# IMPROVING MONITORING METHODS FOR ESTIMATION OF NON-RETAINED SALMONIDS ENCOUNTERED IN COLUMBIA RIVER SPRING CHINOOK FISHERIES

**FINAL REPORT**


*Oregon Department of Fish and Wildlife*
*Ocean Salmon and Columbia River Program*
*Columbia River Management Program*

17330 SE Evelyn Street
Clackamas, Oregon 97015



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**J. Chris Kern**
**Eric Tinus**
**Geoffrey Whisler**
**Tom Neill**


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# INTRODUCTION

Recreational and commercial fisheries in the Columbia River harvest hatchery and wild salmonids. Hatchery fish are primarily provided for harvest as mitigation for significant losses of natural production throughout the Columbia Basin. Wild salmon stocks harvested by these fisheries include healthy runs of summer and fall Chinook. Steelhead are incidentally caught and released in commercial fisheries downstream of Bonneville Dam, but retention of steelhead in these commercial fisheries is prohibited. Recreational fisheries also harvest hatchery steelhead, but are required to release wild steelhead. Detailed descriptions of Columbia River fisheries may be found in a series of annual reports produced jointly by the Oregon and Washington Departments of Fish and Wildlife (ODFW/WDFW 2011).

Columbia River fisheries generate millions of dollars annually and are highly scrutinized in light of intensive efforts to recover 15 stocks and species listed as threatened or endangered under the federal Endangered Species Act (ESA). In addition to in-river fisheries, production of salmon from the Columbia River contributes substantially to large ocean fisheries from California to Alaska.

Two primary factors drive the management of salmon fisheries in the Columbia River. First and foremost is the conservation and recovery of ESA-listed stocks and the protection of healthy wild stocks in the basin. Thirteen stocks of salmon and steelhead in the Columbia River basin are listed under the ESA, and all of these migrate through fisheries in the lower Columbia River below Bonneville Dam. Fisheries impacting listed stocks are reviewed in consultation with NOAA Fisheries to determine if they are consistent with conservation and recovery goals under the ESA. Healthy wild stocks are managed to provide necessary escapement to perpetuate the runs.

In addition, fisheries are managed to harvest hatchery-produced salmon and steelhead provided by various mitigation agreements. These agreements are primarily intended to offset significant losses of naturally-produced fish caused by hydro-electric development throughout the Columbia Basin.

Management of Columbia River salmon fisheries is a balancing act between keeping harvest-related mortality of ESA-listed stocks at or below levels needed to promote conservation and recovery while providing reasonable opportunities to harvest abundant hatchery stocks and healthy wild stocks. Management is further complicated by uncertainties in estimates of handling, harvest, and total mortalities in these fisheries. Fishery managers account for this uncertainty by adopting conservative fisheries plans – an approach that may result in lost opportunities and under-utilization of healthy fish stocks. Greater certainty in estimates of handling and harvest in fisheries would allow for more efficient utilization of the fishery resources in the Columbia River.

To gauge whether management strategies are meeting conservation and fishery goals, total fishery mortality is estimated from commercial landings and from angler creel surveys. As in any fishery that includes substantial encounters of non-retained fish, estimating the total catch of fish not retained by fishers is challenging, and requires different methodologies than those used to estimate retained catch.

For spring commercial mark-selective salmon fisheries, the Oregon Department of Fish and Wildlife (ODFW) and Washington Department of Fish and Wildlife (WDFW) conduct onboard observations of a sample of commercial fishers to monitor catch of non-retained fish (steelhead and unmarked spring Chinook). For mark-selective recreational fisheries, ODFW and WDFW estimate the number of non-retained fish from voluntary responses by anglers interviewed during creel surveys. Mortalities of non-retained fish in both fisheries are estimated by multiplying estimated total encounters by fishery-specific post-release mortality rates.

Substantial background information on current Columbia River fisheries is available in a series of annual reports produced by the Oregon and Washington Departments of Fish and Wildlife (ODFW/WDFW 2011). Information on fisheries prior to recent years are available in Columbia River status reports (ODFW/WDFW 2002) produced annually or bi-annually through 2002. Readers are encouraged to review these documents for full discussions of Columbia River fisheries.

## OBJECTIVES

Objective 1. Commercial fisheries

1a. Determine if encounter rates of steelhead and marked and unmarked spring Chinook caught by commercial fishers are independent of one another.

1b. Estimate the relative encounter rates of steelhead and unmarked spring Chinook in lower Columbia River commercial fisheries through direct observations of catch independent of port or dockside sampling.

1c. Identify the best method(s) for estimating and indexing encounter rates of steelhead and unmarked spring Chinook in this fishery.

1d. Recommend a suitable design for long-term monitoring of encounter rates in this fishery.

Objective 2. Recreational fisheries

2a. Compare observed encounter rates of non-retained fish in recreational fisheries in the lower Columbia River with rates reported by anglers during statistical creel surveys to verify whether angler-reported encounter rates of non-retained fish in recreational fisheries accurately represent true encounter rates.

2b. Identify alternative methods to measure angler encounter rates of non-retained fish and identify the best method(s) for estimating encounter rates.

2c. Recommend a suitable design for long-term monitoring of encounter rates in these fisheries.

## STUDY AREA

Non-Treaty commercial fisheries in the Columbia River are limited to the area from Buoy 10, at the river's mouth, upstream to the fishing boundary at Beacon Rock, approximately four miles downstream of Bonneville Dam (RM 146). For commercial fisheries, this area is subdivided into five geographical zones for purposes of monitoring catch in the fishery (Figure 1). In addition to fisheries in the mainstem Columbia River, recreational and commercial fisheries also occur in off-channel sites, referred to as Select Areas. These areas include Youngs Bay, Deep River, Blind Slough/Knappa Slough, and Tongue Point/South Channel. Aside from these Select Areas, non-Treaty commercial fisheries are prohibited in tributaries of the Columbia.

Recreational fisheries occur from the mouth of the Columbia River at Buoy 10 upstream and are also conducted in most tributaries of the Columbia. The focus of this project is the mainstem recreational fishery occurring from the mouth upstream to Bonneville Dam. This area is slightly larger than the non-Treaty commercial fishing area, and is stratified into ten sections for purposes of estimating catch and effort in a statistical creel survey (Figure 2).

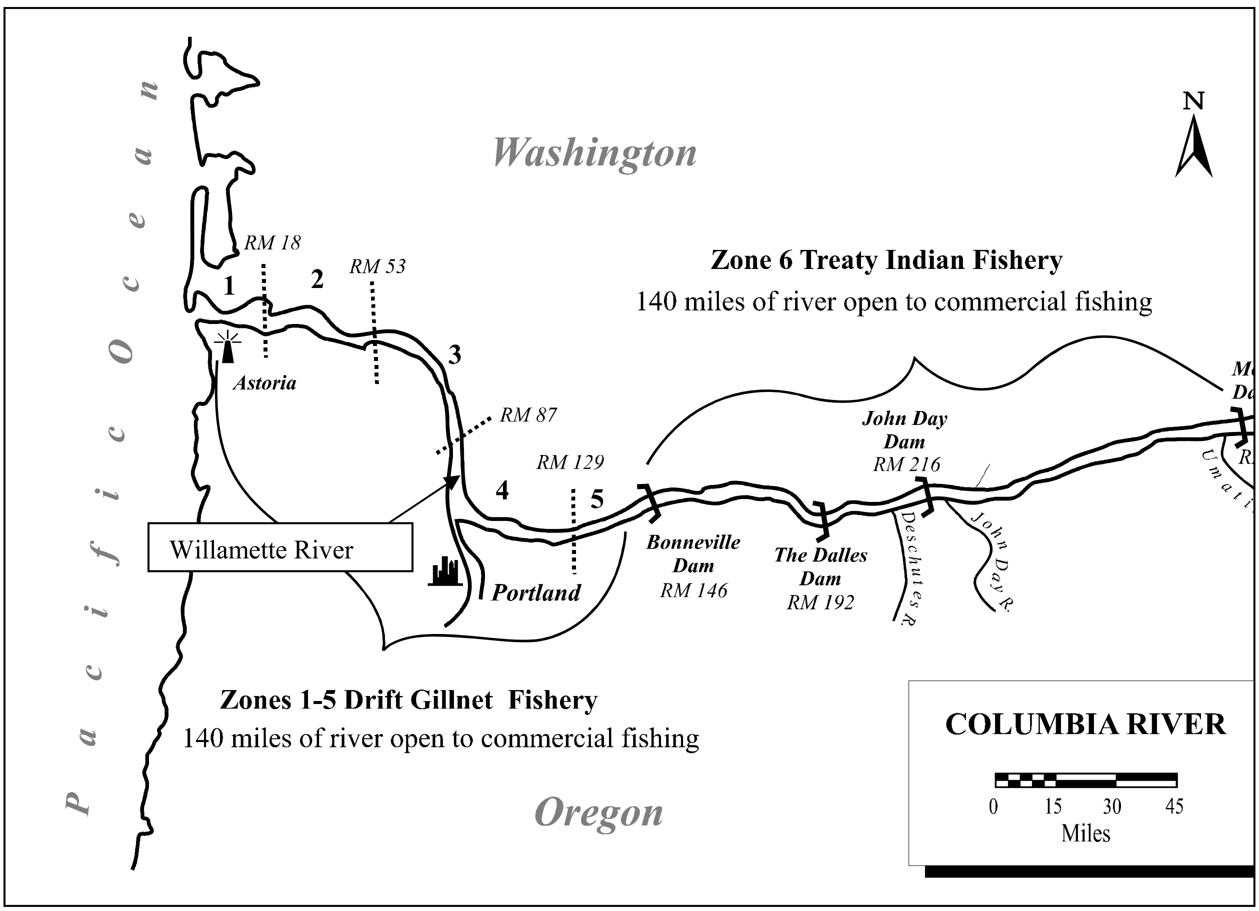


Figure 1. Columbia River commercial fishing zones.

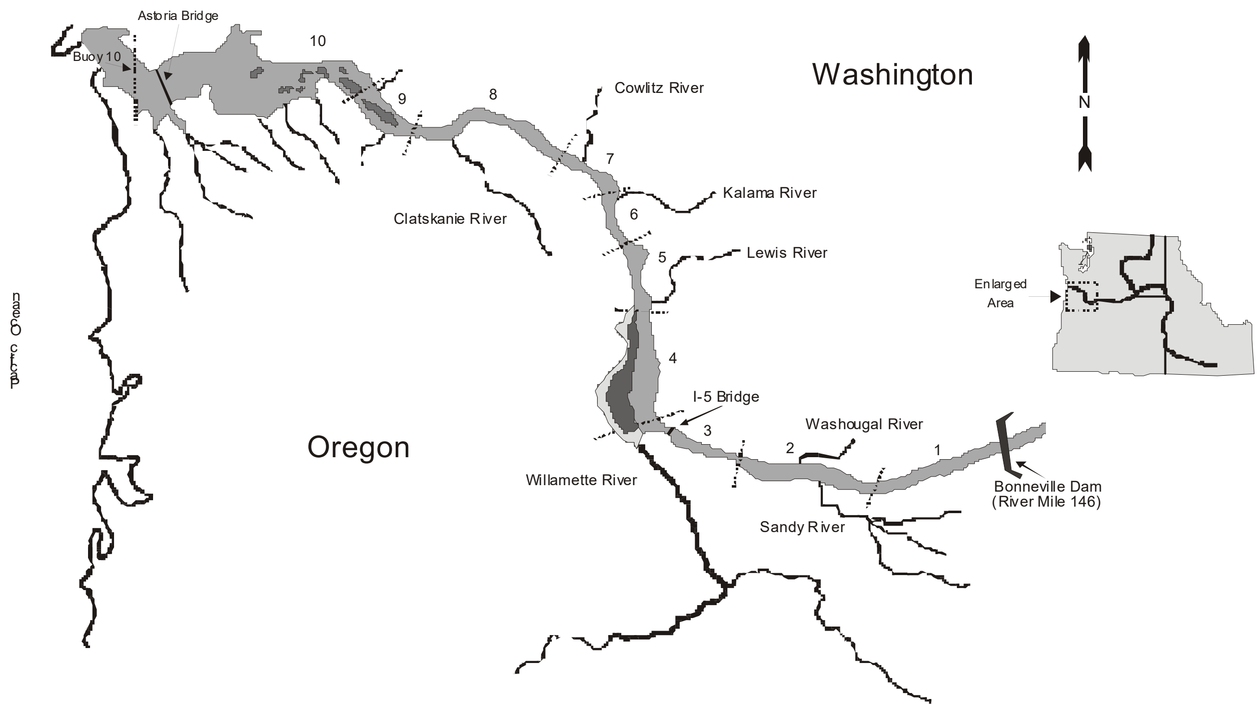


Figure 2. Lower Columbia River recreational fishing sections.

# 1. COMMERCIAL FISHERIES

## BACKGROUND

To reduce catch of upriver spring Chinook, no commercial salmon fishing was allowed upstream of Kelley Point at the Willamette River mouth during spring salmon seasons from 1975-2007. A gillnet minimum mesh size restriction of 7¼-inches (stretched measure) was enacted in 1970 to reduce steelhead handle. Subsequent to the prohibition of sales of steelhead in 1975, the minimum mesh size was increased to 8-inches to further reduce steelhead handle. This mesh size remained in effect until the introduction of small mesh "tanglenets" and live-capture techniques in 2002. The adoption of the Willamette River Spring Chinook Fisheries Management and Evaluation Plan (FMEP) in 2001 required the release of unmarked spring Chinook in commercial and recreational freshwater fisheries. The first spring Chinook mark-selective commercial fishery occurred in 2001. This live-capture fishery consisted of a permit fishery with participation limited to 20 vessels.

The first full fleet live-capture commercial fishery took place in 2002. The fishery was limited to, and remains restricted to, commercial fishers who held appropriate licenses and gear, and had attended a state-sponsored workshop concerning live-capture techniques. The 2002 fishery regulations included a 5½-inch maximum mesh size restriction, 150-fathom (900 feet) maximum net length, soak times not to exceed 45 minutes, use of recovery boxes on lethargic or bleeding fish, and allowed sales of sturgeon and adipose fin-clipped Chinook. The 2003 winter salmon fishery incorporated many of the general fishery regulations adopted in 2002 except gear regulations were modified in response to the high steelhead handle observed in 2002. Large mesh gillnets (8-inch minimum) were required during the early part of the season to minimize steelhead handle, and the maximum mesh size for tanglenets was reduced from 5½ inches to 4¼ inches to improve steelhead survival by minimizing the frequency of gill-capture for steelhead. The voluntary use of nets fitted with steelhead exclusion panels was also initiated in 2003. Beginning in 2004, test fishing was implemented as a tool to help determine the optimum time for fishing periods based on Chinook and steelhead catch rates.

The fishery continues to utilize two distinct net types, large-mesh gillnet (8" stretched measure) and tanglenet (4¼" stretched measure). The tanglenet was developed to improve survival of released salmonids. In addition to mesh size, tanglenets are generally "slackened" and "hung in", meaning that the net is built with a large amount of slack running both vertically and horizontally along the net. This creates a looser net that is more apt to entangle or ensnare fish by the teeth or fins, as opposed to capturing fish around the gills. As a result, tangle-nets are estimated to have a 14.7% post-release mortality on Chinook (18.5% on steelhead), as opposed to an estimated 40% post-release mortality for Chinook caught in large-mesh nets (30% for steelhead; ODFW/WDFW 2011).

Large mesh nets are generally used at times or in areas where steelhead abundance is relatively high in order to reduce encounters of steelhead. Tanglenets are generally used at times or in areas when steelhead abundances are lower, due to the higher likelihood of capture for steelhead in these nets. The use of tanglenets when possible allows the commercial fishery to access more harvestable Chinook per ESA-impact used, due to the lower post-release mortality rate for this gear.

Area restrictions are commonly used to control the stock composition of Chinook catch. For example, in most years, Chinook fishing has been restricted to downstream of the mouth of the Willamette River (RM 101) in order to reduce the proportion of upriver stock spring Chinook in the catch, by focusing fishing effort in the lower river where lower river stocks are also present alongside upriver stocks. In some years, this situation has been reversed, and fishing has been conducted only upstream of the Willamette River in order to reduce catch of weak returns of Willamette Chinook.

Fishers are required to attend training on live-capture techniques, and must adhere to regulations requiring the use of specialized recovery boxes and shortened drift times, in order to maximize survival of released fish.

Since 2004 test fishing has been conducted prior to setting any spring Chinook commercial fishery from March onwards. Early in the development of the live-capture fishery, this was found to be necessary to allow managers to decide whether abundance of steelhead is low enough, mark rate of Chinook is high enough, and stock composition of Chinook is appropriate for setting a fishery.

Onboard monitoring is employed to determine the encounter rates of non-target steelhead and unmarked spring Chinook salmon that are released in the commercial fishery. The spring season fisheries typically occur from mid-February through late-March, but can also occur in April–June. Monitoring crews composed of ODFW and WDFW staff observe the catch via a random sample of commercial fishers, either onboard the participant's vessel, or from a nearby agency vessel. Four boat crews, with two to four observers and one boat pilot on each vessel are typically deployed during each fishery. At times, observers are placed on a vessel at the beginning of a fishery and remain on the vessel until the fishery closes. Observation effort is deployed proportionally to expected catch and effort based on information collected in prior similar fisheries.

Fisheries are typically short, and each spring fishing season is composed of one or more open fishing periods of less than 24 hours in length each, typically 4-12 hours. Within each fishing period, up to 200 or more fishers may participate. Level of participation varies according to which areas are open, time of year, duration of the fishery, indications of fish abundance (such as large landings in test fisheries or prior periods) and at times, market price. The number of spring salmon fisheries conducted annually is variable, and has ranged from 2 to 15 from 2002-2010.

During an open period, an individual boat will make multiple fishing efforts, or "drifts". During spring salmon fisheries, each drift is limited by regulation to no more than 45 minutes in duration. Drift time is measured as the total duration from deployment of the first portion of the net to full retrieval of the net. The number of drifts that can be made per period is unregulated.

In most cases, fishers concentrate their efforts on familiar areas in specific sections of the river, and over the course of time, certain areas have developed "drift rights" whereby an individual fisher is given priority to a fishing area by other fishers, due to their level of historic participation and use of the area. Depending on the fishers' preferred fishing area and which areas are open, a fisher may choose to fish only a portion of the open period, in order to ensure that their fishing effort coincides with the most productive portion of the tidal cycle in that area.

Given the structure of spring salmon fisheries, the sampling units for observation of commercial fishing activities will be referred to in this report as follows:

- "Drift" is a single fishing effort consisting of the deployment and retrieval of gear in a specific fishing location by a single fishing boat. The drift is the basic sampling unit sampled by the current observation program.
- "Trip" is a series of drifts within a specific open fishing period by a single boat, although the boat may fish in multiple areas within a fishery. Current observations may include all or a portion of a given trip. Individual landings (deliveries) of fish on commercial fish landing tickets are the most accurate available estimate of the total number of trips made in a fishery.
- "Opener" or "Fishery" is the sum of all trips made by all fishers participating in a given fishing period.
- "Season" is the sum of all open periods for a given species and/or run type conducted during a specific timeframe of the year.

Following each fishing period, commercial fish landings are reported to the states of Oregon or Washington on fish landing tickets. It is important to note that fishers are not required to report their fishing effort in a given period. This limits the use of effort-based expansions for estimating non-retained catch given the current available data. Currently, the only available record of fleet-wide fishing effort is the number of individual deliveries, that are typically composed of several drifts. Also, the number of deliveries only provides an estimate of successful trips, i.e. those with >0 landed catch. Fishers are required to submit all catches of kept fish on a fish ticket, including any fish that may be retained for personal use. Thus, for our purposes, landings are taken to represent a known quantity.

Portions of landed fish are sampled for biological data and coded-wire tags (CWTs) at buying stations and/or boat ramps and these data are combined with landing reports to determine total landings by species and stock. Steelhead and unmarked Chinook may not be retained and are therefore not included in landed catch.

Estimated catch of steelhead and unmarked spring Chinook in each fishing opener has typically been derived using a ratio estimator (Cochran 1977) for each species. Using this estimator, the ratio of observed released fish to observed kept fish is multiplied by the total landings of kept fish to estimate the total number of released fish encountered in the fishery.

In the ratio estimator, the number of landed Chinook is treated as the auxiliary variable and is used to estimate the total catch of steelhead and unmarked Chinook using the formula:

$$\hat{Y}_R = \frac{y}{x} X \quad \text{eq 1.1}$$

where $\hat{Y}_R$ is the estimated total catch of steelhead (or unmarked Chinook) $y$ is the total observed and released catch of steelhead (or unmarked Chinook) in all drifts sampled, $x$ is the total observed catch of marked Chinook retained in all drifts sampled, and $X$ is the total landed Chinook catch for the fishery opener. This estimator can be used for pooled estimates, or can be used as a stratified estimator.

This estimator will have a variance:

$$v(\hat{Y}_R) = \frac{N(N-n)}{n(n-1)}\left(\sum y_i^2 + \hat{R}^2 \sum x_i^2 - 2\hat{R}\sum y_i x_i\right) \quad \text{eq 1.2}$$

where $N$ is the total number of landed Chinook, $n$ is the number of landed Chinook observed, $\hat{R}$ is the ratio $\frac{y}{x}$, and $y_i$ and $x_i$ are the number of observed steelhead (or unmarked Chinook) and observed kept Chinook in observed drift $i$. The ratio-based estimate is expected to be more precise than effort-based estimates in cases where correlations between the variables are strong (Cochran 1977). However, strong correlation does not necessarily imply interdependence of the variables. For instance, two variables could be correlated because they co-vary according to their relationships with other variables. In the context of the current study, correlations between variables may be primarily due to the relationships of each variable to factors such as total fishing effort.

Historically, the number of wild steelhead encountered has been estimated by applying the percentage of observed steelhead that was wild-origin to the total estimated encounters of steelhead from the ratio estimator. For this study, in order to simplify calculation of variances for wild steelhead catch estimates, we used only wild-origin steelhead observed in calculating the ratio-based estimates rather than estimating total steelhead catch and then adjusting the total for the estimated proportion of wild fish. Some observations categorized steelhead as “unknown” with regard to hatchery/wild origin. This was most often a case of fish removed from the net quickly (as is encouraged) before they could be positively identified. There were also occasions where observations were conducted from a nearby boat and fish could not be examined. In these instances, we apportioned unknown origin fish into hatchery/wild categories according to the ratio of hatchery and wild fish in all known samples by fishery and year.

Given the availability of appropriate data, an alternative estimator to the ratio-based methodology would be an effort-based simple expansion (Cochran 1977) such as:

$$\hat{Y} = N\bar{y} \quad \text{eq 2.1}$$

Where $\hat{Y}$ is the estimated catch of steelhead (or unmarked Chinook), N is the total fishing effort (drifts), and $\bar{y}$ is the mean catch of steelhead (or unmarked Chinook) per fishing effort (drift).

This estimate will have a variance:

$$v(\hat{Y}) = N^2\left(\frac{N-n}{N}\right)\left[\frac{\sum y_i^2 - \frac{\left(\sum y_i\right)^2}{n}}{n(n-1)}\right] \quad \text{eq 2.2}$$

The effort-based expansion requires an accurate estimate of total number of fishing efforts $N$, in this case number of drifts, that is currently unavailable. Estimation of fishing effort using the mean catch of kept Chinook per fishing drift sampled and total landings of Chinook simply reduces the effort-based estimate to the direct ratio estimate. Using current data available, the only effort-based expansion that would be possible would be based on deliveries as the base unit of sampling. To date, the spring commercial fishery observation program has focused on sampling drifts and little data from complete trips, that would be comparable to deliveries as a measure of effort, are available. A further complication of using deliveries for expansion by effort is the likelihood that some fishers may handle steelhead or unmarked Chinook without making a delivery of kept Chinook. Under the ratio estimate, these fishers would be expected to be sampled and to contribute to the overall ratio of kept-to-released fish. However, with deliveries as a base unit of effort, further efforts would be needed to ensure that fishers that landed no Chinook were adequately sampled and accounted for.

As previously noted, current methods rely on total landings of kept Chinook as the auxiliary variable for expansion of sampling data in estimating numbers of non-retained steelhead and unmarked Chinook. Reliable estimation of non-retained catch under this scheme requires: 1) accurate knowledge of total Chinook landings and kept-to-released ratios for the fleet at large and 2) consistent relationships between released and retained fish numbers within a fishery. Under this estimator, there are several sources of potential bias.

One source of bias would be non-representatively sampling the auxiliary or expanded variables. For instance, if the proportion of sampling effort applied by zone was significantly different than the fishing effort being sampled, a bias may be created. However, it is important to note that in order for this bias to result, some other factor, such as distribution of kept Chinook, unmarked Chinook, or steelhead must vary among these areas as well. If fish distribution and abundances in a single zone were adequately representative of the entire fishing area, an unbiased estimate could be generated from sampling in only that zone. Potential bias can also be avoided under a stratified random sampling design that is stratified by fishing zone, which is how the current program is designed.

Similarly, a bias could be produced by non-representatively sampling the fishers themselves. If fishers with a disproportionately high or low catch of any of the key species were sampled at disproportionate rates, a bias may result. However, in order for bias to occur, those fishers would also have to demonstrate different catch ratios of these species than the fleet at large. If the ratios of catch for a given set of fishers are representative of the ratios for the fleet at large, an unbiased estimate could be generated from sampling only those fishers. Potential bias can also be avoided under a stratified sampling design that stratifies samples by some category of fisher performance.

There are known differences in fish distributions by area, thus representative sampling by area is necessary. Historically, this has been addressed by attempting to allocate sampling effort per zone proportionally to the expected catch per zone. However, the question of whether species catch ratios differ substantially by fisher, or whether fishers are sampled representatively to their catch, has not yet been addressed. Because of this uncertainty, past methods of sampling have focused on ensuring a random selection of fishers and areas for sampling using stratified random sampling design where zones represent the strata.

## METHODS

Commercial fishery observations partially funded by this grant were conducted in the spring of 2010. We employed the same observation strategy used in recent years. In coordination with efforts by WDFW, we deployed agency vessels with observers to board and observe commercial vessels. We also conducted some ride along observations, where observers stayed on the vessel for the entire fishing period. For 2010, the commercial fleet was restricted to the area of the river downstream of the I-205 Bridge (RM 113) during spring fisheries. This area is very similar to the area used in other years that is referred to as “mouth to Willamette River”, and is included with this section in most analyses. A total of 187 individual fishing drifts were observed during the two commercial spring Chinook fisheries in 2010.

A large portion of the objectives for this project was focused on analysis of existing commercial fishery observation data collected over the last several years. Methods for recording, entering, and verifying this data varied over the period, therefore, the first task was to standardize and error-check the existing database. During the autumn of 2009 staff line-verified the database using the original datasheets, corrected errors, entered missing and incomplete records, and standardized data fields. Simpler data entry forms and validation checks were added to the database to increase the accuracy, consistency, and efficiency of data entry for the future.

Following the completion of the final database, we compiled a vessel-specific dataset that cross-referenced onboard observations to the landed catch for vessels during each fishing period from 2004 to 2009. Due to normal delays in processing final landings figures, the data needed to cross-reference landings data for 2010 were unavailable at the time of this writing, thus the 2010 data are included in many of the exercises that do not require examination of matched observation and landings data, but excluded from those that do. The dataset that contains the matched records of observations and landings will be referred to as the “matched observations-landings dataset” throughout this report.

The dataset of matched landings and observations was then used to compare distributions of the variables common to both the onboard observations and the landed catch. Variables include fishery dates, catch areas, number of Chinook kept, and catch rate per boat (kept catch per drift for onboard observations and catch per trip for landed catch). Because quantifying non-retained catch currently relies on expansions derived from onboard observations, unbiased observations of these variables are critical to generating unbiased estimates of total incidental catches in a full-fleet fishery.

Tables 1.1 and 1.2 present a summary of the number of paired samples of boat observations and landings. Data collected for landings information and for the observation program differ in some ways, and consequently, the link between observations and deliveries for some records could not be established. We considered an “observed boat trip” to be the total fishing effort conducted by a fisher during one open fishing period in which at least one fishing drift was observed by Agency staff. During an observed boat trip, observers may have been present for only one fishing drift or for all drifts made during the period. Observations conducted on a single boat for an entire fishing period were not uniquely identified and therefore cannot be identified in the database. The proportion of boats that made deliveries and had at least one drift observed during the fishery (observed boat trips divided by number of deliveries by the full fleet) averaged 20% during 2004 – 2009 (Table 1.3).

Using the observation data we summarized the ratios of non-retained fish (unmarked Chinook and all steelhead) to kept Chinook. We compared the distribution of steelhead and unmarked catches to the distribution of kept Chinook catches across several strata (fishery date, fishery area, and individual boat) to determine if encounter rates of steelhead and unmarked spring Chinook are independent of marked Chinook or are correlated.

Table 1.1. Overview of Chinook catch information used to evaluate sampling for handle rates in the Winter/Spring lower Columbia River commercial net fisheries, 2004 - 2009.

| Year | N of Fishing Periods | N of Chinook landed (fleet) | N of landed Chinook observed | N of landed Chinook observed in matched samples |
|---|---|---|---|---|
| 2004 | 9 | 11,408 | 939 | 626 |
| 2005 | 7 | 4,065 | 358 | 286 |
| 2006 | 12 | 3,667 | 171 | 112 |
| 2007 | 5 | 2,318 | 165 | 128 |
| 2008 | 3 | 5,305 | 525 | 416 |
| 2009 | 3 | 3,304 | 457 | 401 |
| Total | 39 | 30,066 | 2,615 | 1,969 |

Table 1.2. Number of commercial Chinook deliveries, boats observed onboard, and drifts observed onboard, 2004 – 2009.

| Year | N of Deliveries (fleet) | N of Drifts Observed | N of Boat Trips Observed (one or more drifts) | N of Observed Drifts in Matched Samples | N of Boat Trips Observed in Matched Samples |
|---|---|---|---|---|---|
| 2004 | 1,096 | 726 | 237 | 393 | 154 |
| 2005 | 685 | 410 | 140 | 227 | 79 |
| 2006 | 642 | 297 | 99 | 160 | 62 |
| 2007 | 332 | 151 | 59 | 100 | 39 |
| 2008 | 196 | 255 | 52 | 176 | 44 |
| 2009 | 383 | 255 | 89 | 213 | 75 |
| Total | 3,334 | 2,094 | 676 | 1,269 | 453 |

### Chinook Catch

Because only a portion of onboard observations could be directly linked to delivery information, we wanted to ensure that the observed kept Chinook in the matched observations-landings dataset that would be used to evaluate the full observation dataset were representative of all onboard observations. We also wanted to ensure that the total landed Chinook in the matched observations-landings dataset that would be used to evaluate the full fleet landings dataset were representative of the full fleet landings. These steps were necessary because the matched observations-landings dataset forms a cross-reference between both of these dissimilar datasets, and ensuring it was adequately representative of each was a key to evaluating what kinds of assumptions we could make from the data.

For comparisons of observed kept Chinook in the matched observations-landings dataset with the total observations dataset, we converted the total Chinook catch in the matched dataset and in the full observations dataset to the proportions of all total Chinook observed in all fisheries or zones that were observed in each fishery or zone in each dataset. For comparisons of total landed catch in the matched observations-landings dataset and the full fleet landed catch, we converted the total landed catch in the matched observations-landings dataset and in the full fleet landings to the proportion of all total Chinook landed in all fisheries or zones that were landed in each fishery or zone. The comparisons of the proportions among each dataset were then used to determine if kept Chinook observations and Chinook landings were consistently related across the datasets.

**Fishing Effort**

We compared the distribution of the number of boats observed from the matched observations-landings dataset with the number of all boats observed and the number of deliveries made in the matched observations-landings dataset with total deliveries (boat trips) for the full fleet across fishery openers and zones. For comparisons of number of observed boats in the matched observations-landings dataset with the total observations dataset, we converted the total number of boats observed in the matched dataset and in the full observations dataset to the proportions of all total boats observed in all fisheries or zones that were observed in each fishery or zone in each dataset. For comparisons of number of deliveries in the matched observations-landings dataset and the number of deliveries in the full fleet, we converted the total deliveries in the matched observations-landings dataset and the number of deliveries from all landings to the proportion of all total landings in each fishery or zone for each dataset. The comparisons of the proportions within each dataset were then used to determine if number of boats observed and total deliveries were consistently related across the datasets.

**Correlations**

As noted by Cochran (1977), a ratio-based estimator would generally be expected to be more precise than an effort-based estimator if the correlation between the variables is strong. For large random samples, a ratio-based estimator $\hat{Y}_R$ will generally be more precise than an effort-based estimator $\hat{Y}$ if the correlation between $y$ and $x$ is greater than $\left(\frac{1}{2}\right)\left(\frac{CVx}{CVy}\right)$ (Cochran 1977).

If the variability of the auxiliary variable *x*, as measured by its coefficient of variation (CV), is more than twice as large as the CV of the variable *y* to be estimated, the ratio-based estimator will always be less precise than the effort-based estimator. Similarly, if the two variables are poorly correlated, the auxiliary variable would not be expected to accurately represent the variable to be estimated.

In order to test this assumption for these fisheries, we tested the correlation between observed numbers of released Chinook and released steelhead and observed numbers of kept Chinook for each fishery. We also tested the correlations among these variables among observed drifts within each fishery. Due to high incidences of zero catches, particularly for steelhead, many of these drift-based correlations are very poor. We calculated the Pearson's correlation coefficients between kept Chinook and released Chinook and between kept Chinook and steelhead for each fishery type and compared the coefficients to the CV's for the observed values from the same fishery type.

**Stratification**

In order to evaluate whether stratification of estimated encounters of released fish was necessary, we conducted an extensive series of contingency table analyses to test for differences in released catch among strata (date and fishing zone). We did not subdivide steelhead into hatchery or wild categories, as we did for generating specific fishery estimates and variances of wild steelhead encounters. Instead, we focused on detecting differences in total steelhead encounters by strata. This was deemed necessary to increase the sample sizes for steelhead encounters, thus allowing for inclusion of some small sample size encounters that would otherwise be lost due to the need to exclude expected encounters below a critical threshold in the contingency table tests. Following recommendations by Snedacor and Cochran (1967), we considered an expected encounter of one or larger to be sufficient for inclusion in the analyses, whereas expected values of less than one were excluded. Zar (1974) and other authors have cited expected values of five or more as minimums, but other authors, including Snedacor and Cochran, have since stated that this threshold has often been found to be unnecessarily conservative.

We tested for differences in observations of released Chinook within each specific fishing zone in each year and fishery type. This was intended to determine if observed catches of released Chinook differed from what would be expected if observed catches were consistently proportional to observed kept Chinook by date in a specific zone, year, and fishery type. Expected values for observed released Chinook were generated from the observed kept Chinook for each zone, date, and fishery type. The same test was performed using the total number of drifts sampled in that specific zone, year, and fishery type to generate expected values for observed released Chinook. This was intended to determine if observed catches of released Chinook differed from what would be expected if observed releases were consistently proportional to sampling effort by date in a specific zone, year, and fishery type.

We also tested for differences in observations of released Chinook within each specific date sampled in each year and fishery type. This was intended to determine if observed catches of released Chinook among zones on a specific date (only one fishery type occurs on any given date) differed from what would be expected if observed releases in each zone on each date were consistently proportional to observed kept Chinook on that date in each zone. Expected values for observed released Chinook were generated from the observed kept Chinook for each zone on each specific date. The same test was performed using the total number of drifts sampled in each zone and date to generate expected values for observed released Chinook. This was intended to determine if observed catches of released Chinook differed from what would be expected if observed releases were consistently proportional to sampling effort on each date and fishing zone.

We tested for differences in observations of steelhead within each specific fishing zone in each year and fishery type. This was intended to determine if observed catches of steelhead differed from what would be expected if observed catches of steelhead were consistently proportional to observed kept Chinook by date in a specific zone, year, and fishery type. Expected values for observed steelhead were generated from the observed kept Chinook for each zone, date, and fishery type. The same test was performed using the total number of drifts sampled in that specific zone, year, and fishery type to generate expected values for observed steelhead. This was intended to determine if observed catches of steelhead differed from what would be expected if observed steelhead catches were consistently proportional to sampling effort by date in a specific zone, year, and fishery type.

We also tested for differences in observations of steelhead within each specific date sampled in each year and fishery type. This was intended to determine if observed catches of steelhead among zones on a specific date differed from what would be expected if observed catches of steelhead in each zone on each date were consistently proportional to observed kept Chinook on that date in each zone. Expected values for observed steelhead were generated from the observed kept Chinook for each zone on each specific date. The same test was performed using the total number of drifts sampled in each zone and date to generate expected values for observed steelhead. This was intended to determine if observed catches of steelhead differed from what would be expected if observed catches of steelhead were consistently proportional to sampling effort on each date and fishing zone.

### Precision

A key consideration in assessing a sampling program is an evaluation of the precision of the estimates generated versus the sampling effort expended. The management of these fisheries depends on estimates of landed catch from landings data, estimates of the total number of unmarked Chinook encountered in the fishery, and the percentage of wild steelhead that are killed during the fishery. We calculated the 95% confidence intervals (CI's) of estimates of unmarked Chinook encountered and wild steelhead mortalities for each fishery from 2003-2010. Fisheries were grouped by similar gear types and area, due to known differences in encounters by gear type (particularly for steelhead) and by fishing area. These groupings were: large mesh nets used from the mouth upstream to the Willamette River (used in March-May of 2003-2007), tanglenets used from the mouth upstream to the Willamette River (used in March and April of 2003-2005, 2007, and 2010), tanglenets used from the Willamette River upstream (used in March and April of 2008 and 2009), and large mesh nets used in all fishing zones (used in May and June of 2006). A single fishery of extremely short duration and low catch was implemented in a highly restricted area (area "2S") in June of 2007. Because of its low catch (30 landed Chinook), low effort, and lack of use since, this fishery was excluded from precision analyses.

### Resampling

As a method to test for bias, we conducted a re-sampling exercise using the 2003-2010 dataset. Re-sampling techniques are common in similar evaluations of sampling programs (Dumont and Schlekte 2004, Kimura and Balsiger 1985)), and a number of researchers have used these techniques to evaluate various sampling techniques for estimating non-retained catch (Allen et al. 2001, Amande et al. 2010, Bellido and Perez 2007). Allen et al. (2001) cite Manly (1997) as stating that "...in the absence of any other knowledge about a population, the distribution of values found in a random sample of size *n* from the population is the best guide to the distribution in the population. Therefore, to approximate what would happen if the population were resampled, it is sensible to resample the sample." They also note that Manly went on to suggest a minimum number of 1,000 bootstrap samples be conducted to assess the properties of such a distribution.

We treated the sum of all past data collected as the sampling universe and used re-sampling techniques to draw samples from it. Drawn samples were then used to generate estimates of non-retained catch and the results were summarized, characterized in terms of precision, and compared to the known "true" values from within the sampling universe they were drawn from. Because this method is a simulation that was intended to evaluate precision and bias only, we did not subdivide the observation data for steelhead into wild-origin fish as was done for other exercises. Results for steelhead from the re-sampling exercises therefore refer to total steelhead encounters, not just wild-origin steelhead.

We subdivided the observation data into groups based on similarities in fishery areas, gears, and timeframes that match those used in precision analyses. These groupings were: large mesh nets used from the mouth upstream to the Willamette River (used in March-May of 2003-2007), tanglenet nets used from the mouth upstream to the Willamette River (used in March and April of 2003-2005, 2007, and 2010), and tanglenet nets used from the Willamette River upstream (used in March and April of 2008 and 2009). The dataset for large mesh nets used in all fishing zones (used in May and June of 2006) was deemed too small to allow for adequate random re-sampling and was excluded from this exercise. No steelhead were observed in this fishery.

We drew a simple random sample of fishing drifts from within the dataset. Our intent was for the simple random sampling to mimic the results that would be obtained from a zone-stratified-random sample that was ideally weighted to fishing effort and catch by fishing zone. For each iteration, a specified number of drift samples were randomly drawn from within this data to approximate a target sample rate expressed as the fraction of landed Chinook observed.

Ratio-based catch estimates and variances were calculated for released Chinook and steelhead using equations 1.1 and 1.2. Effort-based catch estimates were calculated for released Chinook and steelhead from the sample mean of the number of each respective species observed and the known total number of drifts present in the full data set using equation 2.1. Variances for the effort-based estimates were calculated using equation 2.2. The use of the known number of total drifts was necessary due to a lack of a suitable alternative effort estimator. In practical application, the number of drifts would be known with some amount of error. Our re-sampling efforts were unable to add variance that would be associated with uncertainty of total estimated fishing effort, and thus, precision for the effort-based re-sampling exercises is artificially high compared to what would be expected in actual use.

The ratio- and effort-based estimates and variances were then exported to a spreadsheet and a new sample was drawn. This process was repeated 1,000 times for each fishery, gear type, and sampling rate. We then calculated the mean estimate, mean variance, and CV for the ratio-based and effort-based estimators for each fishery, gear type, and sampling rate.

## RESULTS

### Chinook Catch

The proportional distribution of observations of kept Chinook in the matched observations-landings dataset is similar to the distribution of all observations across the sampled fisheries and across fishing zones (Figure 1.1 a and b). We tested for differences between a 1:1 line and a regression through the observed data for the fishery- and zone-specific data and found that there were no significant differences between the regression and a 1:1 line (Figure 1.1a F=0.23, *p*=0.794; Figure 1.1b F=0.72, *p*=0.556). This information suggests that the matched dataset of observations and landings is consistently proportional to and representative of the total observation dataset, and has not likely been biased during the data collection or analysis process. This allowed us to assume that matched observations of kept Chinook could be used for various levels of testing with the results being applicable to the full fleet observations of kept Chinook.

The proportional distribution of landed Chinook in the matched observations-landings dataset compared to the proportional distribution of total landed Chinook was variable across fisheries and across fishing zone (Figure 1.2 a and b). Differences between a 1:1 line and a regression through the observed data were found to be significant among fisheries (Figure 1.2a F=4.81, *p*=0.014), but not among zones (Figure 1.2b F=3.12, *p*=0.185). It appears that the overall observed landings may be biased slightly across some fisheries compared to total landings. The potential effects of this finding are discussed in greater detail in subsequent sections of this report.

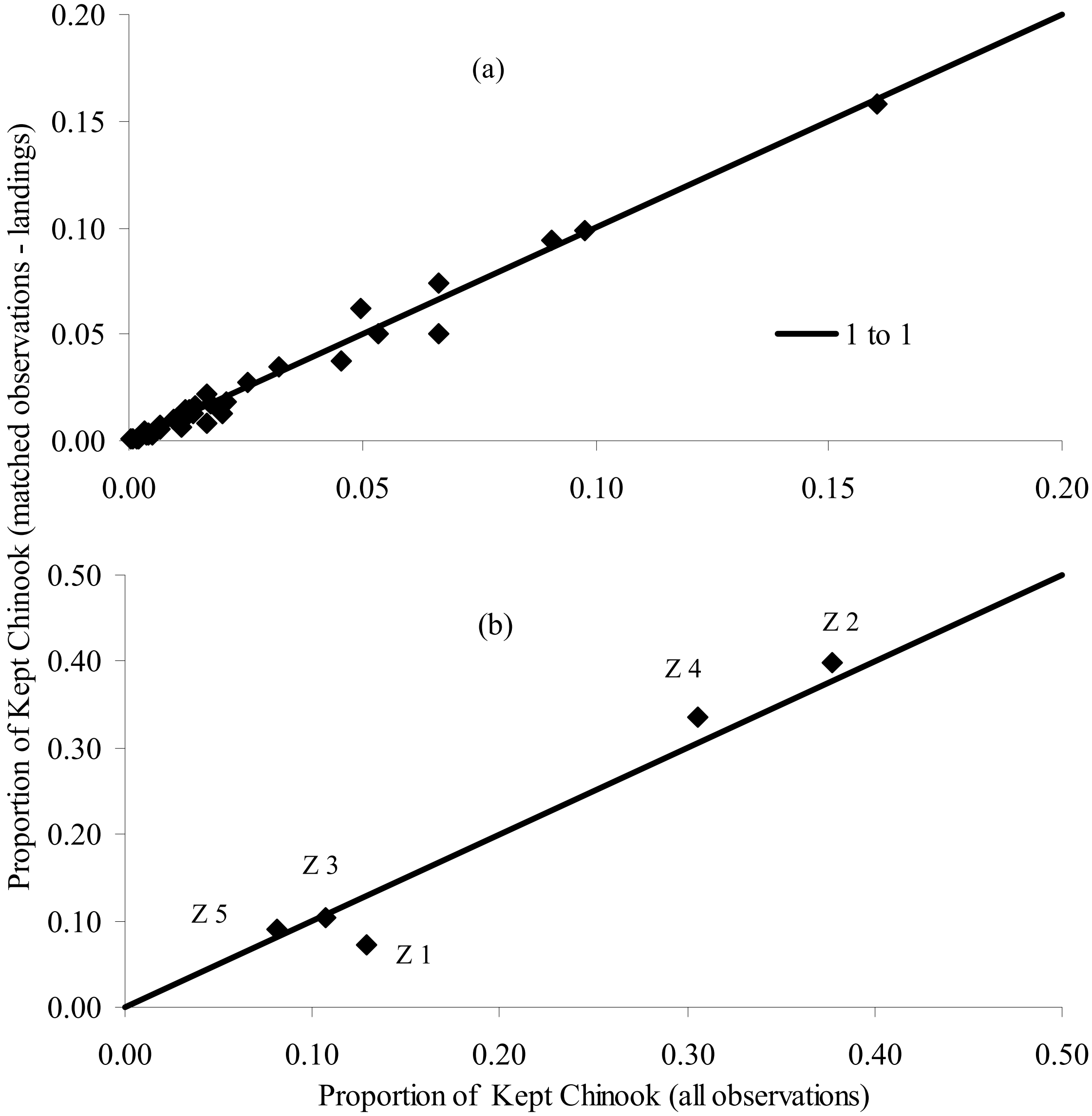


Figure 1.1. Proportions of observed Kept Chinook in the matched observations – landings data compared to proportions of observed Kept Chinook in the total observations data across (a) sampled fishery openers and (b) fishing zones 1 – 5, 2004 – 2009.

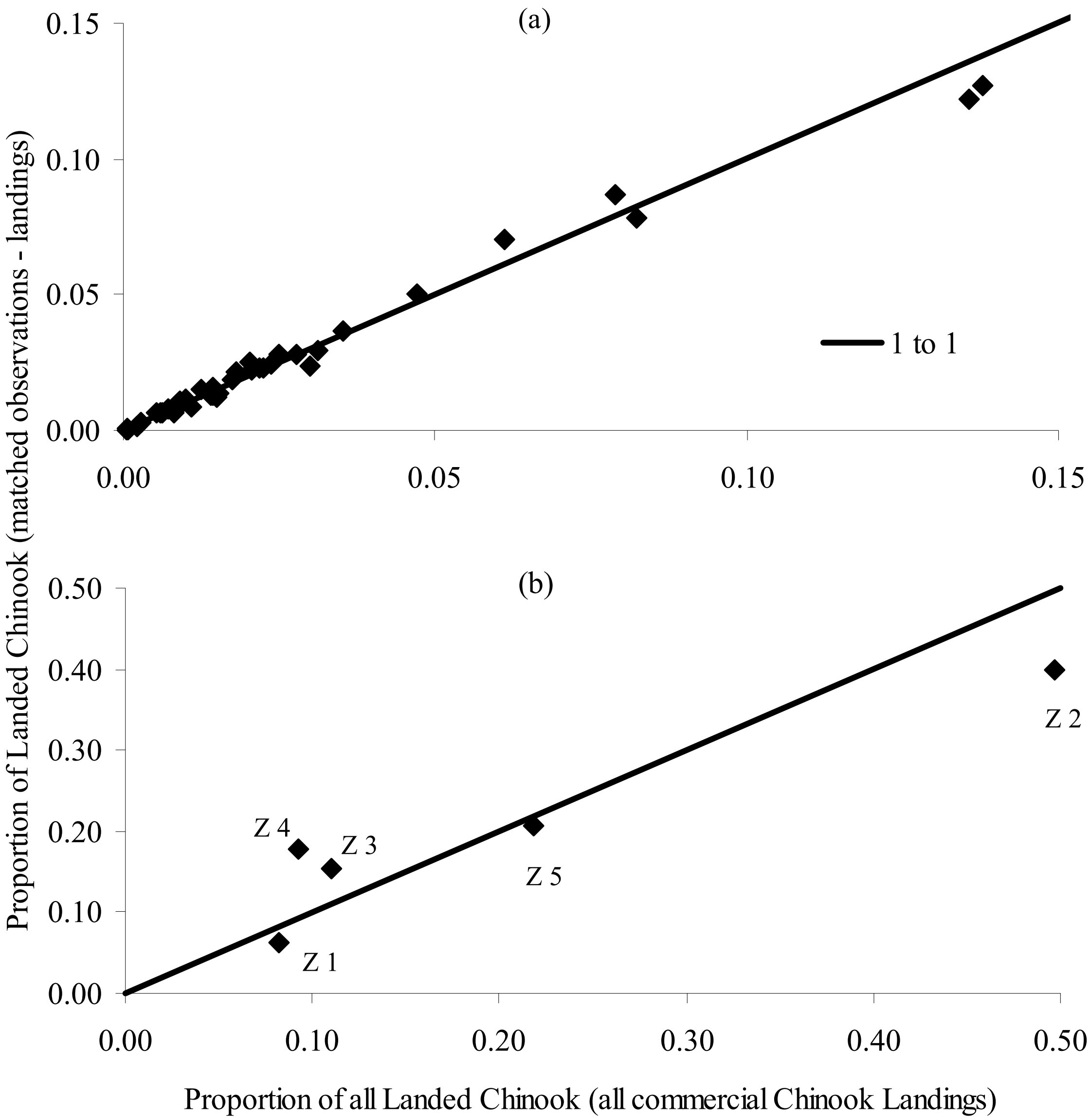


Figure 1.2. Proportions of Landed Chinook in the matched observations – landings data compared to proportions of Landed Chinook in the full fleet Chinook landings data across (a) sampled fishery openers and (b) fishing zones 1 – 5, 2004 – 2009.

**Fishing Effort**

Distributions of total deliveries (i.e. fishing trips) were compared in similar fashion to those for landings and observed kept catch. Figure 1.3 shows the relationship between proportions of boats observed in the matched observations-landings dataset that were observed in each fishery and zone compared to the proportions of total boats observed that were observed in each fishery and zone. We found no significant differences between a 1:1 line and a regression through the observed data in either the fishery- (Figure 1.3a $F=0.44$, $p=0.646$) or zone-specific groups (Figure 1.3b $F=0.21$, $p=0.824$). The matched landings-observation dataset is consistently related to the full observation dataset, indicating that the observed fishing effort from the matched landings-observation dataset is representative of the observed fishing effort from the full observation dataset.

Figure 1.4 shows the relationship between proportions of deliveries (trips) in the matched observations-landings dataset that occurred in each fishery and zone compared to the proportions of total deliveries for the full fleet that occurred in each fishery and zone. Differences between a 1:1 line and a regression through the observed data were found to be not significant for either group (Figure 1.4a $F=0.48$, $p=0.620$; Figure 1.4b $F=2.98$, $p=0.194$).

We compared the distribution of sampled drifts per boat for the observation program with an estimated potential number of drifts per boat for the full fleet. During observations, observers were not asked to note whether they observed a full trip or a partial trip. However, within the observations dataset, 57 records could be identified where observed kept Chinook numbers were similar to total reported landings for the boat and fishery, indicating that all drifts made for those boats had been observed. We estimated the potential number of drifts per boat for observed boats that were not observed for a complete trip by subtracting the boat's observed Chinook catch from its landings, dividing the result by the average observed kept Chinook per drift for that boat trip, and adding the number of drifts observed for that boat. Figure 1.5 shows the frequency distribution of observation effort (N drifts observed) for all boats observed and for observed boats with matched records in the landings data (Figure 1.5 a), as well as boats for which all drifts were observed, and estimated drifts per boat (Figure 1.5 b). These records span the entire dataset of fisheries openers that includes fisheries of different duration (hours) and fishery opener length is directly related to the number of drifts a fisher typically makes in a fishery.

Over the entire dataset, including varying durations of fisheries, the median estimated number of drifts per fisher is about 4 drifts. The majority of observations were conducted for fewer than four drifts per observed boat, with the median value being a single drift. The estimated frequency at which boats delivered Chinook with fewer than four drifts per boat appears to range from 14 to 26%.

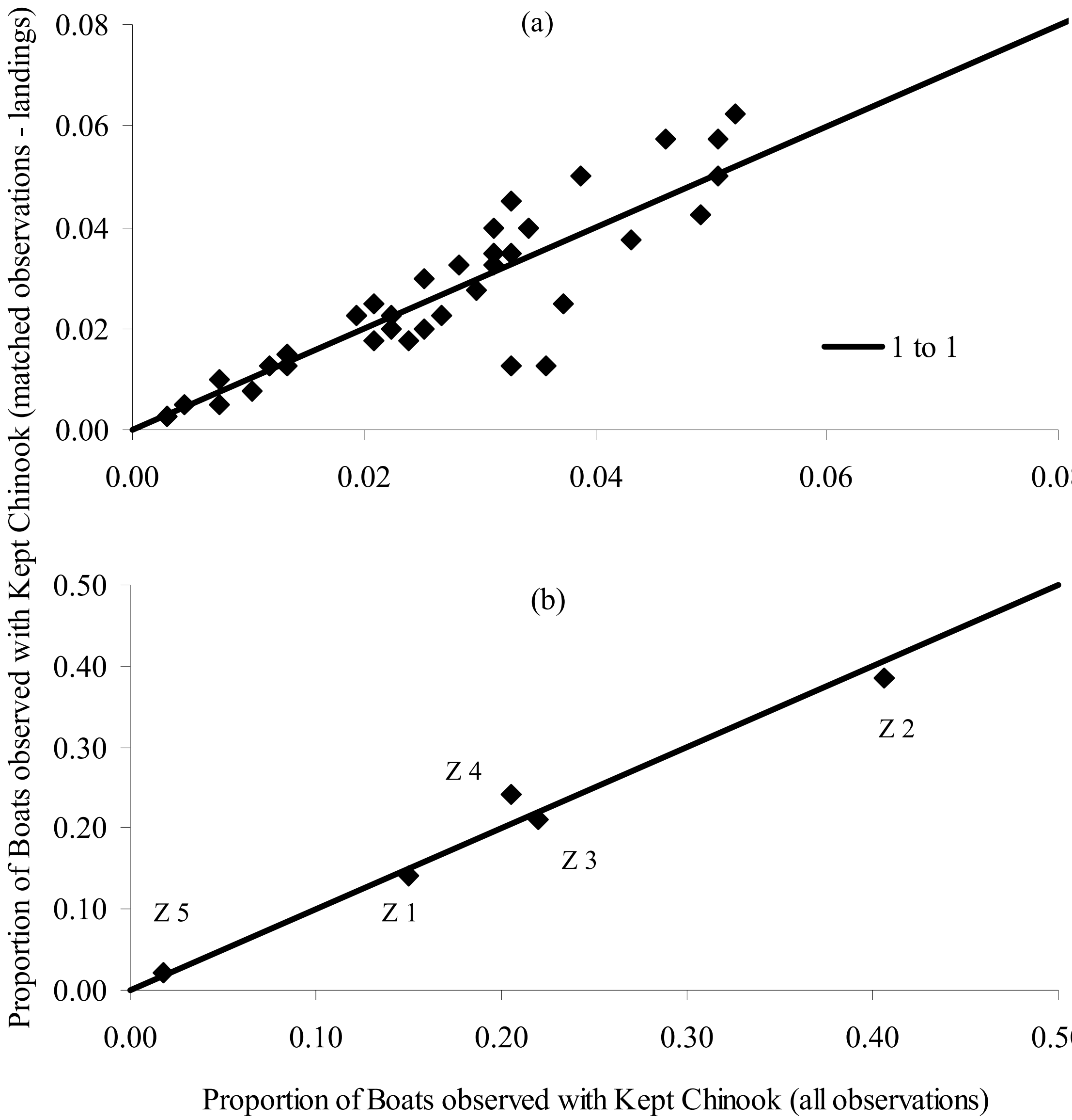


Figure 1.3. Proportions of observed boats with kept Chinook in the matched observations – landings data compared to proportions of observed boats with kept Chinook in the total observations data across (a) sampled fishery openers and (b) fishing zones 1 – 5, 2004 – 2009.

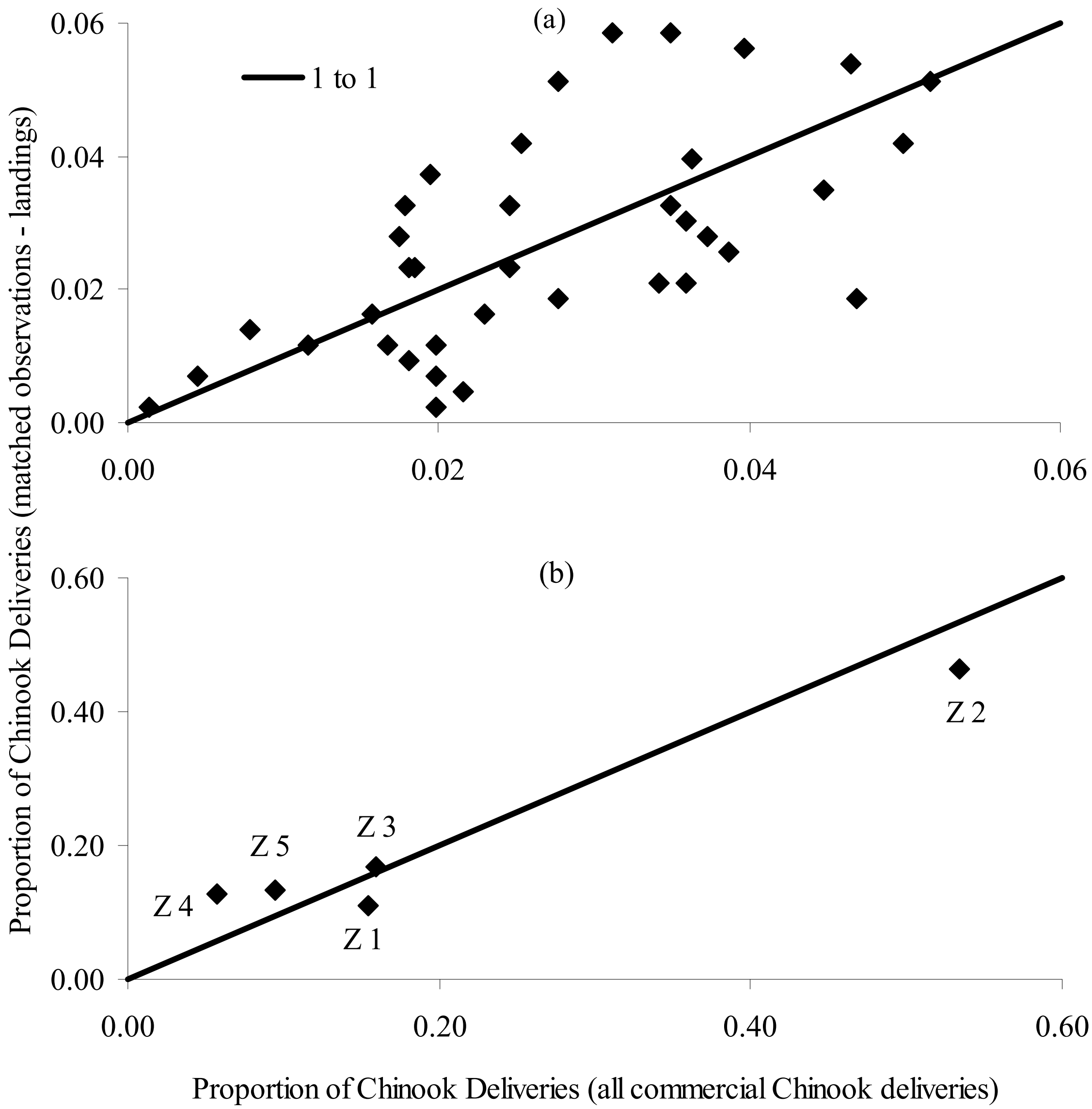


Figure 1.4. Proportions of Chinook deliveries (boat trips) in the matched observations – landings data compared to proportions of Chinook deliveries (boat trips) in the total full fleet Chinook deliveries data across (a) sampled fishery openers and (b) fishing zones 1 – 5, 2004 – 2009.

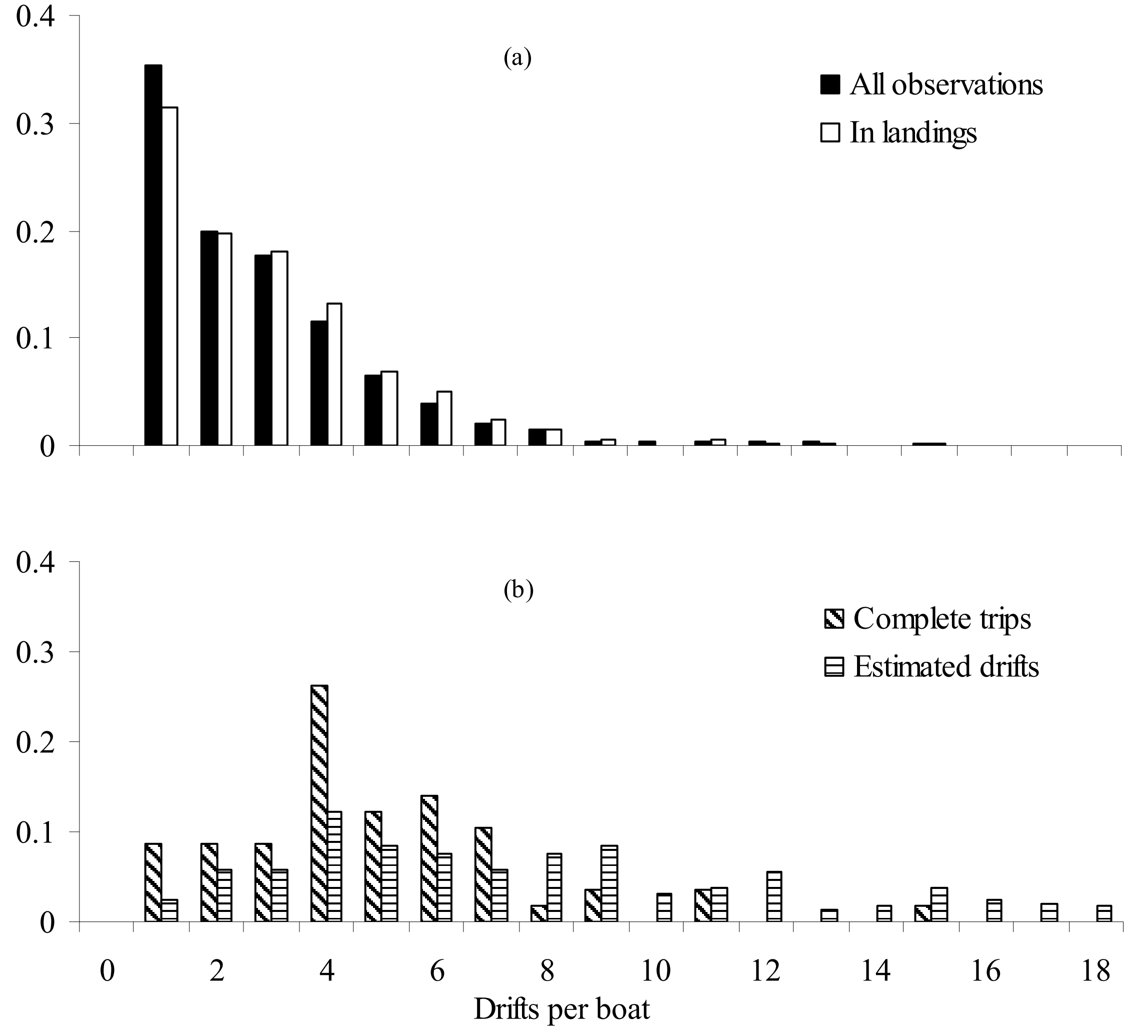


Figure 1.5. Frequency of drifts observed (a) and drifts estimated per boat trip (b).

**Catch per Unit of Effort**

Because observation of complete boat trips was relatively rare (57 records), comparisons between total catch per observed boat and the total landings made per observed boat are difficult. We compared the distribution of the observed catch-per-drift for kept Chinook from the matched landings-observation to catch-per-drift for all observations across the sampled fisheries and fishing zones (Figure 1.6). We excluded boat observations with zero Chinook kept per drift because boats that caught no Chinook would have zero Chinook to deliver and sell. Because not all drifts per trip are observed, whether an observed boat caught and kept fish from drifts that were not observed is unknown. In the matched dataset, of 271 observed boats where no kept Chinook were observed, 41% subsequently made deliveries of Chinook. The distribution of kept Chinook per observed drift for boats in the matched landings dataset is consistently proportional and similar to the distribution for all observations across the sample fisheries and the five fishing zones. Five of 37 fishery openers had average kept Chinook per drift values greater than 3.0 and observations matched to landings were biased slightly higher than the average for all observations in those five fisheries. Differences between a 1:1 line and a regression through the observed data were found to be significantly different for both the fishery- (Figure 1.6a F=23.05, *p*<0.001) and zone-specific (Figure 1.6b F=22.67, *p*=0.015) groups.

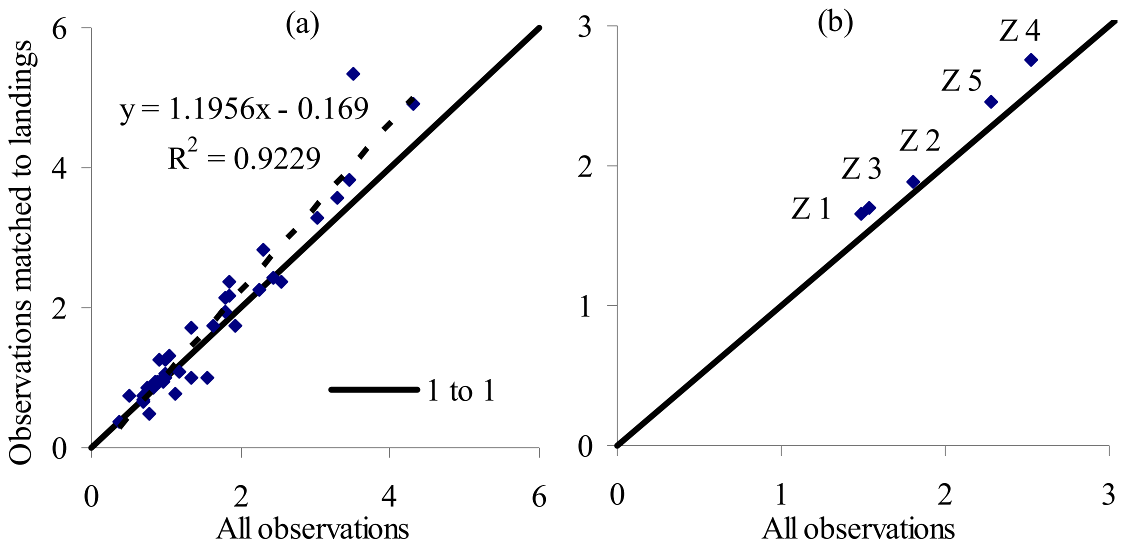


Figure 1.6. Comparison of kept Chinook per drift (CPUE) observed for boats linked to the landings data with kept Chinook per drift for all observations across (a) fishery openers and (b) fishing zones 1 - 5. Observations exclude boats where no kept Chinook were observed.

Because the units of effort differ, we cannot directly compare observed kept catch per drift with landed catch per delivery. However, we can infer how well the distribution of sampling effort matches the effort of the full fleet by evaluating whether paired observation-boat landings are consistently proportional. Figure 1.7 shows average kept Chinook per landing for the full fleet compared to average observed kept Chinook per drift for the sampled fisheries and fishing zones. With few exceptions the observed catch per drift is linearly related and consistently proportional to kept Chinook per landing for the full fleet. Thus, our observations capture across-fishery variability in Chinook catch rates.

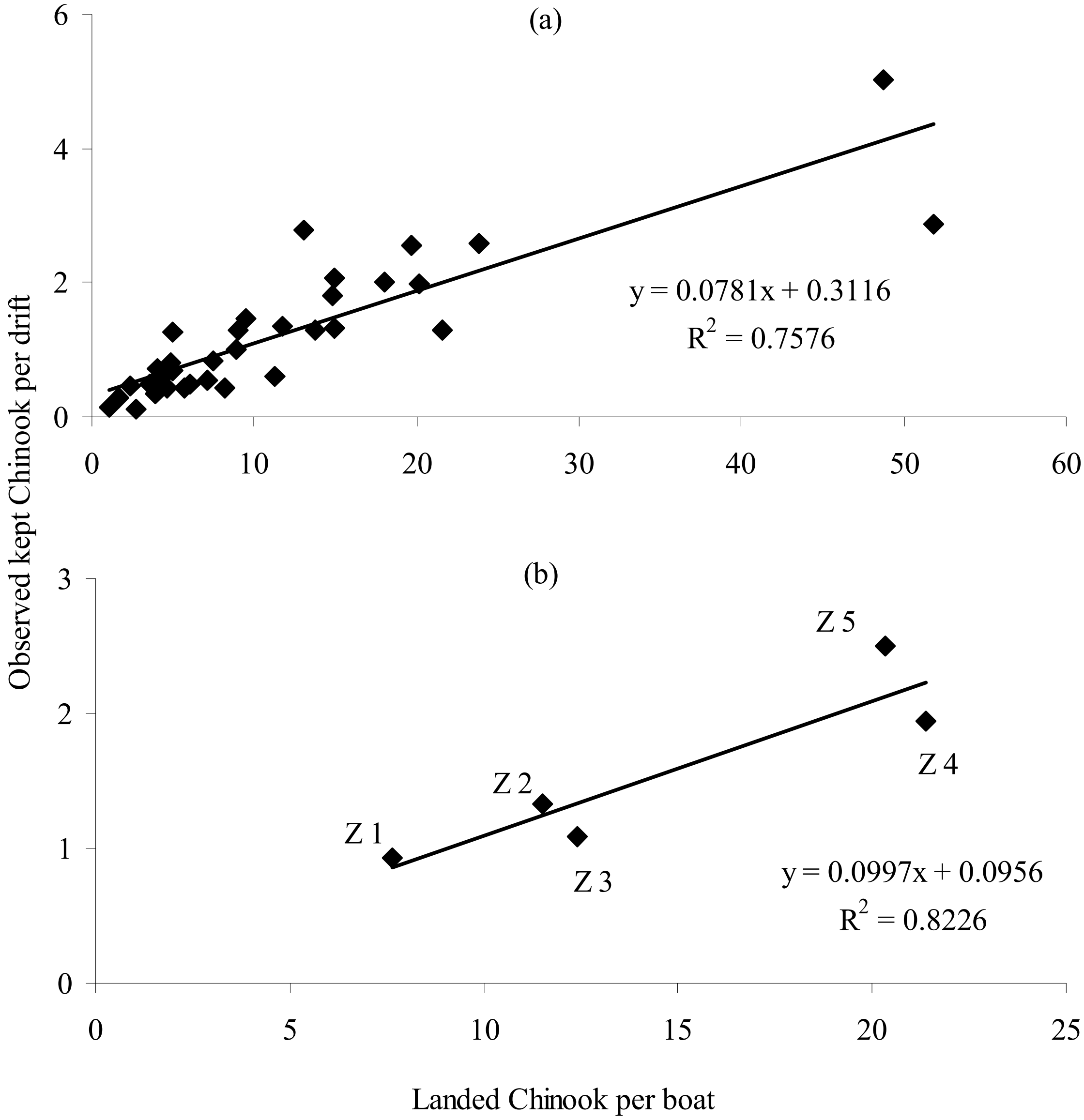


Figure 1.7. Comparison of catch per unit effort for commercial landings (Chinook per boat trip) to onboard observations (kept Chinook per drift) across (a) fishery openers and (b) fishing zones, Z1 – Z5.

We examined the possibility that catches for observed boats could differ from catches for the full fleet by calculating the average kept Chinook catch per opener for observed boats and for the full fleet (Figure 1.8). Across the sequence of all fisheries examined, eight fisheries had observed catch-per-trip rates that were noticeably higher than catch-per-trip rates for the full fleet (Figure 1.9). However, the remainder of fisheries observed appear to have nearly identical rates of kept Chinook among observed trips and full fleet trips. We tested for significant differences between the two groups using a chi square contingency test and found that the two groups were not significantly different ($p$=0.99).

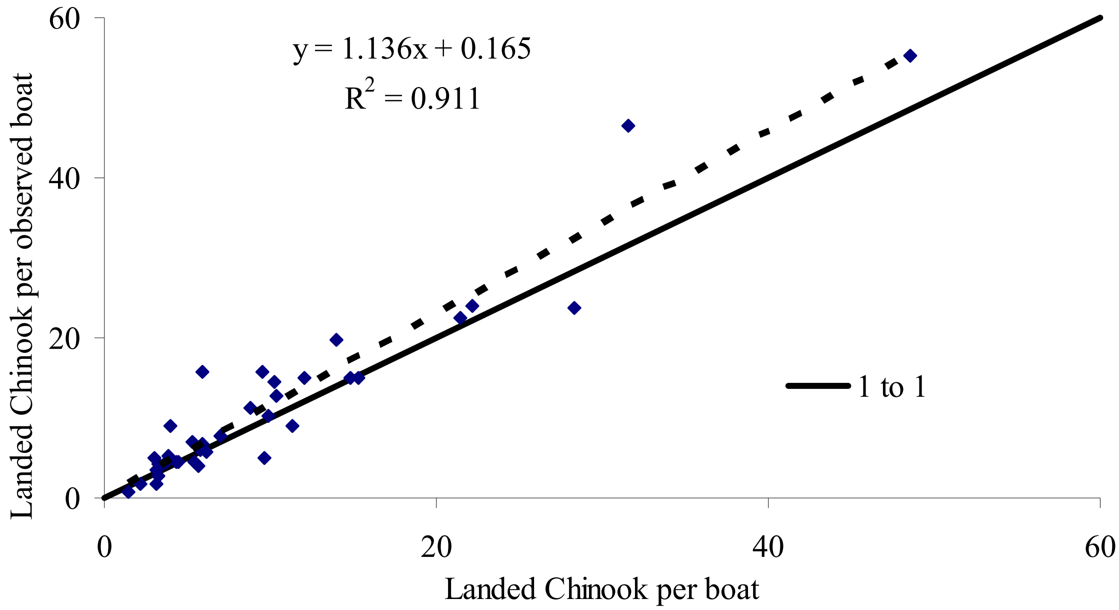


Figure 1.8. Average Chinook catch per boat trip rates in each sampled fishery for all commercial landings and deliveries compared to sampled boats.

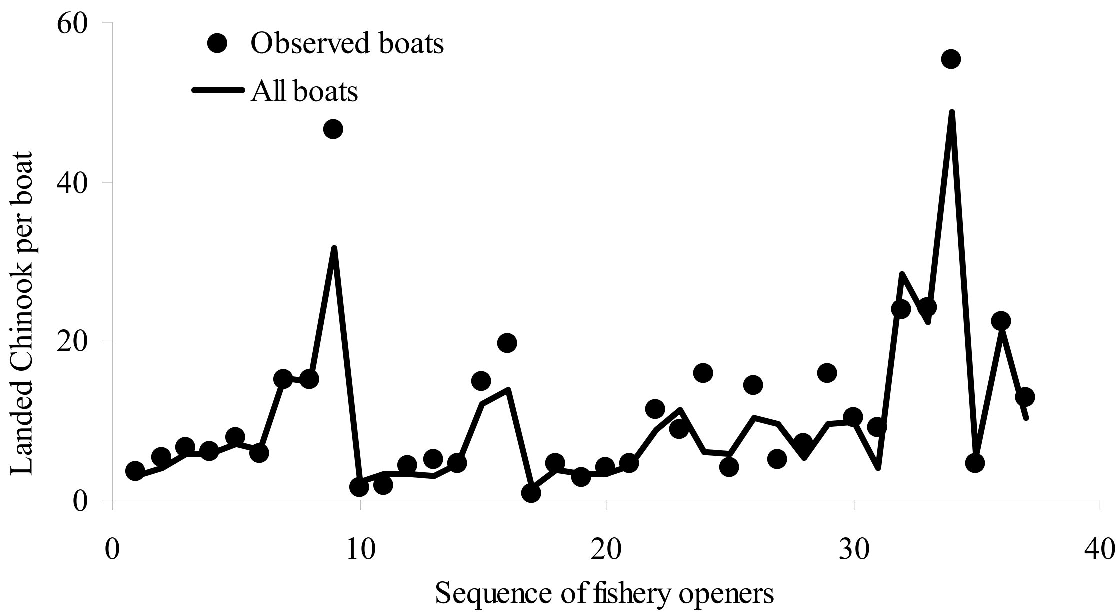


Figure 1.9. Landed Chinook per boat by fishery, 2004 – 2009.

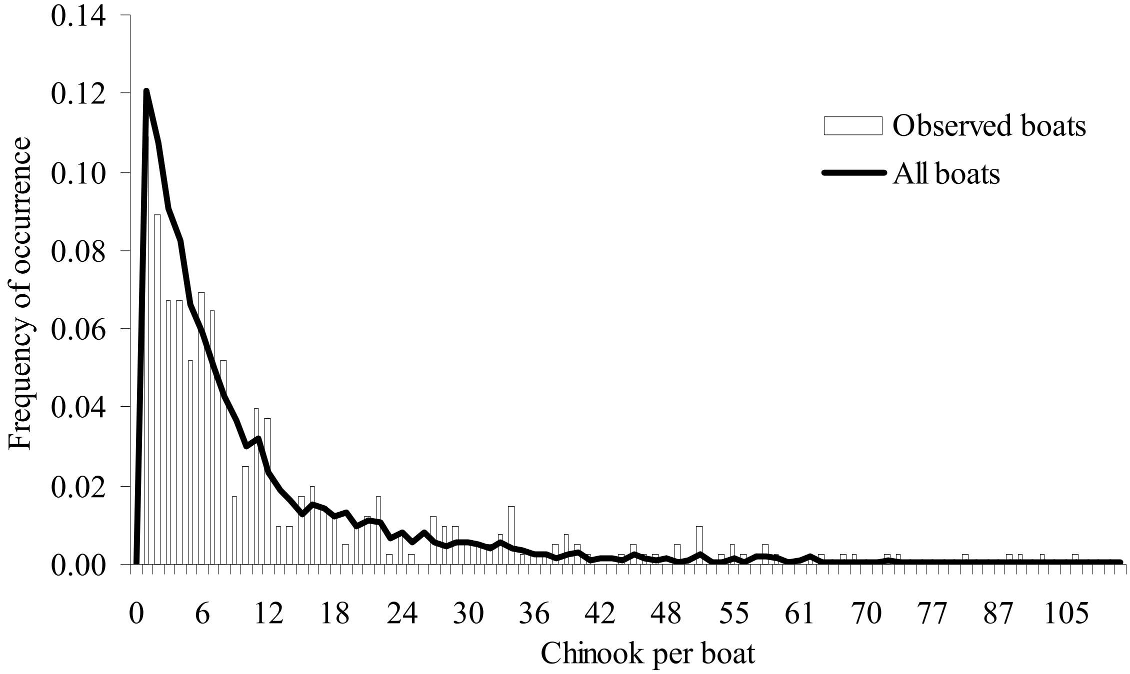


Figure 1.10. Frequency of Chinook catch per boat for all sampled fishery openers, 2004 – 2009.

Large catches of Chinook by individual fishers are relatively rare (Figure 1.10). The frequency of catches per boat per opener is heavily skewed, with most boats landing between 1 and 20 fish per boat per opener. Catches larger than 20 fish in an opener are a relatively rare occurrence. Using a Kolmogorov-Smirnov goodness-of-fit test we found no significant difference between the frequency distributions of catch per boat for observed and full fleet boats ($p$=0.350)

Past information has demonstrated that catches typically vary widely from fisher to fisher. Within the full fleet, some fishers are consistently more or less successful than others. This distribution is expected to be skewed, with very few fishers landing a large number of fish, and many landing relatively fewer. We summarized the total landings by boat license number for the period 2004-2009, then ranked them from the highest total catch to the lowest, beginning with a rank of one for the highest total catch and ending with 171 for the lowest. In the ranked dataset, approximately 50% of all Chinook landed were landed by the top 25% of the ranked fishers/boats.

The catch rankings were carried over to the matched observation-landings dataset, which allowed us to examine whether the sampling program has disproportionately selected fishers of higher or lower catch rankings. We compared the cumulative landings (proportion of total) by fishers' catch ranks in the observation dataset to the frequency distribution in the full fleet deliveries (Figure 1.11). Fishers with higher catch rankings are sampled at a slightly higher rate than the rest of the fleet. This difference was found to be significant using a Kolmogorov-Smirnov goodness-of-fit test ($p$=0.034). The median fisher's catch rank for observed fishers is 43 compared to a median of 58 for the full fleet.

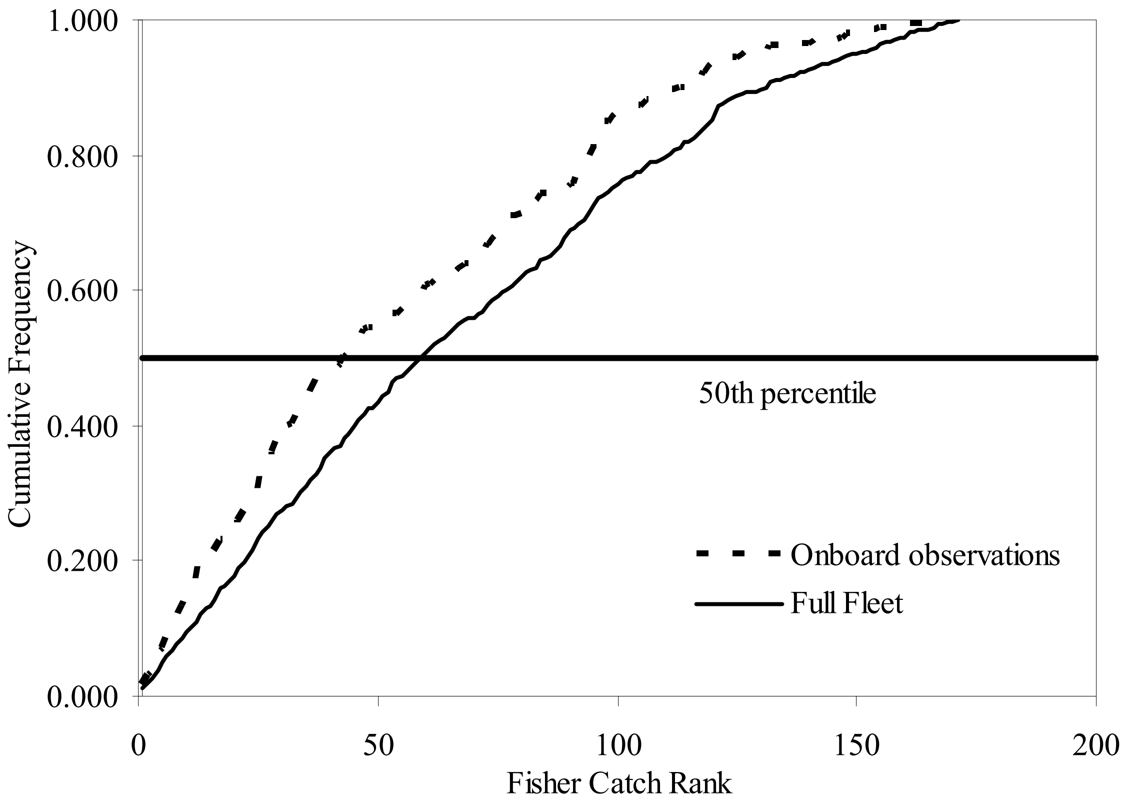


Figure 1.11. Cumulative frequency of fisher's catch rank for onboard observations and Chinook deliveries for the full fleet, 2004 - 2009.

While onboard observations tend to be weighted towards fishers with consistently greater catches, this would not be expected to impose a bias in estimates of released catch if the ratios of kept Chinook to unmarked Chinook and steelhead for high-catch fishers are similar to the fleet average. To test this, we calculated the average ratios of unmarked Chinook to kept Chinook and steelhead to kept Chinook for each ranked fisher. These values were plotted against the rank of the fishers (Figure 1.12). No pattern or trend was detected, indicating that the ratios are not correlated to fisher rank, and that ratios from high-catch fishers are not different than the rest of the fleet.

In examining the resulting plots (Figures 1.12 a and b) we observed a few maxima of kept-to-released ratios (three observations of 2.0 for Chinook and one each of 2.0 and 3.0 for steelhead) that appeared to be potential outliers. We examined these individual observation records for potential errors and none were found, although we noticed that one data point for Chinook and one data point for steelhead were derived from a single record where only one trip was observed. Several low values of kept-to-released ratios were also apparent in records with only one trip observed. To determine whether records from single trip observations were influencing calculations of total kept-to-release rates, we performed a sensitivity analysis by calculating the rates with and without single trip observations. The rate of unmarked Chinook encounters was identical whether the single trip observations were included or not; the rate of steelhead encounters was 0.087 per kept Chinook with the single trip observations included and 0.085 without. In addition, there was no significant trend in kept to release ratios across catch ranks whether these data points were included or not, therefore the single observation data points were retained in the data analysis.

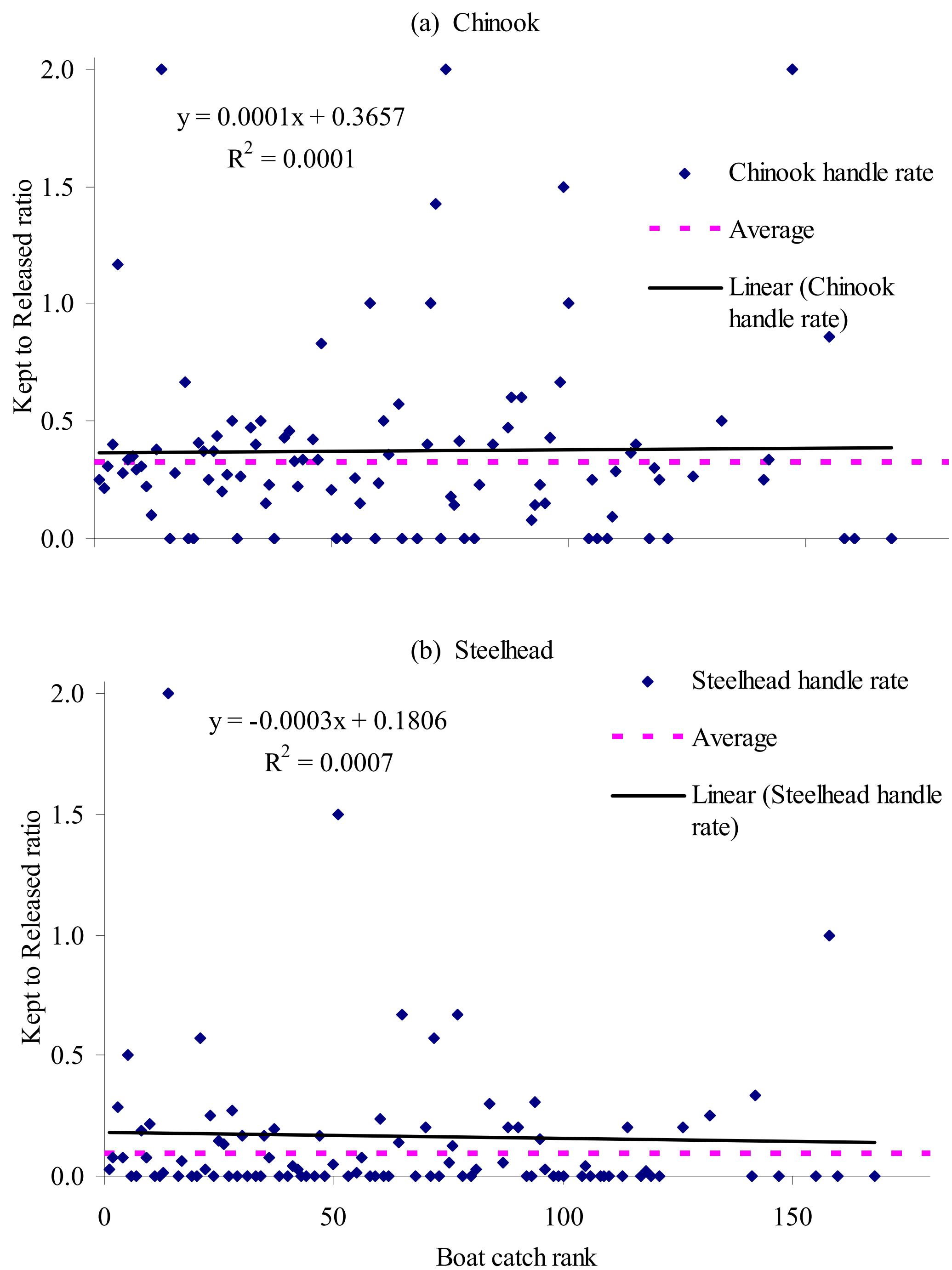


Figure 1.12. Handle rates of (a) released Chinook and (b) steelhead vs. boat/fisher catch rank in number of Chinook caught and landed per boat within the full fleet, 2004 - 2009.

## Correlations

Correlations between kept Chinook and released Chinook were generally strong for all fisheries, regardless of the gear type used or fishery area. The correlation is weakest for the four large mesh fisheries in all five zones observed during 2006. Fewer kept and released Chinook were observed in that fishery than in the others.

Correlations between kept Chinook and released steelhead were greatest for tanglenet fisheries in the Columbia River mouth to the Willamette River area (primarily zones 1-3). Encounters of steelhead are more common in this fishery than in large mesh fisheries or fisheries conducted in zones 4-5.

The association between kept Chinook and released steelhead is weak for fisheries upstream of the Willamette (zones 4-5) and in large mesh fisheries regardless of zone. This is an expected consequence of low encounters of steelhead in large mesh fisheries due to lower vulnerability of steelhead to this gear. Very low abundances of steelhead in zones 4-5 result in low numbers of observed steelhead in tanglenet fisheries that occurred in these areas. No steelhead were observed in the zones 1 – 5 large mesh fisheries conducted in 2006.

Comparisons of CV and Pearson's correlation coefficients of each fishery type indicate that a ratio-based estimator would be expected to result in more precise estimates than an effort-based estimator for estimates of unmarked Chinook in all fisheries, and for estimates of steelhead in tanglenet fisheries conducted downstream of the Willamette River.

Although strong correlation is likely indicative of a situation in which a ratio-based estimator is more precise than an effort-based estimator, poor correlation may not imply that the ratio-based estimator is not applicable. Correlation between two variables is also not necessarily indicative of interdependence between the variables. Instead, variables may co-vary together with some other driving force. This is likely the case for the relationships between Chinook and steelhead encounters in these fisheries. It is probable that the linkage between marked Chinook and unmarked Chinook is in fact interdependent, in that the two groups should have very similar migration patterns, behaviors, and vulnerabilities to the various fishing gears.

Table 1.3. Correlation coefficients and Cochran CV ratio test statistics by fishery gear and type, 2003-2010. ChR = released Chinook, StH = steelhead. Bold values indicate results where correlations exceed the Cochran test statistic.

| | | | Correlation Coefficient (w/kept Chinook) | | Coefficient of Variation | | | Cochran Test Statistic | |
|---|---|---|---|---|---|---|---|---|---|
| Gear Type | Timeframe | Area | ChR | StH | ChK | ChR | StH | ChR | StH |
| Large mesh | May-June | Zones 1-5 | **0.423** | NA | 0.677 | 0.802 | NA | **0.422** | NA |
| Large mesh | Mar-Apr | Mouth to Willamette River | **0.898** | -0.080 | 0.568 | 0.850 | 0.891 | **0.334** | 0.319 |
| Tanglenet | Mar-Apr | Upstream of Willamette River | **0.952** | -0.350 | 0.472 | 0.476 | 0.454 | **0.497** | 0.520 |
| Tanglenet | Mar-Apr | Mouth to Willamette River | **0.982** | **0.829** | 0.805 | 0.823 | 0.586 | **0.489** | **0.687** |

## Stratification

Results of the contingency tables varied among years, fishing dates, fishing zones, species, and test type. However, patterns of differences in certain tests were consistent. Summary results from these tests can be found in Appendix Tables 1-6.

Differences between expected observations of unmarked Chinook derived from observed kept Chinook and actual observed unmarked Chinook were most often not statistically significant. This can be explained by the fact that each test was limited to a single zone, year, and fishery type. Within a given year, the proportion of marked (kept) and unmarked (released) Chinook is relatively constant. Thus, given a known number of marked fish, one would expect a relatively constant number of unmarked fish to be encountered. This assumption will not necessarily hold across years however, given inter-annual variation in mark rates for Chinook.

Differences between expected observations of unmarked Chinook derived from the number of fishing drifts observed and actual observed ummarked Chinook were statistically significant in about half of the tests. This is due to the previously discussed expected changes in total Chinook abundance by date. The catch of released Chinook per fishing drift would not be expected to remain constant across dates. This result would indicate that stratification by date would be necessary in generating full fishery estimates of released Chinook.

Differences between expected and actual observed unmarked Chinook by fishing date and fishery type pooled across zones indicated significant differences in the majority of tests conducted for small mesh fisheries. This held true whether the expected values were generated using observed kept Chinook or number of drifts sampled. However, for large mesh fisheries, the same tests demonstrated no significant differences in the majority of cases.

Differences between expected and actual observed steelhead encounters were generally statistically significant regardless of how the data were combined or how the expected values were generated. However, in many instances, very low numbers of observed steelhead lead to expected values of less than one, which required exclusion of the sample from the tests. This resulted in many tests not being performed for specific fisheries, dates, and fishing zones. Although many tests could not be performed, the frequency of significant differences in cases that could be tested indicates that stratification by date, fishery type, and zone would be appropriate for estimates of steelhead encounters.

Given that we determined that stratification by zone, date, and fishery type is likely necessary to generate estimates of released catch in many of the fisheries examined, we calculated all estimates of released catch and their variances under a fully stratified process. The results of the contingency table analyses indicated that this may not be necessary in all cases; however, we opted to maintain a standard method amongst fisheries to simulate the most likely method of implementation.

In conducting stratified estimates, we calculated estimates of released species using the ratio estimator with landed Chinook as the auxiliary variable for expansions (equation 1.1). Estimates were conducted for each date, fishery type, and fishing zone. Variances were calculated using equation 1.2. For each fishery date, the individual estimates and variances of released Chinook and wild steelhead for each fishing zone were summed to generate the total estimates and variances for each fishery.

In some cases, observations data were not available for specific zones on a given date, although landings were made from that zone. If no observations were conducted, we did not generate an estimate for that zone. These instances most often occur when fishing effort and/or landings in a given zone are very low, making observations difficult. In such cases, managers will need to decide if or how to interpolate missing values. In these cases, one potential method for generating estimates for released catch would be to interpolate using observed ratios from nearby strata. Table 1.4 shows the magnitude of missing samples in terms of observation opportunities missed. Further context is provided by examining the total landings that occurred in non-sampled strata. Across all fisheries covered by the observation program from 2003-2010, 81% of all potential zone and date combinations had one or more drifts observed. Over the same time period, catches in zone and date combinations that were observed accounted for 98% of all landings of kept Chinook. Given these results, the impacts of the missing observations are likely to be quite small, however, as we have excluded missing observations from the estimation process, estimates of released catch shown here must be considered minimums, but are very likely well within the 95% confidence intervals provided.

Table 1.4. Comparison of available combinations of zones and fishery dates (strata) during the 2003-2010 timeframe and number of combinations that received one or more drift observations, and the number and percentage of total landed Chinook available and in strata that were observed.

| | | Available | | | Sampled | | | |
|---|---|---|---|---|---|---|---|---|
| Fishery | Years | Dates | Strata | Landed Chinook | Dates | Strata | Landed Chinook | % of landed |
| Mouth to Kelly Point tanglenet | 2003 | 1 | 3 | 2,526 | 1 | 3 | 2,526 | 100% |
| | 2004 | 3 | 12 | 9,620 | 3 | 9 | 9,511 | 99% |
| | 2005 | 2 | 7 | 3,607 | 2 | 5 | 3,572 | 99% |
| | 2007 | 2 | 8 | 2,255 | 2 | 6 | 2,240 | 99% |
| | 2010 | 2 | 8 | 8,966 | 2 | 7 | 8,568 | 96% |
| Mouth to Kelly Point large mesh | 2003 | 2 | 6 | 518 | 2 | 6 | 518 | 100% |
| | 2004 | 6 | 23 | 3,490 | 6 | 20 | 3,454 | 99% |
| | 2005 | 5 | 18 | 923 | 5 | 15 | 899 | 97% |
| | 2006 | 7 | 23 | 2,236 | 7 | 18 | 2,091 | 94% |
| | 2007 | 1 | 4 | 434 | 1 | 3 | 430 | 99% |
| Upstream of Kelly Point tanglenet | 2008 | 3 | 6 | 5,658 | 3 | 6 | 5,658 | 100% |
| | 2009 | 3 | 6 | 4,154 | 3 | 6 | 4,154 | 100% |
| All zone large mesh (late spring 06) | 2006 | 4 | 20 | 2,087 | 4 | 13 | 1,810 | 87% |
| Sum | | | 144 | 46,474 | | 117 | 45,431 | 98% |

### Precision

In order to calculate the combined effects of precision and sample size, we calculated 95% CI's of ratio-based estimates for each fishery-type from 2003-2010. Estimates and variances were calculated from stratified samples with zone and fishing date comprising the strata. Estimates and variances were summed across strata to generate fishery total estimates and variances. As previously indicated, we did not attempt to generate estimates or variances for strata with no observations. For large mesh gear fished in all zones in May-June of 2006, no steelhead were observed, therefore no estimates or CI values are provided for steelhead in this fishery type. Wild steelhead catch estimates were multiplied by the corresponding gear-specific post-release mortality rate to estimate total wild mortalities (Table 1.5). Unmarked Chinook encounters are presented as total estimated catch to match the management use of this information (Table 1.6).

Table 1.5. Estimated minimum mortalities of wild steelhead, 95% CI's and range, and percent impact on total wild steelhead run, 2003-2010. Impact is calculated as the total mortality divided by the estimated total return.

| Fishery area and gear type | Years | Wild Steelhead Mortalities | CI | 95% CI Range Mortalities | Impact Estimate | CI | 95% CI Range Impact |
|---|---|---|---|---|---|---|---|
| Mouth to Willamette River | 2003 | 238 | 94 | 144-332 | 1.02% | 0.40% | 0.62%-1.42% |
| Tanglenet | 2004 | 280 | 42 | 239-322 | 0.95% | 0.14% | 0.81%-1.09% |
| | 2005 | 55 | 15 | 41 -70 | 0.38% | 0.10% | 0.28%-0.48% |
| | 2007 | 38 | 12 | 26 - 50 | 0.25% | 0.08% | 0.17%-0.33% |
| | 2010 | 118 | 24 | 94 -142 | 0.62% | 0.13% | 0.50%-0.75% |
| Mouth to Willamette River | 2003 | 30 | 21 | 9 - 51 | 0.13% | 0.09% | 0.04%-0.22% |
| Large mesh | 2004 | 70 | 48 | 22-118 | 0.24% | 0.16% | 0.07%-0.40% |
| | 2005 | 21 | 22 | 0 - 43 | 0.14% | 0.15% | 0.00%-0.29% |
| | 2006 | 20 | 28 | 0 - 48 | 0.12% | 0.17% | 0.00%-0.29% |
| | 2007 | 12 | 12 | 0 - 24 | 0.08% | 0.08% | 0.00%-0.16% |
| Upstream of Willamette River[1] | 2008 | 2 | 7 | 0 - 9 | 0.10% | 0.30% | 0.00%-0.40% |
| Tanglenet | 2009 | 1 | 3 | 0 - 4 | 0.04% | 0.11% | 0.00%-0.15% |

[1] In 2008 and 2009, fisheries for Chinook occurred only upstream of Willamette River. In those years, managers from Oregon and Washington, in consultation with the US v Oregon Technical Advisory Group, agreed to use only the component of the wild steelhead return that was destined for areas upstream of Bonneville Dam as the return size for calculating wild steelhead impacts in lower river fisheries. As a result, impact rates for these years appear to be similar to other years even though the number of total mortalities in these years is far lower.

Table 1.6. Estimated minimum catch and 95% CI's and range of unmarked Chinook, 2003-2010.

| Fishery area and gear type | Years | Unmarked Chinook Catch | CI | 95% CI Range |
|---|---|---|---|---|
| Mouth to Willamette River | 2003 | 1,774 | 324 | 1,450-2,098 |
| Tanglenet | 2004 | 3,492 | 970 | 2,522-4,462 |
| | 2005 | 1,153 | 345 | 808-1,498 |
| | 2007 | 420 | 227 | 193 - 647 |
| | 2010 | 1,749 | 529 | 1,219-2,278 |
| Mouth to Willamette River | 2003 | 659 | 224 | 436 - 883 |
| Large mesh | 2004 | 1,451 | 461 | 991 - 1,912 |
| | 2005 | 520 | 200 | 320 - 720 |
| | 2006 | 999 | 376 | 623 - 1,375 |
| | 2007 | 87 | 68 | 18 - 155 |
| Upstream of Willamette River | 2008 | 1,474 | 440 | 1,035-1,914 |
| Tanglenet | 2009 | 1,757 | 251 | 1,506-2,008 |
| All zone | | | | |
| large mesh (late spring 2006) | 2006 | 993 | 902 | 92-1,895 |

As expected, calculated CI's are larger for fisheries with small numbers of observed fish, as well as for those with low sample rates. For fisheries such as Columbia River commercial fisheries, where catch of steelhead is purposely minimized, this interaction makes achieving high levels of precision for steelhead estimates difficult. One main goal of the management of these fisheries is to minimize encounters of steelhead, which simultaneously decreases precision of steelhead estimates. However, it must also be pointed out that a less precise measure of a small estimate results in a low number of encounters and mortalities compared to a more precise measure of a large estimate.

**Resampling**

The ratio-based estimate was generally found to be quite accurate in simple random samples for total estimates of both released Chinook and steelhead (Figures 1.13-1.18). Most outputs indicated a slight positive bias in number of estimated unmarked Chinook and steelhead at small sample sizes. This is not unexpected, as Cochran (1977) indicates that a ratio estimate has bias of the order $\frac{1}{\sqrt{n}}$. This bias is minimal at larger sample sizes. Although biases were detected in the ratio-based estimates, none exceeded 5% for unmarked Chinook or 16% for steelhead at sample rates as low as 3% of kept Chinook observed. Biases were always positive, which would lead to overestimates of released catch. An overestimate of catch would lead to biologically-conservative estimates of catches of unmarked Chinook and impacts for wild steelhead. Even for groups of fisheries with relatively poor correlations between kept Chinook and steelhead encounters, the ratio-estimator closely approximated the true known values in the re-sampling tests.

The effort-based estimate was similarly accurate and was slightly more precise. However, the precision around the effort-based estimate is artificially high given that we were unable to derive a variance for the number of total drifts available for sampling due to the use of the known number of total drifts. The effort-based estimator was typically less biased for Chinook, but tended to be negatively biased for steelhead. This would lead to underestimates for wild steelhead impacts. Although biases were detected, none exceeded 1% for Chinook or 14% for steelhead at sample rates as low as 3% of kept Chinook observed.

In general, differences between ratio-based and effort-based estimates were minimal at most sample rates. Variances for effort-based estimates were always slightly lower than for ratio-based estimates, but the differences were negligible. Given the slight differences, even though the effort-based estimator included known values for effort, it may be that the ratio-based estimator is a more precise estimator in real application, which would be expected for highly correlated variables.

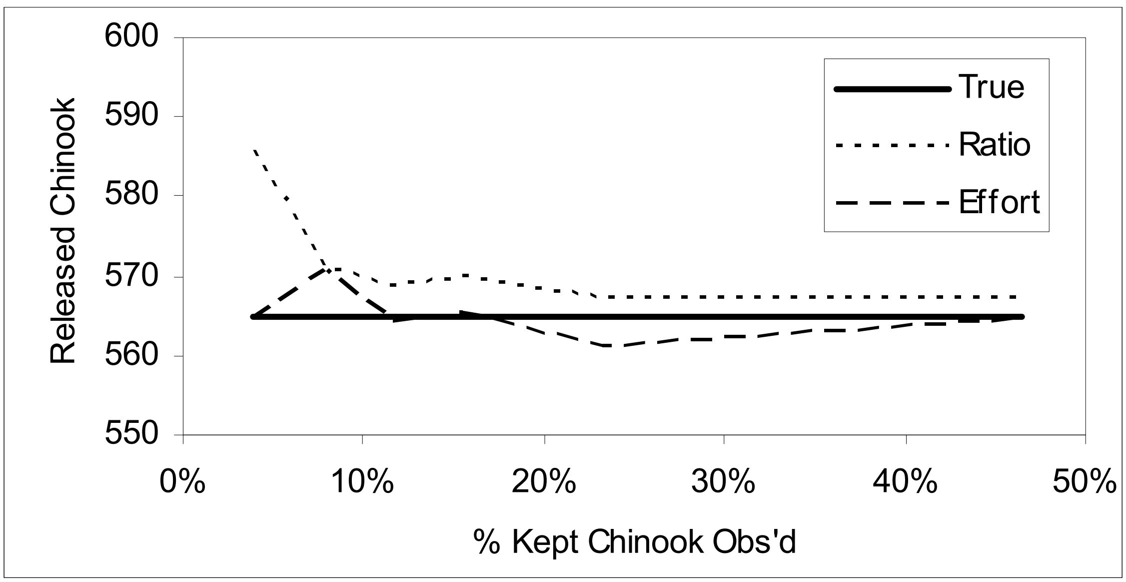


Figure 1.13 Released Chinook estimates, ratio-based and effort-based estimators, tanglenet gear used downstream of Willamette River. True value is 565 fish.

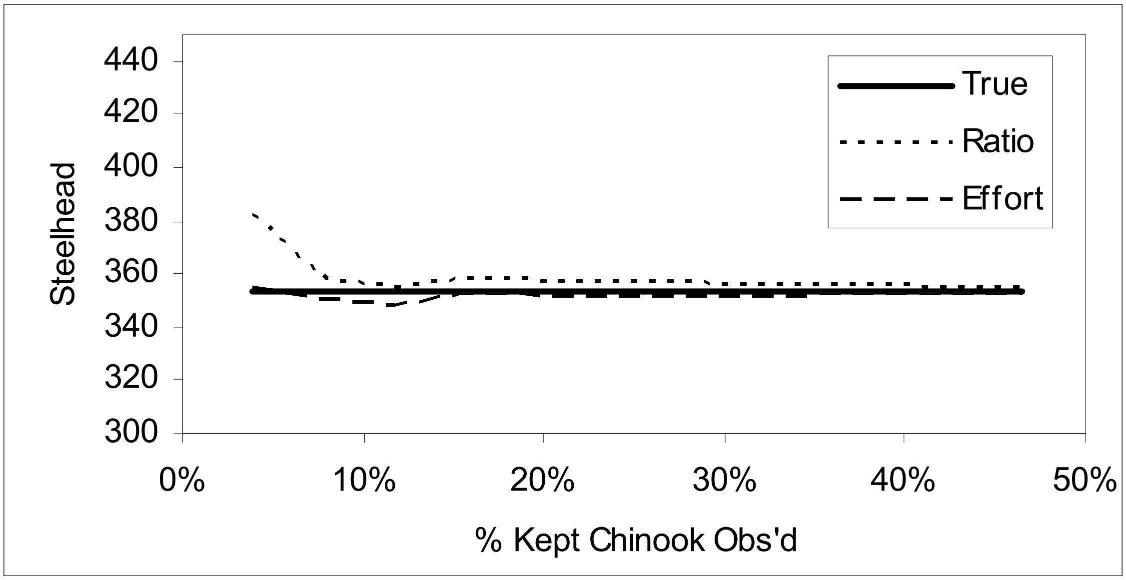


Figure 1.14. Released steelhead estimates, ratio-based and effort-based estimators, tanglenet gear used downstream of Willamette River. True value is 353 fish.

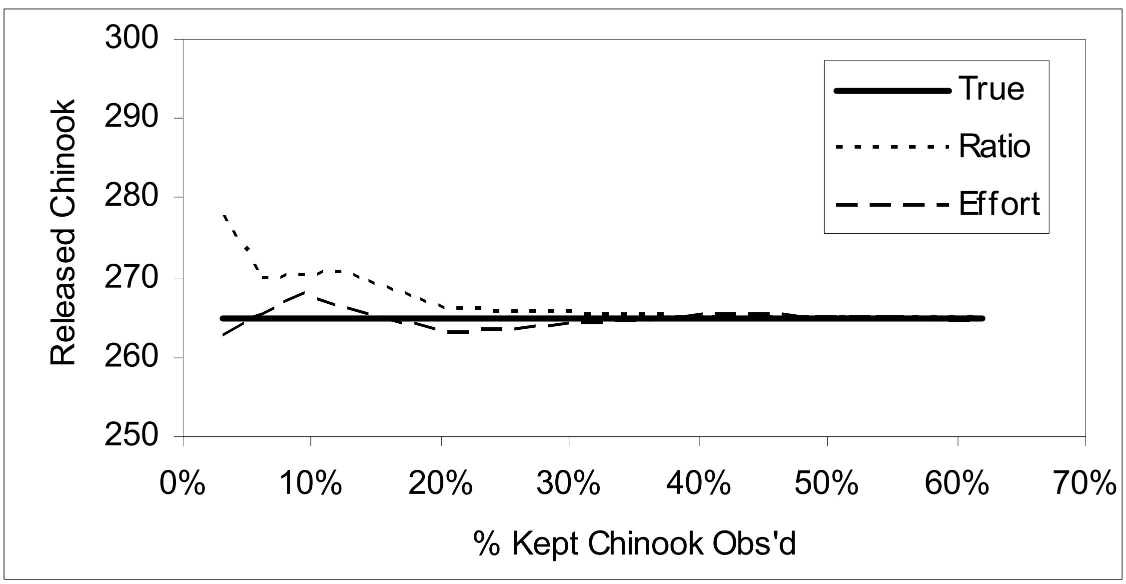


Figure 1.15. Released Chinook estimates, ratio-based and effort-based estimators, tanglenet gear used upstream of Willamette River. True value is 265 fish.

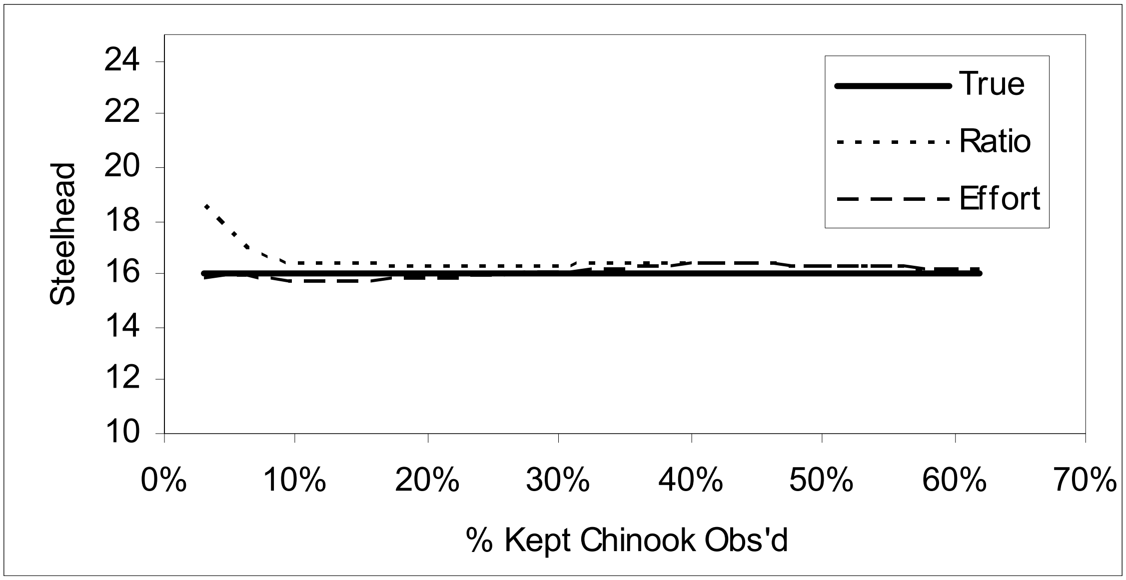


Figure 1.16. Released steelhead estimates, ratio-based and effort-based estimators, tanglenet gear used upstream of Willamette River. True value is 16 fish.

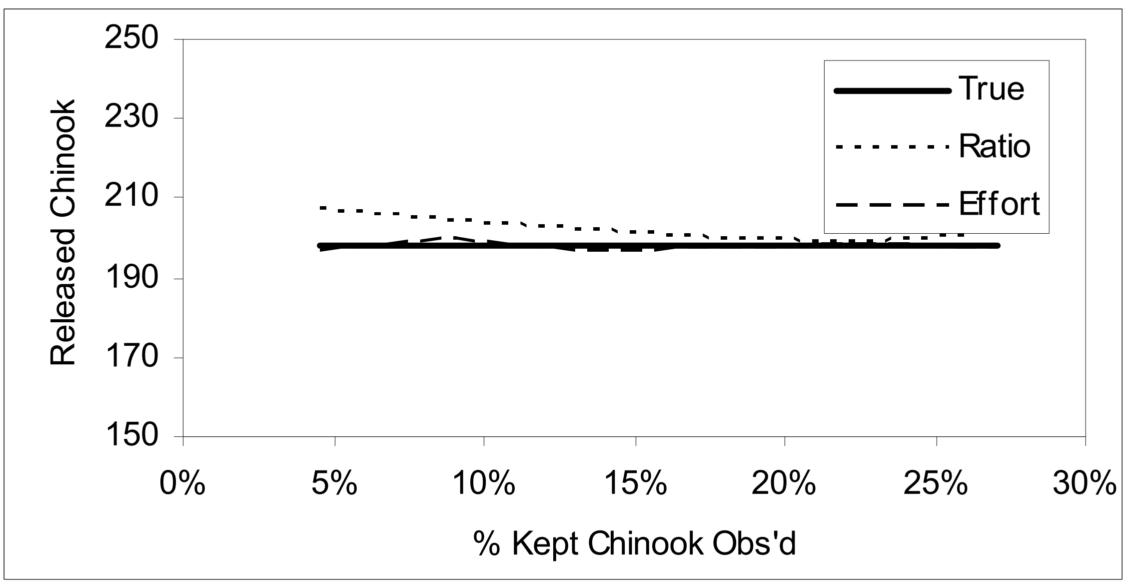


Figure 1.17. Released Chinook estimates, ratio-based and effort-based estimators, large mesh gear used downstream of Willamette River. True value is 198 fish.

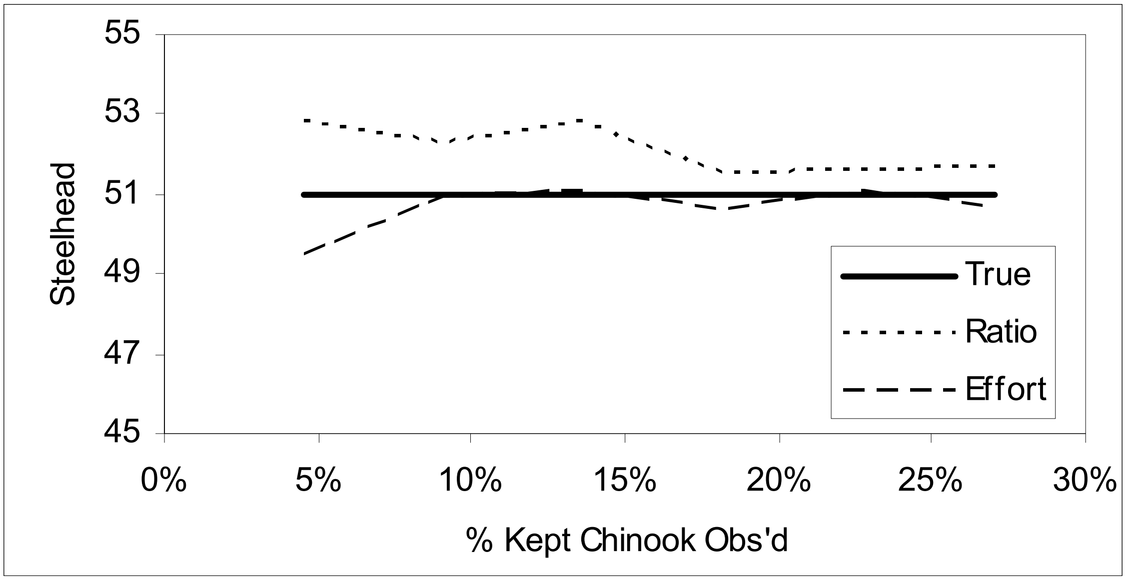


Figure 1.18. Released steelhead estimates, ratio-based and effort-based estimators, large mesh gear used downstream of Willamette River. True value is 51 fish.

## DISCUSSION

Based on our analyses, the current sampling program appears to be suitable and adequate for quantifying total catch of wild steelhead and unmarked Chinook in spring commercial fisheries. Factors we examined included precision, sampling effort, and representativeness across sampling strata and fisher types. We found that stratification by fishing zone, fishery type, and fishing date was a necessary step for generating estimates. This technique matches the stratified random sample design that has historically been used. However, in the past, estimates of released catch have generally been made by pooling observations across zones. We recommend that this practice be replaced with stratified estimates. No changes are needed in the sampling protocol itself to achieve this. It may be possible for managers to use contingency analyses following each fishery to determine whether stratification is necessary or not. However, given that the sampling design must be determined prior to sampling, continuation of a stratified random design for collection of data would be the most practical standard methodology, providing that the level of data needed to conduct stratified estimates for all fisheries is obtained.

Some estimates of wild steelhead catches (and therefore of mortality) generated for some fisheries, zones, or sampling strata have large proportional 95% CI's, although in most of these cases, the absolute values of the estimates and CI's are small. This tends to occur most often in large mesh fisheries, regardless of fishing zone, and for tanglenet fisheries when conducted in the areas upstream of the Willamette River, and is a result of low numbers of steelhead encounters in large mesh fisheries due to gear type, and in the upstream tanglenet fishery due to low abundances of steelhead in that area. However, in terms of management implications and biological impact on the resource, even including the full upper 95% of the CI's as the estimated mortality resulted in low mortality impacts and remained within ESA limits in all cases.

Because we did not attempt to interpolate missing observation data in generating estimates, our analysis did not generate estimates for some individual fishing zones and dates. These instances generally arise in cases where fishing effort is very low in a given zone, thus observations were difficult to collect in those instances. Accordingly, estimates shown here must be regarded as minimum estimates, but would be highly unlikely to be substantially higher than shown if the missing observations were available. Estimates for these areas are currently generated by pooling across zones. As shown in Table 1.4, the overall amount of fishing effort and catch that occurred in areas and times that were not observed was very small.

Estimates of total catch of unmarked Chinook are generally quite precise regardless of zone, fishery, and type. It appears that the current level of sampling provides sufficient precision in the estimates.

Based on our analyses, the observation program appears to have had a slight tendency to sample boats/fishers that have a history of catching more Chinook than the average boat/fisher in the full fleet. However, further testing identified no detectable differences in the ratios of kept Chinook-to-steelhead or in the ratios of kept Chinook-to-unmarked Chinook between fishers with various catch histories. Under an effort-based estimator, the disproportionate sampling of high-catch boats might result in a bias in estimates. This could result due to higher total catches of all species on those boats, which would then be used to derive a mean catch-per-effort rate that would be applied to the full fleet. Under such a scenario, fully random sampling or stratification of fishers might be necessary.

While we found no detectable differences in catch ratios by catch rank, there may be other factors at play that could introduce potential bias to the estimates that we were unable to detect. We recommend maintaining a random selection of boats for sampling to help reduce the risk of unexpected biases. We also recommend that future sampling attempt to obtain and identify more "full-trip" samples to provide more opportunities for examining differences between individual fishers for full fishing trip events.

There were some indications that the proportion of observation effort by zone sometimes differed from the proportion of the full fleet fishing effort by zone. Although in a stratified sampling and estimation procedure, this aspect should not contribute substantial bias, in the future, we recommend more detailed examination of zone-specific deliveries from past years. This process is already a component of the current program, and increasing familiarity with the fishery has lead to improvements in proportional effort allocation over time in the current program. We recommend that the process of selection of sampling units (target drifts/fishers per zone) be formalized based on the results and data analyses generated as part of this study. A sampling effort assignment model has been constructed that will allow for simplification of this process in future years. However, because fishers are allowed to choose which of the open areas they fish, selecting sampling frames exactly in proportion to expended fishing effort will remain a challenge. This is another reason to maintain a stratified sample and estimation design.

During the re-sampling exercise, we found some indications that the ratio-based estimates might be biased slightly high for both unmarked Chinook and for steelhead at very low sampling rates. This bias declines rapidly to zero as sampling rates are increased. The fact that the bias is positive results in a slight overestimate of catch, which is a biologically-conservative estimate. Additionally, the amount of absolute error in these cases was small. It may be possible to use the analyses from this study to create a bias adjustment protocol for estimates that were derived from low sample rate observations, but we do not propose such a process here.

The re-sampling exercise indicated that there may be a potential for small improvements in precision under an effort-based method. However, these gains were quite small, and are confounded by the necessity to use known numbers of drifts for the full-fleet fishery in these simulations. This aspect resulted in variances for the effort-based methods that are in all likelihood underestimated. We were unable to adjust for this factor, but it appears likely that when this is accounted for, differences in precision between the two methods will either be greatly reduced, or that the ratio-based estimate may actually prove to be more precise. An additional factor of the effort-based estimate is that it tended to occasionally generate estimates that were biased low at low sample rates, particularly for steelhead. This would result in underestimating steelhead catch and mortality, and would be less biologically-conservative than the ratio-based estimate.

Our recommendations for future sampling are general, because they will be highly dependent upon the goals that managers specify. If increases in precision on wild steelhead estimates over current methods are a key goal, then our findings indicate that large increases in sampling effort would be needed in most fisheries. This is strictly a result of the relatively low encounters of steelhead observed, particularly in large mesh fisheries and those that occur upstream of the Willamette River. Occurrences of steelhead in catches in these areas are comparatively rare. This is an intended result of the combinations of gear type and area restrictions employed in managing the fisheries. One goal of the management process is to minimize mortality of wild steelhead in these fisheries. Avoidance or reduction of steelhead catches are a key component of this approach, but simultaneously increases the level of difficulty in achieving highly precise estimates. We strongly encourage readers and fishery managers to consider the biological implications of precision at very low estimated numbers in light of these facts.

There may be the potential for improving sampling rates by altering the methods of allocating sampling effort, rather than simply increasing sampling effort or staffing. Samplers could continue to randomly select fishers, but strive to observe more individual drifts on each boat sampled. Over the course of the observation program, observers most often sampled one or two drifts before moving to a different boat. In contrast, most fishers appear to make 4-5 drifts on average. Observing a minimum of 3-5 drifts per boat should increase the number of fish samplers are able to observe per hour of effort expended.

An alternative to the ratio-based estimator is the effort-based method. While we do not currently have the data necessary to fully evaluate this option, we did conduct a re-sampling exercise to attempt to evaluate this method. While this exercise indicated that differences in estimates and precision between the two methods would be minor, it remains a valid potential alternative, and one that is used in many fisheries. In order to implement such a program, a robust estimate or census of fisher effort (drifts) would be needed. Achieving a full census of this variable seems unlikely, and thus we expect that a full-fleet estimate would be instead generated from sampling. The sampling effort requirements for observing individual boats' catches would not be reduced by implementation of this method. Observers would be required to gather the same level of catch information for each observed drift in order to supply the information necessary for expanding estimates.

Given the results of this study, it appears that at least the same level of observation effort and stratification would be necessary to reflect catch information under an effort-based method. Additional sampling effort would be necessary to collect the fishing effort component needed to represent the full-fleet fishery. This would be an additional task for the observation program that is not conducted currently, and would increase total sampling effort and costs.

Options for collecting effort-based information include mandatory or voluntary log books for fishers to record their fishing effort (drifts per fishing zone) per opener. Catch could be collected by fishers via the log book, through the existing observation program, or by a combination of both. The only existing measure of full fishery effort is the number of deliveries that generally reflects the delivery of fish by a single fisher/boat. There is no regulation preventing a fisher from making multiple deliveries of fish caught in a single fishery, although this practice is not common. The use of deliveries to measure effort is also complicated by the fact that deliveries only result from successful fishing trips, with success defined as capture of one or more marked Chinook. Fishers who do not land a marked Chinook during the fishery, but do encounter unmarked Chinook or steelhead during their fishing activities would not be represented in this sampling design. Some additional sampling or estimation procedure to account for this aspect would be needed. It is likely that incidence of zero landed catch in individual openers occurs for some fishers in nearly all openers; however, the number of fishers that do not land any fish is likely a relatively small proportion of the full-fleet. This would make sampling a representative portion of unsuccessful fishers in all required strata difficult. We believe the difficulties in attempting to use deliveries to measure effort in creating an effort-based estimator outweigh the potentially small benefits at this time. If an effort-based estimator is desired, the use of fishing drifts as the basic level of effort would be the most practical application.

An aspect that we felt was important to identify is the effects of the implementation of the pre-fishing test fishery in these fisheries. This program was implemented in 2004. Prior to fishery openers, fishers contracted through WDFW conduct small-scale test fishing to assess the abundance of Chinook, percentage of marked Chinook in the catch, and presence of steelhead (hatchery and wild). The drop in wild steelhead mortality in tanglenet fisheries conducted between the river mouth and the Willamette River since implementation of test fishing is directly attributable to this practice (Figure 1.19).

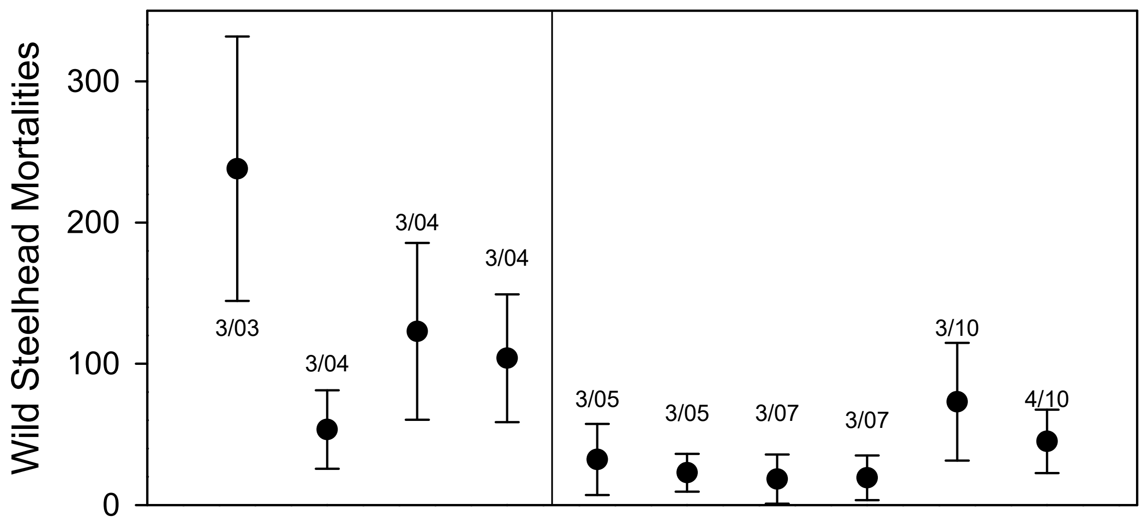


Figure 1.19. Estimated wild steelhead mortalities and 95% confidence intervals in sequential, tanglenet gear fisheries, mouth to Willamette River (2003-2010). Vertical line represents the transition between fisheries that were managed without pre-fishing test and those managed with test fisheries (since 2004). Data labels represent the month and year the fishery occurred in.

There are a number of logistical difficulties in observing these fisheries. Spring fisheries are infrequent, occurring at most once or twice per week for one to two weeks (in recent years). In addition, the hourly open periods in each fishery are variable and often as short as 4 hours. Although participation in Columbia River commercial fisheries is under limited-entry restrictions, and participation in spring seasons is further restricted by requirements of live-capture training (not all fishers have taken the training), participation can still be relatively high, with up to 200 fishers participating in recent years. The fishery can also occur over a large geographic area. The combination of all of the above factors lead to a fishery that is spread out over a relatively large area, has relatively high, but variable, numbers of boats and fishers participating, and tends to occur in short and infrequent intervals. In addition, these fisheries are set on short notice, due to the considerations of run timing, fish abundance, and pre-fishing test fishing results. As a result, the agencies are faced with quickly fielding observation staff on short notice, with the goal of representatively sampling a large number of boats spread over a large area in a short time.

Because these fisheries are jointly managed by the states of Oregon and Washington, final decisions on how modifications are implemented will need to be made jointly by the managing agencies from both states.

# 2. RECREATIONAL FISHERIES

## METHODS

Since 2001, recreational fisheries for spring Chinook in the Columbia River have operated under mark-selective regulations. Prior to this, anglers could retain any spring salmon caught regardless of whether the fish was fin-marked or not. This change has been effective in increasing fishing opportunities, while remaining at or below harvest impact limits. However, little research has been conducted to evaluate the assumptions of the methods used to calculate the number of fish released in this fishery.

The estimated numbers of retained and non-retained fish are estimated by a statistical creel methodology. The details of this program and the effects of different data inputs are beyond the scope of this study. We focused on analyzing whether the data collected for input to the creel program itself appeared to be accurate, rather than attempting to assess the full impact of changes in that data on the overall estimates generated by the creel program.

The number of non-retained fish encountered in recreational fisheries is currently estimated from information gathered during interviews of recreational anglers. Because release of non-retained fish is not directly observed by agency staffs, encounter rates must be estimated from angler responses to interview questions. This process depends on the ability of anglers to accurately recall and report their catch during the interview, and assumes that there are no significant biases, such as number prestige, number preference (Vaske and Beaman 2006), or recall bias, in reporting by individual anglers.

Several factors make direct monitoring of encounters of non-retained fish by recreational anglers difficult and costly. Anglers in the Columbia River are much more numerous than commercial fishers, they tend to fish in a more dispersed fashion, landing fish at numerous boat ramps and bank access points, and individual recreational fishing seasons are open for much longer periods of time than commercial seasons. For example, in 2010 the lower Columbia River spring Chinook recreational fishery was open from January 1 through April 18, although most fishing effort occurred during March and April. During that timeframe, an estimated 172,000 angler trips were expended and an estimated 33,700 (including kept and released catch) spring Chinook were encountered in the fishery (WDFW/ODFW 2011). Physical factors such as weather and geography can also serve to isolate anglers from access by observers.

To observe the Columbia River spring Chinook recreational fishery, we identified locations in eight of the ten creel survey sections open to recreational fishing, where staff were able to visually observe a significant portion of the boat fishers in the area. We attempted to find locations where observers would not be clearly visible to anglers to reduce the chance that observations might affect angler behavior.

Observers were deployed proportionally to fishing effort and mirrored the standard creel program in terms of the areas observed and schedule of sampling; observers were scheduled to observe areas on days and times that creel surveyors sampled the same area. We considered alternate methods for conducting observations, such as riding on selected private boats or with professional fishing guides, but opted to use the remote observation method because we were able to achieve good coverage of sampling areas in this manner and the logistics of conducting the sampling were much simpler. We also felt that conducting observations in this manner would allow us to observe a larger number of anglers and therefore catch events. Observations collected in this manner are also likely to be more representative of the fleet at large than if we had focused solely on sampling trips with professional fishing guides.

Observers recorded individual encounters of salmon and steelhead witnessed at each site. Observations were recorded as “landed”, “released”, “lost”, or “unknown”, and catch was identified to species when possible by visual identification, or noted as a generic “salmonid” if the fish could not be specifically identified as a salmon or steelhead, but was known to not be another species of fish (e.g. white sturgeon). Because the objective of this study was to compare reports of released and kept fish in creel samples to observed released and kept fish, all “lost” or “unknown” disposition fish were excluded from subsequent analyses.

To aid in identification of fish species and disposition, each observer was issued a set of 10-power binoculars and a 60-power spotting scope. Because of difficulties in identifying the species of some fish encountered (Chinook versus steelhead) from a remote observation point, we present observed encounters of both species pooled together as “salmonids”. This difficulty was anticipated, and we believe it will not affect assumptions of representative sampling because factors that would potentially cause a bias in reporting would not be expected to differ substantially based on which species of salmonid an angler encountered. Additionally, during the spring fishery, steelhead make up a relatively small component of the total catch and should have relatively minor effects on results, as the majority of unidentified salmonids would be expected to be Chinook.

The Columbia River spring Chinook recreational fishery was open from January 1 to April 18, 2010. Minimal angler effort occurs prior to March 1, so observation efforts were focused on the March 1–April 18 timeframe.

The Columbia River spring Chinook fishery was closed from April 19 to June 15, although angling for hatchery steelhead was allowed again beginning May 16. During the timeframe the fishery was closed, we conducted observations in the nearby Willamette River recreational spring Chinook fishery that remained open. This fishery also operates under mark-selective rules and catch monitoring and estimation methods are identical to those used in the Columbia River. These similarities allowed us a valuable opportunity to collect information that is applicable to assessing sampling methodologies in the creel programs, although we have analyzed the Columbia and Willamette River information separately from one another.

In the Willamette fishery, large numbers of bank anglers were often visible in close proximity to boat anglers and we were able to observe both groups simultaneously, increasing the number of observations substantially. Observations of catch in the downstream creel sections were quite low, and as a result, the majority of observed events are from the upstream creel section (Figure 2.1).

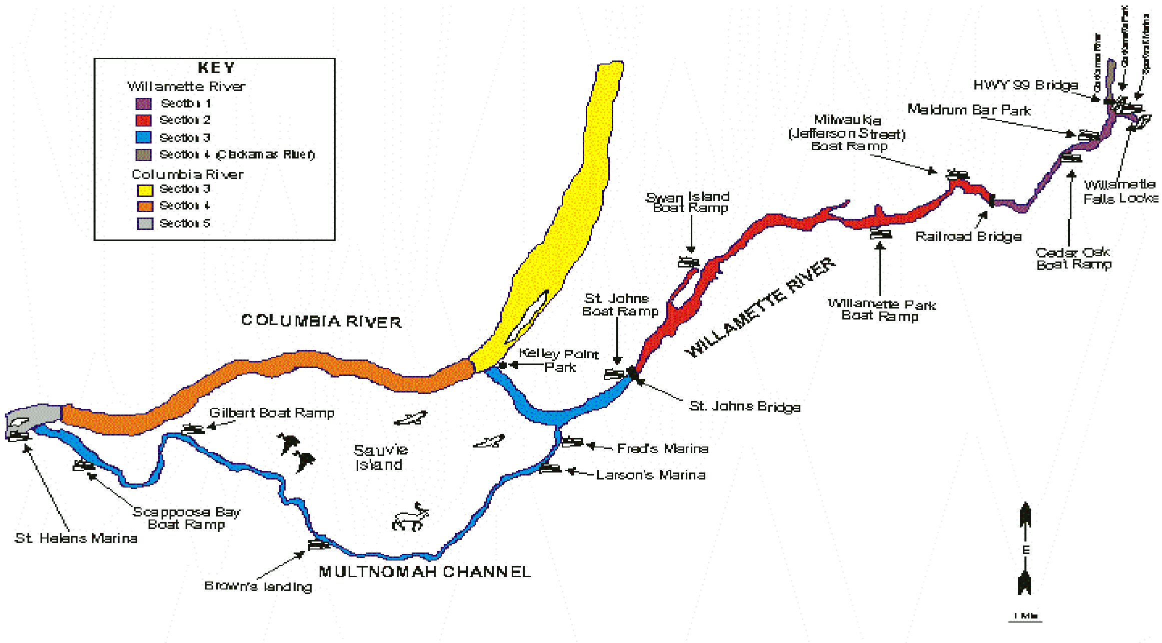


Figure 2.1. Recreational fishery sampling stations and river sections on the Willamette and Clackamas rivers, and recreational fishery sampling sections on the Columbia River near the confluence of the Willamette River.

A mark-selective recreational fishery for summer Chinook was adopted for the period June 16-July 31 for the Columbia River, which allowed us to conduct observations in this fishery. Because of similarities in the fisheries, observation data for this fishery were combined with those for the annual summer steelhead fishery that occurs beginning May 16. Catch rates in this time frame are typically much lower than in the spring, reducing the number of encounters observable. Additionally, more angling effort shifts from boat angling to bank angling, making observation more difficult in some locations. Creel methodologies for this timeframe are identical to those used in spring Columbia River fisheries. However, we analyzed the summer season data separately from both the Columbia River spring and Willamette River spring fisheries.

We compared observed kept and released rates with creel-derived kept and released rates only for dates and areas where collection of data in both programs overlapped. This was done to avoid any potential biases that might be introduced due to natural variations in mark rate over time, allocation of creel and observation effort, or other unknown factors.

Associations between retained and released salmonid data from observations and creel surveys were tested for statistical significance using Fisher's exact test with $\alpha=0.05$. We tested season totals across weeks, as well as season totals across sampling sections to test for the presence of any strata-specific differences, and to allow us to identify the sources (week or sampling section) of any detected differences.

## RESULTS

Over the course of the mainstem Columbia River spring Chinook fishing season, a total of 128 observer days of effort were expended. The majority of discernable salmonid encounters observed and catches estimated from the creel survey occurred in sampling section 3. The majority of creel sampling sections 1 and 2 were closed to angling during the fishing season, and are therefore not included. Observers recorded 611 salmonid encounters that could be identified as either landed or released. Of these, 13.8% were observed to be released compared to a 14.5% release rate calculated from the traditional creel survey data (Table 2.1). This difference was not found to be statistically significant ($p$ = 0.6163) nor were any section-specific differences apparent (Table 2.2).

Observers recorded 1,365 angler encounters with salmonids during the Willamette River recreational fishery, of which 1,145 were ultimately either kept or released, during 68 observer days of effort. Observations pooled by sampling section and week show a 10.9% release rate compared to 12.2% calculated from creel data (Table 2.3). This difference was not found to be statistically significant ($p$ = 0.0725). However, when stratified by sample section, the difference between release percentage calculated from observer and creel data was significantly different for the "lower" section (Table 2.4).

During the mainstem Columbia River summer steelhead and summer Chinook recreational fishery, 140 observer days of effort were expended. A total of 700 observed salmonids could be classified as landed or released. The observed release rate was 47.3% compared to 44.3% calculated from creel data (Table 2.5). This difference was not found to be significant ($p$ = 0.1819; Table 2.6). No observations were conducted in creel sections 2 through 4.

Table 2.1. Summary of observation program and creel survey data, Columbia River recreational spring Chinook fishery, March 1 – April 18, 2010.

| | Visual Observations | | | Creel Surveys | | |
|---|---|---|---|---|---|---|
| Survey Sampling Section | Salmonids Retained | Salmonids Released | Released Percentage | Salmonids Retained | Salmonids Released | Released Percentage |
| 3 | 199 | 31 | 13.5% | 1,432 | 219 | 13.3% |
| 4 | 64 | 8 | 11.1% | 349 | 57 | 14.0% |
| 5 | 64 | 15 | 19.0% | 183 | 50 | 21.5% |
| 6 | 18 | 0 | 0.0% | 33 | 5 | 13.2% |
| 7 | 2 | 0 | 0.0% | 18 | 5 | 21.7% |
| 8 | 25 | 9 | 26.5% | 146 | 40 | 21.5% |
| 9 | 81 | 8 | 9.0% | 281 | 47 | 14.3% |
| 10 | 74 | 13 | 14.9% | 392 | 59 | 13.1% |
| Total | 527 | 84 | 13.8% | 2,834 | 482 | 14.5% |

Table 2.2. Results of Fisher's exact test for association between observation and creel methods of estimating salmonid release rates in the Columbia River recreational spring Chinook fishery, March 1 – April 18, 2010. None of the differences were found to be statistically significant.

| Survey Sampling Section | P value |
|---|---|
| 3 | 0.9175 |
| 4 | 0.4547 |
| 5 | 0.7490 |
| 6 | 0.1641 |
| 7 | 1.0000 |
| 8 | 0.5079 |
| 9 | 0.2188 |
| 10 | 0.6091 |
| Pooled | 0.6163 |

Table 2.3. Summary of observation program and creel survey data, Willamette River recreational spring Chinook fishery, April 19 – June 19, 2010.

| | Visual Observations | | | Creel Surveys | | |
|---|---|---|---|---|---|---|
| Survey Sampling Section | Salmonids Retained | Salmonids Released | Released Percentage | Salmonids Retained | Salmonids Released | Released Percentage |
| Lower | 99 | 3 | 2.9% | 257 | 29 | 10.1% |
| Middle | 76 | 3 | 3.8% | 332 | 38 | 10.3% |
| Upper | 856 | 108 | 11.2% | 746 | 119 | 13.8% |
| Total | 1,031 | 114 | 10.0% | 1,335 | 186 | 12.2% |

Table 2.4. Results of Fisher's exact test for association between observation and creel methods of estimating salmonid release rates in the Willamette River spring Chinook fishery, April 19 – June 19, 2010. Bold indicates statistical significance.

| Survey Sampling Section | P value |
|---|---|
| Lower | **0.0213** |
| Middle | 0.0843 |
| Upper | 0.1026 |
| Pooled | 0.0725 |

Table 2.5. Summary of observation program and creel survey data, Columbia River recreational summer steelhead and summer Chinook fishery, May 16 – July 31, 2010.

| | Visual Observations | | | Creel Surveys | | |
|---|---|---|---|---|---|---|
| Survey Sampling Section | Salmonids Retained | Salmonids Released | Released Percentage | Salmonids Retained | Salmonids Released | Released Percentage |
| 1 | 178 | 220 | 55.3% | 418 | 528 | 55.8% |
| 5 | 9 | 2 | 18.2% | 11 | 8 | 42.1% |
| 6 | 8 | 5 | 38.5% | 33 | 19 | 36.5% |
| 7 | 8 | 7 | 46.7% | 9 | 5 | 35.7% |
| 8 | 43 | 14 | 24.6% | 252 | 111 | 30.6% |
| 9 | 115 | 82 | 41.6% | 290 | 141 | 32.7% |
| 10 | 8 | 1 | 11.1% | 35 | 20 | 36.4% |
| Total | 369 | 331 | 47.3% | 1,048 | 832 | 44.3% |

Table 2.6. Results of Fisher's exact test for association between observation and creel methods of estimating salmonid release rates in the Columbia River recreational summer steelhead and summer Chinook fishery, May 16 – July 31, 2010. Bold indicates statistical significance.

| Survey Sampling Section | P value |
|---|---|
| 1 | 0.8571 |
| 5 | 0.2465 |
| 6 | 1.000 |
| 7 | 0.7104 |
| 8 | 0.4364 |
| 9 | **0.0315** |
| 10 | 0.2507 |
| Pooled | 0.1819 |

## DISCUSSION

In all three sets of recreational fishery observations, we found no significant differences between observed release rates and those determined from creel surveys when samples were combined across sections and dates. Differences were apparent in only two of 23 individual sampling strata examined, and neither was strongly significant. Thus, there are no differences that can be attributed to the use of interviews instead of direct observations. This provides strong evidence that existing methods are suitable for accurately collecting this information. Because of the importance of this information, we recommend that managers consider replicating this or a similar method periodically to confirm the results.

We are unaware of any similar studies that have attempted to validate interview data on released fish in recreational salmon fisheries using these methods. This may represent the first attempt to address uncertainty in this component of angler creel interviews in this manner.

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# APPENDICES

Appendix Table 1. Chi-square contingency test results across dates, large mesh fisheries, mouth upstream to Willamette River.

Observed released Chinook versus expected from observed kept Chinook

| Zone | 1 | | | 2 | | | 3 | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | x2 | test | p | x2 | test | p | x2 | test | p |
| 2003 | 1.600 | 3.841 | 0.206 | 2.063 | 3.841 | 0.151 | 0.200 | 3.841 | 0.655 |
| 2004 | 4.089 | 11.070 | 0.537 | 5.595 | 11.070 | 0.348 | 4.133 | 11.070 | 0.531 |
| 2005 | 4.000 | 9.488 | 0.406 | 2.191 | 9.488 | 0.701 | 1.071 | 7.815 | 0.784 |
| 2006 | -- | -- | -- | 40.040 | 9.488 | 0.000 | 1.276 | 7.815 | 0.735 |
| 2007 | -- | -- | -- | -- | -- | -- | -- | -- | -- |

Observed released Chinook versus expected from number of drifts

| Zone | 1 | | | 2 | | | 3 | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | x2 | test | p | x2 | test | p | x2 | test | p |
| 2003 | 0.987 | 3.841 | 0.610 | 0.391 | 3.841 | 0.532 | 0.258 | 3.841 | 0.612 |
| 2004 | 3.291 | 7.815 | 0.349 | 25.277 | 11.070 | 0.000 | 16.003 | 11.070 | 0.007 |
| 2005 | -- | -- | -- | 7.949 | 9.488 | 0.093 | 0.893 | 9.488 | 0.926 |
| 2006 | -- | -- | -- | 6.951 | 7.815 | 0.073 | 3.935 | 7.815 | 0.269 |
| 2007 | -- | -- | -- | -- | -- | -- | -- | -- | -- |

Observed released steelhead versus expected from observed kept Chinook

| Zone | 1 | | | 2 | | | 3 | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | x2 | test | p | x2 | test | p | x2 | test | p |
| 2003 | 3.000 | 3.841 | 0.083 | 0.006 | 3.841 | 0.939 | -- | -- | -- |
| 2004 | -- | -- | -- | 1.332 | 5.991 | 0.514 | 15.290 | 11.070 | 0.009 |
| 2005 | -- | -- | -- | -- | -- | -- | 1.062 | 3.841 | 0.303 |
| 2006 | -- | -- | -- | 2.606 | 5.991 | 0.272 | 3.273 | 5.991 | 0.195 |
| 2007 | -- | -- | -- | -- | -- | -- | -- | -- | -- |

Observed released steelhead versus expected from number of drifts

| Zone | 1 | | | 2 | | | 3 | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | x2 | test | p | x2 | test | p | x2 | test | p |
| 2003 | 3.571 | 3.841 | 0.059 | 0.111 | 3.841 | 0.739 | -- | -- | -- |
| 2004 | -- | -- | -- | -- | -- | -- | 5.867 | 11.070 | 0.319 |
| 2005 | -- | -- | -- | -- | -- | -- | -- | -- | -- |
| 2006 | -- | -- | -- | 5.199 | 7.815 | 0.158 | 2.805 | 7.815 | 0.423 |
| 2007 | -- | -- | -- | -- | -- | -- | -- | -- | -- |

Appendix Table 2. Chi-square contingency test results across zones, large mesh fisheries, mouth upstream to Willamette River.

| Observed released Chinook versus expected from observed kept Chinook | | | | | Observed released Chinook versus expected from number of drifts | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Areas | x2 | test | p | | Areas | x2 | test | p |
| 2/17/2003 | 1-3 | 4.440 | 5.991 | 0.109 | 2/17/2003 | 1-3 | 2.205 | 5.991 | 0.332 |
| 2/19/2003 | 1-3 | 2.363 | 5.991 | 0.307 | 2/19/2003 | 1-3 | 0.081 | 5.991 | 0.960 |
| 3/2/2004 | 2-3 | 0.910 | 3.841 | 0.340 | 3/2/2004 | 2-3 | 0.263 | 3.841 | 0.608 |
| 3/4/2004 | 1-3 | 0.747 | 5.991 | 0.688 | 3/4/2004 | 1-3 | 0.136 | 5.991 | 0.934 |
| 3/9/2004 | 2-3 | 1.328 | 3.841 | 0.249 | 3/9/2004 | 2-3 | 3.832 | 3.841 | 0.050 |
| 3/11/2004 | 1-3 | 3.432 | 5.991 | 0.180 | 3/11/2004 | 1-3 | 6.318 | 5.991 | 0.042 |
| 3/15/2004 | 1-3 | 0.655 | 5.991 | 0.721 | 3/15/2004 | 1-3 | 4.191 | 5.991 | 0.123 |
| 3/18/2004 | 1-3 | 0.008 | 5.991 | 0.996 | 3/18/2004 | 1-3 | 6.822 | 5.991 | 0.033 |
| 3/1/2005 | -- | -- | -- | -- | 3/1/2005 | -- | -- | -- | -- |
| 3/3/2005 | -- | -- | -- | -- | 3/3/2005 | 2-3 | 0.315 | 3.841 | 0.575 |
| 3/8/2005 | 2-3 | 0.052 | 3.841 | 0.820 | 3/8/2005 | 1-3 | 1.363 | 5.991 | 0.506 |
| 3/10/2005 | 2-3 | 1.158 | 3.841 | 0.282 | 3/10/2005 | 2-3 | 0.077 | 3.841 | 0.077 |
| 3/15/2005 | 2-3 | 1.500 | 3.841 | 0.221 | 3/15/2005 | 1-3 | 4.901 | 5.991 | 0.086 |
| 2/23/2006 | -- | -- | -- | -- | 2/23/2006 | -- | -- | -- | -- |
| 3/2/2006 | -- | -- | -- | -- | 3/2/2006 | -- | -- | -- | -- |
| 3/7/2006 | -- | -- | -- | -- | 3/7/2006 | 2-3 | 0.274 | 3.841 | 0.601 |
| 3/9/2006 | -- | -- | -- | -- | 3/9/2006 | -- | -- | -- | -- |
| 3/14/2006 | 2-3 | 0.312 | 3.841 | 0.577 | 3/14/2006 | 2-3 | 0.005 | 3.841 | 0.944 |
| 3/6/2007 | 1-2 | 1.687 | 3.841 | 0.194 | 3/6/2007 | 2-3 | 0.298 | 3.841 | 0.585 |

| Observed released steelhead versus expected from observed kept Chinook | | | | | Observed released steelhead versus expected from number of drifts | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Areas | x2 | test | p | | Areas | x2 | test | p |
| 2/17/2003 | 1-2 | 0.503 | 3.841 | 0.478 | 2/17/2003 | 1-3 | 2.771 | 5.991 | 0.250 |
| 2/19/2003 | 1-2 | 1.648 | 3.841 | 0.199 | 2/19/2003 | 1-2 | 2.648 | 3.841 | 0.104 |
| 3/2/2004 | 2-3 | 2.010 | 3.841 | 0.156 | 3/2/2004 | 2-3 | 3.018 | 3.841 | 0.082 |
| 3/4/2004 | 1-3 | 3.798 | 5.991 | 0.150 | 3/4/2004 | 1-3 | 7.821 | 5.991 | 0.020 |
| 3/9/2004 | -- | -- | -- | -- | 3/9/2004 | 2-3 | 5.586 | 3.841 | 0.018 |
| 3/11/2004 | -- | -- | -- | -- | 3/11/2004 | 1-2 | 1.237 | 3.841 | 0.266 |
| 3/15/2004 | -- | -- | -- | -- | 3/15/2004 | -- | -- | -- | -- |
| 3/18/2004 | -- | -- | -- | -- | 3/18/2004 | -- | -- | -- | -- |
| 3/1/2005 | -- | -- | -- | -- | 3/1/2005 | -- | -- | -- | -- |
| 3/3/2005 | -- | -- | -- | -- | 3/3/2005 | 1-2 | 1.474 | 3.841 | 0.225 |
| 3/8/2005 | -- | -- | -- | -- | 3/8/2005 | -- | -- | -- | -- |
| 3/10/2005 | -- | -- | -- | -- | 3/10/2005 | -- | -- | -- | -- |
| 3/15/2005 | -- | -- | -- | -- | 3/15/2005 | -- | -- | -- | -- |
| 2/23/2006 | -- | -- | -- | -- | 2/23/2006 | -- | -- | -- | -- |
| 3/2/2006 | -- | -- | -- | -- | 3/2/2006 | -- | -- | -- | -- |
| 3/7/2006 | -- | -- | -- | -- | 3/7/2006 | -- | -- | -- | -- |
| 3/9/2006 | -- | -- | -- | -- | 3/9/2006 | -- | -- | -- | -- |
| 3/14/2006 | 2-3 | 0.938 | 3.841 | 0.333 | 3/14/2006 | 2-3 | 0.147 | 3.841 | 0.701 |
| 3/6/2007 | -- | -- | -- | -- | 3/6/2007 | 2-3 | 1.450 | 3.841 | 0.228 |

Appendix Table 3. Chi-square contingency test results across dates, small mesh fisheries, mouth upstream to Willamette River.

Observed released Chinook versus expected from observed kept Chinook

| | Z1 | | | Z2 | | | Z3 | | | Z4 | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Year | x2 | test | p | x2 | test | p | x2 | test | p | x2 | test | p |
| 2004 | 0.074 | 5.991 | 0.963 | 21.502 | 7.815 | 0.000 | 3.918 | 5.991 | 0.141 | -- | -- | -- |
| 2005 | -- | -- | -- | 0.028 | 3.841 | 0.867 | 0.071 | 3.841 | 0.965 | -- | -- | -- |
| 2007 | -- | -- | -- | 0.419 | 3.841 | 0.518 | -- | -- | -- | -- | -- | -- |
| 2010 | 10.564 | 3.841 | 0.001 | 0.028 | 3.841 | 0.867 | 0.086 | 3.841 | 0.958 | -- | -- | -- |

Observed released Chinook versus expected from number of drifts

| | Z1 | | | Z2 | | | Z3 | | | Z4 | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Year | x2 | test | p | x2 | test | p | x2 | test | p | x2 | test | p |
| 2004 | 1.341 | 5.991 | 0.511 | 9.822 | 7.815 | 0.020 | 36.761 | 5.991 | 0.000 | -- | -- | -- |
| 2005 | -- | -- | -- | 19.129 | 3.841 | 0.000 | 6.049 | 3.841 | 0.014 | -- | -- | -- |
| 2007 | -- | -- | -- | 30.237 | 3.841 | 0.000 | 2.984 | 3.841 | 0.084 | -- | -- | -- |
| 2010 | 1.851 | 3.841 | 0.174 | 39.444 | 3.841 | 0.000 | 18.824 | 3.841 | 0.000 | -- | -- | -- |

Observed released steelhead versus expected from observed kept Chinook

| Year | Z1 | | | Z2 | | | Z3 | | | Z4 | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | x2 | test | p | x2 | test | p | x2 | test | p | x2 | test | p |
| 2004 | 3.075 | 5.991 | 0.215 | 34.212 | 5.991 | 0.000 | 2.742 | 5.991 | 0.254 | -- | -- | -- |
| 2005 | -- | -- | -- | 0.443 | 3.841 | 0.506 | 12.121 | 3.841 | 0.000 | -- | -- | -- |
| 2007 | -- | -- | -- | 1.610 | 3.841 | 0.205 | -- | -- | -- | -- | -- | -- |
| 2010 | 1.441 | 3.841 | 0.230 | 12.681 | 3.841 | 0.000 | 28.058 | 3.841 | 0.000 | -- | -- | -- |

Observed released steelhead versus expected from number of drifts

| Year | Z1 | | | Z2 | | | Z3 | | | Z4 | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | x2 | test | p | x2 | test | p | x2 | test | p | x2 | test | p |
| 2004 | 1.465 | 5.991 | 0.481 | 3.903 | 5.991 | 0.142 | 2.454 | 5.991 | 0.293 | -- | -- | -- |
| 2005 | -- | -- | -- | 8.580 | 3.841 | 0.003 | 6.818 | 3.841 | 0.009 | -- | -- | -- |
| 2007 | -- | -- | -- | 2.766 | 3.841 | 0.096 | 0.771 | 3.841 | 0.380 | -- | -- | -- |
| 2010 | 13.000 | 3.841 | 0.000 | 4.068 | 3.841 | 0.044 | 3.238 | 3.841 | 0.072 | -- | -- | -- |

Appendix Table 4. Chi-square contingency test results across zones, small mesh fisheries, mouth upstream to Willamette River.

| Observed released Chinook versus expected from observed kept Chinook | | | | | Observed released Chinook versus expected from number of drifts | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Areas | x2 | test | p | | Areas | x2 | test | p |
| 3/21/2003 | 1-3 | 6.028 | 5.991 | 0.0491 | 3/21/2003 | 1-3 | 7.992 | 5.991 | 0.018 |
| 3/4/2004 | -- | -- | -- | -- | 3/4/2004 | -- | -- | -- | -- |
| 3/23/2004 | 1-3 | 3.371 | 5.991 | 0.1854 | 3/23/2004 | 1-3 | 11.631 | 5.991 | 0.003 |
| 3/25/2004 | 1-3 | 7.072 | 5.991 | 0.0291 | 3/25/2004 | 1-3 | 34.461 | 5.991 | 0.000 |
| 3/29/2004 | 1-3 | 8.850 | 5.991 | 0.0120 | 3/29/2004 | 1-3 | 24.594 | 5.991 | 0.000 |
| 3/29/2005 | 2-3 | 0.243 | 3.841 | 0.6217 | 3/29/2005 | 2-3 | 1.957 | 3.841 | 0.162 |
| 3/31/2005 | 1-3 | 1.718 | 5.991 | 0.4236 | 3/31/2005 | 1-3 | 13.715 | 5.991 | 0.001 |
| 3/20/2007 | 2-3 | 0.457 | 3.841 | 0.4990 | 3/20/2007 | 2-3 | 0.128 | 3.841 | 0.721 |
| 3/22/2007 | 1-2 | 1.447 | 3.841 | 0.2290 | 3/22/2007 | 1,2,4 | 3.553 | 5.991 | 0.169 |
| 3/30/2010 | 1-3 | 9.049 | 5.991 | 0.0108 | 3/30/2010 | 1-3 | 0.399 | 5.991 | 0.819 |

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 4/7/2010 | 1-4 | 0.976 | 7.815 | 0.8070 | 4/7/2010 | 1-4 | 14.033 | 7.815 | 0.003 |

| Observed released steelhead versus expected from observed kept Chinook | | | | | Observed released steelhead versus expected from number of drifts | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Areas | x2 | test | p | | Areas | x2 | test | p |
| 3/21/2003 | 1-3 | 18.030 | 5.991 | 0.000 | 3/21/2003 | 1-3 | 1.053 | 5.991 | 0.591 |
| 3/4/2004 | -- | -- | -- | -- | 3/4/2004 | -- | -- | -- | -- |
| 3/23/2004 | 1-3 | 9.171 | 5.991 | 0.010 | 3/23/2004 | 1-3 | 2.823 | 5.991 | 0.244 |
| 3/25/2004 | 1-3 | 12.820 | 5.991 | 0.002 | 3/25/2004 | 1-3 | 6.212 | 5.991 | 0.045 |
| 3/29/2004 | 1-3 | 13.334 | 5.991 | 0.001 | 3/29/2004 | 1-3 | 4.165 | 5.991 | 0.125 |
| 3/29/2005 | 2-3 | 11.281 | 3.841 | 0.001 | 3/29/2005 | 2-3 | 4.583 | 3.841 | 0.032 |
| 3/31/2005 | 1-3 | 3.590 | 5.991 | 0.166 | 3/31/2005 | 1-3 | 10.091 | 5.991 | 0.006 |
| 3/20/2007 | 2-3 | 31.360 | 3.841 | 0.000 | 3/20/2007 | 2-3 | 12.007 | 3.841 | 0.001 |
| 3/22/2007 | 1-2 | 0.116 | 3.841 | 0.733 | 3/22/2007 | 1,2,4 | 1.728 | 5.991 | 0.422 |
| 3/30/2010 | 2-3 | 1.077 | 3.841 | 0.299 | 3/30/2010 | 1-3 | 12.241 | 5.991 | 0.002 |
| 4/7/2010 | 1-4 | 20.141 | 7.815 | 0.000 | 4/7/2010 | 1-4 | 6.685 | 7.815 | 0.083 |

Appendix Table 5. Chi-square contingency test results across dates, small mesh fisheries, upstream of Willamette River.

Observed released Chinook versus expected from observed kept Chinook

| | Z4 | | | Z5 | | |
|---|---|---|---|---|---|---|
| Year | x2 | test | p | x2 | test | p |
| 2008 | 4.024 | 5.991 | 0.134 | 0.453 | 5.991 | 0.797 |
| 2009 | 1.384 | 5.991 | 0.500 | 0.082 | 5.991 | 0.960 |

Observed released Chinook versus expected from number of drifts

| | Z4 | | | Z5 | | |
|---|---|---|---|---|---|---|
| Year | x2 | test | p | x2 | test | p |
| 2008 | 18.942 | 5.991 | 0.000 | 5.289 | 5.991 | 0.071 |
| 2009 | 25.035 | 5.991 | 0.000 | 5.449 | 5.991 | 0.066 |

Observed released steelhead versus expected from observed kept Chinook

| | Z4 | | | Z5 | | |
|---|---|---|---|---|---|---|
| Year | x2 | test | p | x2 | test | p |
| 2008 | 1.513 | 5.991 | 0.469 | -- | -- | -- |
| 2009 | 0.951 | 3.841 | 0.330 | -- | -- | -- |

Observed released steelhead versus expected from number of drifts

| | Z4 | | | Z5 | | |
|---|---|---|---|---|---|---|
| Year | x2 | test | p | x2 | test | p |
| 2008 | 0.493 | 5.991 | 0.782 | -- | -- | -- |
| 2009 | 1.078 | 5.991 | 0.583 | -- | -- | -- |

Appendix Table 6. Chi-square contingency test results across zones, small mesh fisheries, upstream of Willamette River.

| Observed released steelhead versus expected from observed kept Chinook | | | | | Observed released steelhead versus expected from number of drifts | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Areas | x2 | test | p | | Areas | x2 | test | p |
| 4/1/2008 | -- | -- | -- | -- | 4/1/2008 | -- | -- | -- | -- |
| 4/8/2008 | 4-5 | 2.270 | 3.841 | 0.132 | 4/8/2008 | -- | -- | -- | -- |
| 4/15/2008 | 4-5 | 0.001 | 3.841 | 0.974 | 4/15/2008 | -- | -- | -- | -- |
| 3/29/2009 | -- | -- | -- | -- | 3/29/2009 | -- | -- | -- | -- |
| 4/7/2009 | -- | -- | -- | -- | 4/7/2009 | -- | -- | -- | -- |
| 4/14/2009 | -- | -- | -- | -- | 4/14/2009 | -- | -- | -- | -- |