c. $\theta = S_2 p_2$,

d. $\gamma = S_2(1-p_2)S_3 p_3$.

The resulting likelihood model then becomes

$$L\left(\underset{\sim}{m}_1, \underset{\sim}{m}_2 \middle| R_1, R_2\right) = \binom{R_1}{m_{12}, m_{13}, m_{14}} (S_1 p_1)^{m_{12}} (S_1(1-p_1)\theta)^{m_{13}}$$

$$(S_1(1-p_1)\gamma)^{m_{14}} (1 - S_1(p_1 + (1-p_1)(\theta+\gamma)))^{R_1 - r_1}$$

$$\cdot \binom{R_2}{m_{23}, m_{24}} \theta^{m_{23}} \gamma^{m_{24}} (1-\theta-\gamma)^{R_2 - r_2} \quad .$$

Assumptions associated with the single release-recapture model are as follows:

1. The test fish are representative of the population about which one seeks mortality information.
2. Test conditions are representative of the conditions of interest.
3. The number of fish released is exactly known.
4. Marking is accurate, i.e., there is no post-release marking mortality and no misread marks.
5. Initial release is instantaneous and all detected fish are re-released immediately upon detection.
6. The fate of each individual fish is independent of the fates of all other **fish.**
7. If replicate studies are conducted, then the data from different releases are statistically independent.

8. Detected fish that are re-released have the same subsequent survival and capture rates as fish alive at the site that are not detected.

9. All fish in the study have the same survival and capture probabilities.

The maximum likelihood estimator for S_1 is

$$\hat{S}_1 = \frac{(r_1 - m_{12})R_2 + r_2 m_{12}}{R_1 r_2}$$

and for the other parameters,

$$\hat{p}_1 = \frac{m_{12}r_2}{m_{12}r_2 + (r_1 - m_{12})R_2} ,$$

$$\hat{\gamma} = \frac{r_2}{R_2}\left(\frac{m_{14} + m_{24}}{m_{14} + m_{24} + m_{13} + m_{23}}\right) ,$$

$$\hat{\theta} = \frac{r_2}{R_2}\left(\frac{m_{13} + m_{23}}{m_{14} + m_{24} + m_{13} + m_{23}}\right) .$$

The variance in the survival rate [i.e., $Var(\hat{S})$] to Lower Granite Dam will depend on five factors; these are:

1. Size of the initial release, R , , above Lower Granite Dam,

2. Size of the re-release of fish, R_2 , at Lower Granite Dam,

3. Fraction of fish from the initial release, p_1 , detected at Lower Granite Dam,

4. Fraction of fish from the re-release of fish at Lower Granite Dam that are detected, $(\theta + \gamma)$, at either Little Goose or McNary Dams,

5. Survival rates, S, , from the initial release site to Lower Granite Dam.

It should be noted that the size of the re-release of fish at Lower Granite Dam, R_2 , is itself a function of the survival rate to Lower Granite (i.e., S_1) and the detection rate there (i.e., p_1) such that $E(R_2) = R_1 S_1 p_1$.

APPENDIX C

COLLECTION EFFICIENCY (CE) - -

ABUNDANCE AND SURVIVAL ESTIMATOR

Prepared by

John R. Skalski

CE ABUNDANCE ESTIMATOR

Following a known release of N_0 PIT-tagged fish, survival can be estimated from the point of release to a specific recovery location by estimating the migrant abundance of the survivors at the recovery site. The general form of the survival estimator (S) to site i is written as follows:

$$\hat{S} = \frac{\hat{N}_i}{N_0}$$

N_0 = number of fish released at initial site,

$\hat{N}_i$ = estimate of migrant abundance at the ith recovery site.

The migrant abundance at site i is estimated by escalating the number of tag recoveries of the release group at the ith site on day $n_j(j=1,\ldots,D)$ by the reciprocal of the collection efficiency on day j. The estimate of migrant abundance is of the form

$$\hat{N}_i = \sum_{j-1}^{D} \frac{n_j}{\hat{p}j}$$

where

n_j = number of tag recoveries of the N_0 fish recovered at Lower Granite Dam on day $j(j=1,\ldots,D)$,

$\hat{p}_j$ = estimated collection efficiency at site i on day $j(j=1,\ldots,D)$.

In order to estimate daily recovery efficiency ($\hat{P}_j$) at site i, independent releases of $M_j (j = 1, \ldots, D)$ fish just above the recovery site would be conducted daily with numbers recaptured $m_j (j = 1, \ldots, D)$ recorded. The subsequent estimate of recovery efficiency is then $\hat{p}_j = m_j / M_j$ for $j = 1, \ldots, D$.

In conjunction with the daily estimates of recovery efficiency, the estimate of migrant abundance reaching site i is then expressed as

$$\hat{N}_i = \sum_{j=1}^{D} \frac{n_j M_j}{m_j} .$$

Hence, survival to site i is estimated as

$$\hat{S} = \frac{1}{N_0} \sum_{j=1}^{D} \frac{n_j M_j}{m_j} \quad (C1)$$

where

n_j = number of tag recoveries of the N_0 fish on day $j (j = 1, \ldots, D)$ at Lower Granite Dam,

M_j = number of PIT-tagged fish released just above Lower Granite Dam on day $j (j = 1, \ldots, D)$,

m_j = number of tag recoveries of the M_j fish released on day $j (j = 1, \ldots, D)$ above Lower Granite Dam.

Assumptions associated with estimating survival via the method of migrant abundance are as follows.

1. The M_j fish used daily in estimating daily recovery efficiency have the same capture probability as the initial release of N_0 PIT-tagged fish at site i on day j .

2. The daily releases of M_j fish to estimate recovery efficiency and subsequent **recoveries** of m_j fish are independent.

3. The M_j fish have independent and equal probabilities of recovery at site i .

4. There is no natural mortality or handling mortality among the M_j fish used to estimate recovery efficiency between their release site and point of recovery at site i .

The variance of the survival estimator (Cl) is derived as follows:

$$Var(\hat{S} \mid S) = Var\left(\frac{1}{N_0}\sum_{j=1}^{D}\frac{n_j M_j}{m_j}\right)$$

$$= \frac{1}{N_0^2}\left[\sum_{j=1}^{D} Var\left(\frac{n_j M_j}{m_j}\right) + 2\sum_{\substack{j=1 \\ j \neq j'}}^{D}\sum_{j'=1}^{D} Cov\left(\frac{n_j M_j}{m_j}, \frac{n_j' M_j'}{m_j'}\right)\right] \qquad (C2)$$

to evaluate this expression, note that:

$$Var\left(\frac{n_j M_j}{m_j}\right) \doteq Var(n_j)\left(\frac{M_j}{m_j}\right)^2_{\mid E(m_j)} + Var(m_j)\left(\frac{-n_j M_j}{m_j^2}\right)^2_{\mid E(n_j),\, E(m_j)}$$

$$= N_0 S p_j(1 - S p_j)\left(\frac{M_j}{M_j p_j}\right)^2$$

$$+ M_j p_j(1 - p_j)\left(\frac{-N_0 S p_j M_j}{M_j^2 p_j^2}\right)^2$$

$$= \frac{N_0 S(1 - S p_j)}{Pi} + \frac{M_j^3 N_0^2 S^2 p_j^3(1 - p_j)}{M_j^4 p_j^4}$$

$$= \frac{N_0 S(1 - S p_j)}{Pi} + \frac{N_0^2 S^2 (1 - p_j)}{M_j p_j}$$

$$Var\left(\frac{n_j M_j}{m_j}\right) = \frac{M_j N_0 S(1 - Sp_j) + N_0^2 S^2 (1 - p_j)}{M_j p_j} \tag{C3}$$

Similarly,

$$\begin{aligned} cov\left(\frac{n_j M_j}{m_j}, \frac{n_j', M_j'}{m_j'}\right) &\doteq Cov(n_j, n_j')\left(\frac{M_j}{m_j}\right)_{|E(m_j)}\left(\frac{M_j'}{m_j'}\right)_{|E(m_j')} \\ &= -N_0 S p_j S p_j'\left(\frac{M_j}{M_j p_j}\right)\left(\frac{M_j'}{M_j' p_j'}\right) \\ &= -N_0 S^2 \ . \end{aligned} \tag{C4}$$

Therefore, the variance of the migrant abundance estimator is found by substituting expressions (C3) and (C4) into **(C2),** where

$$\begin{aligned} Var(\hat{S} \mid S) &= \frac{1}{N_0^2}\left[\sum_{j=1}^{D}\left(\frac{M_j N_0 S(1 - Sp_j) + N_0^2 S(1 - p_j)}{M_j p_j}\right) + 2 \sum_{\substack{j=1 \\ j \neq j'}}^{D} \sum_{j'=1}^{D} (-N_0 S^2)\right] \\ &= \frac{1}{N_0^2}\left[\sum_{j=1}^{D}\left(\frac{M_j N_0 S(1 - Sp_j) + N_0^2 S(1 - p_j)}{M_j p_j}\right) + 2\binom{D}{2}(-N_0 S^2)\right] \end{aligned}$$

$$Var(\hat{S} \mid S) = \left[\sum_{j=1}^{D}\left(\frac{M_j S(1 - Sp_j) + N_0 S(1 - p_j)}{N_0 M_j p_j}\right)\right] - \frac{D(D-1)S^2}{N_0} \ .$$

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