Additional Short-term Requests from the Science Panel – March 26, 2012

NOTE: These requests are predominantly directed to Eric Ward.

1. Provide details of the "autocorrelated random walk" that Eric used to model variation in abundance of Chinook. We would like to see just how he simulated variation in Chinook abundance and we were both concerned that results were "odd" when he used a "CV of 20%". We'd like to know just what he meant by "odd" and just what he means when he says "CV of 20%". We have some reasonable conjectures concerning just what Eric did here, but we would like to know for certain.

Eric's response: ok – here's the pseudo-code for the approach I used (I'm happy to send along the actual R function for the simulation if this isn't clear):

1. **Estimate the autocorrelation parameter from the fall terminal index, the index that was used for projections. I used a Bayesian state-space time series model to estimate this for the log-transformed data, and estimated autocorrelation at ~ 0.7 (median). Ray Hilborn asked me which value I used in workshop 2, but I didn't have the value off the top of my head.**
2. **Given the acf parameter, and the CV I specify (0.05, 0.2, etc), generate a random autocorrelated time series of length n (e.g. 30 years)**
3. **De-trend the data. This is done by fitting a linear regression to the simulated salmon time series, and subtracting the expected values from the actual simulated values. The new time series has mean 0, and no trend.**
4. **Multiply the new values by the ratio of the target CV to the standard deviation of the existing data, e.g. x = x * 0.2 / sd(x) . This new time series has a standard deviation equal to the target (e.g. 0.2)**
5. **Add the target mean to the time series. For example, if the mean in log space is 10, the new time series becomes x = x + 10. This final time series is (1) autocorrelated, (2) has a variance of whatever I specify, (3) has no trend, and (4) has a mean of whatever I specify**

Regarding my comments in workshop 2, I was mostly referring to 'odd' as being the killer whale response to this time series, not how the values I used changed the actual salmon time series. Apologies for the confusion.

Panel's comment

The above description is a great help and is consistent with what Sean Cox had conjectured. We just wished to make sure that his conjecture was correct. But we would still like to learn what was "odd" about the killer whale response to the time series with larger CV.

Eric's response:

By 'odd', I meant that there are a handful of cases in any particular batch of 10000s of simulations when the whale growth rate can become unrealistically high (say > 20%). This isn't as much of a problem with the 5% scenario, because the positive outliers are the source of the problem. As I've told panel members Sean, Ray, and Tom in email exchanges, if the metric we're comparing across actions is the median / mean population size, or number of whales / year, the CV of salmon projections has no effect. IF we're talking about probabilities of extinction, or recovery, than the CV does matter. In all of my responses following workshop 2 (questions 1-3, 5 in the previous request), I used a CV of 20%.

2. We'd like to see the logistic regression analyses expanded so that they not only include an assessment of whether or not chum salmon abundance, by itself, or in combination with Chinook, is a significant explanatory variable, but we would also like to see pinniped abundance (a weighted average of harbor seal and sea lion abundance, weighted by guesstimated average daily consumption of Chinook per individual) included in these analyses. Assuming that fecundity and/or survival of SWKR are related to abundances of chum and Chinook salmon (alternative prey items) and pinnipeds (competitor), we'd like to see Eric drive his simulated 30 year trajectories assuming high (current) pinniped abundance and mean (?) chum abundance, allowing only Chinook abundance to vary, to see how results compare to those based on logistic regression fits again Chinook abundance alone.

Eric's response: This is a really interesting question, and I think there are other components (e.g. hatchery releases) that could also be included. Unfortunately, the major limitation is a data issue. We don't have continuous time series of sea lion and harbour seal abundance – I think there are maybe ~ 10 sea lion data points, and 20 harbour seal data points (both from DFO reports and Washington state / Oregon state surveys) – the most recent harbour seal survey in Washington state, for instance was in 1999 (described in Jeffries et al. 2003). For chum abundance, I have looked previously for a continuous time series, but was unable to find a good one.

Following requests from the science panel, in lieu of real data, we can use Ecopath model output (D. Priekshot, workshop # 2), and include time series of indices of seal and sea lion abundance. I'll call the 'baseline' model of killer whale survival the one I

presented at workshop 2 (Chinook Parken-Kope fall index and whale stages as a predictor. The list of models requested by the science panel are:

	Covariates in kw survival	AIC
1	None	18.25
2	Parken-Kope fall termsinal Chinook (+)	6.95
3	CA sea lions (-)	15.26
4	Harbor seals (+)	20.18
5	Sea lions (-)& Harbor seals (+)	10.70
6	Sea lions (-)& Harbor seals (+) & Chinook (+)	0.74
7	NRKW total population size (-)	18.56
8	NRKW total population size (-) & Chinook (+)	6.86
9	Sea lions (-)& Harbor seals (+) & Chinook (+) & NRKW (-)	0.00

Although we might expect seals and sea lions to have negative effects (seals more on age 2 Chinook, sea lions on older Chinook), the effect of seals in these models is positive. This result may be spurious (and the model with seals only does worse than a model with no covariates). The best model in this list includes all marine mammals and salmon. In general, NRKW abundance isn't strongly supported (because for models 9 and 8, simpler models exist that perform essentially the same; models 2 and 6). The next best model would be model 6, with opposing effects of seals and sea lions.

<u>Panel response:</u>

It's unfortunate that appropriate time series for chum and pinnipeds are not readily available as we feel it is critical to examine the potential connection between SRKW "fecundity" and survival rates and these potential explanatory variables, as well as various measures of Chinook abundance. Finding the appropriate combination of chum stocks to

sum as an appropriate aggregate available for SRKW would pose the identical issues raised for Chinook: summer (maturing/mature fish only), winter (immature and maturing), and corresponding ocean distribution patterns of different stocks.

3. We'd like Eric to have discussions with Robert Kope and Larrie LaVoy to get a better average figure for the increase in relevant terminal run size of Chinook salmon that would result if fishing were curtailed. It is inappropriate to directly compare catch to catch + escapement because the catch consists of both immature and maturing fish whereas the escapement is of mature fish only. Not all of the fish that are not caught would be expected to return in the terminal run where they would be available to SRKW due to (a) incomplete maturation, and (b) ocean mortalities due to other causes of mortality (competing risks argument). Member of the Panel contacted Larrie and Robert on this point and they both agreed that the 20% "improvement" in abundance was probably too high, though it would take some calculating to provide a more reasonable value.

Eric's response: I discussed this with Robert and Larrie. First, to clarify - I didn't select the 20%. Based on my conversations with the panel during workshop 1 (when 20% was suggested as an upper limit) and the science panel report (excerpt from p 21 below), the goal of this exercise for workshop 2 was to evaluate the upper bound of any impact. The 20% (roughly the catch / (catch + terminal run)) from the Parken-Kope indices) was mostly in agreement with previous analyses involving immature and mature fish (FRAM model used in the BiOps), Angelika's analysis involving only mature fish (FRAM model, presentation for workshop 2). As I said during my presentation for workshop 2, other values – 3%, 10%, etc. could be evaluated using the plots I presented.

The suggestion that Robert, Larrie, and myself discussed would be that one correction factor that could be used is the adult equivalent fraction (AEQ) in the total catch. From the most recent Chinook Techinical Committee report,

"Estimated annual catch rates averaged about 27% in nominal numbers and 24% in AEQs from 1985 to 1998 (Hendrich et al. 2008)."

Thus, 27%*89%= 24%, and so the difference (11%) represents the natural mortality of immature fish in future years prior to spawning, if there were no fishing. This is kind of a back of the envelope calculation, but the point is that the fraction of immature is not large relative to mature fish.

<u>Excerpt from Science panel report on workshop 1, p 21:</u>

"The revised analysis should calculate the probability that the future population will

be below the lower limit of the acceptable range, within the range, or above its upper limit. These probabilities should be calculated using scenarios for salmon abundance that reflect achievable increases in salmon abundance–that is something like 10-20%"

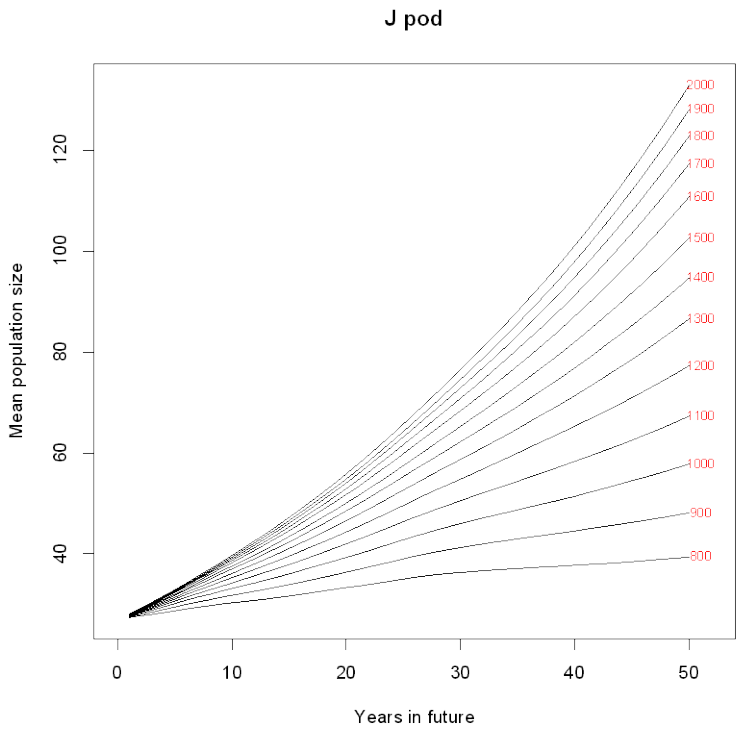
<u>Panel comment:</u>
Several comments here. First, the period 1985-1998 referenced above would no doubt have substantially higher harvest rates than more recent and future settings. Second, 20% is the upper end of the recommendation from the Panel, as quoted above. We therefore recommend: (a) work with LaVoy and Kope to develop a better basis for a single choice of value for increasing availability of Chinook from fishery closures, or (b) carry out the same analyses at lower rates that are also realistic to contrast with the previous 20%.

Eric's Response:
About the first point, we're not going to reach consensus on a single number by the time the panel's draft report is written for the following reasons:

1. **We don't have detailed information on spatial-temporal overlap between whales and salmon**
2. **We don't know what proportion of the salmon that are saved from fishery impacts would be lost to other predators (sea lions), or escapement**
3. **We don't have a good idea about the true harvest rate of the fishery (I say 'true', because while we have estimates of harvest rate, we don't have an estimate of the fraction of the total population of Chinook that the fishery captures – the total population including losses to marine mammals).**

About the second point, "carry out the same analyses at lower rates that are also realistic to contrast with the previous 20%", I already did this and presented all of the results at Workshop 2. In my final talk, "Effects of fishing on lambda", I'll refer you to slides 47-61. For each metric, I calculated what the effect would be of increasing Chinook by 20%, but also for many other levels of salmon (each of those plots depicts 12 adjustments to Chinook abundance, basically across the terminal run sizes that have ever been observed). For example, for the first slide (47), we could look at the effect of increasing Chinook abundance on the population size of J pod from 1200 to 1300, which translates to an effect of (1300/1200) = 1.083, or 8.3%.

Figure 1. Slide 47 from Eric's thrid talk in workshop 2.


J pod
Mean population size
Years in future
40
60
80
100
120
0
10
20
30
40
50
2000
1900
1800
1700
1600
1500
1400
1300
1200
1100
1000
900
800