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April 9, 2013

Felicia Marcus, Chair
State Water Resources Control Board
1001 I Street
Sacramento, California 95814

Subject: Response to San Joaquin River Tributaries Authority reports on Department of Fish and Wildlife Salmon models.

Dear Ms. Marcus:

The California Department of Fish and Wildlife (Department) has reviewed two reports submitted by the State Water Resources Control Board on behalf of the San Joaquin River Tributaries Authority (SJRTA): "Report on Flow vs. Escapement Model and Environmental Data", and "Report on the San Joaquin River Salmon Population Model". These reports advance the opinion that the Department's San Joaquin River Salmon Model Version 1.6 is not appropriate to use as part of the information considered by the Board in setting flow regimes in the San Joaquin River basin. The Department, of course, does not agree with this opinion, and has prepared a commentary on those reports. In summary, the Department continues to advocate that the Board apply the full suite of the best possible decision aids in the process of setting flow regimes, and this model (and its eventual successor) is an appropriate tool.

Our commentary is attached. If you have questions or wish to discuss this, please contact Dean Marston, Environmental Program Manager at (559) 243-4014 extension 241.

Sincerely,

Jeffrey R. Single, Ph.D.
Regional Manager

attachment

ec: Letter only:
Department of Fish and Wildlife
Scott Cantrell, Wendy Bogdan, Chad Dibble, Linda Barrerra, Dean Marston, Dale Stanton, John Shelton

REPLY TO THE SAN JOAQUIN TRIBUTARY AUTHORITY'S REPORT ON FLOW VS. ESCAPEMENT MODEL AND ENVIRONMENTAL DATA AND REPORT ON THE SAN JOAQUIN RIVER SALMON POPULATION MODEL RELATING TO THE REVIEW AND UPDATE TO THE BAY-DELTA PLAN WORKSHOP 3

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Submitted to:

State Water Resources Control Board

On Behalf Of:

California Department of Fish and Wildlife

April 2013

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The authors thank Linda Barrera for her editorial review which greatly increased the reading and comprehension of this document.

I. Introduction

The California Department of Fish and Wildlife (CDFW) respectfully submits this response to the reports titled "Report on Flow vs. Escapement Model and Environmental Data" and "Report on the San Joaquin River Salmon Population Model" prepared by Gary Lorden and Jay Bartoff (hereinafter "L&B reports") on behalf of the San Joaquin River Tributaries Authority (SJTA). During the State Water Resources Control Board's (Board) review and update to the Bay-Delta Water Quality Control Plan (Bay-Delta Plan), SJTA has asserted that the CDFW's San Joaquin River Salmon Model Version 1.6 is not appropriate to use as part of the information considered by the Board in setting flow regimes in the San Joaquin River (SJR) basin. The primary purpose of this reply is to explain that, contrary to this assertion, CDFW's model version 1.6 is valid and relevant, and should be used as part of the overall body of evidence the Board considers in its update to the Bay-Delta Plan.

CDFW built its original SJR Salmon Population Model to address specific resource management issues raised by the Board during its periodic review of the Bay-Delta Water Quality Control Plan in 2004. In particular, the Board's primary management question focused on determining what the magnitude and duration of flow in the SJR at Vernalis should be during the April 1 - May 31 time period to adequately protect outmigrating juvenile fall-run Chinook salmon.[1] During that time, the Board did not seek to identify or assign percent responsibility to all possible factors that influence SJR salmon production in the SJR basin. As such, CDFW's original model, and subsequent revisions, was built to address the Board's specific question which, was precise to time (i.e. April and May), location (i.e. Vernalis), water quality parameter (i.e flow), and biological objective (i.e. juvenile salmon survival through the South Delta). It is important to note that the level of complexity needed in a fish population model is driven by the specific management question(s) being asked. Model Version 1.0 and subsequent versions were built to address the temporal, spatial, and biological objectives inherent to the Board's water quality control standard in the SJR at Vernalis specific to protection of fall-run Chinook salmon.

In the current review and update to the Bay-Delta Water Quality Control Plan, the Board has broadened the primary management question it initially asked in 2004 by expanding the scope of the question temporally, spatially, and biologically. Now, the Board's spring outflow management objective for fall-run Chinook salmon protection is from February 15 – June 30, instead of from April 1 – May 31 (ICF International). Additionally, the Board is including SJR tributaries (Stanislaus, Tuolumne, and Merced Rivers) in its management objective, as well as mainstem of the SJR at Vernalis (ICF International). Furthermore, this revised biological management objective now includes juvenile rearing/production, in addition to juvenile out-migration (ICF International). Notwithstanding the newly broadened scope of the Board's inquiry, CDFW's model version 1.6 remains useable because it still provides valuable information regarding a

[1] 2005 Periodic Review Workshops.

portion (March 15 to June 15) of the overall February through June flow window now being considered by the Board.

CDFW acknowledges that in addition to the expanded flow-related management questions posed by the Board, stakeholders participating in the review and in the development of the update to the Bay-Delta Plan have suggested that non-flow factors may be influencing juvenile production in the SJR basin. These additional factors extend beyond the capabilities of model version 1.6, and a more sophisticated model is needed to fully account for them. However it is noted that the Department evaluated two non-flow parameters for inclusion in model version 1.0, which provided the framework for model version 1.6, that have been identified as influencing abundance of escapement of fall-run Chinook salmon into the SJR, such as ocean harvest and South Delta exports. The Department found that these non-flow parameters have little, or no, relationship to fall-run Chinook salmon population abundance in the SJR and that spring flow magnitude, duration, and frequency all had significant influence upon SJR fall-run Chinook salmon abundance in the SJR (CDFW 2005).

To add more parameters to its model, the CDFW has developed a full life-cycle model for SJR fall-run Chinook salmon (hereinafter "SalSim"). This full life-cycle model provides added insight towards answering broader management questions such as: what is the influence of environmental factors other than flow (for example, predator abundance, Delta exports, ocean harvest, hatchery abundance, etc.) on the SJR salmon population? CDFW looks forward to the Board being able to utilize SalSim once it is completed and available to the public. In the meantime, CDFW advocates continued use of model version 1.6 for the following reasons:

i) the juvenile production prediction model components have been validated (field tested);
ii) the underlying methodologies employed are statistically sound; and
iii) the March 15 to June 15 spring flow time period remains an extremely important time period necessary to protect juvenile production and emigration survival through the South Delta.

In the following sections of this reply, CDFW explains its Model version 1.6 validation methodology, discusses the importance of spring flow for SJR fall-run Chinook salmon, and provides a response to the SJTA's criticisms of CDFW's Model version 1.6 while establishing the value of the appropriate use of this model by the Board.

II. Model Version 1.6 Validation

Model version 1.6 consists of three sub-modules: i) juvenile (i.e. smolt) salmon production, ii) juvenile salmon survival, and iii) adult salmon production. The juvenile salmon production module (aka: Delta sub-module) estimates juvenile salmon production in the SJR at Mossdale annually as a function of prior year number of combined salmon spawners (i.e. escapement) in the SJR and the daily average flow at Vernalis during the March 15^{th} through June 15^{th} time frame. The output from the juvenile production sub-module provides the input for the juvenile salmon survival sub-module. The juvenile salmon survival module estimates survival of juvenile salmon through the South Delta as a function of flow level entering the South Delta in the SJR at Vernalis. The juvenile salmon survival module output provides the input for the adult production sub-module. The adult production module estimates salmon production as a function of the number of juvenile salmon that are estimated to have survived through the South Delta (i.e. survived to Chipp's Island). Model version 1.6 uses flow[2], and both juvenile salmon production data (i.e. CDFW's Mossdale Trawl[3]) and juvenile salmon survival data (i.e. state and federal fish agency sponsored South Delta survival studies[4]) collected from 1987 to 2007.

It is noted that the primary strength of any predictive model is its ability to accurately predict parameters using independent (i.e. separate) data (i.e. flow and both juvenile and adult salmon production data) other than the data used to calibrate the model. For this reason, CDFW calibrated the juvenile production sub-model using annual juvenile production data collected from 1988 to 2007, and validated this sub-model using juvenile salmon production data collected from 2008-2011.[5] Furthermore, CDFW calibrated the adult production (i.e. total brood year escapement[6]) sub-module using adult salmon brood year escapement production years 1987 to 2003, and validated this sub-model using adult salmon brood year escapement production data from 2004-2010.[7] As discussed below, validation testing of the juvenile production sub-module shows that this sub-module does a good job predicting juvenile production although caution is warranted given the few number of years (i.e. 4) that are available to validate this sub-module. Validation of the adult production sub-module indicates that this sub-module does not predict well, and that additional model parameters (i.e. ocean conditions etc.) are needed to enable this sub-module to better predict adult salmon production for the SJR basin.

2.1. Juvenile Production

As shown in Table 1 Model version 1.6 has mixed validation results where when observed smolt abundance is less than 200,000 smolts, the model over predicts by about a 2:1 rate but when observed values are about 500,000 and above, model predictions are quite accurate (i.e. prediction values are at 93% of observed values on average). It is worth noting that the historical range of smolt abundance at Mossdale upon which the juvenile production module within model version 1.6 was built, for the years 1988 through 2011, ranges from 228,949 to 3,664,884[8]. Therefore to be consistent, observed smolt production values at Mossdale within the range of 228,949 to 3,664,884 should be used for formal model validation. Since Mossdale smolt production values for years

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[2] Data from California Department of Water Resources Website: California Data Exchange Center accessed at http://cdec.water.ca.gov/.

[3] California Department of Fish and Wildlife Annual Reports (various).

[4] Vernalis Adaptive Management Reports by the San Joaquin River Group Authority available at http://www.sjrg.org/technicalreport/default.htm

[5] California Department of Fish and Wildlife Annual Reports (various)

[6] The term "brood year escapement" is defined as the total number of adults, of any age (i.e. age 2, age 3, age 4, and age 5), generated from a specific brood (i.e. birth) year.

[7] California Department of Fish and Wildlife

[8] California Department of Fish and Wildlife Annual Reports (various)

2009 and 2010 both occurred outside this range (i.e. were less than 228,949) they should not be considered. For transparency purposes, these values are presented. It is noted that the lowest smolt production prediction for the juvenile production sub-module, for the years used to calibrate the model (i.e. 1988 through 2007) is 340,689 when fall spawner abundance was 590 and average spring flow was 1,101 cfs. For comparative purposes, fall spawner abundance and spring flow level (cfs) for years brood production years 2009 and 2010 was 2,229 and 1,323 (spawners) and 1,676 and 4,195 (flow) respectively. So even though the environmental conditions for spawner abundance and flow level fall within the observed range of values during the time frame used to calibrate the model, they did not produce Mossdale smolt abundance levels that occurred within observed value ranges used to calibrate the juvenile production sub-module. It is again noted that the salmon production sub-module over predicts at the extreme low end of the production range.

From a juvenile salmon smolt production trend perspective, it is also worth noting that validation results indicate that when observed smolt production trends decline, model prediction also declines and when observed smolt production increases, model prediction also increases (Figure 1). Figure 2 compares juvenile salmon sub-model smolt estimates for the calibration (1988 to 2007) and validation (2008 to 2011) time periods. The ability of the model to track production trends (i.e. either up or down) indicates that the juvenile production sub-module, and the spring flow and adult salmon escapement abundance parameters contained in the model, indicate that the juvenile salmon production sub-module within model version 1.6 is a good model for estimating juvenile salmon abundance. CDFW acknowledges that caution is warranted when considering model validation given the few (i.e. four) number of years available to validate the model.

Table 1. Model Version 1.6 Mossdale Smolt Prediction Accuracy

Year	Observed Mossdale Smolt Production	Model Estimated Smolt Production
2008	488,614	419,074
2009	175,566	434,633
2010	106,371	493,099
2011	1,536,887	1,532,777

Figure 1. Mossdale Smolt Production Validation[9]

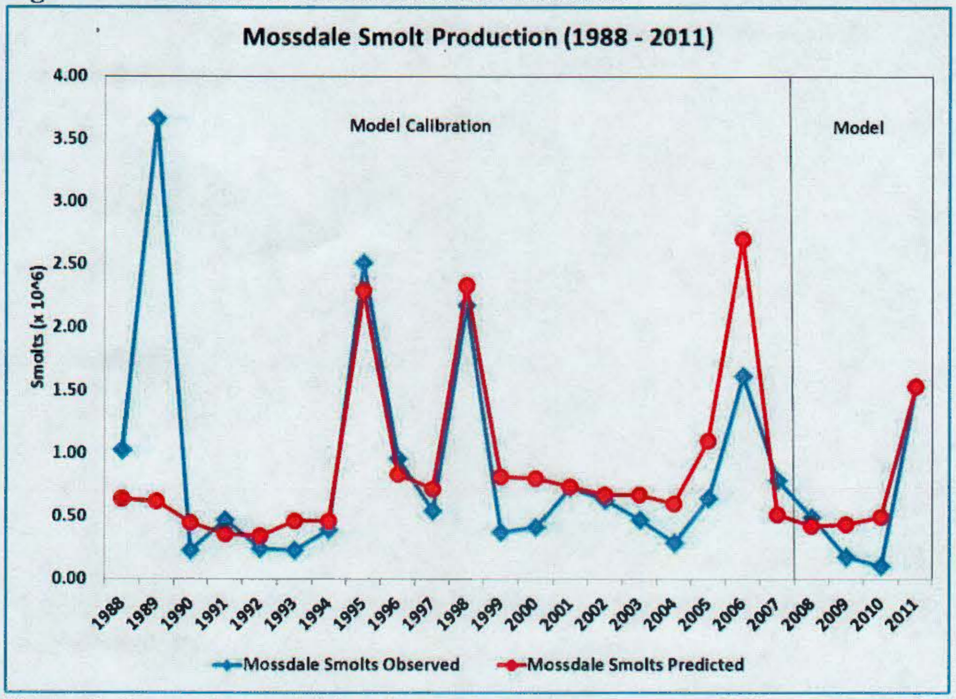

Figure 2 shows observed (blue diamonds and line) vs SJR Salmon Model Verson 1.6 juvenile salmon sub-module prediction (red circles and line) production for years 1988 through 2011. The time period comparisons are separated into model calibration and model validation time periods.

[9] The validation time period depicted in this Figure differs from that shown in the November 2012 State Board Workshop. The Figure shown in the workshop was incorrect, and should have shown validation years beginning in 2008, not 2005.

Figure 2. Mossdale Smolt Production Observed vs. Predicted

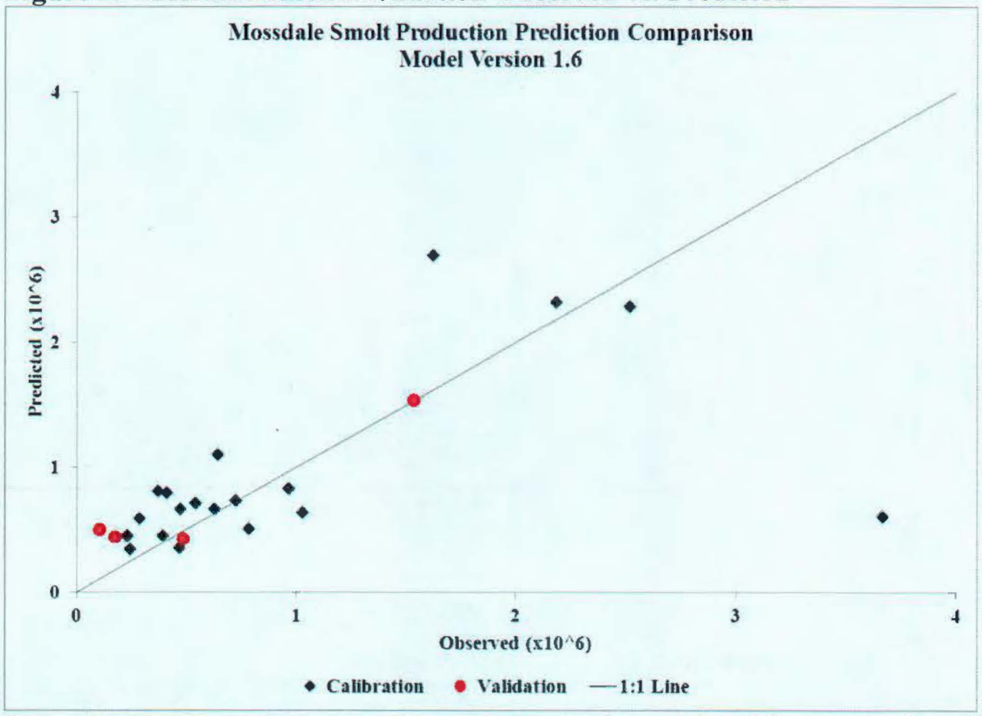

Figure 2 depicts observed annual juvenile production at Mossdale compared to CDFW's SJR Salmon Model version 1.6_Juvenile Production Sub-module's estimated values. The 1:1 line shows what would be expected if the sub-module perfectly estimated juvenile salmon abundance at Mossdale for each year (i.e. 1988 through 2011). The values indicated by black diamonds compare observed versus predicted values, for the juvenile production sub-module time period (i.e. 1988 through 2007), used for model calibration. The values indicated by red circles compare observed versus predicted values, for the juvenile production sub-module time period (i.e. 2007 through 2011), used for model validation.

2.2 Juvenile Survival

Validation testing of survival for juvenile salmon smolts migrating through the South Delta in model version 1.6 cannot be performed, due to insufficient numbers of juvenile hatchery fish from the Department's Merced River Hatchery. Lack of large numbers of hatchery fish has prevented large scale coded-wire-tag smolt survival studies in the South Delta, and this lack of studies prevents this sub-module from being validation tested. If, and when, these studies are resumed then this sub-module can be validated.

2.3 Adult Production

SJR fall-run Chinook salmon escapement estimates exist through escapement year 2011, as reported in CDFW's GrandTab (CDFW 2012). Since Model version 1.6 is a cohort production rather than an ocean escapement model, the results from Model version 1.6 must be processed through a post-processor to convert brood year production estimates into annual ocean escapement estimates. In 2005, CDFW submitted to the Board a description of the methodology for this post-processing in Model version 1.0 (CDFW 2005). As explained in that submission, annual ocean escapement estimates for the SJR tributaries were combined into one overall SJR escapement estimate. This annual escapement estimate was then segregated into several age classes using average proportions derived from multiple years of empirical data for fish of each ages class (i.e. age 2 through 5). This resulted in annual escapement estimates that could be segregated into individual brood production year cohorts by adding the number of fish in each brood production year. This methodology is shown in Table 2, where the column titled "Cohort #" corresponds to the brood production year cohort.

Table 2. Conversion of Annual Escapement Estimates to Brood Year Production Estimates

Escapement Year	Reconstructed Escapement	Smolt Production Year	Cohort #	Age Cohort %				
				Age 1	Age 2	Age 3	Age 4	Age 5
				0.05%	30.00%	55.35%	14.00%	0.60%
1967		1968	276	0	83	153	39	2
1968		1969	98603	49	29,581	54,577	13,804	592
1969		1970	1403	1	421	777	196	8
1970		1971	1119	1	336	619	157	7
1971		1972	461	0	138	255	65	3
1972	14,919	1973	2638	1	791	1,460	369	16
1973	1,547	1974	3645	2	1,094	2,018	510	22
1974	1,213	1975	3304	2	991	1,829	463	20

Table 2 shows how annual salmon escapement abundance estimates, which include salmon of varying brood year age classes (i.e. age 2, age 3 etc), are desegregated into individual year brood year production estimates where all fish from a single brood production year, that escape inland to spawn across multiple ocean fishery escapement years, are accounted for then aggregated into a brood year production total.

CDFW reconstructed annual escapements in Model version 1.6 for years 2004 through 2010. Escapement year 2011 was not calculated because of an insufficient number of brood production years (younger age classes were absent). To validate the model for these additional adult brood production years it was necessary update the computational framework of Model version 1.6 to account for the spring flow during juvenile production and emigration in brood production years. Correspondingly, CDFW added daily flow data for March 15 to June 15 to Model version 1.6 for the 2004 to 2008 time period. In turn, Model version 1.6 could be run to estimate adult brood year production, enabling ocean escapement reconstructions through year 2010. Figure 3 shows model calibration (years 1967 thru 2003) and model validation (years 2004 thru 2010).

Figure 3. Adult Production Model Validation Results

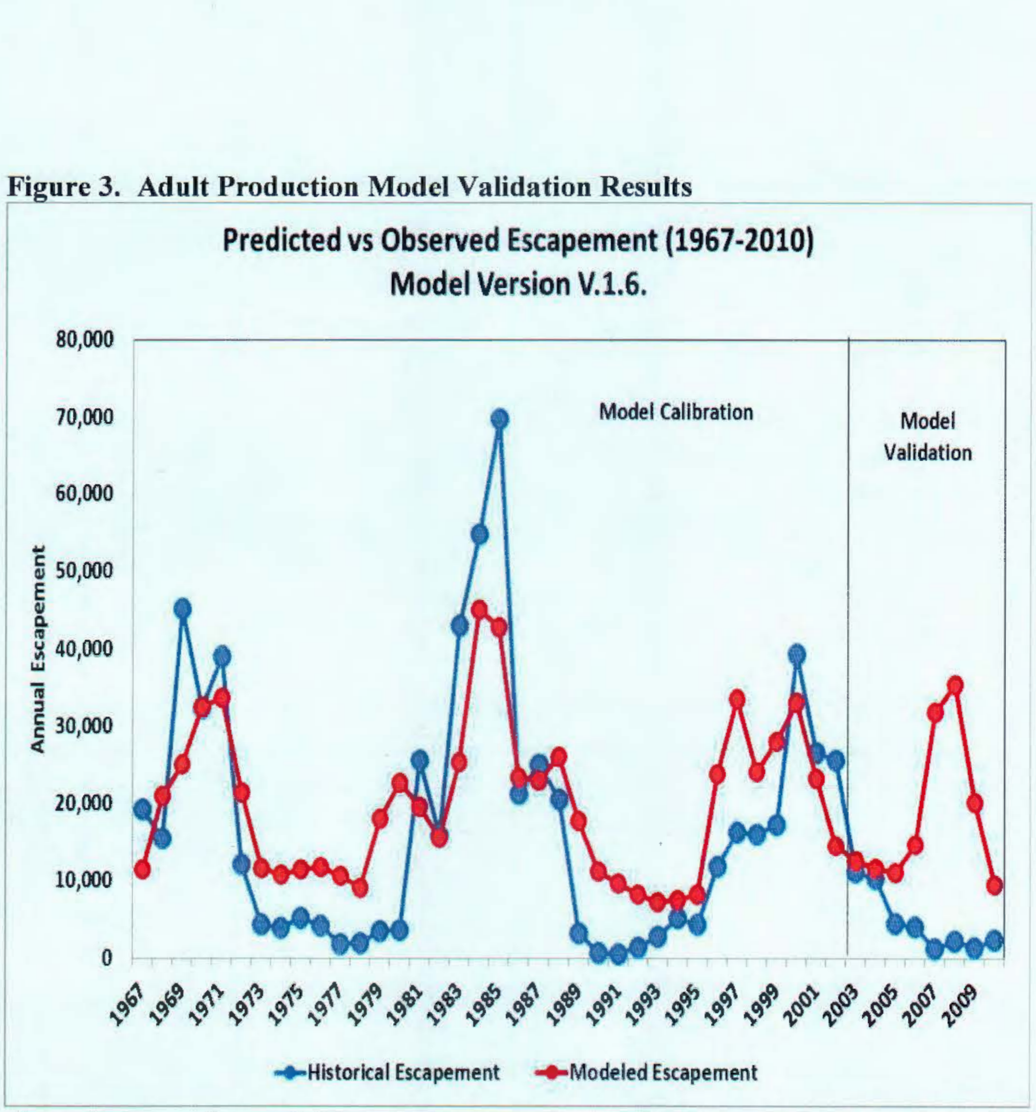

Figure 3 shows observed (blue circles and line) vs SJR Salmon Model's adult production sub-module (red circles and line) predictions for years 1967 through 2010. The overall time period depicted is divided into two time periods: i) model calibration (1967 through 2003) and ii) model validation (2004 through 2010).

CDFW acknowledges that the adult production sub-module component of Model version 1.6 did not correspond well with observed escapement. This error in estimation is attributable to at least two factors: i) the model does not contain an ocean condition parameter(s) which would have enabled model estimates to be influenced by a downturn in ocean conditions in years 2005 and 2006 consistent with the proximate cause of the Sacramento basin salmon decline (Lindley et.al. 2009); and ii) the model does not account for hatchery contribution to escapement. Both of these factors have been added to SalSim. However, it is important to note that a poor adult production prediction trend **does not** render Model version 1.6 unusable for flow setting objectives aimed at protecting juvenile (not adult) salmon. As explained further below, it simply means that Model version 1.6 should not be used to evaluate progress towards attaining the adult narrative production goal.

III. Importance of Spring Flow for SJR fall-run Chinook Salmon

During the Board's current review and update to the Bay-Delta Plan, SJTA has challenged the Board's and CDFW's findings that inadequate spring flows are a significant cause of the decline of fall-run Chinook salmon (Demko et al. 2012a; Demko et al. 2012b; Lorden and Bartroff 2012a; and Lorden and Bartroff 2012b. In response to SJTA's contention that the Board and CDFW are not relying on the best available science in proposing modifications to the flow regime in the SJR Basin, CDFW emphasizes information submitted to the Board in CDFW's letters dated December 6, 2010 and February 7, 2011. In these letters, CDFW outlines (1) the importance of spring flow upon juvenile salmon habitat (i.e., to reduce water temperature and increase floodplain inundation); (2) how improved habitat conditions result in improved juvenile salmon production and survival; and (3) and how improved juvenile salmon production and survival result in improved adult salmon production. CDFW strongly encourages the Board to re-read these letters which refute the suggestion that flow is not an extremely important component associated with juvenile, thence adult, salmon production in the SJR basin.

In summary, spring flow, especially using a percent of unimpaired flow approach, provides variable flow patterns that best restore habitat conditions that meet salmonid biological requirements (i.e., sufficient forage supply, cool water for efficient physiological metabolism, predation shelters, favorable juvenile emigration conditions, etc.) (Moyle et al. 2010; SWRCB 2011). In addition, it is important to note that water temperature warming, which is largely driven by ambient air temperature and decreasing flow levels, can be effectively buffered through elevated reservoir releases which delay warming through increased depths and increased water velocity. These factors synergistically work to drive cooler water downstream, over longer duration, that provide favorable production rearing, and emigration survival, conditions for juvenile salmonids (Dotan et al. 2009; CDFW 2011).

CDFW also notes that water provides the living space for aquatic organisms that live within the water column. This living space has two components, quality and quantity. In order for aquatic populations to be viable, they need both adequate quantity and quality of that living space. The mechanistic components of flow that are directly tied to juvenile salmon biology, such as turbidity, dissolved oxygen, temperature, river width, river depth, velocity, are directly tied to flow level. Generally speaking, as flow increases, the associated mechanistic aspects of flow that fish find favorable increase, and correspondingly, as flow level decreases, they worsen. Thus, it is not a surprise that fish population health and production are closely associated with flow level

3.1. <u>Juvenile Emigration – Timing</u>

Juvenile salmon survival, as measured at Mossdale, near Vernalis, primarily occurs from March 15th through June 15th (CDFW 2005). The majority of parr and smolt size salmon emigrate the SJR basin during this time frame. Figure 4 depicts the average spring juvenile emigration trend. It shows that only a small percentage of the juveniles emigrating are from the early part of the emigration period. Providing improved flow across the entire juvenile salmon (parr and smolt) spring emigration season will have substantial benefits for emigrating juvenile salmon, with carry-over effects on adult numbers. Juvenile survival and successful emigration across the entire emigration season is very important at both the individual fish level and at the fish population level.

Adult salmon enter the spawning grounds over a two to three month period in the fall resulting in a parr and smolt juvenile production/emigration season that also extends over a roughly two to three month period the following spring. Progeny from early arriving adults tend to fair better than progeny arriving from later arriving adults because early departing juveniles leave under the most favorable spring flow conditions (under the way that the SJR basin has been historically managed, with no provision for unimpaired flow patterns). This trend adversely affects the population because mid to late departing juvenile salmon face severe environmental conditions (i.e., high temperatures) associated with high mortality. These juveniles carry beneficial genes that might otherwise substantially contribute to gene pool elasticity, but are cut off from future population recruitment because they die. In human economic terms, we can think of a broad gene pool as a diversified investment portfolio, where more diversity is better for buffering economic downturns, and less diversity carries greater risk of bankruptcy. When more juvenile salmon are contributing to future year adult production, with adequate juveniles being produced and surviving throughout the early, middle, and late parts of their emigration season, the whole salmon population becomes healthier.

Figure 4. Spring Mossdale Juvenile Salmon Emigration Pattern

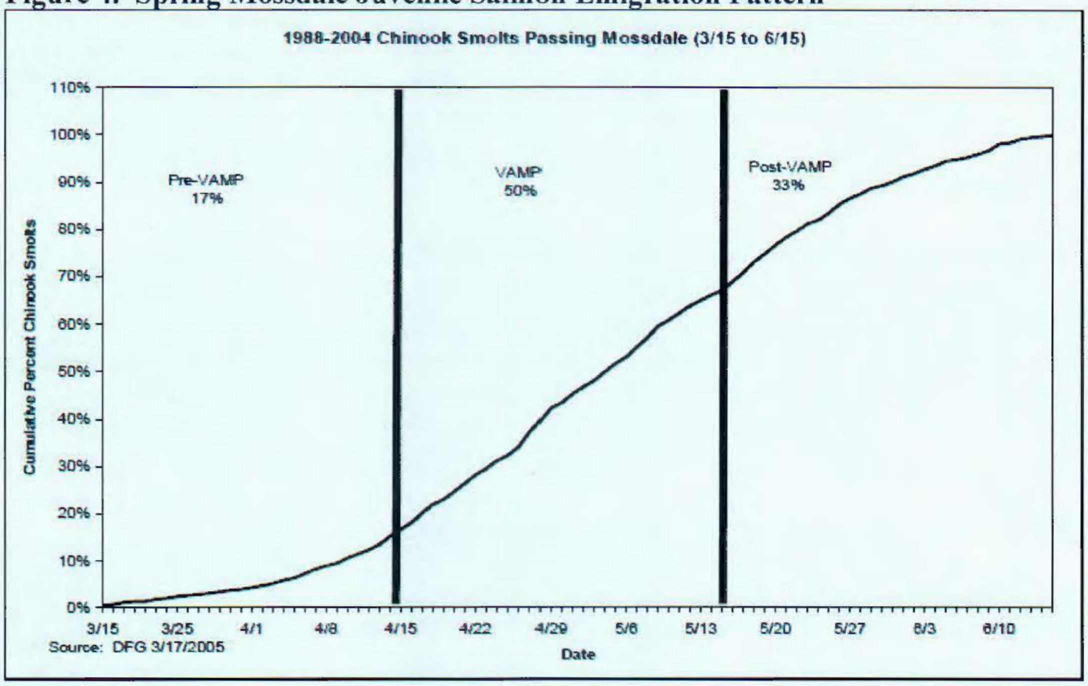

Figure 4 shows the migration pattern for SJR emigrating salmon smolts as percent of cumulative exceedence. "VAMP" means Vernalis Adaptive Management Program and refers to the 31-day juvenile emigration protection window.

IV. Response to Criticisms Regarding Statistical Methodologies Employed in Model Version 1.6

The L&B reports submitted on behalf of the SJTA specifically criticize the regression model of SJR escapement and flow and assert that CDFW's Salmon Survival Model version 1.6 is neither an appropriate approach, nor a valid tool for the Board's use in setting flow standards for the SJR basin.[10] CDFW submits that this assertion is based upon several specific criticisms that are without merit. Furthermore, the L&B reports contain multiple technical and conceptual errors that demonstrate an incorrect understanding of the assumptions and practicalities of Model version 1.6. Contrary to SJTA's assertions, the use of salmon escapement versus instream flow regression models remains a sound method to inform instream flow modifications in the SJR basin, and in particular the CDFW Salmon Survival Model version1.6 is an acceptable tool for management decisions regarding of instream flows in the SJR basin.

4.1. <u>Model Goals versus Model Optimization</u>
Statistical estimation can be thought of an optimization problem, the goal of which is to attain an estimate that minimizes the mean of a loss-function (e.g., the squared distance between a predicted value and the actual value of some outcome of interest, such as adult salmon escapement). One of the most important principles in statistical estimation is that procedure that is optimized for one goal (one particular loss function) will generally be non-optimal for another goal (another loss function).

A simple example is that of a model that does the best job at predicting an outcome (e.g., minimizes the average loss function of predicted versus observed adult salmon escapement) but differs from a model that does a better job at estimating the impact on the outcome from a particular variable (i.e., number of predators in a part of the system). For instance, a model that leaves out a variable related to number of predators might do a better job in predicting the outcome (for instance, survival) given the available data to fit such a model because the addition of the predators to the model results in greater sampling variability in the prediction (well known variance-bias trade-off; e.g., Jame, 2003). This can be true even if there is a true causal association of predators and escapement. Thus, one reduces the bias of prediction by including predators, but in doing so increases the error (variance) of the prediction, resulting in a worse estimate than if the predator term was not included in the model. Thus, generally, more complicated models result in predications that have larger variance and when the goal is prediction, they can results in a worse fit (given limited data) than a simpler model..

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[10] See SJTA cover letter, page 1-2.

The fact that one model cannot be optimal for all goals is ignored in the SJTA's L&B reports, but is crucial to decisions being made about the form of the model and what variables to include. The Board should determine the goals it considers most important in the estimation of a statistical salmon life-cycle model. Then, choices can be made about which model(s) can be appropriately used to answer which question(s). This also means that what appear to be technical flaws of the model in one context, are not necessarily problematical unless there are alternatives that do a better job, *given the available data*, at minimizing whatever loss-function the user of the model considers most important. Thus, many of the comments in SJTA's L&B reports have no relevance because the authors do not show that an alternative model does a better job of predicting juvenile abundance, juvenile survival, or adult escapement.

4.2. Response to the Report on Flow vs. Escapement Model and Environmental Data

It is important to note that the data set analyzed in Attachment One (Lorden and Bartroff 2012a) has nothing to do with data used to estimate model version 1.6. The use of spring flow versus escapement 2.5 years later assumes all variance in the data set is attributable to flow alone [which results in broad confidence intervals and reduced certainty]. It is further noted that the data set to use to evaluate the role of spring flow versus adult production is not one using adult escapement 2.5 years later, but one using reconstructed brood year production cohorts, as CDFW (and others) have done (CDFW 2008).

4.2.1 Evidence Against the Relationship Inferred from the Model Fit

SJTA's statistical calculations to refute the linear relationship between flow and escapement underscore how certain misuses of statistical constructs can lead to misleading analyses. For instance, if data provide strong evidence of no relationship between fish abundance and spring flows, SJTA should be able to provide an estimate of such a null relationship with enough precision to show that biologically significant relationships are inconsistent with the data (e.g., a 95% confidence interval that excludes meaningful relationships of flow and abundance). However, all SJTA has shown is that if one subsets a finite sample, all sub-samples do not give the same relationship, thus all they show is that there is sampling variability in the data.

In addition, smaller samples have less power to reject a false null hypothesis. In fact, the SJTA does not show that when a subset of the data is chosen, based on their criteria, there is a statistically significant change in the relationship of flow and escapement (see L&B's Figure 5 on page 9). Since such a comparison does not show a statistically significant change, all they have shown, again, is that subsets of data result in different estimates, which is of course basic to the idea of statistical inference based on sampling variability.

The SJTA might have intended to show that a model that has different relationships for different flow regimes, by adding more parameters and thus increasing the variance of prediction, does a better job than a model that enforces a single relationship overall years. They have not shown that one can do a better job in predicting escapement by stratifying by bins of flow. For instance, Figure 5 of SJTA's L&B report suggests that a better model would not have 2 parameters (an intercept and slope), but a substantially bigger model with 2*5 or 5 intercepts and 5 slopes. Given the goal of fitting this model and knowing than any model will be an approximation of the "true model," the authors have failed to demonstrate that a more complex model is superior to the existing more simplified, in comparison, model.

4.2.2 Violations of Model Assumptions

As explained above, the data set referenced in the SJTA's L& B reports was <u>not</u> used in Model version 1.6; therefore, any criticism, founded or unfounded, does not apply to Model version 1.6. However, there is still some use in rebutting the claims made related to this analysis as they serve to uncover potential conceptual errors we believe are repeatedly made within this report, and these errors are fundamental to rigorous statistical estimation and inference.

Though diagnostics of regression are relevant to interpretation of the results, the use of them by L&B as a criticism of the model fits in our opinion is misguided. The technical conditions of a *normal* linear regression are essentially never true, for instance the data is never normally distributed, the residual variance is never perfectly constant across the predictors. The only valid way to interpret such a model fit is in a so-called semiparametric model, that is one in which one does not know the true distribution of the data (given the "true" statistical model is almost never known, and certainly not in the type of experiments used for parameterizing Ver 1.6). With providing the technical details, what this means practically is one thinks of the model, appropriately, as an approximation to the true model, and the inference (confidence intervals) should not depend on knowing the form of the distribution of the data (e.g., normal), but on so-called asymptotic theory. In this context, the best model is the not the "true" model which is an unrealistic and unattainable standard, but the best fit among competing models that satisfy the goals of the analysis (see for example formal theory on loss-based estimation in van der Laan, et al., 2007). We do not think the the L&B analyses present rigorous evidence that they have a better alternative using more relevant statistical criteria for judging both estimation and inference we provided. Given the goals of judging the model estimates and inference using appropriate and practical diagnostics, there does exist techniques of evaluating our results (e.g., nonparametric bootstrap; Efron and Tibshirani, 1993). However, L&B do no such analyses.

SJTA also asserts in its L&B report as follows:

> *Another assumption of the model is that observations of the y variable are subject to random variations whose scale is constant and which average out to zero. When this holds, the residual plot should appear as roughly a uniform cloud of points, symmetric around the horizontal dotted line. That is not the case in Figure 2, which on the contrary indicates both a bias (non-zero average) and a non-constant scale of variations. Moreover, the numbered points in Figures 2 and 3 are outliers –points that represent*

deviations from the linear model that are too large to be consistent with that model.[11]

The magnitude of an outlier is only interpretable in the context of the sample size and the influence such a point has on the regression (i.e., the influence of the point on the regression, e.g., Cook's Distance, see Draper and Smith, 1998). For example, if one has a regression with a million data points, an absolute value of a standardized residual with a value of "2" would not be considered an outlier. SJTA does not provide any analysis of potential influence of such data points. Furthermore, simple back of the envelope probability calculations based on the assumptions that the standardized residuals are from a standard normal distribution suggest that there is a high probability of getting 1 or more residuals of > 2.5, indicating that there is no strong evidence that these are outliers. More basically, as discussed above,, given that making the assumptions of a normally linear model in practical situations is almost never warranted, one needs a more rigorous definition of an outlier than provided by L&B.

4.2.3 Lack of Predictive Power

The SJTA's L&B report criticizes the model fitted to the available data,[12] but offers no other model that does a better job of explaining the uncertainty in the data. There is no denying the existence of significant unaccounted for variation in models based on both the fish census data (trawls, carcass counts, and rotary screw trap estimates) and release-capture, coded-wire tag (CWT) experiments. CDFW's approach is to do the best job possible with the best available data. Because of the uncertainty and limited amount of data, we believe that at the given time it is unrealistic to expect any model will be able to forecast escapement accurately on a year by year basis (that is, to develop a model with little residual variance). However, that does not mean the model will not serve as a useful estimate of the *average* escapement during years with equivalent histories (e.g., conditions during which the fish survived). Specifically, the type of data can be used to estimate the *average* impact of how variations in the environmental exposure histories experienced by cohorts of juvenile and adult salmon affect their ultimate survival to escapement.

As discussed above, CDFW is developing a new life-cycle model that can incorporate statistical relationships, like the ones reported in Model version1.6, but can also incorporate independent biological knowledge, to address the unexplained variance as best as possible. SJTA's L&B report does not provide or propose an alternative model that does a better job at predicting survival to escapement using unbiased measures of fit. Thus, SJTA analytical approach and criticisms lack a relevant standard, unlike what we have provided via section II above.

4.2.4 Inferential Problems

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[11] Page 2 in Lorden and Bartroff 2012a.

[12] *Id.*

SJTA asserts that the regression analysis suffers from the "*ecological fallacy.*"[13] The term ecology fallacy is defined as drawing inferences about the association of variables measured at an individual level based upon correlations of variables measured at a group level to which those individual belong. In this specific case, SJTS is asserting that repeated pulses of fish movement across a season, in response to discrete flow time periods, are not influenced by flow movements categorized across the entire season (i.e., it is an ecology fallacy that juvenile salmon respond to the seasonal average of flow). However, this is not a credible critique without much more detail about how this fallacy would bias the results, because SJTA do not even define what parameter they consider the one of interest (bias has no specific meaning unless the estimator, that is the algorithm that produces the estimate, is compared to an explicit parameter of interest, say the true association of a seven-day summary of flow with future survival)..

Specifically, it is technically true, as is the case for *any* analysis where the resolution of the environmental data requires some smoothing (e.g., averaging) of the data across subunits (e.g., flow measured in minutes, recorded as hourly or daily averages) and also given the limits of the resolution of the timing of the environmental or fish survival data (that is, we never observe the precise time a fish dies. This leads to obvious practicalities related to fitting statistical models to data sets of limited sample size, with potentially high dimensional data (fine resolution of flow measurements), such that *technical* ecological fallacy could result (Freedman, 2002). However, SJTA criticism regarding supposed presence of ecological fallacy is not relevant unless it 1) states the parameter of interest (e.g., the survival based on a history of weekly flow averages, daily flow averages, hourly flow averages, etc.), 2) states the parameter that is estimated (i.e., survival given averages over longer time scales), 3) states the implied identifiability[14] conditions such that the estimated parameter is an unbiased estimate of the parameter of interest, and 4) argues why these assumptions are invalid, or why a different estimate from the same raw data (that contains, for instance, daily flows) would have an estimate of the parameter of interest with smaller mean-squared error (closer to the true one). However, SJTA did not provide this needed information to justify their criticism but instead simply listed a standard issue in any analysis of the this type of data, which has no particular relevance to justifying their ecological fallacy criticism as applied here.

SJTA also states in its L&B report:

> *Another relevant fallacy is the Correlation/Causation Fallacy, in which an estimated correlation in a regression analysis is mistaken for causation– i.e. that the variables have a genuine cause-and effect relationship.*

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[13] Page 3 in Lorden and Bartroff 2012a.

[14] Per Wikipedia "identifiability" is a property, which a model must satisfy in order for inference to be possible. A model is identifiable if it is theoretically possible to learn the true value of the model's underlying parameter after obtaining an infinite number of observations from it. Mathematically, this is equivalent to saying that different values of the parameter must generate different probability distributions of the observable variables.

> *Although a robust model fit can indicate a possibility of causation, that is not the case for the sort of linear model proposed between flow and escapement, which is highly non-robust in light of the inconsistencies cited in Section 1.1 and the violations of model assumptions cited in Section 1.2. The proposers have not shown that the estimated correlation corresponds with a causal relationship.*[15]

L&B invoke the standard criticism of statistical analysis, particularly in the context of observational data, that correlation does not equal causation. However, this does not offer any rigorous definition of how a causal association should be defined in this context. Whether an association can be asserted to be equal to some causal association requires several identifiability assumptions such as no unmeasured confounding and assumptions regarding the consistency (the convergence with greater sample sizes to the true model) of the statistical models chosen to fit the data (Pearl, 2000). One can never assert a parameter estimate is an unbiased estimate of a causal association (the association that would result from a hypothetical intervention) when they are estimated from observational data, which is of course the whole purpose behind experiments, like clinical trials. However, when such experiments are not possible, other approaches must be taken, and are common in ecological research. In addition, because one can only make statements regarding the identifiability of specific parameters of association[16], which SJTA has not precisely defined, this is hard to address. SJTA has listed some possible reasons, but these are standard assumptions of any modeling in this context, where unexplained variability is impossible to avoid.

As further illustration of issue, consider that the type of observational ecological data used in Version 1.6 bares similarities to the data encountered in studies of the epidemiology (i.e. patterns, causes, and effects) of disease. In that field, it is expected that models will typically include a large proportion of unexplained variance in the outcome, though the resulting statistical analyses have often proved very useful for finding associations of risk factors and disease (and leading to public health interventions). However, such observational studies have very rarely resulted in models that accurately predict who will actually get the disease in the future (see Shmueli, 2010, for a relevant discussion). Human biology is very complicated and there are typically many unmeasured factors in any study that contribute to whether one develops, for instance, cancer versus not. The studies are not unlike the data analysis behind version Model version 1.6 – this is an incredibly complicated system with lots of competing causes of mortality, some of which are nearly impossible to measure. However, *that does not mean* the data cannot provide useful information about certain measurable environmental factors and what is their relative (average) impact on the health of these fish populations. The evaluation of the utility of Version 1.6 should not be based on

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[15] Page 3 in Lorden and Bartroff 2012a.

[16] If SJTA suggests that CDFW does not have the "true" model, and that is why CDFW does not provide evidence for causation, then SJTA has just discounted a huge proportion of empirical science in many fields where one can never assert a known model from first principles – in other words, this stands far outside the mainstream, so CDFW will assume that is not what is implied.

whether it can perfectly predict, for instance, the size of escapement on a year by year basis, but whether the model provides evidence useful for policy decisions.

2.5 Environmental data

In SJTA's L&B report, the authors fit the following model[17]:

$$y = -14092.5 + 777.7x_1 - 113.0x_1^2 + 14.2x_2 + 5909.3x_3 - 681.9x_3^2 - 4.2x_4 + 4.6x_5,$$

The L&B report proceeds to point to a relatively high value of R^2 and claims that the value might be "somewhat inflated."[18] For the following reasons the L&B report's way to report the fit is misleading. What undermines this criticism by L&B is their conclusions could be based on an analysis of the data that involved, not a pre-specified analysis plan, but involved lots of unsupervised (unrecorded and unplanned) analysis sometimes called data-dredging. Inferences from statistical analysis of data usually assume that there has been non feedback from the data to the choice of the analysis performed, and also more insidious, that only a portion of the analyses that were performed are actually reported, and the set of reported analyses are not a random choice (influenced by the goals of the analyst). Even if not intentionally misleading, the lack of a priori specified analysis plan can produce quite misleading results (see Freedman, 1983). In addition, in the context of any data-adaptive procedure, even if that procedure could be duplicated by a simple algorithm, the true R^2 (that is, for future data based on equivalent experiments/data from which the model was fit), typically can only be estimated consistently using something like cross-validation. Thus, CDFW little idea of how inflated the reported empirical R^2 that L&B reported is. One would expect that, given the implied model has a large number of parameters, for very limited data, that the increased variance in the prediction estimates is large relative to any bias reduced (relative to that reported in Version 1.6) as a result of this more complicated model. So, given CDFW cannot assume this model was chosen a priori, and given that a non-rigorous procedure was used to assess the fit, we cannot evaluate whether the cost (increase in variance) of having this larger model was not larger than the benefit of reducing bias by entering more variables

More importantly, the model L&B proposed ad fit appears to be suspect in what variables were chosen, given the goals of the analysis. If the authors of the L&B report were attempting to estimate the total effect of flow on survival, they are entering variables that are on the causal pathway of flow, and so adjusting for them creates a bias (typically underestimating the association) in the estimate of the true association (measuring total impact) of flow on survival (which, for some reason, SJTA removed from the model). Several of these variables appear to be, and have by empirical observation been shown to be caused by or strongly influenced by flow (i.e. water temperature (Dotan et al. 2009), dissolved oxygen for instance (Mesick 2001)). If one wants to estimate the impact of flow on survival, the adjustment for these variables will essentially "adjust away" the true impact of flow. If, on the other hand, one wants the so-called direct effect of flow, apart

[17] Page 4 Lorden and Bartroff 2012a.

[18] Page 3 in Lorden and Bartroff 2012a.

from these potential pathways, then adjustment could be appropriate. The form of the model should be based on what is the parameter of interest, and putting aside that the reported fit is probably dubious, it is unclear for what goal this model was fit. If one adjusts for all the factors by which flow affects survival, in the case that interventions on flow have a true large effect of survival, of course the ultimate model will have no association of flow, despite the fact that the policy changing flow would have big impacts on survival. This sort of conceptual error specifically on studies of flow has been made by others as well (Zeug and Cavallo 2013), showing a general lack of consideration for tailoring the model to the association of interest (in this case, the total effect versus direct effect – see Pearl, 2000).

V. Response Report on the San Joaquin River Salmon Population Model

5.1 Criticism of the Model

SJTA's L&B report correctly points out that CDFW are stringing (chaining) together a series of statistical models to form a life cycle model.[19] However, it implies that the chaining occurs by including, in the estimation stage, the predicted mean outcomes from the previous model in the cycle as predictors in the subsequent model (e.g., Mossdale Smolt Production to Delta Survival models). Contrary to the L&B's report inferences, this is only true of the so-called Cohort Production module within the overall model, where the predicted number of smolts at Chipps island (as applied from the Delta Survival module to the estimated Mossdale count) are used as the predictor variable. Because the set of data that are used to parameterize this model come from both census data (carcass counts and Mossdale trawl) and release capture experiments (for Delta Survival), which leads itself to fitting models appropriate to each particular data type, chaining in the implementation (not the estimation) is a reasonable method to derive a complete cycle model. However, the potential problems it does cause, as mentioned by L&B (one does not use the entire cycle to estimate the parameters, and so the model fits are meant to optimize the fit for each stage of the fish cycle, but not over the entire cycle), is one of the motivations for the development of the full-cycle model *Salsim,* which CDFW has been working and will shortly release. For this model, one can derive a global fit (that is, the parameters of the model for all the sub-models that make up one cycle can be fit simultaneously to several years of data). SalSim is an answer to the issue of incorporating data from many different sources.

In short, regardless of the chaining and other model decisions, a model is as good as its relative performance however the most important measure of performance is defined (e.g., ability to predict, ability to estimate the association of environmental variables with survival, etc.), and so whether or not another model could do a better job for some specified task is not addressed by the authors of the SJTA's L&B report. Because of the fact that different data sources are used, and the model is constructed as a sequence of modules, the uncertainty in coefficient estimates in the constituent regressions do result

[19] Page 2 in Lorden and Bartroff 2012b.

in relatively large uncertainties in the estimated escapement numbers. CDFW has made a good-faith effort to report the estimated errors in the mean predictions via a type of Monte Carlo simulation we discussed in our previous description of V1.6 (CDFW 2008), where we estimated the uncertainty of the predictions by propagating the uncertainty in the coefficient estimates into estimates of Modeled Escapement Cohort Prediction from the constituent models via a simple Monte Carlo (drawing at random from the estimated joint sampling distribution of the coefficient estimates within each model).

To summarize, it is unclear whether the authors of the SJTA's L&B report understood how the models were fit, as opposed to how they would be applied to future predictions. Specifically, given the following paragraph[20]:

> The above regression coefficients $\beta_i^{(j)}$ are estimated ("fitted") using historical data, and then the model is used for prediction of cohort production as follows. Given values of average Vernalis flow $x_1^{(1)}$, previous year's escapement $x_2^{(1)}$, daily Vernalis flow $x_1^{(2)}$, and HORB in/out $x_2^{(2)}$, predicted values of the Mossdale smolt production $\widehat{Y}^{(1)}$ and the Delta Survival fraction $\widehat{Y}^{(2)}$ are generated using these values, the estimated coefficients, and the above equations. Finally, in the Cohort Production model, the predicted value $\widehat{Y}^{(3)}$ of the cohort production fraction is generated using the number of Chipps Island smolts given by $x_1^{(3)} = \widehat{Y}^{(1)}\widehat{Y}^{(2)}$, the product of the outputs of the two previous models. Finally, the predicted value of cohort production is given by $\widehat{Y}^{(1)}\widehat{Y}^{(3)}$, the product of the predictions of the first and third models.

This paragraph, on which much of the criticisms are based, does not accurately describe how the model parameters were estimated. It appears that the estimation procedure has been confounded with the prediction procedure. That is, the Delta Survival model is not fit based on inputs that include Mossdale smolts, but are based on survival analysis of independent data (CWT releases), in a combined data set that includes releases at Mossdale/Durham Ferry (i.e. entrance to the South Delta) and re-captures at various points downstream. Thus, much of the criticism of the model structure seems to be based on a misconception of what was done, and does not apply.

5.2 Criticism of Individual Components of the Model

5.2.1. Mossdale Smolt Production Model

In criticizing the relationship between flow and smolt production, SJTA's L&B report states as follows:

> "*The scatterplots in Figure 1, the data used to fit the Mossdale Smolt Production model, show a weak relationship between flow and smolt production, which violates a fundamental assumption of the model.*"[21]

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20 Page 2 in Lorden and Bartroff 2012b.

21 Page 4 in Lorden and Bartroff 2012b.

CDFW does not know how to interpret this statement. Because CDFW fit Model version 1.6 using standard maximum likelihood procedures and did not fit the model forcing any particular relationship between flow and smolt production, it is unclear why any particular relationship violates an assumption. We in no way constrain the estimate of the association, so if values near the null of no association provide the best fit according to the loss function (based on maximum likelihood estimation, MLE), then by definition of MLE then that's what the estimation procedure will return. Either we fail understand the point, or it reflects some misunderstanding of our modeling approach. CDFW avoided the mistakes in the L&B report of constructing confidence intervals outside the natural range of the outcome variable ("...we have performed such calculations for this model and found that the confidence intervals are so wide – even including negative numbers for a prediction of fish counts..."). Given we used proper models (that is models that respect the natural range of the variables being predicted), it is not possible for proper estimation to get a confidence interval outside the range of possible values. Obviously, this was not done by L&B, which indicates some fundamental flaw in their modeling. Specifically, CDFW used a log-linear model, in the context of fitting a Poisson regression therefore, CDFW developed an appropriate proper model for this particular module, as we did for each of the modules. Given the high variance of this type of data and the relatively small sample size, one can gain a significant amount of efficiency by using proper models to estimate the components of the life-cycle model, which CDFW has done, but apparently something neglected by L&B.

SJTA's L&B report also mentions outliers that should be removed.[22] However, in the fitting procedure, one can define an outlier by different criteria, and it is never objective as to whether a point is an outlier). We talked above (section 4.22) about the potentially erroneous way outliers were defined within the SJTA L&B report. Simply removing outliers given an arbitrary definition is not a rigorous way to decide how to remove outliers in a way that optimizes the fit.

Furthermore, as discussed in first paragraph of section 2.3 above, these models are practically used to predict the mean (average) value "escapement" for a set of inputs, and thus that is the target of interest, not the entire distribution (i.e., not just the mean, but the entire distribution of for instance escapement around the mean). Some estimates are based upon so-called working models (e.g., log-linear, Poisson regression models), which can be thought of as a way of generating a consistent estimate of a log linear regression model in this case.

5.2.2. Delta Survival Model

In its L&B report, SJTA states as follows:

> *An area of concern regarding the Delta Survival model which likely affects its output and the output of the combined model is that, by the authors' description, an incorrect use of a logistic regression model: the*

[22] Page 2 in Lorden and Bartroff 2012a.

model was fit with proportions (i.e., percent survival estimates from coded wire tag studies) rather than binary data (i.e., data comprised of two outcomes, either a 0 or 1). The two methods are only equivalent if the total number of smolts leaving Mossdale was exactly the same for each data point (i.e., CWT releases groups were all the same size), which is not true for the data used (i.e., CWT release groups were variable within and between years). This discrepancy can be corrected by a re-weighting of the data, but this did not occur in the authors' description. Using the Delta Survival model with fractional data violates the fundamental distributional assumption of logistic regression models.[23]

The statements above repeat common misconceptions about the assumptions behind semiparametric estimation and regression models that generally matter for the typical use of such models. DFW wanted a proper model for the reasons stated above. The outcome is a proportion (as an estimate of a probability) and logistic regression that provides a proper model estimate of the mean of this outcome conditional on the predictors that is bounded between 0 and 1.

It is correct that weighting can result in more efficiency or provide more accurate inference, if in fact the true statistical unit is the released fish. However, it does not necessarily do so in finite samples, particularly when these fish are released in groups (the real unit of observation) and the absence of using weights does not necessarily lead to bias, and in fact can increase the variance of estimation. In advocating for weighting by the number of fish released, the authors of the L&B report are implicitly suggesting the unit of observation is the individual fish, not the experiment. In fact, given the great variability in survival between experiments, the unit is more the entire CWT release. Because of the great variability among CWT survival experiments, the weights will probably not help and could very well hurt the fit. The L&B report provide no evidence the resulting fit is superior, and no strong justification that it should be done.

5.2.3. Cohort Production Model

As explained above, as a consequence of using the best available data including not just census data, but CWT experiments, required the connection of the Mossdale smolt production through the Delta Survival model, and back to Cohort Production. The evaluation of this approach is whether a different approach would have yielded estimates of greater precision. CDFW's modeling approach in SalSim is a full life cycle model, where more basic biological understanding and more detailed data is brought to bear, as well as the ability to define a likelihood over the entire data structure. For version 1.6, the only part where chaining occurs in the estimation is when we use the Delta Survival module, by taking the inputs for that year (number of Mossdale smolts) and transform them, via the Delta Survival module, inot an input in the cohort production model. Thus, one can think of the Delta Survival and Cohort Production Modules, and one combined model that take as the inputs the Mossdale smolts, and the environmental factors in these modules, and produce an output of the probability of escapement. Thus, the main issue

[23] Page 4 in Lorden and Bartroff 2012b.

isn't whether the combination of the Delta Survival/Cohort Production model is legitimate (it's a candidate prediction model among other possible candidates), it is again, whether another modeling strategy does a better job of predicting escapement given what is known up to the Delta Survival module (the data on Mossdale smolts and environmental inputs). L&B offer not evidence that they have produced a superior model.

5.3 SJTA's Analysis of the Model

This includes several analyses, but the first one is telling. The claim the experiment of interest is an algorithm that is applied to the data observed up to that point, and then used to predict in the future year. This is an interesting approach, but not really very applicable in this case, because that is *not* the experiment of interest relevant to this model. It might be, if the actual fitting procedure involved only including say the last 10 years to make a model, and predicting forward from that, but that only makes sense if 1) that's what was done and 2) we had a long enough history of data to conduct enough pseudo-experiments like this to evaluate how well the procedure works. They of course are limited like we are, on the number of years available, so for early years, the fits are based on very little data. In later years, it becomes more akin to what the true experiment of interest is, but then one only has a single year to evaluate the performance – this is also an estimate and thus requires a sufficient amount of data in order to evaluate how well the performance is measured. L&B appear to be struggling here with an important issue, which is how to evaluate the performance of a regression, based on (nearly) all the data, when based on a time series like this. However, that particular evaluation is misleading since that is not the experiment conducted (and will tend to underestimate the fit). Secondly, the jackknife experiment is just for one year (2001), and again, that's hardly a significant evaluation of the model. In short, the motivation for evaluating the fit of the model a reasonable one, but falls short of a rigorous estimate of the fit. This is admittedly a challenge, and is one that requires a thoughtful approach.

VI. Conclusion

Although SJTA's L&B reports do point out the complexity of fitting a statistical life cycle model to the available data (i.e., annual escapement data, daily flow and temperature data, and fish abundance data etc.), most of the criticisms are generic issues regarding estimation of associations given the type of limited, noisy and non-experimental data. These include things such as the ecological fallacy, the distinction of correlation and causation, and issues regarding how to determine an optimal fit with limit data, and a model with several simultaneous goals. If these criticisms are taken seriously, then no model based on the available data will satisfy such standards. In addition, as presented above, L&B make several errors, some apparently based on a misunderstanding of how Version 1.6 was constructed, and some based on what we believe are more fundamental conceptual errors. In addition, because the available information on the number of analyses that were performed by L&B relative to what was in their report, the meaning of the statistical inference provided is difficult to interpret. Most basically, since the true model is unknown and unknowable, and the available data limited, the relative merits of approaches should be based the statistical goals of the model (e.g., predicting brood year escapement, estimating the association of environmental factors and salmon survival, etc.). Version 1.6 is a reasonable attempt at a straightforward, simple statistical modeling approach. The release of SalSim will significantly augment this capacity. In any case, because there is no a priori optimal model for accomplishing the goals of Version 1.6, and SalSim, we welcome sincere attempts to improve the model. Given the complexity of the system, there are an almost infinite number of possibilities for fitting such a model. Thus, CDFW also sincerely hopes for constructive discussions and collaboration in the future among parties that have defensible ideas for improving the model.

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