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## **REVIEW OF LIVE CAPTURE SELECTIVE HARVEST METHODS STUDY<sup>1</sup>**

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**March 3, 2003**

### ***Background***

S. P. Cramer and Associates, Inc. was contracted by *Salmon for All* to review the experimental design and results of research in 2001 and 2002 by the Washington Department of Fish and Wildlife to evaluate live capture selective harvest methods in commercial fishery net gear. The objectives of this review are to 1) evaluate the technical scientific basis for study conclusions in order to assist fishery managers with the effective interpretation and application of results, 2) provide constructive suggestions to the researchers to bolster analyses, and 3) recommend improvements in future studies to decrease uncertainty.

Our review was based on a report of 2001 results (Vander Haegen et al. 2002), a February 13, 2003 comparison table of 2001 and 2002 and our own independent analysis of 2001 and 2002 data. We thank the Washington Department of Fish and Wildlife for providing electronic databases, tag recovery summaries, and especially copies of data sheets.

This review focuses on the strengths and weaknesses of the study approach with particular attention to a series of critical assumptions. Every study design is subject to assumptions. Mark-recapture study results are particularly sensitive to the effects of assumption violations (Ricker 1975, Seber 1982, Beamesderfer and Rieman 1988). Even small violations may prove significant where estimated differences between test and control fish are small as in the case of catch and release harvest mortality studies. However, even where assumptions are violated, the size and the direction of the effect can be identified to provide extremely useful information.

### ***Analysis of Assumptions***

Interpretation of study results relies on three fundamental assumptions:

1. Control fish tagged and released at Bonneville Dam are representative of fish caught and released from nets (i.e. same tagging dates, release locations, recovery distributions, tag recognition).
2. Observed differences between test and control fish are significantly different.
3. Study conditions are representative of commercial fisheries to which they are applied. (i.e. gear, year, and species effects)

We evaluated each of these assumptions in turn to the extent allowed by the available study data.

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<sup>1</sup> Includes minor revisions based on 3/3/03 TAC discussion.

**1A. Representative control: Same tagging dates?**

*Risk: Different dates of tag application risk tagging different components of the spring chinook run. Different components of the run are exposed to different mortality factors as well as destined for different areas where recovery sample rates may vary.*

- ✓ Dates of tag application were examined by comparing median tagging date of test and control groups.
- ✓ In 2001, the timing of tagging was similar between the combined 3.5” & 4.5” test group and the control group. Both groups were largely tagged on the back end of the run.
- ✓ In 2001, the 8” test group was generally tagged much earlier than the control group. The 8” test group tag timing generally matched the peak of the run.
- ✓ In 2002, test fish (4.5” nets) were tagged significantly earlier than control fish. Test fish were heavy to the front of the run. Control fish matched the run.
- ✓ A more appropriate test to ensure that mark and control groups are the same would be to consider travel times from gillnet release areas to Bonneville Dam but data on travel times of these fish is currently limited to a few Bonneville Trap recoveries of test fish.

*Assessment: Differences in tagging timing provide the potential for confounding test vs. control comparisons of 8” catch in 2001 and 4.5” catch in 2002.*

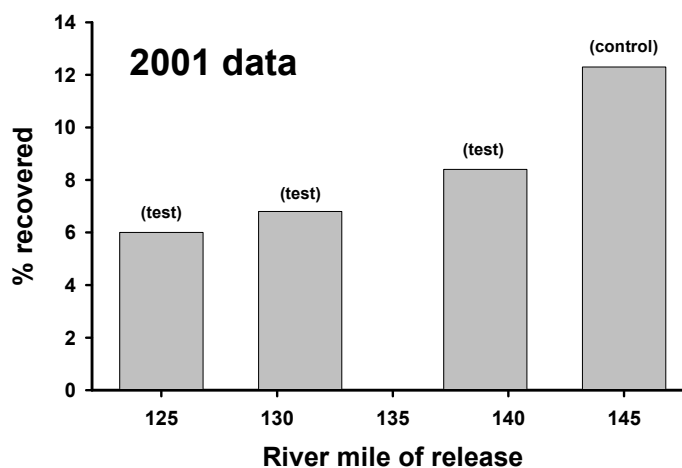
Table 1. Median passage and tagging dates						
	Run	Control	3.5” & 4.5”	Significant <sup>1</sup>	8”	Significant <sup>1</sup>
2001	~Apr 17	~May 1	~May 2	No	Apr 13	Yes
2002	~Apr 30	~Apr 28	~Apr 23	Yes	--	--

<sup>1</sup>Based on chi-square test of net vs. control release group numbers by biweekly intervals (p<0.05).

**1B. Representative control: Same release locations?**

*Risk Different release locations risk exposure of test and control fish to dissimilar mortality factors other than delayed mortality from net capture.*

- ✓ Net mortality will be overestimated relative to the control if net fish are exposed to extra mortality factors from sport harvest and dam passage.
- ✓ Test groups were released 5-20 miles below the dam while controls were released in the fish ladder.
- ✓ Release location effects were reported in the 2001 study report (graph at right based on Vander Haegen et al. 2002 Figure 3).
- ✓ Elimination of lower net sites in 2002 addressed part of this problem.
- ✓ Significant sport fisheries occurred between test and control fish release sites in both 2001 and 2002.
- ✓ Dam conversion losses are significant. Past TAC estimates based on dam counts have averaged 10-12% for spring chinook and in high spill years were substantially greater.



*Assessment: Direct comparisons of test and control recovery rates substantially overestimate delayed test fish mortality where sport harvest and dam conversion mortality of test fish is not considered.*

### **1C. Representative control: Same recovery distribution?**

*Risk: Different run components are exposed to different mortality factors as well as subject to different recovery sampling rates that can confound comparisons of test and control fish.*

- ✓ Tag recovery distributions of test and control groups were compared by region and recovery type.
- ✓ No significant differences between test and control groups in recovery area distributions or recovery method distributions were apparent in either year. More detailed examinations of recovery distributions are appropriate among subareas within each basin, different recovery methods, and between hatchery and wild origin fish.
- ✓ Snake basin recoveries comprised the majority of 2001 recoveries whereas Zone 6 recoveries comprised the majority in 2002.
- ✓ Hatchery recoveries comprised a greater proportion of the total in 2002 than 2001.

*Assessment: This result suggests that test and control groups evenly represented the upriver run in both years despite differences in tagging date distributions. It is unknown if changes between years in the distribution of recoveries among areas and hatchery/non hatchery sources may have contributed to differences in estimated net mortalities among years.*

<b>Table 2. Distribution of Recaptures by Area (%)</b>						
		N	Zone 6	Upper Col.	Snake	Significant <sup>1</sup>
2001	3.5" & 4.5"	52	33	11	57	No
	control	147	35	10	54	
2002	4.5"	181	52	15	33	No
	control	204	45	16	39	

<sup>1</sup>Based on chi-square test of net vs. control release group numbers ( $p < 0.05$ ).

<b>Table 3. Recapture Methods (% by method)</b>						
		N	Non-Hatchery	Hatchery		Significant <sup>1</sup>
2001	All nets	111	72	28		No
	control	148	73	27		
2002	All nets	383	57	43		No
	control	211	59	40		

<sup>1</sup>Based on chi-square test of net vs. control release group numbers ( $p < 0.05$ ).

### **1D. Representative control: Same tag recognition?**

*Risk: Different tag recognition rates of test and control fish would confound estimates.*

- ✓ Control fish were tagged with red tags and test fish with white or yellow.
- ✓ Red tags were apparently undercounted at ladder windows in 2001 relative to white or yellow tags. (Observations only not counted as recoveries)
- ✓ Current test v. control estimates assume that red, white, and yellow tags are equally visible.

*Assessment: This assumption is likely to be met where hatchery sample rates are near 100% and each carcass is closely inspected during spawning ground surveys. Use of the white and yellow tags on the test fish will provide conservative estimates of mortality if red tags are less visible.*

## **2. Test and control differences significant?**

*Risk: Chance results may be misinterpreted where observed differences between test and control groups are not statistically significant.*

- ✓ Chi-square tests were used to identify statistically significant net effects (vs. controls). A non-significant result indicates that observed differences in recovery rates could have occurred by chance or that sample sizes are not sufficient to distinguish actual differences.
- ✓ In 2001, tangle net gear did not result in a statistically significant difference in recapture rates. The observed difference was equivalent to an 11.4% delayed mortality of nets.
- ✓ In 2001, 8" gear produced a statistically significant difference in recapture rates equivalent to a 44.1% delayed mortality rate.
- ✓ In 2002, recovery rates were significantly less from tangle nets than for the control (equivalent to a 26.1% delayed mortality rate).
- ✓ Differences in 2001 and 2002 tangle nets are significant.
- ✓ Greater mortality rate estimates in 2002 than 2001 seem counterintuitive to expected effects of changes in study design. One major change was to concentrate test fishing closer to the dam to minimize chances for confounding mortality with the extra travel distance. However, this effect should have been to decrease rather than increase the 2002 mortality estimate relative to 2001. The other change was to move tagging into a more representative portion of the run timing (2001 was late).

*Assessment: It remains unclear whether substantially different results in 2001 and 2002 mortality estimates result from differences in study implementation or year effects associated with migration conditions or run composition.*

<b>Table 4. Test v. Control Recoveries</b>						
		Release No.	Recovery No.	Rate	Difference	Significant <sup>1</sup>
2001	control	1206	148	12.3%		
	3.5" & 4.5"	534 <sup>2</sup>	58	10.9%	11.4%	No
	8"	815	56	6.9%	44.1%	Yes
2002	control	1037	211	20.3%		
	4.5"	1249 <sup>3</sup>	187 <sup>4</sup>	15.0%	26.1%	Yes

<sup>1</sup>Based on chi-square test of net vs. control release group numbers ( $p < 0.05$ ).

<sup>2</sup>Differs from 512 reported by WDFW.

<sup>3</sup>Differs from 1218 reported by WDFW.

<sup>4</sup>Differs from 169 reported by WDFW.

<sup>5</sup>Difference assuming no Bonneville Dam passage loss of test fish. Revised estimates assuming 10% conversion loss would be 2.0%, 37.8%, and 18.2%, respectively.

**3. Representative results: gear effects?**

*Risk: Actual fishery impacts may not resemble inferences from study results if study gear is not representative of fleet usage.*

- ✓ There can be significant variation in construction and performance of gillnets even where mesh sizes are similar.
- ✓ Several fishers have suggested that the 8" mesh nets used during the 2001 study are not representative of those in current usage.

*Assessment: Data on fleet representation of different net types is not currently available to evaluate this risk.*

**3. Representative results: year effect?**

*Risk: Results in any given year may be atypical if specific to unique migration or run composition differences.*

- ✓ Comparisons of migration conditions (temperature, flow, dam operations), run timing (early, normal, late), and run composition (age, hatchery fraction, and destination) might help explain observed differences among years.

*Assessment: These analyses were beyond the scope of this limited review but might be appropriate as part of the current or future studies.*

**3. Representative results: species effects?**

*Risk: Delayed mortality rates of spring chinook may over or underestimate fishery effects on other species.*

- ✓ The key question is whether chinook results are appropriate for application to steelhead where similar data are not available.
- ✓ Vander Haegen et al. (2002) suggested that post-release mortality was expected to vary between species.
- ✓ Behavior, physiology, life history, and migration behavior might all contribute to species differences.

*Assessment: This question is beyond the scope of the current analysis but might be appropriate for inclusion in future studies.*

### ***Conclusions***

1. The WDFW study represents a credible investigation into the difficult question of delayed mortality. Our review identified several questions that can be addressed by additional analysis of the current data and by future studies. However, we note the inherent difficulty in establishing an appropriate control for mortality studies and the applicability of results where assumptions can be carefully evaluated. These types of studies are extremely difficult, no study can meet all assumptions, but each additional study can help reduce uncertainty.
2. Differences in 2001 and 2002 remain difficult to reconcile. We were unable to relate these differences to violations in assumptions that we could test with the available study data. We were also unable to identify an appropriate rationale for discounting results of either year without identifying the cause of the observed difference.
3. Corrections for different release locations of test and control fish should be considered in any application of study results. Net-caught fish were released downstream of Bonneville Dam and were likely subject to significant dam passage and fishery mortality that was not faced by the control fish that were released into the fish ladder. Assumed conversion rates should be similar to those used for other run reconstruction purposes.
4. Inferences from results of 8" net studies in 2001 should be carefully considered because of questions of disparate run timing of test and control fish and whether the representative 8" gear was used in the experiment.
5. Study results provide no basis for evaluating whether results can be extended to other species such as steelhead.

### ***Recommendations***

1. Given the uncertainty in why 2001 and 2002 are so different, it may be more appropriate to use a weight of evidence application of results. (i.e. Don't hang everything on one study year or even one study. Consider results of studies from other areas if available.)
2. Additional study years are needed to characterize apparent among-year differences in delayed mortality rates. Plans for 2003 to add steelhead, release test and control groups at the same location and PIT tag fish for recoveries at dams are appropriate.
3. Future studies should incorporate directed experiments to evaluate assumptions. For instance, radiotelemetry of gillnet fish might prove helpful. Test netting and tagging of fish immediately upstream of Bonneville Dam would also be an effective means of eliminating the confounding effects of dam passage of test fish. Surveys of the fleet representation of different gears should be included.
4. Future studies should also consider matching test and control fish releases to the early timing of the spring gillnet fishery to ensure representative results.
5. Study reports should identify key assumptions and incorporate analyses of each assumption to the extent possible with the available data.
6. Conduct sensitivity analysis of estimates to risks associated with different biases.
7. Include stratified estimates to explore potential causes for differences in 2001 and 2002 results. For instance, recovery rates might be stratified by area to explore the potential for confounding area effects. Recovery rates of early and late season tag groups might be compared to explore the potential for confounding season effects.

8. Upon completion, research results should be prepared and submitted for peer review in the technical scientific literature (e.g. *North American Journal of Fishery Management*).

***References***

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