Responses to Interagency Project Work Team Comments On the Integrated Modeling Framework for Winter-Run Chinook

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June 2004

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INTRODUCTION

This report describes our response to comments on the Winter-run Integrated Modeling Framework (IMF) that we received from the Interagency Ecological Program (IEP) Winter-run Chinook Project Work Team (PWT). We found the comments to be constructive and we want to provide your team with an accounting of what we do about each comment. At the request of the PWT, we have provided responses to individual PWT comments in a linear fashion based on the order of comments in the PWT memo dated March 23, 2004 (Appendix A). Comments within the PWT memo have been numbered by S.P. Cramer & Associates (SPC&A) to link responses to individual comments. Because there is similarity among some of the individual comments, certain responses refer the reader to a previous response rather than repeat the entirety of the previous response. We feel this approach strikes a balance between the need for individual comment response while minimizing repetition among similar comments.

STEP 1 REPORT

Comment 1

The Step 1 Report includes discussion of several studies and relationships that are not included in the current version of the model. The model documentation should include only those relationships included in development of the current model; it should not be a general thesis on Chinook survival.

We fully agree that model documentation should only include those relationships in the current model; the Step 1 Report served a different purpose and thus included considerable discussion regarding complex relationships within the winter-run Chinook life cycle in the Sacramento River. We have recently completed a draft IMF User's Guide that includes a complete and concise description of model functions and parameters. These descriptions, all in a single document, will include a *summary* of the evidence we relied on. This summary of evidence is necessary to support the multitude of decisions made regarding appropriate parameter values and functions within the IMF. At present, this information is spread among several technical memorandums, and a few parameters have not been described in any narrative.

Comment 2

The documentation should include the intended uses of the model. A clear statement of the proposed uses and limitations of the model is essential.

Primary purposes of the winter-run IMF project are to:

- Provide a basis for prioritizing restoration actions by developing a predictive tool, based on best available science, to compare probable responses of fish populations to a broad range of management actions.
- Identify the weakest links in our understanding of winter-run population biology, and provide a basis for prioritizing research activities.
- Clarify the types of monitoring activities that will be most helpful for insuring that restoration strategies are staying on track.

- Identify measures to accelerate recovery and/or track recovery of ESA-listed salmonids.

The IMF is a spreadsheet-based simulator for use by informed natural resource managers. The model is designed to compare the predicted change in various population metrics (e.g. spawning escapement, recruit per spawner, total catch, etc.) as a result of changing conditions or values of various input parameters (e.g. river temperature, river flow, water export volume, harvest rates, etc.). The outputs of the IMF reflect the quality of the inputs; thus, input parameter values outside a realistic range will produce unreasonable outputs from the model (see response to Comment #33).

The IMF is intended to make transparent the logic and functions of this mechanistic model. As the model becomes more complex, we expect increased complexity of the model spreadsheets. At some point, this complexity – and the analysis needs of the committee - may surpass the ability of a spreadsheet to be an effective platform. This spreadsheet would then serve as a prototype for development of a more sophisticated simulator using a more appropriate platform – which will likely be less transparent to the typical user (i.e., be more "black box"). So, up to that point, this spreadsheet simulator is intended to be a more effective way to communicate with committee members, facilitating their review of the IMF, and actively involving them in model construction.

As with any quantitative model, there is uncertainty associated with the assumptions and data used to develop the model. In the case of the IMF, data specific to winter-run Chinook salmon in the Sacramento River are limited, primarily because of the ESA-listed status of Sacramento winter-run Chinook and the associated restrictions on experimentation with an ESA-listed species. Most data available in the Sacramento River describe conditions for hatchery-reared fall Chinook; thus, to develop a winter-run model, assumptions are necessary to apply fall Chinook relationships to winter-run and hatchery fish relationships to natural fish. These assumptions may or may not be appropriate and each introduces uncertainty to the IMF. However, we have attempted to use the best *available* data to minimize uncertainty; as new and more appropriate data become available, they can be incorporated into the IMF to further reduce uncertainty. Specific winter-run data on the Sacramento River are continually emerging. Users of the IMF should be reminded that, because of the uncertainty associated with the model, model predictions should not be interpreted as absolute numbers that will be realized in the natural environment. Rather, the IMF is useful in predicting trends or evaluating the relative difference of population metrics based on different scenarios of input parameters.

Comment 3

Several very important factors impacting winter-run survival, past and present, have not been included in the current version of the IMF. These were discussed with Steve Cramer at the October 14, 2003 meeting of the Winter-run Project Work Team, but have not yet been included in the model. These factors include:
- *Impacts of Iron Mountain Mine contaminant discharge. Mine discharges had significant impacts on winter-run survival in the upper Sacramento River in past years; this factor*

needs to be included in the model for hindcasting purposes. (Reference the old WPOP model for ways to deal with this factor.)

- *Impacts of ACID dam on winter-run passage. In past years, ACID significantly impacted winter-run passage and spawning distribution.*

- *Impacts of entrainment of winter-run at GCID, RD108, and other major water diversions on the upper river. Prior to the screening of these diversions, these projects had major effects on survival of winter-run juveniles during emigration from the upper river.*

- *Variability in winter-run ocean survival is not accounted for. In the Northwest, ocean survival of Chinook has been known to vary by more than ten-fold from year to year. These changes in ocean survival rates may have much larger effects on the winter-run population than variation in inland survival, yet the model doesn't account for those effects.*

Impacts of Iron Mountain Mine

We agree that Iron Mountain Mine may have been a significant mortality factor in past years. The mine has been identified as the largest discharge of toxic material affecting the Sacramento River area (NMFS 1997). Additionally, NMFS (1997) indicated that discharge from the mine into the Sacramento River has caused massive kills of resident and anadromous fish (Table 1). These data cover the time period from 1940 to 1986; spawning escapement data used in the hindcast simulation started with the return year 1968. Thus, a large part of the available fish kill data (Table 1) does not overlap with the years simulated in the hindcast. Additionally, there are a number of uncertainties regarding the estimated fish kills since 1979, in particular, only events where continuous monitoring occurred are included, mortality calculations are based on conversion of total metals to dissolved metals concentrations, and mortality is based on comparison to separate bioassay results. Because of these uncertainties, it is unknown whether actual mortality is higher or lower than the reported values. Furthermore, the data are not species-specific and are presented as a percentage of fry, not an absolute number of fish killed. Thus, applicability of these data to winter-run Chinook is uncertain. Despite the potential importance of Iron Mountain Mine as a historical mortality factor on winter-run Chinook, we currently lack the specificity in data to adequately model a mortality function in the hindcast simulation.

Table 1. Chinook salmon and steelhead trout mortality episodes in the Sacramento River attributed to trace metal contamination from the Iron Mountain Mine site based on actual observations and bioassay-based calculations (U.S. Fish and Wildlife Service 1959, Nordstrom 1985, CDFG 1978, Finlayson and Wilson 1979, U.S. Environmental Protection Agency 1986, Curtis 1989).

Date	Observation Location	Adult	Juveniles	Estimated Number of Mortalities
1940	Below Shasta Dam	X		Unknown*
Nov 1944	Balls Ferry		X	30% of spawning run*
Winter 1945	Balls Ferry		X	Unknown*
1948	Below Shasta Dam			Unknown*
Apr 1949	Ball Ferry		X	Unknown*
Apr 1955	Redding		X	100,000*
Nov 1955	Keswick Dam	X	X	42*
Feb 1956	Redding	X		Unknown*
Jan 1957	Redding	X		Unknown*
Feb 1957	Redding	X		Unknown*
Feb 1957	Redding	X		25*
Feb 1957	Redding		X	250*
Sept 1957	Redding		X	50,000*
Jan 1959	Keswick Dam	X		422*
Jan 1959	Redding		X	Unknown*
Apr 1959	Redding	X		25*
Dec 1960	Redding			Unknown*
Feb 1961	Redding	X		50*
Feb 1962	Redding	X		98*
1963	Unknown	X	X	>100,000*
Feb 1964	Redding			100,000*
Feb 1966	Redding	X		136 steelhead trout*
Apr 1966	Redding	X		130 steelhead trout*
Jan 1967	Redding	X		785 steelhead trout*
1969	Unknown	X	X	>100,000*
Jan 1978	Redding		X	37% of fry†
Mar 1979	Keswick Dam		X	4 events @ 10% of fry‡
Mar 1979	Keswick Dam		X	1 event @ 50% of fry‡
Feb 1980	Keswick Dam		X	1 event @ 25% of fry‡
Feb-Mar 1981	Keswick Dam		X	13 events @10-20% of fry‡
Nov 1981	Keswick Dam		X	4 events @ 10% of fry‡
Mar 1983	Keswick Dam		X	3 events @ 10% of fry‡
Apr 1983	Keswick Dam		X	1 event @ 10% of fry‡
May 1983	Keswick Dam		X	1 event @ 10% of fry‡
Jun 1983	Keswick Dam		X	1 event @ 10% of fry‡
Feb 1986	Keswick Dam		X	5 events @ 10% of fry‡
Apr 1986	Keswick Dam		X	1 event @ 10% of fry‡
Jun 1986	Keswick Dam		X	Unknown‡

*Actual observations.
‡Mortality estimates reported by Rectenwald (1989) for Spring Creek spill episodes only for events when water quality was constantly monitored. Mortality calculations were based on adjustments of reported total metals concentrations to dissolved values and comparisons of exposures to bioassay results in Finlayson and Verrue (1982).
†Based on an in situ bioassay of eggs and fry.

Impacts of ACID Dam

The Anderson-Cottonwood Irrigation District (ACID) dam was a complete barrier to upstream fish passage when first constructed (1917). The initial fish ladder (1927) was ineffective at passing upstream migrating fish; passage was later improved. Despite the fish passage limitations, the effects of the ACID dam on winter-run Chinook are minimized because of two main factors: the timing of operations of the ACID dam and the potential importance of the ACID dam-to-Keswick Dam reach in relation to the total potential winter-run spawning area.

First, annual operation of the ACID dam typically involves installing the flashboards in early April. Winter-run Chinook migration timing data suggest that, in wet years, about 50% of the run has past Red Bluff Diversion Dam (RBDD) by March; in dry years, migration is generally earlier and about 72% of the run has past RBDD by March. Based on these migration timing data and the temporal operation of the ACID dam, it appears as though the ACID dam may only affect a portion of the run.

Second, most available winter-run spawning distribution data indicate that spawning may occur in the 90 miles of river between RBDD and Keswick Dam. With operational changes at RBDD and Keswick Dam, potential winter-run spawning area appears to extend below RBDD. The reach from ACID dam-to-Keswick Dam is about 3 miles; this represents a small portion of potential winter-run spawning area. Aerial spawning escapement surveys indicate that only about 2% of the total winter-run spawns above ACID dam. It is not clear whether unimpeded access to this reach would substantively increase winter-run Chinook spawning production capacity. Throughout the development of the winter-run Chinook IMF, we have attempted to focus on the factors that have the most significant effects on winter-run survival and abundance. In the case of the ACID dam passage problems, removal of 3 miles of spawning habitat should have very little effect on annual winter-run spawning and juvenile production when an additional 87 miles of potential spawning habitat exist between ACID dam and RBDD.

From a hindcast simulation perspective, we lack the data to incorporate specific winter-run blockage at ACID dam on an annual basis. Furthermore, the hindcast and predictive versions of the winter-run Chinook IMF currently calculate annual fry production using a density independent function, based on the assumption that spawning habitat is not a limiting factor of winter-run Chinook. That is, fry production is based on escapement, percent females, fecundity, temperature mortality, and an average egg to fry survival. Thus, the periodic passage problems at the ACID dam and the resulting loss of access to 3 miles of spawning habitat does not affect current fry production estimates.

Entrainment GCID and other Irrigation Diversions

We agree that entrainment at water diversions can have significant impacts on juvenile winter-run Chinook survival during emigration. Currently, we do not have the data to support whether these impacts are positive or negative. Water diversions are commonly associated with negative impacts, such as direct mortality during entrainment or stranding and isolation from the river system. Occasionally, there are unexpected positive impacts resulting from water diversions. In situations where diversions later

reconnect to the river system, fish that survive initial entrainment may experience better growth and higher survival in the diversion channel compared to the river system, depending on the environmental conditions in each location. This situation has been observed with spring Chinook in the Sutter Bypass in the Sacramento River system.

The fish screens at the GCID water diversion have been ineffective at preventing juvenile salmon entrainment; periodic updates have improved conditions, but the GCID screens remain ineffective (Cramer et al. 1992, NMFS 1997). Cramer et al. (1992) observed juvenile salmon movement downstream and upstream through the fish screens, indicating substantial gaps existed. This also suggests that fish entering the irrigation canal should not be considered a complete loss. However, length data collected at GCID are somewhat inconsistent, potentially because of the time period over which data were collected. Fish length has substantial implications on swimming ability and ability to avoid diversions. Decoto (1978) observed a mean fork length of 32mm in September for entrained fish; Ward (1989) captured fish at the screens from August through October averaging less than 41mm. Cramer et al. (1992) observed a larger average length of over 80mm over the April 1 to September 30 time period, which encompasses much of the GCID water diversion period. At this length, Chinook are likely able to avoid entrainment or impingement on the fish screens. Cramer et al. (1992) did observe a slightly smaller average length of entrained fish versus fish passing the GCID intake.

Comparison of GCID operations and winter-run emigration timing suggests minimal overlap and potential entrainment. During the April 1 to October 31 GCID diversion period, pumping volumes are generally highest during early May and from mid-June to mid-August (GCID et al. 1989). Juvenile emigration data suggest that only about 10% of winter-run Chinook passage at GCID occurs by August (Figure 1). Peak winter-run movement past GCID occurs in October and November, when pumping volume is low or pumping has ceased for the season.

Relative Size and Abundance of Juvenile Winter-Run Chinook at GCID: 1998-2003
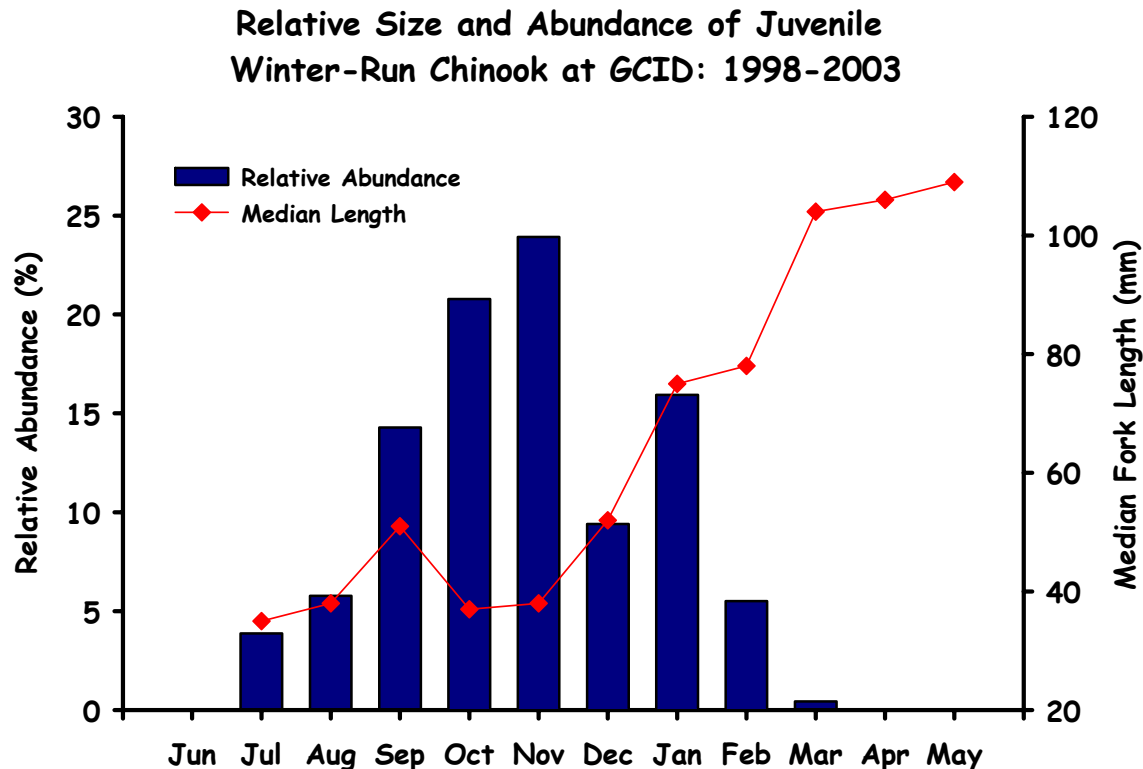

Figure 1. Relative abundance and median length of juvenile winter-run Chinook salmon caught at a USFWS screw trap located in the GCID bypass channel from 1998- 2003.

From the perspective of the hindcast simulation, we lack adequate data to quantify species-specific annual losses of winter-run Chinook to upper Sacramento River water diversions. Thus, without these data and because of difficulties in determining the effects of each diversion, these impacts were not included in the hindcast simulation.

Variability In Winter-Run Ocean Survival
We agree with the PWT sentiment that ocean survival can have substantial effects on annual abundance of winter-run Chinook. A stochastic function that incorporates a range of ocean variability could be added to the IMF; however, that would diverge from the primary purpose of the IMF (see Comment #2). For example, stochastic ocean variability in the IMF would always produce different results, regardless if the input parameter values were changed. Because the IMF is intended to evaluate the effects of changing parameter values, a stochastic ocean variability function would negate the IMF capabilities for comparing such changes.

We currently have no basis in which to calculate annual ocean variability, either in a predictive, forecast manner or in a hindcast manner. Consider a comparison of a winter-run Chinook ocean survival index with survival to age 2 estimates for fall-run Chinook from the Feather River and Coleman hatcheries (Cramer and Chapman 2002; Figure 2). The winter-run ocean survival index was calculated using the hindcast IMF. We ran the hindcast model with available observed data for input parameter values and the observed spawning escapements as inputs for annual spawners. We compared the

predicted spawning escapement from this hindcast model run with the actual observed annual spawning escapements. This ratio provided an index of annual ocean variability because the predicted spawning escapements only incorporated freshwater parameters while the observed escapement includes both freshwater and ocean effects. The winter-run survival index and the fall-run survival to age 2 values were standardized by comparing each individual value to the mean value for the time period 1978-1997 (this time period was chosen because it represents the overlap of available data). Patterns of annual ocean variability are dissimilar for all three populations (Figure 2). There is no correlation among the ocean variabilities for these three populations (Table 2).

Annual Variation in Ocean Survival Index for Winter-run Chinook, 1978-1997

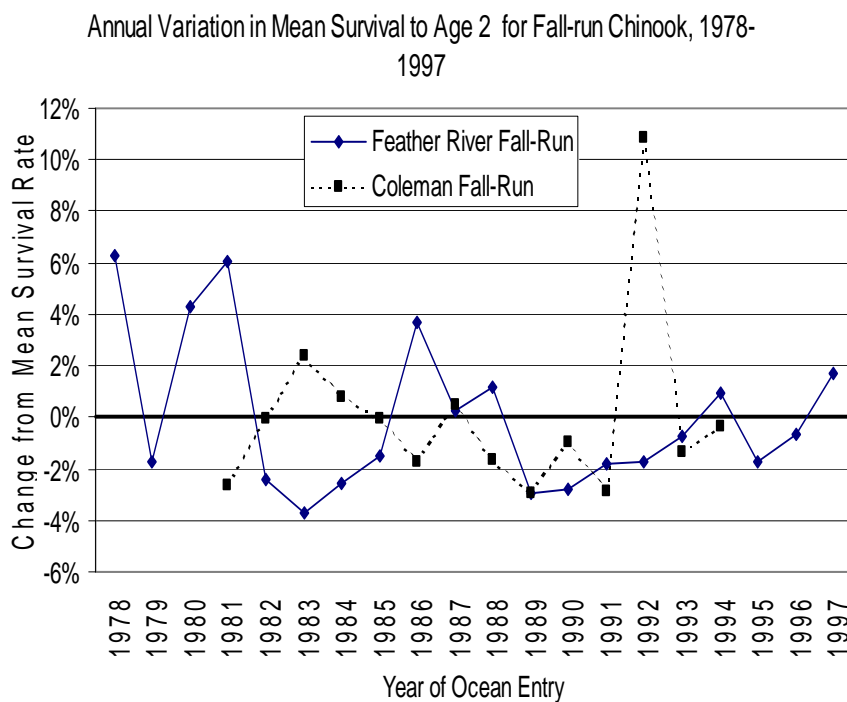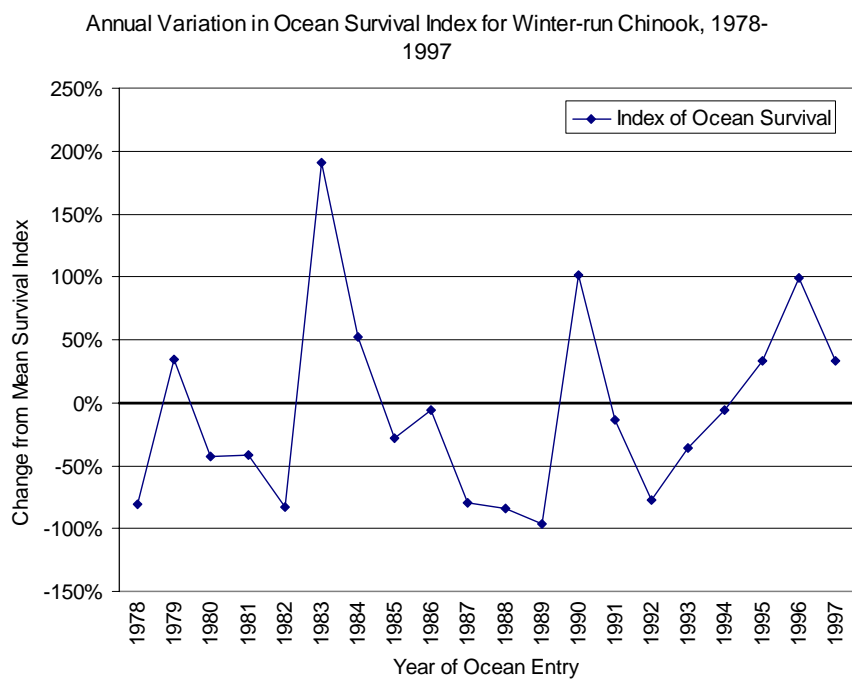Annual Variation in Mean Survival to Age 2 for Fall-run Chinook, 1978-1997

Figure 2. Comparison of ocean variability for winter-run Chinook and fall-run Chinook from the Feather River and Coleman Hatcheries, 1978-1997.

Table 2. Correlation coefficients for the winter-run Chinook ocean survival index and the survival to age 2 for Feather River and Coleman Hatcheries, 1978-1997.

	Winter-Run Chinook Survival Index	Feather River Hatchery Fall-run	Coleman Hatchery Fall-run
Winter-Run Chinook Survival Index	1	-	-
Feather River Hatchery Fall-run	-0.38	1	-
Coleman Hatchery Fall-run	0.04	-0.29	1

Comment 4

One of the basic assumptions of the model is that "habitat for successful spawning and egg incubation above Red Bluff Diversion Dam is the bottleneck for production throughout the lifecycle" (page 42). The part of the model predicting spawning and early rearing is based on the SALMOD model, which assumed that early survival is directly related to a flow/habitat relationship. We strongly disagree with these assumptions and use of the SALMOD model for upstream areas. First, is habitat for spawning, egg incubation, and fry rearing a limiting factor for winter-run in the upper Sacramento River, at current population levels? If you consider that, in recent history, over 100,000 winter-run Chinook utilized the mainstem river for spawning and rearing, and in recent years, only about 8,000 winter-run have returned each year, it is doubtful that habitat quantity currently limits production. If spawning and rearing habitat were limiting, it is doubtful that availability of suitable habitat is directly related to flow. Flows in the upper river are relatively high and stable during winter-run spawning and early rearing and are unlikely to be limiting winter-run habitat.

We agree with the main point of this comment, namely that spawning and early rearing habitat do not appear to be the limiting factor for winter-run Chinook annual production. SALMOD and weighted usable area also support this premise. Nevertheless, this comment refers to an older version of the IMF. In the former version, the weighted usable area and fry capacity values derived from SALMOD and utilized in the IMF did not limit winter-run production at current or potential future population levels. For example, based on weighted usable area, fry capacity, and the number of miles of rearing habitat (i.e. 95, as suggested by the PWT), potential fry production was 17,153,316. If this level of fry production were realized, runs in excess of historical maximums would be possible, even with conservative mortality factors throughout the life-cycle.

Regardless, this portion of the IMF has been revised substantially in the most recent version (V1.2). Using observed winter-run data for various parameters (e.g. spawning escapement, maturity rates, harvest rates, hatchery percent, male/female ratio, fecundity, RBDD blockage, temperature mortality, etc.), we completed a cohort analysis to forward calculate annual fry production and back calculate annual smolt production, by brood year. These results were used to establish a Beverton-Holt stock recruitment curve. At present, the IMF calculates annual fry production in a density independent manner, using the number of females in the escapement, an average female fecundity, and an average egg to fry survival rate based on the Juvenile Production Estimate (JPE) and recent USFWS research on the upper Sacramento (Martin et al. 2001). A recently published IMF User's Manual discusses fry density dependence and development of the Beverton-Holt relationship in detail.

Comment 5

The model also assumes a maximum returning adult to spawner ratio of 7.5. Winter-run cohort replacement rates in some recent years have exceeded this value. This should not be a constraining factor in the model.

The assumed Ricker stock recruitment curve parameters are no longer used in the IMF; see Comment #4 regarding the current use of a Beverton-Holt stock recruitment curve in the most recent IMF.

Comment 6

The model assumes a smolt to age 2 survival rate of 5%, based on fall-run tag returns from Coleman National Fish Hatchery. This rate should be based on winter-run data from cohort reconstructions.

The model now assumes a smolt to age 2 survival rate of 4%; this assumption remains based on the range of estimates from CWT recoveries of surrogate fall-run Chinook from Coleman National Fish Hatchery (Cramer and Chapman 2002). We are unaware of the availability of estimates natural winter-run Chinook early ocean survival. We found no evidence of an estimate of smolt-to-age 2 survival rate in the research by Grover et al. (2004). Any winter-run Chinook specific data available for an estimate of smolt-to-age 2 survival would be considered for incorporation into the IMF.

Comment 7

The documentation includes outdated genetics data from the winter-run carcass survey. Recent years' data should be obtained from USFWS, Red Bluff Office.

Data in Table 7 on page 50 in the Step 1 Report were presented to support the winter-run adult migration timing at RBDD; however, the genetic data have not been used to set specific functions or parameter values in the IMF. These genetic data are not included in the user's guide and model documentation report that was recently completed, based on comments of the PWT to include only those data and relationships relevant to the current IMF version.

EXECUTIVE SUMMARY

Comment 8

Page ii – Pulse flows to aid smelt migration? (If "smolt" migration is intended here, pulse flows aren't used as a management tool in the upper Sacramento River.)

The list of management actions on page ii in the Executive Summary was derived from the "Management Issues to be Addressed" section beginning on page 5 in the main body of the report. This section indicates that the list represents *proposed* key actions to achieve the recovery of winter-run Chinook, based on recommendations from the ESA Recovery Plans and Biological Opinions, the CALFED ERP Implementation Plan, other CALFED Programs, as well as suggestions of biologists from NOAA Fisheries, USFWS, and CDFG.

Comment 9

Page iii – Paragraph 1 – It's highly unlikely that suitable habitat for spawning is limiting the winter-run population at current run sizes. There is no evidence of this occurring.

We agree that winter-run Chinook are unlikely spawning habitat limited (see Comment #4). Additionally, paragraph 1 on page iii summarizes information presented in the Egg Survival and Emergence section beginning on page 10 in the main body of the Step 1 Report. The paragraph does not suggest that spawning habitat alone limits winter-run production, rather, that suitable spawning habitat that also has suitable water temperature throughout the incubation period *may* be limiting. The incubation period extends from March through August; peak emergence timing is September (see Figure 6, page 13, Step 1 Report). As early as April, water temperatures (Table 2, page 12, Step 1 Report) can be high enough to increase mortality in eggs and juveniles (Figure 5, page 11, Step 1 Report). In actuality, it is the incubation water temperature that may introduce the limitation.

Comment 10

Page iii – Paragraph 2 – Due to the highly managed flows downstream of Shasta and Keswick dams, flows in the upper river are relatively stable during winter-run spawning and early rearing periods. Flood flows are typically not a factor affecting egg survival.

We agree with the comment. The statements in paragraph 2 on page iii are general statements regarding the effects of flooding on egg mortality. We have not presented data to suggest such relationships exist on the Sacramento River. There are currently no functions within the IMF to incorporate egg mortality because of flooding; based on available data, we have no intention of incorporating any such egg mortality factor.

Comment 11

Page iii – Paragraph 5 – Vulnerability to entrainment – these statements may or may not be true, depending on the particular situation.

We agree with the comment. Entrainment is affected by many additional factors, such as the location of outmigration fish, the location of the diversion, the location of each in relation to the other (e.g. inside vs. outside bend, top of the water column vs. bottom of the water column), proportion of flow diverted compared to river flow, and the size of fish (i.e., swimming ability).

Comment 12

Page iii – Paragraph 6 – These statements are too simplistic and don't represent the true relationships. For winter-run Chinook, we don't have evidence linking survival during migration to flow – survival is negatively related to temperature, but only when temperatures exceed certain thresholds – and we don't know the relationship between survival and the number of channel junctions along the migration route. Last sentence - The number of juvenile winter-run entrained at the Delta facilities each year is not directly correlated to the magnitude of Sacramento River flow, or export rates. Delta Cross Channel gate closures appear to be important, but only in certain time periods.

As part of the Executive Summary, these statements are intended to be broad and general; the topic of juvenile survival through the Delta was expanded within the main body of the Step 1 Report (see pages 16-22 and 28-30).

We do not have evidence specific to winter run to establish the *true* relationships between winter-run Chinook emigration survival and river flow, river temperature, export rates, Delta Cross Channel (DCC) gate position, or number of channel junctions. As a result, it is necessary to infer winter-run Chinook relationships based on relationships for other species. The USFWS has focused much research effort on release groups of coded wire tagged (CWT) fall Chinook in an attempt to establish emigration survival factors through the Delta. Newman (2003) analyzed paired CWT release groups and established a multiple regression equation to estimate Delta migration survival that incorporates river flow, river temperature, export volume, salinity, turbidity, and DCC gate position. At present, this equation represents one of the best available means to estimate emigration survival through the Delta and has been incorporated in the current version of the IMF. The Newman (2003) analysis was not available at the time the Step 1 Report was published.

Comment 13

Page iii – Paragraph 7 – Winter-run experience lower harvest rates than fall-run Chinook due to their smaller size-at-age, AND due to specific harvest regulations designed to reduce harvest on winter-run (delayed recreation season openings, etc).

We agree that harvest regulations have limited winter-run Chinook harvest rates compared to fall-run Chinook; omission of such a statement in the Step 1 Report was not intended to communicate that harvest regulations are not a factor. Additionally, the current version of the IMF has incorporated recently released harvest impact rates for winter-run Chinook from the 1998-2000 brood years (Grover et al. 2004). These harvest rates are higher than PFMC harvest managers originally expected.

Comment 14

Page iv – Paragraph 3 – The WCOHM model is not currently used by DFG for setting ocean harvest regulations. It is considered outdated; uses data from the '70's.

At the time of publication of the Step 1 Report, we recognized that the CDFG Winter Chinook Ocean Harvest Model (WCOHM) was considered outdated; however, no new data were available to replace the WCOHM. Thus, at the time, the WCOHM represented the best *available* data for harvest impacts, maturation rates, and overwinter mortality. In the meantime, Grover et al. (2004) completed a cohort analysis of Livingston Stone hatchery winter-run Chinook for the 1998-2000 brood years; this analysis now represents the best available data and has been incorporated in the IMF.

Comment 15

Page iv – Last paragraph – It is unlikely that physical habitat for winter-run fry at current population levels is a limiting factor. The upper Sacramento River is very large, and there appears to be no limitation of suitable habitat.

We agree with the comment; see response to Comment #4.

Comment 16

Page v – Critical uncertainties – The ocean life phase needs much more attention in the model. Ocean survival can vary significantly, and may have an even greater effect on population size than inland conditions. Factors related to salmon ocean survival should be included in the model.

Ocean survival factors (i.e. harvest rates, maturity rates, overwinter survival) have been incorporated in the IMF, based on the recently released cohort analysis of Grover et al. (2004). Variability in ocean survival is unpredictable; this variability is inappropriate for a mechanistic model intended to evaluate the response of specified management actions or projects (see Comments #2 and #3). Inclusion of annual ocean variability in the winter-run IMF would make it impossible to identify the effects of management actions.

Although there are difficulties in estimating ocean variability in a predictive manner (see Comment #3), the IMF user can investigate the effects of ocean conditions on the population response to management actions by altering various mortality factors that are affected by ocean conditions (e.g. smolt-to-age 2 survival, adult overwinter survival). Additionally, the Beverton-Holt relationship (see Comment # 4 and IMF User's Guide)

developed to model the fry to smolt density dependent relationship may provide a means for addressing ocean effects. Development of the Beverton-Holt function incorporated freshwater effects, as well as adult harvest impacts. The remaining variability not explained by the Beverton-Holt equation (i.e. the residuals) may include ocean effects; additional study is needed to investigate this possible relationship.

FIRST TECH MEMO DATED NOVEMBER 20, "TESTING THE MODEL: SPAWNER ABUNDANCE"

Comment 17

The hindcast simulation appears to simulate the decline and recent recovery of the winter-run population. However, there are many ways to model a similar pattern, and an apparent fit between observed and predicted data does not necessarily indicate that the model has incorporated the appropriate variables in the correct manner. In the general comments section (above), we have listed several important factors affecting winter-run survival that have not been included in the model. Without these factors, it is doubtful that the model is generating a valid hindcast of winter-run escapement.

See response to Comment #3.

Comment 18

Page 2 - 2nd paragraph - Last sentence - "Spawning escapement estimates via carcass surveys have been deemed more reliable" should be changed to read "Beginning in 2001, spawning escapement estimates based on application of the Jolly-Seber model to carcass survey data have been considered the best available estimate of winter-run escapement." (The official winter-run escapement estimate through 2000 was based on RBDD count data.)

Although the replacement sentence provided in this comment may be accurate, there is currently no support or impetus for updating and publishing revised Technical Memorandums. Additionally, subsequent paragraphs within the Technical Memorandum identify which escapement estimation methods were used for each run year based on recommendations by fisheries management agencies.

Comment 19

Page 2 - Last paragraph - For historical run size, the simulation uses three very different measures of winter-run escapement: RBDD counts, the Petersen model estimate, and the Jolly-Seber model estimate from the carcass survey data. For this simulation, we recommend using a consistent long-term measure of escapement, the estimate based on RBDD counts. Otherwise, you're mixing apples and oranges in a long-term trend analysis. The RBDD estimates show the same trend as the carcass survey data in recent years.

There does not appear to be any *consistent* long-term measure of escapement. Comparing the RBDD counts themselves amounts to comparing apples and oranges as a result of differing operations and methods used to derive escapement estimates, particularly after 1987. NMFS (1997) suggested that the extrapolation of RBDD counts based on run timing lead to estimation errors ranging from 43% to 230%. We have used escapement estimates that are generally accepted by fisheries management agencies as the most accurate; as such, we believe these escapement data provide the best measure of long-term trends.

Comment 20

Egg mortality due to elevated water temperatures has not been a major problem in recent years, but in drought conditions in the future, elevated water temperatures may again be a source of egg mortality. This factor should therefore be included in the baseline IMF.

We have revised how the IMF calculates egg and fry production (see Comment #4). Part of these revisions includes the ability to alter a temperature input parameter that affects egg to fry mortality. However, because of the preliminary framework of the IMF, the input parameters effect all years of the simulation and currently cannot be adjusted on an annual basis; this adjustability is a goal of future model revisions.

SECOND TECH MEMO DATED NOVEMBER 25, "TESTING THE WINTER-RUN CHINOOK MODEL: HISTORICAL HARVEST RATES"

Comment 21

The analysis approach used in this report is no longer considered valid. The agencies involved in ocean harvest management abandoned this approach four years ago. The WCOHM model is no longer used. The current method using cohort reconstructions should be available soon.

See response to Comment #14.

Comment 22

The assumption that ocean harvest rates of winter Chinook are proportional to the CVI is not valid. The assumptions about lure size and size of catch are also not valid in California fisheries. These concepts were developed for northern coho fisheries.

We disagree and feel that these assumptions are valid based on the available data. The CVI harvest index is proportional to age-3 winter Chinook harvest (age 2 harvest does not occur because they have not yet reached the minimum size when the fishery is open and age 4 harvest is negligible because most winter Chinook currently mature at age 3) (Figure 3). Additionally, size restrictions have been used in California marine fisheries for quite some time; thus, fish have attained a larger size by the time they are harvested.
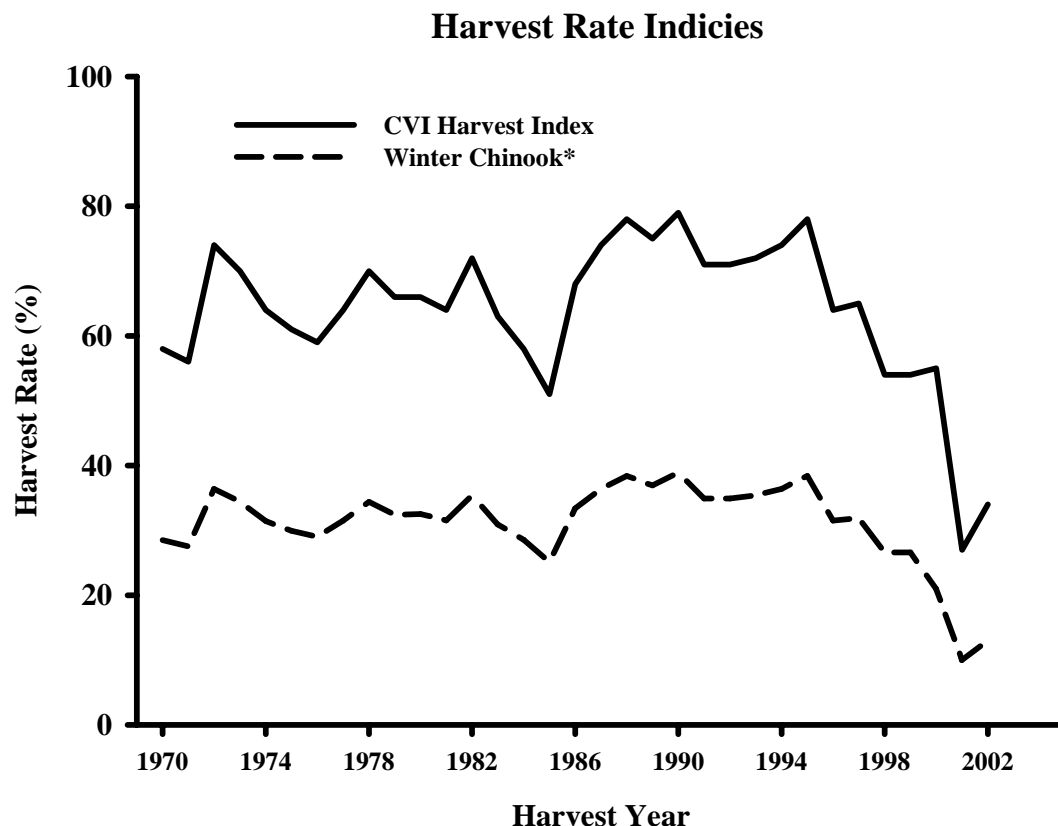
Harvest Rate Indicies

Figure 3. Marine harvest rate comparison of the Central Valley Index and winter-run Chinook (*only age 3 winter-run Chinook are included, which constitutes most of the catch).

Comment 23

The DFG Ocean Salmon Project and NOAA Fisheries should be consulted for current modeling efforts for ocean harvest management.

PFMC, NOAA Fisheries, and CDFG have been consulted regarding Chinook salmon ocean harvest management.

REVIEW OF MODEL SPREADSHEET

Comment 24

The model in its current form uses information that has been available and used for years. Much of this information has severe limitations, usually associated with system complexity, natural variation, and logistics of data gathering that we have noted and taken into account for years. These limitations have been a source of discussion for a long time within the WRSPWT and other forums. The spreadsheet should make the calculations and information used to make them more transparent. However, we are not sure that it does that in its current form. We are concerned that the spreadsheet might give some the impression that we have more confidence in these calculations and the data used to make them than we really do. Also, at this point, we are not convinced that the model reliably and

accurately predicts and interprets the effects of different scenarios on the winter-run population. Largely this is because some factors seem to affect the outcome in unexpected ways and some factors seem to be more influential than they really seem to be (see below), because the outcomes seem to be difficult to interpret and compare, and because variation is not taken into account.

See response to Comment #2, which addresses uncertainty and the subsequent series of Comments (25-38) that address the specific spreadsheet comments. Additionally, the recently completed draft IMF User's Guide should help to illustrate the transparency of the IMF.

Comment 25

The copy that we received for review did not work as described in the instruction sheet. The "use defaults" and other buttons did not work on that copy. We also had difficulty downloading the website version.

The website version of the model (www.spcramer.com/imf.htm) is accessible by clicking the "IMF Access" link and using the password "mykiss". The web version matches the most recent version available at SP Cramer. Both versions appear operable; specifically, the "use defaults" button and other automated functions on both versions appear operational.

Comment 26

It seems that natural variation is not incorporated into the model predictions. Once conditions are set, they don't change over the course of the predictions. How would the predictions change if natural variation were taken into account? At relatively small population sizes for example, one might see a large effect of large amplitude variation. This all gets at how accurate the predictions are, even relative to one another with various actions. The amount of overlap between two outcomes when variation is taken into account, and the expected prevailing conditions (e.g., are we in a drought cycle?), could affect the decision that you make.

One of the objectives of the winter-run Chinook IMF is to evaluate the effects of a particular action or suite of actions on any number of population metrics; thus, incorporating natural variation is not consistent with this purpose (see Comments #2, #3, and #16). Further, the IMF user has a degree of control over the "expected prevailing conditions" based on the values used for each input parameter.

Comment 27

The only way to mimic natural variation was to run the model with successive values and view the results. This is pretty cumbersome. When we did this with, e.g., ocean survival, number of eggs per female, and hatchery contribution, the scale seems to change drastically. So much so that it is hard to see how we could use this practically to make comparisons, knowing that the range of scenarios

includes both very bad and very good conditions. Also, not incorporating variation into the model isolates each trial in a way that seems to allow whatever model run(s) that one chooses to be easily misinterpreted as "the answer".

See response to comments #2, #3, #16, and #26.

Comment 28

How do the authors envision that we will compare model results? Let's suppose that we want to know the number of spawners expected with two different export flows. How will we decide whether a comparison exhibits a significant difference? Can the authors suggest some sort of trend analysis that can be used with individual predictions to determine whether it causes a significant increase or decrease in population size, and a way to statistically compare different scenarios.

This comment illustrates the wide range of potential users and uses of the IMF. Because the IMF can be used to evaluate specific factors within the full winter-run Chinook life cycle and because of the diversity of winter-run Chinook stakeholders, the determination of a significant difference in population metrics will likely mean something different for each IMF user. At present, the IMF has a "benchmark" function that allows multiple scenarios to be compared to an established benchmark condition. The results of these comparisons can be transferred to any number of software programs for further statistical analysis, based on the needs of the IMF user.

We are interested in further exploration of the concept of "performance measures" for IMF outputs and comparing different scenarios. We feel that performance measures would be best developed through a collaborative approach among the agencies and stakeholders.

Comment 29

The number of hatchery fish seems to have an exaggerated effect on the predicted spawner escapement. The scale changes drastically when the hatchery input is changed from 200,000 per year to 0 per year. It seems unreasonable to think that the hatchery input will make that much difference. In 2001, USFWS estimated that 513 of the returning spawners were of hatchery origin, out of an estimated spawning run of 7,996 fish. Based on CWT recoveries, 27.4% were two-year-olds, and 72.6% were three-year-olds. The model predicts more of an effect of hatchery fish on spawner abundance than we are actually seeing.

This comment has helped us to evaluate how hatchery fish are treated in the model. Previously, hatchery fish were injected into the model below the Delta; therefore, hatchery fish experienced a post-release mortality and the same harvest and ocean overwinter mortality as natural fish of the same age. Under this scenario and using the default model parameters, the model predicted that hatchery fish comprised 12.7% of the spawning escapement at equilibrium. This fraction of hatchery fish in the spawning

escapement is slightly higher than the USFWS 2001 estimate of 513 spawners in a spawning escapement of 7,996 fish, or 6.4% hatchery fish.

As a result of this comment, we have changed the manner in which we treat hatchery fish. Hatchery fish are injected into the model above RBDD to reflect the release location at Caldwell Park in Redding. Hatchery fish experienced an initial post-release mortality and then a Sacramento River migration mortality rate, based on CWT data for late-fall Chinook migration from Battle Creek to Ryde. Upon reaching the Delta, hatchery fish are subject to the Delta survival function and then they experience the same harvest and ocean overwinter mortality as natural fish of the same age. Under this new scenario and using the default model parameters, the model predicted that hatchery fish comprise 4.9% of the spawning escapement at equilibrium, which is similar to the USFWS 2001 estimate.

It is reasonable to expect the scale of spawning escapement to change dramatically when hatchery smolt production is decreased from 200,000 to 0, because this is a dramatic change in production. It is important to remember that input parameter values need to be within a range of reality for the IMF to produce reasonable results. At this point, it is not realistic to model 0 hatchery smolt production because USFWS intends to maintain annual production near 200,000 for the foreseeable future. If realistic decreases in hatchery production are simulated in the model, the spawning escapement scale appears to change at reasonable levels. Furthermore, changes in the model as described in the previous paragraph have reduced the prevalence of hatchery fish in the spawning escapement.

In regards to the CWT data presented in this comment, it is not clear what species or brood years were utilized to derive the spawning escapement age composition. We have utilized maturity rates published in Grover et al. (2004) to determine the age composition of the spawning escapement; these data represent the most recently released information specific to winter-run Chinook.

We have not incorporated a hatchery fitness function into the IMF, per se. Hatchery fish do experience post release mortality and an emigration mortality based on CWT data.

Comment 30

The exaggerated hatchery effect stems from a major problem with the model: from Simulation Year 7 forward, density-dependence for fry is the major factor controlling production of naturally-produced juvenile winter-run in the model. The model limits production in the upper river to only 5.6 million fry. That is the reason the predicted spawning returns in the model flatten out, or fail to increase, after a few years. But we know that the river can support more than 5.6 million rearing fry – otherwise, how can we explain the winter-run escapements of the late 1960's/early 1970's of over 100,000 adults? If we ran the model as currently written to compare the relative value of restoration actions, any action to improve fry survival would have exaggerated effects on the population, compared to actions taken to improve survival of other life stages.

See response to Comment #4 for current fry capacity discussion and Comment #29 for hatchery effects.

Comment 31

The calculated Delta survival rate appears too high in the model. The model predicts a survival rate of 91.27% through the Delta, at 15,000 cfs Sacramento River flow, 59 F, 20% E/I ratio, and DCC gates closed. This survival rate under those conditions seems very high. If we ran the model as currently written to compare the relative value of restoration actions, any of the actions taken to improve survival through the Delta would have an extremely small effect on the winter-run population.

This comment refers to a Delta survival function used in a previous version of the IMF; see response to Comment #12 for a discussion of the current Delta survival function. It is important to note that, although the Newman (2003) equation represents our best available method for estimating Delta survival, there are certainly limitations. The Newman (2003) equation was developed with fall-run Chinook; migration timing of winter-run Chinook results in different environmental conditions in the Delta. In particular, river flow and water temperature during the time of winter-run presence in the Delta are more favorable on survival than conditions experienced by fall-run Chinook. Nevertheless, most environmental conditions experienced by winter-run Chinook are within the range of data used by Newman (2003) to develop the Delta survival equation. Additionally, the survival estimates produced by the IMF are similar to those estimated by USFWS based on CPUE ratios estimated in the Sacramento and Chipps Island trawl sampling programs (survival ranged from 0.7 to 0.95).

Comment 32

The authors may want to take into account that USFWS in 2001 noted that the proportion of returning two-year-old hatchery fish was greater than the proportion of two-year-old natural origin winter run. It isn't known whether this is a difference between the two groups or whether it is a statistical anomaly. If it is a true relationship, then the model might be changed to account for it.

We recognize that maturity rates are often different between hatchery and wild fish, with hatchery fish often maturing earlier because of the superior juvenile growth possible in a controlled hatchery environment compared to the natural environment. As a result of recently released data, we are now able to include different maturity rates for hatchery and wild fish within the IMF. For hatchery fish, maturity rates from the recently released cohort analysis of winter-run Chinook 1998-2000 brood years (Grover et al. 2004) were utilized to determine spawning escapement age composition. For wild fish, we used maturity rates based on a cohort reconstruction from age composition of 2001 wild spawners.

Comment 33

We tried setting the Sacramento flow and export flow both to 37,000 cfs and the model still predicted a relatively constant 4,000 to 5,000 spawners. We also set the overall survival rate of juveniles in the Sacramento River to 0%, or in the Delta to 0%, and the model still predicted over 4,000 spawners returning. Similarly, we increased the three-year-old harvest rate to 50% and still got over 10,000 fish over 15 years. This seems unrealistic, even for comparison purposes.

This comment addresses two separate issues: Delta survival (also see Comments #12 and #31) and hatchery fish effects (also see Comments #29 and #30). Additionally, this comment accentuates an extremely important point in running scenarios with the IMF. IMF Technical Memorandum #3 discusses the Delta survival function and Table 3 within Tech Memo 3 displays the data used to develop the Delta survival function. Note that, although the function has recently been modified in the IMF, the range of data used to develop the function remains the same as in Tech Memo 3. If an IMF user inputs data beyond the range of the data or outside the bounds of realism, the Delta survival function may not accurately estimate Delta survival and the resulting predictions of the IMF will be inaccurate. In this comment presented by the PWT, Sacramento River flow and export flow both set at 37,000 cfs is not a realistic scenario, thus, the Delta survival function cannot calculate a realistic output.

The scenario where Sacramento River or Delta survival is set to 0 has been addressed in Comment #29. The 4,000-5,000 spawners produced in the previous version of the IMF under this scenario was an artifact of where hatchery fish were input in the model calculations. If these survival rates are set to 0 in the current IMF, the winter-run predictions will go to extinction.

In regards to the harvest rate portion of the comment, the current version of the IMF produces similar results as suggested in the comment. The default age 3 ocean harvest rate is 21% based on the recent cohort analysis (Grover et al. 2004); increasing this one mortality factor to 50% allows for sizable escapement. If all other mortality factors and input parameter values in the IMF remain constant, this result does not seem unreasonable. Note that, at this age-3 harvest level, age 2 fish comprise approximately 20% of the total escapement at the 15-year increment.

Comment 34

In the associated technical memo called "Hindcast Simulation", there was no hindcast simulation. What happened to the simulation?

There has not been a Technical Memorandum titled "Hindcast Simulation"; the hindcast model was discussed with the Technical Memorandum titled "Testing the Model: Spawner Abundance". Results of the hindcast simulation have also been included in multiple presentations. Regardless, the hindcast model can be made available to any interested PWT member. Note that the hindcast model has been modified to maintain consistency with updates to the IMF.

Comment 35

The model overestimates age 4 fish in the spawning escapement compared to age 3. The model estimates the age 4 component to be approximately 19% of the age 3 component, but in recent years, the composition of the spawning escapement has been more on the order of 5% age 4 fish.

Maturity rates have been revised; see Comment #32.

Comment 36

The calculation of the Spawning Escapement/# Spawners ratio (a cohort replacement rate) is incorrect in the model. The spreadsheet formula shows this ratio to be # of spawners in year x multiplied by the pre-spawning mortality rate, divided by the sum of the # spawners in years x - 4 and x – 3.

This row in the model has been modified relative to the version addressed in the comment. Row 100 is now labeled "Adult Recruits/Spawners"; the formula divides the total run (i.e. catch plus escapement) by the total number of adults in the spawning escapement (i.e. age 3 and age 4 escapement).

Comment 37

There are numerous other mathematical errors in the spreadsheet. For example, on line 174 of the worksheet, the Total Run Size = Sum of (Catch + Escapement + Simulation Year). Obviously, the simulation year shouldn't be added in the calculation of total run size.

The comment refers to the inclusion of "Simulation Year" in this calculation, however, this does not apply to the current version of the IMF. We had previously found and corrected that error. "Total Run Size" (line 105 in the "Worksheet" tab of the IMF) includes "Total Escapement" and "Total Catch" (line 99 and 92, respectively, in the "Worksheet" tab of the IMF).

Comment 38

What is the relevance of the predictions of this model for attaining recovery goals? Recovery goals for winter run Chinook are expressed in terms of growth rate and population size. Can this model address these issues in a predictive way while considering alternatives?

This is precisely what the IMF does: predict population size (or other desired metric) under different alternatives. The graph on the "Home" worksheet displays population size for the first 15 years simulated. The graph on the "Trials" worksheet can display a number of population metrics from a dropdown menu for a specified time period from another dropdown menu; this worksheet can also be used to compare a defined "Benchmark" scenario to any number of alternative scenarios. Graphs on each of these worksheets are linked to outputs in the "Worksheet" page.

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APPENDIX A

Memo detailing IEP Winter-run Salmon Project Work Team comments on the Integrated Modeling Framework for Winter-run Chinook Salmon

(Please note that comment numbers have been added to the IEP Project Work Team memo by SPC&A to facilitate organization of the responses.)

March 23, 2004

IEP Winter-run Salmon Project Work Team
Review of the Integrated Modeling Framework for Winter-run Chinook Salmon
Prepared by S.P. Cramer and Associates for California Urban Water Agencies

REVIEW OF MODEL DOCUMENTATION
STEP 1 REPORT

General Comments

Comment 1
The Step 1 Report includes discussion of several studies and relationships that are not included in the current version of the model. The model documentation should include only those relationships included in development of the current model; it should not be a general thesis on Chinook survival.

Comment 2
The documentation should include the intended uses of the model. A clear statement of the proposed uses and limitations of the model is essential.

Comment 3
Several very important factors impacting winter-run survival, past and present, have not been included in the current version of the IMF. These were discussed with Steve Cramer at the October 14, 2003 meeting of the Winter-run Project Work Team, but have not yet been included in the model. These factors include:

- *Impacts of Iron Mountain Mine contaminant discharge. Mine discharges had significant impacts on winter-run survival in the upper Sacramento River in past years; this factor needs to be included in the model for hindcasting purposes. (Reference the old WPOP model for ways to deal with this factor.)*
- *Impacts of ACID dam on winter-run passage. In past years, ACID significantly impacted winter-run passage and spawning distribution.*
- *Impacts of entrainment of winter-run at GCID, RD108, and other major water diversions on the upper river. Prior to the screening of these diversions, these projects had major effects on survival of winter-run juveniles during emigration from the upper river.*
- *Variability in winter-run ocean survival is not accounted for. In the Northwest, ocean survival of Chinook has been known to vary by more than ten-fold from year to year. These changes in ocean survival rates may have much larger effects on the winter-run population than variation in inland survival, yet the model doesn't account for those effects.*

Comment 4
One of the basic assumptions of the model is that "habitat for successful spawning and egg incubation above Red Bluff Diversion Dam is the bottleneck for production throughout the lifecycle" (page 42). The part of the model predicting spawning and early rearing is based on the SALMOD model, which assumed that early survival is directly related to a flow/habitat relationship. We strongly disagree with these assumptions and use of the SALMOD model for

upstream areas. First, is habitat for spawning, egg incubation, and fry rearing a limiting factor for winter-run in the upper Sacramento River, at current population levels? If you consider that, in recent history, over 100,000 winter-run Chinook utilized the mainstem river for spawning and rearing, and in recent years, only about 8,000 winter-run have returned each year, it is doubtful that habitat quantity currently limits production. If spawning and rearing habitat were limiting, it is doubtful that availability of suitable habitat is directly related to flow. Flows in the upper river are relatively high and stable during winter-run spawning and early rearing and are unlikely to be limiting winter-run habitat.

Comment 5
The model also assumes a maximum returning adult to spawner ratio of 7.5. Winter-run cohort replacement rates in some recent years have exceeded this value. This should not be a constraining factor in the model.

Comment 6
The model assumes a smolt to age 2 survival rate of 5%, based on fall-run tag returns from Coleman National Fish Hatchery. This rate should be based on winter-run data from cohort reconstructions.

Comment 7
The documentation includes outdated genetics data from the winter-run carcass survey. Recent years' data should be obtained from USFWS, Red Bluff Office.

EXECUTIVE SUMMARY
The Executive Summary includes many general statements that may not be valid for Sacramento River winter-run Chinook:

Comment 8
ii – Pulse flows to aid smelt migration? (If "smolt" migration is intended here, pulse flows aren't used as a management tool in the upper Sacramento River.)

Comment 9
Page iii – Paragraph 1 – It's highly unlikely that suitable habitat for spawning is limiting the winter-run population at current run sizes. There is no evidence of this occurring.

Comment 10
iii – Paragraph 2 – Due to the highly managed flows downstream of Shasta and Keswick dams, flows in the upper river are relatively stable during winter-run spawning and early rearing periods. Flood flows are typically not a factor affecting egg survival.

Comment 11
iii – Paragraph 5 – Vulnerability to entrainment – these statements may or may not be true, depending on the particular situation.

Comment 12

iii – Paragraph 6 – These statements are too simplistic and don't represent the true relationships. For winter-run Chinook, we don't have evidence linking survival during migration to flow – survival is negatively related to temperature, but only when temperatures exceed certain thresholds – and we don't know the relationship between survival and the number of channel junctions along the migration route. Last sentence - The number of juvenile winter-run entrained at the Delta facilities each year is not directly correlated to the magnitude of Sacramento River flow, or export rates. Delta cross channel gate closures appear to be important, but only in certain time periods.

Comment 13

iii – Paragraph 7 – Winter-run experience lower harvest rates than fall-run Chinook due to their smaller size-at-age, AND due to specific harvest regulations designed to reduce harvest on winter-run (delayed recreation season openings, etc).

Comment 14

iv – Paragraph 3 – The WCOHM model is not currently used by DFG for setting ocean harvest regulations. It is considered outdated; uses data from the '70's.

Comment 15

iv – Last paragraph – It is unlikely that physical habitat for winter-run fry at current population levels is a limiting factor. The upper Sacramento River is very large, and there appears to be no limitation of suitable habitat.

Comment 16

v – Critical uncertainties – The ocean life phase needs much more attention in the model. Ocean survival can vary significantly, and may have an even greater effect on population size than inland conditions. Factors related to salmon ocean survival should be included in the model.

FIRST TECH MEMO DATED NOVEMBER 20, "TESTING THE MODEL: SPAWNER ABUNDANCE"

Comment 17

The hindcast simulation appears to simulate the decline and recent recovery of the winter-run population. However, there are many ways to model a similar pattern, and an apparent fit between observed and predicted data does not necessarily indicate that the model has incorporated the appropriate variables in the correct manner. In the general comments section (above), we have listed several important factors affecting winter-run survival that have not been included in the model. Without these factors, it is doubtful that the model is generating a valid hindcast of winter-run escapement.

Comment 18

Page 2 - 2nd paragraph - Last sentence - "Spawning escapement estimates via carcass surveys have been deemed more reliable" should be changed to read "Beginning in 2001, spawning escapement estimates based on application of the Jolly-Seber model to carcass survey data have

been considered the best available estimate of winter-run escapement." (The official winter-run escapement estimate through 2000 was based on RBDD count data.)

Comment 19
Page 2 - Last paragraph - For historical run size, the simulation uses three very different measures of winter-run escapement: RBDD counts, the Petersen model estimate, and the Jolly-Seber model estimate from the carcass survey data. For this simulation, we recommend using a consistent long-term measure of escapement, the estimate based on RBDD counts. Otherwise, you're mixing apples and oranges in a long-term trend analysis. The RBDD estimates show the same trend as the carcass survey data in recent years.

Comment 20
Egg mortality due to elevated water temperatures has not been a major problem in recent years, but in drought conditions in the future, elevated water temperatures may again be a source of egg mortality. This factor should therefore be included in the baseline IMF.

SECOND TECH MEMO DATED NOVEMBER 25, "TESTING THE WINTER-RUN CHINOOK MODEL: HISTORICAL HARVEST RATES"

Comment 21
The analysis approach used in this report is no longer considered valid. The agencies involved in ocean harvest management abandoned this approach four years ago. The WCOHM model is no longer used. The current method using cohort reconstructions should be available soon.

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The assumption that ocean harvest rates of winter Chinook are proportional to the CVI is not valid. The assumptions about lure size and size of catch are also not valid in California fisheries. These concepts were developed for northern coho fisheries.

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The DFG Ocean Salmon Project and NOAA Fisheries should be consulted for current modeling efforts for ocean harvest management.

REVIEW OF MODEL SPREADSHEET

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The model in its current form uses information that has been available and used for years. Much of this information has severe limitations, usually associated with system complexity, natural variation, and logistics of data gathering that we have noted and taken into account for years. These limitations have been a source of discussion for a long time within the WRSPWT and other forums. The spreadsheet should make the calculations and information used to make them more transparent. However, we are not sure that it does that in its current form. We are concerned that the spreadsheet might give some the impression that we have more confidence in these calculations and the data used to make them than we really do. Also, at this point, we are not

convinced that the model reliably and accurately predicts and interprets the effects of different scenarios on the winter-run population. Largely this is because some factors seem to affect the outcome in unexpected ways and some factors seem to be more influential than they really seem to be (see below), because the outcomes seem to be difficult to interpret and compare, and because variation is not taken into account.

Specific comments on the model spreadsheet are:

Comment 25
The copy that we received for review did not work as described in the instruction sheet. The "use defaults" and other buttons did not work on that copy. We also had difficulty downloading the website version.

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two-year-olds, and 72.6% were three-year-olds. The model predicts more of an effect of hatchery fish on spawner abundance than we are actually seeing.

Comment 30
The exaggerated hatchery effect stems from a major problem with the model: from Simulation Year 7 forward, density-dependence for fry is the major factor controlling production of naturally-produced juvenile winter-run in the model. The model limits production in the upper river to only 5.6 million fry. That is the reason the predicted spawning returns in the model flatten out, or fail to increase, after a few years. But we know that the river can support more than 5.6 million rearing fry – otherwise, how can we explain the winter-run escapements of the late 1960's/early 1970's of over 100,000 adults? If we ran the model as currently written to compare the relative value of restoration actions, any action to improve fry survival would have exaggerated effects on the population, compared to actions taken to improve survival of other life stages.

Comment 31
The calculated Delta survival rate appears too high in the model. The model predicts a survival rate of 91.27% through the Delta, at 15,000 cfs Sacramento River flow, 59 F, 20% E/I ratio, and DCC gates closed. This survival rate under those conditions seems very high. If we ran the model as currently written to compare the relative value of restoration actions, any of the actions taken to improve survival through the Delta would have an extremely small effect on the winter-run population.

Comment 32
The authors may want to take into account that USFWS in 2001 noted that the proportion of returning two-year-old hatchery fish was greater than the proportion of two-year-old natural origin winter run. It isn't known whether this is a difference between the two groups or whether it is a statistical anomaly. If it is a true relationship, then the model might be changed to account for it.

Comment 33
We tried setting the Sacramento flow and export flow both to 37,000 cfs and the model still predicted a relatively constant 4,000 to 5,000 spawners. We also set the overall survival rate of juveniles in the Sacramento River to 0%, or in the Delta to 0%, and the model still predicted over 4,000 spawners returning. Similarly, we increased the three-year-old harvest rate to 50% and still got over 10,000 fish over 15 years. This seems unrealistic, even for comparison purposes.

Comment 34
In the associated technical memo called "Hindcast Simulation", there was no hindcast simulation. What happened to the simulation?

Comment 35
The model overestimates age 4 fish in the spawning escapement compared to age 3. The model estimates the age 4 component to be approximately 19% of the age 3 component, but in recent years, the composition of the spawning escapement has been more on the order of 5% age 4 fish.

Comment 36
The calculation of the Spawning Escapement/# Spawners ratio (a cohort replacement rate) is incorrect in the model. The spreadsheet formula shows this ratio to be # of spawners in year x multiplied by the pre-spawning mortality rate, divided by the sum of the # spawners in years x - 4 and x – 3.

Comment 37
There are numerous other mathematical errors in the spreadsheet. For example, on line 174 of the worksheet, the Total Run Size = Sum of (Catch + Escapement + Simulation Year). Obviously, the simulation year shouldn't be added in the calculation of total run size.

Comment 38
What is the relevance of the predictions of this model for attaining recovery goals? Recovery goals for winter run Chinook are expressed in terms of growth rate and population size. Can this model address these issues in a predictive way while considering alternatives?

Literature Cited

USFWS. 2003. 2001 Winter Chinook salmon carcass survey annual report. USFWS, Red Bluff Fish and Wildlife Office, Red Bluff CA 96080.